THREE ESSAYS IN PUBLIC ECONOMICS

by

TUAN A. NGUYEN

A DISSERTATION

Presented to the Department of Economics
and the Graduate School of the University of Oregon
in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy

June 2017

DISSERTATION APPROVAL PAGE

Student: Tuan A. Nguyen

Title: Three Essays in Public Economics

This dissertation has been accepted and approved in partial fulfillment of the requirements for the Doctor of Philosophy degree in the Department of Economics by:

Benjamin Hansen	Chair
Glen Waddell	Core Member
Caroline Weber	Core Member
Craig Parsons	Institutional Representative

and

Scott L. Pratt	Dean of the Graduate School

Original approval signatures are on file with the University of Oregon Graduate School.

Degree awarded June 2017

DISSERTATION ABSTRACT

Tuan A. Nguyen

Doctor of Philosophy

Department of Economics

June 2017

Title: Three Essays in Public Economics

This dissertation consists of three separate papers in the field of public economics.

In the first substantive chapter, I examine several factors that affect public sentiment on the death penalty, including individual characteristics as well as state-level measures of exonerations, executions, and botched executions. Using comprehensive data from the General Social Survey, the National Registry of Exonerations, and the Death Penalty Information Center, I find that exonerations significantly decrease the probability of supporting the death penalty by two to three percent from the average level. Nonetheless, neither executions nor botched executions seem to have an impact. I also investigate if there are significant changes in the criminal justice system associated with having more exonerations and find that they decrease the number of death sentences.

In the second substantive chapter (coauthored with Benjamin Hansen and Glen Waddell), I investigate the effect of benefit generosity on claim duration and temporary benefits paid among temporary disability claims for workers' compensation. While previous studies have focused on natural experiments created by one-time large changes in minimum or maximum weekly benefits, we exploit variation around a kink in benefit generosity inherent in all workers' compensation systems in the United States. Using

administrative data on the universe of injured workers in Oregon, we find that more-generous benefits leads to longer injuries, but with implied elasticities that are smaller than the average elasticity from previous difference-in-difference studies. Our preferred estimates suggest that a 10-percent increase in benefit generosity leads to a 2- to 4-percent increase in injury duration. We derive similar duration-benefit elasticities when studying changes in benefits paid at the kink.

In the third substantive chapter, I revisit the subject of measuring the impact of benefit generosity in workers' compensation program in Oregon, but this time I utilize a different econometric model: difference-in-differences. The research design takes advantage of a large increase in maximum benefit for injured workers in Oregon beginning January 1, 2002. Before this date, the threshold for maximum benefit was set at 100% of the state's average weekly wage. The threshold increased to 133% of the state's average weekly wage after this date. I examine how this significant increase in benefit generosity affects injury duration, claim cost, and claim filling behavior and find that injury duration and probability of filing a claim are not influenced by increasing benefit generosity while costs go up by the same percentage as benefit generosity.

This dissertation includes unpublished co-authored material.

CURRICULUM VITAE

NAME OF AUTHOR: Tuan A. Nguyen

GRADUATE AND UNDERGRADUATE SCHOOLS ATTENDED:

University of Oregon, Eugene, OR
University of Texas at Austin, Austin, TX
University of Texas at Arlington, Arlington, TX

DEGREES AWARDED:

Doctor of Philosophy, Economics, 2017, University of Oregon
Master of Science, Economics, 2012, University of Texas at Austin
Master of Arts, Economics, 2010, University of Texas at Arlington
Bachelor of Business Administration, Economics, 2010, University of Texas at Arlington

AREAS OF SPECIAL INTEREST:

Public Economics
Health Economics
Economics of Crime

GRANTS, AWARDS AND HONORS:

Graduate Student Teaching Award, University of Oregon, 2016

Kleinsorge Summer Research Fellowship, University of Oregon, 2016

First Year Econometrics Award, University of Oregon, 2013

Graduate Teaching Fellowship, University of Oregon, 2013-2017

ACKNOWLEDGEMENTS

I am grateful for the advice and guidance I've received from Benjamin Hansen, Glen Waddell, Caroline Weber, and Craig Parsons. This dissertation also benefits from helpful comments courtesy of seminar participants at the University of Oregon's Microeconomic Group, the Southern Economic Association Annual Meeting, and the Western Economic Association Annual Meeting. All remaining errors are my own.

To my parents, Dung A. Nguyen and Mai T. Pham.

TABLE OF CONTENTS

LIST OF FIGURES

Figure Page

LIST OF TABLES

Table | Page

CHAPTER I

INTRODUCTION

This dissertation consists of three papers in the field of public economics. Chapter II examines the determinants and consequences of death penalty support. Chapter III investigates the effect of benefit generosity on injury duration and claim costs in workers' compensation programs utilizing a regression kink design, while Chapter IV studies similar questions but with a difference-in-difference framework. Chapter II and Chapter IV are unpublished solo-authored articles while Chapter III is an unpublished co-authored article with Benjamin Hansen and Glen Waddell.

Chapter II is motivated by the recent decline in support for capital punishment in the U.S. and the hypotheses that have been proposed to explain this phenomenon. Previous research has considered the role of demographic characteristics in predicting support for capital punishment. For instance, being Caucasian, conservative, and living in non-urban settings have been linked to increasing support for capital punishment. In this chapter, I investigate the role of several novel factors in additional to demographic characteristics as predictors for individuals sentiment about the death penalty: exonerations, executions, and botched executions. Each of these factors could individually affect public view on capital punishment: witnessing an exoneration or a botched execution could reduce capital punishment support, possibly due to a revision in beliefs about the validity of the original conviction or a shock from learning about the cruelty of a botched execution, while executions could raise support for capital punishment as they tend to buttress a tough on crime" attitude in the criminal justice system. I analyze these factors jointly to account for possible correlations in their influence on death penalty sentiment.

Consistent with previous studies, I find strong evidence that individual characteristics are significant predictors of death penalty sentiment. Furthermore, being exposed to an exoneration in any given year significantly decreases support for the death penalty by about two to three percentage points. Nonetheless, I find no similar impact from executions or botched executions. I also find evidence that increases in exoneration serve to lower the number of death sentences given out by states, providing an interesting exhibit on the impact of exonerations not only on stated-preference outcomes such as public sentiment but also on revealed-preference based outcomes as well.

Chapter III estimates the impact of benefit generosity on injury duration and claim cost using data from the workers' compensation program in Oregon. Previous research on this topic has found large and significant effects of increasing benefit generosity on duration of injury and cost by exploiting large increases in benefit thresholds in states such as Kentucky, Michigan, and California using a difference-in-differences framework. Such results suggest that there is a strong moral hazard program associated with workers compensation: as benefit generosity increases, people tend to stay on workers compensation longer, resulting in costlier claims. On the other hand, recent studies has reexamined this effect and found that the moral hazard problem might not be as severe as previously documented.

We employ a regression kink framework to examine the impact of more generous benefits on duration of injury and cost. Our research design utilizes a feature that is common to most workers' compensation programs: a maximum threshold for weekly benefits. In Oregon, workers injured on the job are paid an amount equals to 2/3 of their weekly wage pre-injury (commonly referred to as a replacement rate); most crucial to our research design, this amount is not to exceed a specified maximum benefit level. Thus, individuals on one side of the maximum threshold experiences a replacement rate of 2/3 while others on the other side of the threshold experience a replacement rate of less than 2/3 (since their maximum benefits are now constant). Our analysis essentially boils down to a comparison of outcomes between those who are just below and above the maximum thresholds. Since this is a kink in the replacement rate, regression kink methodology is employed instead of the more common regression discontinuity.

Consistent with previous findings, we document an increase in injury duration and claim costs associated with more generous benefits, but the magnitude of such effect is smaller than those found previously. In particular, a 10% increase in generosity is predicted to increase injury duration by about 2 to 4%, less than the increase of 8 to 10% in injury duration reported in past studies using diff-in-diff estimation. We also find that more generous benefits lead to an increase in the probability of workers reinjuring themselves once they come back to their jobs, a form of ex-ante moral hazard that has not been documented in the literature. Our results have strong implications for states that are considering reforming workers' compensation programs due to rising costs. Specifically, we believe that there are significant responses associated with potential reductions in benefit generosity in the form of declines in injury duration, claim cost, and re-injury probability.

In Chapter IV, I revisit the topic of benefit generosity and moral hazard by examining the impact of a large increase in workers' compensation maximum benefit in Oregon. Workers injured before January 1, 2002 receive maximum benefits equal to 100% of the state's average weekly wage (SAWW); this threshold is subsequently updated to be 133% of the SAWW after January 1, 2002. I utilize the difference-in-differences research design to study the effect of this large change in generosity on injury duration, claim cost, and claim filing behavior.

This methodology mimics earlier studies in the literature connecting generosity to moral hazard. Indeed, previous research suggests large injury duration - maximum benefit elasticities: a 10% increase in benefit is associated with an 8 to 12% increase in duration on workers' compensation. The results in my paper allow for an easy comparison of the magnitude of such elasticities between Oregon and other states with similar benefit increases, such as Kentucky, Michigan, and California. Moreover, these results also shed light on the differences between studies using a more traditional difference-in-differences variation versus those utilizing more recent econometric framework such as the regression kink model in Chapter III.

Finally, Chapter V offers a summary of my research agenda, reviews the contributions of my research to the existing literature, suggests possible extensions to the previous chapters, and lists several additional ongoing projects.

CHAPTER II

DETERMINANTS OF PUBLIC OPINION ON THE DEATH PENALTY

Introduction

Public opinion regarding the death penalty has fluctuated significantly since the 1970s, peaking in its support during the mid-1990s and steadily declining ever since. A recent report by the Pew Research Center estimates that while 78% of all Americans were in favor of having the death penalty in 1995, the corresponding number in 2015 was about 56%.[1] A similar conclusion can be drawn from Figure 1, which plots the favorability of capital punishment using data from the General Social Survey: there are upward movements in favorability until the mid 1990s, followed by a period of steady decline. Nonetheless, there are recent indications that the public is changing its view again on capital punishment: voters in California, Nebraska, and Oklahoma all approved measures to bring back the death penalty during the 2016 election.[2]

This paper examines the role of individual characteristics as well as state-level measures of exonerations and executions (both successful and botched) in influencing the public's death penalty support. While previous research has shown that demographic factors are strong predictors of death penalty sentiment, the role of exonerations and executions has not been examined. Each of these factors could individually affect public view on capital punishment: being exposed to an exoneration or a botched execution could reduce capital punishment support, possibly due to a revision in beliefs about the validity of the original conviction or a shock from learning about the cruelty of a botched

[1] Pew Research Center, "Less Support for Death Penalty, Especially Among Democrats", `http://www.people-press.org/2015/04/16/less-support-for-death-penalty-especially-among-democrats`

[2] Additionally, California voters also approve a measure to speed up the process of execution.

FIGURE 1. Death Penalty Favorability

Notes: recoded from the following General Social Survey question: “Do you favor or oppose the death penalty for persons convicted of murder?” “Do Not Know” or “Not Available” responses are discarded.

execution, while executions could raise support for capital punishment as they tend to buttress a “tough on crime” attitude in the criminal justice system. I analyze these factors jointly to account for possible correlations in their influence on death penalty sentiment.

This study contributes to several established areas of research. First, it adds to the literature on the determinants and consequences of death penalty sentiment. Past studies have identified elements such as race, gender, and the crime environment as important components in influencing sentiment regarding the death penalty.[3] In addition to these conventional demographic characteristics, I consider the role of exonerations and executions (successful or not) in affecting public sentiment. I also show that there is a relationship among exonerations, falling sentiment in favor of the death penalty, and a reduction in the number of death sentences, which adds to existing evidences on the legislative and judicial consequences of a changing public perception regarding the death penalty.[4]

Second, this paper contributes to the recent literature in economics examining the impact of being exposed to stigma-attaching events. Since exonerations and executions tend to attract widespread media attention, exposure to such an event could have comparable effects to other types of scandal. Studies suggest that stigma-attaching events can carry immediate and long-lasting consequences; for example, the Catholic Church’s sex abuse scandal in the United States has been shown to lead to a significant fall in the Catholic population (Hungerman (2013)) and a decrease in charitable contributions, both in the short and long run (Bottan and Perez-Truglia (2015)). Studies in the political

[3]Soss et al. (2003) find that in addition to individual characteristics, racial tension, as measured by proximity between white and black populaces, is a significant predictor for death penalty support among whites.

[4]For instance, Brace and Boyea (2008) find that in states where Supreme Courts are elected instead of appointed, public support for capital punishment has a significant influence on the court’s composition and its willingness to uphold convictions with a death penalty sentence.

science literature similarly conclude that scandals can have a negative impact on the evaluation of a political candidate or the probability of being elected.[5]

If it is the case that individuals alter their sentiment about capital punishment after being exposed to an exoneration or execution, they must be updating their beliefs in the presence of new information. There is an extensive literature, in both theory and empirics, on belief-updating and its implications. The third contribution of my study is to the advancement of a recent literature measuring the impact of plausibly exogenous events on the process of learning and belief-updating. For instance, it has been shown that new information on health conditions in the form of tests or screenings lead to changes in belief and behavior among HIV patients (Godlonton and Thornton (2013), among others) and at-risk cancer patients (Lange (2011)).

Using comprehensive data from the National Registry of Exonerations, the Death Penalty Information Center, and the General Social Survey from 1980 to 2014, I find that living in a state that has an exoneration significantly decreases the probability that an individual will support the death penalty by two to three percentage points, a 3% decrease from the average level of support during the time period in my data. Nonetheless, exposure to successful or botched executions does not seem to affect how people feel about capital punishment.

Next, I investigate whether the change in opinion on capital punishment leads to additional changes in the criminal justice system. While individuals could change their view on the death penalty, it is not clear that such a change in stated preference could lead to a significant response when examining revealed-preference based outcomes. Consistent with my previous findings, exonerations have a contemporaneous impact on

[5]See, for example, Basinger (2013) or Vonnahme (2014).

reducing states' annual number of death sentences while executions, botched or successful, do not.

Flanagan et al. (1996) synthesizes previous research in criminology and political science regarding the values of understanding public opinion on crime and justice and argues that it is important for three reasons: it presents a record of historical trends, it may foreshadow legislative shifts, and it reveals a social attitude regarding the acceptable range at which citizens are comfortable with government intervening programs. Indeed, the declining public sentiment in favor of the death penalty has been followed by its legislative abolition in a number of states (e.g. Illinois in 2011, Connecticut in 2012, and Maryland in 2013) while many more are considering either complete abolition or further restrictions on capital punishment as a criminal sentence.[6] Thus, in addition to uncovering the impact of events such as exonerations and executions on death penalty sentiment, this paper has important policy implications; specifically, the findings of this study could be of particular relevance in informing policy makers on the causes and consequences of legislation aiming to abolish the death penalty. As more states have begun to reexamine the need for capital punishment, it is crucial that legislators be advised about the drivers behind the decline in death penalty support and any possible consequence from such decline, such as a decrease in the number of death sentences or a changing political landscape.

The rest of the paper proceeds as follows: Section 2 provides a modern background on the death penalty, exonerations, and executions in the United States and reviews the relevant literatures on the determinants and consequences of death penalty support, the impact of stigmatic events, and belief-updating behaviors. Section 3 describes the data set

[6] Oregon, for example, has been maintaining a moratorium on administering death penalties since 2011. For a detail list of state restrictions on capital punishment as a possible sentence, see National Conference of State Legislatures, "States and Capital Punishment" `http://www.ncsl.org/research/civil-and-criminal-justice/death-penalty.aspx`

and the main regression specification. Estimation results are presented in Section 4, and Section 5 concludes.

Background

The Death Penalty, Executions, and Exonerations in the U.S.

The death penalty has been a controversial subject in America. Proponents of death penalty sentencing often argue that it is the only appropriate punishment for extraordinarily heinous crimes and that it serves to deter future criminals. Opponents counter that it is inhumane, expensive, and arbitrary. While the debate about capital punishment as an appropriate sentence is more philosophical in nature, ample empirical evidences have been provided on the deterrent effect of the death penalty, often with contradictory implications. For example, Dezhbakhsh et al. (2003) and Dezhbakhsh and Shepherd (2006) find evidence of a deterrence effect in the form of a reduction in murder rates while Katz et al. (2003) find little evidence that higher execution rates lower crime rates. Donohue III and Wolfers (2006) and Donohue and Wolfers (2009) study the impact of capital punishment and conclude that existing data does not offer clear evidence of either a deterrent or antideterrent effect and stress that they are "profoundly uncertain" regarding the presence of a deterrent effect from increased executions.

Mirroring the uncertainty in the effectiveness of death penalty sentencing, its constitutional legality has been called into question numerous times. In *Furman v. Georgia* (1972), the Supreme Court ruled that the use of capital punishment on William Henry Furman, a home burglar who murdered a house owner and was convicted after a one-day trial, constituted "cruel and unusual punishment" and therefore was a violation of the Eighth Amendment. This effectively put a moratorium on all but a few death sentences in the U.S. and forced states to rewrite legislation to avoid problems with "arbitrary and capricious" death penalty sentencing.[7] 34 states subsequently passed new statues conforming to this requirement. In *Gregg v. Georgia* (1976), the Supreme Court ruled that new death penalty statues in California, Florida, and Texas were constitutional, thus leaving the choice to maintain capital punishment to state legislatures.[8] Currently, the use of death penalty is legal in 31 out of 51 states, and in all but a few states a death penalty sentence is reserved for only the most severe of offenses, namely, murder.[9]

As the nature of capital punishment legislation evolved, so did the methods of executing prisoners. Recent trends indicated that older, more cruel methods such as electrocution or gas chamber were being phased out in favor of lethal injection, which was perceived to be more humane.[10] To provide a snapshot of executions throughout the years, Figure 2a plots the national execution rate (per 100,000 residents) from 1980 to 2015 using data from the Death Penalty Information Center.[11] Executions steadily increased up until 2000, then fell back to early-1990s level. It is worth noting that the distribution of executions is uneven across states: since 1976, Texas has executed more prisoners (538 out of 1,438 total) than the next six highest states combined.

One of the many proposed explanations for the decline in death penalty support in recent years is the persistent problems in administering executions.[12] Sarat (2014)

[7] "Arbitrary and capricious" was the original language used in determining the the death penalty was unconstitutional during *Furman v. Georgia*.

[8] While there is a federal death penalty, very few cases where federal prosecutors sought a death penalty sentence actually resulted in an execution. Most of these cases settled before an inmate was sent to death row in the form of an acquittal or a reduction of sentence to life in prison.

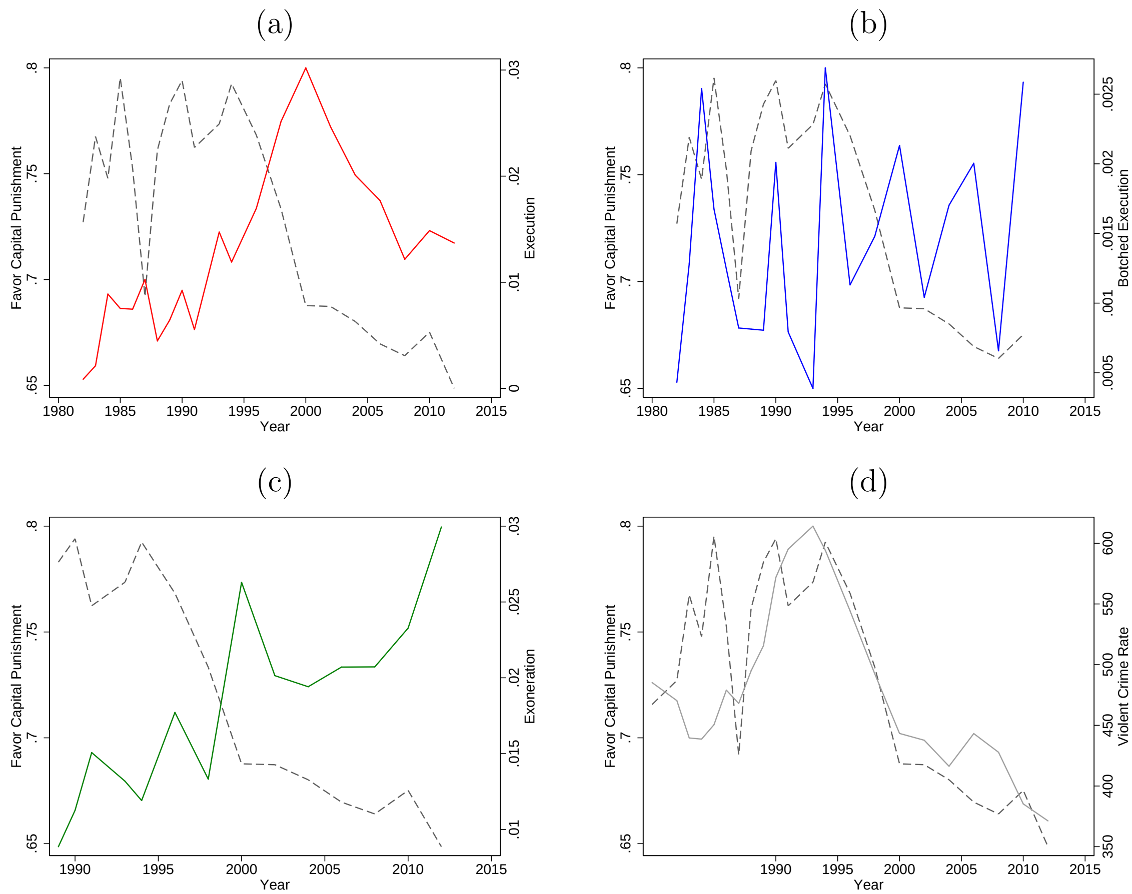
[9] "Murder" in this context includes, but not limited to, capital murder, first-degree murder, and aggregated murder.

[10] Nonetheless, recent shortages of lethal injections have forced states to consider bringing back previous execution methods.

[11] Sentiment on the death penalty (Figure 1) is superimposed on this graph, as well as subsequent graphs, to facilitate easy comparisons.

[12] Time Magazine, "The Death of the Death Penalty"

FIGURE 2. Trends in the Criminal Justice System



Notes: data from Death Penalty Information Center (Panel a), *Gruesome Spectacles* by Austin Sarat (Panel b), National Registry of Exonerations (Panel c), and Uniform Crime Report (Panel d). Sentiment regarding capital punishment (Figure 1) is in dashes.

documents the many incidents of botched execution in the U.S. and estimates that 3% of all executions were botched. Among all methods of execution, lethal injection has the highest rate of failure (about 7%), followed by lethal gas (5.4%) and hanging (3.12%). Notable cases of botched execution can attract widespread media attention and serve as reminders about the potential cruelty of capital punishment. Indeed, in the wake of a gruesome botched execution in Oklahoma in 2014,[13] President Barack Obama called for a reexamination of the way executions were being carried out in the U.S.[14] Figure 2b plots the rate of botched execution per 100,000 residents using information from Sarat (2014): in spite of several notable spikes, there does not seem to be a discernible pattern in the time series.

Another hypothesis for the decrease in public sentiment in favor of capital punishment is the possibility of executing an innocent person. Using survival analysis, Gross et al. (2014) estimate the probability of a wrongful death sentence to be about 4%, conservatively. Additionally, since 1973, 156 death row inmates were eventually exonerated.[15] Together, these statistics strongly suggest that there is a distinct possibility capital punishment can lead to the execution of an innocent person, a fact documented extensively with case studies of previous death sentences by Radelet and Bedau (1998). Huff (2002) surveys personnels in the criminal justice system and points out several possible causes for wrongful convictions: eyewitness errors, overzealous police and prosecutors, false and coerced confessions, and ineffective assistance of counsel. He makes several policy recommendations to reduce these instances; among them the abolition of the

[13]Clayton Lockett was believed to be in pain for more than 40 minutes before dying from a heart attack due to his body's reaction to a new mix of lethal injection that had not been used before.

[14]The Atlantic, "The Cruel and Unusual Execution of Clayton Lockett"

[15]Source: Death Penalty Information Center

death penalty, enactment of measures to compensate those who are wrongfully convicted, and an increase in the use of DNA testing whenever possible.

There has been a steady increase in the number of exonerations,[16] with and without the help of DNA testing. For instance, only one fifth of all exonerations in 2015 relied on DNA evidence while the rest were due to non-DNA factors such as false confession, mistaken witness identification, or official misconduct. The National Registry of Exonerations (NRE) explains that this is due to two reasons: first, the share of DNA-related exonerations will go down as information about less well-known exonerations (those without DNA evidence) become more available. Second, it is increasingly more likely that DNA evidence is utilized before trial, not after, and thus the role of DNA in exonerating the wrongfully convicted post-trial has been diminished. Figure 2c plots the national rate of exoneration with data from NRE.

Determinants and Consequences of Death Penalty Support

Americans have consistently expressed support for the use of capital punishment. Historical data by Gallup indicates that except for the two decades between 1950 and 1970, where death penalty support hovered around 50%, the favorability of capital punishment as a possible sentence for persons convicted of murder has always been above 50% since the 1930s.[17] Consistent with Figure 1, the Gallup data shows a peak in support in the early 1990s at 80% and a decline back to low 60s ever since.

[16] There are various definitions of what exactly qualifies as an exoneration. The National Registry of Exoneration, for example, defines an exoneree as a person who "was convicted of a crime and later was either: (1) declared to be factually innocent by a government official or an agency with an ability to make the declaration or (2) relieved of all the consequences of the criminal conviction by a government official or body with the authority to take that action.

[17] Gallup, "Death Penalty Historical Trends." `http://www.gallup.com/poll/1606/death-penalty.aspx`

Previous studies have documented several determinants of this support. Traditionally, individual factors such as gender, race, place of residence, and political affiliation have been shown to significantly influence death penalty sentiment. For instance, Bohm (1991) and Longmire (1996) both attribute increasing support for capital punishment to being white, conservative, male, and living in a non-urban setting. Using data from the General Social Survey, Stack (2000) finds that increasing racial prejudice is associated with higher death penalty support. Similarly, using Census data, Soss et al. (2003) discover that racial prejudice, as approximated by the black percentage of county residents, is a significant predictor for capital punishment favorability among whites. In my analysis of the determinants of public opinion on the death penalty, I include common demographic controls such as age, gender, race, marital status, education, party affiliation, as well as sentiment on the harshness of the criminal justice system. My findings are broadly in line with previous research regarding the impact of these factors on the favorability of the death penalty.

Crime, or the fear of crime, has often been examined as a predictor for death penalty support. While Warr (1995) and Gross (1997) find little correlation between public perception on crime rate and penalty support, other studies have offer contradicting evidences. For example, Sims and Johnston (2004) observe that fear of crime is associated with higher death penalty support, though this impact is less than the impact of demographic factors. Additionally,Keil and Vito (1991) find that the fear of crime in a neighborhood results in greater support for capital punishment, and that it plays a crucial role in mediating the impact of other demographic factors.

To facilitate a visual comparison between crime rate and death penalty support, Figure 2d plots the national violent crime rate, again, per 100,000 residents, from 1980 to 2014 using data from the Uniform Crime Report. We can observe some resemblances to Figure 1: crime rates increase up until the mid 1990s and decrease ever since, which lends credence to the theory that violent crime rate is an important factor to consider in explaining death penalty sentiment; indeed, my analysis points to a significant contemporaneous relationship between the two factors. In all but a few specifications, I control for the state-level violent crime rate to net out any confounding impact on death penalty sentiment due to changes in the crime environment.

One might wonder if changes in support to the death penalty, a stated-preference variable, could have consequences on revealed-preference based outcomes. Previous research has found a linkage between public opinion on capital punishment and policy and practice at the state level (Norrander (2000), Fisher and Pratt (2006)). More relevant to this study, Brace and Boyea (2008) show that public opinion can have a direct and indirect impact on the judicial system. In states where Supreme Court judges are elected instead of appointed, they find that judges are more likely to uphold capital punishment sentences when the state is more in favor of capital punishment. They take this as evidence of a direct impact of public sentiment on the death penalty. Furthermore, they show that public sentiment can also have an indirect effect by way of changing the composition of Supreme Courts; that is, elective states with higher pro-capital punishment sentiment tend to vote for more conservative judges, thus increasing the likelihood that capital punishment sentences will be upheld. These findings build upon work by Huber and Gordon (2004), who show that judges in elective states tend to become more punitive as elections approach, suggesting that public sentiment plays an important role in influencing the judicial process.

In this study, I consider the effect of individual demographic factors and state-level measures of exoneration, executions, and botched executions on influencing public opinion regarding the death penalty. To the best of my knowledge, the role of exonerations and executions in changing death penalty sentiment has not been previously considered. I analyze these factors jointly to account for any correlation among them in changing public perception. For example, a higher rate of exoneration could coincide with a higher crime rate, since there are presumably more arrests and more wrongful convictions, and so omitting either variable will bias the coefficient estimate for the other variable. Furthermore, one could imagine a scenario where execution and botched execution have opposite effects on death penalty sentiment, as the former could be considered an indicator of a "tough on crime" attitude and thus more likely to increase death penalty support while the latter would decrease support due to its gruesome and public nature. Additionally, my study also contributes to the research on the consequences of a changing public perception on the death penalty by documenting a reduction in the number of death sentences as a result of being exposed to more exonerations.

Stigma-Attaching Events and Belief-Updating Behaviors

An exoneration or an execution might be considered a stigma-attaching event given the associated publicity. There is an established literature measuring the impacts of such events on a number of wide-ranging outcomes. Studying the religious market after the Catholic church sex abuse scandal in the early 2000s, Hungerman (2013) finds that such scandals are responsible for a fall of two million members in the Catholic Church and a $3 billion decrease in donation to the Church that is offset by an increase in non-Catholic participation and donations. Similarly, Bottan and Perez-Truglia (2015) find that within a zip code that witnesses a clergy sex scandal there is a 3% decline in religious participation and 1.3% decline in charitable donations while Dills and Hernández-Julián (2012) document a decrease in the availability of Catholic schooling steaming from the same scandals. Research in the political science literature also suggests that scandals involving politicians can affect a candidate's evaluation (Vonnahme (2014)), political support (Maier (2011)), and the probability of being reelected (Basinger (2013)).

If individuals increase their opposition to the death penalty after witnessing an exoneration or an execution, they must be engaging in belief-updating behaviors in the presence of new information. This paper therefore contributes to the literature on belief-updating as a result of being exposed to plausibly exogenous events. In an experiment in Malawi, Godlonton and Thornton (2013) find that individuals' perceptions about HIV risks alter significantly after learning about the infection status of their friends and neighbors from the experiment. Along the same dimension, Lange (2011) shows that exposure to new medical evidence leads to an increase in cancer risk perception and a subsequent increase in willingness to undertake additional tests and screenings. Oster et al. (2013) study the relationship between behavior and belief in the opposite direction and find that testing rates are higher for individuals with higher ex ante risks of Huntington disease, suggesting that beliefs can also strongly influencing behaviors. Similarly, Paula et al. (2014) find that downward revisions in the belief of being HIV positive can lead to an increase in risky behavior.

The findings in this paper adds to the literature on the impact of stigma-attaching events and belief-updating behaviors. A change in support for the death penalty as a result of being exposed to an exoneration or an execution (botched or not) would provide additional evidence on the impact of stigmatic events; moreover, such results also suggest that individuals choose to update their beliefs and behaviors in the presence of new information.

Data and Methodology

Data

My data on exonerations comes from the National Registry of Exoneration, a project of the University of Michigan Law School. This data set tracks instances of both DNA and non-DNA exonerations in the United States from 1989 to 2014 and classifies them according to a number of factors, such as location, type of crime, reason for conviction overturn, demographic characteristics of the wrongfully convicted, and sentence length.[18] My exoneration measure will be derived primarily from this data set. Data on all executions in the U.S. is obtained from the Death Penalty Information Center, while information on botched executions comes from Sarat (2014). Violent crime rate, to be used as a state-level control variable, is from the Uniform Crime Report, a data collection agency operated under the direction of the FBI. Collectively, my main independent variables of interest are balanced panels of exoneration, execution, and botched execution from 1980 to 2014.[19]

One of my dependent variables, sentiment on the death penalty, come from the General Social Survey (GSS), an extensive data set cultivated by the National Opinion Research Center at the University of Chicago. Every two years, one in roughly 50,000 households is selected to participate in the GSS, and one member in each household is selected to answer the questions in the survey. Questions on the GSS range from personal characteristics to attitude towards religion, culture, politics, law enforcement,

[18] To the best of my knowledge, this is the most extensive database on exonerations in the U.S., but due to nature of tracking exonerations this data set is continuously updated at different point in times. This should not have an effect on my results since most updates to the data address the most recent exonerations and few concern exonerations in the past. The data set used in this paper is current as of February 2016.

[19] Additional checks reveal that property crime rates, the other type of crime rate in the Uniform Crime Report, do not affect sentiment on the death penalty and thus they are not considered in my analysis.

and the criminal justice system, the last two of which are particular relevant to my research question. I use the confidential version of the GSS (which includes information on respondents' places of residency at the time of survey) to match survey participants to exonerations and executions happening within the same geographic boundaries within the same years. The analysis in the following sections classifies both exoneration and execution exposure at the state level, though one could imagine them being classified at the county level as well. The main concern with county-level classification is that there is a possibility that news about an exoneration or an execution travels across county lines more easily than state lines. My analysis essentially boils down to a comparison in sentiment between the treatment group, which is exposed to an exoneration or an execution, and a control group, which is not; thus, county-level classification could lead to the control group being partially treated and so the true impact will be underestimated.

During the early years of my sample (from 1980 to 1993), the GSS surveyed respondents almost every year. Beginning in 1994, it switched to a biennial model where surveys were conducted in even-numbered years only. While this is not a concern during the examination of the contemporaneous impact of exonerations and executions on death penalty sentiment, one could imagine that the effect can last several years due to the prolonged nature of such events and thus leads and lags of exoneration and execution should be included in the model. This creates a challenge in merging GSS and exoneration and execution data due to the unbalanced gap in GSS survey years.

To address this problem, I first create a balanced panel of exonerations and executions and generate (up to two) leads and lags for each variable. I then merge this data set to GSS data. This ensures that, despite the uneven gap in GSS survey years, the leads and lags of my explanatory variables always correspond to the years immediately before and after an event. That is, each person in GSS surveys is associated with the executions and exonerations in the immediate two years preceding and following the year he or she is surveyed.

Lastly, to measure the impact of exonerations and executions on revealed-preference outcomes, I utilize data on death sentences from the Bureau of Justice Statistics' annual reports on capital punishment, curated by the Death Penalty Information Center.

Methodology

My baseline regression model is:

$$\begin{aligned} Support_{ist} = X_{ist}\beta + \delta_1 Exoneration_{st} + \delta_2 Execution_{st} \\ + \delta_3 Botched\_Execution_{st} + \alpha_s + \gamma_t + u_{ist} \end{aligned} \tag{2.1}$$

where $Support_{ist}$ is a binary indicator that equals 1 if an individual i living in state s in year t[20] states that he or she supports the use of the death penalty as a possible sentence for murder convictions, X_{ist} is a vector of individual characteristics, and α_s and γ_t are state and year fixed-effects, respectively. $Exoneration_{st}$, $Execution_{st}$, $Botched\_Execution_{st}$ are some measures of exposure to those events at the state level. Consistent with previous research, the matrix of control variables includes age and binary indicators for gender, race, income, work status, education, political party affiliation, and general view on the criminal justice system. I use two classifications of exoneration and

[20]While exposure to exonerations and executions could be defined at smaller localities such as counties, such definitions are likely to be problematic for two reasons: first, news about exonerations and executions could potentially spread across counties more easily than across states, and thus the chance of the control group receiving treatment is higher under a more restricted event definition. Additionally, GSS data is more representative at the state level than at the county level. All 51 states are covered in the GSS (though some are omitted during specific years) while only 447 counties out of roughly 3,000 U.S. total are covered.

execution exposure: a binary indicator that equals 1 if there is an event in state s during year t and a count of exoneration, execution, and botched execution per 100,000 residents of state s in year t. While the interpretation of the coefficient estimates differs across the two classifications, statistical significance remains rather consistent. Lastly, since the variation of my independent variable is at the state level, standard errors are clustered at that level.

To investigate the impact on death sentence, I run the following regression model:

$$\begin{aligned}\Delta Death\_sentence_{st} &= \Delta X_{st}\beta + \delta_1 \Delta Exoneration_{st} + \delta_2 \Delta Execution_{st} \\ &\quad + \delta_3 \Delta Botched\_Execution_{st} + u_{st} \qquad (2.2)\end{aligned}$$

Since the death sentence data is at the state level, GSS individual data is also aggregated to the state level to be utilized as controls. In some specifications, I also include state fixed effects, which is equivalent to models in level with state-specific time trends. A first differencing model is chosen over an aggregated state-level fixed effects specification because of concerns regarding the impact of time-persistent factors that cannot be easily captured by year fixed effects as well as state-specific linear time trends. Indeed, since the time period in my data set spans over 30 years (1980 - 2014), there is a possibility that sentiment on the death penalty during this extended period evolved in a manner that is not adequately explained by common fixed effects and linear trends, and thus a first difference model is my preferred specification when analyzing state-level data.

Finally, to measure the prolonged impact of exoneration and execution, Equations (2.1) and (2.2) are augmented to include two lags and leads of each of those variables. I find some evidence of lag impact, and more importantly, little evidence of lead impact, which confirms that the results are not driven by trends that are unaccounted for in the models.

Results

Summary statistics are provided in Table 1. Examining Panel A, we can see that the average age at conviction for an exoneree is 28.45 and that about a quarter of exoneration is achieved with the help of DNA evidence. Looking across exoneration characteristics, murder conviction accounts for 41% of all exonerations, 25% of all exonerees originally receive life sentences (with and without the possibility of parole), 7% receive the highest possible sentence (death), and almost half of all wrongfully convicted individuals are African Americans (47%). Panel B and C indicate that the average age of an executed prisoner is 41.32, and that most of executions and botched executions are via lethal injections.

Panel D presents summary statistics for GSS data. Survey respondents are 45 years old on average. Males account for less than half of all respondents (45%), while the proportion of Caucasians is 80%. A majority of the sample have at least a high school degree (81%), half are married, and less than half self-describe as Democrats (47%). 73% have favorable views of the death penalty, a high level of support relative to recent years.

Determinants of Death Penalty Sentiment

The main regression results are provided in Table 2. Here, exonerations and executions are classified as binary indicators: that is, the independent variables $Exoneration_{st}$, $Execution_{t}$, and $Botched\_Execution_{st}$ are 1 if there is an exoneration, execution, or botched execution occurring in state s in year t and 0 otherwise. The top

TABLE 1. Summary Statistics
Exonerations, Executions, and Death Penalty Sentiment

	Mean	Median	Std. Dev
Panel A: Exonerations, N = 1,797			
Age	28.62	26	10.16
DNA	0.23	0	0.42
Murder	0.41	0	0.49
Life	0.25	0	0.43
Death	0.07	0	0.25
Black	0.47	0	0.50
Panel B: Executions, N = 1,436			
Lethal Injection	0.88	1	0.33
Black	0.34	0	0.48
Panel C: Botched Executions, N = 102			
Lethal Injection	0.72	1	0.45
Electrocution	0.25	0	0.44
Panel D: GSS, N = 48,947			
Age	44.54	42	16.99
Male	0.45	0	0.50
White	0.80	1	0.40
High school	0.81	1	0.39
Married	0.59	1	0.49
Democrats	0.47	0	0.50
Support death penalty	0.73	1	0.44

Sources: National Registry of Exonerations (Panel A), Death Penalty Information Center (Panel B), *Gruesome Spectacles* by Austin Sarat (Panel C), and the General Social Survey (Panel D). Summary statistics in Panel D are weighted by the probability weights provided by GSS.

panel of Table 2 displays the impact of state-level measures of exoneration and execution while the bottom panel shows the effect of demographic characteristics.

Results in column (1), which are from a regression that only includes individual characteristics, are broadly consistent with previous findings about determinants of death penalty sentiment. Being older or a member of the Democratic party significantly decreases support for capital punishment while being male, white, in the top income bracket,[21], a full time worker, married, having a high school degree and thinking the court system is too lenient on criminals (a proxy for attitude on the criminal justice system) are all predicted to increase death penalty support, and all but full time work status are significant that the 5% level. Column (2) adds to this specification exonerations, executions, and botched executions, while column (3) augments the model with violent crime rate as an additional control. We can observe that there are little changes to the coefficient estimates or statistical significances for variables in the bottom panel across the three columns; unsurprisingly, demographic characteristics are strong predictors of public opinion regarding capital punishment.

Looking at column (2), exposure to an exoneration is predicted to decrease support for the death penalty by about 2.7 percentage points, and this result is quite robust to the inclusion of violent crime rate in column (3), though the magnitude of the coefficient estimate decreases in the presence of this additional control. Given that the average level of capital punishment support is 73%, being exposed to an exoneration significantly decreases the probability of supporting the death penalty by about 3.2% relative to the mean in the most comprehensive specification (column 3). Neither execution nor botched execution exposure seems to have an impact on death penalty sentiment in either column.

[21] GSS defines the top income bracket as $25,000 or above per year. This low threshold leads to about half of the sample being classified as being in the top income bracket.

TABLE 2. Determinants of Death Penalty Sentiment

VARIABLES	(1)	(2)	(3)
1(Exoneration)		-0.0271***	-0.0240***
		(0.0062)	(0.0061)
1(Execution)		0.0095	0.0053
		(0.0140)	(0.0131)
1(Botched Execution)		0.0183	0.0201
		(0.0130)	(0.0129)
Age	-0.0006**	-0.0006**	-0.0006**
	(0.0002)	(0.0002)	(0.0002)
1(Male)	0.0776***	0.0775***	0.0776***
	(0.0063)	(0.0062)	(0.0062)
1(White)	0.1175***	0.1175***	0.1146***
	(0.0215)	(0.0216)	(0.0216)
1(Top Income Bracket)	0.0770***	0.0761***	0.0759***
	(0.0203)	(0.0202)	(0.0202)
1(Full Time Worker)	0.0155	0.0163	0.0175
	(0.0196)	(0.0194)	(0.0193)
1(Married)	0.0515***	0.0515***	0.0514***
	(0.0068)	(0.0068)	(0.0068)
1(High School Degree)	0.0592***	0.0589***	0.0586***
	(0.0131)	(0.0132)	(0.0129)
1(Democrats)	-0.0469***	-0.0469***	-0.0466***
	(0.0047)	(0.0047)	(0.0046)
Observations	35,133	35,133	35,133
State & Year FEs	Yes	Yes	Yes
Crime Rate	No	No	Yes

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Independent variable: 1 if there is an event in a given state and year, 0 otherwise. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 3. Determinants of Death Penalty Sentiment, Robustness Checks

VARIABLES	(1)	(2)	(3)	(4)	(5)
1(Exoneration)	-0.0240***	-0.0221**	-0.0220***	-0.0269***	-0.0251***
	(0.0061)	(0.0062)	(0.0050)	(0.0066)	(0.0065)
Observations	35,133	35,133	22,927	35,133	30,752
Crime Rate	Yes	Yes	Yes	Yes	Yes
State-linear trends	No	Yes	No	No	No
Post 1990	No	No	Yes	No	No
Region-by-Year Fixed Effects	No	No	No	Yes	No
Excluding Texas & New York	No	No	No	No	Yes

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Independent variable: 1 if there is an event in a given state and year, 0 otherwise. Each cell is a separate regression. All regressions include state and year fixed effects as well as control variables in the bottom panel of Table 2. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 3 investigates the robustness of exoneration exposure to various model modifications. Column (1) reproduces the baseline result in column (3) of Table 2, while columns (2) to (5) examine results when a state-specific linear time trend is included, when the sample is restricted to only include years since 1990 (when the majority of exonerations took place), when region-by-year fixed effects are included, and when outliners such as Texas and New York, the two states with the highest number of exonerations, are excluded. We can see that the estimated effects of exonerations are quite stable across these specification changes, and that all are significant at the 1% level.

Table 4 replicates the specifications in Table 2 except that exonerations and executions are now defined as rates per 100,000 state residents instead of binary exposure indicators to capture the effect of having an additional exoneration instead of having any exoneration at all.[22] The main implications of Table 2 for state-level measures are retained: mainly, there is a significant impact of exonerations on death penalty sentiment while the effects of executions are indistinguishable from zero. For brevity, the coefficient estimates for demographic factors are not reported in this table but they are very similar to those found in the top panel of Table 2. To make sense of the coefficient estimates for exoneration, consider the increase in exoneration rate from 0.011 per 100,000 people in 1990 to 0.022 in 2010 (this increase roughly corresponds to exoneration rate doubling from its average level of 0.01). This rise in exoneration rate over 20 years is predicted to decrease support for the death penalty by about 0.4 to 0.6 percentage points given the coefficient estimates in Table 4.

To further alleviate concerns about the sensitivity of the results to the ways I am defining exonerations and executions, Table 19 in Appendix A reproduces columns (2) and (3) of Table 2, but exonerations and executions are defined as the number of those events in a state s in year t, not scaled by any population measurements. We can observe that the impact of exonerations in reducing support for the death penalty is still apparent. Furthermore, Table 20 in Appendix A uses three different indicators to define exoneration exposure: whether a GSS survey respondent observes one, two, or three or more exonerations in a year. The omitted indicator is one that indicates no exoneration exposure. Results in Table 20 suggest that exposure to any level of exoneration is a significant predictor of decreasing capital punishment support. Thus, in subsequent sections, I primarily utilize a dummy classification for whether an exoneration happens at all in a specific state and year, supplemented with classifications that scale exonerations by state populations.

[22] While there are many ways to define per-capita rates, per 100,000 state residents is selected so there is a consistency in measurement with respect to the crime rate control. Data on state population comes from the Census.

TABLE 4. Determinants of Death Penalty Sentiment
Event Count

VARIABLES	(1)	(2)	(3)
Exoneration	-0.5582***	-0.4577***	-0.3797**
	(0.1647)	(0.1375)	(0.1446)
Execution	0.0713	0.0784	0.0214
	(0.0821)	(0.0735)	(0.0704)
Botched Execution	1.0834	0.8830	0.8885
	(0.8600)	(0.6377)	(0.6575)
Observations	39,204	35,133	35,133
State & Year FEs	Yes	Yes	Yes
Controls	No	Yes	Yes
Crime rate	No	No	Yes

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Independent variable: number of events in a given state and year per 100,000 residents. Standard errors in parentheses, allowing for clustering at the state level. Controls include variables in the bottom panel of Table 2. *** significant at 1%; ** significant at 5%; * significant at 10%.

When a respondent is asked whether he or she supports capital punishment in the GSS, there are four possible responses: yes, no, and do not know.[23] One might wonder the impact on the results of the population choosing to not reveal their preferences for capital punishment. Table 21 in Appendix A reproduces the specification in column (3), but with various definitions for capital punishment support. Column (1) defines those who answer "do not know" as supporters of the death penalty and column (2) assigned this group to the opposite spectrum of capital punishment sentiment. We can observe that the impact of exoneration is bounded between 2.1 and 2.7 percentage points, both significant at the 1% level. Column (3) checks for the possibility that exonerations could increase the probability of answering "do not know" regarding support for capital punishment. The dependent variable in this regression is an indicator equals 1 if GSS respondents select "do not know" as an answer for the question about death penalty support. Results indicate that exonerations do not significantly increase the uncertainty regarding respondents' beliefs about the necessity of capital punishment.

An event study could be conducted to alleviate concerns about the endogeneity in the timing of exonerations and executions. For instance, exonerations could be the result of increasing public sentiment against the death penalty, or, in contrast, more death row prisoners could be executed as the public becomes more receptive to a "law-and-order" attitude regarding criminals and incidences of crime. Nonetheless, a conventional event study is difficult to implement due to the nature of these events. As most states experience periods of rapid succession in exoneration (and to a lesser extent, execution and botched execution), it is not clear how to define pre- and post- periods in accordance to standard event study practice. Instead, I augment Equation (2.1) with the inclusion of two lags and leads of each of the main independent variables to account for their impact

[23] The GSS also includes a category that indicates a respondent not being asked this question

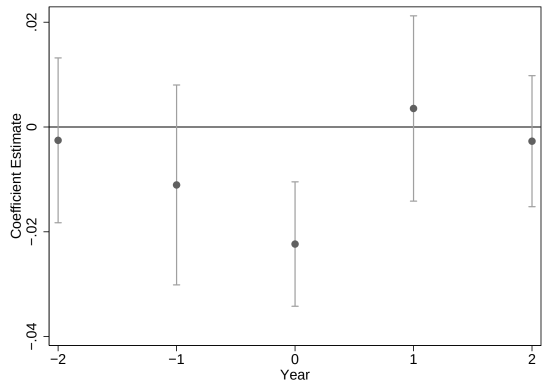
contemporaneously and beyond. Since exonerations and executions can occur over an extended time period,[24] this approach is well-suited to capture the prolonged impact of these events. Coefficients on lagged variables would measure the impact of exonerations and executions after their occurrences and could potentially be significant, which we could take as evidence of a spillover effect beyond the contemporaneous exposure. More problematic is the case where there are significant effects for leading variables. One could take it as evidence of an anticipation effect, but it is rather unlikely that survey respondents would be able to predict when an exoneration or an execution takes place in the future and react appropriately. Most likely, this is evidence of trends that have not been adequately captured in previous estimation. Fortunately, while I find evidence of lagged impact, the coefficients on lead variables are never significant at the 5% level.

Figure 3 plots the coefficients and 95% confidence intervals for the first two lags and leads of exonerations, executions, and botched executions. In each of these figures, exonerations and executions are defined as exposure indicators, and all point estimates and confidence intervals come from the same regressions. Examining Figure 3, we can see that exoneration exposure has both a contemporaneous impact and a decreasing residual impact, though the residual effects are insignificant. Furthermore, the contemporaneous impact is quite similar in magnitude to that in Table 2, suggesting that exonerations have the biggest and most direct impact during the years they actually happen. Executions do not seem to have an impact, contemporaneous or otherwise, while botched executions have a residual impact the year after they happen. Caution should be exercised in interpreting the coefficients on botched executions, however, since there are very few instances of them and they could be correlated with executions.

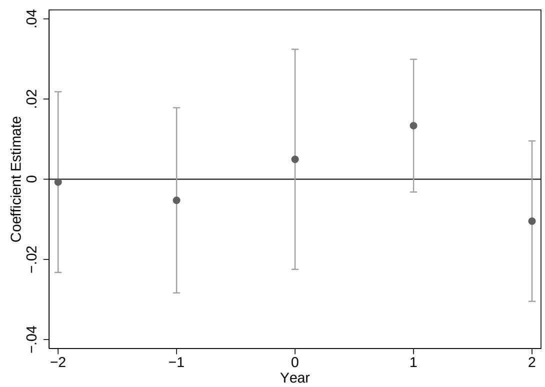
[24] The Innocence Project of Florida estimates that it takes, on average, 8 years for a successful exoneration to be carried out from beginning to end, while according to the Death Penalty Information Center it could take as much as 20 years (and possibly longer) from the time of sentencing and the execution of a death row inmate.

FIGURE 3. Lag and Lead Impact, Event Exposure

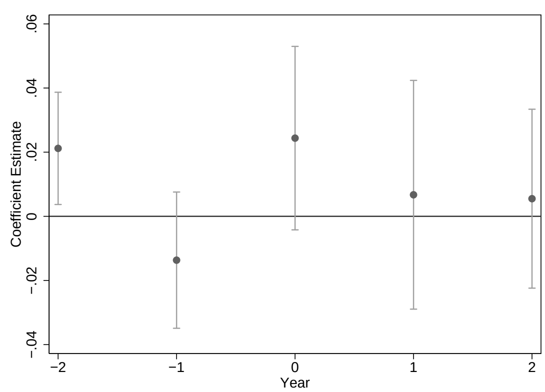
(a) Exoneration



(b) Execution



(c) Botched Execution



Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. All coefficients are obtained from a regression with control variables and state and year fixed effects. All lags and leads of exonerations, executions, botched execution, and violent crime are included. The 95% confidence intervals for each coefficient are also displayed.

Figure 4 presents results from the same analysis of lag and lead impact but with exonerations and executions being defined as rates instead of binary indicators. Again, the implications of previous findings remain: exonerations have a contemporaneous impact (though it is now only marginally significant) while the rest of the state-level independent variables do not. Additionally, exonerations now have a strong residual impact in the first year following their occurrences; nonetheless, such effect dissipates in the second year.

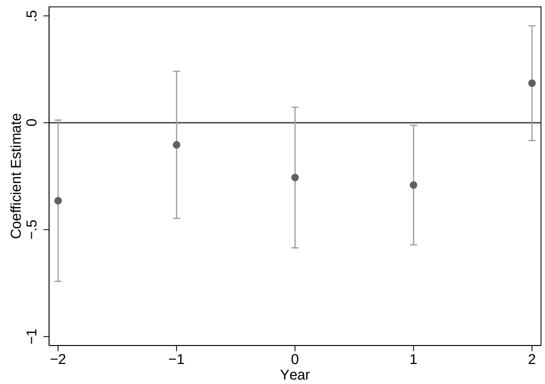
Heterogeneous Impact of Exonerations

Since exonerations are consistently identified as a significant predictor for death penalty sentiment, Table 5 and 6 presents heterogeneity analyses of this impact. Table 5 examines the effect of different types of exonerations while Table 6 analyzes the results when GSS survey respondents are split into different subgroups. In Table 5, only the exoneration variable in the baseline specification varies while the rest remains the same. Each cell is a separate regression, and all specifications utilize the binary classification of exonerations for ease of interpretation. For example, the coefficient estimate of -0.0119 in column (2) suggests that being exposed to any exoneration with a black exoneree decreases death penalty sentiment by 1.19 points, though this effect is not significant. Columns (1) to (4) explore sensitivity of the results to different exoneration classifications: by original crime, race, DNA help, and sentence.

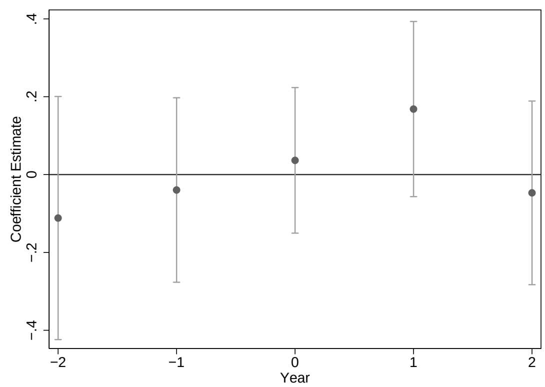
We can see that overturned convictions in murder and homicide appear to have a high impact in changing death penalty sentiment compared to sexual assault or drug-related exonerations, while the impact of black exonerees or exonerations with DNA evidence is indistinguishable from zero. Indeed, the impact of exoneration appears to be driven by non-DNA cases, possibly because the use of DNA evidence has become more

FIGURE 4. Lag and Lead Impact, Event Count

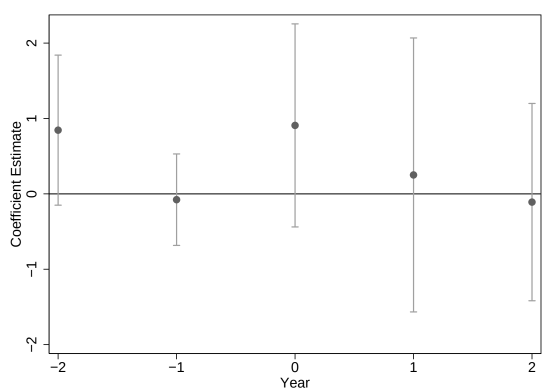
(a) Exoneration



(b) Execution



(c) Botched Execution



Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. All coefficients are obtained from a regression with control variables and state and year fixed effects. All lags and leads of exonerations, executions, botched execution, and violent crime are included. The 95% confidence intervals for each coefficient are also displayed.

TABLE 5. Heterogeneous Impact of Exonerations
by Exoneration Characteristics

VARIABLES	(1) Crime	(2) Race	(3) DNA	(4) Sentence
1(Murder & Homicide)	-0.0171**			
	(0.0084)			
1(Sexual Assault)	-0.0133*			
	(0.0077)			
1(Drug)	-0.0018			
	(0.0085)			
1(Black)		-0.0119		
		(0.0072)		
1(DNA)			-0.0038	
			(0.0067)	
1(Life)				-0.0147***
				(0.0052)
1(Death)				-0.0165
				(0.0102)
1(Death & DNA)				-0.0275
				(0.0267)
Observations	35,133	35,133	35,133	35,133

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Each cell is a separate regression. All regressions include executions, botched executions, crime rates, controls, and state and year fixed effects. Controls include variables in the bottom panel of Table 2 . Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 6. Heterogeneous Impact of Exonerations
by GSS Respondents' Characteristics

VARIABLES	(1) Male	(2) White	(3) Married	(4) HS Degree	(5) Democrats	(6) Harsh Attitude	(7) Over 40
1(Exoneration)	-0.0177**	-0.0136*	-0.0246***	-0.0144	-0.0203**	-0.0237***	-0.0406***
	(0.0087)	(0.0073)	(0.0246)	(0.0089)	(0.0097)	(0.0078)	(0.0098)
Observations	15,916	28,438	18,165	18,402	16,257	26,297	18,811

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Each cell is a separate regression. All regressions include executions, botched executions, crime rates, controls, and state and year fixed effects. Controls include variables in the top panel of Table 2. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

prevalent in pre-trial analysis and is thus not effective in post-trial exoneration.[25] Finally, exonerations with either life in prison or death sentences have relatively high impact, though only the coefficient on life sentence is significant. It is interesting to note that the impact of exonerating a death row inmate with DNA evidence is larger in magnitude than the impact of a death row exoneration alone.

Table 6 presents results when Equation (2.1) is estimated across various subgroups in the GSS data. Compared to the baseline estimated impact in Table 2 (-0.0240), the effect of exoneration exposure is smaller in magnitude among whites, males, and high school graduates while it is roughly the same for those who are married, are self-identified Democrats, or those reporting a harsh attitude towards the criminal justice system (as exhibited by their agreement that the court system is too lenient on criminals). Older age appears to decrease sentiment in favor of capital punishment quite a bit: the estimated impact of an exoneration is almost twice the baseline effect among the population over the age of 40.

[25]The coefficient estimate on a binary indicator for non-DNA exoneration exposure is -0.0241 and it is significant at the 1% level.

TABLE 7. Racial Impact of Exonerations

VARIABLES	(1) White GSS Respondent	(2) Black GSS Respondent
1(White Exoneration)	0.0020	-0.0281
	(0.0065)	(0.0259)
1(Black Exoneration)	-0.0072	-0.0373
	(0.0056)	(0.0251)
Observations	28,438	4,784

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Each cell is a separate regression. All regressions include executions, botched executions, crime rates, controls, and state and year fixed effects. Controls include variables in the top panel of Table 2. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 7 explores the racial implication of exonerations. In this table, I split the GSS data into white and black respondents and estimate the effect of being exposed to an exoneration with white and black exonerees for both groups.[26] Even though all coefficients are imprecisely estimated, the qualitative results are compelling: regardless of the exoneree's race, white respondents exhibit little adjustment in death penalty sentiment[27] while exonerations decrease black respondents' support for capital punishment, and this impact is particularly strong when the exoneree is also of the same race.

The Effect of Exonerations on Death Sentences

To determine if a decrease in public opinion in favor of the death penalty resulting from more exonerations brings about additional changes in the criminal justice system, Table 8 presents results when the dependent variables are death sentences while Figure 5

[26]Since the main classifications for race in GSS data are "white", "black", and "other", it is difficult to identify racial impact of exonerations among Asians or Latinos.

[27]This is consistent with the finding in column (2) of Table 6: exonerations have a smaller and less significant impact among white respondents.

presents an analysis of lag and lead impact of exonerations on the same outcome. Since the outcome variable is a state-level measure, all regression specifications are from the first difference model. The sample size is significantly smaller due to the fact that the unit of observation is a state-year and only states with capital punishment are considered during this analysis.[28]

In Panel A of Table 8, results suggest that having an exoneration decreases the number of death sentence by about 1.1 to 1.2 incidents per year while executions and botched executions do not appear to be significant predictors of death sentences being issued by states. The decrease in death sentence as a result of having an exoneration corresponds to about a 20% decrease from the average level during the time period in the sample (about 5.4 per state per year). In Panel B, while the impact of exoneration on death sentences is statistically insignificant, the sign of the coefficient estimates conform to the findings in Panel A and they also have sensible interpretations. For example, column (1) in Panel B suggests that the rise in the exoneration rate of 0.011 from 1990 to 2010 is associated with a decrease of about 0.25 death sentences per year (0.011×22.0691), or a 5% decrease from the mean level. Finally, Figure 5 indicates that once lag and lead impact are accounted for, exonerations do not seem to significantly decrease death sentences. However, the it is not clear whether such effect is diminished in the presence of lags and leads or that there is not enough power to identify the impact, as the coefficient estimates for the contemporaneously impact are rather similar to those found in Table 8.

Awareness of Exonerations and Executions

Since it is difficult to imagine a significant decrease in death penalty support as a result of more exonerations if individuals are not informed about such events, Table 9

[28]This number varies between 31 and 43 states during the years in my sample.

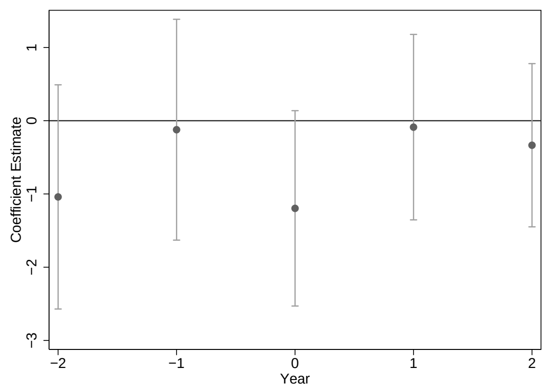
TABLE 8. Impact on Death Sentences Number

VARIABLES	(1)	(2)	(3)
Panel A: Event Exposure			
1(Exoneration)	-1.2205**	-1.1199**	-1.1274**
	(0.4652)	(0.4833)	(0.4992)
1(Execution)	1.2771	1.3299	1.3506
	(1.4910)	(1.4466)	(1.4901)
1(Botched Execution)	0.4198	0.3004	0.2864
	(0.9367)	(0.9934)	(1.0212)
Observations	620	620	620
Panel B: Event Count			
Exoneration	-22.0691	18.2954	-17.9398
	(13.0983)	(14.0164)	(14.4661)
Execution	10.4508	9.5209	9.6061
	(8.2634)	(9.2643)	(9.8193)
Botched Execution	51.9583	51.4434	51.3787
	(39.4268)	(36.7143)	(37.3851)
Observations	620	620	620
Year FEs	Yes	Yes	Yes
Controls	No	Yes	Yes
State FEs	No	No	Yes

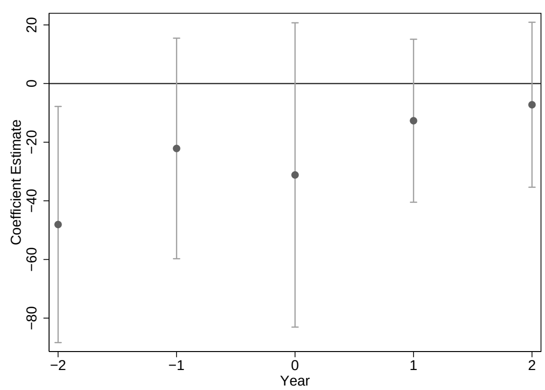
Notes: Dependent variable: Number of death sentences. Sample restricted to states where capital punishment is allowed. Standard errors in parentheses, allowing for clustering at the state level. Regressions are weighted with state populations. Controls include binary indicators for gender, age, race, marital status, education level, family income, political party affiliation, and opinion on the harshness of court sentences. *** significant at 1%; ** significant at 5%; * significant at 10%.

FIGURE 5. Lag and Lead Impact on Number of Death Sentences

(a) Event Exposure



(b) Event Count



Notes: Dependent variable: number of death sentences. All coefficients are obtained from a regression with control variables and state and year fixed effects. All lags and leads of exonerations, executions, botched execution, and violent crime are included. The 95% confidence intervals for each coefficient are also displayed.

provides a possible mechanism linking the two variables: media exposure in the form of Google searches. In this table, I regress a yearly Google Trends index for the volume of searches for the keyword "exoneration", "execution", and "botched execution" on the yearly occurrences of such events. Regressions in this table utilizes Google Trends data from 2004 (the first year it was available) to 2016. Each column is a separate regression, and each regression includes a full set of year and state fixed effects. For instance, the dependent variable in columns (1) and (2) is the yearly Google Trend index for searches of the keyword "exoneration" in state s in year t,[29] while the independent variable in column (1) is an indicator variable for exoneration exposure and the independent variable in column (2) is the number of exonerations in a particular state and year. Columns (3) to (6) provide results for similar estimation for executions and botched executions. Due to the uneven data availability for some states due to insignificant search volumes, the number of observations ranges from 377 to 650.

While the coefficient estimate on the exoneration exposure variable in column (1) is not significant, it does indicate that searches for exonerations go up by 0.80 (an 8% increase from the mean) as a result of having an exoneration. Column (2) suggests that an additional exoneration increases the Trends index by about 0.10, and this is significant at the 5% level. This corresponds to an increase of about 1% from the mean. Columns (3) and (4) suggest that executions do not seem to influence Google searches, while columns (5) and (6) indicate that botched executions could actually decrease searches for such events. A possible explanation is that residents in a state with a botched execution would

[29] Google Trends automatically normalize the index for every state from 2004 to 2016 relative to the period with the highest search volume, which takes on the value of 100. For instance, searches for the word "exoneration" in Oregon is highest during the month of September 2004, so this month has a Google Trends index of 100 while search volumes for the other months are defined according to how close they are to this peak. I aggregate data to the yearly level to be able to merge it with my exoneration and execution data.

TABLE 9. Event Exposure and Google Trends Search Volumes

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
1(Exoneration)	0.8044					
	(0.5969)					
Exoneration		0.1000**				
		(0.0477)				
1(Execution)			0.2256			
			(0.2149)			
Execution				0.0365		
				(.0363)		
1(Botched execution)					-1.0224	
					(0.8153)	
Botched execution						-0.5403
						(0.4555)
Observations	377	377	650	650	442	442
Number of states	29	29	50	50	34	34
Mean dependent variable	9.82	9.82	10.32	10.32	3.46	3.46

Notes: Dependent variable: yearly aggregated Google Trends index for the word "exoneration" (column 1), execution (column 2), and botched execution (column 3). All regressions include state and year fixed effects. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

prefer to not learn more about such a gruesome event and thus are not searching for it as often.

Predicting Variation in Death Penalty Support

In Figure 6, I test the regression model's ability to explain variation in capital punishment support throughout the years. I first save the coefficient estimates from my estimation of equation (2.1), aggregate my data to the yearly level, then use the estimated coefficients to predict the support for capital punishment as a result of variation from 1980 (the first year in my data) of specific variables. For instance, predictive support for the death penalty in Figure 6a is calculated by multiplying each of the coefficient estimates for demographic controls with the difference of those controls relative to the 1980 level. Thus, predictive support in this panel is generated from changing demographic trends only. Figure 6b and 6c add in violent crime rate and indicators measuring exonerations, executions, and botched executions exposure to the model generating predictive support, respectively. The actual capital punishment support is plotted in dashes.

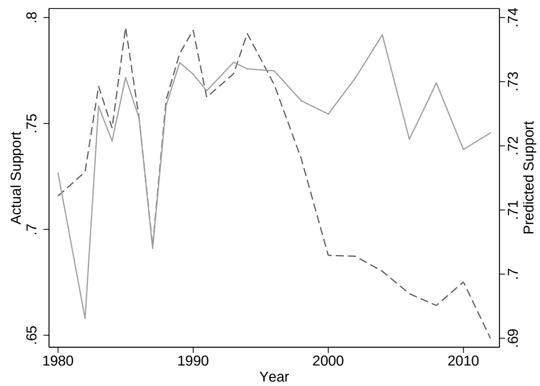
We can see that demographic controls are quite good at predicting variation in capital punishment support up until the early 1990s, when the prediction diverges from the actual support quite substantially. Adding in violent crime rate seems to improve the predictive power for the decline in support beginning in the early 90s, though the model seems to lose predictive power in the 1990s compared to the previous specification. Nonetheless, the full model that includes demographic controls as well as exonerations, executions, and violent crime rate seems to capture most of the variation in capital punishment support reasonably well throughout the entire duration of the sample.

Conclusion

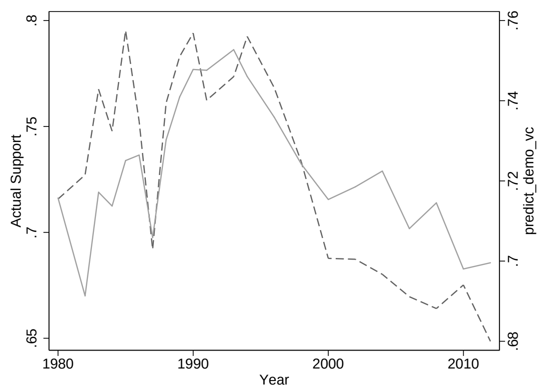
This paper examines the impact of a number of factors on sentiment regarding the death penalty. I find that being exposed to an exoneration significantly decreases the likelihood of supporting capital punishment as a potential sentence for murder convictions. Exposure to incidents of executions and botched executions does not seem to change public opinion. Furthermore, exonerations also decrease the number of death sentences issued by states, and this effect is most significant during the years of their occurrences. My findings contribute to the literature on determinants and consequences of a changing public opinion on capital punishment; furthermore, the results are consistent with a

FIGURE 6. Predicting Variation in Death Penalty Support

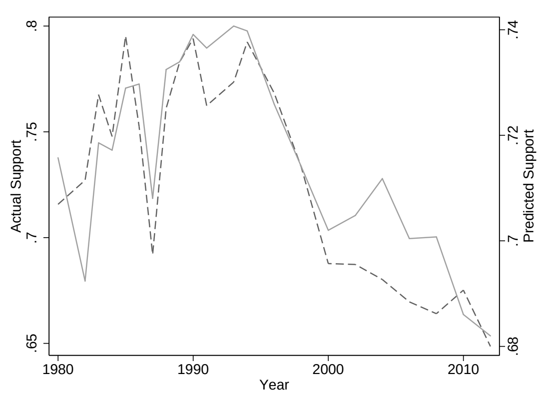
(a) with Demographic Characteristics



(b) with Demographic Characteristics and Violent Crime Rate



(c) with Demographic Characteristics, Violent Crime Rate, Exonerations, Executions, and Botched Executions



Notes: Actual death penalty support in dashes, predicted death penalty support in solid.

narrative about individuals choosing to update their beliefs about the necessity of capital punishment in the presence of new information in the form of a stigmatic event.

To the best of my knowledge, this is the first study to examine the influence of exoneration (and, to a lesser extent, execution) on death penalty sentiment. As such, there are many potential additions to this nascent literature; for instance, future research could attempt to quantify the precise mechanism that links exoneration to a reduction in death penalty favorability and the role of such mechanism through time. It is possible that, as information begins to travel faster with the advent of the internet and mobile technology, the impact of exonerations on decreasing death penalty sentiment could become more pronounced in the coming years. Another possible research area is on the impact of exonerations on other outcomes: if it is indeed the case that judges in states where they are elected instead of appointed are more receptive to public sentiment on capital punishment, the effect of exonerations on the criminal justice system is more likely to be clear-cut and definite in these states. Yet another study could examine the possibility that, if anything, DNA exonerations could increase death penalty support since there is more certainty that a guilty criminal is being convicted. Such an effect could be a potential explanation for why sentiment in favor of the death penalty is increasing again, judging by recent public measures to bring back capital punishment being approved in California, Nebraska, and Oklahoma.

Given that public debates regarding the use of capital punishment are currently ongoing across the U.S., the findings of this paper could be of relevance to legislators that are in the process of examining the need for the death penalty. The evaluation of a death penalty abolition should be processed in conjunction with an understanding of public opinion in favor of such legislative change and the determinants behind it. Furthermore, it is imperative that lawmakers be informed about potential shifts in the political landscape in response to a decline of public favorability in the death penalty. My paper provides empirical evidence in support of exonerations as a driver in affecting sentiment regarding the death penalty; furthermore, exonerations could also impact the criminal justice system by way of reducing the number of death sentences.

This paper represents a substantive contribution of this dissertation to the literature on understanding the determinants and consequences of a public issue. The next chapter continues this research agenda by investigating another question of significant interest to the public and lawmakers alike: a potential moral hazard problem associated with workers' compensation, a safety-net program aimed to provide workers with temporary assistance in situations where an injury occurs at the workplace.

CHAPTER III

BENEFIT GENEROSITY AND MORAL HAZARD: EVIDENCE FROM REGRESSION KINK

This work is an unpublished co-authored journal article. Benjamin Hansen identified the research question, collected the main data set, and, along with Glen Waddell, edited the final version of the paper to account for various robustness checks. I was responsible for data cleanup, literature review, as well as contributing to the preliminary and final econometric analyses.

Introduction

Since its introduction during the early 1910s, workers' compensation has grown to become one of the largest social-insurance programs in the United States. In 2012, it provided coverage to 128 million workers while paying out approximately $62 billion in total benefits (an increase of almost $10 billion from 2002), with about half going to injured workers and half to medical providers.[1] As benefits continue to climb, considering the trade-offs associated with more-generous benefits is increasingly beneficial. On one hand, higher benefits lessen the pressure to return back to work for injured workers and afford them more time to fully heal.[2] On the other, increasing generosity leads to an increase in moral hazard among workers; for example, it could entice workers to extend the duration of claims for workers' compensation, and even increase the probability of reinjury due to carelessness, or result in the extraneous provision of medical services.[3]

[1]Source: National Academy of Social Insurance.

[2]For example,Bronchetti (2012) find that a 10-percent increase in benefit generosity offsets the drop in household consumption by 3-to-5 percent.

[3]See, for example, Butler and Worrall (1985), Butler et al. (1996), or Dionne and St-Michel (1991).

There is a well-established literature examining the effect of increases in benefit generosity on injury duration, with many studies exploiting large, one-time policy changes in workers' compensation benefits to estimate the elasticity of injury duration with respect to generosity. Difference-in-differences around such changes yield elasticity estimates that range from 0.4 to 0.9 (Meyer et al. (1995); Neuhauser and Raphael (2004)). In this paper, we employ a new identification strategy—regression kinks—to estimate the impact of generosity on claim duration and temporary benefits paid. States' restrictions on minimum and maximum payments create a naturally occurring kink in the schedule of the replacement rates for workers just below and above certain wage thresholds. Using administrative data from Oregon, we exploit these changes in the intensity of treatment for workers close to the thresholds to estimate the responsiveness of injury duration and benefits paid with respect to benefit generosity.

Regression-kink designs (RKD or RK) are a relatively new estimation strategy, and we employ them in a framework that allows us to compare the results with previous difference-in-differences estimates. While we confirm that an increase in benefit generosity leads to higher injury duration, the implied elasticity estimates are smaller that would be typical in the literature—our estimates range from 0.2 to 0.4. Given the structural change in benefits paid when replacement rates shift, we obtain larger cost-generosity elasticity estimates, which is expected.

Our results have significant policy implications as a number of states continue to implement workers' compensation reforms under the pressure of increasing costs.[4] Our estimates are particularly well-suited to identifying the likely costs associated with small increases to maximum and minimum thresholds (pegging them to inflation, for instance). Furthermore, our RK framework highlights a potential approach for individual states

[4]See Hansen (2016) for a detailed look at California's attempt to reduce workers' compensation costs.

to assess the expected costs of changing benefit generosity at the margin, as it utilizes a feature common to most workers' compensation programs as the source of exogenous variation, in contrast to the use of a large one-time policy changes exploited in previous studies. In addition, given our ability to link individuals over time, we introduce to the literature new evidence that more-generous benefits increase the likelihood of subsequent claim filings.

The rest of the paper proceeds as follows. In Section 2, we review two different strands of relevant literature: moral hazard in workers' compensation and the RK framework, the latter of which has only recently been applied to considering policy. In Section 3, we describe key institutional features of the workers' compensation program in Oregon and, in Section 4, we present our empirical models and results. In Section 5, we discuss some implications of our analysis and conclude.

Background

Moral Hazard and workers' compensation

Asymmetric information in most social-insurance programs leads to both ex-ante and ex-post moral hazard. In general, ex-ante moral hazard refers to the activities one party might take that affect the probability of an incident, while ex-post moral hazard relates to a party's action once the incident has occurred. Regarding workers' compensation, ex-ante moral hazard would be present if generous benefits induced a worker to be more careless on the job and thus increases the probability of injury in response to additional generosity. Ex-post moral hazard occurs when higher benefits induce workers' into recovering more slowly from injury, or otherwise lengthen their receipt of workers' compensation. Dionne and St-Michel (1991) proposes a theoretical model to separate these two sources of moral hazard, and tests for the presence of moral hazard in the workers' compensation market in

Quebec. Bolduc et al. (2002) develops a model in which both ex-ante and ex-post moral hazards interact to influence workers' behaviors. Also with Quebec data, they show that moral hazard is more prevalent when workers suffer from difficult-to-diagnose injuries. Similarly, Butler et al. (1996) notes that moral hazard explained most of the increase in the proportion of soft-tissue injury claims in the U.S during the 1980s.

Our study is more closely related to the large literature that examines the link between benefit generosity and the number and duration of claims.Butler and Worrall (1983) and Krueger (1990a) find that increasing benefit payments leads to an increase in the number of claims filed, while Biddle and Roberts (2003) observe that benefit generosity has a notable impact on the probability of claim filing for an injured worker. Using data from Illinois, Butler and Worrall (1985) finds that the expected injury duration is significantly affected by changes in benefits. Similarly, difference-in-differences studies using data from Minnesota (Krueger (1990b)), Michigan and Kentucky (Meyer et al. (1995)), and California (Neuhauser and Raphael (2004)) suggest that more-generous benefits, as measured by an increase in the maximum weekly payments, induce workers to stay on workers' compensation longer. Estimates of the implied elasticity of injury duration with respect to benefit range from 0.4 to 0.9.

A recent study by Bronchetti (2012) reexamines the incentive effects in workers' compensation using 25 years of data from the March Current Population Survey. They find that the apparent responsive in workers' claim filing behavior around changes in benefits is quite sensitive to how one controls for the confounding influence of past earnings—the participation-benefit elasticity is less than 0.1 in their preferred specification. This suggests that the moral-hazard problem in workers' compensation may not be severe.

As we anticipate the potential for similar confoundedness, RKDs serve to net out the bias of these factors. We explain this framework in more detail below, but note here that we will retrieve an estimate of the causal impact of benefit generosity on injury duration as our identifying variation is through a comparison of otherwise similar workers, the difference among them being the relative replacement rates due to small differences in their wages. In the end, we do observe incidences of ex post moral hazard in workers' compensation—higher benefit generosity leads to an increase in injury duration and the probability of staying on workers' compensation—but our implied elasticity estimates are smaller in magnitude to those found in previous studies.

Empirical Applications of Regression Kinks

Nielsen et al. (2010) are among the first to employ the RKD, and do so to study the effect of student aid on college enrollment. The intuition of the design is very similar to that of regression discontinuity, but instead of a discrete jump in treatment status at some known value of the running variable there is a change in the intensity of treatment, creating "kinks" around which one can measure changes in the outcomes of interest based on the same limit-arguments that support more-traditional regression-discontinuity designs. Card et al. (2012) and Card et al. (2015) consider nonlinear identification of this framework and characterizes a class of models under which the RKD yields valid causal inference. The key condition to be satisfied is that the conditional density of the running variable has to be continuously differentiable through the kink points, which rules out instances of bunching or sorting. Similar to regression discontinuity, this assumption is empirically testable using some variation of the test proposed in McCrary (2008). Provided that this is satisfied, estimation could be done using both a sharp and fuzzy RKD design.[5]

[5]Dong (2010) shows that it is possible to exploit both a jump and a kink to identify a treatment effect and applies such an estimator to investigating the retirement-consumption puzzle.

There has been a marked increase in the use of RKD as a method of estimation, due to its appealing statistical properties. Simonsen et al. (2016) uses a kink in the schedule of reimbursement schemes in the Danish health care market to study the price sensitivity of demand for prescription drugs. In studies that are similar to ours in spirit, Landais (2015) exploits the kinks in the schedule of unemployment insurance benefits to study the effect of unemployment insurance on labor supply. Gelber et al. (2015) use the "bend points" inherent in disability insurance to examine the impact on subsequent earnings. In other recent work, RKDs have been used to identify the causal effect of government grant on local public employment (Lundqvist et al. (2014)) in the economic incidence of the Pell Grant (Turner (2014)).

Our study offers two main contributions to this growing literature. First, we apply the RKD in a novel setting that, to our knowledge, has not been looked at before. Specifically, we exploit the fact that states' maximum and minimum restrictions on workers' compensation benefits create kinks in a worker's compensation-replacement rate, such that otherwise-similar workers on either side of a wage threshold experience a change in how additional income translates into benefits. We use this quasi-experimental setting to estimate the effect of benefit generosity on injury duration and benefits paid. The rich literature on moral hazard in workers' compensation also affords us the opportunity to compare our results with previous difference-in-differences estimates to understand how different sources of variation lead to different estimates of the same parameters.

To clarify the source of identifying variation, consider a mapping of weekly wages into replacement rates and weekly benefits in Oregon during the 2014 fiscal year, which we plot in Figure 7. Here, replacements are a constant 66 2/3 percent of lost income, with weekly benefits constrained by minimum ($50 per week or 90% of weekly wage, whichever is lower) and maximum ($1,181.55 per week) amounts.[6] It is these restrictions that give rise to identifying variation, as the replacement rate kinks in two places: one at the minimum threshold and the other at the maximum threshold. In this scenario, the associated kinks are located at $75 and $1,772.325. Workers with weekly wages of less than $75 per week receive $50 since their benefits (66 2/3 percent $\times$ weekly wages) are lower than the minimum threshold. For these workers, their replacement rates are higher than 66 2/3 percent. In contrast, workers who earn more than $1,772.325 per week receive the maximum $1,181.55 in weekly benefits, with replacement rates lower than 66 2/3 percent. Our estimation strategy compares outcomes across workers on either side of these kinks, exploiting the marginal increase in intensity of treatment.[7]

Institutional Features and Data Sources

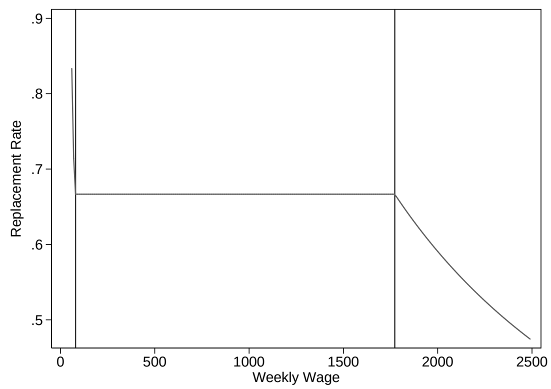
Workers' compensation in Oregon shares many programmatic features common to other states. As in the example above, the weekly benefit is 66 2/3 percent of a worker's weekly wage, as long as this benefit falls within a range defined by a minimum and maximum amount. The minimum weekly benefit is currently the lesser of $50 or 90 percent of a worker's weekly wage, while the maximum weekly benefit is a function of the state's average weekly wage (SAWW). As such, there is potential variation in these thresholds

[6] These amounts are determined by the state's average weekly wage.

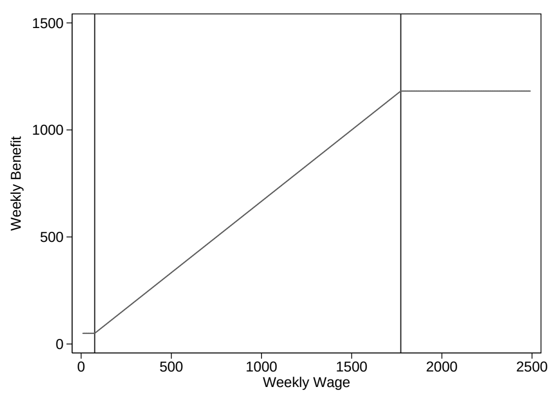
[7] Ideally, we would want to look at the kinks at both the minimum and maximum thresholds; however, with so few observations around the minimum thresholds, we will not have enough power to separately identify the effect at the these levels.

FIGURE 7. Weekly Benefits and Replacement Rates as Functions of Weekly Wages for Fiscal Year 2014

(a) Replacement Rates



(b) Weekly Benefits



across years.[8] In addition, however, the maximum benefit was changed from 100 percent of the SAWW to 133 percent of the SAWW.[9]

One feature that complicates our estimation is the potential increase in benefit associated with increases in SAWW. In particular, if a worker at the maximum weekly benefit experiences an injury that overlaps with a new fiscal year, he thereafter receives a new inflation-adjusted maximum benefit.[10] This creates a source of missmeasurement, as we do not observe this change in maximum benefit in our data (as our unit of observation is a worker-claim). We overcome this challenge by supplementing our analysis with a discrete-time hazard model, where the unit of observation is a worker-week for a single claim. We discuss this model below, and note here that the model allows maximum benefits to vary through time according to known changes in policy parameters that would have been experienced over those worker-weeks.

We obtained comprehensive data on workers' wages, injury duration, and benefit generosity from Oregon's Workers' Compensation Division. Our main dependent variables are the days spent on workers' compensation and the benefits received during that time. In Oregon, injury duration is classified into two categories: temporary total disability (TTD) days, where workers are completely absent from work, and temporary partial disability (TPD) days, where workers return to work partially while recovering from injury. Our measure of injury duration is the sum of these two variables, and our measure of benefit is the amount paid to workers while they are on either TTD or TPD.[11] Our data

[8] The SAWW is determined by the Employment Department by May 15 of each year. See Oregon Revised Statuses 656.210, 656.211.

[9] This one-time increase in generosity occurred on 1 January 2002, and was not an overhaul of workers' compensation in any broad way. There was no other aspect of the program that was affected during this time.

[10] This inflation adjustment applies to all workers, no matter the date of injury.

[11] Stratifying by total or partial disability is problematic, as this distinction is unavailable in our data until the late 1990s, accounting for roughly half of our sample.

encompass the universe of all injured workers in Oregon, from 1990 to 2010, though we exclude from our analyses workers with multiple claims for one accident, workers whose claims are not accepted, and workers who suffer from a permanent injury. With so few observations around the kink in benefits at the minimum benefit, we will also focus our attention on workers whose wages fall around the maximum threshold.

Empirics

Using administrative records from Oregon, we estimate the effect of benefit generosity on injury duration and benefits paid. Below, we discuss the econometric models and then the estimation results.

Econometric Models

RKDs can be estimated in manners similar to models from a standard regression-discontinuity design. The key difference is that in a traditional regression-discontinuity design, the estimated slope of the running variable is treated as a nuisance parameter, or a parameter that is estimated only to ensure the unbiased estimation of other parameters which are truly of interest. In the RKD, it is the slope parameter—or more precisely, the change in the slope of y in the running variable at the threshold—that is economically interesting. As such, our first primary econometric models will be based on the model,

$$\begin{aligned} y_{it} &= \alpha_1 WeeklyWage_{it} + \alpha_2 1(WeeklyWage_{it} > k_t) \\ &+ \alpha_3 WeeklyWage_{it} \times 1(WeeklyWage_{it} > k_t) + X_{it}'\gamma + \delta_t + u_{it} \end{aligned} \tag{3.1}$$

where y_{it} is either injury duration of or benefits paid to an individual i in year t, $WeeklyWage_{it}$ is the weekly wage relative to k_t, the maximum weekly benefit in year t.

X_{it} is a matrix of control variables.[12] Year fixed effects are absorbed in δ_t, and errors are absorbed in u_{it}. The parameter of interest in the RKD is α_3, the interaction between the threshold indicator and the weekly wage. Because benefits are less generous (in terms of replacement rate) on the right side of the threshold, we anticipate that marginal increases in $Weekly\_Wage_{it}$ will decrease injury durations and lower benefits paid to claims on the right side of the threshold (i.e., $\hat{\alpha}_3 < 0$).[13]

To adjust for the presence of non-linearities, we allow injury duration and paid benefits to be quadratic in weekly wage. Formally, then, we estimate,

$$\begin{aligned} y_{it} = {} & \alpha_1 WeeklyWage_{it} + \alpha_2 WeeklyWage_{it}^2 + \alpha_3 1(WeeklyWage_{it} > k_t) \\ & + \alpha_4 WeeklyWage_{it} \times 1(WeeklyWage_{it} > k_t) \\ & + \alpha_5 WeeklyWage_{it}^2 \times 1(WeeklyWage_{it} > k_t) \\ & + X_{it}'\gamma + \delta_t + u_{it} \end{aligned} \tag{3.2}$$

In Equation (3.2), the estimated change in the slope across the threshold is $\hat{\alpha}_4 + 2\hat{\alpha}_5 Weekly\ Wage_{it}$. However, as we are interested in the estimated change at the kink point and weekly wages have been rescaled around the kink, the term $2\alpha_5 Weekly\ Wage_i$ goes to zero in the limit. Hence, for all of the regressions we will be primarily interested in the sign and magnitude on $\hat{\alpha}_3$.

We adopt a bandwidth of \$200 when presenting our main estimates, though we later demonstrate the sensitivity of our results to bandwidth selection. Precision can increase with larger bandwidths, of course, but potential non-linearities can also complicate the estimation of the kink at the threshold, where we are intent on exploiting exogenous

[12]We control for a worker's gender, age, industry type (based on the Standard Occupational Classification), and body part injured.

[13]We subsequently investigate models where we restrict the discontinuity at k to be zero (i.e., $\alpha_2 = 0$).

policy variation for inference statements. Fortunately, our qualitative results are consistent across bandwidths and it is mostly the precision with which we estimate our parameters of interest that varies. As an additional robustness check, we implement a bandwidth-selecting procedure for regression discontinuity and regression-kink designs based on Calonico et al. (2014), which specifies bandwidths that are close to our preferred choice of $200.

Estimation Results

Here, we analyze the effect of more-generous benefits on two main outcomes: the number of temporary disability days (the sum of total and partial disability claims) and the total disability costs.[14]

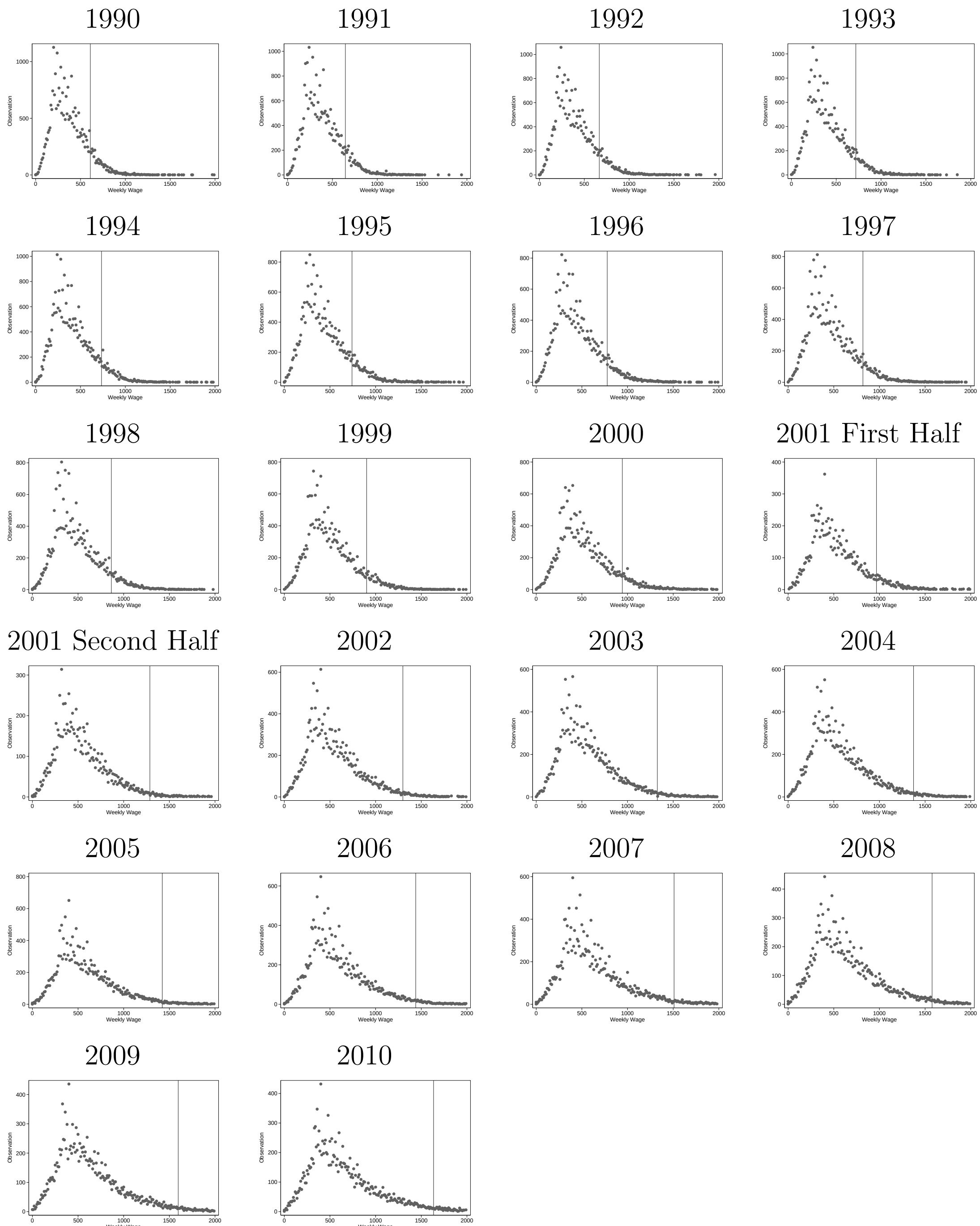
We first consider the smoothness assumptions supporting the validity of our regression-kink design. In Figure 8, we plot the distribution of weekly wage through the years in our sample and, in Figure 9, we plot worker characteristics. In short, there is no evidence of manipulation at the threshold. In Figure 8, we can observe smoothness in the density distribution for every year in our sample. Similarly, in Figure 9 we see that neither workers' age, gender, the proportion of workers in the construction industry, nor claim-acceptance probabilities change systematically across the threshold.[15]

Given this smoothness, in Table 11 we report estimates of the effect of having a weekly wage to the right of the threshold. (We offer two set of models, with and without

[14]Due to the large increase in benefit generosity in 2002—maximum-allowable benefits were increased 30 percent in 2002—we examine the impact of benefits using all observations in our sample, and in an appendix report separate estimates for observations in the periods before and after the large increase in generosity. Results are largely driven by observations in the years prior to 2002, at the lower level of generosity, which we show in appendix tables.

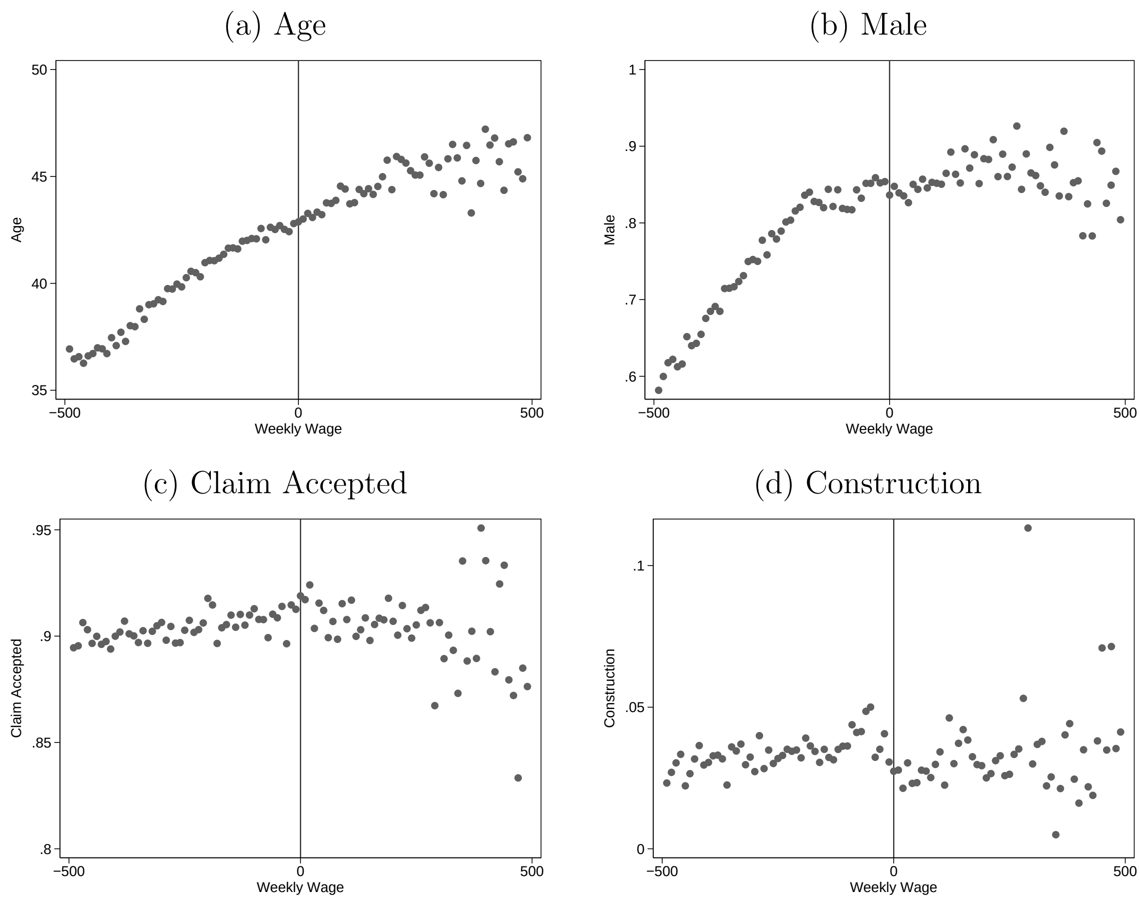
[15]The proportion of workers in the construction industry is a proxy of sorts for occupational type. We also find that the proportion of workers in other major industries such as farming or labor-intensive occupations is smooth across the thresholds.

FIGURE 8. Density Tests



Notes: Based on administrative records from the workers' compensation Division in Oregon. Each cell is a fiscal year from 1990 to 2010. The vertical axis is based on the frequency of observations, with the running variable (weekly wage) on the horizontal axis. The vertical line represents the threshold. The bin width is $10.

FIGURE 9. Workers and Claim Characteristic Across Threshold



Notes: Based on administrative records from the workers' compensation Division in Oregon. The vertical axis is specified in each cell, with the running variable (deviation from the threshold) on the horizontal axis. The vertical line represents the threshold. The bin width is \$10.

the restriction that the discontinuity at the threshold is zero. Estimated elasticities across this restriction are qualitatively similar.) The parameter of interest corresponds to the interaction of weekly wage and an indicator variable capturing that the weekly wage was above the maximum allowable, which range from -.019 to -.033. These appear as somewhat small effects, though the margin here is measured changes induced by an increase in benefit generosity of only 67 cents.[16] The implied benefit-duration elasticities—ranging from .25 to .41—are therefore much more informative, which we report at the bottom of the table.[17] The estimates are relatively robust across specifications as well as to the inclusion of controls though they lack some statistical precision. Panel A of Figure 10 illustrates this same change, where the negative-slope change is evident in going from the left to the right side of the maximum-benefit threshold.

In Table 12, we consider the estimated change in benefits paid. Because we expect benefits levels to increase even if injury duration is unchanged, it's not surprising that we find strong evidence that more-generous benefits lead to more-costly claims. The estimates range from -6.6 to -9.2, and imply cost-generosity elasticities that range from 1.10 to 1.55. This is also evident graphically, in Panel B of Figure 10.

[16]To the left of the threshold, an extra dollar in weekly wage results in an additional 67 cents in benefit. To the right, however, an extra dollar in weekly wage yields no change in benefits, as the maximum benefit was reached at the threshold.

[17]Elasticities are calculated around the threshold. For example, in Column (1) of Table 11, the elasticity of .41 is calculated as the change in injury duration from being above the threshold, scaled by the mean injury duration from Table 10 (-0.0325/61.63), divided by the change in weekly benefit, again scaled by its mean (-0.67/522.10).

TABLE 10. Summary Statistics (at Threshold)

Variable	Mean	Median	99 pct
Weekly Wage	793.91	734.40	1,600.00
Weekly Benefit	522.10	483.20	1,059.03
Injury Duration	61.63	20.00	554.00
Benefits Paid	5,087.78	1,711.83	40,736.00
Reinjury Probability	0.37	0	1

Notes: Sample restricted to those within \$50 of the threshold, with non-missing running variables and non-rejected claims. Injury duration and benefits paid are top-coded at the 99 percentile.

TABLE 11. Effect of Benefit Generosity on Injury Duration
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.0193***	0.0156**	0.0305	0.0282	0.0222***	0.0182***	0.0302*	0.0262
	(0.007)	(0.006)	(0.022)	(0.022)	(0.005)	(0.005)	(0.017)	(0.017)
Weekly Wage^2			0.0001	0.0001			0.0001	0.0001
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	1.0576	0.9273	-0.0385	-0.2447				
	(1.281)	(1.241)	(1.608)	(1.574)				
Weekly Wage × 1(... above maximum)	-0.0325***	-0.0320**	-0.0194	-0.0193	-0.0300**	-0.0298**	-0.0195	-0.0204
	(0.012)	(0.012)	(0.037)	(0.038)	(0.012)	(0.012)	(0.038)	(0.038)
Weekly Wage^2× 1(... above maximum)			-0.0002	-0.0002			-0.0002	-0.0002
			(0.000)	(0.000)			(0.000)	(0.000)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	72,667	72,630	72,667	72,630	72,667	72,630	72,667	72,630
Elasticity	0.41	0.40	0.25	0.24	0.38	0.38	0.25	0.26

Notes: Dependent variable: Days on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = \$10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

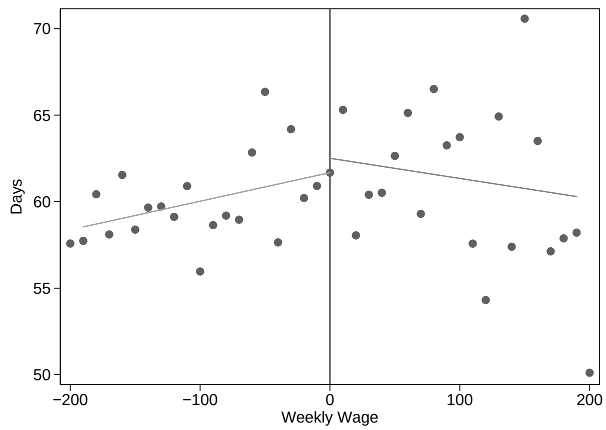
TABLE 12. Effect of Benefit Generosity on Claim Cost
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	6.1973***	5.7968***	7.6521***	7.4493***	6.4764***	6.0602***	8.0166***	7.7907***
	(0.559)	(0.490)	(1.873)	(1.689)	(0.434)	(0.384)	(1.433)	(1.312)
Weekly Wage^2			0.0069	0.0079			0.0084	0.0093
			(0.008)	(0.007)			(0.007)	(0.006)
1(Weekly Wage above maximum)	99.9375	94.4866	45.0255	42.2129				
	(107.399)	(101.859)	(137.824)	(128.090)				
Weekly Wage × 1(... above maximum)	-6.8521***	-7.2624***	-8.2587**	-9.2212***	-6.6181***	-7.0401***	-8.0448**	-9.0202***
	(1.069)	(1.045)	(3.405)	(3.213)	(1.091)	(1.042)	(3.541)	(3.307)
Weekly Wage^2× 1(... above maximum)			-0.0072	-0.0062			-0.0113	-0.0100
			(0.018)	(0.017)			(0.015)	(0.014)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	72,667	72,630	72,667	72,630	72,667	72,630	72,667	72,630
Elasticity	1.05	1.11	1.26	1.41	1.01	1.08	1.23	1.38

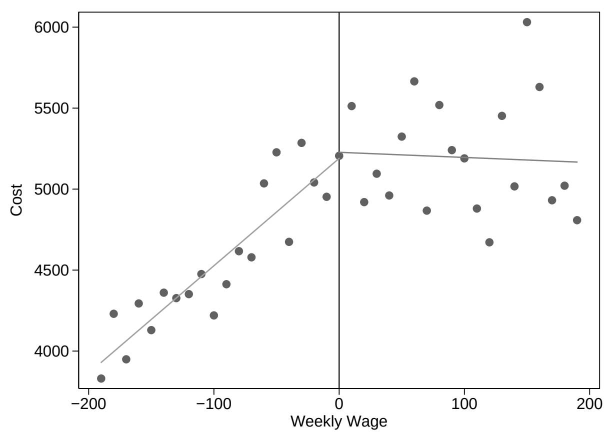
Notes: Dependent variable: Payments to workers on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

FIGURE 10. Effects of Benefit Generosity
Bandwidth: 200

A: On Injury Duration



B: On Claim Cost



Notes: Based on administrative records from the workers' compensation Division in Oregon. Points represent the averages, with fitted values based on linear model in black lines. The vertical axis is duration on temporary benefits in Panel a and temporary benefits paid in Panel b. The horizon axis is the running variable (deviation of weekly wage from the threshold). The vertical line represents the threshold. The bin width is $10.

Robustness Checks and Sensitivity Analyses

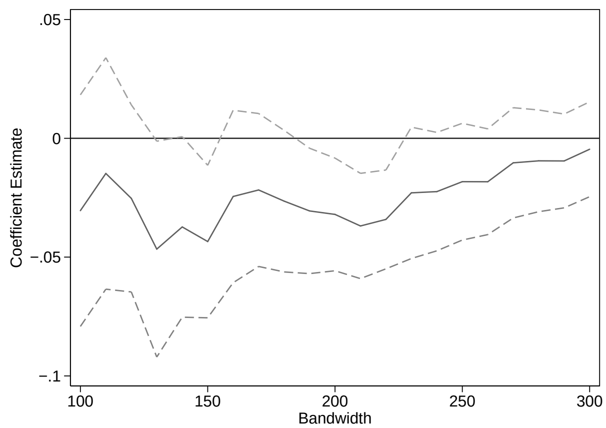
Bandwidth sensitivity

We conduct bandwidth sensitivity checks to investigate the robustness of the main results to bandwidth choices. The top panel in Figure 11 plots the point estimates of the impact of benefit generosity on injury duration as well as the 95% confidence intervals, using bandwidths varying from \$100 to \$300 in \$10 increments. The bottom panel of Figure 11 plots analogous graphs when the dependent variable is paid benefits. These figures are constructed with results from regressions that utilize the full set of control variables and a linear running variable. For the most part, our estimates are quite robust to bandwidth choices.

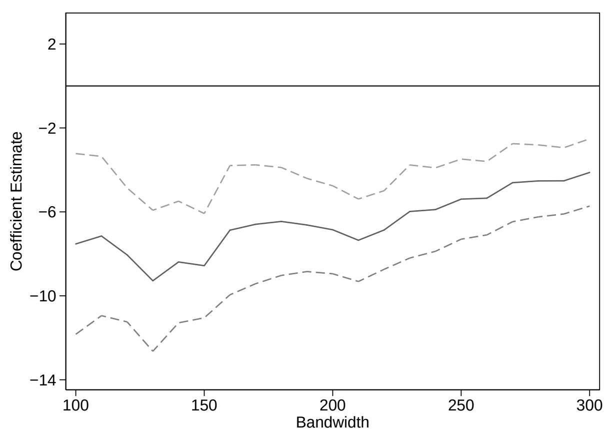
However, to further alleviate concerns about the sensitivity of our results to the choices of bandwidth and polynomial order, we also supplement the main analysis with results from a recent estimation procedure proposed by Calonico et al. (2014). This procedure constructs point estimates and confidence intervals based on a biased-corrected regression discontinuity and regression-kink estimators to account for the considerable sensitivity of RD results to bandwidth choices frequently observed in the empirical literature. Each column in Table 13 is a separate regression, and each regression uses the universe of all injured workers in Oregon, from 1990 to 2010. In the first column, we let the procedure calculate the optimal (bias-corrected) bandwidth and polynomial order. In Panel A, the point estimate for the coefficient of interest α_3 is -0.042. This is within the same order of magnitude of the estimates in Table 11, but we do lose some precision. The procedure specifies an optimal bandwidth of \$267.26—close to our preferred bandwidth of \$200—and a first-order polynomial, again similar to what we use in our preferred specifications. In columns (2), (3), and (4), we adopt bandwidths of \$300, \$400,

FIGURE 11. Bandwidth Sensitivity Checks
Effects of Benefit Generosity

(a) On Injury Duration



(b) On Claim Cost



Notes: Based on administrative records from the workers' compensation Division in Oregon. The solid line represents the coefficient estimates while the dash lines are 95-percent confidence interval. All regressions include control variables and a first degree polynomial order in the running variable (corresponding to a linear specification). Standard errors are clustered at the bin width level.

and \$500, respectively, while letting the procedure choose the optimal polynomial order. In all cases, polynomials of degree one are chosen. In contrast, in columns (5) and (6) we impose second- and third-order polynomials while letting the procedure choose the optimal bandwidth. Overall, we observe that while the choice of bandwidth slightly alters the size and significance of the estimate, the choice of polynomial order seems to have the larger impact, perhaps because we are overfitting the data with higher-order polynomials.

In Panel B of Table 13, we present analogous results for predictions of benefits paid to workers on temporary disability. Consistent with our previous results, the estimated impact of increasing benefit generosity on benefits paid is quite robust to specification. We obtain estimated elasticities ranging from 1.16 to 1.64.

Hazard-model approach

We earlier suggested that there may be some concern about the unit of observation being worker-claim, as we do not observe the increase in benefits experienced for workers who are at the maximum weekly benefit when their injuries overlap with a new fiscal year. Here, we overcome this challenge by supplementing our analysis with a discrete-time hazard model, where the unit of analysis is worker-claim-week.

We expand the data set such that we observe each worker during each week that he remains on workers' compensation and can therefore accurately update his weekly benefit to account for year-to-year inflation adjustments. We then estimate the probability that a worker exits workers' compensation in any given week, anticipating that our main coefficient of interest (α_3) will be *positive*, as benefits to the right side of the threshold are not as generous and workers have less incentive to stay on workers' compensation. Following the convention in discrete-hazard model estimation, we also include indicators for each week a worker is injured to flexibly model the baseline hazard. (We present

TABLE 13. Sensitivity Checks based on Calonico et al. (2014)

	(1)	(2)	(3)	(4)	(5)
			Panel A: On Injury Duration		
Weekly Wage × 1(... above maximum)	-0.042	-0.056	-0.053**	-0.033*	-0.076
	(0.042)	(0.036)	(0.025)	(0.019)	(0.048)
Bandwidth	267.26	300	400	500	464.55
Polynomial order	1	1	1	1	2
Elasticity	0.53	0.71	0.67	0.42	0.96
			Panel B: On Temporary Benefits Paid		
Weekly Wage × 1(... above maximum)	-9.247**	-10.731***	-9.197***	-7.587***	-10.084*
	(3.633)	(2.997)	(2.087)	(1.613)	(5.734)
Bandwidth	246.51	300	400	500	350.99
Polynomial order	1	1	1	1	2
Elasticity	1.42	1.64	1.41	1.16	1.54

Notes: Dependent variable: Days on temporary disability in Panel A and temporary disability paid in Panel B. Standard errors in parentheses. Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

results using OLS for ease of interpretation, but Logit models produce similar average marginal effect estimates.)

We report estimated coefficients for these regressions in Table 22 of Appendix B. As the probability of exiting workers' compensation in a particular week is quite small (about 5 percent, from Table 10), the estimates are also small, and they are estimated somewhat imprecisely. When scaled as elasticities, they range from -0.14 to -0.92. Again, these are smaller than the elasticities from prior difference-in-differences based approaches in the literature.

Pre/post-2002 regimes

We also report, in table 23 and 24 of Appendix B, separate estimates for the pre- and post-2002 regimes, given the increase in allowable maximums beginning on 1 January 2002. Using only the pre-2002 period (when the maximum threshold is based on the 100 percent of the state average weekly wage) yields benefit-injury elasticities that range from -.08 to .34. In the post-2002 period, however, the estimated elasticities are significantly larger—they range from 1.58 to 7.08. We urge caution in interpreting these larger estimates as causal for two reasons. First, the sample size is dramatically smaller in the post-2002 years, as there are far fewer injury claims at the (33-percent) higher threshold.[18] Second, the largest elasticity estimates only arise when we use quadratic models, which can put undue weight on observations far from the thresholds. Indeed, a graphical inspection of Figure 10 suggests it is unclear if there is support for quadratic models.

[18]This is anticipated, as moving the maximum benefit to 133 percent of the state's average weekly wage effectively pushes the threshold far to the right and thus we are only considering the higher earners.

First injuries vs. subsequent injuries

One might be concerned about the selection into the sample by injured workers during their first stay on workers' compensation versus subsequent stays, where they might obtain institutional knowledge of the programs and are perhaps in a position to take advantage of increasing benefit generosity. Tables 25, 26, and 27 in Appendix B reproduce Tables 10, 11, and 12 but with a sample restricted to only include workers on their first injuries. We can observe that the qualitative results from the previous sections still hold: there is a slight increase in injury duration and significant increase in claim cost associated with more generous benefits, though the magnitude of the impact is somewhat dampened by the sample restriction.

Impact on difficult-to-diagnose injuries

Bolduc et al. (2002) document significant heterogeneity in response to increasing benefits among workers with injuries that are difficult to diagnose such as overextension or back injuries. Table 28 in Appendix B examine this potential heterogeneity on injury duration and claim cost. The sample utilized in this table consists of workers with a back injury or an injury due to overextension, bruise, or sprain. Compared to our main results, there does not appear to be a heterogeneous impact along a difficult-to-diagnose dimension.

Reinjury

Of critical public-health concern in workers' compensation is the incidence of reinjury. The factors that can drive reinjury are different than those that would drive other types of social insurance, such as unemployment insurance. While repeated unemployment would typically be seen as a signal of match quality, repeated injury could simply be related to incomplete healing. Alternatively, if workers' are not fully informed about workers' compensation benefits, benefit generosity for *current* claims could affect an individual's choice to file *future* claims, if individuals update their beliefs about generosity based on their own claiming experience.

Given the universe of workers compensation claims in Oregon, we can track individual claimants over time and, therefore, instances of reinjury. For the sample of first-time claim filers—those falling between 1990 and 2000—in Table 14 we consider the probability of subsequent injury given the benefit generosity of their first injury. Reinjury elasticities range from .86 to 1.75 in the unrestricted sample, and are somewhat larger—1.11 to 1.35—when the sample is restricted to only the reinjured. In Table 15, we again report the procedure of Calonico et al. (2014), which yields somewhat larger elasticities. The estimates, and corresponding Figures 12 (graphic illustration of the main results) and 13 (bandwidth sensitivity checks), provide compelling evidence that reducing benefit generosity is associated with sizable reductions in future claim filing.

This amounts to a new type of moral hazard previously undiscovered in workers' compensation. Previous research has identified ex-ante moral hazard (the effect of claim generosity on the likelihood of getting injured), or ex-post moral hazard (the effect of claim generosity on claim duration). We find no evidence of ex-ante moral hazard, and only moderate evidence of ex-post. That current claim benefits affect the likelihood of filing claims can be viewed as behavioral in some way. Though the available administrative records do not allow us to separate them from one another (Kahneman, 2003), we include among the operative mechanisms, that worker's have incomplete information on the benefit schedule, how the schedule maps into earnings, or how difficult it will be to live on a reduced income while recovering from their injury.

TABLE 14. Effect of Benefit Generosity on Reinjury Probability
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.00014***	0.00016***	-0.00013	-0.00015	0.00016***	0.00016***	0.00011	0.00005
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Weekly Wage^2			-0.00000	-0.00000*			-0.00000	-0.00000
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	0.00651	0.00190	0.02984***	0.02364**				
	(0.008)	(0.008)	(0.010)	(0.009)				
Weekly Wage × 1(... above maximum)	-0.00061***	-0.00044***	-0.00083***	-0.00052**	-0.00060***	-0.00044***	-0.00068**	-0.00041
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Weekly Wage^2 × 1(... above maximum)			0.00000***	0.00000***			0.00000	0.00000
			(0.000)	(0.000)			(0.000)	(0.000)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	72,667	72,630	72,667	72,630	72,667	72,630	72,667	72,630
Elasticity	1.28	0.93	1.75	1.10	1.26	0.93	1.43	0.86

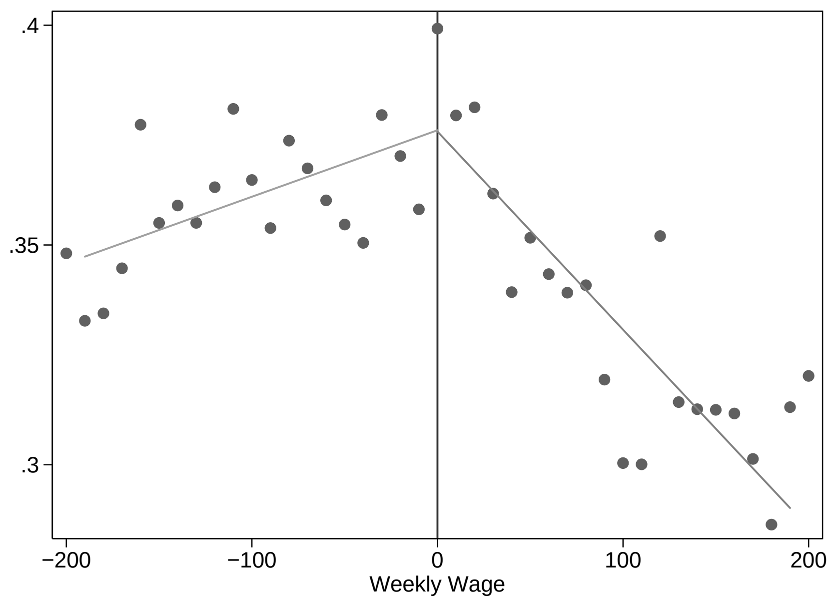
Notes: Dependent variable: Probability of reinjury. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 15. Sensitivity Checks based on Calonico et al. (2014)

	(1)	(2)	(3)	(4)	(5)
Weekly Wage × 1(... above maximum)	-0.00088***	-0.00080***	-0.00071***	-0.00066***	-0.00118***
	(0.00021)	(0.00018)	(0.00013)	(0.00010)	(0.0037)
Bandwidth	271.55	300	400	500	338.47
Polynomial order	1	1	1	1	2
Elasticity	1.85	1.68	1.50	1.39	2.49

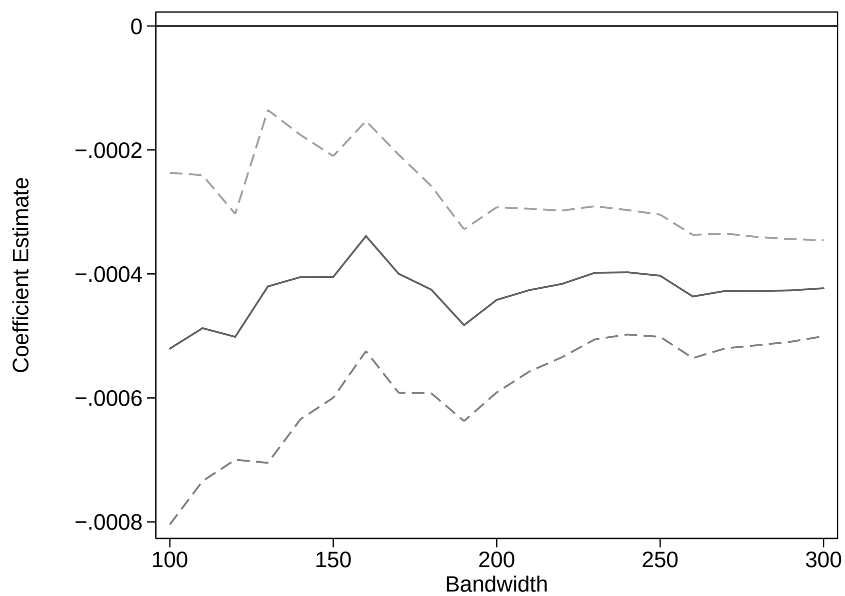
Notes: Dependent variable: Probability of reinjury. Standard errors in parentheses. Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

FIGURE 12. Effects of Benefit Generosity on Reinjury Probability
Bandwidth: 200



Notes: Based on administrative records from the workers' compensation Division in Oregon. Points represent the averages, with fitted values based on linear model in black lines. The vertical axis is reinjury probability among all workers. The horizon axis is the running variable (deviation of weekly wage from the threshold). The vertical line represents the threshold. The bin width is $10.

FIGURE 13. Bandwidth Sensitivity Checks
Effects of Benefit Generosity on Reinjury Probability



Notes: Based on administrative records from the workers' compensation Division in Oregon. The solid line represents the coefficient estimates while the dash lines are 95-percent confidence interval. All regressions include control variables and a first degree polynomial order in the running variable (corresponding to a linear specification). Standard errors are clustered at the bin width level.

For completeness, Tables 29 and 30 in Appendix B examine the impact of more generous benefit on reinjury probability among workers with a difficult to diagnose injury as well as workers injured for the first time. We find no discernible differences in these results compared to those utilizing the full sample.

Conclusions

The effect of more-generous benefits on workers' compensation claims has been a long standing policy question. With administrative data from Oregon, we use a regression-kink design to estimate the effect of more-generous benefits on injury duration and total benefits paid to the universe of temporary disability claims. We find evidence, not surprisingly, that more-generous benefits result in more-costly claims. Indeed, increasing weekly benefit generosity by 10 percent results in total benefits paid increasing by roughly 10-to-15 percent. However, we find limited evidence that more-generous benefits result in longer claims of injury. Prior research using administrative data suggests that injury benefit elasticities may range from 0.4 to 0.9. Our preferred models yield injury benefit elasticities that range from 0.2 to 0.4. Based on a new approach to identifying the causal parameter of interest, this evidence echoes other more-recent studies that have suggested workers are becoming less responsive to changes in workers' compensation benefits.

Furthermore, we find compelling evidence that increases in current claim generosity are associated with substantial increases in the likelihood of filing a subsequent claim. Moreover, workers are quite elastic in this regard, as a 10-percent increase in benefits is estimated to lead to an 11-to-14 percent increase in subsequent claim filing. This goes against the common medically driven arguments that greater benefits would encourage a longer (and therefore fuller) recovery period, and drive down the likelihood of future injuries. (This is not substantively different from the argument that more-generous unemployment insurance benefits will create better job matches and limit future unemployment insurance.) Instead, our findings suggest a behavioral component to claim filing, wherein workers have limited information about the benefits they'll receive upon being injured, but acquire a forecast of the future benefits based on their current claim. While behavioral factors such as peer effects (Dahl et al. (2014)), salience (Chetty et al. (2009)), or default options (Thaler (1994)) have been found elsewhere in economics, this is the first evidence of this type of moral hazard in workers' compensation. From a policy perspective, our findings also suggest previous estimates of the cost of increasing benefits will understate the long-run costs, as increases benefits will increase subsequent claim filing.

The next chapter also investigates similar moral hazard incentives in workers' compensation programs. Nonetheless, it utilizes a different source of variation to identify the economic impact of higher benefit generosity on injury duration and claim cost: an increase of 33% in maximum benefit thresholds for workers injured after January 1, 2002 in Oregon.

CHAPTER IV

MORAL HAZARD IN WORKERS' COMPENSATION: A DIFFERENCE-IN-DIFFERENCES REVISIT

Introduction

Workers' compensation is the prevalent form of social insurance in the U.S. aimed to provide workers with medical assistance and wage replacement in case an injury occurs at the work place. Since its inception during the 1910s, workers' compensation has witnessed significant increases in both medical and wage replacement costs. The latest data from 2014 indicates coverage of 134 million workers and about $60 billion in cost, split in half between medical and cash benefits for lost time.[1]. In the U.S., each state administers its own workers' compensation program, which leads to significant idiosyncrasies among these programs, though they often share some similar characteristics in terms of payment structures.[2]

Associated with workers' compensation is an inherent concern about a moral hazard problem. An increase in benefit generosity, while potentially could afford workers more time to fully recover from an injury, could also lead to either carelessness on the job upon returning to work (from the reassurance of higher payments) or an extended time spent away from work. Indeed, concerns about moral hazard and increasing claim costs have lead many states to consider reforms to their workers' compensation program in recent years.[3]

[1]National Academy of Social Insurance

[2]These include features such as the *replacement rate*, which is the percentage of a worker's wage that is paid during time out of work, as well as the maximum and minimum thresholds for these benefits.

[3]Hansen (2016) examines the impact of a 2014 reform in California designed to reduce cost and claim-filing frequency.

This paper contributes to the literature on the effect of increasing benefit generosity on injury duration, claim cost, and claim-filing behavior. Previous studies utilizing a difference-in-differences research design have provided strong evidence of moral hazard in the form of higher injury duration and more costly claim associated with more generous benefits,[4] though recent results have called into question whether moral hazard in a form of a disincentive to return to work is a significant factor in workers' decision-making proccess (Bronchetti (2012) or Hansen et al. (2017)).

Using the variation from an increase in maximum benefit starting in January 1, 2002 in Oregon, I find that injury duration does not substantially increase as a result of more generous benefits. This stands in contrast with the conclusions from previous research utilizing the variation from similar one-time large increases in benefits, but is consistent with findings from more recent studies regarding the small magnitude of moral hazard in workers compensation. I also find an increase in cost after the change in maximum benefit, but this increase is almost entirely attributable to the structural change in benefit generosity. Nonetheless, there is some evidence that workers are filing claims at increasing frequencies, though the magnitude of this moral hazard problem is small.

These findings carry significant implications for states looking to reform their workers' compensation programs. Increasing claim cost has often been cited as the primary reason for workers compensation reform, but the findings of this paper fit within a recent research agenda suggesting that work disincentives associated with higher benefit generosity might not be as severe a problem as previously thought.

The rest of this paper proceeds as follows: Section 2 outlines previous results on the impact of increasing workers' compensation benefits and describes the research

[4]See, for example, Meyer et al. (1995)

design. Section 3 gives information on data and methodology, while Section 4 provides the estimation results. Section 5 concludes with some thoughts on policy implications.

Background

Bolduc et al. (2002) review the literature on workers' compensation and classify the associated moral hazard into two types: *ex-ante* and *ex-post* moral hazard. The former happens before a workplace injury occurs and the latter happens after an injury occurs. One can imagine higher benefit generosity potentially leading to increased carelessness on the job and thus *ex-ante* moral hazard would predict that the probability of getting injured also increases upon a worker's return to work. As workers' compensation receipts go up, injured workers might find more incentives to stay on workers' compensation longer and thus *ex-post* moral hazard suggests claim costs will be higher due to this behavior.

Previous research has documented the existence of an ex-post moral hazard problem in workers' compensation. Among the earliest studies in this literature, Butler and Worrall (1985) and Krueger (1990a) both find significant disincentives to return to work as a result of increasing benefit generosity. Similar to the estimation strategy in this paper, Meyer et al. (1995) use large increases in maximum benefits in Kentucky and Michigan to estimate their impact on injury duration using a difference-in-differences framework. They find that there is an increase in time out of work associated with higher maximum benefits, with the injury duration - benefit generosity elasticities clustered between 0.3 and 0.4; that is, a 10% increase in benefit generosity increases injury duration by about 3 to 4%. Neuhauser and Raphael (2004) utilize a similar research design to investigate the effect of higher benefit generosity on duration and claim-filing frequency in California. Using the variation from two law changes in the state's worker' compensation program during the mid-1990s, they estimate similar duration - benefit elasticity to Meyer et al.

(1995), about 0.3, while the frequency - benefit elasticity is found to be about 0.5; that is, a 10% increase in generosity leads to a 5% increase in the probability of filing for a claim that includes temporary disability benefits. This result adds to the literature on the determinants of claim-filing decision; for instance, Biddle and Roberts (2003) find that the severity of injury, health, and benefit generosity are important predictors of the decision to file a temporary disability claim, Bolduc et al. (2002) find that workers are more likely to file claim for injuries that are hard to diagnose, and Hansen (2016) provide evidence that reforms to workers' compensation in California reduce the probability of claim filing and claim cost.

Bronchetti and McInerney (2012) reexamine the claim filing frequency effect associated with higher benefit generosity using 25 years of data from the Current Population Survey. In contrast with the findings from Neuhauser and Raphael (2004), they find that the frequency - benefit elasticity is around 0.1, indicating that workers are less responsive to changes in benefit generosity than were previously and that labor supply disincentives associated with higher generosity may not be as severe. This is consistent with recent findings from Hansen et al. (2017), who document a smaller duration - benefit elasticities (0.1 to 0.4) among injured workers in Oregon.

In this paper, I exploit the variation in injury duration and claim cost resulting from a one-time change in benefit generosity for workers' compensation in Oregon beginning January 1, 2002. Prior to this date, injured workers received 2/3 of their weekly wage in compensation up to a maximum benefit equaling 100% of the state's average weekly wage (SAWW), which is updated yearly to account for inflation by the state's Department of Labor. After January 1, this maximum benefit threshold increased to 133% of the SAWW.

Figure 14 gives a graphical demonstration of this increase. This figure plots the actual weekly benefit as a function of raw weekly benefit, calculated as 2/3 of a worker's weekly wage, for workers injured prior to the date of the law change in solid and for those injured after the law change in dashes. If raw benefit is below the maximum threshold (100% of the SAWW pre-2002 and 133% of the SAWW post-2002), the injured worker receive the full benefit amount.[5] However, once the calculated raw benefits exceed the maximum threshold, workers are only entitled to receive 100% of the SAWW if they are injured prior to January 1 2002 and 133% of the SAWW if they are injured after that date.

In my analysis, I define the workers whose raw benefits are less than or equal to 100% of the SAWW as the control group since this group of workers sees no change to their workers' compensation receipt as a result of the law change. Hence, the difference in outcomes before and after 2002 for this group reflects changes that are not due to the law change, such as the nature of work or demographic composition of workers. Define $Diff_{control}$ as this difference. Then, in the notation of Neuhauser and Raphael (2004):

$$Diff_{control} = Outcome_{control-post2002} - Outcome_{control-pre2002} \tag{4.1}$$

I subsequently define three treatment groups associated with the increase in benefit generosity in 2002: treatment group 1 includes those whose raw benefits fall between 100% of the SAWW and 133% of the SAWW, treatment group 2 includes only those with raw benefits exceeding 133% of the SAWW, and treatment group 3 defines treatment status similar to treatment group 2 but uses treatment group 1 as a control group. One could think of workers whose raw benefits exceed 133% of the SAWW as being able to receive the full treatment; that is, this group observes the largest increase in benefit generosity post-2002 (a 33% increase relative to the old maximum threshold). Workers

[5] I do not consider the minimum threshold in my analysis since very few workers receive the minimum payment, as it is relatively low (95% of workers' weekly wages or $50, whichever is less) and has not been updated in the last several decades.

whose raw benefits fall between 100% and 133% of the SAWW are, in a sense, "partially treated": they observe an increase in payment that is not as large as the full increase of 33%. (Empirically, I estimate the increase in benefit generosity for this group at about 13%.) Thus, treatment group 1 includes only partially treated workers, treatment group 2 includes only fully treated workers, and treatment group 3 contrasts the change in outcomes between the fully treated and the partially treated groups.

Regardless of the treatment degree, the outcome differences in the treated group during periods before and after 2002 should reflect the impact of both secular factors and benefit generosity. Thus,

$$Diff_{treatment} = Outcome_{treatment-post2002} - Outcome_{treatment-pre2002} \tag{4.2}$$

The difference-in-differences estimator, defined as:

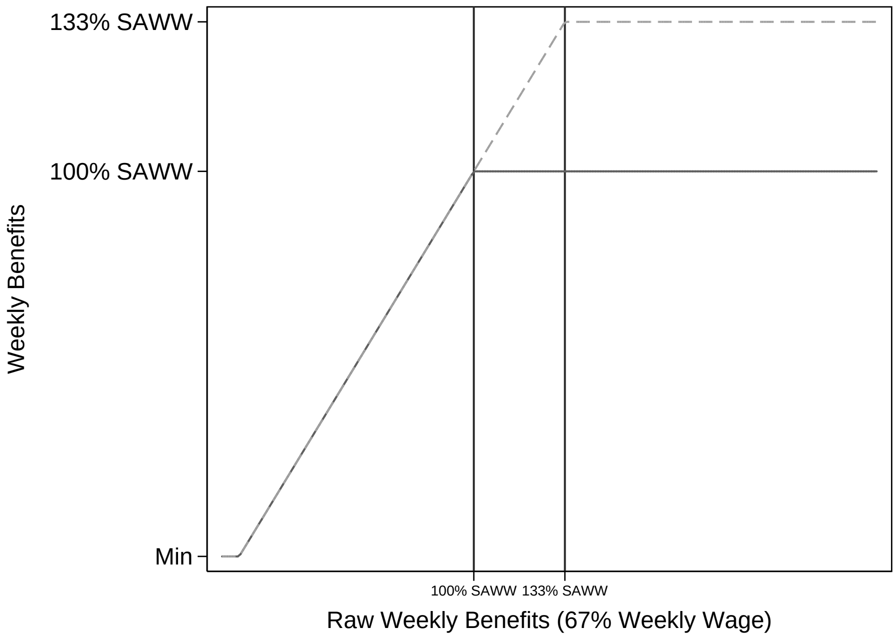
$$DD = Diff_{treatment} - Diff_{control} \tag{4.3}$$

will reflect the impact of increasing benefit generosity, net of other secular changes that affect all workers. In section 4, I present the regression-adjusted estimates of the DD estimator after accounting for differences in outcomes based on individuals' characteristics.

Data and Methodology

Data for this project comes from the Oregon Workers' Compensation Division, an agency under the state's Department of Consumer and Business Services. For every workers' compensation claim, the Division provides information on a worker's job and demographic characteristics. Specifically, I have data on a worker's date of injury, weekly

FIGURE 14. Change in Maximum Benefit in 2002



wage, industry of work, body parts injured, total claim cost, duration of injury, age, gender, and a non-confidential identification code for all workers.

The main regression model is:

$$Y_{it} = \beta_0 + \beta_1 Treated_{it} + \beta_2 Post02_{it} + \beta_3 Treated_{it} \times Post02_{it} + X\Gamma + u_{it} \tag{4.4}$$

Here, i indexes individual worker and t indexes year. Y_{it} is the outcome variable (injury duration, claim cost, and claim filing probability), $Treated_{it}$ is an indicator that equals to 1 if a worker belongs to a treatment group and 0 otherwise, $Post02_{it}$ is an indicator that equals to 1 if the worker is injured on or after January 1, 2002, and X is a matrix of control variables.[6] Some models also include year fixed-effects as additional controls, though their inclusion does not meaningfully impact the results.

In this model, β_2 is the change in the outcome for the control group after 2002, while $\beta_2 + \beta_3$ is the change in the outcome for the treatment group across this same period. One could imagine β_2 picking up the impact on the outcome variable due to other factors beside the increase in benefit generosity such as trends or work place compositional changes. β_3 is the primary coefficient of interest; it accounts for the effect of increasing benefit generosity on the outcome variable net of other factors that affect all workers.

Results

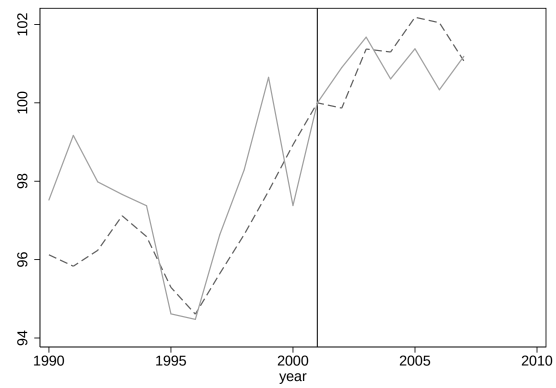
In Figure 15 and Figure 16, I assess the validity of the parallel trends assumption that is necessary to provide an unbiased estimator in the difference-in-differences framework. In these figures, the outcome variables (claim cost and injury duration) are aggregated to yearly level, logged, and normalized relative to the year before the law change (2001). Claim cost and injury duration are plotted for the time period beginning 1990 (the first year in my data set) to 2007 for each of the treated groups (in solid) and the control group (in dashes).[7] Visual inspection of Figure 15 and Figure 16 suggests that during the years prior to 1996, the control group and the treatment group are potentially not on parallel trends. This is particularly apparent for the fully treated group: Figure 15b and Figure 16b indicate that this group was on a divergent path relative to the control group during this period. Thus, I restrict my sample to only include workers injured from 1996 to 2007 to ensure that the parallel trends assumption is feasible in my analysis.

[6] I control for a worker's gender, age, industry type (based on the Standard Occupational Classification), and body part injured.

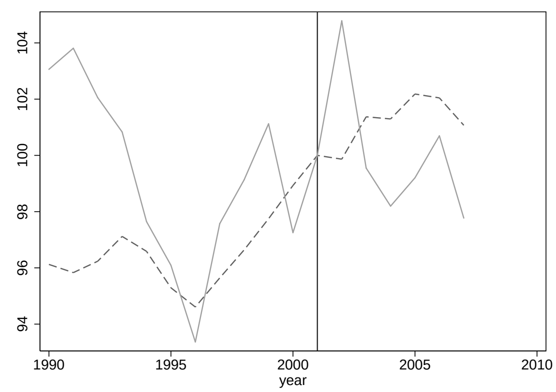
[7] I have data up to the end of 2010; nonetheless, the 2008 Recession, which lasted for several years afterwards, could potentially lead to violation of the parallel trends assumption and thus workers injured after 2008 are dropped from the sample.

FIGURE 15. Normalized Logged Injury Duration, Control vs. Treatment Groups

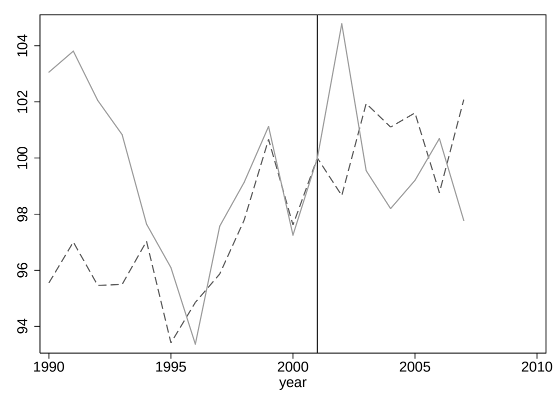
(a) Comparison 1



(b) Comparison 2



(c) Comparison 3



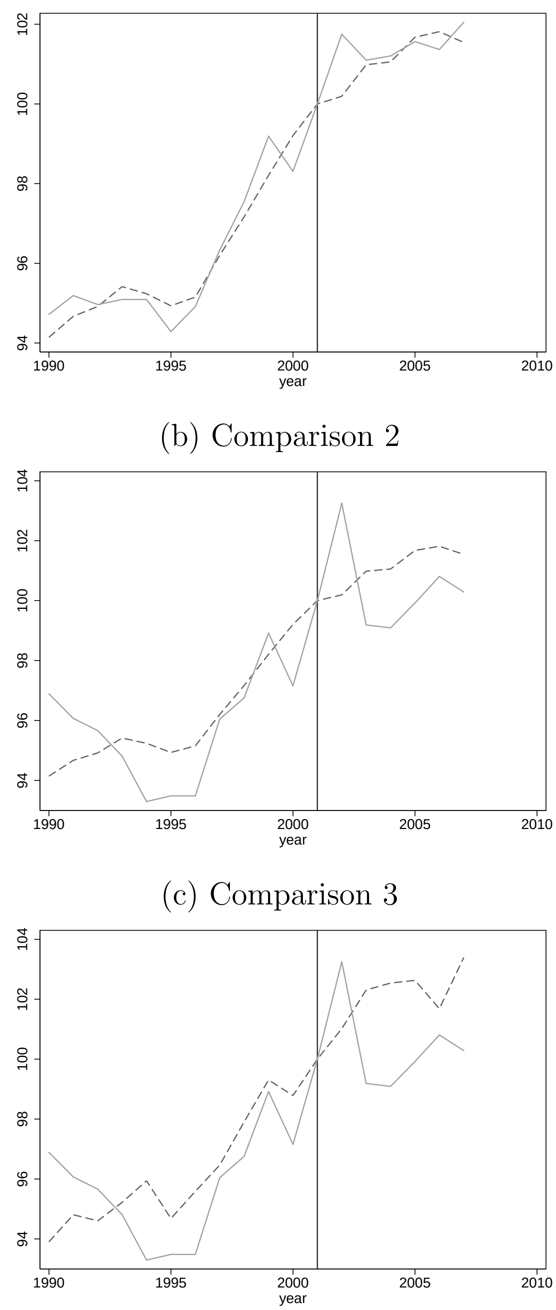
Notes: Individual worker data is aggregated to the yearly level. The time series for the control group is plotted in dashes while each of the treated group is plotted in solid. The vertical line denotes the year 2002, when the law change went into effect.

FIGURE 16. Normalized Logged Claim Cost, Control vs. Treatment Groups

(a) Comparison 1

(b) Comparison 2

(c) Comparison 3



Notes: Individual worker data is aggregated to the yearly level. The time series for the control group is plotted in dashes while each of the treated group is plotted in solid. The vertical line denotes the year 2002, when the law change went into effect.

Estimation results of Equation (4.4) are provide in Table 16 (outcome variable: logged injury duration) and Table 17 (outcome: logged claim cost). In these tables, columns (1) and (2) display results with treatment group 1 (sometimes referred to as Comparison 1), columns (3) and (4) display results with treatment group 2, and columns (5) and (6) display results with treatment group 3. The first column for every treatment group shows estimation result of Equation (4.4) while the second column adds year fixed effects to account for fluctuations across the years. I calculate the duration - generosity and cost - generosity elasticities based on estimated increases in benefit generosities for each of the treated group: 13% in treatment group 1, 33% in treatment group 2, and 20% increase in treatment group 3.

Results in Table 16 suggest that the increasing generosity of the 2002 law change carries no discernible impact on injury duration. Duration decreases by about 3% among the partially treated group, 0.2 to 0.5 % among the fully treated group, and thus, compared to the partially treated group, the workers in the fully treated group increase their duration by about 2%. Nonetheless, none of these estimates are statistically significant. Additionally, adding year fixed effects does not lead to meaningful changes to the results across all treatment groups. The estimated duration - generosity elasticity ranges from about -0.27 to 0.08, quite small compared to estimates from previous studies (around 0.4). Since the main conclusion from this table is that there is no significant impact on injury duration, it is not surprising to see in Table 17 that claim cost goes up by almost the same increase in benefit generosity: around 12% for the partially treated group and 30% for the fully treated group, with the cost elasticity ranging from 0.78 to 0.92.

Tables 31 and 32 in Appendix C replicate the specifications in Tables 16 and 17, but samples are restricted to workers injured with difficult-to-diagnose injuries. These injuries

TABLE 16. Effect of Benefit Generosity on Injury Duration

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	-0.0309	-0.0349	-0.0022	-0.0054	0.0176	0.0177
	(0.029)	(0.028)	(0.052)	(0.052)	(0.060)	(0.059)
Observations	268,910	268,910	255,753	255,753	22,669	22,669
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	-0.23	-0.27	-0.006	-0.015	0.08	0.08

Notes: Dependent variable: Log of days on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 17. Effect of Benefit Generosity on Claim Cost

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	0.1200***	0.1107***	0.2950***	0.2857***	0.1566**	0.1559***
	(0.038)	(0.034)	(0.054)	(0.052)	(0.065)	(0.059)
Observations	268,910	268,910	255,753	255,753	22,669	22,669
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	0.92	0.85	0.88	0.85	0.79	0.78

Notes: Dependent variable: Log of temporary disability paid. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

involve overextension and sprain or relates to the back. Bolduc et al. (2002) and Hansen (2016) both find heterogeneous responses to increasing benefit generosity among workers with such injuries. Nonetheless, results in Tables 31 and 32 suggest that the impact of higher generosity is quite similar among this group of workers relative to the entire sample.

The results so far have suggested that, as the maximum threshold increases to 133% of the SAWW after 2002, workers who receive better compensation are no more likely to prolong their injury duration and thus the increase in cost observed after 2002 is almost entirely due to the structural change in maximum benefit payments. Table 18 explores another possible dimension of moral hazard: in claim-filing behavior. In this table, the left-hand side of Equation (4.4) is an indicator variable that equals 1 if the duration of stay on workers' compensation is short (defined as a claim with duration of 5 days or less) and 0 otherwise. Duration of 5 days is selected because it is the 25th percentile of the entire injury duration distribution, though the finding is quite robust to this selection. We can observe that there is a significant increase in the probability of a short stay after the 2002 increase in benefit generosity among workers in the partially treated group: they are about 2% more likely to file claim for temporary disability benefits after 2002. Nonetheless, I do not find similar impact among the fully treated group. The frequency - generosity elasticity is estimated to be between -0.04 and 0.15. In Tables 33 and 34 in Appendix C, I present results where a short claim is defined as duration less than the 20th or 30th percentile of the injury duration distribution (4 and 6 days, respectively). The main conclusions in Table 18 still hold.

How do these estimates compare to previous results? Meyer et al. (1995) and Neuhauser and Raphael (2004) estimate duration - generosity elasticities of about 0.4 using data from Kentucky, Michigan, and California using a similar difference-in-differences framework while Hansen et al. (2017) estimate this elasticity to be about

TABLE 18. Effect of Benefit Generosity on Probability of Filing a Short Claim
(Short Claim = Injury Duration Less or Equal to 5 Days)

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	0.0178**	0.0186***	0.0086	0.0098	-0.0074	-0.0066
	(0.007)	(0.007)	(0.013)	(0.013)	(0.014)	(0.014)
Observations	268,910	268,910	255,753	255,753	22,669	22,669
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	0.14	0.15	0.02	0.03	-0.04	-0.03

Notes: Dependent variable: 1 if a claim is a short-stay claim (5 days or less on workers' compensation). Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

0.2 using a regression kink design. To put these elasticities into context, note first that generosity increases by 13% for the treatment group in comparison 1, while this increase is about 33% and 20% for the treatment groups in comparisons 2 and 3, respectively. Therefore, if the duration - generosity elasticity is 0.4, that would correspond to increases in injury duration of 5.2%, 13.2%, and 8% for the treatment groups in comparisons 1, 2, and 3 while an elasticity of 0.2 would translate into increases of 2.6%, 6.6%, and 4%, respectively.

From Table 16, the 95% confidence interval for the difference-in-differences estimator for each treatment group is -0.091 to 0.021, -0.107 to 0.096, and -0.099 to 0.134, respectively. We can observe that, for comparison 1, 0.052 is far above the upper bound of 0.021 while 0.026 just exceeds that number. For comparison 2, 0.132 again far exceeds the upper limit of 0.096 while 0.066 is below this limit, yet the implied increases in injury duration for comparison 3 of 0.08 and 0.04 is within the 95% confidence interval. Thus, broadly speaking, the decreases in injury duration for treated workers in comparison 1 are not congruent with previously-estimated elasticities; nonetheless, the impact on workers who are fully treated is more complicated. If the comparison group comprises of workers who are not affected by the law change, my results are more consistent with recent studies showing little moral hazard in workers compensation (e.g. Hansen et al. (2017) or Bronchetti and McInerney (2012)).

Additionally, Neuhauser and Raphael (2004) estimate a frequency - duration elasticity of 0.5, which implies increases in claim-filing probability of 0.065, 0.165, and 0.10 for treatment groups 1, 2, and 3. From Table 18, the 95% confidence interval for the DD estimator is from 0.0048 to 0.0325 for the partially treated group, -0.0156 to 0.0352 for the fully treated group, and -0.0342 to 0.0210 for the fully treated group when compared against the partially treated group. Here, it is apparent that the actual increase in claim-filing probability is not as much as previous frequency - generosity elasticities would imply.[8]

Figures 17, 18, and 20 present sensitivity analyses when the sample is restricted to only include workers injured within a specific years before and after the 2002 law change. In this figure, I plot the coefficient estimate for β_3 in Equation (4.4) and the associated 95% confidence interval in a regression with the logs of injury duration and claim cost as well as the probability of filing a short claim as the left-hand side variable, respectively, with the sample limited to workers injured within certain number of years pre- and post-2002 (one to five). In this framework, the sample is most restricted when it includes only workers injured a year before and after 2002 (from 2001 to 2003), thus moving to the right of the figures increases the sample size and power of the analysis.

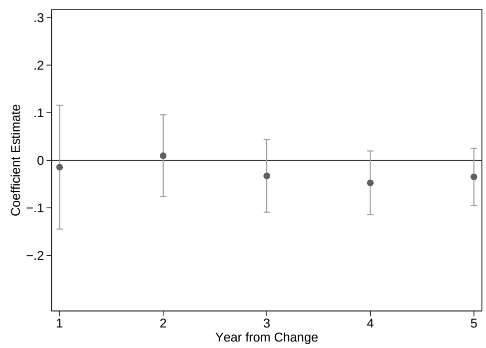
While the small sample size decreases the precision of the estimation routine, a number of conclusions are apparent from inspection of these figures: first, Figure 17 suggests a slight increase in injury duration among the fully treated group in the very short run (one year before and after the law change), though this increase is never statistically significant. Second, the increase in claim cost associated with higher benefit generosity in Figure 18 is quite stable across all treatment groups, and it is close to the structural increase in maximum benefits associated with each treatment group. Third, consistent with the findings in Table 18, only the partially treated group increases the probability of filing a short claim, as evidenced in Figure 19.

Finally, to further alleviate concerns about the possibility of the law change being passed as a result of an increasing (or decreasing) trends in the outcome variables, Figures 20, 21, and 22 plot the results of event studies intended to show the trends in the outcome

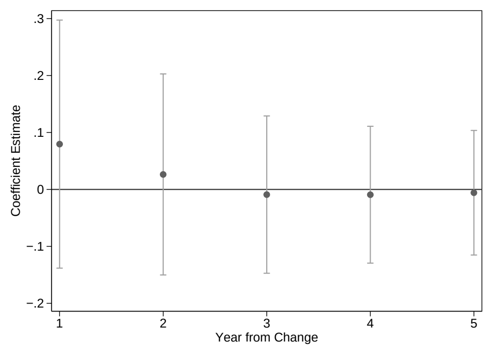
[8] It is helpful to keep in mind that the context of maximum benefit generosity increases might also differ between my study and previous difference-in-differences studies: Oregon's maximum benefit increases as a percentage of previous maximum while Kentucky, Michigan, and California observe absolute increases in dollars, which might imply different behavior changes among injured workers.

FIGURE 17. Effect of Benefit Generosity on Injury Duration
Sensitivity Analysis

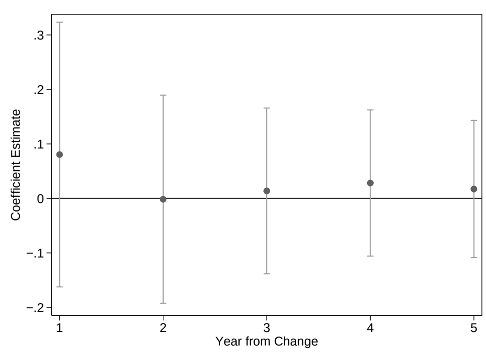
(a) Comparison 1



(b) Comparison 2



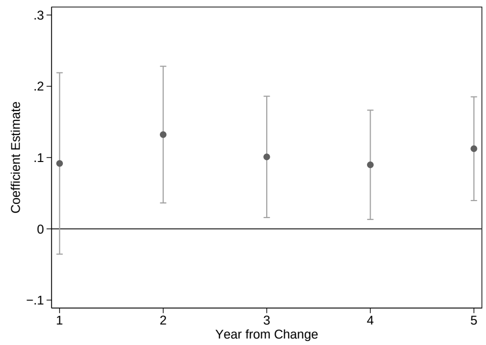
(c) Comparison 3



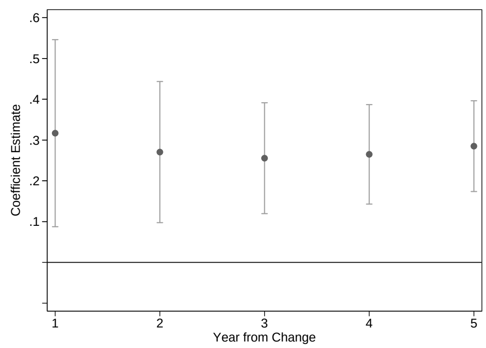
Notes: Each dot is the coefficient estimate for β_3 in Equation (4.4) and the associated line gives the 95% confidence interval. All regressions include control variables. Each point estimate - confidence interval pair corresponds to a regression where the sample is restricted to only include workers injured within specified year from the 2002 law change (both before and after). Standard errors are clustered at the bin width level.

FIGURE 18. Effect of Benefit Generosity on Claim Cost
Sensitivity Analysis

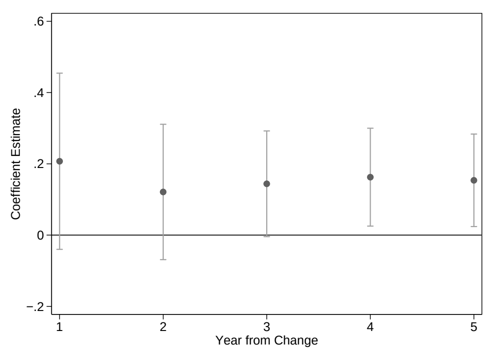
(a) Comparison 1



(b) Comparison 2

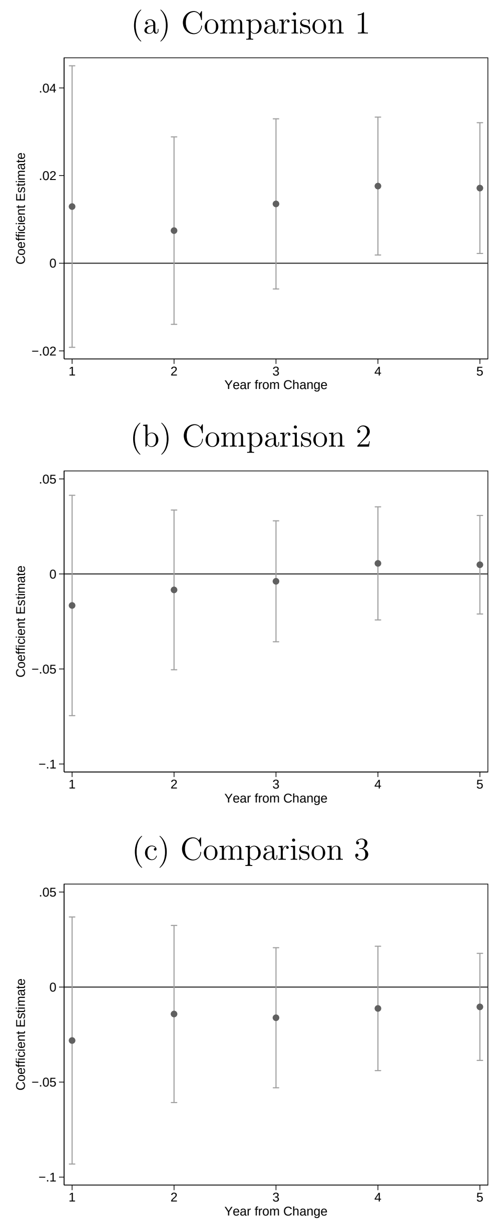


(c) Comparison 3



Notes: Each dot is the coefficient estimate for β_3 in Equation (4.4) and the associated line gives the 95% confidence interval. All regressions include control variables. Each point estimate - confidence interval pair corresponds to a regression where the sample is restricted to only include workers injured within specified year from the 2002 law change (both before and after). Standard errors are clustered at the bin width level.

FIGURE 19. Effect of Benefit Generosity on Probability of Filing a Short Claim (Short Claim = Injury Duration Less or Equal to 5 Days)
Sensitivity Analysis



Notes: Each dot is the coefficient estimate for β_3 in Equation (4.4) and the associated line gives the 95% confidence interval. All regressions include control variables. Each point estimate - confidence interval pair corresponds to a regression where the sample is restricted to only include workers injured within specified year from the 2002 law change (both before and after). Standard errors are clustered at the bin width level.

variables in the years before and after the 2002 law change. I first replace the $Post02_{it}$ variable in Equation (4.4) with a series of dummy variables indicating the years before and after the omitted year (2001, chosen to confirm a jump in outcome in 2002), and each dot and associated 95% confidence band is constructed from the coefficient and standard error estimates of the interaction between treatment status and the year dummies.

One potential reason for the event study is the possibility that the reform in workers' compensation maximum payment occurred as a result of increasing claim cost. If this is the case, we would expect to see increasing coefficient estimates as we approach the year of the law change from the left side (though given that the actual law change increases benefit generosity this is less likely). On the other hand, if law makers are concerned about benefits being too small to afford workers more time to fully heal, we would see a decrease in the coefficient estimates as we move closer to the year of the law change on the left side.

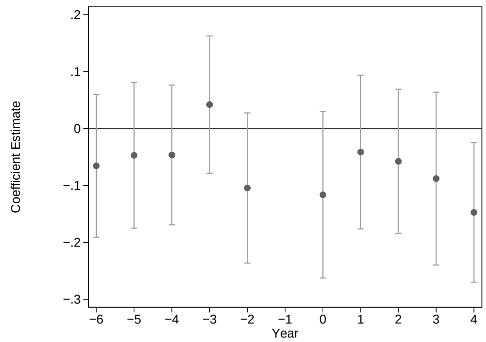
As we can observe in Figures 20, 21, and 22, it is not apparent that the law was passed in response to an increasing or decreasing trend in injury duration, claim cost, or claim filing probability since there does not seem to be an upward or downward trend in the years leading up to 2002. Additionally, we can see that claim cost exhibits the largest increase in 2002 for the fully treated group, consistent with the implementation of the new law.

Conclusion

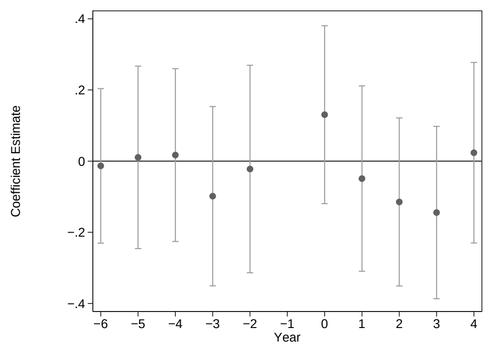
This paper examines the impact of a large benefit increase in maximum workers' compensation benefit on injury duration, claim cost, and claim filing behavior using 10 years of data in Oregon. Similar to previous studies, I document an increase in cost as a result of more generous benefits; nonetheless, this is entirely due to the structural change

FIGURE 20. Event Study: Timing of 2002 Law Change
Dependent Variable: Logged Injury Duration

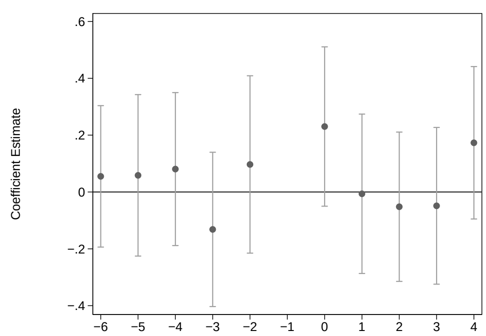
(a) Comparison 1



(b) Comparison 2



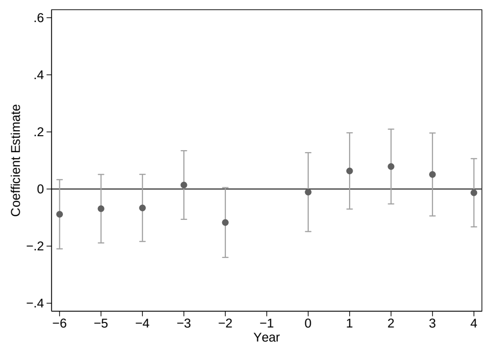
(c) Comparison 3



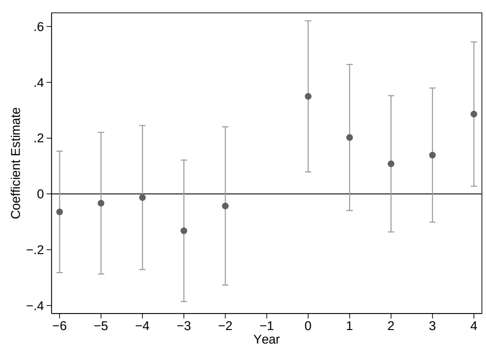
Notes: Each dot is the coefficient estimate for an interaction variable between an indicator for a certain number of years before or after 2002 with the treatment status. All regressions include control variables. Each point estimate - confidence interval pair corresponds to a regression where the sample is restricted to only include workers in each of the treatment groups. Standard errors in parentheses, allowing for clustering in bins (bin width = \$10).

FIGURE 21. Event Study: Timing of 2002 Law Change
Dependent Variable: Logged Claim Cost

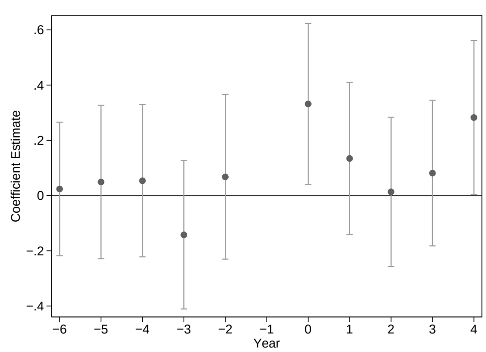
(a) Comparison 1



(b) Comparison 2



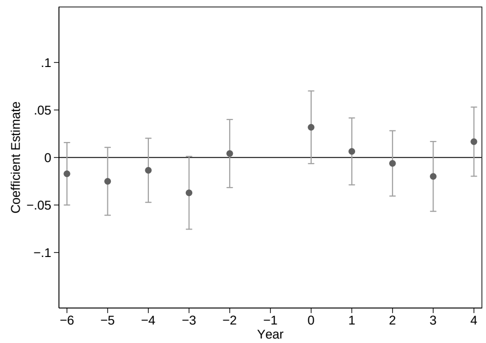
(c) Comparison 3



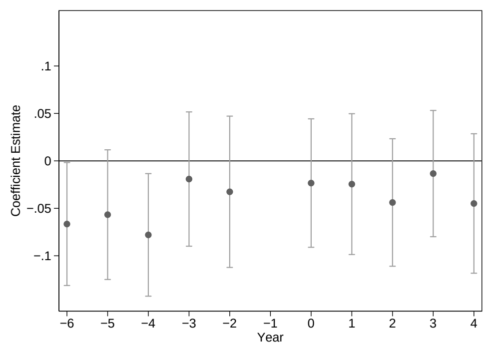
Notes: Each dot is the coefficient estimate for an interaction variable between an indicator for a certain number of years before or after 2002 with the treatment status. All regressions include control variables. Each point estimate - confidence interval pair corresponds to a regression where the sample is restricted to only include workers in each of the treatment groups. Standard errors in parentheses, allowing for clustering in bins (bin width = $10).

FIGURE 22. Event Study: Timing of 2002 Law Change
Dependent Variable: Indicator for Filing a Short-Stay Claim (Short Claim = Injury Duration Less or Equal to 5 Days)

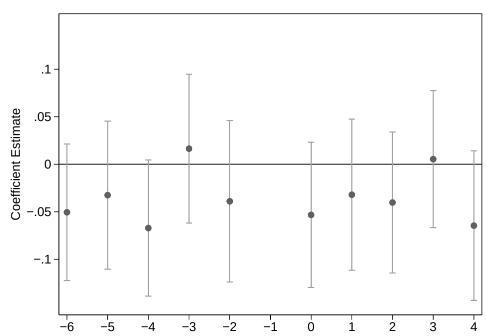
(a) Comparison 1



(b) Comparison 2



(c) Comparison 3



Notes: Each dot is the coefficient estimate for an interaction variable between an indicator for a certain number of years before or after 2002 with the treatment status. All regressions include control variables. Each point estimate - confidence interval pair corresponds to a regression where the sample is restricted to only include workers in each of the treatment groups. Standard errors in parentheses, allowing for clustering in bins (bin width = \$10).

in maximum benefit payments as injury duration does not change significantly, which is not in accordance with previous research that utilizes a difference-in-differences framework. I also document a small moral hazard incidence in claim filing behavior: some workers increase the frequency of filing a short claim as a result of higher benefit generosity, though the magnitude of this increase is small.

These findings have important policy implications for states that are considering reforms to workers' compensation. Similar to results from recent studies on moral hazard in workers' compensation program, I find that labor supply disincentives might not be a severe a problem as previously suggested, and thus cost reduction as a motivation for decreasing benefit generosity might not lead to a desired outcome. Second, the fact that a moral hazard problem exists in the form of an increase in short-stay claim-filing probability could present new challenges to policy makers in devising an optimal design for workers' compensation receipts.

CHAPTER V

CONCLUSION

The three substantive chapters in this dissertation represent a research agenda in public economics that focuses on policy design and evaluation. In Chapter II, I examine the impact of exonerations and executions in changing public sentiment regarding the death penalty. I find that living in a state where there is an exoneration significantly decreases support for the death penalty by about two to three percentage points, though this impact is only present in the short-run. Exonerations are also predicted to decrease the contemporaneous number of death sentences issued by states. Recent years have witnessed significant public interest in the legality and administration of capital punishment; the results in this chapter should be of particular interest for law makers in the process of evaluating the necessity or abolition of the death penalty.

In Chapters III and IV, I investigate the impact of increasing benefit generosity on injury duration and claim cost in workers' compensation program. Inherent in the design of social insurance programs like workers' compensation are concerns about a moral hazard problem. For example, with higher benefit receipts workers could be less careful once they return from the injury or extend their stay on workers' compensation during the injury period. Chapter III utilizes a novel application of the regression kink design to study these questions while Chapter IV uses the difference-in-differences approach often found in previous studies.

Results in Chapter III suggest a small increase in injury duration associated with more generous benefits, though the duration - generosity elasticity is lower than previous estimates. Claim costs are also predicted to increase as a result of increased benefit generosity. Additionally, I document a new form of moral hazard that has not been found in the literature: increasing benefit generosity leads to higher propensity to reinjure once workers return to the job.

In Chapter IV, I do not find a significant increase in injury duration as a result of a 33% increase in maximum benefit payment effective January 1, 2002 in Oregon. I find that claim costs increase after this date, but almost all of the increases are attributable to the structural change in maximum benefit. I also document a small increase in the probability of filing a short claim as a result of increasing benefit generosity.

Taken together, the results in Chapter III and Chapter IV suggest that labor supply disincentives associated with more generous benefits are not as severe as previously thought. Nonetheless, the increase in short-stay claim and reinjury probability present new challenges to policy makers looking to increase efficiency and equity of workers' compensation reimbursement.

The three chapters above are parts of a general research agenda focused on studying the public implications of various (plausibly exogenous) events. Other ongoing works of mine in this area include the evaluation of the role that mental health insurance laws play in possibly reducing violent crime rates and the examination of the impact of community uninsurance on the quality and access to health care for the insured population.

APPENDIX A

ADDITIONAL ROBUSTNESS CHECKS FOR CHAPTER II

TABLE 19. Impact of Event Definition
Unscale Event Count

VARIABLES	(1)	(2)
Exoneration	-0.0044***	-0.0027**
	(0.0010)	(0.0010)
Execution	0.0009	0.0003
	(0.0005)	(0.005)
Botched Execution	0.0096	0.0108
	(0.0072)	(0.0073)
Observations	35,133	35,133
State & Year FEs	Yes	Yes
Crime Rate	No	Yes

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Independent variable: number of event in a given state and year, 0 otherwise. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 20. Impact of Event Definition
Number of Event Indicators

VARIABLES	(1)	(2)
1(Exoneration = 1)	-0.0207**	-0.0222**
	(0.0083)	(0.0089)
1(Exoneration = 2)	-0.0333***	-0.0305***
	(0.0100)	0.0093
1(Exoneration >= 3)	-0.0328***	-0.0223***
	(0.0077)	0.0080
Observations	35,133	35,133
State & Year FEs	Yes	Yes
Crime Rate	No	Yes

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders. Independent variable: dummies for numbers of event in a given state and year. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 21. Impact of “Do Not Know” Responses

VARIABLES	(1)	(2)	(3)
1(Exoneration)	-0.0209***	-0.0273**	0.0064
	(0.0059)	(0.0064)	(0.0039)
Observations	37,232	37,232	37,232
State & Year FEs	Yes	Yes	Yes
Crime Rate	Yes	Yes	Yes

Notes: Dependent variable: binary indicator for supporting the death penalty as a sentence for persons convicted of murders, counting those who answer “do not know” as supporters of capital punishment (column 1) and opponents of capital punishment (column 2). The dependent variable in column 3 is an indicator variable that equals 1 if the GSS respondent answers “do not know” when asked about support for capital punishment. Independent variable: indicators for exposure to a certain number of exonerations. Standard errors in parentheses, allowing for clustering at the state level. *** significant at 1%; ** significant at 5%; * significant at 10%.

APPENDIX B

ADDITIONAL ROBUSTNESS CHECKS FOR CHAPTER III

TABLE 22. Effect of Benefit Generosity on Exiting Probability
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	-8.75e-06**	-6.50e-06	-3.10e-05*	-3.62e-05**	-1.01e-05***	-7.37e-06*	-3.19e-05**	-3.44e-05**
	(4.29e-06)	(4.48e-06)	(1.78e-05)	(1.83e-05)	(3.62e-06)	(3.79e-06)	(1.48e-05)	(1.52e-05)
Weekly Wage^2			-1.06e-07	-1.42e-07*			-1.10e-07	-1.35e-07*
			(8.29e-08)	(8.52e-08)			(7.24e-08)	(7.46e-08)
1(Weekly Wage above maximum)	-0.000493	-0.000322	-0.000113	0.000219				
	(0.000865)	(0.000885)	(0.00128)	(0.00130)				
Weekly Wage × 1(... above maximum)	9.20e-06	1.04e-05	4.70e-05	5.96e-05*	7.91e-06	9.55e-06	4.64e-05	6.08e-05*
	(8.41e-06)	(8.73e-06)	(3.19e-05)	(3.26e-05)	(8.06e-06)	(8.41e-06)	(3.10e-05)	(3.18e-05)
Weekly Wage^2× 1(... above maximum)			2.00e-08	3.34e-08			3.02e-08	1.35e-08
			(1.63e-07)	(1.67e-07)			(1.12e-07)	(1.15e-07)
Observations	1,110,202	1,109,547	1,110,202	1,109,547	1,110,202	1,109,547	1,110,202	1,109,547
Elasticity	-0.14	-0.16	-0.71	-0.91	-0.12	-0.15	-0.70	-0.92

Notes: Dependent variable: Probability worker exists workers' compensation in a given week. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 23. Effect of Benefit Generosity on Injury Duration, For Years Pre & Post 2002 Bandwidth: 200

Panel A: Pre 2002

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.0165**	0.0125*	0.0219	0.0206	0.0207***	0.0158***	0.0230	0.0185
	(0.007)	(0.007)	(0.026)	(0.026)	(0.005)	(0.005)	(0.020)	(0.020)
Weekly Wage^2			0.0000	0.0000			0.0000	0.0000
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	1.4964	1.2045	0.1426	-0.2514				
	(1.376)	(1.315)	(1.716)	(1.649)				
Weekly Wage × 1(... above maximum)	-0.0290**	-0.0269**	0.0073	0.0067	-0.0255*	-0.0241*	0.0080	0.0055
	(0.012)	(0.012)	(0.039)	(0.040)	(0.013)	(0.013)	(0.041)	(0.041)
Weekly Wage^2× 1(... above maximum)			-0.0003	-0.0003			-0.0003*	-0.0002
			(0.000)	(0.000)			(0.000)	(0.000)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	66,532	66,506	66,532	66,506	66,532	66,506	66,532	66,506
Elasticity	0.34	0.32	-0.09	-0.08	0.31	0.29	-0.09	-0.07

Panel B: Post 2002

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.0507***	0.0520***	0.1293	0.1077	0.0409**	0.0450**	0.1202*	0.1141*
	(0.018)	(0.017)	(0.078)	(0.069)	(0.017)	(0.017)	(0.066)	(0.060)
Weekly Wage^2			0.0004	0.0003			0.0003	0.0003
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	-3.5839	-2.5809	-1.1986	0.8452				
	(4.696)	(4.714)	(5.267)	(5.183)				
Weekly Wage × 1(... above maximum)	-0.0759*	-0.0899**	-0.3330**	-0.3300**	-0.0844**	-0.0960**	-0.3397**	-0.3253**
	(0.041)	(0.043)	(0.137)	(0.130)	(0.037)	(0.038)	(0.135)	(0.128)
Weekly Wage^2× 1(... above maximum)			0.0006	0.0007			0.0007	0.0006
			(0.001)	(0.001)			(0.001)	(0.001)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	6,135	6,124	6,135	6,124	6,135	6,124	6,135	6,124
Elasticity	1.58	1.85	6.94	6.88	1.76	2.00	7.08	6.78

Notes: Dependent variable: Days on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Panel A includes workers that were injured between fiscal year 1990 and December 31, 2001. Panel B includes workers that were injured between January 1, 2002 and fiscal year 2010. Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 24. Effect of Benefit Generosity on Temporary Benefits Paid, For Years Pre & Post 2002
Bandwidth: 200

Panel A: Pre 2002

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	5.9442***	5.5169***	6.8903***	6.8626***	6.4509***	5.9187***	7.7286***	7.3320***
	(0.540)	(0.500)	(2.217)	(2.022)	(0.419)	(0.380)	(1.701)	(1.544)
Weekly Wage^2			0.0045	0.0064			0.0079	0.0083
			(0.009)	(0.008)			(0.008)	(0.007)
1(Weekly Wage above maximum)	181.0061	143.8560	103.0429	57.7609				
	(107.839)	(101.307)	(144.872)	(131.851)				
Weekly Wage × 1(... above maximum)	-6.7927***	-6.7397***	-6.1848*	-6.7673**	-6.3687***	-6.4008***	-5.7022	-6.4961*
	(0.993)	(0.983)	(3.381)	(3.266)	(1.057)	(1.009)	(3.801)	(3.489)
Weekly Wage^2× 1(... above maximum)			-0.0131	-0.0137			-0.0224	-0.0189
			(0.017)	(0.017)			(0.013)	(0.013)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	66,532	66,506	66,532	66,506	66,532	66,506	66,532	66,506
Elasticity	1.01	1.01	0.93	1.01	0.95	0.96	0.85	0.97

Panel B: Post 2002

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	9.6812***	9.2756***	16.6623**	14.4127**	8.0232***	8.0711***	14.5927**	14.4265**
	(1.917)	(1.671)	(7.568)	(6.464)	(1.680)	(1.602)	(6.788)	(6.243)
Weekly Wage^2			0.0331	0.0244			0.0248	0.0244
			(0.037)	(0.032)			(0.035)	(0.031)
1(Weekly Wage above maximum)	-609.1230	-442.4886	-271.7370	1.8097				
	(495.782)	(494.335)	(526.772)	(514.493)				
Weekly Wage × 1(... above maximum)	-12.0160***	-13.0711***	-39.0573***	-39.5237***	-13.4507***	-14.1137***	-40.5671***	-39.5136***
	(3.951)	(4.170)	(12.865)	(12.674)	(3.691)	(3.678)	(12.702)	(12.003)
Weekly Wage^2× 1(... above maximum)			0.0741	0.0895			0.0983*	0.0893
			(0.066)	(0.066)			(0.057)	(0.057)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	6,135	6,124	6,135	6,124	6,135	6,124	6,135	6,124
Elasticity	2.14	2.32	6.94	7.03	2.39	2.51	7.21	7.02

Notes: Dependent variable: Payments to workers on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Panel A includes workers that were injured between fiscal year 1990 and December 31, 2001. Panel B includes workers that were injured between January 1, 2002 and fiscal year 2010. Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 25. Summary Statistics (at Threshold)-First Injury

Variable	Mean	Median	99 pct
Weekly Wage	768.62	714.00	1,585.16
Weekly Benefit	505.18	475.11	1,051.21
Injury Duration	60.73	20.00	554.00
Benefits Paid	4,873.01	1,625.56	40,736.00
Reinjury Probability	0.33	0	1

Notes: Sample restricted to those within $50 of the threshold, with non-missing running variables and non-rejected claims. Injury duration and benefits paid are top-coded at the 99 percentile.

TABLE 26. Effect of Benefit Generosity on Injury Duration-First Injury
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.02125***	0.01737***	0.05053*	0.04525	0.02280***	0.01879***	0.03598	0.02991
	(0.006)	(0.006)	(0.026)	(0.027)	(0.005)	(0.005)	(0.023)	(0.024)
Weekly Wage^2			0.00014	0.00013			0.00008	0.00007
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	0.55812	0.51062	-1.79665	-1.89806				
	(1.341)	(1.333)	(1.637)	(1.666)				
Weekly Wage*1(... above maximum)	-0.02816**	-0.02659*	-0.01261	-0.00591	-0.02687**	-0.02541*	-0.02092	-0.01471
	(0.013)	(0.014)	(0.045)	(0.047)	(0.013)	(0.013)	(0.044)	(0.045)
Weekly Wage^2*1(... above maximum)			-0.00038	-0.00040			-0.00022	-0.00023
			(0.000)	(0.000)			(0.000)	(0.000)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	54,779	54,742	54,779	54,742	54,779	54,742	54,779	54,742
Elasticity	0.35	0.33	0.16	0.07	0.33	0.32	0.26	0.18

Notes: Dependent variable: Days on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 27. Effect of Benefit Generosity on Temporary Benefits Paid-First Injury Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	5.93012***	5.57826***	8.76783***	8.55232***	6.21867***	5.82841***	8.38884***	8.06730***
	(0.441)	(0.410)	(1.977)	(1.858)	(0.372)	(0.365)	(1.770)	(1.734)
Weekly Wage^2			0.01349	0.01414			0.01197	0.01219
			(0.010)	(0.009)			(0.009)	(0.009)
1(Weekly Wage above maximum)	103.37333	89.75988	-46.81917	-59.98948				
	(109.401)	(108.228)	(147.735)	(143.838)				
Weekly Wage*1(... above maximum)	-6.25603***	-6.59667***	-7.49914**	-8.16943**	-6.01822***	-6.38927***	-7.71568**	-8.44754**
	(1.152)	(1.129)	(3.690)	(3.644)	(1.094)	(1.054)	(3.445)	(3.350)
Weekly Wage^2*1(... above maximum)			-0.02220	-0.02180			-0.01802	-0.01644
			(0.020)	(0.019)			(0.014)	(0.014)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	54,779	54,742	54,779	54,742	54,779	54,742	54,779	54,742
Elasticity	0.97	1.02	1.16	1.26	0.93	0.99	1.19	1.31

Notes: Dependent variable: Payments to workers on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 28. Effect of Benefit Generosity among Workers with Difficult-to-Diagnose Injuries Bandwidth: 200

Panel A: Injury Duration

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.0136	0.0118	0.0355	0.0327	0.0147**	0.0127*	0.0311	0.0287
	(0.009)	(0.009)	(0.035)	(0.035)	(0.007)	(0.007)	(0.022)	(0.022)
Weekly Wage^2			0.0001	0.0001			0.0001	0.0001
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	0.3665	0.3187	-0.5385	-0.4926				
	(1.412)	(1.405)	(2.235)	(2.250)				
Weekly Wage × 1(... above maximum)	-0.0207	-0.0196	-0.0388	-0.0388	-0.0198	-0.0188	-0.0414	-0.0411
	(0.013)	(0.013)	(0.049)	(0.052)	(0.015)	(0.014)	(0.053)	(0.055)
Weekly Wage^2 × 1(... above maximum)			-0.0001	-0.0001			-0.0001	-0.0001
			(0.000)	(0.000)			(0.000)	(0.000)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	43,983	43,970	43,983	43,970	43,983	43,970	43,983	43,970
Elasticity	0.26	0.25	0.37	0.37	0.25	0.24	0.53	0.53

Panel B: Temporary Benefits Paid

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	5.7545***	5.5168***	7.7348***	7.6068***	5.8339***	5.6366***	7.5855***	7.6542***
	(0.676)	(0.641)	(2.772)	(2.657)	(0.518)	(0.497)	(1.880)	(1.843)
Weekly Wage^2			0.0095	0.0100			0.0089	0.0102
			(0.011)	(0.010)			(0.008)	(0.008)
1(Weekly Wage above maximum)	28.3097	42.8164	-18.3623	5.8445				
	(115.243)	(112.874)	(186.740)	(183.103)				
Weekly Wage × 1(... above maximum)	-6.0275***	-6.5186***	-8.9559*	-10.0546**	-5.9601***	-6.4159***	-9.0441*	-10.0264**
	(1.185)	(1.170)	(4.535)	(4.633)	(1.216)	(1.188)	(4.659)	(4.732)
Weekly Wage^2 × 1(... above maximum)			-0.0042	-0.0020			-0.0025	-0.0025
			(0.023)	(0.023)			(0.016)	(0.016)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	43,983	43,970	43,983	43,970	43,983	43,970	43,983	43,970
Elasticity	1.01	1.10	1.51	1.69	1.00	1.08	1.52	1.69

Notes: Dependent variable: Days on temporary disability (A); Payments to workers on temporary disability (B). Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 29. Effect of Benefit Generosity on Reinjury Probability among Workers with Difficult-to-Diagnose Injuries
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.00016***	0.00017***	-0.00010	-0.00013	0.00018***	0.00017***	0.00016	0.00006
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Weekly Wage^2			-0.00000	-0.00000			-0.00000	-0.00000
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	0.00801	-0.00057	0.03205**	0.02303				
	(0.011)	(0.011)	(0.015)	(0.016)				
Weekly Wage × 1(... above maximum)	-0.00065***	-0.00046***	-0.00093**	-0.00062	-0.00063***	-0.00046***	-0.00078*	-0.00051
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Weekly Wage^2 × 1(... above maximum)			0.00000**	0.00000**			0.00000	0.00000
			(0.000)	(0.000)			(0.000)	(0.000)
Observations	43,983	43,970	43,983	43,970	43,983	43,970	43,983	43,970
Elasticity	1.37	0.97	1.96	1.31	1.33	0.97	1.64	1.07

Notes: Dependent variable: Reinjury probability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 30. Effect of Benefit Generosity on Reinjury Probability among Workers with Same-Type Reinjuries
Bandwidth: 200

	Discontinuity at Threshold Estimated				Discontinuity at Threshold = 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Wage	0.00007***	0.00007***	-0.00008	-0.00008	0.00011***	0.00010***	0.00012	0.00007
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Weekly Wage^2			-0.00000	-0.00000*			0.00000	-0.00000
			(0.000)	(0.000)			(0.000)	(0.000)
1(Weekly Wage above maximum)	0.01317**	0.00840	0.02526***	0.01852**				
	(0.006)	(0.005)	(0.009)	(0.008)				
Weekly Wage × 1(... above maximum)	-0.00036***	-0.00021***	-0.00044**	-0.00022	-0.00033***	-0.00019***	-0.00032	-0.00014
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Weekly Wage^2× 1(... above maximum)			0.00000**	0.00000*			-0.00000	-0.00000
			(0.000)	(0.000)			(0.000)	(0.000)
Observations	72,667	72,630	72,667	72,630	72,667	72,630	72,667	72,630
Elasticity	2.16	1.26	2.64	1.32	1.98	1.14	1.92	0.84

Notes: Dependent variable: Reinjury probability of the same type. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured during fiscal years 1990-2010, with weekly earnings within the bandwidth, whose claims were not rejected. Controls include fiscal year indicators, age, gender, nature of injury and industry indicators. Elasticity is calculated at the mean around the threshold. *** significant at 1%; ** significant at 5%; * significant at 10%.

APPENDIX C

ADDITIONAL ROBUSTNESS CHECKS FOR CHAPTER IV

TABLE 31. Effect of Benefit Generosity on Injury Duration
Among Workers with Difficult-to-Diagnose Injuries

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	-0.0066	-0.0093	0.0202	0.0211	0.0216	0.0261
	(0.033)	(0.032)	(0.074)	(0.074)	(0.083)	(0.083)
Observations	146,702	146,702	139,319	139,319	12,113	12,113
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	-0.05	-0.07	0.06	0.06	0.11	0.13

Notes: Dependent variable: Log of days on temporary disability. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 32. Effect of Benefit Generosity on Claim Cost
Among Workers with Difficult-to-Diagnose Injuries

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	0.1418***	0.1346***	0.2860***	0.2820***	0.1368	0.1403*
	(0.042)	(0.039)	(0.075)	(0.074)	(0.085)	(0.082)
Observations	146,702	146,702	139,319	139,319	12,113	12,113
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	1.09	1.04	0.87	0.85	0.68	0.70

Notes: Dependent variable: Log of temporary disability paid. Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 33. Effect of Benefit Generosity on Probability of Filing a Short Claim
(Short Claim = Injury Duration Less or Equal to 4 Days)

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	0.0148**	0.0155**	0.0051	0.0060	-0.0086	-0.0081
	(0.007)	(0.007)	(0.011)	(0.011)	(0.013)	(0.013)
Observations	273,207	273,207	259,783	259,783	23,308	23,308
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	0.11	0.12	0.02	0.03	-0.04	-0.03

Notes: Dependent variable: 1 if a claim is a short-stay claim (4 days or less on workers' compensation). Standard errors in parentheses, allowing for clustering in bins (bin width = \$10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

TABLE 34. Effect of Benefit Generosity on Probability of Filing a Short Claim
(Short Claim = Injury Duration Less or Equal to 6 Days)

	Comparison 1		Comparison 2		Comparison 3	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Treated)*1(Post 2002)	0.0227***	0.0235***	0.0112	0.0123	-0.0096	-0.0089
	(0.007)	(0.007)	(0.013)	(0.013)	(0.015)	(0.015)
Observations	273,207	273,207	259,783	259,783	23,308	23,308
Year fixed effects	No	Yes	No	Yes	No	Yes
Elasticity	0.17	0.18	0.03	0.04	-0.05	-0.04

Notes: Dependent variable: 1 if a claim is a short-stay claim (6 days or less on workers' compensation). Standard errors in parentheses, allowing for clustering in bins (bin width = $10). Sample restricted to those injured from 1996-2007 whose claims were not rejected. Control variables include age, gender, nature of injury, and industry indicators. Each comparison group corresponds to a previously-defined treatment status. *** significant at 1%; ** significant at 5%; * significant at 10%.

REFERENCES CITED

Basinger, S. J. (2013). Scandals and congressional elections in the post-watergate era. *Political Research Quarterly*, 66(2):385–398.

Biddle, J. and Roberts, K. (2003). Claiming behavior in workers' compensation. *Journal of Risk and Insurance*, 70(4):759–780.

Bohm, R. M. (1991). *The death penalty in America: Current research*. Academy of Criminal Justice Sciences, Northern Kentucky University.

Bolduc, D., Fortin, B., Labrecque, F., and Lanoie, P. (2002). Workers' compensation, moral hazard and the composition of workplace injuries. *Journal of Human Resources*, pages 623–652.

Bottan, N. L. and Perez-Truglia, R. (2015). Losing my religion: The effects of religious scandals on religious participation and charitable giving. *Journal of Public Economics*, 129:106–119.

Brace, P. and Boyea, B. D. (2008). State public opinion, the death penalty, and the practice of electing judges. *American Journal of Political Science*, 52(2):360–372.

Bronchetti, E. T. (2012). Workers' compensation and consumption smoothing. *Journal of Public Economics*, 96(5):495–508.

Bronchetti, E. T. and McInerney, M. (2012). Revisiting incentive effects in workers' compensation: Do higher benefits really induce more claims? *ILR Review*, 65(2):286–315.

Butler, R. J., Durbin, D. L., and Helvacian, N. M. (1996). Increasing claims for soft tissue injuries in workers' compensation: cost shifting and moral hazard. *Journal of Risk and Uncertainty*, 13(1):73–87.

Butler, R. J. and Worrall, J. D. (1983). Workers' compensation: benefit and injury claims rates in the seventies. *The Review of Economics and Statistics*, pages 580–589.

Butler, R. J. and Worrall, J. D. (1985). Work injury compensation and the duration of nonwork spells. *The Economic Journal*, 95(379):714–724.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Card, D., Lee, D., Pei, Z., and Weber, A. (2012). Nonlinear policy rules and the identification and estimation of causal effects in a generalized regression kink design. Technical report, National Bureau of Economic Research.

Card, D., Lee, D. S., Pei, Z., and Weber, A. (2015). Inference on causal effects in a generalized regression kink design. *Econometrica*, 83(6):2453–2483.

Chetty, R., Looney, A., and Kroft, K. (2009). Salience and taxation: Theory and evidence. *The American Economic Review*, 99(4):1145–1177.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014). Peer effects in program participation. *The American Economic Review*, 104(7):2049–2074.

Dezhbakhsh, H., Rubin, P. H., and Shepherd, J. M. (2003). Does capital punishment have a deterrent effect? new evidence from postmoratorium panel data. *American Law and Economics Review*, 5(2):344–376.

Dezhbakhsh, H. and Shepherd, J. M. (2006). The deterrent effect of capital punishment: Evidence from a judicial experiment. *Economic Inquiry*, 44(3):512–535.

Dills, A. K. and Hernández-Julián, R. (2012). Negative publicity and catholic schools. *Economic Inquiry*, 50(1):143–152.

Dionne, G. and St-Michel, P. (1991). Workers' compensation and moral hazard. *The Review of Economics and Statistics*, pages 236–244.

Dong, Y. (2010). Jumpy or kinky? regression discontinuity without the discontinuity. *Working Paper*.

Donohue, J. J. and Wolfers, J. (2009). Estimating the impact of the death penalty on murder. *American Law and Economics Review*, page ahp024.

Donohue III, J. J. and Wolfers, J. (2006). Uses and abuses of empirical evidence in the death penalty debate. Technical report, National Bureau of Economic Research.

Fisher, P. and Pratt, T. (2006). Political culture and the death penalty. *Criminal Justice Policy Review*, 17(1):48–60.

Flanagan, T. J., Flanagan, T. J., and Longmire, D. R. (1996). Public opinion on crime and jusice: History, development and trends. *Americans View Crime and Justice*, 229.

Gelber, A., Moore, T., and Strand, A. (2015). The impact of disability insurance on beneficiaries earnings: is the income effect large? In *2015 Fall Conference: The Golden Age of Evidence-Based Policy*. Appam.

Godlonton, S. and Thornton, R. L. (2013). Learning from others' hiv testing: Updating beliefs and responding to risk. *The American Economic Review*, 103(3):439–444.

Gross, S. R. (1997). Update: American public opinion on the death penalty-it's getting personal. *Cornell L. Rev.*, 83:1448.

Gross, S. R., OBrien, B., Hu, C., and Kennedy, E. H. (2014). Rate of false conviction of criminal defendants who are sentenced to death. *Proceedings of the National Academy of Sciences*, 111(20):7230–7235.

Hansen, B. (2016). Californias 2004 workers compensation reform: Costs, claims, and contingent workers. *ILR Review*, 69(1):173–198.

Hansen, B., Waddell, G., and Nguyen, T. (2017). Benefit generosity and moral hazard: Evidence from regression kink. *Working Paper*.

Huber, G. and Gordon, S. C. (2004). Accountability and coercion: Is justice blind when it runs for office? *American Journal of Political Science*, 48(2):247–263.

Huff, C. R. (2002). Wrongful conviction and public policy: The american society of criminology 2001 presidential address. *Criminology*, 40(1):1–18.

Hungerman, D. M. (2013). Substitution and stigma: Evidence on religious markets from the catholic sex abuse scandal. *American Economic Journal: Economic Policy*, 5(3):227–253.

Katz, L., Levitt, S. D., and Shustorovich, E. (2003). Prison conditions, capital punishment, and deterrence. *American Law and Economics Review*, 5(2):318–343.

Keil, T. J. and Vito, G. F. (1991). Fear of crime and attitudes toward capital punishment: A structural equations model. *Justice Quarterly*, 8(4):447–464.

Krueger, A. B. (1990a). Incentive effects of workers' compensation insurance. *Journal of Public Economics*, 41(1):73–99.

Krueger, A. B. (1990b). Workers' compensation insurance and the duration of workplace injuries. Technical report, National Bureau of Economic Research.

Landais, C. (2015). Assessing the welfare effects of unemployment benefits using the regression kink design. *American Economic Journal: Economic Policy*, 7(4):243–278.

Lange, F. (2011). The role of education in complex health decisions: evidence from cancer screening. *Journal of health economics*, 30(1):43–54.

Longmire, D. R. (1996). Americans' attitudes about the ultimate weapon: Capital punishment.

Lundqvist, H., Dahlberg, M., and Mörk, E. (2014). Stimulating local public employment: Do general grants work? *American Economic Journal: Economic Policy*, 6(1):167–192.

Maier, J. (2011). The impact of political scandals on political support: An experimental test of two theories. *International Political Science Review*, 32(3):283–302.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics*, 142(2):698–714.

Meyer, B. D., Viscusi, W. K., and Durbin, D. L. (1995). Workers' compensation and injury duration: evidence from a natural experiment. *The American Economic Review*, pages 322–340.

Neuhauser, F. and Raphael, S. (2004). The effect of an increase in worker's compensation benefits on the duration and frequency of benefit receipt. *Review of Economics and Statistics*, 86(1):288–302.

Nielsen, H. S., Sørensen, T., and Taber, C. (2010). Estimating the effect of student aid on college enrollment: Evidence from a government grant policy reform. *American Economic Journal: Economic Policy*, 2(2):185–215.

Norrander, B. (2000). The multi-layered impact of public opinion on capital punishment implementation in the american states. *Political Research Quarterly*, 53(4):771–793.

Oster, E., Shoulson, I., and Dorsey, E. (2013). Optimal expectations and limited medical testing: evidence from huntington disease. *The American Economic Review*, 103(2):804–830.

Paula, Á. D., Shapira, G., and Todd, P. E. (2014). How beliefs about hiv status affect risky behaviors: Evidence from malawi. *Journal of Applied Econometrics*, 29(6):944–964.

Radelet, M. L. and Bedau, H. A. (1998). The execution of the innocent. *Law and Contemporary Problems*, 61(4):105–124.

Sarat, A. (2014). *Gruesome spectacles: botched executions and America's death penalty*. Stanford University Press.

Simonsen, M., Skipper, L., and Skipper, N. (2016). Price sensitivity of demand for prescription drugs: exploiting a regression kink design. *Journal of Applied Econometrics*, 31(2):320–337.

Sims, B. and Johnston, E. (2004). Examining public opinion about crime and justice: A statewide study. *Criminal Justice Policy Review*, 15(3):270–293.

Soss, J., Langbein, L., and Metelko, A. R. (2003). Why do white americans support the death penalty? *The Journal of Politics*, 65(2):397–421.

Stack, S. (2000). Support for the death penalty: A gender-specific model. *Sex Roles*, 43(3):163–179.

Thaler, R. H. (1994). Psychology and savings policies. *The American Economic Review*, 84(2):186–192.

Turner, L. J. (2014). The road to pell is paved with good intentions: The economic incidence of need-based student aid. *Working Paper*.

Vonnahme, B. M. (2014). Surviving scandal: An exploration of the immediate and lasting effects of scandal on candidate evaluation. *Social Science Quarterly*, 95(5):1308–1321.

Warr, M. (1995). Poll trends: Public opinion on crime and punishment. *The Public Opinion Quarterly*, 59(2):296–310.