Public Safety Intervention: Analyzing the Effects of Altering Traffic Safety Policies in the States

By

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Abstract

This research focuses on analyzing the effects of shifting traffic safety policies within the United States. These naturally occurring shifts are examined to determine the effectiveness of altering traffic safety policies. Three substantive chapters are presented focusing on different policy shifts. Examining the speed limit increase in Kansas reveals that the 2011 increase did not result in increased fatality figures; however, it did produce a significant increase in serious injuries sustained from crashes. The probable reason is attributed to the increase only affecting four-lane divided highways; these crashes are not the deadly head-on crashes but with vehicles traveling the same direction at different speed. Studying the upgrade to primary enforcement of seatbelt laws in Kansas reveals considerable responsiveness from local law enforcement officers when state legislators change the parameters of enforcement. Primary enforcement increases the ability of officers to conduct traffic stops while imposing a monetary limit on the citation acts as a reduction in profitability for cities. Kansas' $10 seatbelt citation fine does not even cover the cost for municipalities of enforcing the policy, thus cities with managers substantially reduce enforcement efforts. Finally, analyzing three distracted driving policies for all states reveal that only completely banning cell phone usage while operating a vehicle, for all drivers is associated with a reduction in the seriousness of crash outcomes. Policies banning texting while driving and novice drivers from using cell phones while driving are ineffective at reducing serious crash outcomes. Actually, policies banning teens from cell phone usage is associated with an increase in crashes caused by cell phone based distractions. This finding shows the limited long-term effects of policies targeting teens, in shaping their behavior in a desirable direction. Overall, this project provides statistical support for specific policies that are important for policymakers to consider when contemplating shifts in their state's traffic safety policies.

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Tables and Figures

Chapter 1 - Introduction

Social regulatory policies are present in nearly every aspect of our daily lives; their purpose is to influence individual behavior in a direction deemed desirable by policymakers (Tatalovich and Daynes 1988). “Social regulatory policies seek to change behavior that is linked to a normative debate concerning the morality of individual actions and the subsequent consequences of those actions to the rest of society” (Haider-Markel 1998). Traffic safety policies identify undesirable behavior, and attempt to shift behavior with (dis)incentives by delivering punishment to offenders that have harmed society through their actions.

Although many studies have examined how policies are identified, formulated, and ultimately passed by Congress at the national and state levels, comparatively little effort has been spent on examining how public policy analysis provide an evaluation of implemented policies. Theories of policy change, such as the advocacy coalition framework (Sabatier and Weible 2007), only addresses the policy process until actors have achieved their desired policy outcome. Punctuated-equilibrium theory (True, Jones and Baumgartner 2007) is useful in examining a broader spectrum of the policy process; although provisions for positive and negative feedback are included the main focus is on how policies change over time. While these approaches are large developments on how we explain policy change they do not place emphasis on the ‘backend’ of the policy cycle where feedback is provided to determine the appropriateness or effectiveness of the policies implemented.

Public bureaucracies are often charged with implementing policies passed by the US Congress or state legislatures. Several themes have emerged from implementation research; first, development of analytical models, second, the distinction between top-down versus bottom-up approaches; third, the attempt to identify important key variables to implementation (Schofield

2001). Top-down approaches to implementation has been supplied by many scholars, favoring the rationalist public administration model separating policy from implementation, most notably Sabatier (1993). However, the implementation process in not linear in the sense that policies passed by Congress are implemented by bureaucrats in its intended form (Lipsky 1980). Further, policy 'delivery channels' are at the heart of implementation studies (O'Toole 1993) and the type of organizational structure implementing policies has been uncovered as an important variable to the implementation process (Hjern and Porter 1981). How useful is policy analysis in the policy process? Although much research has examined policy formulation, passage, and implementation there is a lack of congruence in how political scientists view the usefulness of the evaluation mechanisms in the policy process. My goal with this project is to show how policy analysis is a useful aspect of the policy process and, if results are interpreted correctly, is a valuable tool in assessing the appropriateness and effectiveness of social regulatory policies in solving the identified policy problem(s).

Rationale

This dissertation explores the impact of shifts in policies at the state level, and could be used as feedback about the effect of these shifts by evaluating them statistically. The constitutionally designed vertical division of government power – federalism – provides a prime research opportunity for many social issues in the United States since it allows for considerable variation. Bureaucrats at different levels of government use their discretion when charged with implementing policies directed to them from above. More importantly, do they reevaluate already implemented policies? In this dissertation I examine transportation safety policies distinct to the state level, and in one chapter how it influences the local level. Policy outcomes

are analyzed in an attempt to make inferences about the effects of shifting these policies in a new direction. Results from these analyses are then used to evaluate the effectiveness of shifting the speed limit in a state; upgrading to primary enforcement of seatbelt compliance; and regulating distracted driving. These policies are distinct to their respective levels of government and overlapping jurisdiction is non-problematic. The constitution reserves the rights of controlling highway policies to the states, enable municipals to enforce compliance with local ordinances, and provide a comparative advantage when examining state policies aimed at combating distracted driving. Federalism thus provides ample opportunity for research analyzing changes in outcomes following policy shifts.

In this project I examine three different types of public safety policies at the state and local level. The chapters are essentially individual studies, where outcomes are evaluated following policy shifts, in order to make generalizable predictions about the effectiveness of these policies given the identified public safety problem. The studies will rely on available data that best addresses different research questions collected under the dissertation's theme; transportation safety. Results from studying the outcomes of shifting these policies will complement our theoretical understanding of feedback in the policy process. Evaluating the effectiveness of traffic safety policies, by analyzing outcomes of altering these policies, natural experiments are introduced to the field of research on the policy feedback mechanism.

Research Questions and Significance

This examination of traffic safety policies' appropriateness at the state and local level is guided by the following research questions:

1. How can policy analysis be a valuable tool in evaluating the effects of policy shifts?
2. Are *local* enforcement officers responsive to policy shifts made at the *state* level?
3. How effective are state policies aimed at reducing occurrences of distracted driving?

The potential impact on population groups from policy shifts may pose problems to how the interests of these groups are represented by specific policies. If the effects of policy shifts are ignored, policies may not mirror the legislators' original intention or represent the best interest of the public. Thus, revising policies based on outcome analysis, affect both the wellbeing of citizens and may shift the policy towards its intended purpose. The three policies I examine in this dissertation have the potential to impact the lives of nearly all Americans.

States enjoy reserved powers to regulate highway travel by determining appropriate speed limits. Alterations in these regulations pose a potential hazard to state residents and non-state residents alike. Many commuters nationwide pass through territory controlled by states they do not reside in; therefore, almost all travelers in a region are at risk if increases in speed limits results in more fatalities and serious injuries. Municipalities derive authority from state constitutions and are charged with enforcing local ordinances. When local law enforcement officers (LEOs) are given broader authority to enforce policy compliance they are expected to increase the scope of their activities. However, when the state limits the weight of the penalty for non-compliance LEOs are able to use discretion to focus enforcement elsewhere. Finally, the use of hand held devices has increased dramatically during the past decade and distracted driving pose a significant hazard to the public safety. By analyzing various policies states have adopted to combat distracted driving, I provide feedback on the effectiveness of these policies given the identified problem of distracted driving. In sum, alterations in the proposed policies will impact millions of Americans and may disproportionally impact the lives of many minorities. Bureaucratic implementation may have further shifted a policy away from its intended purpose,

but adjusting policies based on analysis and evaluation may return the policy closer to legislators' original intent.

Methodology

Few policy studies have employed a quasi-experimental design; in fact, most of published research in political science relying on experiments has not addressed any elements of public policy. Between 1926 - 2000 only 105 articles have been published, in political science journals, where researchers rely on an experimental design; 80 of these were published in five journals (*Public Opinion Quarterly, Political Psychology, American Political Science Review, American Journal of Political Science,* and the now defunct *Experimental Study of Economics*), and 94 articles addressed topics unrelated to public policy such as voting behavior, bargaining, games, committee work, race, and media (McDermott 2002, 43-45). This lack of reliance on experimental approaches makes the public policy area ripe for research employing such a design. When analyzing the effects of increases in speed limits, upgrading to primary enforcement of seatbelts, and the effectiveness of distracted driving policies I utilize a natural experiment design – a subgroup of the larger experimental design. This approach realizes several of the advantages associated with regular, "gold standard," experiments. In the following paragraphs I will address the advantages of experiments and some concerns many political scientists have regarding this methodology.

Experimental research designs offer distinct advantages over other methods making this approach the *"gold standard"* for quantitative research. *Causality* – the ability to confidently make causal claims – is the primary motivation for experimentation, and is derived from the ability to control recruitment, the assignment to random conditions and treatments, and

intentionally produce variation in independent variables (McDermott 2002). These advantages assure internal validity – the fact that the experimental treatments make a difference in this specific experimental instance (Campbell and Stanley 1963, 5) – which is how experimenters are able to confidently make causal claims that variation in one variable produced variation in another variable. Many academic disciplines – i.e. the hard sciences – have relied on experimentation for decades due to how this approach addresses potential problems inherit to all research.

A major issue for all research designs is how reliable it is; studies that are easily *replicable* increase the *reliability* of any results. An experimental approach *standardizes* measures, allows for large numbers of measures to be analyzed, and control for any factors that might introduce bias in the study, in order to improve reliability (Zimbardo and Gerring 1996). *Randomization* ensures that unknown variation in the subjects' background is cancelled out when researchers have no influence in subjects' group assignment. Thus, experimenters "ensure that no unrelated or spurious factors vary consistently within a given population and therefore bias the results" (McDermott 2002, 33). This is useful when comparing *between-subjects*, as in a treatment group compared to a control group. *Within-subject* examinations also benefits from randomization, where time before treatment acts as the control compared to time after treatment. Therefore, both *cross-sectional* and *longitudinal* designs increase internal validity – the certainty that change in X caused changes in Y. However, political scientists are generally more concerned with *external validity* than *internal validity* (McConahay 1973, 542).

Political scientists are often concerned with external validity of research as they focus on the *generalizability* of results to other similar cases and situations. Campbell (1968) has outlined six threats to external validity. First, it may be difficult to generalize results when *subjects are*

sensitive to the variables being examined. If the study itself increases subjects sensitivity then results may be biased and produce an inflated outcome since subjects are more aware than before participating in the study. Second, an *unrepresentative sample* will also produce biased results. The classic example would be studies relying on college sophomores as the sample population, although sophomores systematically differ from non-sophomores results are inferred on the general population (Sears 1986). While an experiment enjoys a high degree of internal validity it does not necessarily mean the results are generalizable to other populations. Third, people may alter their behavior when observed, a phenomenon called the *Hawthorne effect* (Roethlisberger and Dickson 1939). For example, constant supervision by foremen will change the behavior of workers and how they perform their tasks. Fourth, *professional participants* may be able to anticipate the underlying hypothesis examined in the experiment thus skewing the results in a specific direction. On college campuses students may participate in several studies at any given time thus increasing their ability to detect the elements being tested in an experiment. Fifth, the experiment itself could produce *spuriousness* as a result of subjects providing systematically irrelevant responses that are then considered effects of the experiment. Sixth, irrelevant aspects of the experiments could produce *irrelevant measures* that appear to be outcomes of the experiment. These six threats to external validity needs to be taken seriously when designing experiments, if not addressed they might introduce biased results that affect the generalizability of the research findings. Thus, political scientists have relied less on experimental research designs than many other disciplines due to the paramount focus on theory testing and generalizability of results from one case to another similar situation. Although “external validity is only fully established through replication” (McDermott 2002, 40) results from experiments may be less applicable outside the controlled confines of the laboratory.

The artificially created environment of a laboratory setting – where experiments are often conducted – may interfere with any observed outcome(s). Whereas study participants might be unrepresentative of the population researchers want to make inferences about, study settings – i.e. the laboratory – is unrepresentative of most real world settings. Elections, wars, recessions, natural disasters etc. are situations difficult to replicate in a sterile laboratory. However, it may be unethical to attempt replication of these situations. Discovering a cure to cancer, AIDS, Ebola, or measles would be easier if human subjects were introduced to these illnesses, and then divided into control-and treatment groups. Researchers could study the effectiveness of vaccines and determine their effectiveness by comparing how many people were cured compared to a control group where nobody received a lifesaving vaccine. This scenario is obviously highly unethical but highlights a problem with the generalizability of results produced in a sterile laboratory setting. Further, study participants may act differently in a setting where outside structures and relationships are irrelevant since they cannot be replicated in the experiment (Walker 1976). Making decisions in a bureaucratic setting is quite removed from the laboratory, where supervisors and consequents are relatively absent. Therefore, experimental designs have been less frequently deployed in political science than other disciplines due to concerns over generalizability. External validity of study results and the lack of real world relevance further exacerbate concerns over the generalizability of results from testing theories and hypotheses.

Public policy analysis research could successfully deploy variations of an experimental research design. When analyzing the effects of increases in speed limits, upgrading to primary enforcement of seatbelts, and the effectiveness of distracted driving policies I utilize a natural experiment design; also called “field studies” (McDermott 2002), “Nature’s or society’s experiments” (Morgan 2013), and “administrative experiments” (D. T. Campbell 1967/1988).

This experimental design relies on naturally occurring events outside the control of the researcher that are representative and achieves a high level of realism (McDermott 2002, 32). Since the researcher lacks control over the variables of interest these types of studies are necessarily retrospective and are often organized around "before and after" data to test hypotheses. Although there are several different subcategories of natural experiments – four according to Morgan (2013) – the three substantive chapters in this project are *ceteris paribus* studies, where all other things are held at the same levels before and after a naturally occurring event. This approach realizes several of the advantages associated with regular, "gold standard," experiments. Due to the lack of control over important variables interfering causes and small disturbances must be shielded out by the researcher, which is often done through "reverse designing" (Morgan 2013, 349). The result is a naturally occurring incident examined as it was designed *a priori* to follow the same chain of events that actually took place. Several studies have successfully employed this reverse research design, ranging from the supply of dirty drinking water from two treatment plants to find the cause of cholera in mid-nineteenth century London (Snow 1855), to explaining transition shocks experienced only by firms in East Germany following reunification (Kogut and Zander 2000). Through reverse design researchers are able to compare before and after effects of an event and provide meaningful insight into the processes at hand. For example, Card and Krueger (1994) put natural experiments on the map for economists by studying changes in New Jersey minimum wages and comparing them to other industries that did not shift wages. They found that an increase in the minimum wage did *not* reduce employment figures in affected industries. By mimicking the *gold standard* experiment, natural experiments can provide real-world evidence and the results might be more applicable to other similar situations than laboratory experiments.

Chapter organization

Chapter 1: Introduction

This chapter introduces the research questions; the underlying methodology used in the three substantive chapters, and discusses the role policy analysis and evaluation plays in providing feedback on the effectiveness, thus appropriateness, of a policy. As briefly noted, theoretical gaps exists in the literature thereby providing a limited overarching theoretical framework ready for adoption in this project. Later chapters will deal with selected policies and how outcomes of policy shifts guide our understanding of the effectiveness of such policy alterations. They will rely on different theories, where applicable, or examine empirical questions that have been unsatisfactorily answered by previous research. Findings from these individual studies will guide the contribution of my dissertation to the literature on the appropriateness of certain policies given the perceived public safety problem.

Chapter 2: Killer Speed

In the summer of 2011 authority to set speed limits on Kansas roads devolved to the Kansas Department of Transportation. KDOT determined which road stretches were suitable for an increase to 75 mph. This study analyzes the effects – of increasing speed limits – on levels of fatalities and serious injuries. A Poisson model was used on count accident data from the 2011 policy shift, and the results were compared to data from an earlier speed limit increase in 1996. A positive relationship between travel speed and levels of fatal accidents exists, but the 2011 shift was not statistically significant. However, travel speed's relationship with severe injuries is positive and significant. For 1996; the relationship between speed limits and fatalities/injuries is

positive and significant while the speed limit increase is negatively associated with both fatalities and injuries. Therefore, comparing the results from these two events in Kansas, reveal that under certain circumstances speed limits may be increased without resulting in significantly higher injury and fatality figures. While the speed limit was increased on all public highways in 1996, it was only raised on four-lane divided interstates in 2011. Road conditions play a large role in determining the seriousness of crash outcomes. Injuries sustained from crashes on divided highways are less serious due to vehicles traveling in the same direction but at different speeds. Crashes on undivided highways more often results in serious and fatal injuries as vehicles are involved in head-on crashes at higher frequencies.

Chapter 3: Bureaucratic Discretion and the primary enforcement of seatbelts

How do municipal governments respond when the state imposes restriction on local enforcement efforts? Previous research on bureaucratic discretion indicates that street-level bureaucrats will rely on discretion to alter the distribution of government services and resources when principals at the state level signal and alteration in preferences. This study examines the number of tickets issued in five Kansas cities for failing to wear safety-belts following a state imposed $10 maximum fine and upgrade to primary enforcement. A negative binomial count model provides evidence supporting municipal responsiveness to legislative alterations at the state level following the upgrade to primary enforcement. Examining the ticket ratio through a beta distribution model indicate that officers' are spending more time enforcing other criminal behavior following the statewide maximum fine imposed by state legislators, set at $10, on municipal governments. This change in number of tickets issued is attributed to the use of officers' discretion when enforcing the seat-belt law. Additionally, cities with mayor-councils

was found to significantly increase the number and ratio of tickets issued for non-compliance with the seat-belt law while council-manager cities focus more on the cost-effectiveness of enforcement.

Chapter 4: The effectiveness of distracted driving policies.

Technological developments in communication have revealed itself as a traffic safety problem. Distracted driving has become such a large issue that even the Alliance of Automobile Manufacturers, representing 11 major car companies, joined the efforts to ban the use of handheld devices while driving (Reuters 2009). Studies have shown that distracted driving increase the risk of crashing by 23 times compared to non-distracted drivers, and talking on cell phones while driving increase your chances of collision by more than 30% (VTTI 2010). Between 1999 and 2008 distracted driving resulted in a total of 51,857 fatalities (Wilson and Stimpson 2010). More specifically, the increase in texting (SMS) volumes has resulted in thousands of additional road fatalities in the states.

Currently, all states except Montana has some type of handheld device usage ban while operating a vehicle (Baumann 2014). This chapter is a comprehensive, cross-sectional examination on the topic of distracted driving. Cross-sectional data on the cause of highway fatalities are available through the National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS). This program collects data from all participating states and contains observations from 1975 to 2013, the most recent available data, and identifies if distracted driving was the culprit of a fatality. However, only data for 2010-2013 incorporate detailed information on the type of distraction(s) a driver experienced immediately prior to a crash. Distracted driving policies dealing with restricting texting while driving, teens from using

cell phones while driving, and banning cell phone use for all drivers, were analyzed and evaluations were made on their effectiveness given the identified policy issue. The only policy significantly impacting the occurrence of cell phone distracted driving was policies restricting novice drivers from using their phones while driving. Although this relationship was found to be significant it was positive, meaning that these policies are not associated with long-term shifts in target-group behavior. Only complete bans on cell phone usage for all drivers are effective at reducing all type of injuries sustained from vehicle crashes.

Chapter 5: Conclusion

This chapter will discuss the results from the three individual studies and assess the overall contribution to the literature. A brief summary of the substantive chapters is provided, addressing the research question examined, the data and methods used in the analysis, results and discussion. The implications of findings in each chapter are discussed in a condensed format compared to this section in the chapters. Obviously, the research designs employed throughout this project result in certain limitations. For example, case studies examining conditions in one particular case may not be generalizable to other cases that are significantly dissimilar. Employing a cross-sectional design increases generalizability but may pay little attention to contextual factors leading to limited applicability in a particular case. Although these limitations are general and revolve around the research design of this project, it is important to address these concerns.

Chapter 2 - Killer Speed: Analyzing the effects of increasing speed limits on Kansas roads.

Introduction

Over time road safety has become a growing concern to politicians, infrastructure users, and insurance agencies to name a few. Thousands of people lose their lives on the country's roads every year and annually hundreds of thousands sustain injuries from traffic related accidents. The economic implications of safety is estimated to cost the country hundreds of billions of dollars and the cost increases over time due to increased medical costs, lost earnings, and emotional costs from lost family members. Although the National Maximum Speed Limit (NMSL) was not intended to reduce fatalities it did cause a reduction in fatality figures. When the NMSL was relaxed in the late 1980s and abolished in the mid-1990s traffic fatalities and injuries increased nationwide.

Observing these trends presents the question; why would any state increase the speed limit on their roads knowing that higher speeds tend to produce additional fatalities and injuries? This study seeks to uncover the rationale for state authorities to increase the speed limit in their jurisdictions. First, a background on the debate in Kansas over speed limit is provided and stakeholders are identified; second, a review of previous research on the impact of increasing speed limits elsewhere follows; third, accident data from Kansas Department of Transportation (KDOT) has been analyzed and the results are presented before discussing the study's implication for lawmakers. The push for changes to the maximum speed limit in Kansas originated in the state legislature.

Background

Influences external to the issue of passenger safety prompted the passage of the most influential highway safety legislation in the 1970s. Middle Eastern oil producing countries enacted an oil embargo against the United States in the early 1970s resulting in the adoption of a National Maximum Speed Limit (NMSL) of 55 miles per hour in 1974 (Yamane and Bradshaw 2008). The US Congress effectively took power of setting speed limits away from states and made the 55 mph speed limit permanent. Initially Congress wanted to conserve energy by adopting a lower speed limit a due to the imposed oil embargo; however, a positive side effect of the move to curb gasoline consumption was a decrease in road fatalities from 54,000 in 1973 to 45,000 in 1974 nationwide (NHTSA-FHWA 1998). In 1987 Congress relaxed the NMSL and allowed states to adopt a 65 mph speed limit instead of maintaining the old 55 mph limit on rural highways (Balkin and Ord 2001). Congress returned control, of setting speed limits on roads, to states completely in 1995 when the NMSL was repealed altogether (Vernon, et al. 2004). The earliest relaxation of NMSL only affected rural sections of the interstate system while the repeal allowed increased speed limits on all roads without further federal-restrictions. The effect of reducing fatalities nationwide was not the intended goal when Congress imposed a national speed limit and assessing outcomes of this effect becomes a normative endeavor. However, reducing traffic fatalities have major impacts on factors associated with highway travel, family life, social settings, and the overall economy. Injuries and fatalities from traffic accidents are a terrible strain on society. “Traffic crashes cost the nation about $230 billion each year in medical expenses, lost productivity, property damage and related costs. Kansas pays $1.9 billion of these costs, amounting to about $700 for every resident of the state, each year” (Aldana 2010). Advances in technology have produced ever safer personal vehicles, vehicle accessories,

highway and monitoring systems potentially reducing fatalities while there has been a steady growth in the number of highway travelers.

The debate over speed-limits and possible policy shifts in Kansas

Many states opted to increase the speed limits on their roads when this power was returned to the states from the federal government, with the repeal of the NMSL, in 1995. Kansas increased the maximum speed limit from 65 to 70 mph in early 1996 and from 70 to 75 mph in the summer of 2011. Kansas has suffered during the economic recession but lawmakers attempted to alleviate economic hardship for businesses with 'innovative' public policies. In 2010 Overland Park Republican Representative Marvin Kleeb pushed for legislation that allowed "the secretary of transportation to set the new speed limit on divided, four-lane highways" (Hanna 2011). Kleeb argued that shipping companies were bypassing Kansas highways because surrounding states allowing for faster transportation through the Midwest due to higher speed limits. The Republican was quoted: "It will make us more competitive. There's a lot of east-west, and even north-south options. This will bring business – logistics and distribution business – across Kansas" (Hanna 2011). The legislation Kleeb initiated – HB 2192 – passed the Kansas House by a107-13 vote and the Senate by a 23-14 margin (Hanna 2011). The bill was signed into law by Governor Brownback, and took effect July 1, 2011 (Clark 2011). Only limited access rural freeways experienced the increase in the maximum speed limit, totaling 807 miles of road including most of Interstate 70 and 35, and US highways 69 and 81 (Rothschild 2011). Kleeb's legislation authorized KDOT to set new speed limits and the agency looked at safety statistics on different stretches of freeways before it decided to change any speed limits (Mann 2011). Kansas highway 10 was excluded due to heavy congestion said director of KDOT's Division of Planning

and Development, Chris Herrick (K-DOT 2011). The political rationale for the increased maximum speed limit in Kansas was to aid the economic recovery as more shipping industries relocate their Midwestern trucking routes to the state.

Supporters of increasing the speed limit

Politicians hoped that an increase in the maximum speed limit would aid Kansas' economic recovery by creating an incentive for companies to relocate their shipping routes and through the state. An increase in travel speed is often correlated with an increase in fuel consumption. "The average car is likely to experience a [fuel economy] penalty of 33 percent [at higher speeds]" (Energy and Environmental Analysis 2001). When vehicles travel faster tend to consume more fuel generally resulting in increased fuel cost for the same distance traveled. The Department of Energy claims that "each 5 mph you drive over 50 mph is like paying an additional $0.27 per gallon for gas" (fueleconomy.gov 2012). One reason the increase in speed limits would aid the economic recovery was that the increased fuel consumption would increase the state's income from fuel taxes (Oberholtz 2011). Increased tax revenue would help the state pay expenses incurred during the recession while providing an incentive for shipping companies to relocate trucking routes through the state. For the generally Republican leaning state government speeding up the economic recovery has been a priority and relaxing government regulations on travel speed. However, there are those that object to the increase of speed limits from a non-financial standpoint.

Opponents of increasing the speed limit

The drawback of increasing highway speed limits was the potential increase in road fatalities, serious injury crashes, and the direct costs associated with raising the speed limit on select stretches of highway. Even Republicans state legislators were split in their view on the consequences of raising the speed limit from 70 to 75 mph. “I think when the speed limit’s 70, people drive 75 or 80. I think when it’s 75, they drive 80 or 85. We will see an increase in fatalities on our highways and we will see an increase in the severity of injuries on our highways” said state Senator Vicki Schmidt, a Republican from Topeka (quoted in Hanna 2011). Critics of representative Kleeb’s legislation claim several studies had shown that increasing the speed limit resulted in a statistically significant increase in fatalities and severe injuries on those roads affected by the increase. One study showed that in states where the speed limit was raised from 65 to 75 mph the deathrate increased by 38 percent; those states that increased the speed limit to 70 mph saw a 35 percent increase in the highway deathrate (Mann 2011). Nobody supports legislation that increses the likelihood of fatal accidents. However, supporters of the legislation altered the policy image by minimized the negative consequences, and refered to studies showing no significant correlation between speed limit increases and increases in fatality rates; a study focusing on the 1996 Kansas speed limit increase found no statistical significant increase in fatalities and serious injuries on rural roads (Najjar, et al. 2000). Due to the uncertainty of the impact of an increase in speed limits on fatalities and injuries opposition to the legislation pushed by Rep. Kleeb was weak. Rep. Kleeb forsaw objections to his legislation and claimed that none of the 14 other states – that have increased their speed limits to 75 or even 80 mph – have moved back to pre-increase speeds (Capital-Journal 2011). The core of the argument

centered around other states maintaining speed limit increase as 'proof' of the neglible impact on fatality rates.

Opposition to the increased speed limit also concerned the cost of the move –potential financial and human costs. AAA spokesperson in Kansas – Jim Hanni – said that no *group* opposed the passage of legislation enabling the change in speed limits on highways (Rothschild 2011). To save cost associated with implementing the speed limit change, KDOT retrofitted existing signs with an aliminium insert indicating the 75 mph limit where applicable. KDOT estimated the direct cost for new signs to range between $16,000 and $24,000 (Capital-Journal 2011). Objections to the increase were primarily limited to concerns with a potential increase in fatal and serious injury crashes and the financial cost of the move. Besides these concerns there were no real political objections over raising the speed limit on Kansas highways.

There had been public support for the measure for quite some time (Capital-Journal 2011). However, the outcome of the legislation had the potential to affect any Kansan. Anyone who drives on highways with the new speed limit are at risk – either from own fault or from other drivers. Novice drivers and male drivers are especially at greater risk of being involved in a crash (Bedard, et al. 2002); increasing the speed limit does nothing to mitigate this trend.

Implementing the policy shift

From an implementation perspective the process of raising the speed limit was simple and clear. The legislation pushed by Rep. Kleeb allowed the Secretary of Transportation to determine the stretches of separated multilane highways where the speed limit would increase (K-DOT 2011). Opposition to this political move was not effective at preventing passage of the legislation and politicians gave KDOT the authority to change the speed limits. The legislature

signaled that there was majority political support for an increase in the speed limit on certain highways. The bureaucracy – KDOT – was charged with implementing the politically favored policy change. The process of increasing the speed limit was straight forward; first, KDOT determined which stretches of highway could handle an increased speed limit based on safety analyses; second, the agency had to implement the change by physically swapping speed limit signs; third, informing the public about changes in the speed limit and the highways affected was necessary to ensure a safe transition before the July 4th holiday. Under the new legislation the agency had the authority to determine the stretches of highway where an increase in the speed limit would occur. The agency could have decided to raise the maximum speed limit on all highways but limited the change to rural stretches with controlled access and intersections because their analysis was based on a variety of technical and safety factors.

> We considered a number of factors, such as traffic volumes, crash history and roadway geometrics, to determine where to raise the limit. We will continue to monitor these routes under the new speed limit and consider whether it makes sense to increase the maximum speed on other highways, said KDOT'S Division of Planning and Development director Chris Herrick (K-DOT 2011).

This statement made it clear that the agency will continue to monitor the highways based on a variety of criteria leaving the possibility open for future increases in speed limits on other highway stretches if deemed appropriate.[1]

[1] Conversely, if agency monitoring deems it appropriate to discontinue the 75 mph speed limit on some highways, will KDOT revert to a lower speed limit?

Literature review:

Finding a positive relationship between fatalities and increased speeds

Previous literature examining the impact of changes in speed limits on highway fatalities has produced mixed results but is generally divided into two camps. The first group of research finds that increases in speed limits – usually from 55 to 65 mph – produces increases in the number of road fatalities, injuries, or total number of accidents. Koshal (1976) initially examined the capacity of highways in the US and found that introducing a 10 percent increase in the speed limit would increase the accident rate per motor vehicle by 4.5 percent. Using structural time-series modeling for all US states Balkin and Ord (2001) found that the speed limit increases in 1987 increased rural road fatalities in 19 states at statistically significant levels while the 1995 change slightly increased rural highway fatalities in 10 states but had no statistically significant impact on urban interstates. Ossiander and Cummings' (2002) Poisson regression model indicates that the "fatal crash rate on Washington State's rural interstates was 110% (95% CI 60-170%) higher after 1987, when the speed limit was raised to 65 mph, than it would have been if the speed limit had not been changed" (15). While Ossiander and Cummings found evidence for an increase in the fatality rate an increase in number of total crashes cannot be supported by their data. Much of the research showing increases in road fatalities associated with increases in the speed limit tends to not extend support to injuries, total number of injuries or frequency of crashes. Results from other studies dispute the positive relationship between increasing speed limits and fatalities.

Negative relationship between fatalities and increased speeds

A second group of authors finds decreased levels of fatalities, injuries, or total number of accidents following an increase in the speed limit. Examining the 40 states that raised the speed limit to 65 mph following the relaxation of the speed limit laws Lave and Elias (1997, 615) found that the fatality rate decreased 4.68 percent in 1987 and another 1.55 percent in 1988. States that did not adopt an increase in speed limits experienced no change in the fatality rate in 1987 while a 2.55 percent decrease generally occurred the following year. "The difference in fatality rates between the two groups of states indicates that the speed limit increase reduced the fatality rate by 3.62%" (ibid). All research done in the United States examines increases in the speed limit while examinations of speed limit decreases are usually undertaken abroad. Johansson (1996, 85-86) found that speed limit decreases reduced minor injuries and vehicle damages on Swedish roads while a reduction in severe injuries did not approach statistical significance ($p=0.17$). Even though Johansson found decreasing the speed limit resulted in fewer minor injuries this reduction was not significant for severe injuries or road fatalities. Increases in fatalities, injuries, and crash figures are sometimes associated with increased speed limits while the converse should be true as well; however, until a study – such as Johansson's – has been conducted in the United States this hypothesis is only supported by evidence from abroad.

No relationship between fatalities and increased speeds

A third group of research that tends to find that no relationship exists or the relationship between increases in the speed limit and fatalities, injuries, and total number of accidents is inconsistent. Rock (1995) examined rural highways in Illinois following the 1987 speed limit

relaxation. His "ARIMA model suggests that around 345 more accidents, 15 more deaths, and 150 more injuries occur monthly on rural Illinois highways due to the 65 mph speed limit" (212). However, Rock does not address the real statistical findings of his model; only increases in injuries following an increase in the speed limit achieved statistical significance. His results are therefore conflicting with regards to the cause of the increase in fatalities – which he acknowledges. Using panel reported data from all 50 states Michener and Tighe (1992) found evidence suggesting increased number of fatal accidents following increases in speed limits but the results did not achieve statistical significance (456). Yamane and Bradshaw (2008) examined the issue from a different angle by employing a case-based case-control design and found varying support for different categories of driver deaths.

> For non-collision driver death, the association [between three motor vehicle crash categories and driver death] was moderately strong, consistent, and significant. Interestingly, the association was stronger for women. For collision with motor vehicles in transit driver death, the association was somewhat milder but still consistent. For collision with stationary objects driver death, the direction of association was not totally consistent (1692-1693).

These results show that there was an association between some categories of crashes and driver death but the relationship was inconsistent across all groups of crashes. These results are consistent with Vernon et al.'s (2004) findings of increased speed limits resulting in varying impacts on fatalities, crashes, and injuries conditioned by road type: rural or urban interstate or high speed non-interstate. They conclude that "on both Interstate and non-Interstate highways, there was no change in the likelihood of a crash being associated with injury of fatality" (228). Results that are mixed or inconsistent appear to trend in certain studies. Mixed results within categories of the dependent variable or among the variables under examination will often produce statistical evidence that is hard to interpret as it supports conflicting hypotheses. It is

worth mentioning that the unit of analysis varies in almost all the referenced research and studies range from examining a single state and aggregating results from several or all U.S. states.

Other considerations and alternative measurements regarding levels of speed

The importance of investigating changes in speed limits and subsequent changes in fatalities, injuries, and accidents stems from the conventional wisdom that 'speed kills' (Rock 1995). Independent variables that may influence fatalities, injuries, and accidents could potentially include economic activity, highway design, vehicle design, safety features, seat belt usage, alcohol usage, and weather conditions (Calkins and Zlatoper 2001). Most interstates and roads where travelers enjoy high speed limits are divided, meaning the major impact on fatalities should stem from the variance in speed between travelers. Rodriguez (1990) positively associated speed variance with the fatality rate. Whatever the case may be – speed or variance – the argument remains that increasing the speed limit will significantly increase roadway fatalities, injuries, and the number of crashes. Previous evidence has produced varying degree of support for this conventional wisdom; while many US states have increased their speed limits several foreign states have decreased speed limits.

Rationales behind changes in the speed limit often originate with cost-benefit analyses. In their cost-benefit analysis Forester, NcNown, and Singell (1984) concludes that reducing the speed limit from 65 mph and adopt the 55 mph limit instead would save 7,466 lives per year on US highways. In reality such an analysis is practically impossible to implement currently as there are 50 different states in control of setting speed limits. Therefore, individual state's cost-benefits are expected to be substantially different than the national cost-benefit analysis for reducing the speed limit. For example, western states incorporate vast geographic areas and a reduction in the

legal speed limit would increase road travel time in many rural areas; decreasing gasoline consumption impacts local economies; and it remains to be seen if drivers actually comply with posted speed limits.

Why would any state increase the speed limit on their roads knowing that higher speeds tend to produce additional fatalities and injuries? Accident data from KDOT was obtained and will be analyzed, attempting to find an answer to this question. The goal is to analyze the effects of policy shifts, in this case increasing the speed limit on Kansas roads. Based on the results of this analysis guidance on the implications of raising the speed limit to 75mph is provided.

Research Design

Previous research has introduced interesting observations, findings, and conclusions about the impact of speed limits on road fatalities. From other studies several hypotheses have surfaced and I aim to test the following:

Hypothesis 1: Speed limits are positively related to levels of traffic fatalities.
Hypothesis 2: The 2011 speed limit increase in Kansas caused an increase in the number of fatal crashes.
Hypothesis 3: The 1996 speed limit increase in Kansas caused an increase in the number of fatal crashes.
Hypothesis 4: The 2011 speed limit increase in Kansas caused an increase in the number of injury crashes.
Hypothesis 5: The 1996 speed limit increase in Kansas caused an increase in the number of injury crashes.

While I have chosen to investigate the issue from the general theory found in the literature, there is statistical support for upward shifts in speed limit policies impacting road fatalities in both directions. This theory – that speed kills – is reflected in the above hypotheses and serves as the starting point for this study of road fatalities on Kansas highways in conjuncture with the relaxation of the speed limit in the summer of 2011.

Data

The data for this study is from the Kansas Department of Transportation; Geometric and Accident Data Unit with the Bureau of Transportation Planning. KDOT collects data on many types of road accidents and variables related to accidents. Weather, light conditions, time of day, stretch of road, type of crash, number of involved vehicles, and direction of travel to name a few. The dataset is available from KDOT per request and it is continually updated. Variables of interest are number of fatalities, number of injuries, and the speed limit and date where a crash occurred. The most recent data – *the 2011 dataset* – covered the period from January 1, 2010 – November 24, 2011, which include the 1 July 2011 speed limit increase from 70 to 75 mph on some Kansas highways and sections of the interstates. An older dataset – *1996 dataset* – covered January 1, 1993 – December 31, 1999 and include the 22 March 1996 speed limit increase from 65 to 70 mph. The intervention – representing the speed-limit increase is operationalized as a dummy variable. Including this variable provide results for how influential the speed limit increase was across the range of other variables. The speed limit variable – coded as the real value of the limit at the location of the accident – only provide results about the impact of the increase from accidents where the limit had been set at 75 mph. Gender is the only demographics variable available in the dataset provided by KDOT while information about the location of the accident and lighting conditions are available – representing road conditions and externalities respectively. Crashes occurring at interchange areas, in intersections, and at intersection-related areas were collapsed into the variable *accident at an intersection.* Non-intersection accidents were compiled from accidents occurring at medians, crossovers, and other non-intersection areas such as on a shoulder. On interstate accidents is a dummy variable coded 0 if an accident did not

occur on a piece of road designated as an interstate, and 1 if the accident occurred at any "I" labeled stretch of road.

Methods

This is a *within-comparison* where the main focus is death and injuries on Kansas roads before and after the speed limit was increased during the 2011 summer. I operationalized these variables by relying on the number of fatalities and accidents resulting in injuries reported by KDOT; these serve as the dependent variables in the main analysis. Balkin and Ord (2001) contest that using fatal crashes provide a more reliable guide to road safety conditions. Since the data is represented in regular intervals and the variables of interests are counts of fatalities, accidents, injuries, crashes etc., a Poisson-family count model is appropriate when examining the relationship between the variables and their impact on road fatalities. Several studies have previously employed Poisson count-models successfully (Breslow and Day 1987, Kuhn, Davidson and Durkin 1994) because this family of models is based on two assumptions; "i) the probability that an event occurs within a small interval of time, Δt, is given by $\lambda \Delta t$; and ii) occurrences in disjoint time intervals are independent events" (Michener and Tighe 1992, 452). A crash on the highway does occur in a relatively short time interval while there is no reason to believe that past accidents are related to future accidents. There has been a long history of applying this family of modeling to several types of fatality measures. For instance, this type of model dates back to 1898 when the Prussian Army modeled deaths related to mule-kicks (Michener and Tighe 1992) with the rationale of reducing the likelihood of soldier death; note that death as a result from interaction with the enemy was acceptable.

The 2011 dataset provided by KDOT consists of 1059 days of observations; 546 days of observation prior to the implementation of the speed limit policy shift and 513 days post-intervention. Between 1 January 2010 and 24 November 2012 a total of 129,652 reported crashes took place on Kansas roads. 19,275 were crashes involved injuries to vehicle occupants while 430 of the crashes resulted in a fatality. Prior to the implementation of increased speed limits the total number of crashes was 31,984, where 6,537 resulted in injuries and 293 were fatal. From 1 July 2011 until 24 November 2012 a total of 97,562 crashes were reported comprised of 12,718 injury crashes and 137 crashes resulting in at least one fatality. Before and after the policy change the maximum number of fatalities in a single day were four and three respectively. Maximum daily figures for injury crashes were 63 and 74 respectively. The maximum daily figures for total crashes before and after were 313 and 557 respectively.

Results from a Poisson regression modeling fatal-crashes

A Poisson count-model was employed when assessing the impact from increasing the speed limit on fatalities and injuries. The Poisson regression results (Imai, King and Lau 2007) for the 2011 speed limit increase indicate a lack of statistical significance. This finding provides some support for lawmakers arguing economics rather than safety, as the rationale for allowing KDOT to re-evaluate the speed limit on the state's divided four-lane roads. Increasing the speed limit to 75 mph on certain roads did not by itself increase the number of fatal accidents at levels significantly distinguishable from no influence. Similar to previous studies, the limit on the road where accidents occur is positively associated with the number of fatal accidents. The magnitude of the relationship is fairly small though. Also, this relationship is significant at a high level of probability.

Table 2:1 - Modeling fatal accidents for the 2011 speed limit increase.

Variable	*Coefficient*	*Std. Error*	*z-value*	*p-value*
Increase in the speed-limit	0.223	0.173	1.284	0.199
Speed-limit	0.070	0.008	9.259	0.000 ***
Number of vehicles involved	0.095	0.016	5.898	0.000 ***
Gender				
Female	-1.270	0.228	-5.568	0.000 ***
Male	-1.265	0.242	-5.227	0.000 ***
Location of accident				
At an intersection	1.446	0.292	-4.951	0.000 ***
Not at an intersection	-1.026	0.236	-4.343	0.000 ***
In a parking lot	-0.754	0.473	-1.593	0.111
On interstate	-1.261	0.163	-7.731	0.000 ***
Lighting condition(s)				
Dark and no street lights	-1.540	0.717	-2.148	0.032 *
Dark but street lights were on	-1.298	0.737	-1.762	0.078 ·
Dawn	-1.991	0.783	-2.543	0.011 *
Daylight	-1.068	0.711	-1.501	0.133
Dusk	-1.261	0.782	-1.611	0.107
Constant	-5.342	0.891	-5.993	0.000 ***

**** pr ≤ 0.000, ** pr ≤ 0.001, * pr ≤ 0.05, · pr ≤ 0.1; N = 31,179; Psuedo R^2 = 0.0331*

Gender is the only demographics variable available for analysis and provide an interesting finding. Both male and female vehicle occupants are negatively associated with a fatal crash; however, one has to compare the coefficients when interpreting this result. The relationship between female occupants and fatality as the outcome of an accident is negative with a 1.270 coefficient. Compared to men, women enjoy a smaller chance of becoming a fatality when involved in vehicle crashes. A variety of explanations could support this finding; overall,

women drive less than men, females take shorter trips when driving, and women are not speed demons. Whatever the reason for this relationship female occupants are less likely to become fatalities when involved in an accident on Kansas roads.

Examining results for road conditions and externalities – Table 2:1 – important information is revealed about what conditions affect crashes in a fatal direction. Crashes occurring on the interstate or at non-intersections are negatively related to a fatal outcome. On the interstate road conditions are favorable for safety; the design of four-lane divided interstates reduces the chance of head-on collision. Crashes between vehicles traveling in the same direction but at different speed do not tend to result in a fatal outcome. The same holds for accidents at non-intersections for similar reasons. This variable captures accidents occurring on regular roads where travel predominantly flows in the same direction. At lower speeds a fatal outcome is much less likely when involved in a collision with another vehicle or stationary object. Speed tends to be low in parking lots as well and the results in Table 2:1 supports this negative relationship. Three variables show a positive relationship to fatalities at statistically significant levels; the speed limit, the number of vehicles involved, and whether or not the accident occurred at an intersection.

Results for the 1996 increase in the speed limit provide similar results for the model and are based on a higher number of observations. When the speed limit in Kansas was raised from 65 to 70 mph the number of fatal accidents on the interstate decreased. Table 2:2 depicts the relationship between fatal accidents and the variables for the 1996 increased speed limit. Four variables achieved significance, two are positively- and two are negatively related to the number of fatal accidents. The increase in speed limits seems to have lowered the overall number of fatal accidents on Kansas roads.

Table 2:2 - Modeling fatal accidents for the 1996 speed limit increase.

Variable	*Coefficient*	*Std. Error*	*z-value*	*p-value*
Increase in the speed-limit	-0.541	0.062	-8.723	0.000 ***
Speed-limit	0.092	0.004	25.039	0.000 ***
Number of vehicles involved	0.450	0.023	19.592	0.000 ***
Gender				
Female	-0.040	0.092	-0.428	0.669
Male	-0.086	0.105	-0.815	0.415
Location of accident				
At an intersection	11.403	153.384	0.074	0.941
Not at an intersection	11.439	153.384	0.075	0.941
In a parking lot	11.557	153.374	0.075	0.940
On interstate	-1.241	0.074	-16.755	0.000 ***
Lighting condition(s)				
Dark and no street lights	0.092	0.339	0.271	0.786
Dark but street lights were on	0.105	0.341	0.306	0.759
Dawn	-0.077	0.369	-0.208	0.835
Daylight	0.068	0.335	0.203	0.839
Dusk	0.133	0.362	0.366	0.714
Constant	-21.514	153.385	-0.140	0.888

**** $pr \leq 0.000$, ** $pr \leq 0.001$, * $pr \leq 0.05$, · $pr \leq 0.1$; N = 144,142; Psuedo $R^2 = 0.0582$*

Accidents on the interstate are also negatively related with fatalities showing that increasing the speed limit from 65 to 70 mph did not result in a greater number of fatal accidents on the interstates throughout Kansas. Contrary to the 2011 increase in the speed limit, the 1996 shift applied to all appropriate roads in the state. The results in Table 2:2 could be explained as a *redirection of traffic*.

Table 2:3 - Modeling injury accidents for the 2011 speed limit increase.

Variable	*Coefficient*	*Std. Error*	*z-value*	*p-value*
Increase in the speed-limit	0.141	0.039	3.636	0.000 ***
Speed-limit	0.011	0.001	9.423	0.000 ***
Number of vehicles involved	0.070	0.005	13.661	0.000 ***
Gender				
Female	-0.031	0.042	-7.429	0.000 ***
Male	-0.529	0.050	-10.603	0.000 ***
Location of accident				
At an intersection	-0.162	0.074	-2.198	0.028 *
Not at an intersection	-0.360	0.071	-5.090	0.000 ***
In a parking lot	-0.057	0.105	-0.541	0.588
On interstate	0.060	0.031	1.928	0.054 ·
Lighting condition(s)				
Dark but street lights were on	0.704	0.051	13.918	0.000 ***
Dawn	-0.112	0.090	-1.253	0.210
Daylight	0.656	0.040	16.393	0.000 ***
Dusk	0.414	0.091	4.540	0.000 ***
Constant	-2.244	0.106	-21.187	0.000 ***

**** pr ≤ 0.000, ** pr ≤ 0.001, * pr ≤ 0.05, · pr ≤ 0.1; N = 31,179; Psuedo R^2 = 0.0164*

Drivers were able to travel on all roads at increased speeds, not only interstates, so the increase could have redirected traffic flow away from the interstates in favor of more convenient non-interstate roads. On the other hand, the speed limit and number of vehicles involved are still positively and significantly related to the number of fatal accidents.

Results from a Poisson regression modeling injury-crashes

Applying the same model as for fatal accidents to accidents where the outcome is injuries provide similar results but with notable differences. A main difference is the significant and positive relationship between the increase in speed limit and the number of injury accidents. Interstate travel was negatively related to fatalities – from Table 2:1 and Table 2:2 – and the increase in speed limits only applied to divided, four-lane roads. An increase in injury accidents may result from designed safety features incorporated into the road preventing fatalities by instead increasing injuries. These results provide evidence that the planning and subsequently engineering of roads and highway successfully emphasize certain safety elements required by law (SAFETEA-LU 2005). Combining the findings of a positive relationship between interstate travel and injury accidents – albeit at lower levels of significance – fairly strong evidence exists that safety planning has been successfully incorporated into highway designs. When considering externalities such as lighting conditions all variables that are significant and show a positive relationship with the number of injuries.

The 1996 speed limit increase's impact on the number of injury accidents – Table 2:4 – is similar to the results following the 2011 policy shift – Table 2:3. The notable exception would be that in 1996 traffic shifted towards more interstate travel resulting in greater numbers of injury crashes on highways while the overall level of injuries declined. Following the 1996 shift in the maximum speed limit data showed an increase in the number of injuries sustained from crashes on the interstates but a decrease in the level of fatalities. The shift itself was negatively associated with both fatal and injury accidents at significant levels. The results from the 2011 increase are less consistent, possibly due to fewer observations.

Table 2:4 - Modeling injury accidents for the 1996 speed limit increase.

Variable	*Coefficient*	*Std. Error*	*z-value*	*p-value*
Increase in the speed-limit	-0.104	0.009	-10.930	0.000 ***
Speed-limit	0.003	0.000	7.825	0.000 ***
Number of vehicles involved	0.335	0.005	61.559	0.000 ***
Gender				
Female	-0.004	0.016	-0.234	0.815
Male	-0.016	0.018	-0.879	0.379
Location of accident				
At an intersection	0.108	0.152	0.709	0.478
Not at an intersection	0.112	0.151	0.742	0.458
In a parking lot	0.110	0.153	0.718	0.473
On interstate	0.029	0.012	2.384	0.017 *
Lighting condition(s)				
Dark and no street lights	-0.046	0.056	-0.828	0.408
Dark but street lights were on	-0.030	0.056	-0.537	0.592
Dawn	-0.056	0.060	-0.919	0.358
Daylight	-0.001	0.055	-0.021	0.983
Dusk	-0.113	0.061	-1.861	0.063 ·
Constant	-2.065	0.159	-12.981	0.000 ***

**** pr ≤ 0.000, ** pr ≤ 0.001, * pr ≤ 0.05, · pr ≤ 0.1; N = 144.142; Psuedo R^2 = 0.0131*

The results of these two Poisson models points in the direction that the increase in allowed travel speed did not result in increased fatality figures. However, the shift is positively related to injury accidents at the conventional level of statistical significance (p=0.05) for the most recent policy change but not for the first shift. These diverging results indicate that specific stretches of road have an impact on the observed levels of serious injuries. Thus, this study has uncovered clear implications for policy makers.

Discussion and Implications

What should lawmakers take away from this study on their decision to devolve authority to KDOT? We know that politicians often devolve authority to a government agency in order to show voters, critics, and supporters that actions have been taken to solve a perceived problem (Fiorina, et al. 2010). Allowing an agency to re-evaluate the feasibility of increasing the speed limit on certain roads insulate politicians from potential harm if the 'experiment' does not produce a desirable outcome. Politicians could redirect blame to the agency responsible for implementing the law if the shift in speed limits resulted in significant increased fatality and injury figures (Bourdeaux 2005, Heiman 1992). Political and economic costs have so far been small and potential economic benefits to the state, businesses, and the overall economy remain unrealized. The argument is that the incremental increase in the speed limit has been a rational political act. Low costs and potential yet unrealized economic benefits are associated with the political authorization devolving authority to KDOT for identifying roads suitable for speed limit increases. However, in this study I have not been able to examine the impact of the shift in policy regarding speed limits on economic activity, state tax income, or business revenues. This limitation presents an avenue for further research, preferably after tax reports have been submitted to the state.

Conclusion

Marvin Kleeb – the main driving force behind the legislation authorizing KDOT to re-evaluate the maximum speed limit on Kansas roads – argued that increasing the speed limit would make Kansas more attractive to the trucking industry by allowing faster transit through the

state east-west or north-south. He did not cite research supporting his claim. Opponents of Kleeb's legislation argued that safety would be at risk if KDOT was allowed to raise the maximum speed limit. In this study I have presented supporting evidence that there has been no significant impact on the number of fatal crashes by increasing the speed limit to 75 mph on 807 miles of Kansas roads. The previous increase in 1996 also supported a negative relationship between increasing the speed limit and the number of fatal crashes on Kansas roads. However, a positive relationship between the intervention and injury accidents appeared significant at a high level of probability. Vehicles traveling in the same direction on divided roads are at reduced likelihood of crashing with oncoming traffic. The policy shift did not have an effect on the number of fatal accidents on Kansas roads but was positively related to the number of accidents resulting in injuries to vehicle occupants.

Examining the 2011 speed limit increase on Kansas roads has shed light on the preliminary effect of this policy change. Results indicate that speed kills. This finding is consistent across all the models examined although at varying levels of significance. I confidently conclude that hypothesis 1 – speed has a positive impact on levels of fatalities – is supported by empirical evidence. Hypothesis 2 – the 2011 increase produced more fatalities – is not supported. A similar policy shift in 1996 is negatively associated with the number of fatal accidents (hypothesis 3). The major finding is inconclusive. Increasing the speed limit in 2011 may have increased the number of fatal crashes but the results do not achieve statistical significance. As for the research questions; does speed kill? The answer is a resounding yes! Speed shows up repeatedly as a significant predictor of traffic fatalities. What is the effect of increased speed limits on traffic fatalities? The results for the 1996 speed limit increase are actually negatively related to the number of fatal accidents. However, the results from the 2011

speed limit increase are inconclusive. I cannot confidently reject the null hypothesis based on this dataset.

Chapter 3 - Bureaucratic Discretion: Local LEOs' Response to State Alterations in Enforcement Parameters

Introduction

Much of previous research on bureaucratic discretion and policing reveals a tendency to focus on specific sections of the relationship between clients and street-level bureaucrats. Research examining influences external to this relationship has been limited in scope and often focuses on the organizational structure of an agency. Findings indicate that rules, standards, and formalized cultures decrease an agents' flexibility when interacting with citizens. Little of this research has specifically dealt with police discretion following state intervention in a policy area enforced by local governments. Further, research on intergovernmental relations and the effect of federalism on policy enforcement at the municipal level is mostly limited to federal directives and Congressional legislation. Therefore, this chapter is organized around providing an answer to the following question: are *local* enforcement officers responsive to policy shifts made at the *state* level?

Previous research has not provided proper attention to the impact of state level legislation on enforcement efforts at the local level or this relationship has been found to be insignificant. My project contributes to the literature in two ways; I examine legislative alterations at the state level and determine how municipal street-level bureaucrats are responsive to this adjustment by their principals; and relying on bureaucratic discretion allow police officers to subsequently change enforcement levels of the altered policy. This type of examination is significant as it speaks to issues of fairness in and effectiveness of municipal service delivery and since

bureaucratic accountability is illustrated by the responsiveness of street-level agents to the fluctuating wishes of state-level principals.

Background

"Wearing a seat belt can make the difference between life and death in a crash, so always buckle up on every trip, every time… seat belts save lives, but they only work if you wear them" said U.S. Transportation Secretary Ray LaHood (Aldana 2010). Injuries and fatalities from traffic accidents are a terrible strain on society; in Kansas, 76 percent of those who die in a traffic accident at night are not wearing seat-belts and 57 percent who die from a traffic accident during the day are not wearing seat-belts (The Capital-Journal 2012). In May 2010 Democratic Governor Mark Parkinson signed the bill into law upgrading enforcement of the mandatory seat-belt law to primary-enforcement; violations would be punished with $5 fines for the first year followed by a doubling the next year to $10 (Koranda 2010). "This bill creates a "primary" seat belt law, meaning that a law enforcement officer can stop a car when the officer believes someone in the car may not be wearing a seat belt" (Weeks 2010).

Enforcement of seat-belt compliance most often resides with municipal governments who collect fines to fund continued provision of public safety services. How do municipal governments respond when the state shifts policies, effectively increase the amount of discretion afforded to local law enforcement officers (LEOs)? Subsequently, how do local governments respond when the state limits the monetary size of fines municipal governments issue? This chapter examines citations issued for violating Kansas' mandatory seat-belt law before and after the shift to primary enforcement. First, I present the concept of bureaucratic discretion and its' connection to seat-belt enforcement; second, a brief literature review of relevant research; third,

data and models along with results are presented; fourth, the implications of primary enforcement and state limits on citations are discussed, followed by a conclusion that agents of local governments use discretion to respond to state imposed shifts on enforcement efforts.

Bureaucratic discretion

The ability of street-level bureaucrats to rely on some sort of professional judgment when interacting with citizens clearly impacts the level and quality of government services clients receive. Examining local response to state imposed limit on seat-belt fines I rely on an established understanding of discretion where "police are required… to decide overtly how much of an effort is to be made to enforce specific laws. It recognizes that actions short of arrest may achieve the desired goal" (Goldstein 1963, 140). This interpretation of discretion argues that police officers have to base decisions on professional standards regarding specific subfields of enforcement, such as enforcement of traffic policies. Factors that influence the individual officers' decision-making process compose a critical element of how discretion is exercised with regard to lawbreakers. In this paper I argue that local law enforcement officers (LEOs) will alter their use of discretion in predictable directions when the state changes local enforcement parameters. Officers' enforcement efforts are made 'easier' with the passage of primary seatbelt enforcement, and should subsequently increase enforcement of this ordinance. Imposing a nominal fee limit on non-compliance should decrease enforcement efforts, as arbitrarily low fines do not even cover the cost of issuing them.

Studies examining bureaucratic discretion often focus on the relationship between street-level bureaucrats and their clients that is determined by individual characteristics of either actor. Some research has expanded on this concept and included organizational factors that may

influence bureaucrats to act in a certain direction. Organizational rules and standards limit the level of discretion individual bureaucrats are able to exercise and at times these rules and standards ensure fairness and equal treatment of clients. However, it is possible that a rigid organizational structure discriminate against certain individuals or groups of people because they lack specific documentation or other necessities required for benefits. Therefore, allowing for the use of discretion could potentially lead to increased fairness and equal distribution of government benefits. In turn, the use of discretion effectively established certain public policies (Lipsky 2010).

Bureaucratic activities tend to receive the least amount of public or media attention unless their activities violate some form of law or social standard. Actions undertaken by the legislative branch dominate the political landscape (Heclo 1994) while unnoticeable bureaucrats determine "who gets what, when, and how" (Lasswell 1936). Bureaucrats are often charged with implementing policies that have successfully navigated a long political obstacle course. Much attention has been awarded to studying the legislative path while comparatively few studies in political science have examined the effect of discretion in implementing these policies. As part of dealing with the uncertainty of street-level bureaucratic activities, agencies develop rules that individual agents then apply in specified situations carrying out policies enacted by legislators (Lowi 1969). Bureaucrats effectively rely on their discretion by emphasizing certain elements when implementing policies, an inescapable feature of public life (Hawkins 1992), when providing citizens with goods and services (Goodsell 1981).

No study examining the impact of bureaucratic discretion on policy implementation is complete without paying credit to Lipsky's (1980) seminal work; "Street Level Bureaucracy." According to Lipsky, discretion allows government agents room to maneuver. "At best, street-

level bureaucrats invent benign modes of mass processing that more or less permit them to deal with the public fairly, appropriately and successfully. At worst, they give in to favoritism, stereotyping, and routinizing—all of which serve private or agency purposes" (Lipsky 1980, xii). The 'continuation' literature has further emphasized the role of bureaucrats in policy implementation: "Indeed those responsible for local authority policy formation have usually been practitioners themselves and are well aware that procedures will be ignored or interpreted by practitioners who in so doing will themselves formulate policy" (Preston-Shoot 2001, 9). The central thesis of Lipsky's work has been that policy in effect becomes what street-level bureaucrats' make of it. Perhaps even more important, the policy distortions developed at the street level are often tacitly accepted by managers as solutions to 'getting the job done' when faced with real world problems (Lipsky 1980, 18).

By exercising discretion bureaucrats are able to make an impact on the implementation of policy. Several researchers have endorsed a positive attitude towards discretion as a tool for more equitable outcomes, allocation of resources where they will do most good, and addressing special needs of clients (B. D. Jones 1978, Nivola 1978, Kroeger 1975). Other scholars have feared the negative aspects of discretion, a loss of equity, where clients may be denied access to benefits, e.g. 'bureaucratic disentitlement' (Lipsky 1984, 4-5). Bureaucrats may also discriminate against a group of clients due to political pressures or moral reasons (Hasenfeld 1987). Further, bureaucratic discretion could be used to undermine the intention of the legislation authorizing a policy, thereby working contrary to the wishes of policymakers (Keiser og Soss 1998). Movements towards devolution of power from the federal government in the 1990s (Lieberman and Lapinski 2001) have made the need for general theories of administrative behavior apparent. "Students of public policy require empirically grounded theories that can explain why discretion

in the administrative process produces particular outcomes in some circumstances but not in others" (Keiser og Soss 1998, 1134). However, public administration scholars might have a firmer theoretical grip on bureaucratic discretion's impact on policy outcomes. Implementation studies are located between political science and public administration, two fields that tend to be 'polarized' on this topic (Schofield 2001, 245).

Scholars have identified a range of internal factors influencing the use of discretion and ultimately the direction of policy implementation. Internal factors include agency rules and values and the amount of available resources for consumption and distribution to clients. Street-level agents may not be familiar with the myriad of organizational rules thereby failing to implement them (Brehm and Hamilton 1996). Rules are sometime hard to follow due to an agency's contradictory goals (J. Q. Wilson 1989). Also, bureaucrats show a tendency to 'pick and choose' what rules to follow when faced with heavy workloads and time constraints (Lipsky 1980). Further, limited resources make a distributive decision unattractive for bureaucrats who may rely on discretion to avoid having to choose among a range of equally 'needy' clients (Evans og Harris 2004). Similarly, bureaucrats could hide behind discretion in order to protect themselves from blame (Parton 2001).

External factors also influence the use of bureaucratic discretion. Pressure from the political environment agents operate in exemplifies external influences on discretion (Keiser 2001). Similarly, local community groups also influence the use of discretion as they are able to petition on behalf of clients (Soss 2000). Research has also been carried out examining client characteristics as a factor influencing policy benefits. Keiser, Mueser and Choi (2004) found that client race and local racial makeup negatively influence the amount of welfare benefits distributed in Missouri.

Contrary to the type of research addressed above, some scholars argue that bureaucratic discretion is non-existent or marginally influential on policy implementation. This 'curtailment' literature focuses on the success of managerial control and worker compliance (Howe 1991). Studies focusing on managerial tactics of curbing the use of discretion argue that street-level bureaucrats have become more compliant as such tactics have become institutionalized as mechanisms of control (Langan 2000, Lymbery 2000, Clarke and Newman 1997). Examining social workers Jones (1999) argues that "social work has been transformed from a self-regulating professional activity into a managed and externally regulated set of tasks" (38). This transformation of the bureaucracy has created a "much more mundane and routinized relationship with clients which could not be described as social work" (C. Jones 2001, 552). However, managerial control of bureaucratic discretion could be rather limited.

Although these scholars have attempted to downplay the role of discretion in policy implementation a vast amount of studies and evidence suggests otherwise. "The proliferation of rules and regulations should not automatically be equated with greater control over professional discretion; paradoxically, more rules may create more discretion" (Evans and Harris 2004, 873). Bureaucrats rely on discretion to complete their tasks in situations where they face uncertainty while implement vaguely phrased laws and policies (Handler 1973). The literature on bureaucratic discretion indicates a hypothetical if not measurable influence on the implementation process.

Research on Seatbelt Laws and Discretion

Previous studies have determined that compliance with seatbelt ordinances reduce the number of traffic fatalities. At the national level (50 states and the District of Columbia) a one

percentage increase in the seat-belt usage rate is estimated to save 136 lives; extending this relationship implies that increasing usage to 90 percent would save an estimated 1500-3000 lives annually (Cohen and Einav 2003, 829). Researchers in Canada found a 17-21 percent drop in vehicle-occupant fatalities following the enactment of mandatory seat-belt laws there (Sen and Mizzen 2007). Since a significant relationship exists between compliance and fatalities, how are states able to reduce traffic deaths?

Much research has been carried out with regards to the effectiveness of primary enforcement of mandatory seatbelt laws. Researchers have found that in states with mandated primary enforcement of seatbelt policies compliance tends to be higher (Shults, et al. 2004, Dinh-Zarr, et al. 2001). Cohen and Einav (2003, 829) found that "whereas a mandatory seat belt law with secondary enforcement increases the usage rate by about 11 percentage points, a mandatory seat belt law backed by primary enforcement increases usage by about 22 percentage points." These rates are consistent with the findings of Mujumdar, Noland, and Ochieng (2004) who found secondary enforcement to increase seat-belt usage by 9 percent and 16 percent under secondary and primary enforcement, while Dee (1998) found usage rates increased 17 and 26 percentage points respectively. Thus, the type of enforcement has a substantial effect on seat-belt compliance rates. "Switching from secondary to primary enforcement increases the [seat-belt] usage by about 13 percentage points" (Cohen and Einav 2003, 839). Therefore, the federal government has urged states with only secondary enforcement laws to upgrade to primary enforcement of seat-belt laws (NHTSA 2004, 1995). Under primary enforcement of seat-belt laws the certainty of punishment is greater than under secondary enforcement due to police officers' ability to issue citations for non-compliance without observing any other infraction (Houston and Richardson 2006). Lawmakers send a signal to enforcement officers to prioritize

seat-belt violations when upgrading to primary enforcement laws that may result in police officers issuing an increased number of citations (Russell, Dreyfuss and Cosgrove 1999). Consequently, higher levels of compliance should result in fewer traffic fatalities.

Primary enforcement of seatbelt laws reduces fatality figures through the mechanism of increasing compliance. Few studies actually consider the impact primary enforcement of seat-belt laws has on the number of fatalities (Houston and Richardson 2006). By examining 9 states and the District of Columbia that upgraded to primary enforcement in the 1989-2003 period and comparing the results to 14 states with secondary enforcement Farmer and Williams (2005) found that such a shift reduced driver fatality rates by 7 percent. Both primary and secondary enforcement of seat-belt laws experience a negative relationship with the levels of road fatalities, although secondary enforcement reduces fatalities at roughly half the rate of primary enforcement (Houston and Richardson 2004, 652) or even less than half as effective in some studies (Dee 2001, 120). Regardless of the type of enforcement, the bottom line is that several researchers have found lives are being saved by seat-belts (Legge 1990, L. Evans 1986, Wagenaar, Maybee and Sullivan 1988). Lives are saved through seat-belt usage and compliance is "virtually certain to increase actual usage rates… higher usage, in turn, should imply increased highway safety, as reflected in such measures as fatal and nonfatal injury rates" (Asch, et al. 1991). Increased compliance should lead to safer roads and subsequently reduce fatal and serious injury accidents.

Expectations

Previous literature on bureaucratic discretion and the concern of local reaction to state intervention naturally developed the following expectations. First, by stepping up to primary

enforcement the number of citations should subsequently increase. Allowing officers to stop vehicle operators due to non-compliance with the law provides an additional tool for enforcement compared to secondary enforcement where violations could only be ticketed when stopped for other infractions that vehicle occupants were also ticketed for. Second, primary enforcement allows for increased emphasis on enforcement of seat-belt violations. If failure to wear safety-belt is the only infraction observed officers could not issue tickets for this behavior before the upgrade to primary enforcement. Currently, officers are able to issue citations for non-compliance of the seat-belt law regardless of the presence of other undesirable behavior. Third, a reasonable expectation exists indicating that a reduction in the number of tickets issued by local law enforcement officers to vehicle occupants for failing to wear their safety-belts will follow the state imposed $10 fine ceiling. State actions do not direct local enforcement efforts in any capacity other than restricting the amount cities levy against non-compliance of the seat-belt laws. Finally, from the discretion literature I hypothesize that police officers use discretion to decrease the proportion of time spent on enforcing seat-belt violations following the $10 maximum fine. The state's decision to nominally limit this fine indicates – to law enforcement officers – that enforcement of this undesirable behavior suffers from low political priority at the state level. City level data on the number of seat-belt tickets was obtained from five cities in the state of Kansas to test these expectations.

Research Design and Data

In order to operationalize variables of concern for analyzing bureaucratic discretion – specifically the level of police discretion following the change in Kansas seat-belt laws – municipal data on the number of citations issued was collected. At times I worked with city

police departments while other cities provided these records through their court administration or city attorney's office. These cities included a variety of citation types issued by local law enforcement officers within their jurisdictions for 2009 – 2013. For *Newton* the court administrator – Greg Nickel – enthusiastically provided data on citations issued from 1/1/2009 – 4/24/2013 including seat-belt tickets that was later used to calculated the dependent variable(s). Lieutenant Jim Tilton provided monthly citation data for *Emporia* from 2009 – 2012. Dave Wilson from the *Lenexa* Police Department kindly provided daily ticked data from 1/4/2010 – 4/7/2013. The county provides policing services for the city of *Manhattan* – where Kansas State University is located – and the records management system supervisor for Riley County Police Department – Kari Breault – provided data on number of citations issued within city limits during the period 1/1/2002 – 3/31/2013. The city attorney for *Leawood* – Patricia Bennet – provided a nicely organized dataset with citations for the 1/1/2009 – 12/31/2012 period. Figures from these five cities were combined into an aggregated monthly dataset ranging from the beginning of 2009 to the end of 2012.

Variables and Measurement

A number of variables were collected for the purpose of controlling for influences on the dependent variable found in previous studies. The Kansas Bureau of Investigation (KBI) provides information about the number of police officers working for city governments with the exception of Riley County that provides public safety services for Manhattan, KS. Also, KBI provides sheriff employment figures for respective counties; these figures were included to account for all government agents with jurisdictions enabling them to issue tickets within city limits. The gender of the government agent was accounted for both police officers and sheriffs

and provides an important component of the model while addressing the findings of studies focusing on the individual characteristics of street-level bureaucrats. Agents of the state – state troopers and KBI agents – with authority to issue tickets were excluded as they rarely enforce these laws within city limits. Figures for city population in addition to data from the Crime Index statistics at the state level were included in the final dataset to account for any variation in the states' general crime pattern/trend. Finally, there is a vast literature on the influence of institutional choice on performance of city services; information about the type of local government was provided by each city where two cities (Leawood and Lenexa) are led by mayor-councils and three (Manhattan, Emporia, and Newton) employ city managers to administer daily municipal operations. The dependent variables – seat-belt citations – is a count variable and the ratio of these tickets to all other tickets issued within city limits provides the operationalized second dependent variable, for the level of police discretion. Upgrading to primary enforcement of seat-belt laws in June 2010 Kansas became the 31st state to allow this type of enforcement and is represented in the dataset as a dummy variable. The unit of analysis in this dataset is city-months and all months after the enactment of primary enforcement were coded 1 while all months prior to enactment were coded 0. The same logic applied to the passage of a $10 ceiling by state legislators for not wearing seat-belts. Prior to this ceiling all observations were coded 0 while post-passage observations were coded 1.

Methods

By combining the various dependent and independent/control variables in a single dataset I created a multivariate model of changes in police discretion following the upgrade to primary seatbelt enforcement and the states passage of the $10 fine limit on these violations. It is

important to realize that the passage of a fine ceiling for lack of seat-belt usage did not change the legal interpretation of this behavior nor did it affect the stated level of enforcement desired by state law-makers. Police officers and sheriffs were not directed to alter their level of enforcement of seat-belt violations in any capacity; lawmakers simply limited the nominal fine violators would pay when ticketed. Supporters of the bill claimed it would create legal uniformity across the state and thus, ultimately increase seat-belt usage (Associated Press 2010). However, the ceiling could potentially alter the cost-benefit of enforcement at the local level as revenues for municipal government were restricted while costs of enforcement remained unchanged. Several municipal officials publicly criticized the fine ceiling on the basis that $10 would not cover the cost of paying an officer to issue the citation nor pay for the cost of processing the ticket through local courts (Cooper 2011). I examine the impact of the 2010 state seat-belt enforcement law on the number of issued tickets within the 5 city sample and find no significant relationship between the fine change and the number of tickets issued by local law enforcement officers.

Models

The data on number of seat-belt tickets issued by law enforcement officers within city limits qualify to be modeled as count data. Research modeling independent event data – such as the number of seat-belt citations – has often relied on count models. Much research on traffic fatalities has successfully been based on the family of Poisson count-models (McCullagh and Nelder 1989, Frome 1983) due to its simplicity. However, when there is evidence of over-dispersion in the data negative binomial distributions are favored due to its inherent capabilities of accounting for over-dispersed data (Noland 2003).

> Modeling count variables is a common task in economics and the social sciences. The classical Poisson regression model for count data is often of limited use in these disciplines because empirical count data sets typically exhibit over dispersion and/or an excess number of zeros. The former issue can be addressed by extending the plain Poisson regression model in various directions: e.g., using sandwich covariances or estimating an additional dispersion parameter (in a so-called quasi-Poisson model). Another more formal way is to use a negative binomial (NB) regression (Zeileis, Kleiber and Jackman 2008, 1).

Examining the dataset revealed considerable over-dispersion indicating that a negative binomial model should be employed to correctly account for the over-dispersed distribution of seat-belt tickets. With the statistical program R the Zelig package was used to analyze the city level data through a negative binomial model (Imai, King and Lau 2007). Modeling count data with negative binomial estimation provides easy and straight forward interpretation of the regression analysis; the coefficient calculated for each variable signifies a real increase in the dependent variable qualified by the unit of analysis. Table 3:1 indicates a relationship between the number of seat-belt tickets and the number of female law enforcement officers that is statistically different from no-relationship; meaning that for each additional female officer in the city the number of tickets issued for seat-belt violations increased by 0.817 per city-month (the unit of analysis). In this sense, the coefficient represents a real and tangible relationship between the dependent and independent variable(s).

To properly investigate the level of police discretion, following the state imposed fine ceiling for a driver failing to wear safety belt(s), a ratio of these tickets compared to all other tickets was calculated. This ratio is able to give insight into how law enforcement officers decide to spend their time while on duty, i.e. the level of attention officers give to enforce a certain illegal activity. Note that this ratio does not include down-time such as time spent traveling to the scene of a crime, filing reports, or appearing in court. I assume this amount of an officer's time to remain fairly constant over time and alterations in state law should have no impact on these

non-enforcement activities. Seasonal variation could potentially prove a major problem for this analysis and is accounted for by the sequence of observations before and after the state upgraded to primary enforcement. Conveniently, state law-makers put the law into action mid-summer 2010 roughly falling in the middle of the seasonal cycle providing a roughly equal amount of observations for the before-and after analysis for that year. Further, monthly observations for the entire 2009 and 2011 provide additional weight for the periods being analyzed and average any seasonal variation throughout a given year. In essence, the observations represent roughly 18 months of data before and after the upgrade to primary enforcement of seat-belt laws. Similar circumstances were present when the $10 fine ceiling went into effect mid-summer 2011; sufficient observations before and after – the time point when this law became active – provide confidence for the claim that any seasonal variation has been accounted for in the analysis. I rely on a beta-distribution model; calculating the ratio of seat-belt tickets to the total number of tickets issued would operationally represent police discretion. The results provide statistical support for my argument that the level of discretion local law enforcement officer's use when determining to issue tickets for seat-belt violation has been impacted when state legislators upgraded to primary enforcement, and later imposed a ceiling on the fine for non-compliance.

Results and Analysis

Examining the relationship between the primary enforcement of seat-belt violations and later the state imposed fine ceiling to the number of citations issued, provided interesting results with regards to the level of police discretion. First, examining this relationship with a negative binomial count model shows that upgrading to primary enforcement of seat-belt laws enjoys a positive and significant relationship with the number of tickets issued by police for non-

compliance with seatbelt ordinances. The model and results are depicted in Table 3:1. The negative binomial regression results indicate that approximately one additional ticket for seat-belt violations is issued per city-month; meaning that for the five cities examined, roughly 65 additional tickets would be issued every year for upgrading to primary enforcement of mandatory seatbelt laws in Kansas.

Table 3:1 – Count modeling issued seat-belt citations.

Variable	*Coefficient*	*Std. Error*	*z-value*	*p-value*
Mayor-Commission	54.200	14.253	3.803	0.000 ***
City Population	-0.000	0.000	-2.404	0.016 *
State Index Crimes	0.000	0.000	3.612	0.000***
Interventions				
$10 seat-belt tickets	-0.356	0.273	-1.339	0.180
Primary enforcement	1.090	0.237	4.602	0.000 ***
Law Enforcement Officers				
Male LEOs	0.031	0.073	0.426	0.670
Female LEOs	0.817	0.215	3.793	0.000 ***
Male Sheriffs	0.024	0.030	0.790	0.429
Female Sheriffs	-0.896	0.306	-2.925	0.003 **
Constant	3.695	0.876	4.216	0.000 ***

**** pr ≤ 0.000, ** pr ≤ 0.001, * pr ≤ 0.05, · pr ≤ 0.1; N = 132; Pseudo R^2 = 0.07.*

Second, the results provide support for the characteristics of individual bureaucrats influencing the number of tickets issued. The number of female law enforcement officers and female sheriffs with jurisdiction over a city are both significantly related to the number of tickets issued at a similar magnitude albeit in opposite directions. Each additional female police officer

issue roughly ten more tickets during a work year than their male counterparts, providing some support for the impact of differences among individual bureaucrats on the dependent variable. Interestingly enough, the presence of female sheriffs results in a negative relationship to tickets issued. This could stem from the fact that the actual number of female sheriffs is very low at 15 percent of the total labor force. The highest number of female sheriffs in the dataset occurred during 2012 when 86 female sheriffs were patrolling the cities examined here while there were 549 male sheriffs at the most during this period. As an explanation, perhaps female sheriffs were ordered to patrol remote areas of the county by their supervisors or predominantly issued tickets for seat-belt violations outside city limits.

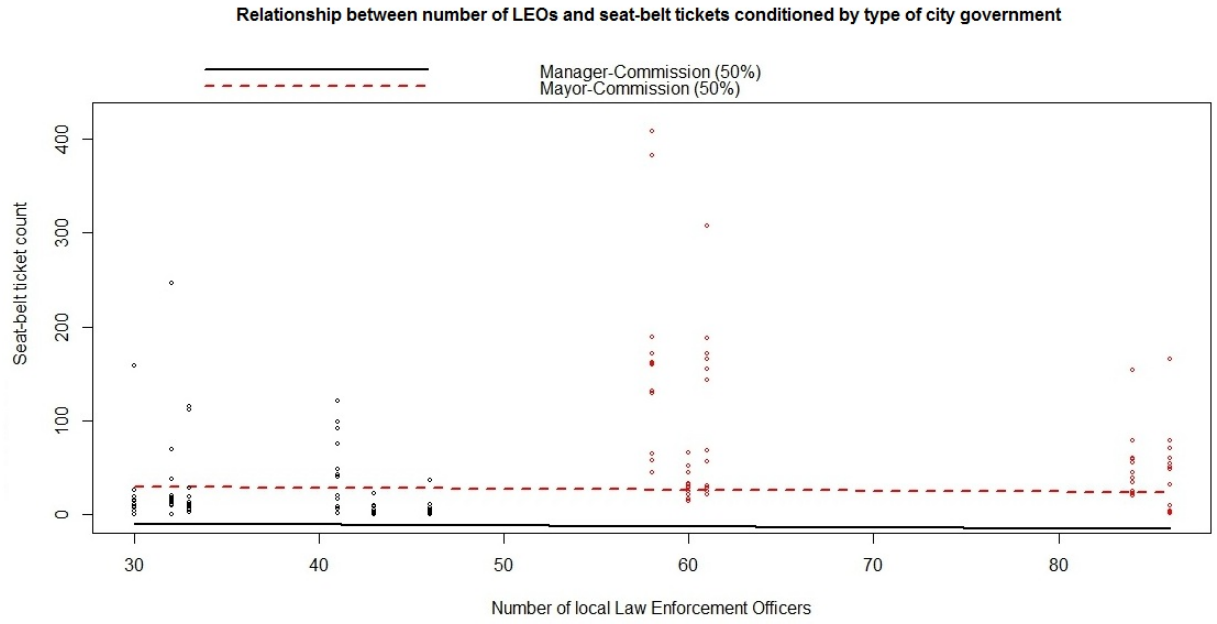
Third, the data presented here does not reveal any characteristics of the individual clients who failed to wear their safety belt and received tickets for this behavior. As the population of a given city increases there is a general decrease in the number of tickets issued but the magnitude of the relationship is undetectable. Also, it is worth noting that the coefficient representing the relationship between the dependent variable and index crimes at the state level is infinitesimal. As the general statewide crime index rate increases – calculated from number of murders, robberies, sexual-and regular assaults – so does the number of tickets issued but the magnitude is very small.

Fourth, cities under mayor leadership tend to issue much higher numbers of tickets for seat-belt violations than their manager counterparts. The dataset provides observations about 12,778 tickets for failure to wear safety-belts and a total of 111,397 tickets for every type of violation. The two cities with mayor-councils – Lenexa and Leawood – were modeled to issue 1,300 more tickets on average for violating the seat-belt ordinance than cities with manager-councils. Even though state law-makers imposed a $10 fine ceiling for this kind of behavior

cities with mayors may still ‘provide a certain level of public safety’ responsive to citizens’ preferences. Since the cost of issuing these tickets are higher than the revenue they generate city managers may instruct police officers to focus their attention on other illegal behavior that is more cost-effective to enforce.

Last, when state legislators imposed the fine ceiling the number of tickets issued for failing to wear safety-belts declined; the relationship is negative but statistically indistinguishable from no-impact. A variety of factors could influence this relationship and combined with the finding on institutional choice I am inclined to argue that a minimum level of public safety is required even when enforcement proves unsustainable financially. However, in examining the impact of the fine ceiling on police discretion a negative and significant relationship emerges.

Figure 3:1 – Number of LEOs and seatbelt citations by local government type.



My argument throughout this chapter has been that police officers with use their discretion to alter the level of seat-belt law enforcement following the state's shift in local enforcement efforts. In this section I present support for this argument by relying on a beta distribution model; discretion was operationalized as the ratio of seat-belt tickets to all other tickets to account for the actual activities officers direct their focus towards while on duty. A low ratio indicate that officers are spending a small amount of time enforcing seat-belt violations while a higher ratio indicate a higher level of enforcement. The only changes throughout this dataset are represented by the state imposed fine ceiling and the upgrade to primary enforcement. Table 3:2 represents the results from the beta distribution model.

Table 3:2 - Ratio of seat-belt tickets to other citations issued.

Variable	*Coefficient*	*Std. Error*	*z-value*	*p-value*
Mayor-Commission	33.730	11.000	3.066	0.002 **
City Population	-0.000	0.000	-1.306	0.192
State Index Crimes	0.000	0.000	2.329	0.020 *
Intervention(s)				
$10 seat-belt tickets	-0.419	0.180	-2.331	0.019 *
Primary enforcement	0.589	0.166	3.539	0.000 ***
Law Enforcement Officers				
Male LEOs	0.070	0.054	1.304	0.192
Female LEOs	0.451	0.163	2.768	0.001 **
Male Sheriffs	-0.028	0.022	-1.254	0.210
Female Sheriffs	-0.343	0.225	-1.524	0.127
Constant	-4.060	0.654	-6.207	0.000 ***

**** $pr \leq 0.000$, ** $pr \leq 0.001$, * $pr \leq 0.05$, · $pr \leq 0.1$; N = 132; Pseudo $R^2 = 0.577$*

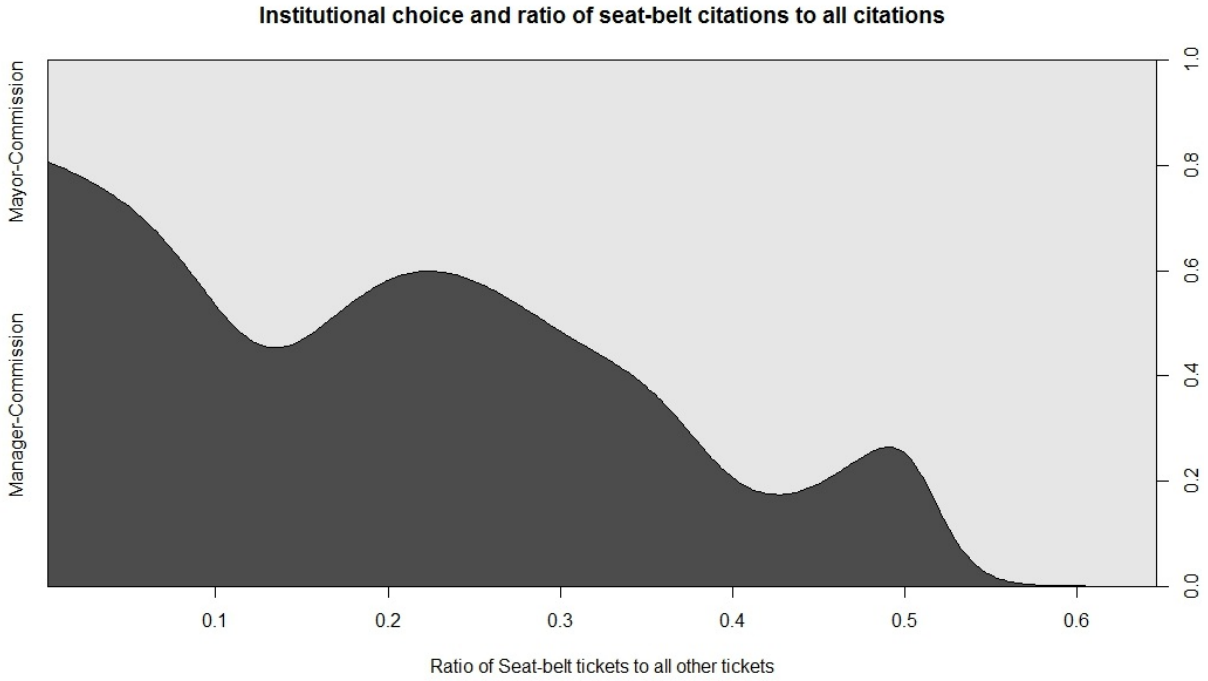
The $10 uniform fine for violating the seat-belt law imposed by state legislators in mid-summer 2011 enjoys a significant and negative relationship with the seat-belt citation ratio. Following the state passage of this legislation local law enforcement officers are relying in their professional discretion to focus their attention on enforcing other types of illegal behavior. Officers may make individual decisions that enforcing seat-belt violations are not cost effective resulting in lower enforcement or officers may be instructed to focus more on more cost-effective types of criminal activity by their supervisors. Although the data does not distinguish between individual or hierarchical discretion the results show that some form of discretion exists on the enforcement level of seatbelt non-compliance. Future research should obtain highly detailed data to examine the distinction between these two types of discretion as a response to state meddling in local affairs.

The analysis reveals an interesting relationship between cities with mayors and the level of seat-belt enforcement compared to all other enforcement. Cities employing managers tend to exhibit a lower seat-belt enforcement ratio than mayor controlled cities. In fact, the only cities that show the highest ratio are cities with mayors while cities with managers are clustered at the lower end of the spectrum. Moving up the range of the ratio the likelihood that cities will have managers decrease and the predominance of mayor cities manifests itself. The relationship shown by the beta model seems similar to that revealed by the negative binomial count model; cities with mayors are continually providing a minimum level of public safety protection despite limits on the revenue collected. Manager cities are more likely to focus enforcement on cost-effective criminal activities by directing attention away from enforcing compliance with seatbelt policies and both models presented in this paper support that claim.

Discussion

Further, imposing a $10 fine ceiling on non-compliance limits the revenues local governments are able to collect to fund continued public safety enforcement without impacting other policing activities. Cities struggling financially may be more willing to pressure officers to, use their discretion to enforce laws that, provide higher economic pay-off for the city treasury. The law also signals unwillingness at the state level to prioritize traffic safety for vehicle occupants. Rational citizens will realize that a $10 fine is not much and that police officers are probably less willing to issue citations for only this illegal behavior when conducting vehicle checks.

Figure 3:2 – Mayors and Managers use of discretion in enforcing seatbelt policy.



Conclusion

This study was aimed at answering the question about how local governments respond when the state modifies the parameters of a policy area enforced by municipal officials. Previous literature has found a wealth of knowledge in the areas of bureaucratic discretion and agency response to alterations in the preferences of principals at the federal level. This study finds support that local governments are responsive to their principals at the state level when faced with limited autonomy in enforcing mandatory seatbelt policies. Police officers change the amount of focus spent on enforcing seat-belt compliance through the mechanism of professional discretion. As governmental agents they enjoy vast powers to punish citizens engaged in criminal behavior; when the state limits the size of this punishment level of enforcement drops significantly. However, this finding does not reveal whether individual officers or their supervisors are the source for the shift in enforcement of compliance.

The influence of electoral representativeness on levels of enforcement is found to be positive and statistically significant. Officers in cities employing managers issue vehicle occupants fewer tickets for failing to wear safety-belts than officers in cities lead by mayor-councils. Managers may increasingly focus attention on enforcement of cost-effective illegal behaviors while cities where leaders are accountable to citizens provide at least a minimum level of public safety. Results from both the negative binomial model (Table 3:1) and the beta distribution model (Table 3:2) support this claim; meaning, enforcement levels are conditioned by the city's political institution. This finding speaks to the literature on political institutions when parameters are changed at the state level impacting enforcement efforts at the municipal level.

Chapter 4 - Driving Distracted: How effective are state policies at preventing distracted driving?

Introduction

Distracted driving has grown to become a major contributor to injuries and fatalities figures on public roads in the United States. The number of people killed in vehicle crashes caused by distractions increased 6.7 percent from 2012 and included over 3,150 people in 2013, while 424,000 receive some sort of injuries from crashes (US Department of Transportation 2015). Cell phone usage is responsible for 12 percent of distracted driving fatalities and drivers who are in their 20s account for 27 percent of the distracted driver of a vehicle involved in a fatal crash (NHTSA 2013). The Insurance Institute for Highway Safety reports that drivers spend almost 12 percent of their driving-time solely interacting with passengers and 6.5 percent of driving-time talking on their cell phone (IIHS 2015). Distracted driving includes talking on a cell phone and texting while driving; eating and drinking; interacting with passengers; reading and watching videos; using the navigation system and adjusting vehicular controls such as radios and air-conditioning systems (NHTSA 2013). Despite this broad definition, the American public has made cell phone usage synonymous with distracted driving. Opinion polls show public support for state legislatures to address the issue of distracted driving with regulatory policies (New York Times/CBS News 2009). Given the growing danger of technology use while driving, it is important to inform policy makers and the public about the effectiveness of policies implemented to counteract the impact of distracted driving. Due to the lack of research on the effect of cell phone restrictions (Kwon, Yoon and Jang 2014, Lim and Chi 2013, Farmer, Braitman and Lund 2010) on crashes among the general driving population the Governor's Highway Safety Association encourages states to wait for evidence of effectiveness before

adopting cell phone bans (GHSA 2011). It is especially critical to identify the sort of regulatory policies that are effective and those that are less effective since they will directly impact peoples' lives.

Background

Although the authority to regulate transportation mostly lies with the states, the federal government has made combatting distracted driving a priority under President Obama's administration. The national government cannot directly regulate traffic policies in the states but is able to do so indirectly through awarding financial incentives to states that enact traffic safety laws. Under Secretary Ray LaHood, the U.S. Department of Transportation showed its' commitment to reducing distracted driving by all transportation modes. In September 2010, the Federal Motor Carrier Safety Administration (FMCSA) issued texting bans for bus and commercial truck drivers and 13 months later completely banned all hand-held device usage for these drivers (FMCSA 2011). In 2009, the National Transportation Safety Board (NTSB) restricted all staff from texting during work hours while driving, and from using cell phones while driving on personal time if those phones were federally issued (Hersman 2009). As the first federal agency to call for a ban on non-emergency use of portable electronic devices for its 400 employees, the NTSB has also published a number of safety recommendations: the enactment of restrictions on nonemergency use of cell phones while driving in all 50 states in addition to high-visibility enforcement and a public campaign targeting drivers at risk (NTSB 2011).

Many states have implemented policies that aim at reducing distracted driving; however, these policies vary considerably and may not be effective at reducing the problem of distracted

driving. New York implemented the first state-ban on hand-held cell phone usage while driving in 2001 (New York 2001). In 2007, Washington became the first state to implement a complete ban on texting while driving, for all drivers (Washington 2007). Further, Maine and New Jersey were the first states to restrict teenagers from using cell phones while driving, in 2002 (Maine 2002, New Jersey 2002). More recently, in 2013, Hawaii and Florida became the 40th and 41st states to ban texting while driving, respectively (Chase 2014). Several states have followed suit and implemented regulation on texting while driving and teenager cell phone usage while driving, but few states fully restrict the use of cell phones while driving for all drivers.

Literature Review

Policies that restrict individual behavior while confined to one's own vehicle are invasive regulations. This fact is evident in the number of states that have implemented policies restricting some or all cell phone usage while driving. For example, 35 states and territories had bans on texting while driving in 2010; only 10 completely banned all cell phone usage for all drivers the same year. In 2012 those figures were 44 and 12 respectively. These figures indicate that while states are reluctant to fully restrict cell phone usage while driving, they are much more willing to restrict aspects of hand-held devices. Policies regulating texting bans appear a popular public policy for lawmakers in many states as a solution to distracted driving. However, these policies only restrict a specific activity of the target population and may not be effective at shaping behavior in the long run, nor are they easily enforced. Texting has become almost as common as talking on the phone and younger drivers are much more likely to embrace this new technology than older drivers (Madden and Lenhart 2009). Knowing this, several states have implemented restrictions on texting while driving. However, North Carolina's policy restricting teenage

drivers from using cell phones while driving does not appear to reduce the occurrence of teen usage. In fact, one study shows that although South Carolina did not implement a similar policy, results from these two states are not statistically significantly different; meaning that the policy restricting teen cell phone usage has not produced any long-term shifts in target group behavior (Goodwin, O'Brien and Foss 2012).

Policies restricting teens and novice drivers from using cell phones while driving are ineffective at reducing fatalities for this group of drivers. Graduated driver licensing (GDL) programs are also ineffective at reducing fatal outcomes for crashes where teens operate vehicles (Lim and Chi 2013). While enforcing policies outlawing novice cell phone usage is a daunting task for law enforcement in the absence of an age identifier, GDL programs were initially effective but have lost their impact over time. Although these policies focus on teens, only policies completely restricting cell phone usage for all drivers are effective at reducing fatality figures among young adults (Lim and Chi 2013). This type of finding indicate the difficulties of enforcing policies that only apply to a targeted group of drivers, and/or display the lack of compliance, with these policies, shown by teen and young drivers. Drivers, restricted from cell phones based on age, might feel unfairly treated as their older friends and family members drive unrestricted

In 2001 New York implemented a complete ban on the use of hand-held devices while driving; it was the first state to enact such restrictive distracted driving policies (Nikolaev, Robbins and Jacobson 2010) and few states have followed over the ensuing decade. Examining usage figures pre-and-post ban, McCartt and Geary (2004) did not find a statistical significant difference in usage as a result of this policy. However, fatality rates in 10 of 62 New York counties decreased significantly as a result of this policy while injury rates dropped significantly

in 46 of the 62 counties (Nikolaev, Robbins and Jacobson 2010). Before and after results from California's 2008 restrictions on hand-held cell phone use indicate a reduction in fatalities and injuries stemming from this policy (Ragland 2012). Further, Kwon, Yoon, and Jang (2014) attribute the decreasing trend in cell phone related collisions to policies completely banning cell phone use in California. Nonetheless, examining collision claim data from Connecticut, New York, California, and the District of Columbia, the Highway Loss Data Institute (2009) found no significant influence of policies, banning handheld devices, on trends of collision claims. Findings such as these indicate that previous research on the effectiveness of distracted driving policies has focused either on cell phone usage or fatal crash outcomes. Therefore, in this study both will be examined to provide statistical evidence for the effectiveness of state policies in reducing usage and fatalities.

Considerable research, laboratory simulations and naturalistic observations, has been conducted establishing a causal relationship between cell phone usage and distracted driving. Evidence supports a strong relationship between distractions, including cell phones, and reduced driving safety (Wilson, et al. 2010). Cellular phone use has been linked to elevations in crash risk (McEvoy, et al. 2005). However, current research on the effectiveness of distracted driving policies has been limited and inconsistent in part because different policies have been examined and in part due to the research designs used to study these policies.

Other variables of interest

Previous studies have determined that compliance with seatbelt ordinances reduce the number of traffic fatalities. At the national level (50 states and the District of Columbia) a one percentage increase in the seat-belt usage rate is estimated to save 136 lives; extending this

relationship implies that increasing usage to 90 percent would save an estimated 1500-3000 lives annually (Cohen and Einav 2003, 829). Therefore, drivers who wear seat-belts are less likely to suffer fatal injuries if involved in a crash.

Several studies have examined the impact of speed on injuries and fatalities resulting from vehicle crashes. Koshal (1976) initially examined the capacity of highways in the US and found that introducing a 10 percent increase in the speed limit would increase the accident rate per motor vehicle by 4.5 percent. The relationship between travel speed and fatalities has been supported by subsequent studies establishing a strong, positive, and significant correlation.

Other characteristics such as driver age, shows a positive relationship with the number of road fatalities, meaning as age increases so does the number of fatal accidents (Houston and Richardson 2004, Cohen and Einav 2003, Dee 1998). According to the Center for Decease Control and Prevention (2015) the leading cause of death for teens in the US is motor vehicle crashes. Actually, “the risk of motor vehicle crashes is higher among 16- to 19-year-olds than any other age group” (Irons, et al. 2014, 735). Due to the prevalence of fatalities and injuries from crashes where teens or young drivers operate a vehicle, many states have implemented policies specifically aimed at influencing the behavior of this group.

While studying the underlying motivation to text while driving, Struckman-Johnson et al. (2015) found gender differences affect the type of texting a driver performs. When males’ text and drive their messages are often longer, ranging from 1-5 sentences, while women’s messages are most often shorter than one complete sentence. They attribute this gender difference to males being overly-optimistic about how undistracted they are when texting; however, women are more often fearful of the consequences of texting while driving, therefore, they text shorter messages than men. Consequently, “we speculate that to the extent that women send short texts,

they have a safety advantage in getting their eyes and thoughts quickly back on the road" (Struckman-Johnson, et al. 2015, 227). However, Russo et al. (2014) found that female drivers were statistically more likely to use cell phones while driving on Michigan roads. Further, their observational study reveals that SUV drivers, weekend drivers, and drivers observed during morning or afternoon traffic peaks are more likely to use cell phones while driving.

Additionally, individual income is negatively related to the number of fatalities (Houston and Richardson 2004, Cohen and Einav 2003); this finding may be a result of the relationship between wealth and safer vehicles. As income increases individuals are able to spend more of their resources acquiring safer vehicles that may ultimately be more effective at saving lives in case of an accident. While wealthier individuals purchase newer, larger, and safer vehicles, the less wealthy may have to settle for the vehicles they can afford that tends to be older and have fewer safety features. However, income is positively associated with fatalities for young drivers while high unemployment rates result in less fatal crash involvement for drivers in this age group (Lim and Chi 2013). For teens and young adults, income may reflect their ability to acquire technological devices and sophisticated cell phones, which in turn distracts their already less experienced driving performance.

Data

The National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS) database contains a variety of demographic information associated with crashes occurring on public roads in the US. These observations must result in a fatality, recorded when occurring within 30 days of the corresponding crash. The database in compiled of information collected from emergency medical or coroner reports, hospital records, state registration and

licensing files, death certificates, and police reports (Wilson and Stimpson 2010). Witness statements, from vehicle passengers or pedestrians, are used when police reports are unavailable; however, police reports take precedence when statements conflict (US. Department of Transportation 2014). This study examines FARS observations made during the time period 2010-2012. It is the most recent period available and the first time detailed information is incorporated in the dataset, regarding the type of distraction responsible for crashes. Also, this period provides considerable variation in the type of distracted driving policies found in many states.

Each crash observation recorded in FARS accounts for demographic information about the driver, driver-related factors, vehicle-related factors, and crash-related factors. Additionally, observations are made about the type of distraction the driver experienced in the moments before a crash occurred. Factors that distracted the operator from the primary task of driving include carelessness, emotions, or preoccupation with communication devices. For example, eating or drinking, other occupants in the vehicle, smoking, adjusting controls such as air conditioning, and cell phones are just some of the distractive factors provided for drivers involved in crashes. Demographics include age, sex, race, and licensing status in addition to information about previous accidents or driving suspensions. Vehicle-related factors include make, model, and year while crash-related factors include variables such as lighting conditions and weather type at the time of a crash. Information about the time, day, and month of the crash is also provided.

State laws regulating the usage of cell phones while operating a vehicle were collected from two similar studies. Ibrahim et al. (2011) utilized Westlaw and Lexis-Nexis to create a dataset containing laws regulating cell phone usage in the 50 states. Their primary objective was to create an open-source dataset to “facilitate future public use of the data set” (Ibrahim, et al.

2011, 659). The second study was used to confirm the list of state laws provided by the first set of researchers. Chase (2014) provides information about state laws banning texting while driving that is consistent with Ibrahim et al. (2011). State laws completely banning the use of hand-held devices while operating a vehicle are almost identical. When considering policies regulating teenager cell phone usage while driving, the two sources vary slightly for the year state laws were recorded as implemented. In this study, Ibrahim et al (2011) provides law data for Arkansas, Colorado, Illinois, Mississippi, Missouri, New Jersey, Oklahoma, Oregon, Texas and Utah. Chase (2014) acts as the source for laws in the remaining states. Combined, these studies lay a comprehensive groundwork for how the 50 states regulate driver's use of communication equipment while operating a vehicle.

Methods

The main focus of this study is to examine the effectiveness of state policies aimed at reducing the frequency of distracted driving and the severity of crash outcomes. While growing public awareness of the consequences of distracted driving, and the propensity for young people to embrace technology (Madden and Lenhart 2009) make distracted driving policies important alone; from a public policy perspective they might be inappropriate given the issue they are designed to address. The limited previous research on the effectiveness of distracted driving policies has focused either on cell phone usage or (fatal) crash outcomes. Consequently, this study will examined both how distracted driving policies impact the frequency of crashes and the effectiveness of polices in reducing serious- and fatal injuries sustained in crashes. Many states have implemented policies specifically aimed at influencing the behavior of cell phone using drivers.

Table 4:1 - Descriptive Statistics	2010	2011	2012
Distracted driving fatalities, no.	20,842	9,048	10,156
Proportion of all fatalities	31.5%	27.8%	30.2%
Distracted by cell phone fatalities, no.	736	324	359
Proportion of distracted driving fatalities	3.5%	3.6%	3.5%
Proportion of all fatalities	1.1%	1.0%	1.1%
Driver's gender			
Male	65.5%	66.5%	66.6%
Female	34.5%	33.5%	33.4%
Driver's race			
Asian	1.0%	1.0%	1.0%
Black	12.7%	12.7%	12.7%
Hispanic	12.7%	12.7%	12.7%
Native American	1.5%	1.5%	1.5%
White	70.6%	70.6%	70.6%
Driver's age, mean.	38.22	38.36	38.56
Driver's age			
16-29	32.1%	31.7%	30.8%
30-49	28.3%	28.4%	27.5%
$\geq$ 50	30.9%	31.2%	33.6%
Driver's previous accidents			
Yes	12.1%	12.4%	12.1%
No	87.9%	87.6%	87.9%
Driving alone			
Yes	53.2%	53.9%	54.4%
No	46.6%	45.8%	42.5%
Driving impaired by alcohol			
Yes	22.3%	21.2%	21.5%
No	77.7%	78.8%	78.5%
Type of collision			
Rear-end	6.2%	6.7%	6.2%
Head-on	10.3%	10.5%	10.5%
Other	68.5%	66.9%	67.7%
Non-vehicular	14.9%	15.8%	15.6%
Location of crash			
National Highway System	34.8%	34.8%	34.8%
State & local roads	65.2%	65.2%	65.2%
Safety Restraint Used			
Shoulder Belt	0.5%	0.6%	0.4%
Lap Belt	2.4%	2.2%	1.7%
Shoulder and Lap Belt	86.0%	85.7%	86.3%
None Used	40.6%	39.7%	37.2%

If states aim at reducing usage and crash severity, caused by distracted drivers, they need to know the type of policies that effectively reduce cell usage, injuries, and fatalities. This chapter introduces two models to bridge the gap in existing literature between research examining usage and the studies examining crash outcomes. The first model, results presented in Table 4:3, is a bivariate variable where occurrences of distractions by cell phone usage are modeled. The second dependent variable of interest for this study is a categorical variable where crash outcome; minor injury, incapacitating injury, and fatal injuries are compared to crashes resulting in no-injury. The unit of analysis for both models is individual crash observations.

Table 4:2 - State Distracted Driving Policies	**2010**	**2011**	**2012**
State Laws, no.			
Texting ban	35	42	44
Teen cell phone usage ban	32	35	37
Complete cell phone usage ban	10	11	12

Between 2010 and 2012, distracted driving was responsible for 43.4% of all crashes that resulted in fatal injuries. Distracted drivers counted for 38.3% of all serious and incapacitating injuries; 38.8% of all minor injuries; 36.6% of all 'possible' injuries; while 231,361 did not receive any injuries from crashes. Cell phone usage was responsible for 3.54% of all fatal injuries caused by distracted driving; 3.52% of all serious/incapacitating injuries caused by distracted driving; 3.83% of all minor injuries caused by distracted driving; and 3.45% of all 'possible' injuries produced by distracted drivers. These figures show the true impact of distracted driving on fatalities and injuries sustained from crashes on U.S. public roads.

Previous studies examining the effects of cell phone usage on driving performance have produced an abundance of findings. The main aspect of distracted driving deals with how driving performance is affected; reaction time, maintaining lane position, stopping distance or time,

following distance, maintaining appropriate speed, complying with traffic signals, appropriate steering, and perceived workload (McCartt, Hellinga and Bratiman 2006). In simulated settings drivers who were distracted by cell phone usage experienced greater difficulties maintaining regular driving performance at higher speed (Shinar, Tractinsky and Compton 2005). However, in the real world drivers adjust for the increase in perceived danger from being distracted at higher speeds, leading to *fewer* occurrences of cell phone distractions:

> …blacks and speeding motorists were less likely to be using cell phones. In addition, drivers older than age 45 and those with a front-seat passenger were less likely to be using cell phones. Cell phone use was also less likely to be observed on weekends and late at night, between midnight and 8 A.M (Johnson, et al. 2004).

The distractive element of cell phones usage is the conversation, impacting thought processing, and taking eyes off the road while dialing or texting. Strayer and Johnston (2001) found a greater distraction effect of hands-free phone conversation than listening to books on tape. Further, Strayer et al. (2005) reports greater effects from both hands-free and hand-held phone conversations than from listening to the radio. Drivers who talk on their phones are at least four times more likely to miss a highway exit than if conversing with a passenger (Drews, Pasupathi and Strayer 2008).

Additionally, gender and age are contributing factors in maintaining or regaining control over a vehicle if engaged in distracted driving. Briem and Hedman (1995) found males to exhibit better vehicle control for cases involving both radio and hands-free phone conversations. Teenagers (16-18) distracted by cell phones detect far fewer events in front of their vehicle and exhibit more lane deviations (Greenberg, et al. 2003). Many factors contribute to a crash when drivers experience distractions while operating a vehicle. This study controls for race, age, gender, type of distraction, day during week, month, lighting and weather conditions at the time of a crash, the presence of alcohol, and whether the driver was speeding.

In addition to demographic and event-related observations, vehicle-related factors are included in this analysis. The age of a vehicle involved in a crash will likely reflect the state of safety technology at the time of manufacturing; meaning, newer vehicles should be safer as manufacturers continue to improve safety features. A vehicle's body type will also play a role when it comes to safety. Larger vehicles have room for additional safety systems and the sheer size of these vehicles may protect the driver to a larger degree than smaller vehicles. Combined these factors could help offset part of the injury-seriousness sustained from crashes caused by distracted driving.

Results

To determine the connection between certain public policies and the occurrence of distracted driving where cell phones usage was the main responsible factor causing a crash, I estimated a logit model. The analysis, shown in Table 4:3 reveal that policies, completely restricting teenagers from using cell phones while driving, are associated with a significant increase in crashes caused by cell phone based distracted driving. This finding is consistent with previous research finding little lasting effect on distracted driving of policies restricting teenager cell phone usage while driving (Goodwin, O'Brien and Foss 2012). Further, the analysis reveals an increased likelihood of cell phone based distractions when drivers are wearing lap or shoulder seatbelts. A possible reason could be that drivers' feel safer while wearing seatbelts and therefore engages more in risky behavior. Older drivers are less likely to be distracted by cell phones while driving as are males. Operating older vehicles decreases the likelihood a driver will be distracted by cell phone usage; however, conventional levels of statistical significance were not reached. A possible reason for this decreased likelihood could indicate that drivers of older vehicles do not

have the means to acquire and/or use cell phones, their cell phones are harder to manipulate while driving, or these drivers over-compensate knowing that their vehicles are less safe than newer models. The analysis indicate that exceeding the speed limit (measured by the real miles per hour in excess of the limit), is a significant determining factor for drivers to experience cell phone base distractions. However, this revelation is inconsistent with research finding fewer cell phone distractions at high speeds (Johnson, et al. 2004).

Interestingly, a significant likelihood exists between cell phone distractions and single drivers. The likelihood is similar for drivers with passengers in both the front and rear seats (many passengers). These results are consistent with previous research finding support for only front-seat passengers intervening when drivers are using cell phones, resulting in fewer occurrences of distracted driving. These coefficients do not reach a conventional statistical significance level, but are significant at 0.1 levels of probability.

In order to examine the effectiveness of public policies, aimed at reducing the impact of distracted driving on public roads, several control variables were included in the model in addition to dummy variables representing each state with the corresponding laws. A majority of states have enacted policies banning texting while driving. Compared to other policies this is the least restrictive while a complete ban on cell phone usage while driving is considered the most invasive policy. A policy banning teenagers or novice drivers from using cell phones while operating a vehicle falls short of the invasiveness of a complete ban but is more invasive than bans on texting while driving for this target-group. As these policies become more invasive the number of states with these policies decline. For example, as depicted in Table 4:2, 35 states and territories had bans on texting while driving in 2010; only 10 completely banned all cell phone usage for all type of drivers the same year. In 2012 those figures were 44 and 12 respectively.

Table 4:3 - Logit modeling cell phone usage as determined to be the cause of crashes.

Variables:	Estimate	Std. Error	z-value	p-value
Policy:				
Text Ban	0.079	0.119	0.67	0.503
Teen Ban	0.434	0.118	3.678	0.000***
Usage Ban	-0.111	0.111	-0.999	0.318
Race:				
Asian	-11.580	158.000	-0.073	0.942
Black	0.005	0.179	0.029	0.977
Hispanic	-0.382	0.232	-1.647	0.099+
Native American	0.482	0.387	1.244	0.213
Pacific Islander	-0.067	0.713	-0.095	0.924
White	0.170	0.095	1.791	0.073+
Vehicle occupants:				
Driver only	1.936	1.004	1.929	0.054+
Driver and front passenger(s)	1.521	1.006	1.512	0.131
Driver and many passengers	1.682	1.006	1.672	0.095+
Restraint type:				
Lap or shoulder belt	0.950	0.333	2.856	0.004**
Lap and Shoulder	0.527	0.262	2.009	0.044*
Other restraint	-0.599	0.390	-1.540	0.124
No restraint	0.588	0.261	2.255	0.024*
Age of driver	-0.017	0.002	-7.299	0.000***
Males	-0.497	0.076	-6.500	0.000***
Vehicle age	-0.011	0.006	-1.693	0.000+
Exceeding speed limit	0.004	0.000	4.475	0.000***
(Intercept)	-6.932	1.039	-6.674	0.000***

*+ $p \leq 0.1$, * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$, N = 300,224*
Source: FARS Data

Policies regulating texting bans appear a popular public policy for lawmakers in many states as a solution to the problem of distracted driving. Possible reasons for this observation might be that elected officials are unable to enact legislation that is most invasive while appearing to take the issue serious when implementing less invasive distracted driving policies.

The effectiveness of distracted policies might be another concern to policymakers. Limited academic research in this area could be a reason why few states have adopted these policies. The calculative impact of distractive driving lies with the ability to measure adverse effects on the human body. In this case, crash outcomes of no-injury, minor injuries, major injuries, and fatalities acts as the dependent variable. As the unit of analysis is individual crashes, the dependent variable is organized as a categorical variable with three possible crash outcomes compared to the baseline of no-injury sustained. Other variables of interest are coded according to the FARS handbook, and are in some cases simplified. For example, all subgroups of Hispanics are coded as such, including Cubans, Mexicans, Puerto Rican and Central or South America. Those of Asian heritage are coded as such, including Japanese, Chinese, Korean, and Vietnamese. Where possible the travel speed at crash-time has been compared to the posted speed limit to create a variable measuring the level of speeding a driver engaged in immediately prior to the crash. In addition to demographics, these variables are included to account for results from a variety of previous research and to empirically support this study's contribution to the limited body of research on the effectiveness of policies regulating distracted driving.

Table 4:5 show results from the multinomial logit regression of injury seriousness modeled by a variety of independent variables; crashes resulting in no-injuries acts as the reference category. Values with a * indicate a significant p-value with a two-tailed significance test while values with a + indicate significant p-values with one-tailed significance test. The only state policy that appears to have an impact on all types of crash injuries is the complete ban on hand-held device usage while driving. State policies, banning cell phone usage for all drivers, reduce the likelihood of crashes where the outcome is a serious or fatal injury.

Table 4:4 - Multinomial logit model of crash severity with reference to no-injury outcomes.

Crash Outcome: **No-injury**	**Minor injury**			**Incapacitating injury**			**Fatal injury**		
	Estimate	Std. Error	z-value	Estimate	Std. Error	z-value	Estimate	Std. Error	z-value
Policy:									
Text Ban	0.204	0.223	0.917	0.306	0.224	1.377	0.276	0.223	1.239
Teen Ban	-0.168	0.221	-0.759	0.078	0.221	0.355	-0.160	0.222	-0.724
Usage Ban	-1.054	0.177	-5.945*	-1.277	0.177	-7.216*	-1.236	0.178	-6.952*
Race:									
Asian	-21.021	113.110	-0.186	-21.074	116.870	-0.180	-20.368	113.090	-0.180
Black	-13.364	1.009	-13.251*	-12.667	0.718	-17.654*	-20.265	25.084	-0.808
Hispanic	-20.745	26.632	-0.779	-20.846	27.674	-0.753	-20.224	27.119	-0.746
Native American	-20.378	72.087	-0.283	-20.391	75.160	-0.275	-20.326	74.364	-0.273
Pacific Islander	-20.799	98.152	-0.219	-20.859	100.500	-0.276	-20.185	98.175	-0.206
White	-14.420	0.722	-19.976*	-14.023	0.598	-23.452*	-14.576	1.010	-14.431*
Vehicle occupants:									
Driver only	-0.080	0.603	-0.132	-0.511	0.595	-0.858	-0.778	0.595	-1.307
Driver and front passenger(s)	-0.056	0.616	-0.091	-0.003	0.608	-0.006	-0.061	0.608	-0.101
Driver and many passengers	0.534	0.618	0.863	0.640	0.610	1.049	0.499	0.610	0.818
Restraint type:									
Lap or shoulder belt	0.337	0.820	0.411	0.539	0.820	0.657	0.860	0.823	1.045
Lap and Shoulder	0.688	0.543	1.270	0.668	0.541	1.787+	1.079	0.543	1.986*
Other restraint	-3.874	0.566	-6.840*	-2.000	0.560	-3.575*	-0.987	0.561	-1.757
No restraint	-2.340	0.526	-4.450*	-0.827	0.525	-1.574	0.146	0.528	0.276
Lighting Conditions									
Dark but lighted	0.078	0.200	0.392	-0.318	0.200	-1.590	-0.351	0.200	-1.752+
Dawn	-0.548	0.507	-1.082	-0.088	0.505	-0.174	0.019	0.506	0.038
Daylight	-0.290	0.170	-1.708+	0.174	0.170	1.025	0.160	0.170	0.939
Dusk	0.370	0.553	0.670	0.353	0.553	0.638	0.389	0.554	0.700
Weather Conditions									
Clear	-0.614	3.313	-0.185	-1.428	3.306	-0.432	-1.616	3.316	-0.318
Cloudy/Fog/Smoke	-1.056	3.316	-0.318	-1.732	3.310	-0.523	-1.855	3.314	-0.560
Rain/Sleet/Hail	-1.343	3.320	-0.405	-1.885	3.314	-0.569	-2.001	3.318	-0.603
Severe Crosswinds	0.501	4.810	0.105	-0.025	4.802	-0.005	0.304	4.806	0.063
Snow	0.329	3.408	0.097	0.065	3.401	0.019	-0.344	3.406	-0.101
Age of driver	-0.000	0.004	-0.091	-0.003	0.004	-0.912	0.003	0.004	-0.909
Males	0.207	0.154	1.346	-0.016	0.154	-0.109	-0.204	0.154	-1.323
Vehicle age	0.008	0.015	0.731	0.019	0.011	1.679+	0.019	0.011	1.721+
Exceeding speed limit	-0.009	0.002	-4.862*	-0.004	0.002	-2.052*	-0.000	0.002	-0.506
Distracted by Cell	0.395	0.920	0.429	0.309	0.919	0.336	0.257	0.922	0.279
Constant	6.514	3.410	1.910+	6.441	3.403	1.893+	5.655	3.407	1.660+

+ $p \leq 0.1$, * $p \leq 0.05$, N = 300,224
Source: FARS Data

The z-value for this coefficient is *noticeably* large indicating a strong impact of these policies on the seriousness of crash outcomes. Unexpectedly, cell phone based distracted driving is not a significant factor in determining the severity of crash outcomes. Drivers' who exceed the speed limit are at increased likelihood to experience distractions from cell phones. Speeding results in fewer minor and incapacitating injuries but this coefficient is not significant for fatal injuries, meaning the effect of speeding on fatal injuries is undetermined. Although the coefficient is statistically significant, it is very small per mile per hour exceeding the posted speed limit. Other restraints, such as child restraints and motor cycle helmets reveal a similar impact on crash outcomes as speeding. Race is the last variable revealed as a significant predictor. Black drivers are less likely to receive minor and major injuries following a crash but the coefficient is not significant for fatal injuries. It appears that black drivers are disproportionally involved in crashes that result in fatal injuries. White drivers are more likely to receive major injuries than minor or fatal injuries in a crash. These results empirically support state policies that completely ban the usage of hand-held devices while operating a vehicle. Other policies, such as texting bans and restricting teenagers from using cell phones while driving, are not effective at preventing bodily harm as measured by crash outcomes. This is a clear message that simply enacting these types of policies will not solve the problem of distracted driving. A complete usage ban followed by a public awareness campaign and rigorous enforcement[2] is the most effective policy when seeking to reduce the bodily-harm produced by distracted driving.

Several variables show *insignificant* coefficients with the dependent variable, crash outcomes. The interpretation focuses on their lack of significant contributions to minor,

[2] Public awareness and enforcement levels are recommended by the National Transportation Safety Board (2011) to complement complete cell phone usage ban, increasing effectiveness.

incapacitating, and fatal injuries respectively. Policies aimed at reducing distracted driving that focus on restricting texting for all drivers and restricting teenagers from using cell phones while driving are not effective at reducing any type of injury sustained in a crash. Racial background of the driver varies significantly with large margins of error, except for black and white drivers. The standard errors for modeling Asian, Hispanic, Native Americans, and Pacific Islanders are very large which might indicate that few crashes result in injuries/fatalities with drivers from these racial backgrounds. Whereas driving alone or with many passengers cause distractions, none of the vehicle occupant categories are significant predictors of the severity of crash outcomes.

Further, lighting and weather conditions on the road at the crash time do not appear to be significant factors in the severity of injury sustained from a crash. Although daylight conditions result in fewer crashes with minor injuries and lighted roads at night reduce fatal injuries, these coefficients are only significant when analyzed with one-tailed significance tests. Contrary to modeling distracted driving by cell phones; age, gender, and vehicle age have no significant impact on the outcome severity of a crash when conducting two-tailed significance tests. Ultimately, the variables significantly associated with crash outcome severity are different from those that are significantly associated with cell phone distractions reflecting the contradictions in previous research on the effectiveness of distracted driving policies.

Discussion

Due to the limited previous research on the effectiveness of these policies (Kwon, Yoon and Jang 2014, Lim and Chi 2013, Farmer, Braitman and Lund 2010) the Governor's Highway Safety Association has encouraged states to wait for evidence of effectiveness before adopting cell phone bans (GHSA 2011). The goal of this study was to examine the effectiveness of three

distracted driving policies. Results from logit and multinomial logit models provide empirical support for policies that completely restrict cell phone usage for drivers operating a vehicle. Policies that restrict texting and teen usage are not effectively solving the hazards of distracted driving on public roads in the United States. Restricting teenagers, from using cell phones while driving, actually increases the occurrence of distracted driving. These findings are consistent with previous research (Goodwin, O'Brien and Foss 2012), where complete restrictions on teenagers have not produced long-term behavioral shifts in the targeted population.

Results for other variables are consistent with findings from previous research. Speeding motorists, older drivers, and front-seat passengers contribute to the distractions experienced by drivers (Johnson, et al. 2004). Gender is a contributing factor for drivers distracted by cell phones. The results presented here cannot support Briem and Hedman's (1995) finding that males to exhibit better vehicle control for cases involving both radio and hands-free phone conversations. Here, males are simply less likely to be distracted by cell phones while driving than women. Further, consistent with Greenberg et al. (2003) younger drivers are more often distracted by cell phone usage. The analysis shows that exceeding the speed limit (measured by the real miles per hour value in excess of the limit), is revealed as a significant predictor for cell phone distractions. However, this coefficient is inconsistent with research finding fewer cell phone distractions at high speeds (Johnson, et al. 2004).

Contrary to previous research, policies completely restricting cell phone usage for all drivers, significantly decreases the likelihood of sustaining minor-, incapacitating-, and fatal-injuries from vehicle crashes. McCartt and Geary (2004) did not find a statistical significant difference in usage before and after New York passed its complete cell phone usage ban. Although this study relies on cross-sectional longitudinal data from all 50 states, the District of

Columbia, Puerto Rico, and the Virgin Islands, effects of policies completely banning cell phone usage may vary within one specific state. Results from different research designs are not directly comparable and speaks to the greater lack of consistent, yet limited research on the effectiveness of distracted driving policies.

Conclusion

Distracted driving poses a great danger to public safety on US roads and highways. However, there is limited empirical evidence examining the effectiveness of these policies. This project takes advantage cross-sectional FARS data when examining the effectiveness of three distracted driving policies. Findings indicate that policies restricting young drivers from using hand-held devices while driving are ineffective at reducing distracted driving occurrences. Also, the relationship between policies restricting texting while driving and distracted driving and crash outcomes is not significant. Only policies completely restricting hand-held device usage for all drivers are effective at reducing harmful crash outcomes. This study provides empirical evidence supporting the effectiveness of one policy over others when the aim is to reduce distracted driving and the seriousness of crash outcomes.

Although these results are strong, more research is needed that include contextual variables that may or may not influence policy effectiveness at subnational levels. Effects of policies completely banning cell phone usage may vary within one specific state. Results from different research designs are not directly comparable and speaks to the greater lack of consistent, yet limited research on the effectiveness of distracted driving policies. Findings from this cross-sectional study contradict previous research employing a case study model. More research is needed to determine contextual factors that may influence the effectiveness of

distracted driving policies. Interestingly, findings in this chapter indicate that policies banning novice drivers from using cell phones, are ineffective at reducing the frequency of cell phone based distracted driving. Although consistent with previous research, a thorough examination is needed to determine the underlying impact of these policies on the frequency of distracted driving caused by cell phone usage.

Chapter 5 - Conclusion

Providing feedback on policy effectiveness, by analyzing the consequences of policy shifts, is an important yet understudied aspect of the policy process. Many studies have examined policy- identification, formulation, passage, and implementation but fall short when it comes to addressing the effects of these policies. Theories have been developed based on insight from these studies yet only pay marginal attention to evaluating the effectiveness of implemented policies. Sabatier and Weible (2007), Baumgartner and Jones (1993), and Berry and Berry (1990) have extensively contributed to our theoretical understanding of how coalitions influence policymaking, how policy changes incrementally over time, and how/why states adopt new policies, respectively. While these approaches are large developments on how we explain policy change they do not place emphasis on the 'backend' of the policy cycle where feedback is provided to determine the appropriateness or effectiveness of the policies implemented. My contribution with this project has been to evaluate shifts in traffic safety policies in order to provide statistical evidence about the effectiveness of these altered or 'new' policies.

This dissertation has examined the impact of shifts in traffic safety policies at the state level, and provided feedback about the effectiveness of these shifts through statistical analyses. Results from these analyses were then used to evaluate the effectiveness of shifting the speed limit in a state; upgrading to primary enforcement of seatbelt compliance; and regulations on distracted driving. In doing so, natural experiments are introduced to the field of research on the policy feedback mechanism.

The following research questions have guided each of the three individual chapters in this dissertation:

1. How can policy analysis be a valuable tool in evaluating the effects of policy shifts?
2. Are *local* enforcement officers responsive to policy shifts made at the *state* level?
3. How effective are state policies aimed at reducing occurrences of distracted driving?

States enjoy reserved powers to regulate highway travel by determining appropriate speed limits. Alterations in these regulations pose a potential hazard to state residents and non-state residents alike. Many commuters nationwide pass through territory controlled by states they do not reside in; therefore, almost all travelers in a region are at risk if increases in speed limits results in more fatalities and serious injuries. Municipalities derive authority from state constitutions and are charged with enforcing local ordinances. When local law enforcement officers (LEOs) are given broader authority to enforce policy compliance they are expected to increase the scope of their activities. However, when the state limits the weight of the penalty for non-compliance LEOs are able to use discretion to focus on enforcing other policies. Finally, the use of hand held devices has increased dramatically during the past decade and distracted driving pose a significant hazard to the public safety. By analyzing various policies states have adopted to combat distracted driving, I provide feedback on the effectiveness of these policies given the identified problem of distracted driving. In sum, alterations in the proposed policies will impact millions of Americans and may disproportionally impact the lives of many minorities. Bureaucratic implementation may have further shifted a policy away from its intended purpose, but adjusting policies based on feedback may return the policy closer to legislators' original intent.

Methodologically, this project is focused around the advantages of 'gold standard' experiments, by analyzing before and after effects of naturally occurring phenomena's. These reversely designed (Morgan 2013) studies increase internal validity since they closely control the variables of interest and exclude other interferences as the reason for observing changes in a dependent variable. Although many political scientists are more focused on external validity and

generalizability, other 'harder' sciences have successfully taken advantage of quasi-experiments for decades. For example, by studying an increase in the fast-food industry minimum wage in New Jersey and comparing it to Pennsylvania, that did not experience such a shift, Card and Krueger (1994) were able to determine that such an increase did not impact employment figures negatively. Or the classic example where Snow (1855) investigated the cholera pandemic in mid-nineteenth century London by studying higher frequency of cholera downstream of a treatment plant found to be supplying dirty drinking water. By employing more natural experiments; also called "field studies" (McDermott 2002), "Nature's or society's experiments" (Morgan 2013), and "administrative experiments" (D. T. Campbell 1967/1988), the political science field could greatly benefit in similar ways that other disciplines have already.

In chapter 2 – killer speed, I analyze fatality data from Kansas Department of Transportation (KDOT) to determine the impact of increasing the speed limit on highways in Kansas. Due to the oil embargo in the early 1970s Congress adopted a National Maximum Speed Limit (NMSL) of 55 mph for the entire United States (Yamane and Bradshaw 2008). Although the national government does not have the authority to regulate highway travel in the states, it enticed state governments with financial incentives in exchange for compliance with the NMSL. In 1995, Congress repealed the NMSL thus returning control of highway travel to the states and many immediately increased their speed limits.

Studies on the effect of increasing the speed limit have found a positive relationship with fatality figures. Ossiander and Cummings' (2002) Poisson regression model indicates that the "fatal crash rate on Washington State's rural interstates was 110% (95% CI 60-170%) higher after 1987, when the speed limit was raised to 65 mph, than it would have been if the speed limit had not been changed" (15). Similar research, typically examining the shift from 55 to 65mph,

reaches very similar conclusions. Balkin and Ord (2001) found that the speed limit increases, from 55 to 65mph, increased rural road fatalities in 19 states at statistically significant levels while the increase from 65 to 70mph slightly increased rural highway fatalities in 10 states but had no statistically significant impact on urban interstates. However, researchers have found contradictory relationships between speed limit increases and fatalities. When examining the 40 states that raised the speed limit from 55 to 65mph, Lave and Elias (1997, 615) found that the fatality rate decreased 4.68 percent in 1987 and another 1.55 percent in 1988. States that did not adopt an increase in speed limits experienced no change in the fatality rate in 1987 while a 2.55 percent decrease generally occurred the following year.

The results from modeling the impact of increasing the speed limit on fatality figures reveal interesting findings. Increasing the speed limit to 75 mph, on certain roads in 2011, did not by itself increase the number of fatal accidents at levels of statistical significance. As travel speed increases the likelihood that a crash results in a fatality increases. The speed limit increase in 1996, from 65 to 70mph, did not result in a greater number of fatal accidents on the interstates throughout Kansas. The results from modeling the impact on injury accidents reveal a different relationship than the one found between fatalities and the speed limit increase. Both the policy shift and the travel speed are positively related with serious injury figures for the 2011 and 1996 speed limit increases. These results indicate a redirecting of traffic and a change in crash outcomes.

By increasing the speed limit on Kansas highways in 2011 state lawmakers increased the number of serious injuries sustained in vehicle crashes, but not the number of fatalities. This shift in the speed limit only applied to four-lane divided highways and may have altered traffic patterns. Vehicles traveling in the same direction on divided roads are at reduced likelihood of

being involved in head-on crashes. Increasing the speed limit in 1996 actually reduced the number of fatal crashes. This policy shift applied to all Kansas roads and opens the possibility for a redirection of traffic albeit different than for 2011. However, the results from both shifts in the speed limit clearly show that higher levels of speed kills. Speed shows up repeatedly as a significant predictor of traffic fatalities. This finding is consistent across all the models examined but at varying levels of significance.

In chapter 3 – primary seatbelt enforcement, I analyze citation data from five cities in Kansas to determine the effects of shifting from secondary enforcement of seat-belt laws to primary enforcement. In essence, this upgrade to primary enforcement acts as a reduction in the restraints on how local law enforcement officers exercise their authority; they are able to commit vehicle stops for the sole reason that occupants are not wearing seatbelt (Houston and Richardson 2006). Lawmakers send a signal to enforcement officers to prioritize seat-belt violations when upgrading to primary enforcement laws that may result in police officers issuing an increased number of citations (Russell, Dreyfuss and Cosgrove 1999). Additionally, I analyze this data over time to determine the effect of state legislators capping the fine for seatbelt violations at $10. At this fine-ceiling, cities are not even able to cover the cost of issuing this type of tickets.

The type of enforcement has a substantial effect on seat-belt compliance rates. Researchers have found that in states with mandated primary enforcement of seatbelt policies compliance tends to be higher (Shults, et al. 2004, Dinh-Zarr, et al. 2001). “Switching from secondary to primary enforcement increases the [seat-belt] usage by about 13 percentage points” (Cohen and Einav 2003, 839). Therefore, the federal government has urged states with only secondary enforcement laws to upgrade to primary enforcement of seat-belt laws (NHTSA 2004,

1995). However, few studies actually consider the impact primary enforcement of seat-belt laws has on the number of fatalities (Houston and Richardson 2006). Both primary and secondary enforcement of seat-belt laws experience a negative relationship with the levels of road fatalities, although secondary enforcement reduces fatalities at roughly half the rate of primary enforcement (Houston and Richardson 2004, 652) or even less than half as effective in some studies (Dee 2001, 120). Thus, increased compliance should lead to safer roads and subsequently reduce fatal and serious injury accidents.

The results of the two models indicate that local law enforcement officers (LEOs) are quite responsive when state legislators change the parameters of their enforcement efforts. Upgrading to primary enforcement of seatbelt ordinances produces an increase in the number of citations issued by officers in the five Kansas cities examined. This relationship is statistically significant and for the city sample examined, means an increase of roughly 65 citations for seatbelt violations, per year as a result of upgrading to primary enforcement. Further, when state legislators imposed a $10 ceiling on citations for seatbelt violations, LEOs use their discretion to shift enforcement efforts. The $10 uniform fine for violating the seat-belt law imposed by state legislators in mid-summer 2011 enjoys a significant and negative relationship with the seat-belt citation ratio. Following the state passage of this legislation local law enforcement officers are relying in their professional discretion to focus their attention on enforcing *other* laws. Officers may make individual decisions that enforcing seat-belt violations are not cost effective resulting in lower enforcement or officers may be instructed to focus more on more cost-effective types of criminal activity by their supervisors. Even though the data does not distinguish between individual or hierarchical discretion the results show that some form of discretion exists on the enforcement level of seatbelt non-compliance.

By uniformly setting the fine arbitrarily low local enforcement of seat-belt compliance was negatively affected. Municipal agents were able to reduce enforcement through discretion, and it is clear from the data that cities have not encouraged maintaining high enforcement from their police officers. When cities are only able to collect $10 for seatbelt citations they are unable to cover the cost of enforcing this law and direct their enforcement efforts elsewhere. Cities with managers are much more likely to employ this cost-benefit analysis of enforcement efforts. Cities led by elected officials, mayors, are seen as providing some minimum level of enforcement despite the marginal revenue collected from issuing these citations. This analysis has revealed the effects of shifting these two policies; upgrading to primary seatbelt enforcement reduces the threshold needed for officers to conduct vehicle stops while implementing a fine ceiling for these citations reduces the effort local LEOs dedicate to enforcing this type of unwanted behavior.

In chapter 4 – distracted driving, I analyze Fatality Analysis Reporting System (FARS) data from the National Highway Traffic Safety Administration (NHTSA) to determine the effectiveness of three distracted driving policies at the state level. Every year thousands of people are injured and killed in crashes on public roads in the United States. 424,000 injuries and 3,150 fatalities were caused by distractive driving in 2013 (US Department of Transportation 2015). States have implemented various policies to combat the rising threat of distracted driving, yet the academic community has provided little research on the effectiveness of these policies. Much research of the research that has been carried out is technical in nature, either from laboratory simulations or observational, connecting distractions to a reduction in driving performance (Wilson, et al. 2010). Among the limited research on the effectiveness of policies aimed at reducing distracted driving are studies examining the effectiveness of banning novice

drivers from using cellphones while driving (Goodwin, O'Brien and Foss 2012); research examining the effectiveness of Graduated driver licensing (GDL) (Lim and Chi 2013); scholarship examining the effectiveness of total cell phone bans on usage (McCartt and Geary 2004), and fatality and injury figures (Nikolaev, Robbins and Jacobson 2010). Insurance claims have also been studied to identify any changes in the trend of claims submitted from distracted driving crashes (HLDI 2009). Findings such as these indicate that previous research on the effectiveness of distracted driving policies has focused either on cell phone usage or fatal crash outcomes. Therefore, in this study both cell phone usage and crash outcomes were modeled to provide statistical support for the effectiveness of state policies in reducing usage and fatalities.

The results from analyzing roughly 300,000 observations from the FARS dataset indicate that only complete bans on cell phone usage are effective at reducing all types of crash outcomes. Policies restricting novice drivers from using cell phones and policies restricting texting while driving are ineffective at reducing the seriousness of crash outcomes. Actually, state policies banning teens/novice driver cell phone usage is associated with higher occurrence of distracted driving caused by cell phone usage! This is a clear message that simply enacting these types of policies will not solve the problem of distracted driving. A complete usage ban followed by public awareness campaign and rigorous enforcement is the most effective policy when seeking to reduce the bodily-harm produced by distracted driving.

Little research has been conducted examining the effectiveness of state policies aimed at reducing the negative outcomes of distractive driving. For this reason, the Governor’s Highway Safety Association has encouraged states to wait with adopting these types of policies until more research has been undertaken (GHSA 2011). The goal of this study was to examine the effectiveness of three distracted driving policies. Results from logit and multinomial logit models

provide empirical support for policies that completely restrict cell phone usage for drivers operating a vehicle. Policies that restrict texting and teen usage are ineffective at solving the hazards of distracted driving on public roads in the United States. Banning teenagers, from using cell phones while driving, is actually associated with an increase in the occurrence of distracted driving. These findings are consistent with previous research (Goodwin, O'Brien and Foss 2012), where complete restrictions on teenagers have not produced long-term behavioral shifts in the targeted population. Contrary to previous research, on the effectiveness of policies completely restricting cell phone usage while driving, the analysis in this study finds a significant and negative relationship between this policy and the three harmful crash outcomes. McCartt and Geary (2004) did not find a statistical significant difference in usage before and after New York passed its complete cell phone usage ban. My study is arguably more generalizable then research examining policy effects in a single state. I have relied on a cross-sectional dataset that encompasses all 50 U.S. states in addition to the District of Columbia, and the territories of Puerto Rico and the Virgin Islands. Roughly 300,000 observations over a three year period were analyzed and the results provide statistical support only for policies that completely ban cell phone usage while driving. Although these policies are effective at reducing the negative outcomes of distracted driving, many states have yet to implement similar policies. In 2012, only twelve states have completely banned hand-held cell phone usage for all types of drivers. Other, ineffective policies, such as texting bans and restrictions on teen cell usage are popular around the country which might reflect the difficulty of implementing the most restrictive type of distracted driving policies.

In summation, I have several goals in this project, but ultimately I wanted to show the usefulness of employing natural experiments to the field of policy studies. Research designed

around this approach is able to rely on the ‘gold standard’ of experiments. The strength of internal validity is beneficial when determining the causal relationship between variables. However, external validity is less clear, especially when conducting case studies. Two of the three substantive chapters in this project are focused around policy shifts in the state of Kansas and may be less applicable to states with considerably different demographics, political environment, and infrastructure needs. I was able to combine the strength of experiments’ internal validity with external validity when examining distracted driving policies in all 50 states. Findings from this chapter are more generalizable to other states and provide statistical support for the effectiveness of complete cell phone bans over other distracted driving policies. Focusing this project on shifts in various traffic safety policies expand our understanding of the important variables at play within this field of research. If my findings can be extended to other policy fields remain to be seen.

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