THREE ESSAYS IN APPLIED MICROECONOMICS ON THE TOPICS OF CRIME, POLLUTION, AND NATIONAL PARKS

AN ABSTRACT

SUBMITTED ON THE FOURTH DAY OF APRIL 2018

TO THE DEPARTMENT OF ECONOMICS

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

OF THE SCHOOL OF LIBERAL ARTS

OF TULANE UNIVERSITY

FOR THE DEGREE

OF

DOCTOR OF PHILOSOPHY

BY

MICHAEL STEVEN SPANBAUER

APPROVED: ______________________

Alan Barreca, Ph.D.
Director

____

James Alm, Ph.D.

____

Patrick Button, Ph.D.

THREE ESSAYS IN APPLIED MICROECONOMICS ON THE TOPICS OF CRIME, POLLUTION, AND NATIONAL PARKS

AN ABSTRACT

SUBMITTED ON THE FOURTH DAY OF APRIL 2018

TO THE DEPARTMENT OF ECONOMICS

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

OF THE SCHOOL OF LIBERAL ARTS

OF TULANE UNIVERSITY

FOR THE DEGREE

OF

DOCTOR OF PHILOSOPHY

BY

Signed on 4/4/2018

MICHAEL STEVEN SPANBAUER

APPROVED: *Signed on 4/4/2018*
Alan Barreca, Ph.D.
Director

Signed on 4/4/2018
James Alm, Ph.D.

Signed on 4/4/2018
Patrick Button, Ph.D.

Note: Original signatures archived at Tulane University

Abstract

I present three essays in applied microeconomics. In the first, I use police records to explore whether changing self defense policies, known as *Stand Your Ground*, have differential effects across race. I find that implementing these policies leads to an additional 1.611 monthly killings of black Alleged Perpetrators of Crimes, 70.8 percent of whom are killed by black citizens, while only causing an additional 0.345 monthly killings of white Alleged Perpetrators, 97.7 percent of whom are killed by white citizens. In the second, I examine the causal relationship between waterborne uranium exposure and birth outcomes in order to more fully understand the external costs of the activities that increase the probability of human exposure to uranium. I find that waterborne uranium contamination does not cause an observable decrease in human capital endowment proxies. In the third, I estimate the national average passive use value for Alaskan National Parks. I find that respondents are willing to pay $115 to $409 for a 5 percent expansion of the Denali National Park.

THREE ESSAYS IN APPLIED MICROECONOMICS ON THE TOPICS OF CRIME, POLLUTION, AND NATIONAL PARKS

A DISSERTATION

SUBMITTED ON THE FOURTH DAY OF APRIL 2018

TO THE DEPARTMENT OF ECONOMICS

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

OF THE SCHOOL OF LIBERAL ARTS

OF TULANE UNIVERSITY

FOR THE DEGREE

OF

DOCTOR OF PHILOSOPHY

BY

Signed on 4/4/2018

MICHAEL STEVEN SPANBAUER

APPROVED: *Signed on 4/4/2018*
Alan Barreca, Ph.D.
Director

Signed on 4/4/2018
James Alm, Ph.D.

Signed on 4/4/2018
Patrick Button, Ph.D.

Note: Original signatures archived at Tulane University

TABLE OF CONTENTS

Chapter 2: Effect of Waterborne Uranium Exposure on Human Capital Endowment Proxies.

Chapter 3: How Valuable are National Parks? Evidence from a Proposed National Park Expansion in Alaska.

List of References

LIST OF TABLES

LIST OF FIGURES

STAND YOUR GROUND: SELF-DEFENSE POLICY, JUSTIFIED HOMICIDES, AND RACE.

By Michael Spanbauer

I use police records to explore whether changing self defense policies, known as Stand Your Ground, have differential effects across race. I find that implementing these policies leads to an additional 1.611 monthly killings of black Alleged Perpetrators of Crimes, 70.8 percent of whom are killed by black citizens, while only causing an additional 0.345 monthly killings of white Alleged Perpetrators, 97.7 percent of whom are killed by white citizens. Tests indicate that these racial disparities are significant in all cases, while falsification and robustness tests address concerns of endogenous policy creation. Results provide evidence that Stand Your Ground policies cause unequal outcomes between races.

Keywords: crime; self-defense; Stand Your Ground; criminal policy; discrimination.

JEL: K42, Z18.

1 Introduction

Affirmative self-defense policies are among the more controversial laws in the United States. These policies, colloquially referred to as *Stand Your Ground* (*SYG*), can mitigate a defendant's culpability in civil or criminal proceedings after a fatal shooting. While proponents insist that *SYG* protects innocent individuals from frivolous prosecution, opponents argue that it lowers the cost of using deadly force and results in increased homicide rates, such as one recent empirical study that finds *SYG* policies cause approximately 30 additional homicides each month (McClellan and Tekin, 2016; hereafter MT). Furthermore, opponents regularly argue that *SYG* policies induce important racial disparities; it is frequently claimed that *SYG* laws "make it easier to kill blacks."[1] These racial disparities are the concern of this paper.[2]

In this paper I empirically measure the racial disparities that are directly attributable to *SYG*. Specifically, I utilize the cross-race and own-race killings of Alleged Perpetrators of Crimes (APCs) as the metric to measure the policy's cost to different racial groups.[3] Each human life has a measurable economic value (Conley, 1976; Droman, 2009), implying that each killing imposes a cost to society in the form of lost human capital investment funded by the community through public schools and other social programs (Schultz, 1961; Glomm and Ravikumar, 1992) and the deceased's forfeited lifetime earnings (Rice and Cooper, 1967). I measure these costs across race groups by examining the following categories of killings, hereafter referred to as "race-pairs": blacks killing black APCs, whites killing black APCs, blacks killing white APCs, and whites killing white APCs. By examining the disparate effects across these race categories, my research directly addresses the media claims that the black community disproportionately pays the costs of *SYG* policies.

This paper makes several contributions to the existing base of literature. Most importantly, this paper provides the first causal exploration of *SYG*'s effects on reportedly justified homicides between race-pairs.[4] I accomplish this by combining

[1]See news stories such as "States Are Quietly Resurrecting a Law That Makes It Easier to Kill Blacks" (accessed on 28 July 2017 from theroot.com/states-are-quietly-resurrecting-a-law-that-makes-it-eas-1794633188), "McKnight killing shows how Louisiana's stand your ground' law codifies bigotry" (accessed on 28 July 2017 from thelensnola.org/2017/01/06/mcknight-killing-shows-how-louisianas-stand-your-ground-law-codifies-bigotry), "Stand Your Ground Laws Complicate Matters For Black Gun Owners" (accessed on 28 July 2017 from npr.org/sections/codeswitch/2017/02/27/517109271/stand-your-ground-laws-complicate-matters-for-black-gun-owners).

[2]I do not discuss the moral, ethical, and legal arguments for or against these policies because they are thoroughly examined by many authors in journals of law and policy, including Catalfamo (2006), Ross (2007), Megale (2010), Lawson (2012), and Lave (2012).

[3]I create the term "Alleged Perpetrators of Crimes" to dispassionately describe the person(s) killed because the deceased cannot be posthumously convicted of a crime related to the "reportedly justified homicide" during which they were killed. As these deceased individuals are unable to explain or defend their actions, I refrain from using terms that imply guilt.

[4]I also explore racial disparities in urban and rural settings to determine if the observed effects

a rich set of policy variables with individual-level covariates and then employing an identification strategy that permits causal interpretation under plausible identifying assumptions. A novel aspect of this approach is my use of justified fatal shooting records to identify the race of both the shooter and the APC killed. The most closely related antecedents, Cheng and Hoekstra (2012; hereafter CH) and MT, isolate the causal effects of *SYG* on total homicides,[5] but do not fully explore the racial disparities due to data limitations that prevent identification of the shooter's race. Other papers, including a well publicized Urban Institute report, identify correlative evidence of racial disparities without exploring causal differences in general or by race.

I construct a panel dataset by combining detailed *SYG* policy implementation data with police records obtained through the FBI's Supplementary Homicide Report. These data allow me to identify the race of the perpetrator and the race of the victim for all reportedly justified homicides where a private citizen uses a firearm.[6] I then employ a generalized difference-in-differences model to calculate the change in the reportedly justified use of lethal force caused by the implementation of *SYG* policies. The key identifying assumption is that, in the absence of *SYG*, the average change in homicide rates would have been similar for states that have and have not enacted the policy (Mora and Iliana, 2012). Although this assumption is not directly testable, I conduct a number of falsification and robustness tests to support the assumption and address the natural concerns of endogenous policy creation.

The first key finding of this paper is that *SYG* policies significantly increase the number of black APCs killed each month. The second key finding of this paper is that the incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the individual

are specific to metropolitan regions. These results are presented in the appendix.

[5] I replicate multiple CH and MT results, supporting their conclusions.

[6] I follow the work of MT and use firearm-related homicides so my results can be accurately referenced in future discussions of gun policy. Results using all reportedly justified homicides are similar in sign and magnitude; these are available upon request.

purported to shoot in self-defense. The empirical magnitudes are large, both in levels and proportionately. For example, fatal shootings of black APCs increase by 6–14 percent ($p < 0.05 - p < 0.01$) while fatal shootings of white APCs increase by only 0–3 percent (not statistically significant). In terms of human lives lost, an average of 1.611 additional black APCs and 0.345 additional white APCs are killed each month nationally. These effects are larger within race than across race ($p < 0.01$): 70.8 percent of the 1.611 black APCs killed are killed by black citizens, and 97.7 percent of the 0.345 white APCs killed are killed by white citizens.

2 *Stand Your Ground* Institutions and Related Literature

United States law commonly extends strong protections to individuals who defend their person or family while inside their homes. However, individuals in public venues have historically been obligated by law to attempt a safe retreat prior to using force in self-defense, a requirement known as one's "duty to retreat" (Levin, 2010). Self defense policy began substantially changing between the years 2005 and 2014, during which twenty-seven states enacted an explicit set of rules enhancing an individual's right to defend their person and their family while outside their home. By creating this affirmative defense, *SYG* policies "reduce the expected cost of using lethal force" (CH). Becker (1968) explains that a reduction in expected punishment will increase a citizen's propensity to perform the punishable action, suggesting that *SYG* policies will increase the likelihood that a citizen will kill an APC.

The first major change during this period began with Florida's Senate Bill 436, passed in October of 2005. 18 more states passed similar policies in 2006 and 2007, and eight more states followed suit over the following seven years. A graphical depiction of the observed policy changes over time can be found in Figure 1.

Figure 2 illustrates the geographic location of *SYG* states. It can be seen that many south-eastern states have enacted these policies; states that have frequently shared similar political views since the adoption of the "Southern Strategy" by Republicans (Boyd, 1970). This is not a concern for this analysis because Figure 3, which plots the political affiliation of *SYG* states' legislative bodies, indicates that both bipartisan support and bipartisan opposition exists for *SYG* policies. To create Figure 3, I plot the composition of the State Senate and the State House of Representatives at the time the state enacted the policy alongside the political affiliation of non-*SYG* states in the year 2010, which is the midpoint of the treatment period. 10 of the 27 states that enacted *SYG* policies during the observed treatment period had either one or both chambers of their state legislature controlled by Democrats. In five other states, no policies were enacted during the observed time period despite both chambers of state legislature being controlled by Republicans.

The *SYG* policies enacted by these states effectively remove the individual's duty to retreat (Boots, Bihari, and Elliott, 2009), allowing them to use deadly force even if they are able to safely retreat and deescalate the situation. In the first systematic analysis of these policies, CH examine police records and find that *SYG* causes an 8 percent increase in reported murders and non-negligent homicides annually. MT extend the analysis by using monthly mortality data, rather than annual police records, and by modifying their definition of *SYG*. MT find that *SYG* causes approximately 30 additional murders or non-negligent homicides each month. Both sets of authors briefly examine the policies' effect on reportedly justified homicides, but only incidentally and without considering race-pair interactions.

In practice, *SYG* removes the duty to retreat by providing the individual with an "affirmative defense."[7] This affirmative defense requires the government to presume that the citizen reasonably believed that deadly force was necessary and also to

[7] An affirmative defense is a legal tool that mitigates a defendant's culpability in civil or criminal proceedings. See LAW.CORNELL.EDU/WEX/AFFIRMATIVE_DEFENSE, accessed 13 June 2017.

presume that the APC had the intention of using violence against the individual. These two presumptions, taken together, permit the citizen to claim self-defense and grant the individual immunity to prosecution. If these presumptions are disproved throughout the course of the police investigation, then the protections granted by the *SYG* policy are revoked.

3 Data

3.1 Data Sources

I conduct my analysis using panel data aggregated to the state and month level, which requires two key components. First, detailed information pertaining to each homicide is needed to calculate the per capita reportedly justified homicide rate in each observed month for each race-pair, including the demographics for both the shooter and the deceased. Second, the month that each *SYG* policy was enacted for each state is needed to distinguish the reportedly justified shootings occurring after the change.

I take homicide data from the FBI's monthly Uniform Crime Reporting (UCR) program's Supplementary Homicide Report (SHR), 2000–2014. The program records details of each homicide "incident," as defined by the UCR program. The FBI's SHR counts all reported homicides and categorizes them by the method of – and the reason for – death, making it possible to identify homicides performed as acts of self-defense. Each observation also includes information on the victims, the offenders, the weapons used, and the circumstances surrounding the homicide. This makes the SHR distinctive for its ability to provide data on both the deceased and the shooter, whereas other data sets, such as the Center for Disease Control's Multiple Cause of Death (MCOD) report, only provide information about the deceased. As a result, the SHR permits a unique investigation into the interactions between racial groups, which is impossible to do with other data sets.

The SHR data are available in two forms: summary files providing total numbers of homicides in each state but a limited number of other identifying variables, and raw files containing details of every individual reported homicide event. Unlike previous studies, I elect to use the raw files, and then separate each event into unique observations for each victim. Through this process, I am able to obtain an accurate count of the total homicides in each state, as is available in the summary files, while also maintaining access to the rich set of covariates. Other authors who use the raw SHR data files employ a binary variable to indicate when a homicide event involves multiple victims (Roman, 2013), which makes interpreting the results difficult. My process facilitates the interpretation of my results, which are presented as *SYG*'s cost to human life.

I identify *SYG* policies by locating the public records of each original legislative action.[8] The effective dates of each state's *SYG* policy, along with the name of the bill creating the protections, can be found in Table 1. It can be seen that 27 states changed their laws during the observed period, one state had a *SYG* policy in place prior to the observed period, and the remaining states never enacted these expanded self-defense rules. Of these 27 states, Florida is excluded from my analysis for reasons discussed in Section 3.2.

[8] I utilized each state's public directory of statutes. I also searched for mentions of any other state laws or policies in non-academic sources, such as websites hosted by politically motivated lobbying groups and websites intended to provide information to firearm enthusiasts. Through these sources, I discovered a 2007 Oregon State Supreme Court ruling regarding enforcement practices of the existing self-defense statute, ORS 161.219. The court's decision on the case, *State of Oregon v. Sandoval*, included the following statement: "On a purely textual level, ORS 161.219 contains no specific reference to 'retreat,' 'escape,' or 'other means of avoiding' a deadly confrontation. Neither, in our view, does it contain any other wording that would suggest a duty of that kind." After this decision, the law in Oregon was enforced in the same manner as a state that passed new *SYG* legislation. Therefore, for the purpose of analyzing the changing self-defense rules, the effect of the court ruling is identical to the effect of a legislative action.

3.2 Data Quality

Two data quality issues should be noticed. The first is the availability and quality of homicide records from the state of Florida. The second issue is the potential for incomplete reporting, or measurement error, of homicides nationally.

The first issue arises because the FBI purposefully excluded Florida when it compiled and published the SHR data. CH, the *SYG* researchers who also use SHR data, directly contacted the Florida Department of Law Enforcement Office and obtained numbers to use in place of the excluded FBI data. I also obtained the Florida Department of Law Enforcement Office data, but I then contacted the FBI and inquired why Florida is excluded from their reports. I was told Florida does not follow the FBI's data quality guidelines for reporting.[9]

To determine if I should use this data, I test my model with and without the data from the Florida Department of Law Enforcement Office. I find that excluding the Florida data causes my results to converge towards zero, but does not alter their practical interpretation. Based on this test and my conversation with the FBI, I elect to exclude the Florida data from my analysis. If the Florida data truly merit exclusion, then my results represent the true treatment effect. If the Florida data should have been included, then my results represent the lower bound for the true treatment effect and maintain their validity. I consider this to be the most conservative solution to the problem at hand, since the FBI did not clarify which of their data reporting guidelines was violated.[10]

The second issue arises because the SHR's reporting requirement is not strictly

[9]The UCR program guidelines are published at UCR.FBI.GOV/DATA-QUALITY-GUIDELINES-NEW. Some published requirements could affect the data's quality if they are violated, such as the requirements for "logical consistency," "reasonableness," and "adherence to sound estimation methodologies." Other published requirements would not affect the data's quality if they are violated, such as the requirement to "allow adequate time for reviews" or "provide methodologies, origins of data."

[10]I spoke with an FBI representative and inquired why Florida was excluded from the SHR data. When I requested a quotable statement for this paper, the representative provided me with the following written statement: "The SHR data reported by the state of Florida does not follow UCR program guidelines and are not used."

enforced, implying that the data may not include all perpetrated homicides.[11] This would only present a problem for my analysis if the reporting behavior covaried with changes in *SYG* policies. However, CH postulate that "there is no reason to believe that any total homicide reporting issue at any state level should be systematically correlated with changes in *SYG* law." I verify this by examining reporting behaviors in Section 5.1. I find that reporting behaviors are not correlated with changes in policy, and I show that homicide reporting does not present a risk to my analysis or the analysis of CH.

3.3 Sample Selection and Summary Statistics

I construct an outcome variable that allows my results to be interpreted as *SYG*'s national cost to human life each month. To accomplish this, I first categorize each shooting by the race of the citizen and the APC, and then tally the total number of reportedly justified shootings for each race-pair at the state level. I make these state-totals comparable across state lines by dividing by the population of all reporting agencies and multiplying by 1,000,000.[12] I identify reportedly justified homicides committed after the enactment of *SYG* by using the policy dates listed in Table 1. I report the mean and standard deviation of the reportedly justified homicide rates in Table 1; full data statistics are listed in column 1, statistics for states that never enacted *SYG* policies are listed in column 2, and statistics for states that at enacted *SYG* during the observed time period are listed in column 3. Columns 4 and 5 further examine states that enacted *SYG* by listing the mean reportedly justified homicide rates before and after the policy, respectively.

[11] See Wiersema, Loftin, and McDowall (2000) for a thorough discussion.

[12] Following the example of MT, I use the population of the deceased's race.

4 Econometric Methodology

I empirically measure racial differences that are directly attributable to SYG by examining how enacting SYG policies affects the cross-race and own-race killings of APCs. To accomplish this, I use variation in state policy as a natural experiment and employ a generalized difference-in-differences model to analyze how these policies influence the reportedly justified homicide rates between race-pairs over time. The outcome variable used is the monthly number of reportedly justified homicides per 1,000,000 citizens in reporting jurisdictions, aggregated to the state level.

I follow convention and transform my outcome variable so as to interpret my results in terms of a percent-change. This transformation is commonly performed using the natural logarithm of the outcome variable, but my data set contains many zeros at which the logarithmic transformation would be undefined. Cheng and Hoekstra (2013) solve this problem in their data by adding one to each state's observed homicide count, but I elect to use the Inverse Hyperbolic Sine (IHS) transformation.[13] The IHS transformation has the same interpretation as the logarithmic transformation, but has the benefit of being defined at zero. As discussed by Pence (2006), the transformation of an outcome variable, X, is defined as

$$sinh^{-1}(X) = ln(X + \sqrt{(X^2 + 1)}\ )$$

The transformation of large values of X becomes $sinh^{-1}(X) \approx ln(2) + ln(X)$, a vertical displacement of the logarithmic transformation of X, while the transformation of $X = 0$ is simply $sinh^{-1}(0) = ln(1) = 0$. Whereas a logarithmic transformation may require dropping the zero values, which may cause the model to overestimate causal relationships (Friedline, Masa and Chowa, 2015), the IHS transformation yields

[13]This transformation was first proposed by Johnson (1949), discussed in economic applications by Burbidge, Magee, and Robb (1988), MacKinnon and Magee (1990) and Pence (2006), and also has been used Card and DellaVigna (2013).

precise estimates in the presence of zero values.

I model the IHS of the justified homicide rate ($Y_{s,t}$) for each race-pair as:

$$Y_{s,t} = \alpha + \delta(P_{s,t}) + \lambda_s + \mu_t + \varepsilon_{s,t} \tag{1}$$

where the coefficients α and δ are unknown parameters and $\varepsilon_{s,t}$ is an idiosyncratic error term. On the right-hand side of the regression equation, I incorporate the changing policies with a binary variable ($P_{s,t}$) equal to 1 if the state (s) has already enacted a SYG policy in the observed month and year (t). I also include λ_s, a vector of fixed effects controlling for variations caused by the state in which the homicide event occurred, and μ_t, a vector of fixed effects controlling for variations caused by the month and year in which the homicide event occurred. The inclusion of λ_s and μ_t prevent bias from spurious correlations between the enactment of SYG policies and prominent events at the month- or state-level. I do not include linear time trends, but, in Section 5.1, I show that their inclusion does not change my estimates. I follow the example of CH and the suggestions of Solon, Haider and Wooldridge (2015) by weighting my observations by the average population[14] measured over the sample period. I also repeat these regressions without including weights; results are similar in sign, magnitude, and statistical significance. Finally, I use robust standard errors clustered at the state level to account for spatially correlated errors. With this framework, my estimation of δ is interpreted as the percent change in the monthly homicide rate caused by the implementation of SYG policies.

To demonstrate that my methods yield results similar to the work of published authors, I use my methods to construct the IHS of the total homicide rate and to replicate the primary result of CH: the "8 percent net increase" of total homicides highlighted in their abstract. I match CH's sample time period[15] and treatment

[14] Following the example of MT, I use the population of the deceased's race
[15] 2000–2010.

classifications,[16] and then I replicate their estimate in Table 3, columns 1 and 2. The similarity of our results provides strong evidence supporting the analysis and findings of CH, while also corroborating the conclusions of MT.[17]

The remainder of Table 3 presents regression results as I make individual changes to CH's model and data until it more closely resembles the data and model used in this analysis. For brevity, I refer to observations from states which have enacted *SYG* as "the treatment group," and all other observations as "the control group." In specification 3, I broaden my sample to include 2000-2014 data, which moves my estimate closer to 6 percent. In specifications 4 and 5, I modify the treatment group,[18] which moves my estimate closer to 4 percent. I change from measuring homicides per 100,000 to homicides per 1,000,000 in specification 6, which has a negligible effect. I switch from annual to monthly data measurements in specification 7, increasing my sample size from 829 to 8,149 and increasing the magnitude of the treatment effect to 13 percent. The increase in magnitude is expected, given the timing of *SYG* enactment: since enactment occurs mid-year in all but one instance, aggregating observations to annual levels will reduce the estimated treatment effect by include untreated observations into the treatment group or treated observations into the control group. Finally, in specification 8, I follow the example of MT and narrow my sample to include only firearm-related homicides so that my results can be discussed in the context of firearm policy. This has a negligible impact on the estimated treatment

[16] Thier classification includes Florida in the sample and classifies Oregon as untreated. This required the use of the data that I obtained from the Florida Department of Law Enforcement Office.

[17] I also check if my methods can corroborate the results reported by CH and MT regarding reportedly justified shootings committed by private citizens. CH employs an unweighted OLS regression using a simple count of justified homicides as the outcome variable as well as a negative binomial regression. Their OLS model estimates an increase of 3.2 justified homicides per state, a result that cannot be interpreted in terms of percent-change, and their negative binomial model estimates an increase of 28 to 57 percent per state. MT employs OLS and Poisson regressions on simple counts of reportedly justified homicides across and also found statistically significant coefficients, but these coefficients do not have a practical interpretation. I examine moderately similar outcomes in Tables A.3 and A.4 and find results similar in sign and magnitude, providing supporting evidence for the conclusions of CH and MT.

[18] I include Oregon and exclude Florida from the treatment group, for the reasons discussed in Section 3.

effect.

4.1 Assumptions

Difference-in-differences models have been employed in several seminal articles, such as Ashenfelter (1978), Ashenfelter and Card (1985), and Card and Krueger (1993), and they rely on a key assumption for the estimates to be consistent and unbiased (Bertrand, Duflo, and Mullainathan 2004). The key identifying assumption is that, in the absence of SYG, the average change in homicide rates would have been similar for states that have and have not enacted the policy. This assumption permits non-SYG states to serve as counterfactuals for what would have happened in SYG states if the policy had not been enacted (Mora and Iliana, 2012).

Although direct verification of this assumption is unattainable, inspection of the periods prior to passage of SYG policies can support the assumption's veracity. Figure 4 plot the number of justified homicides of black APCs and white APCs in Panels A and B, respectively. It can be seen that the impact of time is consistent for all citizens: reportedly justified homicide trends generally rise and fall together during the period before these policy changes begin. It can also be seen that the impact of residing in a state that eventually did, or did not, enact a SYG policy is consistent across time: the difference between reportedly justified homicide rates is generally constant during the period before these policy changes begin. I repeat these graphs using the IHS transformation of justified homicides, and find identical results; these graphs are presented in the appendix. Given these findings, I consider states that did not change their policies to be good counterfactuals for the states that did.

The key assumption is further corroborated by an event study. I aggregate the data at the annual level and then interact five lead and five lag period dummies with SYG indicators using the following model where k is the year that the SYG policy is enacted:

$$Y_{s,t} = \alpha + \sum_{j=-5}^{5} \beta_j P_{s,t}(t = k + j) + \lambda_s + \mu_t + \varepsilon_{s,t} \tag{2}$$

I plot the results in Figure 5, omitting the 12 months prior to *SYG* enactment so that all interactions are expressed relative to this period. If states that do not change their policies are to be good counterfactuals for the states that did, this event study must not show a statistical difference between the two groups in the periods prior to *SYG* enactment; *e.g.*, insignificant results for periods -5 through 0 (Pischke, 2005; Angrist and Pischke, 2008). Results from periods prior to *SYG* enactment are statistically insignificant, supporting my decision to use these observations as counterfactuals in my analysis.

5 Primary Results

I now present the racially disparate results of my primary analysis. Table 4a presents my estimate of δ, the percent change in the monthly reportedly justified homicide rate caused by the enactment of *SYG* policies.[19] These results show that *SYG* policies significantly increase the number of black APCs killed each month. I also estimate these results while excluding population weights; results are similar in sign, magnitude, and statistical significance. Given the timing of *SYG* enactment, as presented in Table 1, these results should be interpreted as the average increases occurring in the 2–8 years following the policy change. Table 4b tests the joint significance of these estimates to determine if the incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed.

The first two columns of Table 4a show that *SYG* policies induce a 14.34 percent increase ($p < 0.01$) in the number of black APCs shot by blacks, and a 5.56 percent

[19]I repeat my analysis for urban and rural jurisdictions to determine if the observed effects are specific to metropolitan regions. These results are available in the appendix.

increase ($p < 0.05$) in the number of black APCs shot by whites. These findings partially support the claims that *SYG* "makes it easier to kill blacks." However, these results neither support nor disprove the implied claims of institutional racism, as it can be seen that the majority of the APCs are killed during own-race interactions.

In contrast to the clearly defined effect for black APCs, the measured effects of *SYG* on white APCs is small and statistically insignificant. The final two columns in Table 4a indicate that the increase of white APCs shot by blacks is approximately 0.52 percent, the smallest point estimate of my primary results, but the number of white APCs shot by members of their own race increases by 2.26 percent.

To aid interpretation of my results, I convert[20] the percent changes into the monthly number of lives lost in the United states as a result of these policies. My analysis finds that 1.611 additional black APCs are shot and killed nationally each month as a result of *SYG* policy. Of these, 1.410 (70.8 percent) are killed by blacks and 0.201 (29.2 percent) are killed by whites. These results are significant at the one percent and five percent levels, respectively. For white APCs, this number is far smaller: only 0.345 additional white APCs are shot and killed nationally each month. Of these, 0.009 (2.3 percent) are killed by blacks and 0.337 (97.7 percent) are killed by whites. Neither of these results are statistically significant.

I follow the methods discussed by Cameron and Trivedi (2009) to confirm that these point estimates are significantly different. For each race-pair combination, I jointly estimate a system of Seemingly Unrelated Regression (SUR) equations and test for equality between coefficients. In this test, the critical value for significance at the one percent level is 6.63. Table 4b presents the relevant χ^2 test statistics, and indicates that one difference is significant at the five percent level while all others

[20] Calculation performed using changes in reportedly justified rates from Table 4, average number of reportedly justified shootings from Table 1, the average historical percentage of black citizens (12.48 percent) and white citizens (83.29 percent) as recorded in SEER data (accessed on 30 June 2015 from SEER.CANCER.GOV), and current population within the states that have enacted *SYG* (180,767,212) as estimated by U.S. Census Bureau at the time of writing (accessed on 30 June 2017 from CENSUS.GOV/POPCLOCK).

are significant at the one percent level. This test strongly suggests the presence of a disparity in how *SYG* policies influence the reportedly justified homicide rates within and between racial groups.

Despite identifying and measuring the racially disparate effects induced by these policies, I cannot fully calculate the welfare implications of *SYG*. As discussed earlier, critics of *SYG* claim that these policies impose substantial costs to human life while proponents insist that the policies protect innocent individuals from frivolous prosecution, thereby creating a social benefit. My analysis indicates that the costs are disproportionately paid by the black community, but I can neither identify nor disprove the purported benefits, which prevents a complete calculation of the net welfare implications.

5.1 Placebo, Sensitivity, and Robustness Checks

I now present a series of tests to provide evidence that supports the validity of my analysis. I conduct each test using the same model as described in Section 4. All outcome variables are calculated per 1,000,000 citizens, aggregated to the state and month level, and IHS transformed.

5.1.1 Reporting Behavior

I begin by confirming that total homicide reporting is not systematically correlated with changes in *SYG* policy, as discussed in Section 3. I use UCR program data[21] to examine the total number of homicides reported each month. This report records every murder or nonnegligent manslaughter event regardless of whether the event was eventually deemed to be fake, baseless, unfounded, or only an "attempted" murder. If reporting activity was systematically correlated with changes in *SYG* law, this

[21] Data taken from the UCR's Offenses Known and Clearances by Arrest dataset, rather than the UCR's Supplementary Homicide Report dataset.

correlation would be observed in Table 5a. I find no correlation, and conclude that total homicide reporting is not systematically correlated with SYG policies.

An additional concern is that police agencies might use policy changes to manipulate their reports or create more favorable numbers. I therefore examine three additional measures of reporting behavior over time. The first is the number of homicides deemed to be unfounded, baseless, or fake during the course of the reported month.[22] The second is the "actual" homicides in a given month, or the difference between the reported and the unfounded cases in a specific month. The third is the "clearance rate," or the number of arrests[23] divided by the number of actual homicides for that month. Tables 5b, 5c and 5d present these results: I find SYG is not correlated with any of these reporting behaviors, suggesting widespread misreporting did not begin occurring as a result of SYG policies.

5.1.2 Placebo and Falsification Tests

Next, I examine three events that should be exogenous to changes in SYG policy: homicides performed in manners that are unrelated to self-defense, traffic fatalities, and unemployment rates. If my analysis is truly capturing the effect of enacting a SYG policy, rather than some other unobserved trend in fatalities, then I expect to find no correlation between the enactment of SYG policies and any of these events. I also separate each analysis by race, to determine if any racial trends are present.

I conduct the first placebo test by restricting my raw data to homicides performed in manners that are unrelated to self-defense,[24] rather than reportedly justified homicides. This test serves to check if an underlying homicide trend exists, which may be spuriously correlated with SYG policies. Using the same treatment assignments

[22] According to the reporting manual, reportedly justified homicides should be included in the unfounded category in the month they are determined to be justified.

[23] This also includes clearance by "exceptional means," such as when a murderer commits suicide and cannot be arrested

[24] Including poison, arson, explosion, or by causing a drug overdose.

and the same data window, I repeat my analysis with these placebo homicides as the outcome variable. All results are small in magnitude and statistically insignificant, which provides evidence to support my analysis.

I conduct the second placebo test by examining the traffic fatality rates for both black and white individuals. I use publicly available traffic fatality data, obtained from the National Highway Traffic Safety Administration, and attempt to identify correlations between SYG policies and trends in national traffic fatalities. I include this test to check for an underlying trend in the general number of fatalities nationwide, which should not be correlated with SYG. I again find no significant results, which supports my analysis.

I conduct the last placebo test using Federal Reserve Economic Data. I examine unemployment rates for black and white citizens to determine if unemployment trends are coincidently correlated with SYG policies. I include this test because negative economic conditions around the time of the policy changes may cause an increase in criminal activity, which would introduce bias to my analysis. However, I find no evidence of a correlation for either race group, suggesting the results of my primary analysis are indeed driven by SYG rather than underlying economic conditions.

5.1.3 Sensitivity Analysis

I test the sensitivity of my results to my model's specification by including various fixed effects and modifying the sample time period. I do not explore the effect of including other controls, such as police presence or incarceration rates, as these have been shown to be of little importance by the existing literature (Cheng and Hoekstra, 2013; Roman, 2013; McClellan and Tekin, 2016). I report these results in Figure 6, and I include my primary results for comparison. Test 1 adds State×Month fixed effects to the original model, and Test 2 replaces State fixed effects with Regional fixed effects. Test 3 includes linear time trends to the original model, which does not impact the sign or magnitude of the results. Tests 4 and 5 restrict the sample time period around the years 2005–2007, when the bulk of the states enacted a *SYG* policy. It can be seen that all results are well within the confidence interval of the original estimates, indicating that my results are robust to different choices in specification and sample periods. This analysis also provides evidence that my sample time period was not selected to accentuate a preconceived set of results.

6 Discussion and Conclusion

6.1 Potential Mechanisms for Racial Disparities

Several mechanisms could drive the results found in this analysis. One such explanation could be a simple selection issue: if a percentage of all human interactions degrade into reportedly justified homicides and if this percent varies between race-pairs, then a reduction in a homicide's expected cost would generate the racial disparities observed in this paper. If this is the case, then it could be said that *SYG* itself induces the racially disparate increase in killings. Other plausible mechanisms include, but are not limited to, implicit biases held by the individuals who commit the reportedly justified homicides, public perceptions of law enforcement and prosecutorial behaviors, or the availability of law enforcement officers in a given geographic location.

Implicit biases, or evaluations that are "activated outside of conscious attention" (Bargh, Chaiken, Govender, and Pratto, 1992), can cause racial minorities to appear "more aggressive, even when exhibiting the same behaviors as Caucasians" (American Bar Association, 2015). These biases can also be activated by an object or person and then mistakenly attributed to a different object (Greenwald and Banaji, 1995); for example, observing a black individual and then nonconsciously evaluating a harmless object to be a dangerous weapon. Studies show that implicit bias is more influential when making quick decisions under pressure (Dovidio, Gaertner, and Kawakami, 2002).

Considering that the average self-defense shooting occurs in 3–5 seconds (Beretta, 2014), it is plausible that implicit bias may influence an individual's threat perception during a confrontation with an APC.

An unpublished and non-random study conducted by Project Implicit has identified the existence of individuals who hold an implicit bias associating black persons with dangerous weapons.[25] The existence of such individuals suggests it is plausible for *SYG* policies to increase the killings of minorities, who would be implicitly associate with deadly weapons. If implicit bias can cause a black APC to appear more aggressive than an otherwise identical white APC, then this mechanism could explain the first part of my findings: why the killings of blacks increase significantly more than the killings of whites ($p < 0.01$). If this bias is held by blacks as well as whites, then this mechanism could also explain the second part of my findings: incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the citizen purported to shoot in self-defense ($p < 0.01$).

Public perception of law enforcement and prosecutorial behaviors may also drive the results of this analysis. Racial disparities in prosecution and sentencing (Cole, 1999; Mustard, 2001) may influence an individual's belief about how a law will be enforced, causing racial minorities to fear unfair prosecution far more than their white counterparts prior to the passage of *SYG*. If this is the case, the affirmative defense provided by a newly enacted *SYG* policy could sharply change racial minorities' paradigms while affecting white individuals far less, explaining why I find changes in blacks killing black APCs to be far larger in magnitude than whites killing black APCs ($p < 0.01$). Similarly, if both white and black individuals widely suspect the criminal justice system of bias against racial minorities, then both races may perceive the expected cost of killing black APCs to be lower than the expected cost of killing

[25] The Race-Weapons Task, available at implicit.harvard.edu/implicit/Launch?study=/user/demo.us/demo.weapons.0002/weaponsdemo.expt.xml (accessed 13 May 2017) has been completed by 530,817 website visitors between the years 2004–2015 and found that 73% had either a slight, moderate or strong implicit attitude that associates black persons with dangerous weapons.

white APCs, explaining why white APCs are being killed at a lower rate than black APCs ($p < 0.01$) and aligning with the aforementioned media claims that *SYG* "makes it easier to kill blacks."

Availability of law enforcement services may also drive the results of this analysis, as the availability of these services have been shown to vary based on neighborhood characteristics and caller demographics. This suggests that black 911-callers and black neighborhoods receive slower police responses than their white counterparts (Lee, Lee, and Hoover, 2016). Anecdotal evidence also suggests that differences exist in how blacks perceive the availability of law enforcement personnel.[26] These differences in law enforcement availability, real or perceived, may incite blacks to assume more responsibility for their own protection. If this is the case, then a change in policy that decreases the expected cost of killing an APC could create larger incentives for blacks to use lethal force, relative to whites. This mechanism only partially explains my findings; it explains the increased use of lethal force by blacks, but fails to fully explain why black APCs are also being killed at a higher rate by whites.

6.2 Summary and Concluding Remarks

The widespread implementation of *SYG* has created a natural experiment, which I exploit to measure racial disparities attributable to the policies. My approach is novel because I use justified fatal shooting records to identify the race of both the shooter and the deceased APC, which allows me to examine the interactions between race groups. By examining these racial disparities, my research directly addresses the frequent media claims that *SYG* laws "make it easier to kill blacks" and the

[26] See news stories such as "In New Orleans, call 911 and wait for an hour" (accessed 1 June 2017 from economist.com/blogs/democracyinamerica/2015/12/police-response-times), "Is 911 'still a joke' for African-Americans?" (accessed 1 June 2017 from thegrio.com/2014/04/23/is-911-still-a-joke-for-african-americans) or "Newly-released data shows City continues to deny equitable police services to South and West Side neighborhoods" (accessed 1 June 2017 from aclu-il.org/newly-released-data-shows-city-continues-to-deny-equitable-police-services-to-south-and-west-side-neighborhoods).

implications that the black community disproportionately pays the costs of these policies. Although this analysis cannot fully calculate *SYG*'s net welfare benefits, I am able to intimate the severity and the disproportionate burden of the policy's cost to human life.

I use a generalized difference-in-differences analysis to measure the effect of *SYG* policies on the cross-race and own-race killings of APCs. I present event studies to show that these effects are not caused by pre-existing trends, and I conduct a number of placebo and sensitivity tests to rule out spurious correlations in reporting behaviors or mortality trends. I also replicate and corroborate results published by CH and MT, which, in the words of MT, is "an important step towards building a consensus on the debate."

I find that *SYG* policies significantly increase the number of black APCs killed each month, and that the incremental number of black APCs killed is statistically larger than the incremental number of white APCs killed regardless of the race of the individual purported to shoot in self-defense. Fatal shootings of black APCs increase by 6–14 percent ($p < 0.05 - p < 0.01$) while fatal shootings of white APCs increase by only 0–3 percent (not statistically significant). This translates to an average of 1.611 additional black APCs killed each month nationally ($p < 0.05 - p < 0.01$), 70.8 percent of whom are killed by black individuals, and an average of 0.345 additional white APCs killed each month nationally, 97.7 percent of whom are killed by white individuals.

In general, my findings support the claims of those critical of *SYG* by indicating that the policies unequally affect the black community. However, since the majority of all APCs are killed during own-race interactions, my findings can neither support nor disprove critics' claims of institutional racism. Regardless, these significant racial differences provide strong evidence that *SYG* has imposed unequal costs, measured in terms of lives lost each month, across racial groups; these costs are 4.6 times larger for the black community than for the white community.

Table 1: Changes by State: Sources of Change

State	Source	Date
Alabama	2006 AL. SB 283	06/2006
Alaska	2005 AK. SB 200	09/2006
Arizona	2006 ARIZ. SB 1145	04/2006
Florida†	2005 FLA. SB 436	10/2005
Georgia	2005 GA. SB 396	07/2006
Indiana	2006 IND. HEA 1028	07/2006
Kansas	2005 KAN. SB 366	07/2006
Kentucky	2006 KY. SB 38	07/2006
Louisiana	2006 LA. HB 89	08/2006
Michigan	2005 MI. HB 5143	10/2006
Mississippi	2006 MISS. S.B. 2426	07/2006
Missouri	2007 MO. SBs 62 and 41	08/2007
Montana	2009 MT. HB 228	04/2009
North Carolina	2011 N.C. HB 650	05/2011
North Dakota	2007 N.D. HB 1319	02/2007
New Hampshire	2011 N.H. SB 88	11/2011
Nevada	2011 NEV. AB 321	05/2011
Ohio	2008 OH. SB 184	09/2008
Oklahoma	2005 OK. HB 2615	11/2006
Oregon††	State of Oregon v. Sandoval	03/2007
Pennsylvania	2011 PA. HB 40	06/2011
South Carolina	2005 S.C. HB 4301	06/2006
South Dakota	2006 S.D. HB 1134	07/2006
Tennessee	2007 TENN. HB 1907	05/2007
Texas	2007 TX. SB 378	09/2007
Utah†††	Utah Code 76-2-(402-404)	03/1994
West Virginia	2008 W.V. SB 145	02/2008
Wisconsin	2011 WISCONSIN ACT 94	12/2011

Notes: A list of states that enacted a *Stand Your Ground* policy.

†Excluded from sample for reasons discussed in Section 3.

††Oregon's law did not change, but the 2007 Supreme Court case *State of Oregon v. Sandoval* ruled that the existing law effectively does not require a victim to retreat before using deadly force, thereby causing a change in prosecutorial behavior in the same manner as new legislation.

†††Because the law changed *prior* to observed sample period, Utah is included in the control group. This allows the results to be interpreted as the effects of a change in *SYG* policy.

Source: Original legislation and court documents as listed in this table.

Table 2: Summary Statistics: Average Reportedly Justified Homicides per 1,000,000 Citizens of APC Race, per Month

Mean
(Standard Deviation)

		All States, Full Sample	Non-*SYG* States Full Sample	*SYG* States Full Sample	*SYG* States Before *SYG*	*SYG* States After *SYG*
Blacks killing...	...Black APCs	0.411	0.184	0.571	0.436	0.730
		(1.13)	(0.90)	(1.24)	(1.06)	(1.41)
	...White APCs	0.008	0.003	0.013	0.011	0.016
		(0.08)	(0.05)	(0.10)	(0.08)	(0.11)
Whites killing...	...Black APCs	0.162	0.111	0.196	0.160	0.241
		(0.73)	(0.66)	(0.77)	(0.61)	(0.92)
	...White APCs	0.084	0.050	0.121	0.099	0.147
		(0.29)	(0.23)	(0.34)	(0.29)	(0.38)

Notes: Summary of reportedly justified homicide rates committed with a firearm. Monthly statistics are calculated per 1,000,000 citizens of the APCs' race in a law enforcement agency's jurisdiction.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table 3: Replication of Cheng and Hoekstra

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SYG Effect	0.0801**	0.0841**	0.0612*	0.0652*	0.0466	0.0479	0.1310***	0.1312***
	(0.0342)	(0.0347)	(0.0352)	(0.0344)	(0.0329)	(0.0333)	(0.0336)	(0.0487)
Cheng and Hoekstra's original result	✓							
Log Transformation	✓							
Replication of C&H's original result		✓						
IHS Transformation		✓	✓	✓	✓	✓	✓	✓
Use 2000-2014 data			✓	✓	✓	✓	✓	✓
OR. in treatment				✓	✓	✓	✓	✓
Drop FL.					✓	✓	✓	✓
Rate: Per 1 Million						✓	✓	✓
Use monthly data							✓	✓
Outcome: Firearm Homicide								✓
Fixed Effects:								
Year	✓	✓	✓	✓	✓	✓		
Year×Month							✓	✓
State	✓	✓	✓	✓	✓	✓	✓	✓
Observations	550	550	846	846	829	829	8,149	8,149

Notes: A replication attempt of Cheng and Hoekstra's (2013) primary result using Supplementary Homicide Report raw data files. Column 1 lists Cheng and Hoekstra's original result and column 2 reports the result of my replication – I successfully replicate their result. Results should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: (1) United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014. (2) Florida Department of Law Enforcement, Crime in Florida Report Abstract, 2000-2014

Table 4: Primary Results

Table 4a: Effect of Legislation on Citizens Justifiably Shooting APCs

	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs	Whites killing White APCs
SYG Effect†	0.1434***	0.0556**	0.0052	0.0226
	(0.0473)	(0.0239)	(0.0041)	(0.0189)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	8,149	8,149	8,149	8,149
†*Additional lives lost each month due to policy change*	*1.410*	*0.201*	*0.009*	*0.337*

Table 4b: Differences in Point Estimates, by Race

$\chi^2(1)$ *Test Statistics:*

Race-pairs	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs
Whites killing Black APCs	12.76***	-	-
Blacks killing White APCs	54.99***	32.95***	-
Whites killing White APCs	36.95***	17.38***	4.55**

Notes: Results from difference-in-differences analysis of Justified Firearm-Related Homicides using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Each combination of equations is jointly estimated and the coefficients are tested for equality. A statistically significant result indicates the null hypothesis of equality is rejected, and the increased use of lethal force measured by the equations is statistically different. The critical value for the $\chi^2(1)$ test statistic 6.63 for significance at the 1% level, and 3.84 for significance at the 5% level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table 5: Effect of Legislation on Reporting Behaviors

	Reported Homicides	Unfounded Homicides	Actual Homicides	Clearance Rate
SYG Effect	0.0234	0.0029	0.0209	-0.0000
	(0.0205)	(0.0022)	(0.0209)	(0.0000)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	9,996	9,996	9,996	9,996

Notes: Results from difference-in-differences analysis of Offenses Known and Clearances by Arrest records using population weights. It can be seen that no specification is significant at any level, suggesting that the mechanism causing the change in behavior is correct. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the reporting or classification of homicides caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest, 2000-2014.

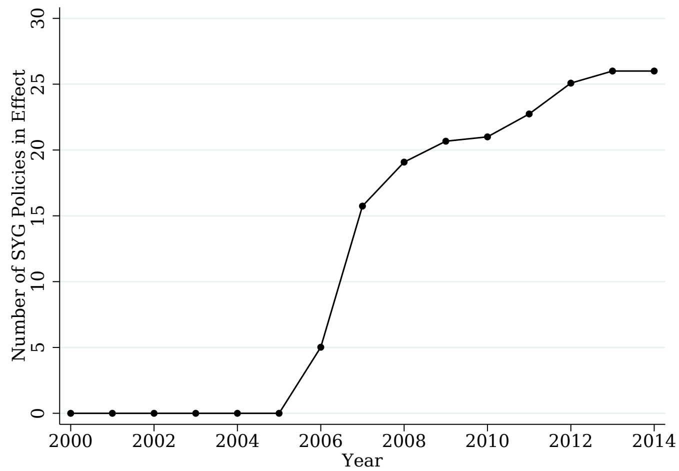
Table 6: Placebo Tests

	Placebo Homicide		Traffic Fatality		Unemployment	
	Black	White	Black	White	Black	White
SYG Effect	-0.0192	-0.0479	-0.1186	-0.2704	-0.0679	-0.0364
	(0.0798)	(0.0975)	(0.2024)	(0.2243)	(0.1185)	(0.0555)
Fixed Effects:						
Year×Month	✓	✓	✓	✓	✓	✓
State	✓	✓	✓	✓	✓	✓
Observations	1,914	1,914	8,817	8,817	8,776	8,820

Notes: Results from difference-in-differences analysis of placebo outcomes. It can be seen that no specification is significant at any level, suggesting that the mechanism causing the change in behavior is correct. Tests use population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: (A) US DOJ, FBI SHR, 2000-2014 (B) US DoT, NHTSA, Traffic Fatality Data, 2000-2014 (C) Katrina Stierholz, Federal Reserve Bank of St. Louis, State Level Unemployment Rate, 2000-2014.

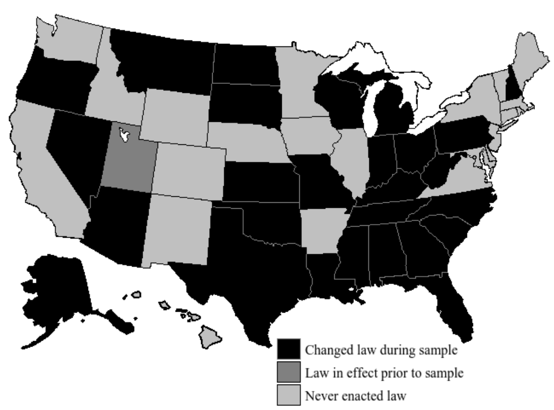
Figure 1: Legislation Changes Over Time



Note: Graphical depiction of the number of states changing *Stand Your Ground* policies over the observed time period, based on legislation changes and court rulings. Observed period: 2000-2014.

Source: Original legislation and court documents listed in Table 1.

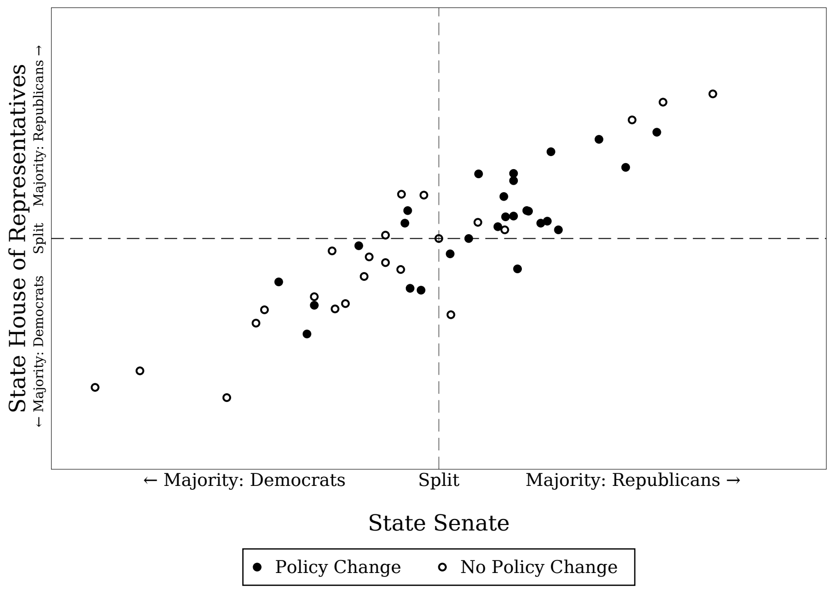
Figure 2: Legislation Changes by State



Note: Graphical depiction of states that enacted *Stand Your Ground* policies over the observed time period. States without *SYG* policy changes and states enacting policies prior to the observed time period are selected into the control group. States enacting new *SYG* policies during observed period are selected into treatment group. Observed period: 2000-2014.

Source: Original legislation and court documents listed in Table 1.

Figure 3: Political Composition of Legislative Bodies
at Time of Policy Change



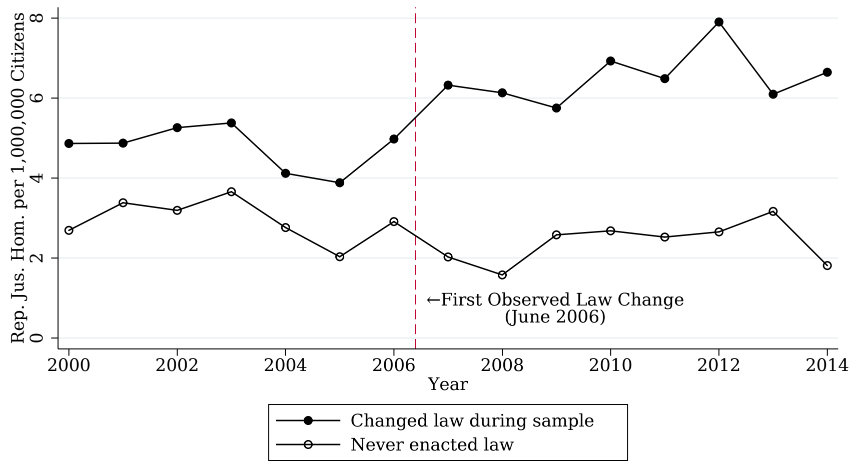
Note: Political composition of the State House and the State Senate for each state. Composition reported at time of policy change for states that enact *Stand Your Ground* policies. Composition reported at midpoint of treatment period (year 2010) for states that did not enact *Stand Your Ground* policies. Axes extend from the center lines (Split), which indicate an even split of political affiliation, out to 100% political composition by either party.

10 of the 27 states that enacted *SYG* policies during the observed treatment period had either one or both chambers of their state legislature controlled by Democrats. In five other states, no policies were enacted during the observed time period despite both chambers of state legislature being controlled by Republicans. This suggests bipartisan support and bipartisan opposition exists for these laws.

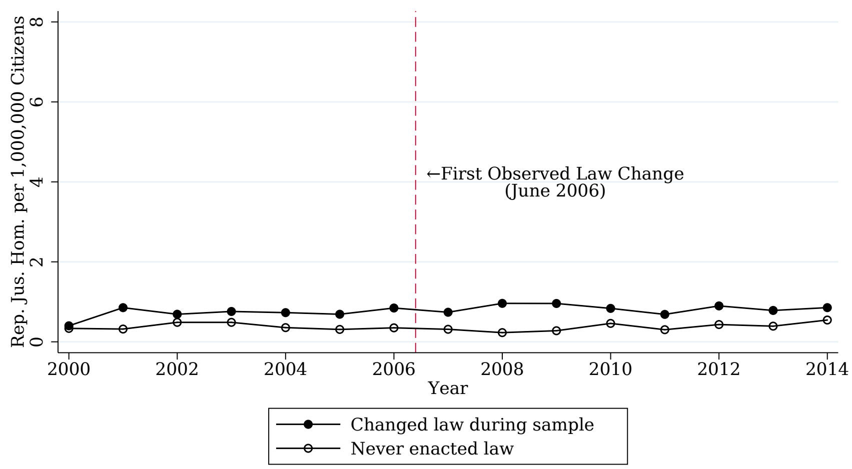
Source: POLIDATA Demographic & Political Guides, Party Control Tables 2004-2012; legislation and court documents listed in Table 1.

Figure 4: Homicide Trends: Reportedly Justified Shootings of APCs

Panel A: Reportedly Justified Shootings of Black APCs



Panel B: Reportedly Justified Shootings of White APCs

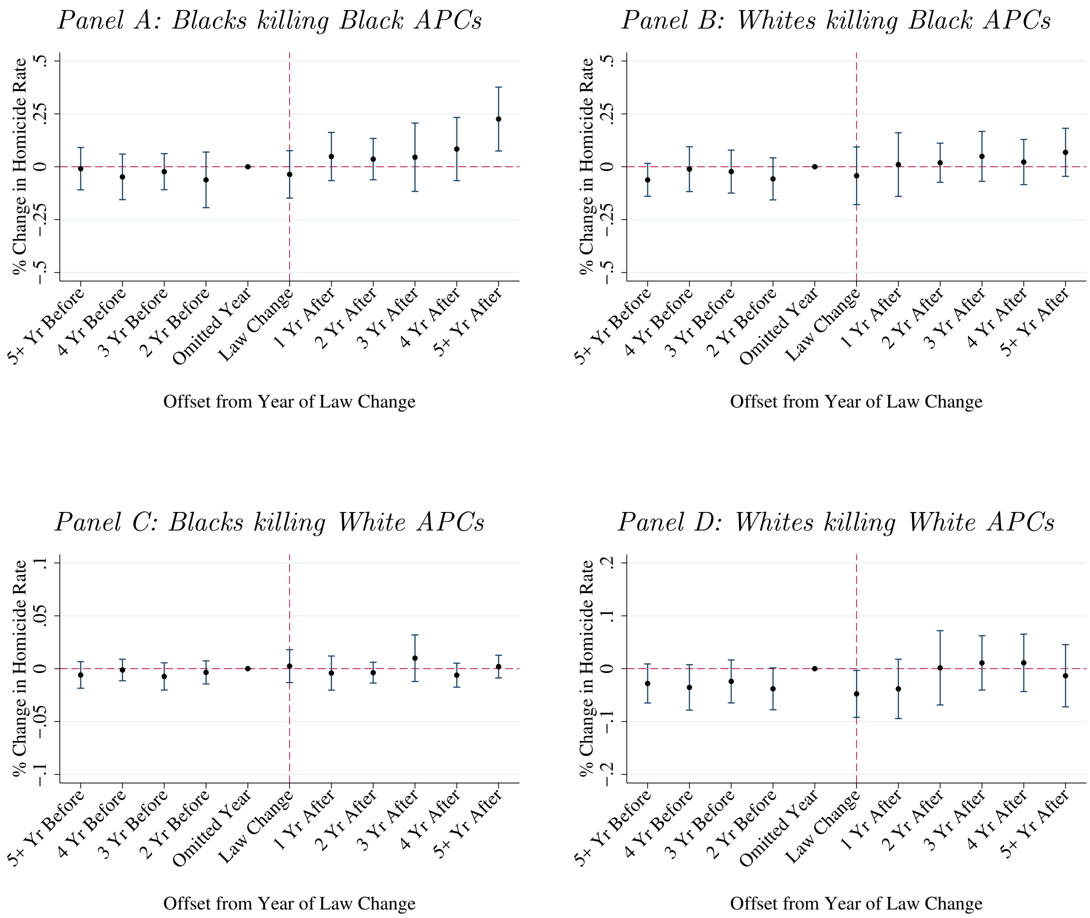


Note: Homicide trends by type and category of homicide. All results are measured per 1,000,000 citizens in the reporting jurisdiction and calculated using race-specific population weights.

The impact of time is consistent for all citizens and the impact of residing in a state that eventually did, or did not, enact a *SYG* policy is consistent across time. Given these findings, I consider states that did not change their policies to be good counterfactuals for the states that did.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Figure 5: Change in Homicide Rate Over Time

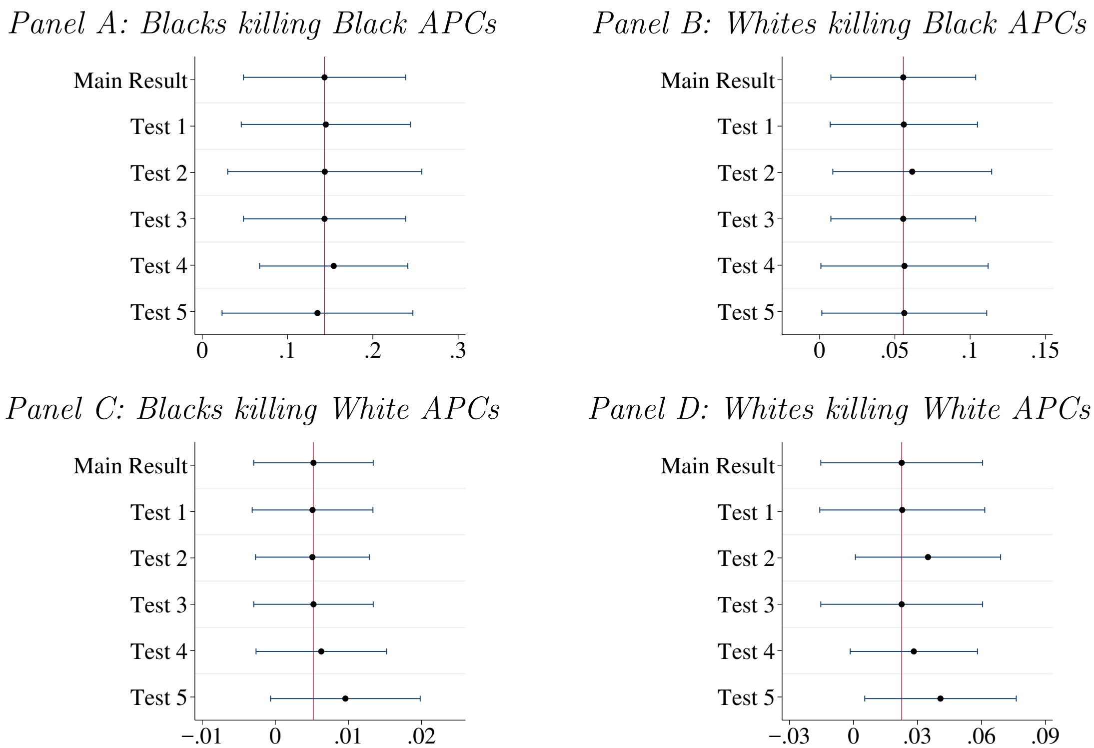


Note: Results from event study analysis of Justified Firearm-Related Homicides by Black Citizens (Panel A) and White Citizens (Panel B) using State and Year fixed effects. Coefficients of annual indicator variables and their 95% confidence intervals illustrating the percent change in homicides for states enacting *Stand Your Ground* policies during observed time period. Confidence intervals utilize robust standard errors clustered at the state level. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by exposure to treatment over time. Effects are normalized to zero in the year prior to treatment.

Insignificant results for periods -5 through 0 is evidence that non-SYG states are good counterfactuals for the states that enact the policy. All results from all periods prior to SYG enactment are statistically insignificant, supporting my decision to use these observations as counterfactuals in my analysis.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014

Figure 6: Sensitivity Tests



	Original Estimate	Test 1	Test 2	Test 3	Test 4	Test 5
Fixed Effects:						
Year×Month	✓	✓	✓	✓	✓	✓
State	✓	✓		✓	✓	✓
State×Month		✓				
Region			✓			
Linear Time Trends				✓		
Restrict Years:					2000–2012	2002–2010

Notes: Sensitivity analysis for difference-in-differences results. Models use population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014

Supplementary Graphs and Results:

Table A.1: Effect of Legislation on Justified Shootings, by Jurisdiction

Table A.1a: Urban Jurisdictions

	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs	Whites killing White APCs
SYG Effect	0.1530***	0.0583**	0.0056	0.0233
	(0.0486)	(0.0248)	(0.0043)	(0.0170)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	7,735	7,735	7,735	7,735

Table A.1b: Rural Jurisdictions

	Blacks killing Black APCs	Whites killing Black APCs	Blacks killing White APCs	Whites killing White APCs
SYG Effect	0.1347	0.0049	0.0037	0.0129
	(0.0835)	(0.0227)	(0.0045)	(0.0274)
Fixed Effects:				
Year×Month	✓	✓	✓	✓
State	✓	✓	✓	✓
Observations	5,685	5,685	5,685	5,685

Notes: Results from difference-in-differences analysis of Urban (Panel A) and Rural (Panel B) Justified Firearm-Related Homicides using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

I categorize urban and rural jurisdictions using data from the Center for Disease Control's National Center for Health Statistics (NCHS) and the Urban-Rural Classification Scheme for Counties from 1990, 2006 and 2013. I define "rural" to include *population clusters of less than 50,000* and all smaller counties. I define "urban" to include the categories of *small metro populations between 50,000 and 250,000* up through *large metropolitan centers of 1 million or more.*

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table A.2: Summary Statistics by Population Density:
Average Reportedly Justified Homicides per 1,000,000 Citizens of APC Race, per Month

	Combined		Urban		Rural	
Blacks killing...	...Black APCs	0.41	...Black APCs	0.39	...Black APCs	0.60
	...White APCs	0.01	...White APCs	0.01	...White APCs	0.01
Whites killing...	...Black APCs	0.16	...Black APCs	0.16	...Black APCs	0.23
	...White APCs	0.08	...White APCs	0.08	...White APCs	0.30

Notes: Summary of reportedly justified homicide rates committed with a firearm. Monthly statistics are calculated per 1,000,000 citizens of the APCs' race in a law enforcement agency's jurisdiction.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table A.3: Effect of Legislation on Justified Shootings, by Shooter

Table A.3a: Committed by Law Enforcement

	Combined	Urban	Rural
SYG Effect	0.0563*	0.0563*	0.0250
	(0.0324)	(0.0322)	(0.0291)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Table A.3b: Committed by Citizens

	Combined	Urban	Rural
SYG Effect	0.0786***	0.0833***	0.0501*
	(0.0248)	(0.0242)	(0.0288)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Notes: Results from difference-in-differences analysis of Law Enforcement and Citizen IHS[Justified Firearm-Related Homicides] using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Table A.4: Effect of Legislation on Justified Shootings, by Race of Deceased

Table A.4a: Black APCs

	Combined	Urban	Rural
SYG Effect	0.1809***	0.1908***	0.1643
	(0.0580)	(0.0602)	(0.1037)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

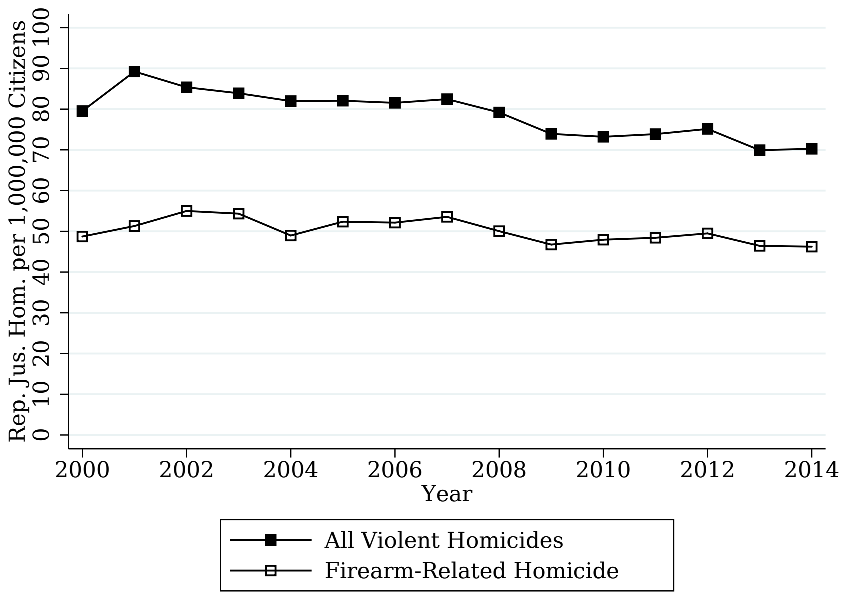
Table A.4b: White APCs

	Combined	Urban	Rural
SYG Effect	0.0298	0.0307	0.0188
	(0.0201)	(0.0184)	(0.0272)
Fixed Effects:			
Year×Month	✓	✓	✓
State	✓	✓	✓
Observations	8,149	7,735	5,685

Notes: Results from difference-in-differences analysis of Law Enforcement and Citizen IHS[Justified Firearm-Related Homicides] using population weights and fixed effects. Results are measured per 1,000,000 citizens and should be interpreted as the percent change in the homicide rate caused by treatment. Robust standard errors are reported in parenthesis and are clustered at state-level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

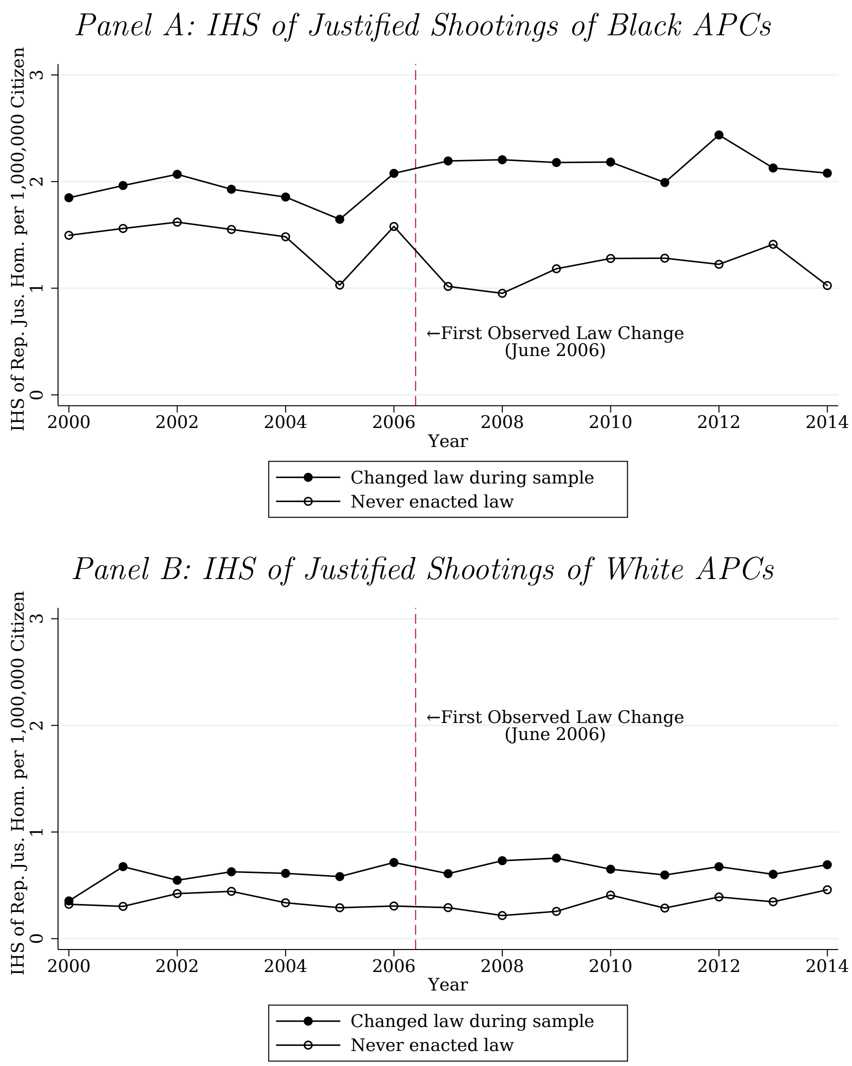
Figure A.1: Homicide Trends: All Homicides and Shootings



Note: Homicide trends by type and category of homicide. All results are measured per 1,000,000 citizens in the reporting jurisdiction.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

Figure A.2: Inverse Hyperbolic Sine Homicide Trends: Justified Shootings of APCs



Note: Homicide trends by type and category of homicide. All results are measured per 1,000,000 citizens in the reporting jurisdiction and calculated using race-specific population weights.

Source: United States Department of Justice, Federal Bureau of Investigation, Uniform Crime Reporting Program Data: Supplementary Homicide Reports, 2000-2014.

EFFECT OF WATERBORNE URANIUM EXPOSURE ON HUMAN CAPITAL ENDOWMENT PROXIES.

By Michael Spanbauer

This paper examines the causal relationship between waterborne uranium exposure and birth outcomes in order to more fully understand the external costs of the activities that increase the probability of human exposure to uranium, such as the prevalent military use of depleted uranium munitions. I use the Church Rock Uranium Mill industrial accident as a natural experiment, in which children born in specific counties are exposed to uranium via a contaminated water supply. I examine changes in birth outcomes, which approximate human capital endowment at birth, and I find that waterborne uranium contamination does not manifest via observable decreases in birth outcomes, specifically birth weight, or via changes in gender ratios. I also provide evidence suggesting that migratory responses to the contamination are not driving a change in the population's determinants of birth outcomes. Collectively, these results fail to disprove modern militaries' claims that the risk of unintentional harm by uranium based weapons are "negligible."

Keywords: uranium; birth outcomes; human capital endowment; Church Rock.

JEL: D62; I18; I39; J24.

1 Introduction

Uranium is a naturally occurring radioactive metal used in the construction of depleted[1] uranium (DU) munitions. DU munitions are extolled by modern militaries

[1]Depleted uranium is the byproduct of the uranium enrichment process, necessary for the construction of nuclear reactors and nuclear weapons. Enriched uranium contains 0.72 percent of the

for their ability to penetrate heavy armor plating, but the projectiles also contaminate the surrounding area with uranium dust that is washed by rainfall into a population's water supply, such as rivers and wells, causing a significant health risk (Boice, *et al*, 2010; Brown, 2003; Canu, *et al*, 2012; EPA, 2012b; Ritz, 1999). One area of the world that has endured years of exposure to DU munitions is the Persian Gulf; an estimated 340 tonnes[2] of DU were fired during the First Gulf War,[3] and an additional 2,000 tonnes[4] were deployed in the first three weeks of the Second Gulf War,[5] from 19 March 2003 to 30 April 2003 (Brown, 2003). These conflicts, fought in close proximity to civilian environments, dispersed thirty-thousand times more uranium than was contained in the bomb that ended World War II.

Although modern militaries claim that the health effects of DU munitions are negligible (Miller, *et al*, 2008), this paper tests the hypothesis that contaminating a population's water supply with uranium imposes a cost to the exposed population in the form of decreased health stock and human capital endowment at birth, which may lead to reduced economic stability later in life. The foundation of my hypothesis is supported by circumstantial evidence linking uranium to debilitating physiological effects (Boice, *et al*, 2010; Ritz, 1999; Miller, *et al*, 2008). For example, the population of Fallujah, Iraq, developed 4.22 times more cancer cases in than populations not exposed to DU munitions in the period following the United States' 2004 seige (Busby, Hamdan, and Ariabi, 2010). Additionally, nine deaths in the Italian military and

---

fissile isotope U-235, all remaining uranium is considered "depleted." Depleted uranium is not less harmful, only less suitable for use in nuclear fission.

[2]374.786 tons.

[3]Two major operations occurred, code-named Desert Shield, 2 August 1990 - 17 January 1991, and Desert Storm, 17 January 1991 - 28 February 1991. The latter mission lasted exactly 42 days before the capitulation of the Iraqi forces. The Iraqis surrender has been primarily attributed to the United States warplanes, loaded with PGU-14/B depleted uranium bullets, dispatching the opposition's armored tank divisions.

[4]2,204.623 tons.

[5]One example of uranium dispersion is the coalition attack on the Headquarters of the Planning and Information Ministry in Baghdad, Iraq. On 9 April 2003, the building was shot 49 times with PGU-14/B depleted uranium bullets (Zecevic, Terzic, Catovic and Serdarevic-Kadic, 2010). Each bullet contained 0.63 lb of depleted uranium, totaling 30.87 lb of uranium. This is equivilant to approximately $1/4^{th}$ of the uranium used in the Little Boy nuclear bomb.

wide-spread illnesses in the militaries of France, Netherlands, Spain, Belgium, and Portugal are correlated with handling depleted uranium ammunition (Gupta, 2001).

It would be difficult to directly test this hypothesis using data from war-zones contaminated by uranium-based munitions because wars destroy infrastructure, increase demand for health services, and induce stress; changes that may be negatively correlated with birth outcomes and that may adversely influence future labor market outcomes. I therefore isolate the effect of waterborne uranium exposure on my dependent variables by identifying an incident where a large volume of uranium is quickly introduced to the water supply by an exogenous shock and where the exposure to the contamination is randomly assigned to a clearly-identifiable population. I then construct a panel dataset by combining samples from the National Center for Health Statistics' natality files with timing and geographic data collected in a survey of archived periodicals. I employ a difference-in-differences model to compare the exposed individuals' birth outcomes with a counter-factual group drawn from uncontaminated areas. The key identifying assumption is that, had the contamination not been introduced into the water supply, the average annual change in these outcomes would have been similar to the observed changes of unexposed individuals (Mora and Iliana, 2012).

This analysis builds on a body of research which has established a link between health at birth and human capital endowment. The Fetal Origins Hypothesis (FOH) postulates that adult health is affected by *in utero* conditions[6] (Barker, 1995), and research indicates that children with low birth weight, a metric commonly used to asses health at birth, tend to have reduced IQ, test scores, cognitive development, high school completion rates, and adult earnings; more specifically, a birth weight decrease of 481 grams can lead to a 6 percent decrease of lifetime earnings (Almond,

[6]Lucas, *et al*, (1999) emphasized the importance of proper statistical analysis when interpreting effects of the FOH, Almond and Currie (2011) how *in utero* conditions can cause abnormalities to manifest in adulthood, and additional discussion of the FOH can be found in Therapontos, *et al*, (2009), Kaestner and Lee (2005) and Wehby, *et al*, (2009).

Chay and Lee, 2005; Black, Devereux and Salvanes, 2007; Behrman and Rosenzweig, 2004). Furthermore, Almond and Currie (2011) observe latent effects that lead healthy children to manifest significant abnormalities in adulthood as a result of *in utero* exposure to a harmful substance. These links between health at birth, human capital endowment, and adult socioeconomic outcomes suggest that early childhood or *in utero* exposure to a toxic substance, such as uranium, can plausibly decrease a population's human capital endowment and create lingering adverse economic effects.

The first finding of this paper indicates that exposure to the contamination is not negatively correlated with determinants of birth outcomes, such as parental education and age. This suggests that the contaminated geographic locations did not experience a significant demographic change that adversely affects the population's birth outcomes and therefore supports the validity of my subsequent analysis. The second finding of this paper is that uranium contamination in a population's water supply appears to have a negligible effect on the exposed population's birth outcomes; my analysis indicates that maternal ingestion of contaminated water does not manifest as a significant adverse change in the available birth metrics, specifically birth weight and gender ratios, and it also indicates that there is no observable culling effect influencing the estimates. The implication of these results is that *in utero* exposure to waterborne uranium contamination does not cause an observable decrease in proxies for human capital endowment at birth, and therefore activities which increase the likelihood of human exposure to uranium contamination may not have significant external effects on the exposed population's birth outcomes and human capital endowment. The conclusions of this analysis do not disprove the military's claims that the unintentional harm caused by DU munitions are negligible.

2 Natural Experiment

2.1 Regarding the Church Rock Uranium Mill Spill

On 16 July 1979 at 5:20 am, a 20-foot section of the Church Rock Uranium Mill's holding pond collapsed. 1,100 tons of solid uranium mill waste and 93,000 gallons of a liquid uranium solution were released into the Rio Puerco, traveled down the Little Colorado river, washed into the Colorado river, and spread through Lake Mead. The course of the rivers brought the contaminants through low-income Native American reservations in Arizona. Residents of these reservations relied on the rivers and lakes for drinking water as well as to irrigate crops, water livestock, catch fish, bathe, and play (Brugge, deLemors, and Bui, 2007), but these water sources quickly became unfit consumption. Precise chronological data on radiation levels downstream are not available, but measurements of water drawn from Lake Mead in 1992 exceeded the federal standards for safe drinking water by 40 percent despite being purified in water-treatment facilities (LVS, 1998).

United Nuclear Corporation, owner of the Church Rock Uranium Mill, later admitted that less than 1 percent of the contamination was removed from the water supply (Brugge, deLemors, and Bui, 2007), implying that residents were subject to uranium exposure up to 7,000 times larger than the "allowable standard" (Johansen, 1997). Despite an official report by the EPA (1983), which states that the Church Rock Uranium Mill spill had almost no adverse effects, historical evidence suggests that the effect of the spill was downplayed by the government. The Navajo Tribal Council's Emergency Services Coordinating Committee asked the Arizona Governor to declare a state of emergency and to assist the tribe with caring for the rapidly increasing number of sick individuals near the contaminated area, but this request, and other similar requests for emergency relief, were denied (Brugge, deLemors, and Bui, 2007). The Navajo Nation turned to the federal government for assistance and on 3 April

2014, 35 years after the event, the federal government awarded the Navajo Nation $1 billion dollars to "address uranium contamination" by attempting to decontaminate the abandoned uranium mines left behind by United Nuclear Corporation and other mining firms (W.I.S.E., 2015).

Shortly after the spill, the executive vice president and CEO of United Nuclear Corporation petitioned Congress for permission to resume mining activities. Permission was granted and mining resumed less than 4 months after the holding pond wall collapsed.

2.2 Event Selection

My research has uncovered four instances of wide-spread human exposure to unnatural quantities of uranium, of which only the Church Rock event can be used due to the clearly identifiable geographic areas of contamination and the availability of a rich set of publicly available records. Two of the other three instances, namely the use of DU munitions in combat zones and the August 1945 bombing of Hiroshima,[7] can not be utilized because large and protracted military conflicts prevent detailed record-keeping, cause physical damage to people and infrastructure, and also induce unobservable stressors which may be correlated with decreased human capital and labor market outcomes. The final instance occurred on 4 January 1986, when a storage tank in Oklahoma ruptured and released 29,500 tons of gaseous uranium hexaflouride. This event can not be used because the gas disseminated in unknown directions and identification of exposed individuals likely impossible.

It is prudent to acknowledge that other nuclear events and accidents have occurred, such as the testing of nuclear weapons or various reactor malfunctions, but these events can not be used as a natural experiment for this research. Historic usage and testing of nuclear weapons are unsuitable for this analysis because, aside from the

[7] 64.15 kilograms of uranium were used nuclear weapon, codenamed "Little Boy" (Sublette, 2007)

single aforementioned uranium bomb, the weapons employ a combination of plutonium and high-explosives to reach critical mass.[8] The relative stability of plutonium based weapons made them the preferred design for testing, development and military use; only five uranium bombs were ever constructed (Federation of American Scientists, 1998), four of which were later disassembled due to stability concerns (Nuclear Weapon Archive, 2006). Research shows "plutonium that is ingested from contaminated food or water does not pose a serious threat to humans because the stomach does not absorb plutonium easily, it passes out of the body in the feces" (EPA, 2012b). Additionally, this analysis can not consider a nuclear reactor accident such as Three-mile Island, Chernobyl, Fukushima Daiichi, or Hanford. These events expose people to iodine-130, a radioactive isotope that can cause severe physiological effects in large doses, but is considered "therapeutic" in appropriate doses and, with a half-life 8.2 days, rapidly decays (EPA, 2012a).[9]

Unlike the elements released in the nuclear weapon tests or reactor accidents, uranium is linked with an increased risk of circulatory system diseases (Canu, *et al*, 2012), an increased risk for all types of cancer, and severe liver damage (EPA, 2012c). Due to the different physiological effects of each of these elements, a study examining a plutonium based weapon test or the iodine-130 released during a nuclear reactor accident could not be extrapolated to discuss the effects of civilian exposure to uranium or DU munitions.

[8]Critical mass: The smallest amount of fissile material needed for a sustained nuclear chain reaction (Goertzel, 1954)

[9]Uranium exists in three common isotopes with three different half-lives. Uranium-234: 244,000 years. Uranium-235: 700 million years. Uranium-238: 4.47 billion years. (EPA, 2012c)

3 Methodology

3.1 Data Sources and Identification

I use county-level natality data from the National Center for Health Statistics (NCHS), collected from 1970 through 1988. The relevant data from years 1970 through 1985 are 50 percent samples of the population, so observations from these years are appropriately weighted. Publicly available data collected after 1988 is unsuitable for analysis because county-level identification was not available observations from counties with populations less than 100,000.

Identification of exposed observations is accomplished by evaluating the flow of the Rio Puerco, Little Colorado, and Colorado rivers; births occurring in counties with contaminated water flowing through are considered to have received exposure. Figure 1 illustrates the course of the Arizona river network contaminated by the spill. The Rio Puerco flows from east to west, entering the state of Arizona through Apache county. It terminates into the Little Colorado River near the Navajo-Apache county boarder, which flows northwest through Coconino county. The Little Colorado joins the Colorado River approximately in the middle of Coconino county, snakes across the eastern boarder of Mohave county, and drains into Lake Mead, which supplies the drinking water to Mohave County, Arizona. Once the water passes through the Hoover Dam,[10] it travels south through Yuma and La Paz counties[11] (United States Geological Survey, 2014). The control group is comprised of all remaining counties in Arizona.

As this event primarily impacted the Native American residents in the exposed counties, I restrict my natality data to include only the Native American population in both the treatment and the control counties by selecting birth records where either

[10]No other dams exist between the spill site and the Hoover Dam.

[11]In 1979, La Paz county did not exist. The land that is now La Paz County was part of Yuma County at the time.

the mother or father identify their race as Native American. A map of all treatment and control counties from which Native American births are observed is presented as Figure 2.

3.2 Outcome Variables and Summary Statistics

Use of the NCHS data permits the analysis of two recognized birth outcome metrics: birth weight and the ratio of extremely low birth weight incidents within a cohort, where extremely low birth weight is defined as a birth occurring with a mass of less than 1500 grams. As previously discussed, birth weight is a metric commonly used to asses health and is linked to human capital endowment at birth. In the data, birth weight is measured in grams and observed at the individual level; I use the log transformation so as to interpret my results in terms of percent-change. Extremely low birth weight is measured as a percentage of the births occurring in a given county and in a given month. Data on other commonly used metrics, such as APGAR score and gestational length, are not available in the data used for this analysis

In addition to these two metrics of health and human capital endowment at birth, I also explore the ratio of male to female births. Research indicates that male births tend to experience higher numbers of spontaneous abortions when the mother endures stressful conditions (Catalano, Bruckner, & Smith, 2008; Catalano, Bruckner, Hartig, & Ong, 2005). Therefore, a reduction in male births relative to female births can indicate maternal stress following exposure. I use the NCHS data to construct this ratio of male to female births, which is measured as a percentage of the births occurring in a given county and in a given month.

I report the mean and standard deviation of the metrics employed in this analysis in Table 1; full population statistics are listed in column 1, statistics for unexposed individuals are listed in column 2, and individuals exposed to waterborne uranium contamination during the observed time period are listed in column 3. Columns 4 and

5 further examine the exposed individuals by listing the mean values of each metric before and after the exposure occurred.

3.3 Empirical Model

I employ a difference-in-differences model[12] to evaluate the effect waterborne uranium contamination on individuals, i, born in county c at time t, and I cluster my standard errors at the county level to account for possible serial correlation in birth trends (Bertrand, Duflo, and Mullainathan, 2004).

Observations are assigned a value of $E_{i,c} = 1$ if they are born in the counties that were contaminated by the Church Rock Uranium Mill spill, as identified in Section 3.1. I also identify periods of time, $P_{i,t}$, before and after the date of the spill, 16 July 1979. Observations born prior to 16 July 1979 are considered to be in the pre-event group, $P_{i,t} = 0$, and observations born after the event are included in the post-event group. However, average gestational length is 268 days (Jukic, *et al*, 2013), implying observations born immediately following the spill were clearly gestating prior to the event. Therefore post-event observations born within 268 days are assigned a fraction[13] $P_{i,t} = \frac{Spill\ Date - Birth\ Date}{268}$ and subsequent observations are assigned $P_{i,t} = 1$.

I calculate my estimates with and without a set of covariates, $\mathbf{X}$, controlling for characteristics that are causally related to the observed outcomes. These characteristics include gender, mother's age, parent's education, number of prenatal visits, and indications if the birth was a singleton, if it occurred in a hospital and if a doctor was present. Both Almond, Chay and Lee (2005) as well Black, Devereux and Salvanes (2007) recognize these variables as determinants of birth outcomes, and I include them in my model to improve the precision of my estimates.

[12] See Ashenfelter (1978), Ashenfelter and Card (1985), and Card and Krueger (1993) for examples and discussions.

[13] Examples of this strategy are found in Donohue and Ayres (2003), Lott and Mustard (2006), and other published works.

With this framework, birth weight can be modeled by

$$Y_{i,c,t} = \alpha + \beta E_{i,c} + \gamma P_{i,t} + \delta(E_{i,c} \times P_{i,t}) + \mathbf{X}'_{i,c,t}\mathbf{\Theta} + \lambda_c + \mu_t + \varepsilon_{i,c,t}$$

where the coefficients α, β, γ, δ, and ρ are all unknown parameters, λ_i is a vector of fixed effects controlling for variations caused by county residence, μ_t is a vector of fixed effects controlling for variations caused by month and year of birth, and $\varepsilon_{i,c,t}$ is an idiosyncratic error term assumed to be independent of all other terms in the model. The inclusion of λ and μ (the vectors of fixed effects accounting for location and month of birth) prevent bias from spurious correlations between the timing of the waterborne uranium contamination and other prominent events, while the smaller variations that effect individual observations are captured by the error term. I cluster my standard errors at the state level to account for the possibility of serial correlation in birth trends, as suggested by Bertrand, Duflo, and Mullainathan (2004), and, where appropriate, I use the log-transformation of the outcome variable to aid in interpretation of my results.

I also estimate my model while accounting for county-specific linear time trends. Including linear time trends captures the average change as each additional month passes. This controls for the general direction, or trend, of the outcome in each county. Allowing these time trends to be county-specific allows each county to follow a unique trend as time passes, rather than imposing the assumption that each county's trends are identical over time. It can be seen in the results section that this step is somewhat unnecessary, as results are similar with and without this additional regressor.

3.4 Assumptions and Limitations

The difference-in-differences model relies on a key identifying assumption for the estimations to be consistent and unbiased, commonly referred to as the "parallel paths" assumption. This assumption asserts that, in the absence of exposure, the exposed group's average changes would have been similar to the observed individuals who were not exposed to the waterborne contamination. In this analysis, the justifications for this assumption are similar to that of a randomized control trial in the sense that treatment assignment was exogenously determined by the timing of an unpredictable accident and the natural path of a river, suggesting that assignment into the treatment and control group is pseudo-random. This assumption permits the observations from unexposed locations to serve as counterfactuals for what would have happened had the Church Rock holding pond not collapsed (Mora and Iliana, 2012).

One potential violation of this model is a form of attrition or selection bias. It is possible that the residents of exposed counties who have the means to relocate could have fled after being adversely affected by the spill. These individuals, who were treated, could then either live in the control counties or live in an area not observed by this analysis. If these individuals move to control counties or unobserved counties while their prior homes remain vacant, then my estimates would be biased towards zero and my results can be interpreted as the lower bound of the treatment effect. However, if these individuals flee and create a sudden surplus of low-cost housing, their vacant homes may be filled by individuals with a higher propensity to give birth to children with lower birth outcomes and lower future adult outcomes.

If these unobserved migration patterns exist, and if they are negatively correlated with birth outcomes, then the effects measured by this analysis illustrate a significant change the demographic composition of the affected area rather than a physiological change to a static group of exposed persons. Although a significant decrease in human capital endowment caused by either mechanism is socially undesirable, I acknowledge that a limitation of this analysis is the inability to confidently distinguish between these two mechanisms. However, I provide evidence in Table 2 suggesting that there is not a negative correlation between birth determinants and unobserved migration trends; supporting my hypothesis that waterborne uranium contamination causes adverse physiological changes and negatively affects the outcomes of the exposed population.

4 Results

4.1 Contamination Response

I have thus far discussed the mechanism by which the uranium contamination has caused *in utero* and childhood exposure, the link between *in utero* exposure and birth outcomes, and the implications of birth outcomes for an individual's human capital endowment. I now examine correlations between the contamination event and the determinants of birth outcomes. The purpose of this check is to ascertain if my analysis is biased by unobserved migration trends that are negatively correlated with birth outcomes. Mother's age, parental education, and level of prenatal care are all considered to be determinants of health at birth (Almond, Chay & Lee, 2005) and examining these determinants as outcome variables can lend insight to the population's reaction to the event. Table 2, Panels A and B, report regression results using the following determinants as the binary outcome variables: mother being of recommended childbearing age, mother being a high school graduate, father being a high school graduate, mother receiving the recommended prenatal care during pregnancy.

A statistically significant decrease in these categories would suggest a migratory response within the population that is negatively correlated with birth outcomes, implying that the results of this analysis are at least partially driven by change in the population's demographics. Conversely, a statistically significant increase in these categories would suggest that an unobserved migratory response has the potential to improved birth outcomes and therefore my primary analysis would underestimate the true harm caused by exposure.

It can be seen in Table 2, Panel A, that point estimates for mother's age and father's education are negative in sign, but are small in magnitude and statistically insignificant. However, the proportion of mothers completing high school and the proportion of mothers receiving prenatal care significantly increases following the event. I introduce county specific time trends in Panel B and thereby relax the assumption that each county experiences identical trends in high school completion and medical care over time. All estimates for determinants of birth converge towards zero with the inclusion of this set of controls. Together, this suggest thats there is no significant demographic response to the event that would artificially inflate the magnitude of my results; these results therefore support the validity of the subsequent analysis.

4.2 Outcomes at Birth

I now turn to discuss the results of my analysis, which numerically evaluate the causal effects of uranium exposure on these outcomes.

Figure 5, Panel A, depicts the mean annual birth weight over time for the treatment and control groups. A visual inspection of this graph shows a mild decrease in average birth weight immediately following the event, but it is difficult to draw strong conclusions from this graph given the presence of other swings in mean weight observed in both the treatment and control group, some which are larger in magnitude. Panels B through D present other metrics for assessing birth outcomes; percent of cohort born with extremely low weight (mass < 1500 grams), percent of cohort born male, and the size of each cohort over time.[14] Each of these remaining plots is similar to Panel A in the sense that there is no clear visual proof of waterborne uranium contamination adversely influencing the post-treatment birth trends of the exposed population.

Tables 3 and 4 support this visual inference by reporting my estimates for the same four birth metrics presented in Figure 5. Results in Table 3 should be interpreted as

[14] Data on other commonly used metrics, such as APGAR score and gestational length, are not available in the data used for this analysis.

the percent-change in birth weight caused by exposure to the waterborne uranium contamination. Results are consistent in sign and small in magnitude, ranging from eight-hundredths of one percent up to approximately two tenths of one percent, and not statistically significant. In nominal terms, this translates to a change of approximately 2.3 – 6.7 grams from the mean birth weight of 3327.5 grams. In Table 4, Panel A presents the change in the probability of experiencing extremely low birth weight as a result of exposure, Panel B presents the change in the probability of experiencing a male birth, and Panel C presents the percent-change in the treatment group's cohort size following exposure to the contamination. In all cases, point estimates are small and close to zero. Furthermore, given the small standard errors for these estimates, it is reasonable to conclude that any effects of *in utero* exposure to waterborne uranium contamination do not manifest via the birth outcome metrics available for this analysis.

It should be noted that the results of this analysis do not indicate that waterborne uranium exposure is safe or free of harm. Instead, these results only fail to disprove the claims of modern militaries. The analogy between the Church Rock industrial accident and military use of uranium-based weaponry begins to break down when considering the severity of exposure. One major difference between these events is that the volume of uranium utilized in conflict zones far exceeds that which was distributed into the rivers of Arizona (Zecevic, Terzic, Catovic and Serdarevic-Kadic, 2010; Brown, 2003; LVS, 1998). A second major difference is the actual exposure to the contaminated water; the observed counties are large in area and many residents live a great distance away from the singular source of exposure, whereas the dispersion of uranium in conflict zones is widespread and may therefore be difficult to avoid.

5 Summary and Conclusion

In this article I examine the effects of exposure to waterborne uranium contamination. I isolate these effects by selecting an event which psudo-randomly assigns exposure to waterborne uranium and then I quantify these effects by analyzing changes in birth outcomes, which approximate human capital endowment at birth.

I present evidence indicating that this event is not negatively correlated with adverse changes in several determinants of birth outcomes, addressing the natural concern that an undesirable change could be driven by a structural change in the population following the contamination event. I then asses the changes in birth outcomes and find that prenatal exposure to uranium by means of maternal ingestion of contaminated water does not manifest as an adverse change in the available birth metrics, specifically birth weight and gender ratios. Despite the possibility of these estimates being a lower bound of a significant adverse effect of treatment, the evidence presented in this article suggests does not support my hypothesis that *in utero* uranium exposure measurably influences birth outcomes.

The novel contribution of this analysis is my method for isolating and exploring the effect of waterborne uranium contamination on human capital endowment proxies. Although the results of this test are inconclusive, the foundations of my analysis can be used in future research into the external costs of utilizing depleted uranium munitions; these munitions are known to leach uranium into the water supply and can therefore cause uranium exposure in a manner similar to the Church Rock accident. Other studies on major nuclear events cannot be extrapolated in this manner because the primary radionuclides released in those events produce distinctly different physiological effects (Campaign for Nuclear Disarmament, 2011; Comprehensive Nuclear-Test-Ban Treaty Preparatory Committee, 2012; EPA, 2012; EPA, 2015).

The contamination caused by the Church Rock Uranium Mill spill is a tragedy, as is the uranium contamination caused by the use of depleted uranium munitions in conflicts fought near civilian areas and agricultural lands. Although the results of this analysis do not contradict the claims that risks associated with exposure to depleted uranium munitions are "negligible," we do not yet fully understand the long-term effects of uranium-based weapons on the health and economic stability of the civilians living near the war zones. Many military actions are intended to create long term stability in the conflict zone by "winning the hearts and minds of the people" (Berman, *et al.*, 2011; US Army Field Manual 3-24), therefore more research is required to ensure we are not poisoning the populations we are attempting to stabilize.

Table 1: Summary Statistics: Mean (S.D.) Values of Observed Birth Metrics

	All Counties, Full Sample	Unexposed Counties, Full Sample	Exposed Counties, Full Sample	Exposed Counties, Before Exposure	Exposed Counties, After Exposure
Birth Weight	3327.5	3381.6	3300.5	3276.7	3319.6
	(551.1)	(573.1)	(537.8)	(534.5)	(539.8)
% Births < 1500 g	0.0084	0.0093	0.0079	0.0079	0.0085
	(0.09)	(0.10)	(0.09)	(0.08)	(0.09)
% Male Birth	0.503	0.498	0.505	0.506	0.504
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)

Notes: Summary statistics for birth outcomes before and after exposure event. Exposed and unexposed counties defined in Figures 1 and 2.

Source: National Center for Health Statistics (1972-1988).

Table 2: Effect of Event on Determinants of Birth

Panel 2a: Without Controlling for Time Trends

	Mom C.B. Age	Mom H.S. Grad.	Dad H.S. Grad.	Rec. Prenatal Care
Effect of Exposure	-0.0183	0.1076***	-0.0185	0.0763***
	(0.0142)	(0.0215)	(0.0325)	(0.0178)
Year×Month FE's	Yes	Yes	Yes	Yes
County FE's	Yes	Yes	Yes	Yes
Covariate Set	Yes	Yes	Yes	Yes
Linear Time Trends	No	No	No	No
Observations	96,412	96,412	96,412	96,412

Panel 2b: Controlling for Time Trends

	Mom C.B. Age	Mom H.S. Grad.	Dad H.S. Grad.	Rec. Prenatal Care
Effect of Exposure	-0.0132	0.0334	0.0035	0.0273
	(0.0203)	(0.0263)	(0.0382)	(0.0536)
Year×Month FE's	Yes	Yes	Yes	Yes
County FE's	Yes	Yes	Yes	Yes
Covariate Set	Yes	Yes	Yes	Yes
Linear Time Trends	Yes	Yes	Yes	Yes
Observations	96,412	96,412	96,412	96,412

Notes: Results are interpreted as the percent change population demographic. Difference-in-differences analysis using covariates as outcome variables. Covariates include: Mother of recommended childbearing age, Mother graduated high school, Father graduated high school, and provision of recommended prenatal care. Regressions calculated using Year×Month and State×County Fixed Effects, a partial set of Covariates, and county-specific Linear Time Trends. Treatment and control groups defined in Figures 1 and 2. Robust standard errors are reported in parenthesis and are clustered at the county level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: National Center for Health Statistics (1976-1984).

Table 3: Effect of Exposure on Birth Weight

	(1)	(2)	(3)	(4)
Effect of Exposure	0.0012	0.0008	0.0019	0.0016
	(0.0052)	(0.0054)	(0.0085)	(0.0096)
Mother younger than 20		-0.0268***		-0.0269***
		(0.0025)		(0.0025)
Mother older than 35		0.0252***		0.0253***
		(0.0041)		(0.0042)
M. Edu. less than H.S.		-0.0010		-0.0009
		(0.0032)		(0.0032)
M. Edu. only H.S.		-0.0025		-0.0025
		(0.0019)		(0.0019)
F. Edu. less than H.S.		-0.0176***		-0.0177***
		(0.0022)		(0.0022)
F. Edu. only H.S.		-0.0050***		-0.0051***
		(0.0012)		(0.0012)
Female		-0.0248***		-0.0248***
		(0.0018)		(0.0018)
No prenatal care		-0.0183***		-0.0182***
		(0.0030)		(0.0029)
Plurality		-0.3656***		-0.3653***
		(0.0261)		(0.0261)
Year×Month FE's	Yes	Yes	Yes	Yes
State×County FE's	Yes	Yes	Yes	Yes
Linear Time Trends	-	-	Yes	Yes
Observations	96,412	96,412	96,412	96,412

Notes: Results are interpreted as the percent change in birth weight. Difference-in-differences analysis of exposure groups calculated using Year×Month and State×County Fixed Effects, and county-specific Linear Time Trends. Treatment and control groups defined in Figures 1 and 2. Robust standard errors are reported in parenthesis and are clustered at the county level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: National Center for Health Statistics (1972-1988).

Table 4: Effect of Exposure on Birth Outcomes

Table 4a: Probability of Extremely Low Birth Weight

	(1)	(2)	(3)	(4)
Effect of Exposure	-0.0032	-0.0029	-0.0005	-0.0002
	(0.0021)	(0.0021)	(0.0049)	(0.0049)
Year×Month, County FE's	Yes	Yes	Yes	Yes
Covariate Set	-	Yes	-	Yes
Linear Time Trends	-	-	Yes	Yes
Observations	4,536	4,536	4,536	4,536

Table 4b: M:F Gender Ratio

	(1)	(2)	(3)	(4)
Effect of Exposure	-0.0110	-0.0125	0.0024	-0.0037
	(0.0145)	(0.0137)	(0.0405)	(0.0350)
Year×Month, County FE's	Yes	Yes	Yes	Yes
Covariate Set	-	Yes	-	Yes
Linear Time Trends	-	-	Yes	Yes
Observations	4,536	4,536	4,536	4,536

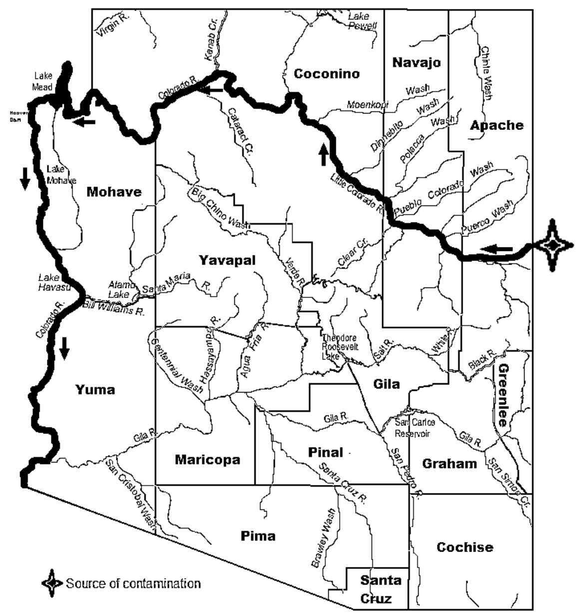
Table 4c: Cohort Size

	(1)	(2)	(3)	(4)
Effect of Exposure	-0.0553	-0.0573	-0.0529	-0.0596
	(0.0884)	(0.0884)	(0.0693)	(0.0670)
Year×Month, County FE's	Yes	Yes	Yes	Yes
Covariate Set	-	Yes	-	Yes
Linear Time Trends	-	-	Yes	Yes
Observations	4,536	4,536	4,536	4,536

Notes: Results for Panels A and B are interpreted as the change in the probability of experiencing the measured outcome, while results for Panel C are interpreted as the percent change in cohort size. Difference-in-differences analysis of exposure groups calculated using Year×Month and State×County Fixed Effects, and county-specific Linear Time Trends. Treatment and control groups defined in Figures 1 and 2. Robust standard errors are reported in parenthesis and are clustered at the county level. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Source: National Center for Health Statistics (1972-1988).

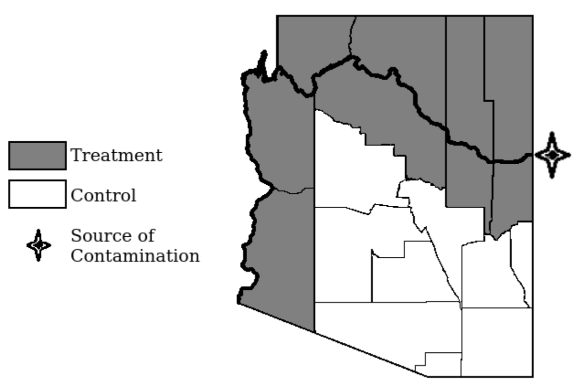
Figure 1: Map of Contamination Spread



Note: The Church Rock uranium mill holding pond failure released 1,100 tons of solid uranium mill waste and 93,000 gallons of liquid uranium tailing solution into the Rio Puerco, which connects to the Little Colorado and the Colorado river. Boundaries of contaminated waterways are not drawn to scale. Contaminated waterways are exaggerated in size to enhance visibility for the reader.

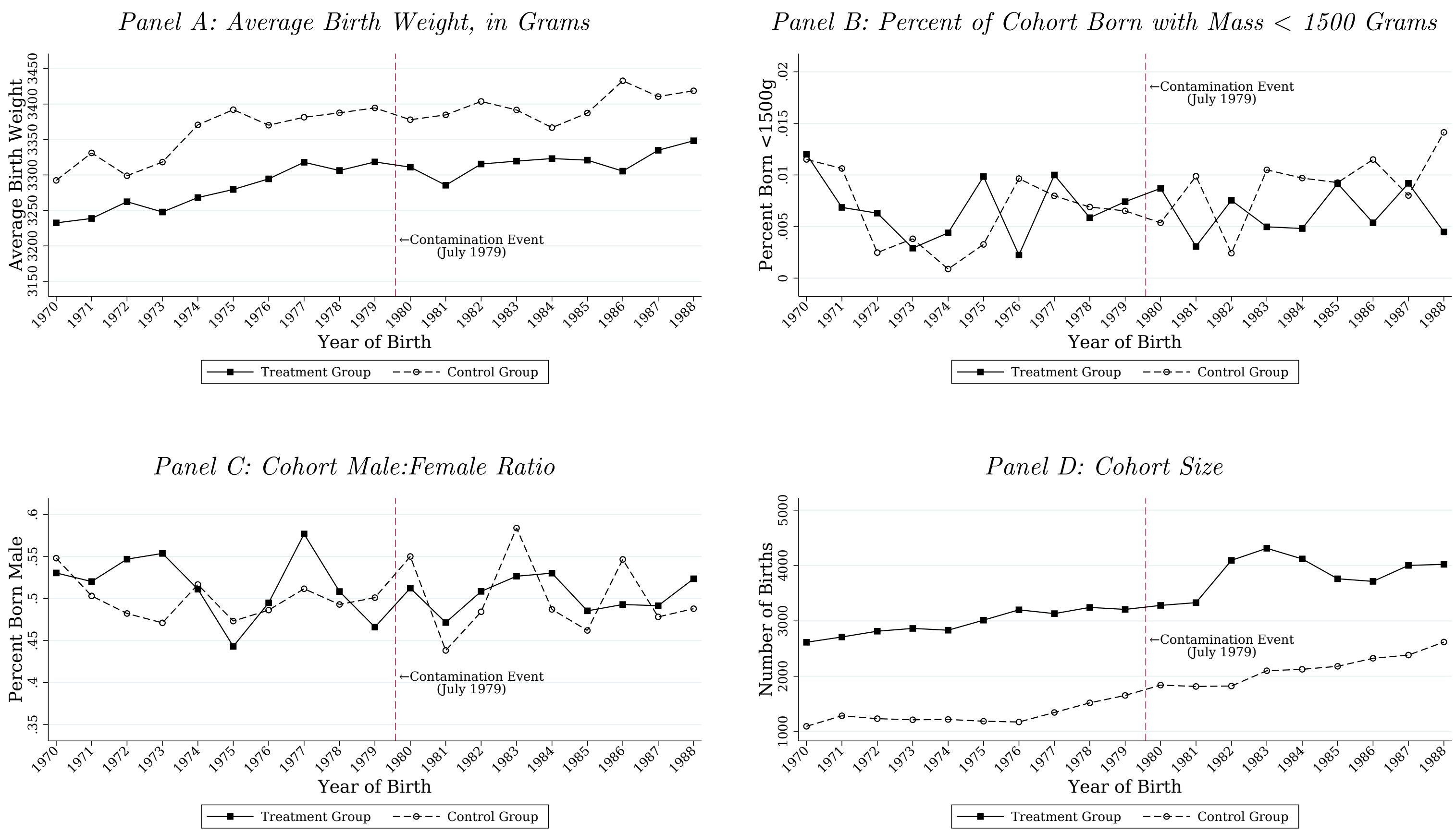
Source: United States Geological Survey, 2014.

Figure 2: Map of Treatment and Control Groups for Birth Outcome Analysis



Note: Counties are assigned to treatment and control groups according to the spread of contamination shown in Figure 1. Treated counties therefore include Apache, Navajo, Coconino, Mohave and Yuma Counties, Arizona, as well as Clark County, Nevada. Notice that in 1979, La Paz County was part of Yuma; this detail is accounted for in all of my analyses.

Figure 3: Birth Outcomes Over Time



Note: Birth outcomes over time for treated and untreated Native American Populations. Treatment and control groups defined in Figures 1 and 2.

Source: National Center for Health Statistics (1976-1984).

HOW VALUABLE ARE NATIONAL PARKS? EVIDENCE FROM A PROPOSED NATIONAL PARK EXPANSION IN ALASKA.

By Michael Spanbauer, Lindsay Johnson, Patrick Button

Executive Summary

In the interest of balancing government spending and environmental preservation, it is critical to evaluate how taxpayers value national park land and for what they are valuing it. One key component of this evaluation is to calculate a taxpayers passive use value, or their willingness to allocate tax dollars to the protection of land that they may never directly use. We estimate this passive use value using a questionnaire, where we use the contingent valuation method to determine a willingness to pay (WTP) for expanding the size of Denali National Park by 5%.

We field a questionnaire designed around a realistic policy proposal which would increase the size of Denali National Park in Alaska by 5%, or 325,340 acres. Similar park expansions have occurred regularly since the establishment of the National Park Service. The survey first educated respondents on Alaskan geography, the current status of protected land and wildlife in Alaska, and common arguments for and against National Park expansion. The survey then described a proposal to expand the Denali National Park in area by approximately 5% and asked a series of questions designed to bound the respondents WTP. Finally, the survey asked respondents to answer questions about what motivated their support for the program.

We use interval regressions to calculate WTP, which is the standard approach used for dichotimous-choice data that bounds responses in an interval. We estimate that the national average WTP for a 5% expansion of Denali National is a single payment of $115 to $409, according to our preferred specifications. This range of WTP estimates is higher than WTP estimates from past studies.

Respondents' answers to questions regarding the motivations for their support of the program collectively indicate that support is driven by passive use values. The motivation that was most often listed as either "very important" or "important" was to increase protection from oil spills (85.8% very important or important), followed by increasing and protecting Alaska's biodiversity (84.3%) and preserving Alaska's beauty (84.2%). These results suggest that there are significant passive use values that should be considered when deciding whether to expand national parks or to permit the economic development of protected land.

Keywords: national parks; willingness-to-pay; nature conservation; contingent valuation; biodiversity; environmental policy.

JEL: Q24, Q28, Q51, Q57, Q58, R52.

1 Introduction

In 1916, President Woodrow Wilson created the National Park Service as a bureau under the Department of the Interior, which manages all National Parks, national monuments, and natural and historical areas.[1] Today, the National Park Service draws an annual budget of approximately $2.3 billion and employs more than 20 thousand individuals. These government employees care for 84 million acres of land, 54 million of which are located in Alaska.[2]

Alaska is outside the Contiguous United States (CONUS), and therefore does not have frequent visits from the majority of American taxpayers. It does, however,

[1] `https://www.nps.gov/aboutus/history.htm` United States National Park Service (2015a). U.S. National Park Service-Alaska Regional Office- Alaska Parks. U.S. Department of the Interior Web. 13 Dec.2015.

[2] `https://edit.doi.gov/sites/doi.gov/files/uploads/2017_Highlights_Book.pdf` United States National Park Service (2015b). History (U.S. National Park Service). U.S. Department of the Interior Web. 13 Dec. 2015.

possess a rich history of oil pipelines and oil exploration which primarily dates back to the construction of the Trans-Alaska Pipeline System in the mid 1970s. Not only was it one of the longest pipelines ever constructed at the time, it was also one of the most controversial due to its crossing of traditional Native Alaskan land, concerns about its effect on the delicate arctic tundra, and other unique aspects of the Alaskan wilderness. A similar debate has occurred recently over the Dakota Access Pipeline. Proponents have argued that the pipeline would lower oil production costs and offer improved safety over existing rail-transport systems. Those opposed to the pipeline argue that it would desecrate tribal lands, endanger the water supply and thus be a cost to the Standing Rock Sioux tribe without providing the tribe an economic benefit.[3]

In the interest of balancing government spending and environmental preservation, it is critical to evaluate how taxpayers value national park land and for what they are valuing it. Since 64.3 percent of National Park acreage is located in remote Alaskan terrain, a crucial component of this evaluation is estimating the park's value for citizens who will never directly utilize it. Bateman and Langford (1997) describe this non-use value as being comprised of two categories: *bequest value*, or the value of passing the National Park to future generations, and *existence value*, or the value of preserving a wildlife habitat. Non-use value is also broadly referred to in the economics literature as *passive use value*.[4] The primary method for estimating passive use values is contingent valuation. This method uses a survey approach to determine how much an individual is willing to pay for a particular good.

A number of studies use contingent valuation methods to identify a consumer's

[3] See, for example, `https://www.nytimes.com/2016/11/30/learning/lesson-plans/battle-over-an-oil-pipeline-teaching-about-the-standing-rock-sioux-protests.html?_r=0`.

[4] This term was adopted by the United States Federal Court of Appeals to describe the existence value of an object. See *Ohio v United States Department of Interior, 880 F.2d 432*, available at `www.doi.gov/sites/doi.gov/files/migrated/restoration/upload/laws_Ohio1989.pdf` (accessed 10 July 2017). This term was subsequently used in economics literature to describe various values, including the 'existence-,' 'preservation-,' 'stewardship-,' 'bequest-,' 'inherent-' and 'option-value' (Bateman and Willis, 2001)

willingness to pay (WTP) for environmental goods: however few focus on the passive use values of remote wilderness preservations. Studies often focus instead on local environmental issues (*e.g.* Hanley and Craig (1991), Bateman and Langford (1997), Amigues et al. (2002)) or preserving readily-accessible National Parks (*e.g.* Bateman et al. (1992), Willis and Garrod (1993), Hadker et al. (1997), Bateman and Langford (1997), White and Lovett (1999)). Alaska provides a unique example of National Park land where the vast majority of Americans will never physically see or directly utilize it, similar to Stefanski and Shimshack's (2016, hereafter Stefanski and Shimshack) study of the passive use for a remote marine sanctuary.

To estimate the national average WTP for Alaskan National Parks, we conducted a survey of 753 people and then estimate upper and lower bounds for their WTP using the contingent valuation method. The survey respondents indicate if they would be willing to pay a one-time tax to the federal government, which would be added to their federal taxes in a single year for the purpose of expanding a single Alaskan National Park by 325,340 acres, or roughly five percent. Following the contingent valuation method, respondents are randomly given a price ($4, $10, $20, $40, $80, or $120) and asked if they would be willing to pay for it. If they say "Yes", then they are asked if they would pay double this amount, or half if they say "No". This allows us to create an upper and lower WTP bound for those that say (Yes, No) or (No, Yes). For those that indicate that they would be willing to pay some amount (they answer "Yes" at least once), we ask them why they value national park land in order to learn what motivates their WTP.

Results from our preferred specifications indicate that individuals value our proposed national park expansion at $115 to $409, which is a larger WTP than most of the environmental literature. This difference is likely caused by our proposed method of payment, which required only a one-time payment rather than a perpetual monthly or annual fee. Responses to questions about attitudes toward environmental connection and questions about connection to Alaska indicate that this value is driven by passive use. We use several different empirical specifications and find that estimates are either similar or are larger, indicating high passive use values amongst a nationally representative[5] sample of individuals.

2 Background and Literature

2.1 The U.S. National Park System and National Parks in Alaska

In March of 1872, with the creation of Yellowstone, the United States became the first country to create a national park. Advocates like John Muir and Theodore Roosevelt argued that national parks were an expression of democracy at its best. The U.S., as a part of the new world, offered vast wilderness, great ecological treasures and sacred landscapes. Americans viewed their land as the last great wilderness and seized the unique opportunity to protect it. Proponents explained national parks could be the environmental monuments of the United States, contrasted to the architectural monuments of Europe. Furthermore, unlike many of the European monuments, national parks would be accessible to all Americans, not just the wealthy.[6] Today, the National Park Service comprises more than 400 areas spanning the 50 states, the District of Columbia, American Samoa, Guam, Puerto Rico, and the Virgin Islands.[7]

Charles Sheldon worked to establish a park around Mount McKinley, succeeding on February 26, 1917 when President Woodrow Wilson signed the bill creating Mount McKinley National Park (renamed Denali National Park by President Obama in 2015) (Nash, 2001; Davis, 2015). In 1980, the U.S. Congress passed the Alaska National

[5]Sample is nationally representative following our raking procedure, as discussed in Section 3.1.
[6]`http://www.pbs.org/nationalparks`
[7]`https://www.nps.gov/aboutus/faqs.htm`

Interest Lands Conservation Act, designating 104 million acres as either national parks, forests, or preserves and also protecting 50 million more acres as wilderness. Mount McKinley National Park was renamed Denali National Park and Preserve after being expanded from 2 million acres to 6 million. Property rights were retained throughout the preserve, as were hunting and trapping rights in some sections (Clynes, 2016).

In Alaska today, the government and the National Park Service play a huge role in determining land use because the federal government owns 60 percent of the land in Alaska. There are currently 15 national parks in Alaska, which protect over 54 million acres of land.[8] Alaska is not only unique for how much land is managed by the National Park Service, but it is also unique for how few Americans will visit it.

Despite being unused by the majority of Americans, supporters of National Parks argue for its benefits. These benefits include its role as a critical habitat for endangered species, its biological diversity, and its carbon sequestration. The park also provides natural resources for Alaskan Natives to continue their subsistence living and cultural traditions on historically important lands. Finally, the park provides recreational activities and tourism jobs while also preserving unique American wilderness and beautiful landscapes. Critics of Alaskan National Parks, including many Alaskan residents, argue national parks harm commercial fishing activities, prevent oil development, stifle job creation, slow the economy for local residents, and stop potential development that locals may desire.

2.2 Related Literature

As previously discussed, a number of studies use contingent valuation methods to identify a consumer's willingness to pay (WTP) for environmental goods. These studies often focus instead on local environmental issues or preserving readily-accessible National Parks rather than measuring the passive use value of physically remote

[8] `https://www.nps.gov/aboutus/history.htm` United States National Park Service (2015a)

wilderness preservations. Our research contributes to the existing base of literature by examining a National Park that will never be seen or utilized by the vast majority of Americans, similar to the study conducted by Stefanski and Shimshack regarding a remote marine sanctuary.

Appendix Table A7 summarizes several prominent environmental economic publications which use Contingent Valuation methods. It can be seen that WTP values for National Parks and nature preservations range from as little as $0.33 per month up through $252.48 per year, in 2017 dollars. Studies focusing on local environmental protection efforts found smaller WTP values, between $9.67 and $43.62 in 2017 dollars. It can be seen that the existing body of literature focuses on recurring annual or monthly payments, with the exception being Stefanski and Shimshack; they use a single-payment scheme and find a WTP of $28-107 for marine sanctuary expansion. We follow the methods set by Stefanski and Shimshack and employ a single-payment scheme for our analysis.

3 Questionnaire Design

Our questionnaire is designed around a realistic policy proposal which would increase the size of Denali National Park in Alaska by 5%, or 325,340 acres. Similar park expansions have occurred regularly since the establishment of the National Park Service. In 2014, congress passed a bill with bipartisan support that included the largest expansion to the National Park Service since 1978. The bill designated seven sites as official national park units, adding about 120,000 acres to be managed by the park service. Despite being larger in magnitude than past expansions, our proposal is still realistic; the land we propose to include into the National Park System has been reserved by Alaska's Department of Natural Resources as an outdoor recreation area since 1970.[9]

We distributed the questionnaire through Qualtrics Research Core™ to 753 respondents from April to September 2016.[10] The questionnaire opens by asking questions which gauge the policy priorities of the respondents. The first question has respondents indicate, using a Likert scale, if more or less money should be spent on several societal problems (crime, environmental protection, public education, national debt, health care, foreign aid). The next question asks them to indicate, using a Likert scale, how important specific environmental goals are (reducing air pollution, protection of endangered species, safe recreational areas, reducing water pollution, clean rivers, lakes and beaches, public waters safe for drinking and swimming). Next, respondents were educated regarding Alaskan geography, the current status of protected land and wildlife in Alaska, common arguments for and against National Park expansion, and how expanding the existing park will affect human activities such as commercial development, fishing, and oil exploration and drilling.

The survey then described a proposal to expand the Denali National Park in area by approximately 5%, incorporating uninhabited bordering land, rivers, and mountains into the existing protected area. This expansion is shown in Figures 1a and 1b.

Respondents were then told their household would be required to pay $\$X$ to support the program:

> *At present, government officials estimate the program will cost your household a total of \$X. You would pay this in a special one time charge in addition to your regular federal taxes. This money would be used only for the Denali National Park expansion program. If the program cost your household a total of \$X, would you vote for the program or against it?*

If they agreed to pay $\$X$ ("Yes"), they were asked if they would instead accept paying

[9] `http://dnr.alaska.gov/parks/units/denali1.htm` Alaska Department of Natural Resources (2015). Denali State Park. 5 July 2017.

[10] The entire questionnaire is presented in Appendix 2, available online.

$2X$. If they refused to pay X ("No"), they were asked if they are willing to pay $\$\frac{1}{2}X$. This approach attempts to bound the respondents' WTP, as those who answer (Yes, No) or (No, Yes) have a WTP that is bounded between either ($\$X$, $\$2X$) or ($\$\frac{1}{2}X$, $\$X$), respectively. We use this bounding approach because, as discussed by Johnston et al. (2017), open-ended questions often lead to unrealistic responses.

The questionnaire was administered in two phases. The first phase (April 6-7, 2016) had 226 respondents and had randomized initial program costs of \$4, \$10, \$20, \$40, and \$80, which were the initial program costs used by Stefanski and Shimshack. In the second phrase (August 26 to September 4, 2016) we had 527 respondents. In the second phase we replaced the initial program cost of \$4 with \$120 as \$4 proved far too low given the high proportion of (Yes, Yes) responses in the first phase for this initial value (77.4%).

Those that indicated they they would pay some amount for the program [(Yes, No), (No, Yes), (Yes, Yes)] were asked follow-up questions to determine what motivated their WTP.[11] They were asked to indicate on a Likert scale how important certain components of environmental protection (see Figure 4, Panel A and B) were to their WTP. Those that did not indicate support (No, No) were asked follow-up questions to determine why they did not support the program (see Appendix Table A3).

At the end of the survey, we collected socio-economic and demographic information (gender, age, race, Hispanic/Latino ancestry, employment status, industry, household income, education, political affiliation, urban/rural/suburban residence), similar to in previous studies (e.g., Hadker et al. (1997), Bateman and Langford (1997), White and Lovett (1999), Stefanski and Shimshack 2016)). To learn to what extent affiliation or connection with Alaska matters, which speaks to if the WTP is driven by passive use or not, we also asked for current state of residence, state of upbringing, how often the

[11] We made an error in the first wave of the questionnaire and those that answered (Yes, No) were accidentally not asked these follow-up questions. This meant that these responses are not available for 6.7% of the individuals who were supposed to be asked these questions.

respondent has visited Alaska, and if the respondent had visited a national park. We also asked respondents about their attitudes toward increasing federal taxes to pay for national park expansion.

3.1 Sample Composition

While our goal is to estimate a nationally-representative average WTP, unfortunately, our sample is not selected to be nationally representative. To correct for this, we raked the data following the procedures discussed by Kolenikov (2014), giving respondents from under-represented groups more weight and respondents from over-represented groups less weight. Raking the sample in this way makes the sample better reflect the average socio-economic and demographic characteristics of the national population. Table 1, columns 1 and 3, show that the survey over-sampled women, individuals aged 18-44, college graduates, Democrats, and individuals in households with income less than $150,000.

We used three data sources to construct weights using raking so that our raked sample matched the national population based on the variables in Table 1. Statistics on the national distribution of gender, Hispanic origin, race, and age are taken from July 2015 Census data.[12] Statistics on educational attainment and household income were drawn from the 2015 American Community Survey.[13] Political party affiliation was created by calculating the mean of bi-weekly Gallup Politics survey results, conducted from January 6, 2016, to October 9, 2016.[14]

Table 1 indicates that, after raking, our raked sample closely matches the national averages of these socio-economic variables. Henceforth, we use the raked sample for all of our results, although results from the unraked sample are available in the appendix. These unraked results indicate a slightly lower WTP (for example, the levels WTP is

[12] `https://www.factfinder.census.gov`, Dataset ID: PEPAGESEX and PEPALL5N (accessed 12/2016).

[13] `https://www.factfinder.census.gov`, Dataset ID: S1901 (accessed 12/2016).

[14] `http://www.gallup.com/poll/15370/party-affiliation.aspx` (accessed 12/2016)

on-average \$4.34 less).

4 Methodology

We use interval regression to estimate a WTP, which is the standard approach used for dichotimous-choice data that bounds responses in an interval (see, e.g., Cameron and Trivedi (2005), Cawley (2008), Viscusi, Huber and Bell (2012), Stefanski and Shimshack (2016)). We observe a bounded interval for the WTP for those that answer (Yes, No) ($\$X$, $\$2X$) and for those that answer (No, Yes) ($\$\frac{1}{2}X$, $\$X$). We first perform double-bounded dichotomous choice interval regressions in levels, using interval regressions. We then perform this regression in logs to adjust for the right-skew in the WTP responses (see Figure 2), as is common (e.g., Stefanski and Shimshack (2016)). For both the levels and logs regressions, we estimate the WTP with and without covariates. First, we add socio-economic and demographic covariates to observe how these factors influence WTP. We then we add covariates capturing region of residence and attachment to Alaska to see if WTP value are driven by actual use value rather than passive use value. Finally, we add preferences for environmental protection efforts and tax policies. Table 3 defines and summarizes each covariate used. We repeat this analysis using a single-bound dichotomous choice probit model[15] and a double-bounded dichotomous choice constrained bivariate probit model.[16] This is done to check the robustness of our model against changes to the functional form, as discussed in Section 5.1.1.

[15] The unobserved WTP is modeled as $y_i^* = \mathbf{x}'\beta + \varepsilon$ where we observe $y_i = 1$ if $y_i^* > \$X$ or $y_i = 0$ if $y_i^* \leq \$X$ and $P(y_i = 1|\mathbf{x}) = \int_{-\infty}^{\mathbf{x}'\beta} \phi(t)dt$ and $\phi(\cdot)$ is the standard normal density.

[16] A two-equation model, where $y_{1i} = 1$ if $y_{1i}^* > \$X$, $y_{2i} = 1$ if $y_{2i}^* > \$X$, and zero otherwise.

5 Results

Table 2 and Figure 2 summarizes the complete set of possible responses for both waves of the survey. Support for the program generally declines with the initial program cost, although support for the program is high for all initial program costs. (Yes, Yes) is the most common response for all initial program costs. Even for the most expensive initial program cost ($120), the most common answer is (Yes, Yes) at 39.4%, giving a WTP of greater than $240 for this group.

For the smallest initial program cost ($4), 77.4% have a WTP of $8 or more, 3.8% have $4 to $8, 3.8% have $2 to $4, and 15.1% have a WTP of less than $2. The results for this $4 initial program cost suggest two things. First, the average WTP is significantly higher than $4, such that this initial value does a poor job of bounding the WTP. This motivated us to replace this initial value with $120 in our second phase of the questionnaire, which does a better job of bounding WTP even though it is significantly higher. Second, there are a significant number of people with very low, likely zero, WTP. We probe this issue by asking those who answer (No, No) a follow up question in order to determine how many respondents have a true WTP near zero versus how many are "protest nos" and are responding negatively for reasons unrelated to having a near-zero WTP. We examine this issue further in Section 5.1.2.

We now turn to our interval regression analysis to estimate the average WTP. Table 4 presents our interval regression estimates in both level and log form. For the levels regressions (columns (1), (2), (3), and (4)), the WTP estimates are $111.10, $121.04, $115.82, and $116.19 respectively. For the logs regressions (columns (5), (6), (7), and (8)), the WTP estimate varies much more: $81.50, $123.60, $142.07, and $225.46, respectively. The inclusion of covariates seems to significantly affect the WTP estimate only in the logs regressions, but not in the level regressions. This magnitude appears partly driven by the raked data, as the range is somewhat smaller for the log regressions using unraked data ($83.44 to $186.77) (see Appendix Table A1). The

WTP estimates under levels are similar regardless of if the data is raked or not (range for unraked estimates is \$110.43 to \$114.17).

It can be seen that significant determinants in respondent's WTP are generally consistent across all specifications. Significant determinants decreasing an individual's WTP include spending the majority of childhood in Alaska ($p < 0.01$) and the feeling of opposition to increasing federal taxes to expand national parks ($p < 0.01$). While the latter is not unexpected, the fact that those who spent a majority of their childhood in Alaska have a lower WTP could be surprising. One might expect individuals who grew up in Alaska to have a stronger preference towards preserving its natural state. However, those who grew up in Alaska may also be more aware of the economic benefits to expanding the use of land for economic development. This effect could be driven by outliers, as only seven individuals indicated that they spent a majority of their childhood in Alaska. The effect of gender is significant and negative in specifications 2 and 6, but this significance disappears as other covariates are included. This corroborates the findings of Stefanski and Shimshack, who found gender to be unimportant.

Significant determinants increasing an individual's WTP include, unsurprisingly, preferences towards increased tax expenditures on national parks ($p < 0.01$). Frequent visits to the state of Alaska also increases WTP ($p < 0.01$ for more than five visits), a finding consistent with the literature that finds direct users have a higher WTP than passive users for environmental goods (e.g., Carson and Mitchell (1993), Carson (1997), Stefanski and Shimshack (2016)). College education also tends to increase WTP, but this relationship is slightly weaker ($p < 0.05$) and not significant across all specifications.

We do not find a significant relationship between age and WTP, contrary to the literature showing that WTP for environmental public goods appears to decline with age (Hoehn, 1991; Whitehead and Blomquist, 1991; Carson, 1997). Furthermore, having personal goals aligned with environmental protection is not significantly related to WTP for the park expansion.

We asked those who supported the park expansion proposal (they said "Yes" to at least one program cost) why they supported the program. The results, shown in Figure 4, indicate that most respondents see a variety of benefits to national parks and see national parks as a very important contributor to more than one goal. However some goals were deemed relatively more important by respondents. The goal that was most often listed as either "very important" or "important" was to increase protection from oil spills (85.8% very important or important), followed by increasing and protecting Alaska's biodiversity (84.3%) and preserving Alaska's beauty (84.2%). While all goals receive large support, the least supported goals are supporting recreational opportunities (77.1% very important or important, 5.8% not important at all), carbon sequestration (77.0%, 3.85%), and protecting Alaska Native populations (82.6% , 4.9%). The least supported (but still highly supported) goals are thus those linked more to active use of the park land rather than passive use.

5.1 Robustness and Limitations

We investigate several possible issues that may affect our WTP estimates. In most cases addressing for these issues makes our WTP estimates even higher. Since our estimated WTP is already high, this suggests that it could be even higher and that our estimates are likely lower bounds.

5.1.1 Functional Form

Since our dependent variable, the dichotomous choice for or against paying the one-time tax, is not continuous, the linear interval regression[17] model may not provide

[17] Interval regressions are a generalization of the Tobit Maximum Likeliehood Estimator. Tobit estimations require that regression errors are heteroskedastic and normally distributed, but are inconsistent if these assumptions are violated (Long, 1997).

the most consistent estimates. For this reason, we also estimate a probit model using the initial program cost and a constrained bivariate probit model using both the initial and follow-up program costs. As above, we repeat our analysis using logs and with and without control variables. These estimates are presented in Table 5 (with unraked estimates in Appendix Table A2). The levels results are similar in range to the interval regression results, ranging from \$143.59 to \$174.98. The log estimates, however, are larger in magnitude, ranging from \$161.68 to \$408.62. While our WTP estimates do not seem robust to this change in the functional form, they do indicate that our preferred WTP estimates in Table 4 are lower bounds.

5.1.2 "Protest Nos"

The possibility of "protest nos" may bias WTP estimates towards zero. A "protest no" would be an answer of (No, No) such that the upper bound for their WTP estimate ($\$\frac{1}{2}X$) is lower than their true WTP. This means that the individual is indicating a lack of support for the proposal for reasons other than their WTP, such as objecting to any taxes or to any government program. "Protest nos" seem likely for some respondents, as 15.1% who were given an initial program cost of \$4 supposedly have a WTP of less than \$2, as judged by their (No, No) responses. For many this is likely not a true "near zero" WTP. We ask those who answer (No, No) to give reasons why they do not support the program. Appendix Table A3 indicates the reasons for this lack of support. We group these reasons into those that suggest a true "near zero" WTP[18] and those that suggest a "protest no."[19] We deem any respondent who indicates one of the "protest no" reasons to be a "protest no," and we re-estimate the main results

[18] "I cannot afford to pay any amount at this time" (22.8% indicated this), "I live far from Alaska and do not see how the program is relevant to me" (14.7%), "Society has more important problems than protecting Alaskan wilderness" (6.5%), "I feel that the protection of Alaskan wilderness is unimportant" (2.72%).

[19] "I am opposed to paying any new taxes" (12.5%), "I do not want to put a monetary value on protecting Alaskan wilderness" (10.3%), "I do not trust the institutions that will handle the money for this program" (9.2%), "I do not believe that paying will solve the problem" (4.9%), "It is not my responsibility to protect Alaskan wilderness" (3.8%).

in Table 4 without these respondents. These results are shown in Appendix Table A4 and are similar, ranging from \$110.76 to \$210.84.

5.1.3 Confusion over Payment Vehicle

Another possible threat to our WTP estimates is that some individuals did not understand how they would be asked to pay for our proposed national park expansion. Such confusion could threaten the validity of our estimates, as discussed in Johnston et al. (2017). We state in the questionnaire that it is a one-time charge. But if respondents thought it was a charge over multiple years, then they may have indicated a lower WTP. This suggests that our WTP could be a lower bound. We included a debriefing question to assess if respondents thought that this was a one-time versus multiple-year charge. 49.04% (48.73% without raking weights) indicated that they did not think this was a one-time charge. We re-estimate our main results in Table 4 with these respondents dropped. These results are presented in Appendix Table A5. The WTP estimates decrease slightly, ranging from \$90.67 to \$184.06, and there are no significant changes to the factors linked to WTP.

5.1.4 Sensitivity to Scope

As discussed more by Stefanski and Shimshack, our WTP estimates could be sensitive to scope. Essentially, our WTP estimates may depend on the amount of the public good we offer in our proposal. We propose a 5% expansion of Denali National Park, and thus our WTP estimates reflect an increase of this magnitude. We use a single sample approach, that is, we only proposed one type of expansion of the park. We did this because we had limited funding to disseminate our questionnaire, and we wanted to focus on statistical power to provide a more precise estimate of the WTP. However, we agree with the recent literature (*e.g.*, Diamond (1996), Whitehead, Haab and Huang (1998), Hausman (2012), Haab et al. (2013), Carson et al. (2013)) that it would have been ideal to have had multiple samples with different sized proposed expansions of the park (*e.g.*, 10% increase instead of just 5% increase) to see if more or less of this good being provided leads to increased or decreased WTP.[20]

5.1.5 Sensitivity to Media Coverage of Oil Spills and Pipelines

We consider factors which may have increased our WTP estimates. One factor we consider is increased coverage of pipelines spills that may have occurred at the time of the second phase of the survey. Appendix Figure A1 shows increased search activity for "pipelines" at the same time as the second phase. While individuals can search for "pipelines" for a variety of reasons, we see this increase as being driven by coverage of Native Americans protesting an oil pipeline being built in North Dakota. On August 15th, 11 days before our survey was conducted, Dakota Access, LLC, filed a lawsuit against the Native American tribal leaders.[21] On August 22nd, four days before our survey was conducted, media reports began circulating that 29 protestors were arrested near the work site.[22]

Increased pipeline coverage may, for legitimate reasons, increase the WTP of individuals as they are more aware of the benefits of national parks as a way to protect land from environmental disasters. Evidence of this is seen by evaluating the respondents' ranking of "Increasing oil spill protection for Alaska" on a Likert importance scale. 70.02% of second phase respondents value this category as either important or very important, while only 52.65% of first phase respondents held the same beliefs.

As can be seen in Figure A1, Phase 1 occurred during a low-interest period. This

[20] Johnson et al. (2012) is an example of a paper that has done this.

[21] *Dakota Access, LLC, vs. Dave Archambault II, Jonathan Edwards, Dana Yellow Fat, Valerie Dawn Wolfnecklace, Clifton Verle Hollow, Donald Dennis Strickland, Aaron Gabriel Neyer, and John and Jane Does*, Case 1:16-cv-00296-DLH-CSM filed in the United States District Court for the District of North Dakota, Western Division.

[22] `www.chicagotribune.com/news/nationworld/ct-dakota-access-pipeline-timeline-dapl-20161219-htmlstory.html` Howard, Brandon (2017). Timeline: History of the Dakota Access Pipeline. Chicago Tribune. 24 Jan 2017.

allows us to re-estimate our main results using Phase 1 and Phase 2 survey respondents separately.[23] A comparison of Phase 2 results with Phase 1 results will determine if this event skewed our full-sample results. Appendix Table A6 presents these estimates, and it can be seen that WTP values are similar across phases. This is evidence indicating the increased search interest in pipelines during Phase 2 does not influence our full-sample results.

6 Discussion and Conclusion

National Parks provide many important benefits, but could also be used for economic development. Alaska has a history of developing land for oil exploration and transportation, similar to recent land developments related to the Dakota Access Pipeline. Determining the best use of land requires a full understanding of the costs and benefits of National Parks, as well as how taxpayers value the land in question. A key component of this is to estimate the "passive use" value that individuals attribute to protected National Park land that they may never directly use. We estimate this passive use value using a questionnaire, where we use the contingent valuation method to determine a willingness to pay (WTP) for expanding the size of Denali National Park by 5%.

We find that respondents do value National Parks. We estimate that the national average WTP for a 5% expansion of Denali National is a single payment of $115 to $409, according to our preferred specifications, but this WTP may be even higher under other plausible specifications. Moreover, from respondents answers to questions about what motivated their support for the program, we find that this WTP is largely driven by passive use value.

Our range of WTP estimates is higher than in other studies. A number of possible

[23] We drop those with initial program costs of $4 from phase one and those with initial program costs of $120 from phase two so that both phases have the equivalent distribution of initial program values.

explanations for this difference exist, such as respondents holding higher passive use values of Alaskan wilderness reserves. This is possible as national parks have a higher profile relative to other areas such as marine ecosystems or less prominent park lands. It is also possible some of our large WTP estimates are driven by the particular functional forms of the model, such as the probit regressions. Finally, it is possible this difference arises because our survey described the expansion's cost as a one-time payment; most other authors asked respondents to commit to monthly or annual fees in perpetuity.

Our estimated WTP for national park land suggests that there are significant passive use values that should be considered in any assessment of whether to expand national parks or to permit the economic development of protected land.

Table 1: Sample Composition

Characteristic	Our Sample (%)	Raked Sample (%)	U.S. Population (%)
Gender: Female	61.27	51.26	51.32
Ethnicity: Hispanic or Latino	15.14	15.53	15.51
Race: White	79.05	78.35	78.39
Race: Black	11.95	12.75	12.71
Race: Asian	5.58	5.71	5.73
Age: 18-44	71.97	46.75	46.79
Age: 45 and older	28.03	53.25	53.20
Educ: Bachelor's deg or higher	37.18	26.69	26.65
Party: Identify as Democrat	41.64	30.41	30.42
HH Income: <$25,000	25.77	23.11	23.05
HH Income: $25,000 - $49,999	29.35	23.50	23.49
HH Income: $50,000 - $74,999	18.99	17.80	17.81
HH Income: $75,000 - $99,999	12.88	12.08	12.12
HH Income: $100,000 - $149,999	10.09	13.15	13.13
HH Income: >$150,000	2.92	10.36	10.39

Notes: U.S. Population information obtained from the United States Census Bureau. Statistics on Gender, Origin, Race and Age calculated using July 2015 Census data. Statistics on Educational Attainment and Household Income calculated using 2015 American Community Survey data. Political party affiliation obtained from Gallup Politics. Political party affiliation statistic created by calculating the mean of bi-weekly survey results, which were conducted from January 6, 2016, to October 9, 2016, by Gallup Politics. N=753.

Table 2: Dichotomous Program Response by Questionnaire Version

Version	Yes-Yes (%)	Yes-No (%)	No-Yes (%)	No-No (%)	N
A ($4, $8, $2)	77.36	3.77	3.77	15.09	53
B ($10, $20, $5)	71.24	13.07	5.23	10.46	153
C ($20, $40, $10)	45.70	25.17	9.27	19.87	151
D ($40, $80, $20)	47.71	18.30	16.99	16.99	153
E ($80, $160, $40)	41.61	30.87	7.38	20.13	149
F ($120, $240, $60)	39.36	22.34	13.83	24.47	94
Total	51.93	20.58	9.83	17.66	753

Notes: Summary of the complete set of possible response outcomes for the combined survey data. Yes-Yes represents respondents who voted in favor of the program for both the first and second (doubled) price. Yes-No represents respondents who voted in favor of the program for the first price and against the program when presented the second (doubled) price. No-Yes represents respondents who voted against the program when presented the first price and then voted for the program when presented the second (halved) price. No-No represents respondents who voted against the program for both the first and the second (halved) price. Versions A to E were randomly given with equal probability in phase one, while B to F were given in phase two.

Table 3: WTP and Determinants of WTP Estimation: Summary Statistics

Characteristic	Description	Mean	Raked Mean
WTP	Approximate WTP (using midpoint method)	66.89	75.30
White	Race: White, Not Hispanic or Latinx	0.672	0.784
Young	Age: 18-34	0.471	0.305
Female	Gender: Female	0.614	0.512
College	Education: Bachelor's degree or higher	0.372	0.267
Low Income	Household Income: Less than $20,000	0.195	0.182
Democrat	Party: Democrat	0.417	0.304
NPS Visitor	Experience: Has visited a National Park	0.699	0.721
AK Visit 1	Experience: Has visited Alaska 1 time	0.089	0.097
AK Visit 2	Experience: Has visited Alaska 2 times	0.070	0.058
AK Visit 3-5	Experience: Has visited Alaska 3-5 times	0.028	0.015
AK Visit >5	Experience: Has visited Alaska 5+ times	0.016	0.036
AK Birth	Experience: Born in Alaska	0.009	0.004
Env. Protect More	Attitude: Nation should spend more on Environmental Protection	0.628	0.565
Env. Protect Less	Attitude: Nation should spend more on Environmental Protection	0.096	0.144
Air	Attitude: Air pollution reduction is an important personal goal	0.495	0.457
Endangered	Attitude: Endangered species protection is an important personal goal	0.434	0.415
Water	Attitude: Water pollution reduction is an important personal goal	0.689	0.681
Support Tax	Attitude: Support increasing federal taxes to expand national parks	0.514	0.499
Oppose Tax	Attitude: Oppose increasing federal taxes to expand national parks	0.276	0.278
Observations		753	753

Notes: Summary statistics of respondents to Qualtrics online survey and definitions of WTP determinants used in various models.

Table 4: WTP and Determinants of WTP Estimation:
Interval Regressions, Raked Data

	Double-Bounded Dichotomous Choice Interval Estimations in Levels				Double-Bounded Dichotomous Choice Interval Estimations in Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	111.10***	124.15***	92.98***	42.23*	4.40***	4.59***	4.04***	3.13***
	(11.77)	(22.79)	(23.51)	(23.44)	(0.15)	(0.34)	(0.37)	(0.39)
White		-31.08	-24.96	-3.42		-0.47	-0.45	-0.08
		(19.45)	(17.17)	(16.82)		(0.29)	(0.28)	(0.27)
Young		14.30	5.11	4.84		0.30	0.20	0.20
		(14.63)	(13.03)	(12.66)		(0.24)	(0.23)	(0.21)
Female		-32.84**	-20.63	-15.88		-0.52**	-0.33	-0.25
		(15.21)	(14.13)	(12.34)		(0.26)	(0.25)	(0.21)
College		38.60**	15.72	16.67		0.57*	0.23	0.23
		(18.78)	(16.87)	(15.04)		(0.30)	(0.30)	(0.26)
Low Income		-17.58	-6.68	-3.08		-0.25	-0.06	0.00
		(17.22)	(16.24)	(14.70)		(0.30)	(0.29)	(0.25)
Democrat		48.60***	44.15***	29.55**		0.75***	0.71***	0.48**
		(15.14)	(13.85)	(13.29)		(0.23)	(0.22)	(0.21)
NPS Visitor			22.50	16.06			0.50**	0.40*
			(14.45)	(12.17)			(0.25)	(0.21)
AK Visit 1			31.25	34.12			0.59	0.64
			(25.75)	(22.39)			(0.43)	(0.40)
AK Visit 2			24.84	3.72			0.41	0.06
			(29.35)	(36.64)			(0.43)	(0.56)
AK Visit 3-5			88.70	75.03*			1.40	1.19
			(60.76)	(44.53)			(1.02)	(0.74)
AK Visit >5			296.07***	232.11***			4.29***	3.08***
			(56.98)	(46.77)			(0.84)	(0.64)
AK Birth			-99.25**	-89.80***			-1.72**	-1.55***
			(48.07)	(33.65)			(0.79)	(0.55)
Env. Protect More				20.12				0.32
				(14.92)				(0.26)
Env. Protect Less				12.53				0.40
				(17.98)				(0.32)
Air				11.39				0.25
				(15.61)				(0.26)
Endangered				-3.17				0.02
				(15.10)				(0.24)
Water				8.56				0.18
				(14.83)				(0.27)
Support Tax				76.34***				1.23***
				(14.86)				(0.26)
Oppose Tax				-61.27***				-1.16***
				(14.51)				(0.26)
Observations	753	753	753	753	753	753	753	753
Log Likelihood	-956.90	-918.22	-894.43	-796.46	-920.74	-887.56	-867.63	-765.09
Predicted WTP ($)	**111.10**	**121.04**	**115.82**	**116.19**	**81.50**	**123.60**	**142.07**	**225.46**

Notes: Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: WTP and Determinants of WTP Estimation: Probit and Bivariate Probit, Raked Data

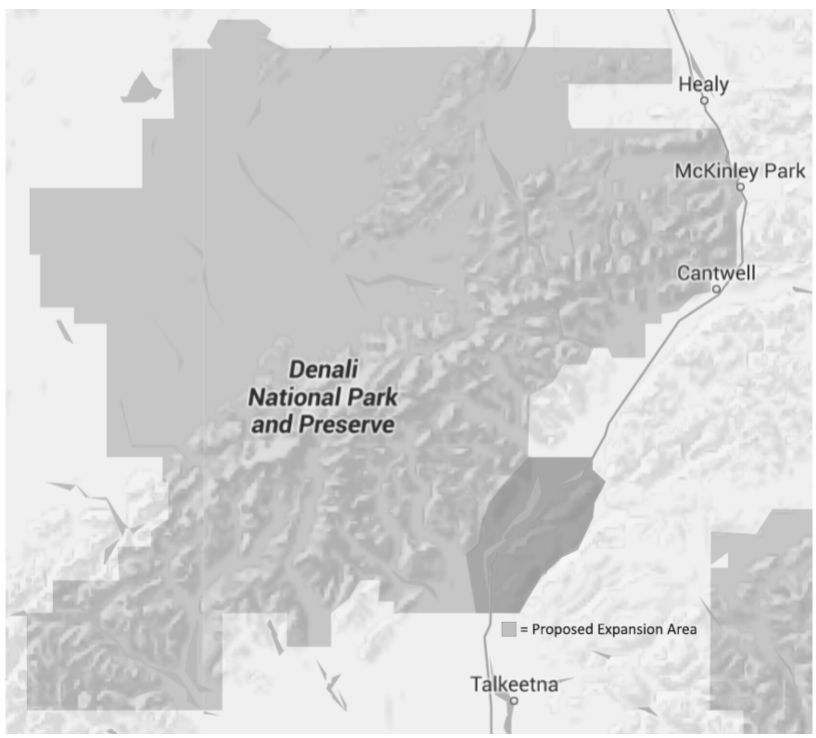
	Single Bound Dichotomous Choice Probit				Double-Bounded Dichotomous Choice Constrained Bivariate Probit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.68***	0.27	1.19***	0.91**	0.69***	0.31	1.36***	1.17***
	(0.12)	(0.32)	(0.27)	(0.37)	(0.09)	(0.24)	(0.25)	(0.33)
Initial Program Cost	-0.00*	-0.01***			-0.00***	-0.01***		
	(0.00)	(0.00)			(0.00)	(0.00)		
LN(Initial Program Cost)			-0.20***	-0.29***			-0.27***	-0.37***
			(0.08)	(0.08)			(0.07)	(0.06)
Full Covariates	-	Yes	-	Yes	-	Yes	-	Yes
Observations	753	753	753	753	753	753	753	753
Log Likelihood	-463.16	-347.14	-459.48	-344.89	-895.20	-731.21	-900.92	-737.08
Predicted WTP (in dollars)	**174.98**	**161.92**	**352.75**	**408.62**	**143.59**	**150.49**	**161.68**	**205.74**

Notes: Set of full covariates includes all coefficients used in Table 4. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Map of the Proposed Expansion to Denali National Park

Panel A: Alaska State

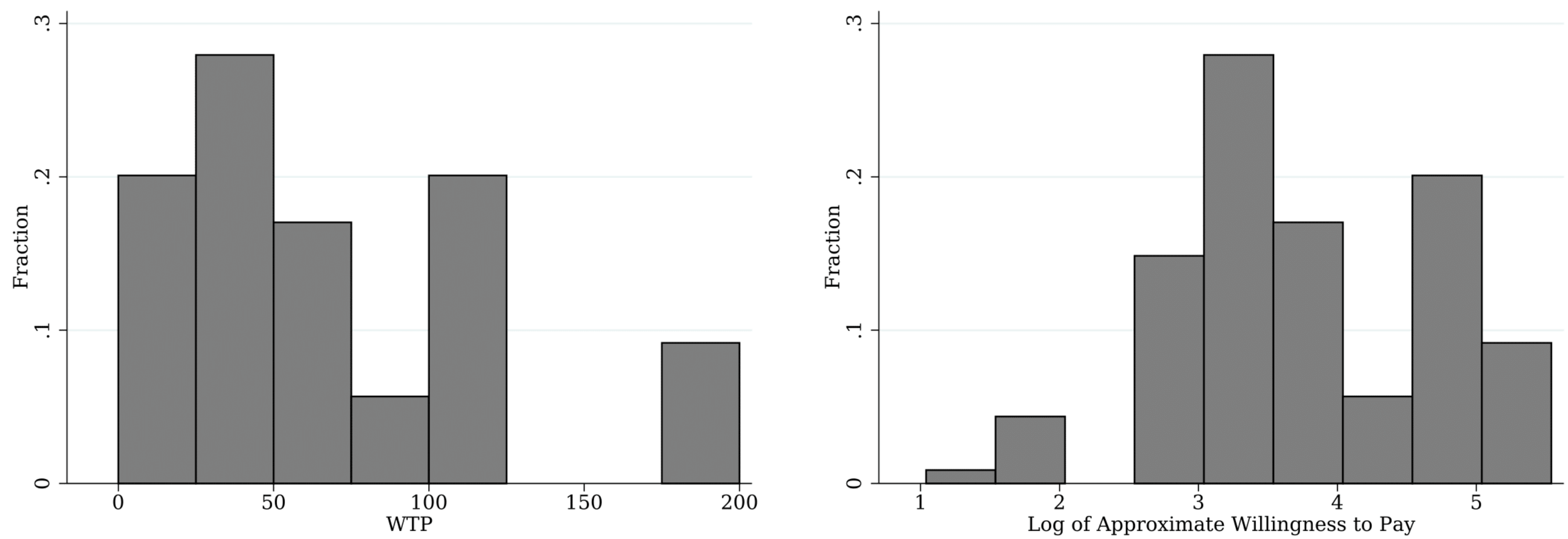
Panel B: Denali National Park



Notes: Figure 1a illustrates the size and location of Denali National Park in Alaska, while Figure 1b illustrates the proposed expansion area. The map in Figure 1b was shown to survey respondents prior to inquiring about their willingness to pay for the expansion project.

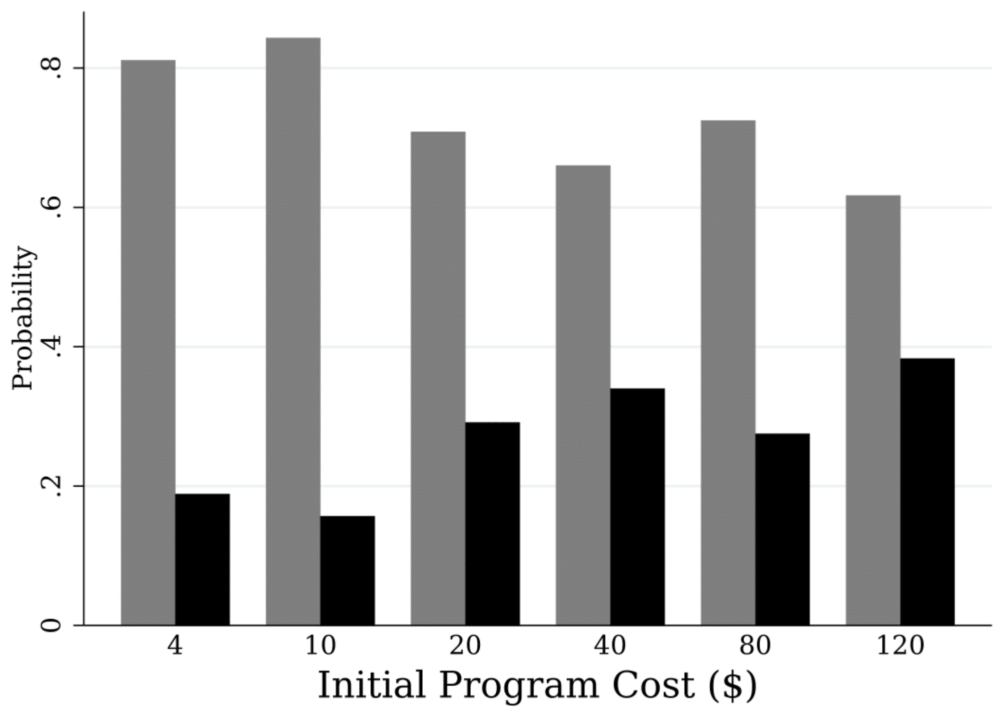
Source: Map data ©2015 Google. Expansion area added to map using commercially available photo-editing software.

Figure 2: Distribution of Willingness-to-Pay (WTP) Estimates



Notes: Midpoint value of WTP, as reported in survey data, reported in both level and log form. WTP is only calculated when both interval endpoints are observed in survey data (when responses are (Yes, No) or (No, Yes)).

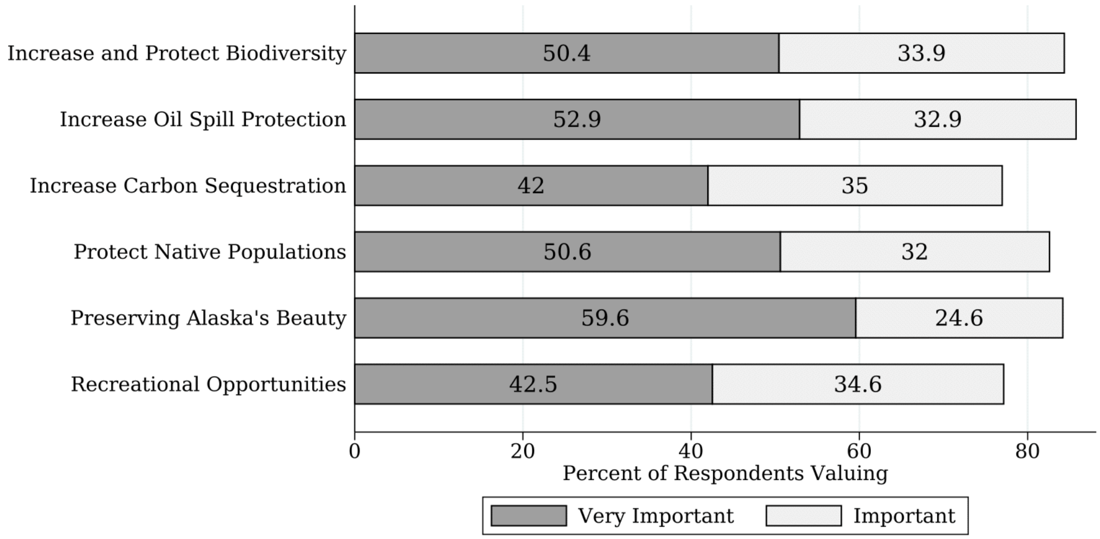
Figure 3: Probability of "Yes" and "No" Responses



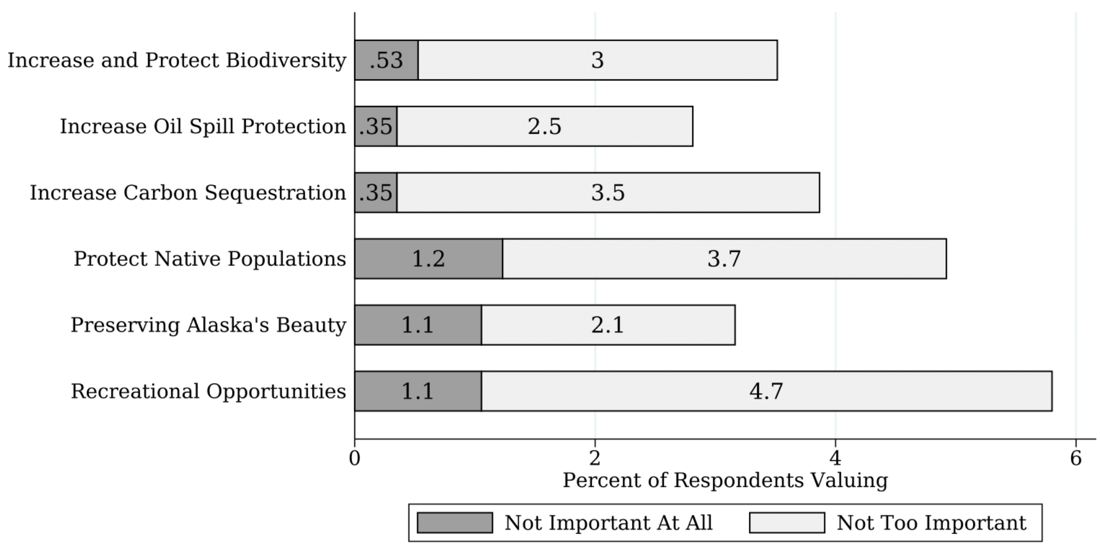
Notes: Probability of "Yes" (light bar) or "No" (dark bar) responses to each initial program cost. Initial program costs were randomized.

Figure 4: Respondents' Perspective of Goals, Motivations for Affirmative Vote

Panel A: Percent of Respondents Viewing Goals as Important



Panel B: Percent of Respondents Viewing Goals as Not Important At All



Notes: Respondents were asked to rank each goal on a scale of [Very Important, Somewhat Important, Neutral, Not Too Important, Not Important At All, Not Sure]. Very Important and Somewhat Important categories are classified as viewing the particular goal as Important. Not Too Important and Not Important At All categories are classified as viewing the particular goal as Unimportant. Results reported as percent of Affirmative voters.

Supplementary Graphs and Results:

Table A1: WTP and Determinants of WTP Estimation:
Interval Regressions (Unraked)

	Double-Bounded Dichotomous Choice Interval Estimations in Levels				Double-Bounded Dichotomous Choice Interval Estimations in Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	110.43***	116.61***	87.70***	43.85**	4.42***	4.42***	3.86***	3.14***
	(6.12)	(15.20)	(16.82)	(18.33)	(0.09)	(0.25)	(0.28)	(0.32)
White		-11.46	-11.76	-7.81		-0.10	-0.12	-0.04
		(11.11)	(11.06)	(10.42)		(0.19)	(0.19)	(0.17)
Young		1.41	-0.09	-2.66		0.08	0.06	0.02
		(10.00)	(10.05)	(9.51)		(0.17)	(0.17)	(0.16)
Female		-27.14**	-18.17*	-23.21**		-0.44**	-0.28	-0.38**
		(10.62)	(10.74)	(10.23)		(0.18)	(0.18)	(0.17)
College		22.51**	11.78	12.61		0.39**	0.19	0.22
		(10.63)	(11.05)	(10.39)		(0.18)	(0.18)	(0.17)
Low Income		-24.50*	-15.70	-10.57		-0.37*	-0.20	-0.11
		(12.69)	(12.86)	(11.86)		(0.22)	(0.22)	(0.20)
Democrat		33.51***	33.12***	14.67		0.56***	0.55***	0.23
		(10.26)	(10.11)	(9.77)		(0.17)	(0.17)	(0.16)
NPS Visitor			28.30**	12.80			0.58***	0.30*
			(11.10)	(10.03)			(0.19)	(0.17)
AK Visit 1			25.32	26.86			0.46	0.48
			(20.00)	(18.06)			(0.33)	(0.30)
AK Visit 2			39.61*	46.10*			0.58*	0.69*
			(21.64)	(23.75)			(0.34)	(0.38)
AK Visit 3-5			31.87	23.52			0.53	0.35
			(34.70)	(35.68)			(0.56)	(0.53)
AK Visit >5			171.25***	138.93***			2.90***	2.32***
			(49.22)	(36.97)			(0.77)	(0.58)
AK Birth			-69.81*	-70.16***			-1.34*	-1.31**
			(37.67)	(27.06)			(0.73)	(0.57)
Env. Protect More				28.10**				0.52***
				(11.18)				(0.19)
Env. Protect Less				-9.57				-0.02
				(15.77)				(0.28)
Air				4.84				0.08
				(11.75)				(0.19)
Endangered				7.59				0.13
				(11.12)				(0.18)
Water				13.73				0.28
				(11.71)				(0.20)
Support Tax				83.49***				1.33***
				(11.41)				(0.19)
Oppose Tax				-33.63***				-0.70***
				(11.17)				(0.19)
Observations	753	753	753	753	753	753	753	753
Log Likelihood	-985.63	-967.35	-956.31	-870.37	-924.33	-907.71	-894.99	-805.00
Predicted WTP ($)	**110.43**	**110.49**	**111.72**	**114.17**	**83.44**	**94.05**	**108.67**	**186.77**

Notes: Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: WTP and Determinants of WTP Estimation: Probit and Bivariate Probit (Unraked)

	Single Bound Dichotomous Choice Probit				Double-Bounded Dichotomous Choice Constrained Bivariate Probit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.79***	0.21	1.23***	0.71***	0.69***	0.22	1.33***	0.86***
	(0.08)	(0.22)	(0.18)	(0.28)	(0.06)	(0.17)	(0.16)	(0.22)
Initial Program Cost	-0.00***	-0.00***			-0.00***	-0.00***		
	(0.00)	(0.00)			(0.00)	(0.00)		
LN(Initial Program Cost)			-0.18***	-0.22***			-0.25***	-0.26***
			(0.05)	(0.06)			(0.04)	(0.04)
Full Covariates	-	Yes	-	Yes	-	Yes	-	Yes
Observations	753	753	753	753	753	753	753	753
Log Likelihood	-437.90	-335.38	-435.97	-333.63	-897.75	-757.34	-898.78	-755.11
Predicted WTP (in dollars)	**196.84**	**206.49**	**812.91**	**1066.65**	**160.91**	**223.24**	**207.52**	**531.17**

Notes: Set of full covariates includes all coefficients used in Table 4. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Respondents' Reasons for Negative Vote (No, No)

Motivation	Count	Percent	Raked Count	Raked Percent
"Near Zero" WTP				
I cannot afford to pay any amount at this time	22	16.5	15.3	11.5
I live far from Alaska and do not see how the program is relevant to me	19	14.3	19.1	14.4
Society has more important problems than protecting Alaskan wilderness	8	6.0	4.8	3.6
I feel that the protection of Alaskan wilderness is unimportant	4	3.0	3.3	2.5
"Protest No"				
I am opposed to paying any new taxes	22	16.54	29.6	22.2
I do not want to put a monetary value on protecting Alaskan wilderness	14	10.5	14.7	11.1
I do not trust the institutions that will handle the money for this program	11	8.3	15.6	11.7
I do not believe that paying will solve the problem	7	5.3	4.0	3.0
It is not my responsibility to protect Alaskan wilderness	7	5.3	7.0	5.24
No answer	10	7.5	5.5	4.2
Other	9	6.8	14.0	10.5

Notes: This question was asked to respondents who responded "No" to both program costs (No, No). Of the 753 survey respondents, 133 were asked this question. 51 additional respondents were asked this question by mistake; their responses are omitted from this table.

Table A4: WTP and Determinants of WTP Estimation: Interval Regressions, Raked Data, Dropping "Protest Nos"

	Double-Bounded Dichotomous Choice Interval Estimations in Levels				Double-Bounded Dichotomous Choice Interval Estimations in Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	131.31***	148.36***	119.16***	55.23**	4.71***	4.95***	4.44***	3.33***
	(12.79)	(24.23)	(22.49)	(23.42)	(0.15)	(0.34)	(0.34)	(0.38)
White		-12.49	-7.84	6.97		-0.18	-0.20	0.06
		(19.21)	(15.89)	(15.83)		(0.27)	(0.25)	(0.23)
Young		-2.78	-10.82	-7.35		0.02	-0.06	0.01
		(14.41)	(12.27)	(12.23)		(0.22)	(0.21)	(0.20)
Female		-47.30***	-35.54**	-28.73**		-0.72***	-0.54**	-0.43**
		(15.39)	(13.85)	(11.81)		(0.25)	(0.24)	(0.20)
College		29.09	8.10	10.90		0.41	0.10	0.13
		(19.50)	(16.83)	(14.89)		(0.30)	(0.30)	(0.25)
Low Income		-22.83	-12.25	-6.82		-0.32	-0.14	-0.04
		(16.43)	(15.28)	(14.12)		(0.29)	(0.27)	(0.24)
Democrat		36.76**	31.36**	21.27		0.55***	0.51**	0.34*
		(14.85)	(12.96)	(13.09)		(0.21)	(0.20)	(0.19)
NPS Visitor			22.35*	13.64			0.52**	0.38*
			(13.53)	(11.74)			(0.24)	(0.20)
AK Visit 1			25.01	19.48			0.49	0.40
			(23.89)	(21.48)			(0.39)	(0.37)
AK Visit 2			8.59	-8.17			0.14	-0.14
			(29.23)	(34.78)			(0.42)	(0.52)
AK Visit 3-5			94.28*	75.99*			1.53*	1.25**
			(50.47)	(39.43)			(0.80)	(0.60)
AK Visit >5			279.04***	224.21***			3.82***	2.79***
			(53.66)	(44.05)			(0.80)	(0.60)
AK Birth			-96.07**	-80.26**			-1.66**	-1.42***
			(44.90)	(31.46)			(0.71)	(0.48)
Env. Protect More				17.44				0.28
				(14.16)				(0.25)
Env. Protect Less				22.96				0.60*
				(18.16)				(0.32)
Air				1.74				0.14
				(14.28)				(0.23)
Endangered				6.34				0.15
				(14.24)				(0.21)
Water				14.85				0.25
				(14.69)				(0.26)
Support Tax				76.66***				1.21***
				(14.31)				(0.25)
Oppose Tax				-33.59**				-0.70***
				(14.50)				(0.26)
Observations	692	692	692	692	692	692	692	692
Log Likelihood	-834.32	-802.89	-778.17	-710.64	-795.44	-769.32	-749.24	-673.96
Predicted WTP ($)	**131.31**	**132.25**	**126.65**	**127.86**	**110.76**	**134.57**	**154.67**	**210.84**

Notes: Results using Raked data while dropping "Protest Nos." Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: WTP and Determinants of WTP Estimation:
Interval Regressions, Raked Data, Dropping Those Confused About Payment Vehicle

	Double-Bounded Dichotomous Choice Interval Estimations in Levels				Double-Bounded Dichotomous Choice Interval Estimations in Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	114.93***	121.20***	110.45***	54.43*	4.51***	4.58***	4.28***	3.32***
	(10.08)	(25.67)	(28.86)	(29.93)	(0.14)	(0.37)	(0.41)	(0.47)
White		-19.19	-28.83	-9.73		-0.30	-0.50	-0.19
		(21.47)	(21.36)	(20.59)		(0.33)	(0.34)	(0.33)
Young		2.73	2.45	-2.34		0.15	0.15	0.08
		(16.21)	(16.55)	(15.91)		(0.28)	(0.28)	(0.27)
Female		-7.32	-3.16	1.89		-0.17	-0.08	-0.02
		(17.68)	(17.74)	(14.80)		(0.31)	(0.31)	(0.26)
College		29.59	26.04	33.12*		0.49*	0.39	0.50*
		(19.17)	(20.21)	(19.41)		(0.29)	(0.31)	(0.30)
Low Income		-11.63	-3.55	-5.75		-0.16	0.01	-0.00
		(20.20)	(19.91)	(17.46)		(0.36)	(0.34)	(0.29)
Democrat		18.00	17.40	6.41		0.31	0.32	0.16
		(17.11)	(16.73)	(16.99)		(0.27)	(0.26)	(0.26)
NPS Visitor			12.51	10.83			0.39	0.37
			(18.10)	(15.92)			(0.30)	(0.27)
AK Visit 1			59.34	47.65			0.97	0.81*
			(39.83)	(29.83)			(0.63)	(0.49)
AK Visit 2			-1.08	-27.33			0.01	-0.42
			(32.64)	(39.62)			(0.47)	(0.59)
AK Visit 3-5			107.21	82.27			1.59	1.15
			(73.42)	(58.62)			(1.25)	(0.99)
AK Visit >5			175.31***	125.58***			3.22***	2.33***
			(53.95)	(40.37)			(1.05)	(0.77)
AK Birth			-127.38***	-98.77**			-2.53***	-2.05***
			(45.04)	(42.43)			(0.92)	(0.79)
Env. Protect More				23.00				0.33
				(21.44)				(0.37)
Env. Protect Less				-7.87				0.04
				(20.52)				(0.35)
Air				-2.30				0.07
				(20.28)				(0.33)
Endangered				4.71				0.07
				(19.86)				(0.31)
Water				26.86				0.46
				(20.07)				(0.35)
Support Tax				54.60***				0.89**
				(20.25)				(0.35)
Oppose Tax				-33.67*				-0.62*
				(18.67)				(0.32)
Observations	386	386	386	386	386	386	386	386
Log Likelihood	-498.54	-492.39	-485.28	-450.15	-485.25	-478.52	-470.30	-433.54
Predicted WTP ($)	**114.93**	**121.23**	**125.88**	**123.97**	**90.67**	**109.81**	**145.50**	**184.06**

Notes: Results using Raked data while dropping respondents who reported being confused about payment amounts or methods. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: WTP and Determinants of WTP Estimation:
Interval Regressions, Raked Data, Phase 1 vs Phase 2

Table A6a: Phase 1 Respondents, Drop Initial Cost of $4

	Double-Bounded Dichotomous Choice Interval Estimations in Levels				Double-Bounded Dichotomous Choice Interval Estimations in Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	76.98***	156.35***	142.57***	64.85*	4.09***	5.92***	5.56***	3.83***
	(14.37)	(38.90)	(41.44)	(37.56)	(0.29)	(0.85)	(0.89)	(0.76)
Demographic Covariates	-	Yes	-	-	-	Yes	-	-
Experience Covariates	-	Yes	Yes	-	-	Yes	Yes	-
Attitude Covariates	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Observations	179	179	179	179	179	179	179	179
Log Likelihood	-237.59	-223.10	-219.89	-171.97	-229.01	-213.92	-210.35	-160.76
Predicted WTP ($)	**76.98**	**85.72**	**92.95**	**96.49**	**59.77**	**133.73**	**461.01**	**606.77**

Table A6b: Phase 2 Respondents, Drop Initial Cost of $120

	Double-Bounded Dichotomous Choice Interval Estimations in Levels				Double-Bounded Dichotomous Choice Interval Estimations in Logs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	88.03***	85.41***	54.44**	37.26	4.30***	4.24***	3.53***	3.17***
	(9.16)	(22.76)	(23.89)	(24.59)	(0.18)	(0.50)	(0.53)	(0.56)
Demographic Covariates	-	Yes	-	-	-	Yes	-	-
Experience Covariates	-	Yes	Yes	-	-	Yes	Yes	-
Attitude Covariates	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Observations	433	433	433	433	433	433	433	433
Log Likelihood	-480.81	-466.82	-455.95	-421.60	-461.66	-448.80	-438.60	-404.01
Predicted WTP ($)	**88.03**	**99.87**	**101.76**	**105.40**	**73.88**	**118.91**	**157.72**	**240.96**

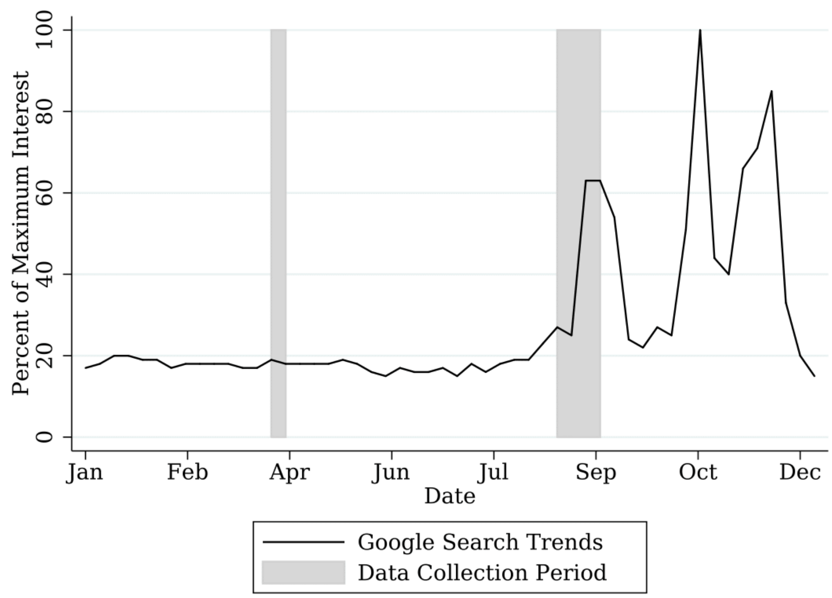
Notes: Results using Raked data, by survey phase, dropping respondents with initial costs of $4 (in Phase 1) or $120 (in Phase 2). WTP values are similar across phases, indicating the event described in Figure A1 does not influence our full-sample results. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Sample of Previous Studies; Publications Regarding WTP for Environmental Preservation.

Authors	Year	WTP	Approximate Value in 2017 Dollars	Payment Schedule	Notes
Stefanski & Shimshack	2016	$28.07–107.50	$28.07–107.50	One-time	Expansion of marine sanctuary
Amigues, *et al.*	2002	$7.00–25.00	$9.67–34.55	Annually	5-year program, local river preservation
White & Lovett	1999	£119.05	$252.48	Annually	Preservation of 11 National Parks
Bateman & Langford	1997	£23.29	$53.50	Annually	Preservation of a National Park
Bateman, *et al.*	1996	£9.94	$22.83	Annually	Providing local recreation spaces
Hadker, *et al.*	1996	₹7.50	$0.33	Monthly	Preservation of National Park
Willis, Garrod	1993	£24.56	$68.31	Annually	Cleaning Yorkshire Dales
Bateman, *et al.*	1992	£76.74	$194.37	Annually	Preservation of Norfolk Broads
Siep & Strand	1992	200kr–1000kr	$59.37-296.89	Annually	Membership to Norwegian Ecology Club
Hanley & Craig	1991	£16.60	$43.62	Annually	Preventing local deforestation

Notes: 2017 dollar values obtained by (1) calculating original currency inflation then (2) converting into dollar values using exchange rates as of 29 June 2017. Inflation calculations made using publication year, unless authors present their results in prior year's values; *e.g.*, Bateman, *et al.* (1992) express their results in 1990 values, therefore 1990 values are used for inflation calculations.

Figure A1: Timing of Search Trends for Pipelines in America



Notes: Google search trends for keyword "pipeline" in the year 2016. Shaded areas represent timing of survey. Increased interest in pipelines could indicate increased media coverage of controversial environmental activities, which may have contributed to the larger WTP values found in our analysis relative to existing literature. Notice that Phase 1 occurred during a low-interest period, so comparison of Phase 2 results with Phase 1 results will determine if this event skewed our full-sample results.

Source: Google Trends (www.google.com/trends)

LIST OF REFERENCES

List of References, Chapter 1

Angrist, J. D., & Pischke, J. S. (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.

American Bar Association (2015). *National Task Force on Stand Your Ground Laws, Final Report and Recommendations*. Accessed on 10 September 2017 from americanbar.org/content/dam/aba/images/abanews/SYG_Report_Book.pdf.

Ashenfelter, O. (1978). Estimating the effect of training programs on earnings. *The Review of Economics and Statistics*, 47-57.

Ashenfelter, O., & Card, D. (1985). Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs. *The Review of Economics and Statistics*, 67(4), 648-60.

Bargh, J. A., Chaiken, S., Govender, R., & Pratto, F. (1992). The generality of the automatic attitude activation effect. *Journal of Personality and Social Psychology*, 62(6), 893.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach. *Journal of Political Economy*, 76(2), 169-217.

Beretta (2014). Ten Essential Tips for CCW Holders: The Ultimate Guide to CCW Carry. *Beretta USA Corp.*, 14-15.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1), 249-275.

Boots, D. P., Bihari, J., & Elliott, E. (2009). The state of the castle an overview of recent trends in state castle doctrine legislation and public policy. *Criminal Justice Review*, 34(4), 515-535.

Boyd, J. (1970). Nixon's southern strategy: It's all in the charts. *New York Times*, 17, B23.

Burbidge, J. B., Magee, L., & Robb, A. L. (1988). Alternative transformations to handle extreme values of the dependent variable. *Journal of the American Statistical Association*, 83(401), 123-127.

Cameron, A., & Trivedi, P. (2012). Microeconometrics Using Stata. *Stata Press*, Page 563.

Card, D., & Krueger, A. B. (1993). Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania. *American Economic Review*, 84, 772-793.

Card, D., & DellaVigna, S. (2013). Nine facts about top journals in economics. *Journal of Economic Literature*, 51(1), 144-161.

Catalfamo, C. (2006). Stand your ground: Florida's castle doctrine for the twenty-first century. *Rutgers Journal of Law & Public Policy*, 4(3), 504-545.

Cole, D. (1999). No equal justice: Race and class in the American criminal justice system. *The New Press*.

Conley, B. C. (1976). The value of human life in the demand for safety. *The American Economic Review*, 66(1), 45-55.

Cheng, C., & Hoekstra, M. (2013). Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine. *Journal of Human Resources*, 48 (3), 821-854

Dovidio, J. F., Kawakami, K., & Gaertner, S. L. (2002). Implicit and explicit prejudice and interracial interaction. *Journal of Personality and Social Psychology*, 82(1), 62.

Friedline, T., Masa, R., & Chowa, G. (2015). Transforming wealth: Using the inverse hyperbolic sine (IHS) and splines to predict youth's math achievement. *Social Science Research*, 49, 264-287

Glomm, G., & Ravikumar, B. (1992). Public versus private investment in human capital: endogenous growth and income inequality. *Journal of Political Economy*, 100(4), 818-834.

Greenwald, A. G., & Banaji, M. R. (1995). Implicit social cognition: attitudes, self-esteem, and stereotypes. *Psychological Review*, 102(1), 4.

Johnson, N.L. (1949). Systems of frequency curves generated by methods of translation. *Biometrika*, 36: 149-176

Lave, T. R. (2012). Shoot to Kill: A Critical Look at Stand Your Ground Laws. *University of Miami Law Review*, 67(4), 827-860.

Lawson, T. F. (2012). A Fresh Cut in an Old Wound-A Critical Analysis of the Trayvon Martin Killing: The Public Outcry, the Prosecutors' Discretion, and the Stand Your Ground Law. *University of Florida Journal of Law & Public Policy*, 23(3), 271-310.

Lee, J. S., Lee, J., & Hoover, L. T. (2017). What Conditions Affect Police Response Time? Examining Situational and Neighborhood Factors. *Police Quarterly*, 20(1), 61-80.

Levin, B. (2010). A Defensible Defense: Reexamining Castle Doctrine Statutes. *Harvard Journal on Legislation*, 47, 523-553.

MacKinnon, J. G., & Magee, L. (1990). Transforming the dependent variable in regression models. *International Economic Review*, 315-339.

McClellan, C., & Tekin, E. (2016). Stand your ground laws, homicides, and injuries. *Journal of Human Resources*, 0613-5723R2.

Megale, E. B. (2010). Making Murder Legal: How Laws Expanding Self-Defense Allow Criminals to "Get Away with Murder." works.bepress.com/elizabeth_megale/1

Mora, R., & Iliana, R. (2012). Treatment Effect Identification Using Alternative Parallel Assumptions. *Universidad Carlos III de Madrid, Departamento de Economa*.

Mustard, D. B. (2001). Racial, ethnic, and gender disparities in sentencing: Evidence from the US federal courts. *The Journal of Law and Economics*, 44(1), 285-314.

Pence, K. M. (2006). The role of wealth transformations: An application to estimating the effect of tax incentives on saving. *The BE Journal of Economic Analysis & Policy*, 5(1).

Pischke, J. S. (2005). Empirical Methods in Applied Economics Lecture Notes. Retrieved 14 September 2017 from econ.lse.ac.uk/staff/spischke/ec524/evaluation3.pdf

Rice, D. P., & Cooper, B. S. (1967). The economic value of human life. *American Journal of Public Health and the Nations Health*, 57(11), 1954-1966.

Roman, J. (2013). Race, justifiable homicide, and stand your ground laws: Analysis of FBI supplementary homicide report data. *The Urban Institute*.

Ross, L. P. (2007). Transmogrification of Self-Defense by national Rifle Association-Inspired Statutes: From the Doctrine of Retreat to the Right to Stand Your Ground. *Southern University Law Review*, 35(1), 1-47.

Schultz, T. W. (1961). Investment in human capital. *The American Economic Review*, 51(1), 1-17.

Solon, G., Haider, S. J., & Wooldridge, J. M. (2015). What are we weighting for? *Journal of Human Resources*, 50(2), 301-316.

Wiersema, B., Loftin, C., & McDowall, D. (2000). A comparison of supplementary homicide reports and national vital statistics system homicide estimates for US counties. *Homicide Studies*, 4(4), 317-340.

List of References, Chapter 2

Almond, D., Chay, K., & Lee, D. (2005). The Costs Of Low Birth Weight. *Quarterly Journal of Economics*, 1031-1083.

Almond, D., & Currie, J. (2011). Killing Me Softly: The Fetal Origins Hypothesis. *Journal of Economic Perspectives*, 153-172.

Ashenfelter, O. (1978). Estimating the effect of training programs on earnings. *The Review of Economics and Statistics*, 47-57.

Ashenfelter, O., & Card, D. (1985). Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs. *The Review of Economics and Statistics*, 67(4), 648-60.

Ayres, I., & Donohue III, J. J. (2002). Shooting down the more guns, less crime hypothesis (No. w9336). *National Bureau of Economic Research.*

Barker, D. (1995). Fetal Origins Of Coronary Heart Disease. *British Medical Journal*, 171-174

Behrman, J., & Rosenzweig, M. (2004, May). Returns to Birthweight. *The Review of Economics and Statistics*, 506-601.

Berman, E., Shapiro, J., & Felter, J. (2011). Can Hearts and Minds Be Bought? The Economics of Counterinsurgency in Iraq. *Journal of Political Economy*, 119(4), 766-819.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates?. *The Quarterly Journal of Economics*, 119(1), 249-275.

Black, S., Devereux, P., & Salvanes, K. (2007). From the Cradle to the Labor Market? The Effect of Birth Weight on Adult Outcomes. *Quarterly Journal of Economics*, 409-439.

Boice, J., Mumma, M., & Blot, W. (2010, November). Cancer Incidence and Mortality in Populations Living Near Uranium Milling and Mining Operations in Grants, New Mexico, 19502004. *Radiation Research*, 624-636.

Brown, P. (2003, April 25). Gulf troops face tests for cancer. *The Guardian*.

Brugge, D., deLemors, J., & Bui, C. (2007). The Sequoyah Corporation Fuels Release and the Church Rock Spill: Unpublicized Nuclear Releases in American Indian Communities. *American Journal of Public Health*, 1595-1600.

Busby, C., Hamdan, M., & Ariabi, E. (2010). Cancer, Infant Mortality and Birth Sex-Ratio in Fallujah, Iraq 2005-2009. *International Journal of Environmental Research and Public Health*, 2828-2837.

Canu, I., Garsi, J.-P., Caer-Lorho, S., Jacob, S., Collomb, P., Acker, A., & Laurier, D. (2012). Does uranium induce circulatory diseases? First results from a French cohort of uranium workers. *Occupational Environmental Medicine*, 404-409.

Card, D., & Krueger, A. B. (1994). Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania. American Economic Review, 84, 772-793.

Case, A., Deaton, A. (2005). Broken Down by Work and Sex: How Our Health Declines. *Analyses in the Economics of Aging*, 185-212.

Catalano, R., Bruckner, T., Hartig, T., & Ong, M. (2005). Population stress and the Swedish sex ratio. *Paediatric and Perinatal Epidemiology*, 19(6), 413-420.

Catalano, R., Bruckner, T., & Smith, K. R. (2008). Ambient temperature predicts sex ratios and male longevity. *Proceedings of the National Academy of Sciences*, 105(6), 2244-2247.

Environmental Protection Agency. (1983). The Church Rock Uranium Mill Tailings Spill: A Health and Environmental Assessment. *Environmental Protection Agency*.

Environmental Protection Agency. (2012a). *Iodine*. Retrieved from Radiation Protection: www.epa.gov/radiation/radionuclides/iodine

Environmental Protection Agency. (2012b). *Plutonium*. Retrieved from Radiation Protection: www.epa.gov/radiation/radionuclides/plutonium

Environmental Protection Agency. (2012c). *Uranium*. Retrieved from Radiation Protection: www.epa.gov/radiation/radionuclides/uranium

Environmental Protection Agency. (2015, February). *Radiation From Solar Activity*. Retrieved from RadTown USA: www.epa.gov/radtown/solar-activity.html%what-you-can-do

Goertzel, G (1954). Minimum Critical Mass and Flat Flux. *Journal of Nuclear Energy*, 2(4), 193-201

Gupta, A. (2001). Lethal Dust: Effects of Depleted Uranium Ammunition. *Economic and Political Weekly*, 454-456

Johansen, B. (1997). The High Cost of Uranium in Navajoland. *Akwesasne Notes* 2(2), 10-12.

Johnson, A. (1977). Behavior of Spent Nuclear Fuel in Water Pool Storage. *Energy Research and Development Administration*. Battelle Pacific Northwest Laboratories.

Jukic, A. M., Baird, D. D., Weinberg, C. R., McConnaughey, D. R., & Wilcox, A. J. (2013). Length of human pregnancy and contributors to its natural variation. *Human Reproduction*, 28(10), 2848-2855.

Kaestner, R. & Chan Lee, W. (2005), The effect of welfare reform on prenatal care and birth weight. *Health Economics*, 14: 497511. doi:10.1002/hec.921

Lucas, A., Fewtrell, M., & Cole, T. (1999, July). Fetal Origins of Adult Disease: The Hypothesis Revealed. *British Medical Journal*, 245-249.

LVS. (1998, November 10). Water experts baffled by increase in Lake Mead radiation levels. *Las Vegas Sun*.

Lott, Jr, J. R., & Mustard, D. B. (1997). Crime, deterrence, and right-to-carry concealed handguns. *The Journal of Legal Studies*, 26(1), 1-68.

Miller, G., Colvin, A., Hutchison, P., Tait, H., Dempsey, S., Lewis, D., & Soutar, C. (2008). A normative study of levels of uranium in the urine of British Forces personnel. *Occupational and Environmental Medicine*, 398-403.

Mora, R., & Iliana, R. (2012). Treatment Effect Identification Using Alternative Parallel Assumptions. *Universidad Carlos III de Madrid, Departamento de Economa*.

National Center for Health Statistics. Natality, 1976-1982, National Center for Health Statistics, Hyattsville, Maryland.

Reber, S. J. (2005). Court-ordered desegregation successes and failures integrating American schools since Brown versus Board of Education. *Journal of Human resources*, 40(3), 559-590.

Ritz, B. (1999). Radiation Exposure and Cancer Mortality in Uranium Processing Workers. *Epidemiology*, 531-538.

Ruggles, S., Genadek, K., Goeken, R., Grover, J., & Sobek, M. (2015). Integrated Public Use Microdata Series: Version 6.0 [dataset]. Minneapolis, MN: University of Minnesota. http://doi.org/10.18128/D010.V6.0.

Sublette, C. (2007, July 7). The First Nuclear Weapons. Retrieved from Nuclear Weapon Archive: http://nuclearweaponarchive.org

Therapontos, C., Erskine, L, Gardner, E, Figg, W, Vargesson, N & Tabin, C. (2009). Thalidomide Induces Limb Defects by Preventing Angiogenic Outgrowth during Early Limb Formation. *Proceedings of the National Academy of Sciences of the United States of America*, 8573-8578.

United States Army (2007). Field Manual 3-24, Counterinsurgency Field Manual. Chicago: U. of Chicago Press.

United States Census Bureau (2014). *Arizona Household Income*. Retrieved from www.census.gov

United States Geological Survey (2014). *USGS Colorado River*. Retrieved from National Water Information Systems: www.waterdata.usgs.gov

United States Geological Survey. (2014). *USGS Little Colorado River*. Retrieved from National Water Information Systems: www.waterdata.usgs.gov

Wehby, G. L., Murray, J. C., Castilla, E. E., Lopez-Camelo, J. S. & Ohsfeldt, R. L. (2009), Quantile effects of prenatal care utilization on birth weight in Argentina. *Health Economics*, 18: 13071321. doi:10.1002/hec.1431

World Information Service on Energy. (2015, May 2). *Federal Agencies release second Five-Year Plan to address uranium contamination in the Navajo Nation.* Retrieved from WISE Uranium Project: http://www.wise-uranium.org/udusa.html#NAVAJO

Zecevic, B., Terzic, J., Catovic, A., & Serdarevic-Kadic, S. (2010, April) Dispersion of PGU-14 Ammunition During Air Strikes by Combat Aircrafts A-10 Near Urban Areas. *Conference: New Trends in Research of Energetic Materials*, 797-814

List of References, Chapter 3

Amigues, J. P., Boulato, C., Desaigues, B., Gauthier, C. & Keith, J. (2002). The benefts and costs of riparian analysis habitat preservation: a willingness to accept/willingness to pay contingent valuation approach. *Ecological Economics*, 43: 17-31.

Bateman, I. J., & Langford, I. H. (1997). Non-users' Willingness to Pay for a National Park: An Application and Critique of the Contingent Valuation Method. *Regional Studies*, 31(6): 571-582.

Bateman, I., & Willis, K. G. (2001).Valuing Environmental Preferences: Theory and Practice of the Contingent Valuation Method in the US, EU, and developing Countries. Oxford University Press.

Bateman, I. J., Willis, K. G., Garrod, G. D., Doktor, P., Langford, I., & Turner, R. K. (1992). Recreation and environmental preservation value of the Norfolk Broads: a contingent valuation study.*Report to the National Rivers Authority, Environmental Appraisal Group, University of East Anglia.*

Cameron, A. C., & Trivedi, P. K. (2005).*Microeconometrics: Methods and Applications*. Cambridge University Press.

Carson, R. T., Conaway, M. B., Hanemann, W. M., Krosnick, J. A., Mitchell, R. C., & Presser, S. (2013).*Valuing oil spill prevention: a case study of Californias central coast*(Vol. 5). Springer Science & Business Media.

Carson, R. T. (1997). Contingent valuation: theoretical advances and empirical tests since the NOAA panel.*American Journal of Agricultural Economics*,79(5), 1501-1507.

Carson, R. T., & Mitchell, R. C. (1993). The value of clean water: the public's willingness to pay for boatable, fishable, and swimmable quality water.*Water Resources Research*,29(7), 2445-2454.

Cawley, J. (2008). Contingent valuation analysis of willingness to pay to reduce childhood obesity.*Economics & Human Biology*,6(2), 281-292.

Clynes, T. (2016). How Can 6 Million Acres at Denali Still Not Be Enough? *National Geographic*.
www.nationalgeographic.com/magazine/2016/02/denali-national-park-alaska/

Davis, J. H. (2015). Mount McKinley Will Again Be Called Denali. *New York Times* A9.
www.nytimes.com/2015/08/31/us/mount-mckinley-will-be-renamed-denali.html

Diamond, P. (1996). Testing the internal consistency of contingent valuation surveys. *Journal of Environmental Economics and Management*, 30(3), 337-347.

Haab, T. C., Interis, M. G., Petrolia, D. R., & Whitehead, J. C. (2013). From hopeless to curious? Thoughts on Hausman's dubious to hopeless critique of contingent valuation. *Applied Economic Perspectives and Policy*, 35(4), 593-612.

Hadker, N., Sharma, S., David, A., & Muraleedharan, T. R. (1997). Willingness-to-pay for Borivli National Park: evidence from a contingent valuation. *Ecological Economics*, 21(2), 105-122.

Hanley, N., & Craig, S. (1991). Wilderness development decisions and the Krutilla-Fisher model: the case of Scotland's flow country. *Ecological Economics*, 4(2), 145-164.

Hausman, J. (2012). Contingent valuation: from dubious to hopeless. *Journal of Economic Perspectives*, 26(4), 43-56.

Hoehn, J. P. (1991). Valuing the multidimensional impacts of environmental policy: theory and methods. *American Journal of Agricultural Economics*, 73(2), 289-299.

Johnson, B. K., Whitehead, J. C., Mason, D. S., & Walker, G. J. (2012). Willingness to pay for downtown public goods generated by large, sports-anchored development projects: The CVM approach. *City, Culture and Society*, 3(3), 201-208.

Johnston, R. J., et al. (2017). Contemporary guidance for stated preference studies. *Journal of the Association of Environmental and Resource Economists*, 4(2), 319-405.

Kolenikov, S. (2014). Calibrating survey data using iterative proportional fitting (raking). *The Stata Journal*, 14(1), 22-59.

Scott Long, J. (1997). Regression models for categorical and limited dependent variables. *Advanced Quantitative Techniques in the Social Sciences*, 7.

Nash, R. (2014). *Wilderness and the American Mind*. Yale University Press.

Stefanski, S. F., & Shimshack, J. P. (2016). Valuing Marine Biodiversity in the Gulf of Mexico: Evidence from the Proposed Boundary Expansion of the Flower Garden Banks National Marine Sanctuary. *Marine Resource Economics*, 31(2), 211-232.

Viscusi, W. K., Huber, J., & Bell, J. (2012). Heterogeneity in values of morbidity risks from drinking water. *Environmental and Resource Economics*, 52(1), 23-48.

Whitehead, J. C., & Blomquist, G. C. (1991). Measuring contingent values for wetlands: effects of information about related environmental goods. *Water*

Resources Research, 27(10), 2523-2531.

Whitehead, J. C., Haab, T. C., & Huang, J. C. (1998). Part-whole bias in contingent valuation: Will scope effects be detected with inexpensive survey methods?. *Southern Economic Journal*, 160-168.

White, P. C. L., & Lovett, J. C. (1999). Public preferences and willingness-to-pay for nature conservation in the North York Moors National Park, UK. *Journal of Environmental Management*, 55(1), 1-13.

Willis, K. G., & Garrod, G. D. (1993). Valuing landscape: a contingent valuation approach. *Journal of Environmental Management*, 37(1), 1-22.