2018

Essays on Measuring Dynamic Community Responses to Environmental Events

Patrick Prendergast
University of Rhode Island, pprendergast@my.uri.edu

ESSAYS ON MEASURING DYNAMIC COMMUNITY RESPONSES TO ENVIRONMENTAL EVENTS

BY

PATRICK PRENDERGAST

A DISSERTATION SUBMITTED IN PARTIAL FULFILLMENT OF THE

REQUIREMENTS FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

IN

ENVIRONMENTAL AND NATURAL RESOURCE ECONOMICS

UNIVERSITY OF RHODE ISLAND

2018

DOCTOR OF PHILOSOPHY DISSERTATION

OF

PATRICK PRENDERGAST

APPROVED:

Dissertation Committee:

Major Professor Emi Uchida

Corey Lang

Judith Swift

Robert Thompson

Nasser H. Zawia

` DEAN OF THE GRADUATE SCHOOL

UNIVERSITY OF RHODE ISLAND

2018

ABSTRACT

One of the things that makes economics such an interesting social science sub-field to study is the analysis of unintended or unforeseen consequences. In order for policies to be efficient and equitable, it is important to first understand how endogenous policy decisions and exogenous events can affect communities either directly or indirectly. In this dissertation research, I examine how communities react to discrete environmental events over time. I do so in the context of tropical storm and hurricane activity in U.S. counties and media markets as well as land conservation spending decisions in Massachusetts and New Jersey municipalities. Using micro-level data on environmental events and behavior in difference-in-differences and dynamic regression discontinuity frameworks, I test whether: (1) hurricane strikes affect poverty levels in impacted counties, (2) tropical storms and hurricanes create a window of opportunity where the affected population is interested in taking action to mitigate against future costs, and (3) local municipal conservation actions cause crowd-in or crowd-out conservation behavior from the state and neighboring local governments. The use of micro-level data (at the sub-state level) allows for the possibility of rigorous treatment identification that hold important implications for policymakers in all three settings.

ACKNOWLEDGMENT

First and foremost, I wish to thank my advisor, Dr. Emi Uchida. I never would have decided to pursue a PhD if it was not for her encouragement, patience, and mentorship. Second, I thank Dr. Corey Lang and Dr. Shanna Pearson-Merkowitz. I can honestly say that I have become a much better researcher due to their willingness to impart (some of) their expertise and trust in me. I also want to thank my dissertation committee members Dr. Judith Swift and Dr. Robert Thompson who encouraged me to think a bit broader than my initial narrow scope.

I will always be grateful to Patrick Cavanagh, Kristina DiSanto, Carrie Gill, Clayton Michaud, Kyle Montanio, Andy Boslett, Nate Merrill, Chao “Zorro” Zhou, Matt Altonji, and David Ryder for humoring me as we endured the struggles of graduate school together.

Lastly, I thank my family and Chelsey for their love and support through the peaks and valleys of the past few years.

PREFACE

I use a manuscript format for this dissertation that contains three independent chapters which when combined constitute the entire work. The first two of the chapters are being prepared for publication and the third chapter is currently under review for publication. The goal of this dissertation is to measure how communities respond to discrete environmental events over time.

The first chapter is an examination of indirect effects of hurricane strikes in the United States. We measure how hurricane strikes affect the poverty rates of impacted counties and how long the effect lasts. We find that hurricanes dynamically decrease county level poverty rates through two possible mechanisms – an increase in business activity and a decrease in population.

The second chapter is an examination of community engagement and interest in reducing future damage costs after experiencing a strong storm. We measure how tropical storm and hurricane strikes affect relative internet search activity for *flood insurance* of the populations in impacted media markets and how long the effect lasts. We find that tropical storms and hurricanes dynamically increase relative search activity for *flood insurance*.

The third chapter is an examination of state and local government responses to changes in local conservation policy. We measure how local municipal conservation policies crowd-in or crowd-out neighboring municipalities' actions and state government conservation spending and how long the effect lasts. We find local municipal conservation decisions do not affect neighboring and municipality or state conservation activity.

The three chapters are followed by a conclusion chapter that summarizes all three manuscripts. The appendices provide supplemental analysis to the main chapters that are not intended for publication.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

MANUSCRIPT 1

The Effect of Hurricane Strikes on County Poverty Measurements in the United States

(To be submitted to Environmental and Resource Economics)

By

Patrick Prendergast [a] and Emi Uchida [a]

[a] *Department of Environmental and Natural Resource Economics, University of Rhode Island, Kingston, RI*

Abstract

The direct and indirect costs of hurricane strikes have been examined in prominent studies over the years, but little is known about how hurricanes affect poverty levels. This study uses a difference-in-differences approach to observe the dynamic within county variation of poverty levels from hurricane strikes in the United States. We find that hurricane strikes reduce overall county level poverty and the number of children in poverty until one and three years after a strike, respectively. To give context to this change, we also investigate dynamic changes in personal income, wages and salaries, total employment, and population. We observe an increase in per capita personal income and decreases in wages and salaries, total employment, and population. This implies the change in poverty levels after a hurricane may be due to a combination of labor and population dynamics. We also find families around the poverty line may be disproportionately more affected by hurricanes than other families.

1.1 INTRODUCTION

Natural disasters such as hurricanes, tornadoes, wildfires, droughts, and freezes cause enormous environmental and economic damage in the United States. Since 1980, there have been 212 weather events where the overall damages have reached or surpassed $1 billion. In 2017 alone, there were 15 weather and climate disaster events with 282 deaths and financial losses over $1 billion (NCEI, 2017). Despite the magnitude of these losses, we lack full understanding of the complete economic effects of natural disasters. Growing research has shown that a person's socioeconomic status often determines vulnerability to natural disaster impacts (Fothergill and Peek 2004). This link between income and natural disaster impact leads to such questions as – do natural disasters affect the poverty levels in the areas they occur? What happens to economic activities? How do natural disasters affect the population of areas they hit over time?

We address these questions in the context of hurricanes that strike the United States. We use a panel of coastal counties in states that border the Atlantic Ocean and Gulf of Mexico to identify the dynamic effect hurricane strikes have on poverty levels over a course of seven to eight years after a storm. To give context to our findings, we also investigate the effect hurricane strikes have on per capita personal income, per capita wages and salaries, per capita total employment, and population. We find evidence that hurricane strikes reduce overall county level poverty and the number of children in poverty up until one and three years after the initial strike, respectively. We also observe that hurricane strikes increase personal income per capita, and decrease wages and salaries per capita, total employment per capita, and the population in the counties that get hit. Finally, we test the robustness of our results and observe that families around the poverty line may be more vulnerable to hurricane strikes than families in other income groups.

The relationship between an area's income and poverty with the costs that are incurred from natural disasters is well established. Many studies explicitly control for a country's income level and in general find a negative relationship between income and natural disaster vulnerability, damages, and fatalities (Kahn 2005; Anbarci, Escaleras, and Register 2005; Toya and Skidmore 2007; Masozera, Bailey, and Kerchner 2007; Kellenberg and Mobarak 2008). Other studies note the importance of normalizing natural disaster damages by wealth when making comparisons over time (Pielke and Landsea 1998, Brooks and Doswell 2000, Pielke et al. 2003, Pielke et al. 2008). Others investigate the role income has in adaptation to natural disaster damages as countries develop (Nordhaus 2010, Hsiang and Narita 2012, Fankhauser and McDermott 2014, Bakkensen and Mendelsohn 2016). Most studies agree that richer areas are not as negatively impacted by natural disasters as poor areas.

While these studies are important for understanding the link between natural disasters and income, there are surprisingly few analyses that examine the direct effect natural disasters have on various poverty, income inequality, and human development measurements (Karim and Noy 2016). Studies that have done so either look at a single environmental event or measure poverty changes at a single point in time. These studies include the effect of El Niño on poverty at the household level in the Philippines (Datt and Hoogeveen 2003), the effect of experiencing a natural disaster on household poverty in Vietnam (Bui et al. 2014), the effect of Hurricane Mitch on people in poverty in the rural areas of Honduras (Morris et al. 2002), and the effect of a drought on poverty and income inequality at the household level in Burkina Faso (Reardon and Taylor 1996). In a study that looks at the dynamic effects of natural disasters, Yamamura (2015) uses a panel of countries to observe changes in inequality over time. Yamamura observes floods increase a country's Gini coefficient up until one year after the occurrence but does not find a dynamic effect for storms and earthquakes.

Very few studies investigate the dynamic effect natural disasters have on poverty measurements within a single country. One study that is most related to this study in that regard is Rodriguez-Oreggia et. al (2013). The authors look at the effect of natural disasters on human development and poverty indices over time at the municipal level in Mexico. Using a two-period panel dataset, the authors use a fixed-effects model to estimate the treatment effect of a municipality experiencing a natural disaster on a human development index and various poverty indices. They find the occurrence of a natural disaster between 2000-2005 reduce the human development index by about 1% and increases poverty between 1.5 and 3.7% depending on the measure.

Unfortunately, their dataset was not rich enough to disentangle the effects of individual natural disaster incidents instead of aggregate natural disaster treatment between 2000-2005. This means their treatment effect estimates could be influenced by events of different magnitudes or multiple natural disaster events hitting the same municipalities over their sample period. Their estimates might also not account for any recovery that happens after natural disasters that occur earlier in their sample. In this study, we overcome these limitations by observing effects on our dependent variables from individual storms, conditioned on past hits, while observing the dynamic effects over seven to eight years after the strike.

The main contribution of this study is to examine the dynamic effects multiple hurricanes have on poverty levels within a country over time. Other studies have looked at the dynamic effect hurricanes on employment and earnings (Belasen and Polachek 2009), government fiscal outcomes (Ouattara and Strobl 2013, Belasen and Dai 2014, Deryugina 2017), fertility (Evans, Hu, and Zhao 2010), economic and personal income growth (Strobl 2011, Hsiang and Jina 2014, Bakkensen and Barrage 2016), and of a single hurricane on economic impacts (Coffman and Noy 2011). However, to the best of our knowledge our study is the first to measure the effects of hurricanes on poverty levels over time within a single country. We follow in the footsteps of these studies by using a difference-in-differences approach that identifies the within-county variation of poverty levels and other outcome variables over time.

1.2 DATA

This section describes the data sources we used for the different parts of our analysis: 1) dependent variables at the county level and 2) hurricane wind model for identifying "treated counties." Data were collected for all coastal counties in the US states that border the Atlantic Ocean and Gulf of Mexico where dependent variables were compared between counties that were hit by a hurricane and those that were unaffected but are geographically close.[1]

1.2.1 County Level Dependent Variables

The main dependent variables in our analysis are county level poverty estimates from the Small Area Personal Income and Poverty Estimates (SAIPE). SAIPE reports

[1] The use of unaffected coastal counties in the 21 states that border the Atlantic Ocean and Gulf of Mexico as a control group is consistent with the literature. See Strobl (2011) as an example.

estimates of the amount of people in poverty of all ages, ages 0-17, ages 5-17, and under the age of five, as well as household median income. Data are reported at school district, county, and state levels for years 1989, 1993, and 1995-2015. We focus our analysis either on years 2005-2014 or 2006-2014 depending on the variable being analyzed.[2] Table 1.1 shows the summary statistics for the number of people of all ages in poverty (People in Poverty) and the number of school age children (ages 5-17) in poverty (Children in Poverty) for all the coastal counties in the sample. The average county had about 29,698 people in poverty and about 6,584 school age children in poverty. Poverty in coastal counties seem to occur at higher levels than the national averages for the same figures. Averages for all counties reported in the SAIPE dataset from 2005-2014 were about 14,000 people in poverty and 3,150 children in poverty.

To help give context to any observed changes to county poverty levels after a hurricane strike, we also investigate how hurricanes affect county level per capita personal income, wages and salaries per capita, total employment per capita, and population. We collected per capita personal income, wages and salaries, and total employment from the Bureau of Economic Analysis (BEA). From 2005-2014, average per capita personal income in coastal counties was about $40,500, average per capita wages and salaries was about $17,050, and average per capita total employment was about 0.5 (Table 1.1). Population data was collected from the National Cancer Institutes' Surveillance, Epidemiology, and End Results Program (SEER). From 2005-2014, the

[2] The US Census Bureau cautions against making year-to-year comparisons between certain years in the SAIPE dataset due to estimations based off different data sources (e.g. estimates based on the Census, Current Population Survey (CPS), or American Community Survey (ACS) surveys). Therefore, we limit our analysis to either years 2005-2014 or 2006-2014 depending on the dependent variable to reduce the probability identified variation in the data is coming from different survey methodologies.

average county had about 213,800 residents (Table 1.1). Again, the coastal counties in our sample have higher values than the national averages for per capita personal income ($37,200), per capita wages and salaries ($14,580), and population (99,220). Average per capita total employment is right in line with the national average of 0.52, however.

1.2.2 Hurricane Strike Treatment

Figure 1.1 shows the central storm tracks of the hurricanes included in our sample. We used the Regional and Mesoscale Meteorology Branch's (RAMMB) Extended Best Track Dataset to identify which US coastal counties were affected by hurricanes from 1998-2014. Studies that identify hurricane strikes at a sub-national level in the United States typically either only identify treatment based on the area directly around the storm's eye (e.g. Belasen and Polachek 2007), the radius of maximum wind (e.g. Deryugina 2017) or limits their analysis to hurricanes of category 3 or higher on the Saffir-Simpson scale (e.g. Strobl 2011). This could lead to some counties being misidentified as control units when they could have been affected by hurricanes further outside the radius of maximum wind or hurricane eye or by weaker hurricanes. Instead, we used the RAMMB's Extended Best Track Dataset to estimate a complete wind field to identify the treatment of counties by hurricanes during the sample period. [3]

The Extended Best Tracks Dataset reports the latitude, longitude, maximum wind intensity, and minimum central pressure of the center of tropical cyclones as well as information about the storm structure such as the maximum radial extent of 34, 50, and 64 knot winds in four quadrants at six-hour intervals from 1988-2015. We used the

---

[3] Available at: http://rammb.cira.colostate.edu/research/tropical_cyclones/tc_extended_best_track_dataset/. Accessed December 2016.

latitude and longitude coordinates of the center of the storm and the reported radial wind distances to approximate the path of a complete wind field through the life of a hurricane[4]. We assumed the storm track and wind quadrant radii are linear between consecutive points and interpolated the storm path and wind strength radii to half hour intervals. We then used ArcGIS to create a hurricane wind field by interpolating between central storm maximum wind measurements and the 34, 50, and 64 knot wind extents. As an illustration, Figure 1.2 shows the complete wind field for the 2005 storm Katrina. We then determined the maximum wind speed each county was exposed to in a given year during our sample and qualified hurricane treatment as a county that was estimated to experience a hurricane strength of at least 64 knots. With our simulated wind fields, we are also able to identify coastal counties surrounding those that are affected by hurricane strength intensity that experience tropical storm strength intensity.[5]

1.3 METHODOLOGY

To estimate the impact hurricane strikes have on poverty levels in coastal counties, we use a difference-in-differences framework that compares the outcome variables of interest between treatment counties that get hit by a hurricane and unaffected coastal control counties over time. We observe yearly changes in our dependent variables either up to seven or eight years after a hurricane strike due to the dependent variable data restrictions mentioned in Section 1.2.

[4] Any missing values for the radial extent of the 34, 50, and 64 knot winds were interpolated based from the minimum central pressure of the storm.
[5] Storm classification is based off the Saffir-Simpson scale which considers tropical storms wind speeds to be 34-63 knots and hurricane wind speeds to be above 64 knots.

While observing changes in poverty levels over time, we assume conditional convergence where counties with initially high levels of poverty will decrease faster over time than those with lower levels of poverty. Studies show that there is conditional convergence in global poverty levels (Cuaresma, Klasen, and Wacker 2016) and in personal income among counties within the United States (Higgins, Levy, and Young 2006; James, Harrison, and Campbell 2013). In the context of the effects of hurricanes, we follow Strobl (2011) and use a conditional convergence growth equation for our econometric model. Our model takes the following form:

$$Growth\left(Y_{i,(t-1)\to t}\right) = \sum_{t=0}^{T}[\beta_t \times \log\left(Hurricane\ Max\ Wind_{i,(t-T)}\right)] + \sum_{t=0}^{T}\left[\rho_t \times \log\left(Tropical\ Storm\ Max\ Wind_{i,(t-T)}\right)\right] + \gamma Y_{i,(t-1)} + \alpha_i + \delta_t + \theta_i \times t + \epsilon_{i,t} \qquad (1)$$

where $Growth\left(Y_{i,(t-1)\to t}\right)$ is the growth rate of the dependent variable for county i from time $(t-1)$ to time t, $Hurricane\ Max\ Wind_{i,(t-T)}$ is the maximum wind speed of a hurricane county i experienced in a $t-T$ window, $Tropical\ Storm\ Max\ Wind_{i,(t-T)}$ is the maximum tropical storm wind speed county i experienced in a $t-T$ window if they did not experience hurricane intensity, α_i is a county fixed effect, δ_t is a time fixed effect, and $\theta_i \times t$ is a county-specific time trend. [6], [7] We perform a log(x + 1) transformation on $Hurricane\ Max\ Wind_{i,(t-T)}$ and $Tropical\ Storm\ Max\ Wind_{i,(t-T)}$ in order to preserve the "zero" values of the control coastal counties that neither

[6] $Growth(Y_{i,(t-1)\to t})$ was calculated as the difference between $\log(Y_{i,t})$ and $\log(Y_{i,(t-1)})$.
[7] $Y_{i,(t-1)}$ is in logged terms.

experience hurricane nor tropical storm intensity winds from the hurricanes in our sample.

The parameters of interest are the coefficients β_t which show the marginal effect of maximum wind of a hurricane on the growth of our dependent variables in a $t - T$ window, conditioned on past hurricane strikes. Though they are of secondary importance to β_t, the parameters ρ_t $(t = 0, \dots, T)$ serve two purposes in our analysis. First, they allow us to examine and variation in the data between the unaffected control counties and counties that are further away from the hurricane intensity but are still affected by the tropical storm intensity parts of the hurricanes in our dataset. These coastal counties may not sustain the same wind damage of the counties that experience hurricane strength winds, but they still are at risk of flooding which could affect the outcome variables. Second, counties affected by the tropical storm intensity portion of hurricanes surround the counties that are affected by hurricane intensity. Allowing these counties to have their own slope gives us the opportunity to examine if counties that surround the ones affected by hurricane intensity experience upticks in population or business activities to compensate for those lost in the hurricane stricken coastal counties.

We take careful considerations in our analysis to control for issues that can arise from correlations between geographically close locations and using the same treatment and data to test multiple hypotheses. We use an Ordinary Least Squares estimator and correct the standard errors for spatial and time correlation (Conley 1999, Hsiang 2010). We follow Deryugina (2017) and allow for serial correlation of up to 5 years and spatial correlation between counties of up to 200 km. We also adjust the p-values of our coefficient estimates to reduce the probability of false rejection of null hypotheses across a family of dependent (our outcome variables) and independent variables (the lags associated with hurricane strikes) (Veazie 2006).[8] We adjust the p-values using a free step-down resampling method as outlined in Anderson (2008).[9]

1.4 RESULTS

We first use Equation (1) to investigate the effect of hurricane strikes on poverty levels for coastal counties in the United States. We also investigate the effect hurricane strikes have on per capita personal income, per capita wages and salaries, per capita total employment, and population. We then check the robustness of our results by expanding the sample of counties to all counties in the states that border the Atlantic Ocean and Gulf of Mexico and investigate how hurricane strikes affect family income distributions in counties that take part in the American Community Survey (ACS).

1.4.1 Changes in Poverty After a Hurricane Strike

We find that hurricane strikes dynamically affect county poverty levels in the United States (Table 1.2 and Figure 1.3). Hurricane strikes have dynamic effects on the number of people in poverty (Column 1) and the number of children in poverty (Column 2). Both measures of poverty show patterns of initial decreases in the first few years after a hurricane strike with increases later. Statistically significant coefficient estimates range from -0.0102 to 0.0059 for the number of people in poverty and -0.011 to 0.0071 for children in poverty. The interpretation of coefficient estimates from a hurricane that

[8] For the purposes of p-value adjustments, we define a family of hypotheses as the matrix of hurricane strike lags and dependent variables they are used to explain. So, hurricane strike estimates were adjusted for 53 hypothesis tests (five dependent variables with nine lags of hurricane strikes plus eight lags for overall poverty). Tropical storm strength strikes are considered their own family of hypotheses and are also adjusted for 53 hypothesis tests.

[9] Stata code for p-value adjustment available at https://are.berkeley.edu/~mlanderson/ARE_Website/Research.html

struck a county a year ago is that with a 1% increase in hurricane maximum wind intensity there is expected to be a 1.02% decrease in the number of people in poverty and a 1.03% decrease in the number of children in poverty.

While these point estimates seem small, applying the effects to dependent variable mean values and assuming stronger storms quickly shows that these estimates are economically meaningful. Applied to the mean values presented in Table 1.1, a 1% increase in the maximum wind of an average hurricane of 73.56 knots that struck a typical county one year ago translates to a reduction of about 300 people in poverty and a reduction of about 68 children in poverty. Increasing the maximum wind of a hurricane by one standard deviation to 83.88 knots (an increase of about 13% from the logged values) would reduce the amount of people in poverty by about 3,900 people and the number of children in poverty by about 885 children.

Counties that miss getting hit by hurricane intensity winds but still get hit by the tropical storm strength winds further away from the eye of the storm also experience changes in poverty levels (Table 1.2 and Figure 1.3). These surrounding coastal counties do not experience the same initial dip in overall poverty as the counties that got hit by hurricane intensity, but they do experience similar increases in poverty levels that hurricane-stricken counties do five and six years after. Coastal counties also seem to experience an initial dip in the number of children in poverty after experiencing the tropical storm intensity winds of a hurricane.

The initial decreases in poverty levels in coastal counties that are affected by hurricanes is a new finding in the literature. Most studies that investigate the effect natural shocks have on poverty and inequality find they decrease human development

(Rodriguez-Oreggia et al. 2013), increase poverty (Datt and Hoogeveen 2003), and increase inequality (Reardon and Taylor 1996, Bui et al. 2014, Yamamura 2015) in the areas they occur. To investigate why we observe different poverty outcomes than what is typical in the literature, we observe the effect hurricanes have on other outcome variables.

1.4.2 Changes in Personal Income after a Hurricane Strike

We find that hurricane strikes affect per capita personal income, wages and salaries, and total employment (Table 1.2 and Figure 1.3). Per capita personal income (Column 3) increases immediately after a hurricane hits and lasts for two years afterwards before a noticeable decrease that starts around four years after the hurricane strike. Estimates range from an increase of 0.31% and 0.5% during the first two years after a hurricane hits to decreases between 0.16% and 0.21% between four and eight years after a hurricane strike for a 1% increase in maximum wind. Applied to the mean value of $40,447 per capita and increasing the maximum wind speed of a hurricane by one standard deviation would result in annual personal income increases between $1,625-$2,625 per capita and then decreases between $845-$1,105 per capita between four and eight years after the strike of a coastal county. Personal income per capita also changes in the surrounding counties that are affected by the tropical storm intensity portions of hurricanes. In these areas, personal income per capita raises at a smaller magnitude than the hurricane-stricken counties, but growth stays positive for longer until turning negative in the eighth year after a strike.

This initial increase in personal income per capita of counties that are affected by a hurricane before a later decrease is also a new finding in the literature. In analyzing the effect of only strong hurricanes on personal income in coastal counties in the United States from 1970-2005, Strobl (2011) finds evidence of an initial decrease in personal income per capita with no lasting effect up to five years after the strike. Differences between our results and that of Strobl may be due to different time periods of our datasets (our dependent variable measurements start where his ends), the fact that we included all hurricanes instead of just those of category 3 or higher, or that we use more recently updated BEA data.[10] In contrast, Coffman and Noy (2011) find that Hurricane Iniki decreased total personal income but did not seem to affect per capita personal income in Kauai County in Hawaii.

1.4.3 Changes in Salaries and Wages and Total Employment after a Hurricane Strike

Per capita wages and salaries (Column 4) and per capita total employment (Column 5) follow a similar trend as personal income per capita by initially increasing after a hurricane strike before decreasing over time. Applied to the means of wages and salaries per capita and total employment per capita from Table 1.1, a one standard deviation increase in the maximum wind speed of a hurricane would increase wages and salaries by about $730 per capita the year of the strike before annual decreases between $440-$1,175 per capita from two to eight years after. Also, total employment per capita would increase between 0.014 and 0.017 jobs per capita up to one year after a hurricane strike before annual decreases between 0.006 and 0.012 jobs per capita between three and eight years after a hurricane strike. Surrounding coastal counties that experience tropical

[10] In 2014, BEA revised it's estimates for personal income data due to methodological improvements. Information can be found at: https://www.bea.gov/scb/pdf/2015/12%20December/1215_local_area_personal_income.pdf

storm intensity winds also experience initial increases in wages and salaries and total employment, though at a lower magnitude, before experiencing decreases later.

The gradual decrease in employment and wages in areas directly hit by hurricanes is consistent with other findings in the literature. Coffman and Noy (2011) find Hurricane Iniki decreased private sector employment in the county that was affected. Belasen and Polachek (2009) find that Florida counties that are directly hit by a hurricane experience an immediate growth in earnings and employment before a downturn. Unlike Belasen and Polachek, we find surrounding counties that experience tropical storm intensity of the hurricanes follow the same general pattern as the counties hit by hurricanes, but at a smaller magnitude.

1.4.4 Changes in Population after a Hurricane Strike

We also find that hurricane strikes affect county level population (Table 1.2 and Figure 1.3). Population (Column 6) decreases in coastal counties over time after a hurricane strike. Statistically significant coefficient estimates show annual decreases that range from .08% to 0.12% within the eight-year lags we test. With a one standard deviation increase in max wind speed, this translates to annual decreases in population between 2,225-3,335 people for a typical coastal county. Coffman and Noy (2011) also find evidence of a dynamic decrease in county population after a hurricane strike.

Interestingly, the surrounding coastal counties that experience tropical storm intensity winds show statistically significant increases in population three to seven years after a hurricane strike. These increases in population for the surrounding coastal counties coincide with decreases in employment per capita and wages and salaries per capita and occur after the population decrease in hurricane affected counties which suggests there may be a labor shock in the surrounding counties that are driving down employment and wages per capita.

1.4.3 Robustness of Results

We test the robustness of our findings by examining if results are sensitive to changes in the county control group and if analyzing additional data results in the same conclusions.[11] First, we expand the number of counties that are in the control group. Up until this point, coastal counties that border the Atlantic Ocean and Gulf of Mexico have been analyzed where changes in outcome variables over time have been compared between coastal counties that experience hurricanes (or tropical storm strength intensity of those hurricanes) and coastal counties that were not affected by hurricanes in our sample period. We re-estimate Equation (1) with the inclusion of all the counties in the 21 states that border the Atlantic Ocean and Gulf of Mexico (Table 1.3 and Figure 1.4).[12,13]

Results show nearly identical post storm trends as those reported in Table 1.2 and Figure 1.3 for coastal counties. The main differences between the two sets of results are that the model fits slightly decrease, and some coefficient estimates lose significance when the control group is expanded. Our main conclusions hold, however. Hurricanes still appear to decrease overall poverty up to one year after the strike, the number of

[11] The p-values of the robustness check results are also adjusted for multiple hypothesis testing but are considered separate families of hypotheses than our main results because they are performed on different samples of counties (e.g. all counties in states that boarder the Atlantic Ocean and Gulf of Mexico or counties that take part in the American Community Survey vs. just coastal counties).

[12] Using all the counties that are unaffected by hurricanes in the states that border the Atlantic Ocean and Gulf of Mexico that as a control group is consistent with the literature. See Deryugina (2017) as an example.

[13] These states are Alabama, Connecticut, Delaware, Florida, Georgia, Louisiana, Massachusetts, Maine, Maryland, Mississippi, New Hampshire, New Jersey, New York, North Carolina, Pennsylvania, Rhode Island, South Carolina, Texas, Vermont, Virginia, and West Virginia.

children in poverty decreases over the first three years after, personal income per capita increases before decreasing later, wages and salaries decrease over time, total employment per capita increases at first, and population decreases over time. Statistical significance in the surrounding counties that are affected by tropical storm intensity portions of hurricanes is a little sparser than our previous estimates, but clear patterns of a reduction in the number of children in poverty and an increase in population in the years following the storm remain.

Second, we test the robustness of our main poverty results on additional data. Of the two main poverty outcome variables we analyze, the reduction of the number of children in poverty that results from hurricane strikes both lasts longer than changes in overall poverty and is more robust to control group changes. This suggests families around the poverty line may be a group of people that are consistently affected by hurricanes. To further test if families around the poverty line are affected by hurricanes more than families with more income, we re-estimate Equation (1) with data on family income distribution from the American Community Survey (ACS). The ACS provides estimates of the number of families in specified income bins for a sample of counties in each state from 2007-2013.[14] We estimate how hurricanes affect the number of families in these income bins for coastal counties that take part in the ACS (Table 1.4) and all ACS counties in coastal states (Table 1.5). What becomes immediately clear from comparing results in these two tables is the robustness of the decrease of number of families in the $15,000-$24,999 income bin. According to the U.S. Department of Health and Human Services, families in this income bin were considered below the poverty line

[14] We use the 3-year estimates from the ACS because 5-year estimates are not available before 2010.

in 2013 if they had at least three people in the family.[15] This reduction of families in an income bin that would be considered below the poverty line after a hurricane strike is consistent with a reduction of children in poverty and an initial increase in personal income per capita we observed in our main results.

1.5 CONCLUSION

From Katrina in 2005 to Harvey and Irma in 2017, hurricanes have caused large destruction in the United States. Often the severity of a hurricane strike is determined by the direct costs such as fatalities and capital damages. Indirect costs such as economic growth, government finances, and employment in the aftermath of hurricane strikes have been studied in the literature, but only a few examine the effect on poverty. We contribute to the literature by analyzing the dynamic effects hurricanes have on poverty within coastal counties in the United States, an analysis that has not been done before. We use a difference-in-differences model, while correcting standard errors for spatial and time correlation, to observe within county variation in poverty levels after a hurricane strike over time.

We find hurricane strikes cause statistically significant changes to poverty at the county level. Results suggest overall county level poverty decreases between 0.5 to 1% for each year from the hurricane strike until two years after and the poverty levels of children between the ages of 5 and 17 decrease between 0.6 and 1.1% for each year from the hurricane strike until three years after. We also show there is somewhat of a "rebound

[15] Information available at: https://aspe.hhs.gov/2013-poverty-guidelines

effect" later on where the number of people in poverty and children in poverty increase starting five years after a hurricane hit.

Drawing any broad conclusions from analysis using aggregated data inevitably requires a certain level of ecological inference. With hurricanes also affecting income per capita, wages and salaries per capita, total employment per capita, and population, it is not entirely clear if county poverty levels decrease at first due to spurring business activity involved with initial cleanup or by displacing people that are poor to other counties. Our robustness analysis on how hurricanes affect the income distribution of families in counties that are hit suggests it is the latter – perhaps there is a reduction in the number of people and children in poverty because families around the poverty line face more incentive to leave damaged areas to seek employment elsewhere.

This analysis does not propose any recommendations on how to mitigate indirect costs of hurricanes in the United States. It does, however, provide evidence that hurricanes can affect areas in which they occur differently than what would be expected and what the literature suggests by dynamically decreasing poverty levels. Further research can focus on individual data to investigate if decreases in county poverty levels and increases in personal income are primarily being driven by the outmigration of people in poverty after a hurricane strike. Future research could also focus on families around the poverty line and investigate if they are, indeed, more vulnerable to natural shocks than other families or single people in poverty.

REFERENCES

Anderson, Michael L. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American statistical Association*103.484 (2008): 1481-1495.

Anbarci, Nejat, Monica Escaleras, and Charles A. Register. "Earthquake fatalities: the interaction of nature and political economy." *Journal of Public Economics* 89.9 (2005): 1907-1933.

Bakkensen, Laura, and Lint Barragey. *Do disasters affect growth? A macro model-based perspective on the empirical debate*. No. 2016-9. Working Paper, Brown University, Department of Economics, 2016.

Bakkensen, Laura A., and Robert O. Mendelsohn. "Risk and adaptation: evidence from global hurricane damages and fatalities." *Journal of the Association of Environmental and Resource Economists* 3.3 (2016): 555-587.

Belasen, Ariel R., and Chifeng Dai. "When oceans attack: assessing the impact of hurricanes on localized taxable sales." *The Annals of Regional Science* 52.2 (2014): 325-342.

Belasen, Ariel R., and Solomon W. Polachek. "How disasters affect local labor markets the effects of hurricanes in Florida." *Journal of Human Resources* 44.1 (2009): 251-276.

Bui, Anh Tuan, et al. "The impact of natural disasters on household income, expenditure, poverty and inequality: evidence from Vietnam." *Applied Economics* 46.15 (2014): 1751-1766.

Brooks, Harold E., and Charles A. Doswell III. "Normalized damage from major tornadoes in the United States: 1890–1999." *Weather and Forecasting* 16.1 (2001): 168-176.

Coffman, Makena, and Ilan Noy. "Hurricane Iniki: measuring the long-term economic impact of a natural disaster using synthetic control." *Environment and Development Economics*17.2 (2012): 187-205.

Conley, Timothy G. "GMM estimation with cross sectional dependence." *Journal of econometrics* 92.1 (1999): 1-45.

Crespo Cuaresma, Jesus, Stephan Klasen, and Konstantin M. Wacker. "There is poverty convergence." (2016).

Datt, Gaurav, and Hans Hoogeveen. "El Niño or El Peso? Crisis, poverty and income distribution in the Philippines." *World Development* 31.7 (2003): 1103-1124.

Deryugina, Tatyana. 2017. "The Fiscal Cost of Hurricanes: Disaster Aid versus Social Insurance." *American Economic Journal: Economic Policy*, 9(3): 168-98.

Evans, Richard W., Yingyao Hu, and Zhong Zhao. "The fertility effect of catastrophe: US hurricane births." *Journal of Population Economics* 23.1 (2010): 1-36.

Fankhauser, Samuel, and Thomas KJ McDermott. "Understanding the adaptation deficit: why are poor countries more vulnerable to climate events than rich countries?." *Global Environmental Change* 27 (2014): 9-18.

Fothergill, Alice, and Lori A. Peek. "Poverty and disasters in the United States: A review of recent sociological findings." *Natural hazards* 32.1 (2004): 89-110.

Higgins, Matthew J., Daniel Levy, and Andrew T. Young. "Growth and convergence across the United States: evidence from county-level data." *The Review of Economics and Statistics* 88.4 (2006): 671-681.

Hsiang, Solomon M. "Temperatures and cyclones strongly associated with economic production in the Caribbean and Central America." *Proceedings of the National Academy of sciences* 107.35 (2010): 15367-15372.

Hsiang, Solomon M., and Amir S. Jina. *The causal effect of environmental catastrophe on long-run economic growth: Evidence from 6,700 cyclones*. No. w20352. National Bureau of Economic Research, 2014.

Hsiang, Solomon M., and Daiju Narita. "Adaptation to cyclone risk: Evidence from the global cross-section." *Climate Change Economics* 3.02 (2012): 1250011.

Karim, Azreen, and Ilan Noy. "Poverty and Natural Disasters—A Qualitative Survey of The Empirical Literature." *The Singapore Economic Review* 61.01 (2016): 1640001.

Kahn, Matthew E. "The death toll from natural disasters: the role of income, geography, and institutions." *The Review of Economics and Statistics* 87.2 (2005): 271-284.

Kellenberg, Derek K., and Ahmed Mushfiq Mobarak. "Does rising income increase or decrease damage risk from natural disasters?" *Journal of Urban Economics* 63.3 (2008): 788-802.

Masozera, Michel, Melissa Bailey, and Charles Kerchner. "Distribution of impacts of natural disasters across income groups: A case study of New Orleans." *Ecological Economics* 63.2 (2007): 299-306.

Morris, Saul S., et al. "Hurricane Mitch and the livelihoods of the rural poor in Honduras." *World development* 30.1 (2002): 49-60.

Nordhaus, William D. "The economics of hurricanes and implications of global warming." *Climate Change Economics*1.01 (2010): 1-20.

NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters (2017). https://www.ncdc.noaa.gov/billions/.

Ouattara, Bazoumana, and Eric Strobl. "The fiscal implications of hurricane strikes in the Caribbean." *Ecological economics*85 (2013): 105-115.

Pielke Jr, Roger A., and Christopher W. Landsea. "Normalized hurricane damages in the United States: 1925–95." *Weather and Forecasting* 13.3 (1998): 621-631.

Pielke Jr, Roger A., et al. "Hurricane vulnerability in Latin America and the Caribbean: Normalized damage and loss potentials." *Natural Hazards Review* 4.3 (2003): 101-114.

Pielke Jr, Roger A., et al. "Normalized hurricane damage in the United States: 1900–2005." *Natural Hazards Review* 9.1 (2008): 29-42.

Reardon, Thomas, and J. Edward Taylor. "Agroclimatic shock, income inequality, and poverty: Evidence from Burkina Faso." *World Development* 24.5 (1996): 901-914.

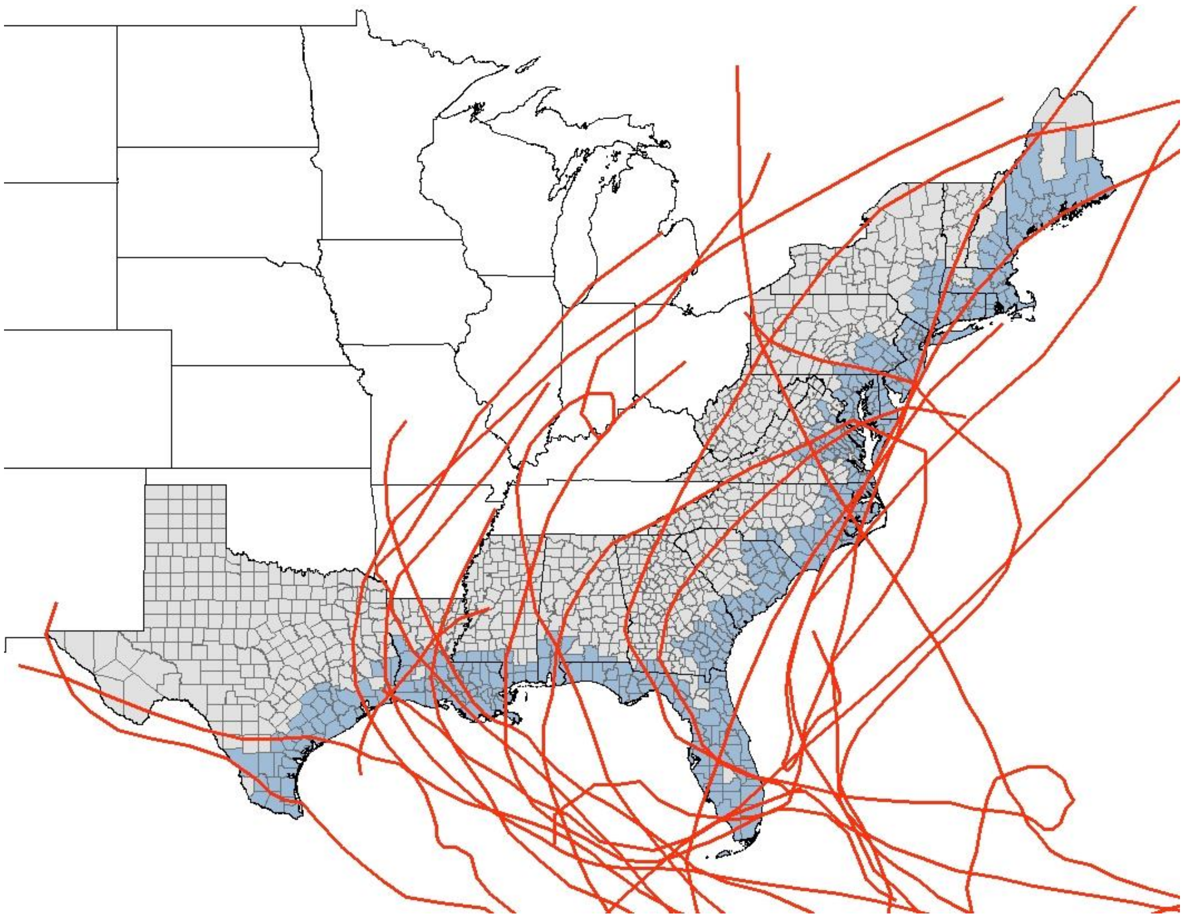
Rodriguez-Oreggia, Eduardo, et al. "Natural disasters, human development and poverty at the municipal level in Mexico." *The Journal of Development Studies* 49.3 (2013): 442-455.

Toya, Hideki, and Mark Skidmore. "Economic development and the impacts of natural disasters." *Economics Letters* 94.1 (2007): 20-25.

Veazie, Peter J. "When to combine hypotheses and adjust for multiple tests." *Health services research* 41.3p1 (2006): 804-818.

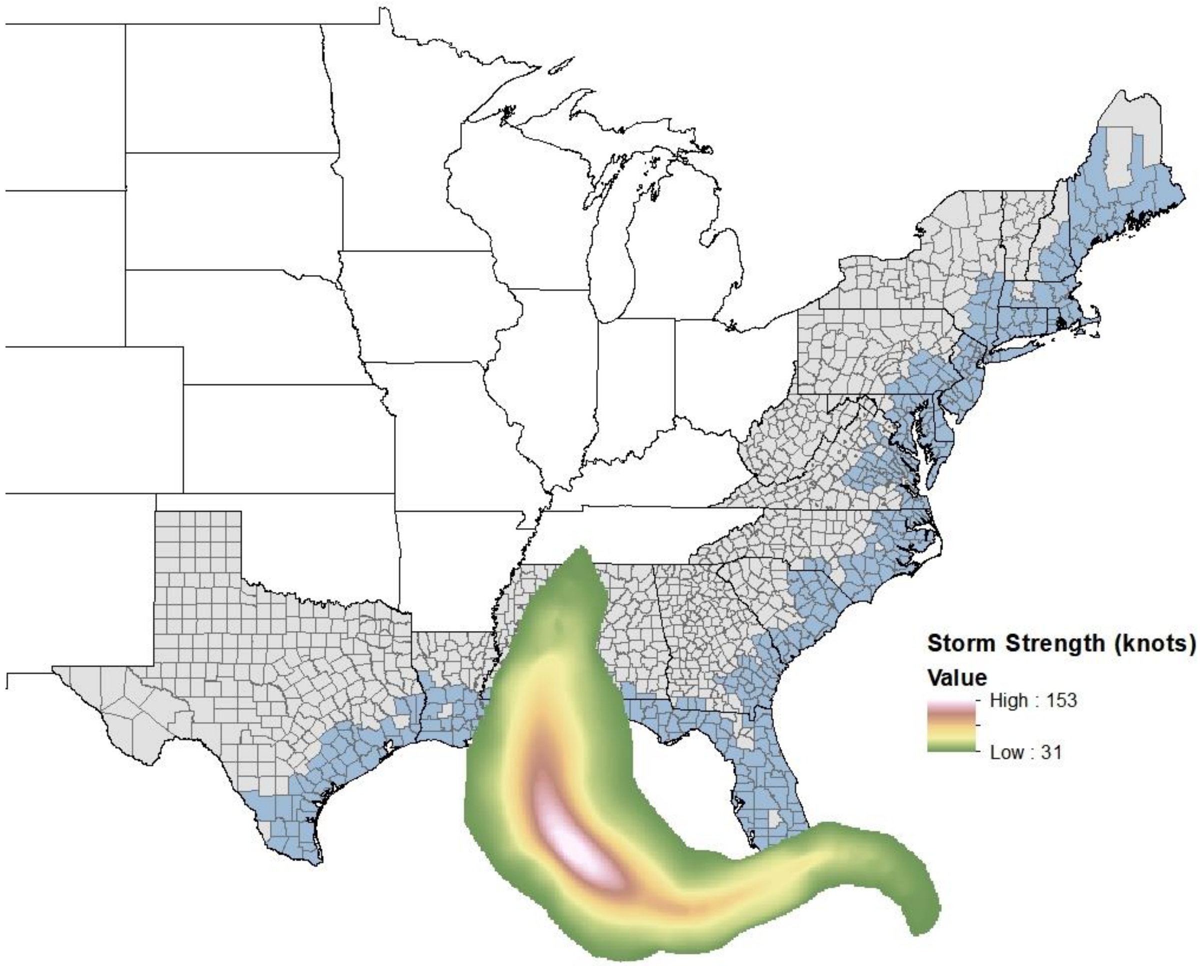
Yamamura, Eiji. "The Impact of Natural Disasters on Income Inequality: Analysis using Panel Data during the Period 1970 to 2004." *International Economic Journal* 29.3 (2015): 359-374.

Figure 1.1: Tracks of Hurricanes that Affected US Counties 1998-2014



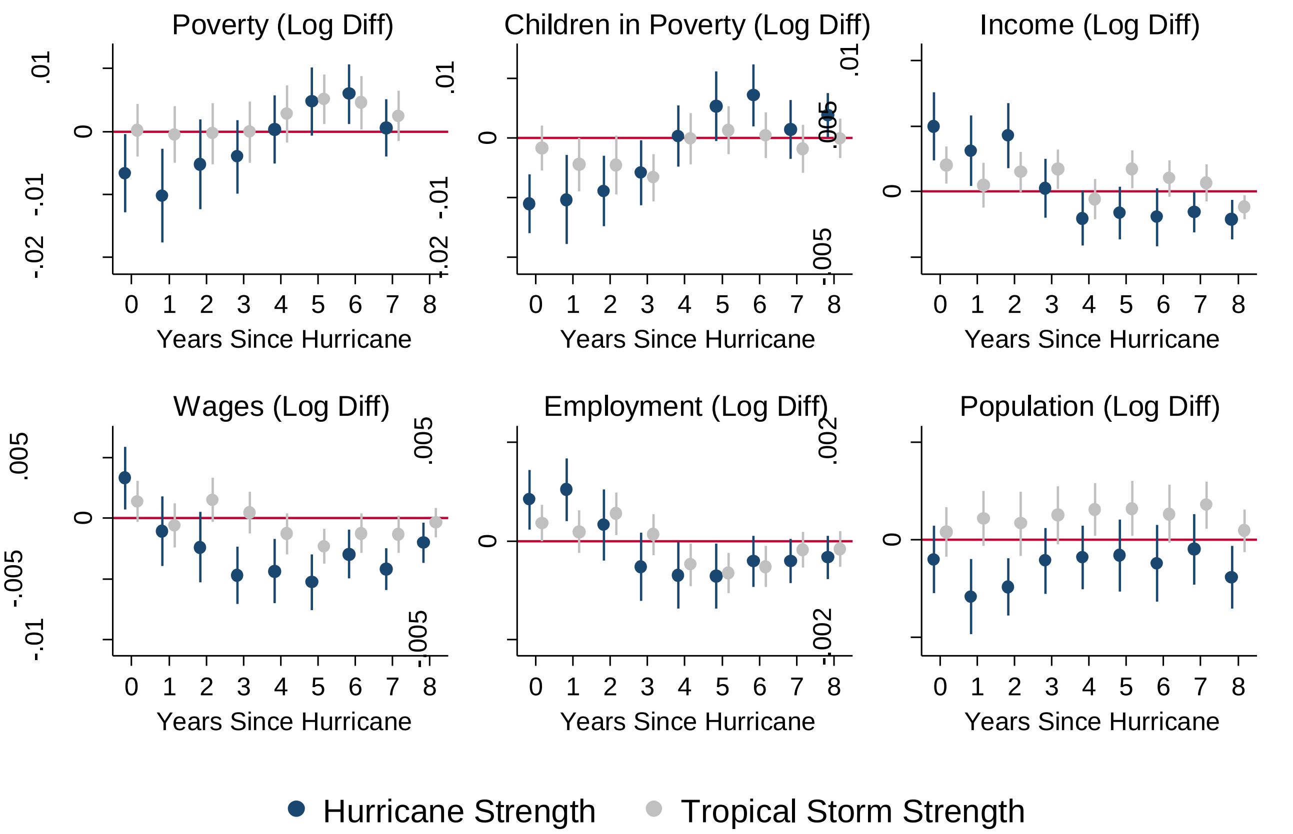
Notes: Figure shows the central storm paths of all hurricanes that were estimated to pass over coastal counties (blue) or other counties in coastal states that border the Atlantic Ocean and Gulf of Mexico (gray).

Figure 1.2: Hurricane Katrina (2005) Wind Field Estimation



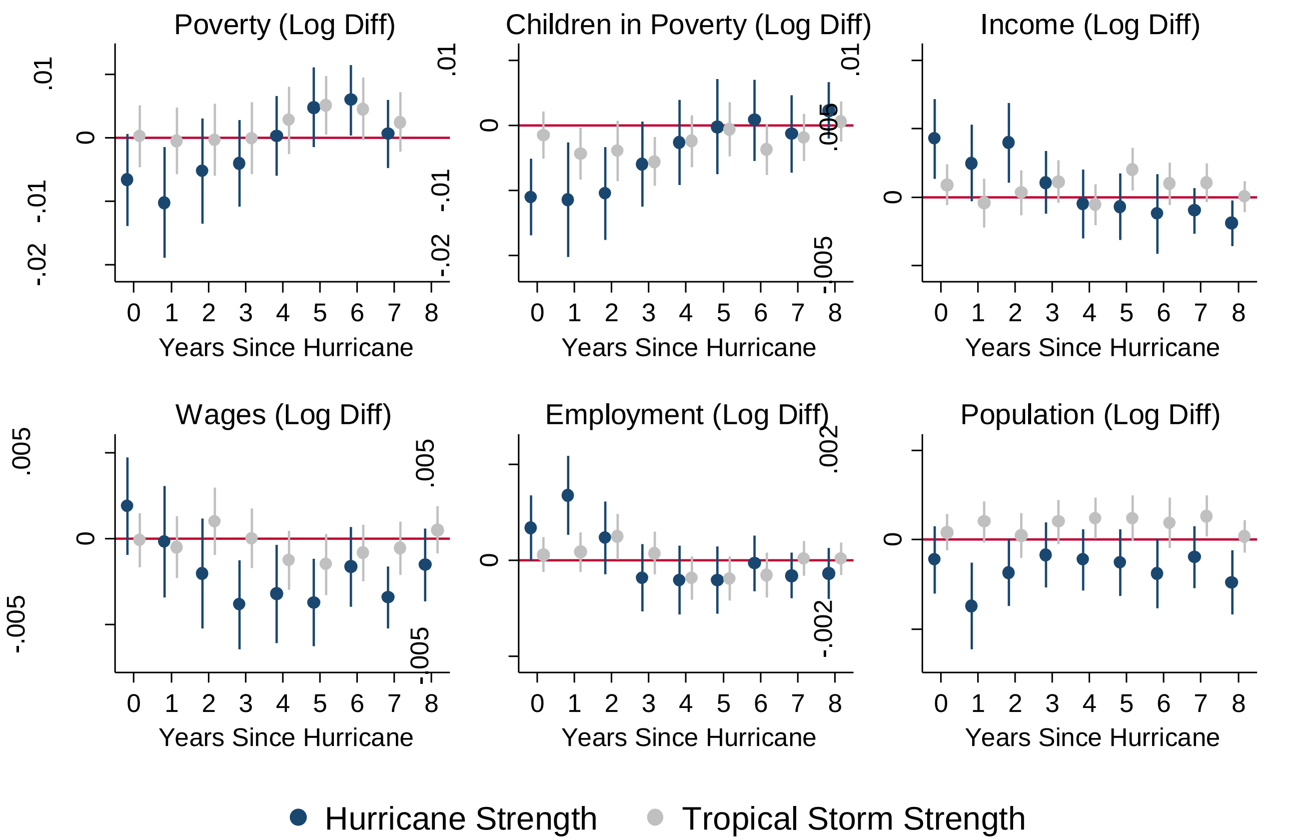
Notes: Figure shows the estimated wind field of Hurricane Katrina with pixelated values of wind speed measured in knots.

Figure 1.3: The Dynamic Effect of Hurricane Strike on Coastal Counties that Border the Atlantic Ocean and Gulf of Mexico



Notes: Graphs show point estimates from Equation (1) and 95% confidence intervals. Standard errors are corrected for spatial correlation up to 200 km around a county's centroid and time correlation up to 5 years. P-values are further adjusted for multiple hypothesis testing. Controls include a one-year lag of the dependent variable, county fixed effects, year fixed effects, and a county-year time trend.

Figure 1.4: The Dynamic Effect of Hurricane Strike on Counties in States that Border the Atlantic Ocean and Gulf of Mexico



Notes: Graphs show point estimates from Equation (1) and 95% confidence intervals. Standard errors are corrected for spatial correlation up to 200 km around a county's centroid and time correlation up to 5 years. P-values are further adjusted for multiple hypothesis testing. Controls include a one-year lag of the dependent variable, county fixed effects, year fixed effects, and a county-year time trend.

Table 1.1: Summary Statistics for County Level Variables

Variable	Sample	Description	Source	Mean	Std. Dev.
Max Wind	1998-2014	Maximum sustained wind of a county in a given year	RAMMB	73.56	10.32
People in Poverty	2006-2014	Number of people in poverty	SAIPE	29,698	66,105
Children in Poverty	2005-2014	Number of children ages 5 to 17 in poverty	SAIPE	6,584	15,789
Personal Income	2005-2014	Per capita personal income ($)	BEA	40,447	13,335
Wages and Salaries	2005-2014	Per capita wages and salaries ($)	BEA	17,049	1,922
Total Employment	2005-2014	Per capita jobs	BEA	0.50	0.16
Population	2005-2014	County residents	SEER	213,801	387,583

Notes: Summary statistics for Max Wind reported for non-zero values.

Table 1.2: Dynamic Effect of Hurricane Strike on Coastal Counties

VARIABLES	(1) People in Poverty	(2) Children in Poverty	(3) Personal Income	(4) Wages and Salaries	(5) Total Employ	(6) Population
Max Wind (H)						
Concurrent Year	-0.007**	-0.011***	0.005***	0.003**	0.002**	-0.000
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
1 Year Ago	-0.010**	-0.010**	0.003**	-0.001	0.003***	-0.001***
	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.000)
2 Years Ago	-0.005	-0.009***	0.004***	-0.002*	0.001	-0.001***
	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
3 Years Ago	-0.004	-0.006**	0.000	-0.005***	-0.001*	-0.000
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
4 Years Ago	0.000	0.000	-0.002*	-0.004***	-0.002**	-0.000
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
5 Years Ago	0.005*	0.005*	-0.002*	-0.005***	-0.002**	-0.000
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
6 Years Ago	0.006**	0.007**	-0.002*	-0.003***	-0.001*	-0.000
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
7 Years Ago	0.001	0.001	-0.002**	-0.004***	-0.001*	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
8 Years Ago		0.004**	-0.002**	-0.002**	-0.001*	-0.001**
		(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
Max Wind (TS)						
Concurrent Year	0.000	-0.002	0.002***	0.001*	0.001*	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
1 Year Ago	-0.001	-0.004*	0.001	-0.001	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
2 Years Ago	-0.000	-0.005*	0.002*	0.002*	0.001**	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
3 Years Ago	-0.000	-0.007***	0.002**	0.000	0.000	0.001*
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
4 Years Ago	0.003	-0.000	-0.001	-0.001	-0.001**	0.001**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
5 Years Ago	0.005**	0.001	0.002**	-0.002***	-0.002***	0.001**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
6 Years Ago	0.005**	0.000	0.001	-0.001	-0.001**	0.001*
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
7 Years Ago	0.002	-0.002	0.001	-0.001*	-0.000	0.001***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
8 Years Ago		-0.000	-0.001**	-0.000	-0.000	0.000
		(0.001)	(0.000)	(0.001)	(0.000)	(0.000)

Table 1.2: (continued)

Initial Value						
Dep. Var. (t-1)	-1.117***	-1.065***	-0.784***	-0.661***	-0.756***	-1.087***
	(0.0242)	(0.0248)	(0.0604)	(0.0674)	(0.0793)	(0.0706)
Observations	3,448	3,879	3,879	3,879	3,879	3,879
R-squared	0.67	0.6727	0.663	0.6357	0.6996	0.9179
Adjusted R-squared	0.5559	0.5754	0.5627	0.5274	0.6103	0.8935
Within R-squared	0.5682	0.5585	0.51	0.3849	0.4624	0.8846

Notes: Dependent variable units are in log differences. Maximum wind and initial values are in log transformations. Standard errors are shown in parentheses and are corrected for spatial correlation up to 200 km around a county's centroid and time correlation up to 5 years. P-values are further adjusted for multiple hypothesis testing. Controls include county fixed effects, year fixed effects, and a county-year time trend. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 1.3: Dynamic Effect of Hurricane Strike on all Counties in Coastal States

VARIABLES	(1) People in Poverty	(2) Children in Poverty	(3) Personal Income	(4) Wages and Salaries	(5) Total Employ	(6) Population
Max Wind (H)						
Concurrent Year	-0.006*	-0.011***	0.004***	0.002	0.002*	-0.000
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
1 Year Ago	-0.010**	-0.011***	0.002*	-0.000	0.003***	-0.001***
	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.000)
2 Years Ago	-0.006	-0.010***	0.004***	-0.002	0.001	-0.001*
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
3 Years Ago	-0.004	-0.006*	0.001	-0.004***	-0.001	-0.000
	(0.003)	-0.003	(0.001)	(0.001)	(0.001)	(0.000)
4 Years Ago	-0.002	-0.003	-0.000	-0.003**	-0.001	-0.000
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
5 Years Ago	0.001	-0.000	-0.001	-0.004***	-0.001	-0.001
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
6 Years Ago	0.002	0.001	-0.001	-0.002	-0.000	-0.001*
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
7 Years Ago	-0.000	-0.001	-0.001	-0.003***	-0.001	-0.000
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.000)
8 Years Ago		0.002	-0.002**	-0.002	-0.001	-0.001***
		(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
Max Wind (TS)						
Concurrent Year	0.000	-0.002	0.001	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
1 Year Ago	-0.000	-0.004**	-0.000	-0.001	0.000	0.000*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
2 Years Ago	0.000	-0.004*	0.000	0.001	0.001**	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
3 Years Ago	-0.001	-0.006***	0.001	0.000	0.000	0.000*
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
4 Years Ago	-0.000	-0.002*	-0.001	-0.001	-0.001*	0.000**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
5 Years Ago	0.001	-0.001	0.002**	-0.002*	-0.001*	0.000*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
6 Years Ago	-0.000	-0.004*	0.001	-0.001	-0.001*	0.000*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
7 Years Ago	0.001	-0.002	0.001	-0.001	0.000	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
8 Years Ago		0.001	0.000	0.001	0.000	0.000
		(0.001)	(0.000)	(0.001)	(0.000)	(0.000)

Table 1.3: (continued)

Initial Value						
Dep. Var. (t-1)	-1.129***	-1.063***	-0.893***	-0.607***	-0.658***	-0.976***
	(0.0153)	(0.0157)	(0.0336)	(0.0326)	(0.0382)	(0.0894)
Observations	9,752	10,971	10,971	10,971	10,971	10,971
R-squared	0.6549	0.6578	0.6251	0.5788	0.6256	0.8408
Adjusted R-squared	0.5383	0.5586	0.5164	0.4568	0.517	0.7946
Within R-squared	0.5676	0.539	0.4958	0.3201	0.3612	0.743

Notes: Dependent variable units are in log differences. Maximum wind and initial values are in log transformations. Standard errors are shown in parentheses and are corrected for spatial correlation up to 200 km around a county's centroid and time correlation up to 5 years. P-values are further adjusted for multiple hypothesis testing. Controls include county fixed effects, year fixed effects, and a county-year time trend. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 1.4: Dynamic Effect of Hurricane Strike on Income Distribution in ACS Coastal Counties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	< $10,000	$10,000 - $14,999	$15,000 - $24,999	$25,000 - $34,999	$35,000 - $49,999	$50,000 - $74,999	$75,000 - $99,999	$100,000 - $149,999	$150,000 - $199,999	> $200,000
Max Wind (H)										
Concurrent Year	0.004	-0.003	-0.013*	-0.001	-0.010**	-0.003	-0.001	0.001	-0.013	0.006
	(0.009)	(0.008)	(0.006)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.007)	(0.011)
1 Year Ago	0.010	0.004	-0.023***	0.000	-0.009*	-0.002	-0.000	0.004	-0.009	0.024*
	(0.009)	(0.007)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.007)	(0.009)
2 Years Ago	-0.003	-0.011	-0.019**	-0.000	-0.010**	0.000	0.005	0.005	0.005	0.024*
	(0.010)	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.009)	(0.010)
3 Years Ago	0.007	-0.007	-0.020***	-0.001	-0.010***	-0.000	-0.004	0.011**	0.001	0.017*
	(0.007)	(0.007)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)	(0.009)
4 Years Ago	0.006	0.001	-0.008*	0.005	0.001	0.002	0.000	0.009**	0.006	0.005
	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.006)	(0.007)
5 Years Ago	0.011	-0.002	-0.001	0.001	-0.001	0.004	-0.001	0.007*	-0.000	-0.004
	(0.005)	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)
Max Wind (TS)										
Concurrent Year	0.010	0.004	0.004	0.007*	-0.007**	-0.003	-0.001	0.001	-0.010	0.020*
	(0.007)	(0.006)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.007)	(0.008)
1 Year Ago	0.010	0.005	0.007*	0.005	-0.004	-0.003	0.000	0.002	-0.005	0.029***
	(0.007)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.007)	(0.007)
2 Years Ago	0.008	-0.001	0.005	0.002	-0.005*	-0.001	0.001	-0.003	-0.004	0.023**
	(0.008)	(0.006)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.006)	(0.007)
3 Years Ago	0.017*	0.001	0.000	0.005*	0.000	-0.001	-0.002	-0.001	-0.008	0.013*
	(0.007)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)	(0.006)
4 Years Ago	-0.002	-0.001	0.002	0.002	0.004*	-0.002	-0.003	0.001	-0.004	0.007
	(0.005)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)
5 Years Ago	-0.001	-0.001	0.001	0.000	0.004**	-0.001	-0.004**	-0.000	-0.008*	-0.000
	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.004)

Table 1.4: (continued)

Initial Value										
Dep. Var. (t-1)	-0.734***	-0.972***	-0.916***	-0.924***	-0.912***	-0.941***	-0.917***	-0.923***	-0.945***	-0.903***
	(0.136)	(0.0333)	(0.0384)	(0.0330)	(0.0372)	(0.0371)	(0.0379)	(0.0366)	(0.0485)	(0.0589)
Observations	2,133	2,133	2,133	2,133	2,133	2,133	2,133	2,133	2,133	2,133
R-squared	0.5082	0.6449	0.6247	0.6227	0.6345	0.6203	0.6381	0.6682	0.6887	0.6419
Adjusted R-squared	0.2512	0.4593	0.4286	0.4256	0.4436	0.4219	0.449	0.4948	0.5261	0.4547
Within R-squared	0.3336	0.5076	0.4664	0.4697	0.489	0.4778	0.4745	0.4817	0.517	0.4456

Notes: Dependent variable units are in log differences. Maximum wind and initial values are in log transformations. Standard errors are shown in parentheses and are corrected for spatial correlation up to 200 km around a county's centroid and time correlation up to 5 years. P-values are further adjusted for multiple hypothesis testing. Controls include county fixed effects, year fixed effects, and a county-year time trend. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 1.5: Dynamic Effect of Hurricane Strike on Income Distribution in ACS Counties in Coastal States

VARIABLES	(1) < $10,000	(2) $10,000 - $14,999	(3) $15,000 - $24,999	(4) $25,000 - $34,999	(5) $35,000 - $49,999	(6) $50,000 - $74,999	(7) $75,000 - $99,999	(8) $100,000 - $149,999	(9) $150,000 - $199,999	(10) > $200,000
Max Wind (H)										
Concurrent Year	0.004	-0.001	-0.012*	-0.001	-0.005	-0.000	-0.004	0.001	-0.001	-0.013
	(0.008)	(0.007)	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.010)
1 Year Ago	0.008	0.001	-0.022***	-0.000	-0.004	0.001	-0.004	0.004	0.003	0.009
	(0.008)	(0.007)	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.008)
2 Years Ago	-0.001	-0.017*	-0.016**	0.001	-0.005	0.001	0.001	0.006	0.014	0.014
	(0.010)	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.009)
3 Years Ago	0.007	-0.001	-0.020***	-0.004	-0.009**	0.005	-0.007**	0.010**	0.01	0.009
	(0.006)	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.008)
4 Years Ago	0.008*	0.002	-0.007*	0.005	0.000	0.006**	-0.001	0.009**	0.008	0.005
	(0.005)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.005)	(0.007)
5 Years Ago	0.007	-0.002	-0.003	0.001	-0.002	0.005	-0.001	0.008**	-0.006	0.005
	(0.005)	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.005)	(0.007)
Max Wind (TS)										
Concurrent Year	0.013**	0.002	0.002	0.005*	-0.003	-0.001	-0.001	-0.001	0.008	0.005
	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)
1 Year Ago	0.014**	0.003	0.003	0.006**	-0.001	-0.001	-0.001	-0.001	0.016***	0.01
	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.005)
2 Years Ago	0.011	-0.008	0.003	0.004	-0.002	0.002	0.000	-0.004	0.019***	0.005
	(0.006)	(0.005)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
3 Years Ago	0.020***	-0.004	-0.002	0.005**	0.002	-0.001	-0.003	-0.003	0.009	-0.000
	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)
4 Years Ago	0.006	0.001	0.002	0.002	0.002	-0.002	-0.001	-0.002	0.002	0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)
5 Years Ago	0.000	-0.002	0.000	-0.001	0.002	-0.001	-0.002	-0.000	-0.009**	-0.001
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)

Table 1.5: (continued)

Initial Value										
Dep. Var. (t-1)	-0.828***	-0.970***	-0.903***	-0.929***	-0.919***	-0.938***	-0.935***	-0.927***	-1.027***	-0.930***
	(0.0735)	(0.0211)	(0.0229)	(0.0209)	(0.0225)	(0.0230)	(0.0232)	(0.0259)	(0.0527)	(0.0337)
Observations	5,026	5,026	5,026	5,026	5,026	5,026	5,026	5,026	5,026	5,026
R-squared	0.5626	0.6344	0.6199	0.608	0.6285	0.6167	0.6344	0.6722	0.659	0.659
Adjusted R-squared	0.3387	0.4472	0.4253	0.4073	0.4383	0.4204	0.4473	0.5044	0.4844	0.4844
Within R-squared	0.3997	0.5032	0.452	0.4656	0.4791	0.4804	0.4791	0.4914	0.5466	0.5466

Notes: Dependent variable units are in log differences. Maximum wind and initial values are in log transformations. Standard errors are shown in parentheses and are corrected for spatial correlation up to 200 km around a county's centroid and time correlation up to 5 years. P-values are further adjusted for multiple hypothesis testing. Controls include county fixed effects, year fixed effects, and a county-year time trend. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

MANUSCRIPT 2

Is There a Window of Opportunity for Future Damage Mitigation After a Hurricane Strike? Evidence from Google Search Terms

(To be submitted to Climatic Change)

By

Patrick Prendergast [a] and Emi Uchida [a]

[a] *Department of Environmental and Natural Resource Economics, University of Rhode Island, Kingston, RI*

Abstract

Climate events such as hurricanes, droughts, floods, and tornados can cause billions of dollars in damages per year in the United States. Environmental damage cost mitigation strategies will need to be enacted at both the individual and community levels and depend on supportive and engaged voters and stakeholders to be effective. We test whether storm experience increases the attention a common cost mitigating strategy is given in a panel of media markets. We find that tropical storm and hurricane strikes cause statistically significant positive and dynamic changes to the relative internet search popularity of *flood insurance* in the areas they affect. We believe these results are useful to policymakers that want to take advantage of a window of opportunity to propose environmental damage mitigating policies where people are more engaged and willing to learn about mitigation measures soon after experiencing a storm.

2.1 INTRODUCTION

Every year, vulnerable communities in the United States are negatively affected by climatic events. Perhaps this fact has never been more salient than in 2017 when U.S. experience a record breaking $306.2 billion in cumulative damage costs from weather and climate events (NOAA 2018). Hurricanes were the most notable of these events with Harvey, Maria, and Irma accounting for about 85% of total costs. Unfortunately, hurricane damages are expected to increase in future years from an increased probability of major hurricanes due to climate change and continued coastal development along the East and Gulf coasts (CBO 2016). Research suggests collective community actions can be a key component of mitigating storm damage costs through actions like conserving coastal wetlands (Costanza et al. 2008), land use planning (Burby et al. 2000), and improved building codes (Leatherman et al. 2007). The success of such actions depends on community stakeholder cooperation and support. Attaining efficient and equitable protection from natural hazard damage means taking advantage of policy opportunities when support is most likely to be highest.

Our goal is to test whether there is a window of opportunity where people in communities affected by tropical storms and hurricanes might be more interested in acting to mitigate against potential future weather damages. We use Google Trends data of internet searches for the term *flood insurance* as a proxy for public interest in taking mitigating action. Using a panel of 170 designated media areas (DMAs) in the contiguous United States, we observe how the experience of tropical storms and hurricanes dynamically affect the relative popularity of monthly searches for *flood insurance* from 2004-2014. Results indicate that experiencing a storm increases search interest during the concurrent month and up to two months after. We also find a statistically significant increase in search interest in the months leading up to the one-year anniversary of a storm, indicating people may be anticipating potential damages of the hurricane season after a recent experience.

Certain events can trigger a window of opportunity for new policy support (Kingdon 1984). In the case of weather and climatic events, many studies have noted that there is a window of opportunity soon after a major disaster where there is likely to be support for future hazard mitigating policies (Prater and Lindell 2000; Pelling and Dill 2006; Birkmann et al. 2010; McSweeney and Coomes 2011). While it is certainly important for policymakers to note this opening after a rare event, less is known about more regularly occurring environmental phenomena like tropical storms and hurricanes that occur on a yearly basis. By including both tropical storms and hurricanes in our analysis, we are able to test if there is a generalizable increase in damage mitigation interest that extends beyond catastrophic natural disasters.

Purchasing flood insurance is one of the most straightforward ways to mitigate against future weather-related costs. It is an action that individuals can take on their own that has relatively immediate effects compared to changing development patterns, retrofitting homes to withstand flood damage, or waiting for policymakers to propose coastal wetland protection and restoration. Flood insurance premium rates have also historically been heavily subsidized where costs to policyholders often did not reflect actual risks (GAO 2013). Even so, market penetration rates of National Flood Insurance Program (NFIP) eligible households are estimated to be below 50% (Dixon et al. 2006, Atreya et al. 2015). This low market penetration rate has been one of the primary reasons why the relationship between weather and events and flood insurance preferences has been studied extensively in the literature.

We examine this relationship from a different perspective than the previous literature. While exploring the determinants of flood insurance purchases is undoubtedly valuable at a time when extreme weather events cause billions of dollars in costs, there are disadvantages to conclusions drawn from studies that examine people's willingness to pay for flood insurance in contingent markets (e.g. Botzen et al. 2009, Botzen et al. 2013, Raschky et al. 2013) or insurance purchasing patterns following weather events (e.g. Kriesel and Landry 2004, Michel-Kerjan and Kousky 2010, Michel-Kerjan et al. 2012, Atreya et al. 2015). Results from studies that use survey data might suffer from hypothetical bias while observations of flood purchasing decisions are based on observations where both the community participates in the NFIP and people's willingness to pay for damage protection is at least as high as the flood insurance cost. Changes in behavior or preferences noted in these types of studies after an environmental event might not fully reflect actual willingness or interest of people in affected communities to take action and, in turn, could undervalue the effects of a well-timed policy proposal that relies on stakeholder engagement and support.

We draw upon an extensive breadth of literature that uses internet search activity data. Google Trends search term popularity has been used to help forecast economic indicators (Choi and Varian 2012), measure the popularity of conservation related topics (Ficetola 2013, Proulx et al. 2013, Nghiem et al. 2016), explain reduction in teen childbirth rates (Kearney and Levine 2015), and to track disease outbreaks (Carneiro and Mylonakis 2009). More closely related to the present study, many studies have found that average weather and extreme weather events drive internet information seeking for terms like *hurricane* and *global warming* (Sherman-Morris et al. 2011, Cavanagh et al. 2014, Lang 2014, Lang and Ryder 2016). Many of these studies note that internet information seeking is an indication of a populations attention (Swearingen and Ripberger 2014).

The use of Google Trends data in our context allows us to examine if public attention shifts to future cost mitigating strategies, like information seeking behavior about flood insurance, after experiencing a weather event. We are able to measure changes of attention in a cost mitigating strategy relative to all other searched topics, regardless if the individuals seeking information can afford insurance or not, and for how long this attention is maintained for. We believe this information is useful to policymakers that want to know if there is an optimal time period (window of opportunity) after an environmental event to propose individual or community level mitigating actions against future environmental damages.

2.2 DATA

This section describes the two data sources used in our analysis: 1) Google Trends data used as the dependent variable, and 2) Storm data used to identify hurricane and tropical storm treatment of DMAs.

2.2.1 Google Trends Data

We use Google Trends data for aggregate searches for the term *flood insurance* as the dependent variable in our analysis. Google Trends is a service provided by Google Inc. that allows users to analyze the search activity of words or phrases over a specified time frame in the form of a relative popularity index. This index shows how often a word or phrase is searched by people using Google Search relative to the total search volume either for a cross-section of regions or within a single region over time. The index is normalized to be on a scale from 0 to 100. For example, an index value of 100 in a time series analysis for a particular search term within a geography (such as a state or country) would indicate the time period where that search term had the biggest share of total searches within the geography. Index values above 0 and below 100 indicate percentages of the time period with the biggest share of searches (e.g. an index value of 55 in a time period would indicate the share of searches for a particular search term was 55% of the time period that had the greatest share of total searches for that search term). Google censors the data by assigning a value of 0 when search volume for the specified search term does not exceed an undisclosed threshold.

We used Google Trends to download relative search activity for the term *flood insurance* in December of 2017. Data were downloaded for 170 designated media markets (DMAs) in the continental United States.[16] Data was downloaded for each DMA separately, so search index values represented relative search popularity for *flood insurance* within each DMA at the monthly level from January 2004 to December 2014. With an average index value of about 10, *flood insurance* is not a particularly popular search term, however, interesting patterns start to emerge when viewing a histogram of search popularity by month (Figure 2.1). The top left panel of Figure 2.1 shows that average search activity for *flood insurance* is higher during hurricane season in North America (with an average index value of 11.7) than other months (with an average index value of 9.1). This pattern still holds when splitting the DMAs into groups based on the

[16] There are 206 DMAs in the continental United States, but search volume for *flood insurance* never exceeded the threshold for 36 DMAs during the time period we analyzed.

likelihood of experiencing a tropical storm or hurricane. DMAs that border the Atlantic Ocean and Gulf of Mexico have a slightly higher index average for the hurricane season months (with an average value of 12.1) and lower index average for other months (with an average index value of 9) when compared the other panels. Our analysis will show if actual tropical storm or hurricane experience is one of the factors that drives the popularity of *flood insurance* searches in DMAs.

2.2.2 Storm Treatment Data

Since our dependent variable is observed at the DMA-month level, we define storm treatment as the maximum intensity a DMA experiences each month from tropical storms and hurricanes. We follow the methodology of Prendergast and Uchida (2018) and use GIS and the RAMMB's Extended Best Track Dataset to estimate a complete, pixelated wind field from the maximum wind speed of the center of each storm out to 34 knots for each hurricane and tropical storm that hit the United States from 2003-2014. As an illustration, Figure 2.2 shows which DMAs fall under the wind field of the 2005 storm Rita. Each DMA-month is assigned the maximum estimated storm intensity pixel it experiences from hurricanes and tropical storm and assigned a value of zero otherwise. Table 2.1 shows that of DMAs that experienced a storm, the average intensity (Max Wind) was about 47 knots, with 470 instances of DMAs experiencing a tropical storm strength winds and 54 instances of DMAs experiencing hurricane strength winds according to the Saffir-Simpson scale.

2.3 METHODOLOGY

To estimate the impact hurricane and tropical storm strikes have on Google Trends search data, we use a difference-in-differences framework that compares the search volume of *flood insurance* between treatment DMAs that get hit by hurricanes and tropical storms and unaffected DMAs over time. We observe monthly observations of our dependent variable for each DMA and estimate storm treatment effects up to one year afterwards. Our econometric model takes the following form:

$$flood\ insurance_{i,m,y} = \sum_{m=0}^{M} [\beta_m \times \log(Max\ Wind_{i,(m-M)})] + \alpha_i + f(\delta_m, \gamma_y) + \epsilon_{i,m,y} \qquad (1)$$

where $flood\ insurance_{i,m,y}$ is the relative search rate for searches related to "flood insurance" in DMA i for month m in year y, $Max\ Wind_{i,(m-M)}$ is the maximum wind speed that DMA i experiences in an $m - M$ window, α_i is a DMA fixed effect, δ_m is a month fixed effect, and γ_y is a year fixed effect. We test the robustness of coefficient estimates using different combinations of month and year fixed effects, which is why they are expressed as a function in the Equation (1). We perform a log(x + 1) transformation on $Max\ Wind_{i,(m-M)}$ in order to preserve the "zero" values of the months when a DMA does not experience a storm. The parameters of interest are the coefficients β_m which show the marginal effect of maximum wind of a storm on the monthly relative search volume of *flood insurance* in a $m - M$ window.

We also investigate whether there is heterogeneity in the effect storm strength has on Google search share by testing whether hurricanes and tropical storms have differing effects. Our second econometric model takes the following form:

$$flood\ insurance_{i,m,y} = \sum_{m=0}^{M} (\lambda_m \times Hurricane_{i,(m-M)}) +$$

$$\sum_{m=0}^{M} (\phi_m \times Tropical\ Storm_{i,(m-M)}) + \alpha_i + f(\delta_m, \gamma_y) + \epsilon_{i,m,y} \quad (2)$$

where $Hurricane_{i,(m-M)}$ is a dummy variable equal to one if DMA i experienced a storm with a maximum wind strength of $x \geq 64$ knots in an $m - M$ window, $Tropical\ Storm_{i,(m-M)}$ is a dummy variable equal to one if DMA i experienced a storm with a maximum wind strength $34 \leq x < 64$ in an $m - M$ window. The parameters of interest are the coefficients λ_m and ϕ_m which show the marginal effect of a hurricane and a tropical storm wind strength strike on the monthly search volume of *flood insurance* in an $m - M$ window.

We perform multiple regressions to identify storm treatment effects on Google searches for *flood insurance*. Models (1) and (2) are conducted using the full set of DMAs in our sample as well as just those in coastal states in order to compare outcomes in DMAs that are both geographically close and have a chance of actually experiencing a hurricane or tropical storm. We use ordinary least squares as our estimation technique for our main results. As discussed in the data section, the dependent variable in our models has many zero values due to censoring by Google. With a dependent variable that is overdispersed (mean = 9.998, variance = 294.718), a logical option may be to use a negative binomial or tobit estimation technique. However, studies have warned against the use of fixed effects in these settings. Depending on the number of time periods in a panel, the incidental parameters problem can cause biased slope estimations from a negative binomial fixed effects regression and biased standard deviations from a tobit fixed effects regression that ultimately biases marginal effects (Green 2004, Greene 2007). For completeness, we present results from negative binomial and tobit fixed effects regression in the appendix, however, interpretation of the coefficient estimates should be done cautiously.

2.4 RESULTS

2.4.1 The Effect of Storm Experience on Flood Insurance Search Volume

Table 2.2 presents the results for the estimates of the effect maximum wind has on search activity for *flood insurance* as modeled in Equation (1). Results show statistically significant and dynamic coefficient estimates. Depending on the set of fixed effects used, a 1% increase in the maximum wind that a DMA experiences increases the *flood insurance* search index between 1.1 and 1.5 points in the month of the storm. This effect lasts for multiple months, with a 1% increase in maximum wind increasing the search index by 0.8 to 1.1 points the month after a strike and an increase of 0.51 to 0.53 points two months after. Applied to a mean relative search volume of 9.998 (Table 2.1), a 1% increase in maximum wind of an average storm of 46.83 knots translates to an overall level of relative *flood insurance* search volume between 10.5 and 11.1 points during the month of and two months following a storm strike (using coefficient estimates from Column 3).

Table 2.2 also shows some interesting dynamic results further down the line after a storm hits. After months of no discernible effect on the *flood insurance* search index, there are statistically significant and robust coefficient estimates 11 to 12 months after a storm strike. This may seem curious at first, but the seasonal nature of hurricane and tropical storm activity may explain the pattern of treatment effects shown. The coefficient estimates between the concurrent month and up to two months after a storm strike shows that people that directly experience a tropical storm or hurricane may be interested in things like the availability and price of flood insurance in their area for a short time period after as a reaction to that experience. The positive coefficients at the 11 and 12-month time lags show that after people experience a storm, they may be *anticipating* the consequences of an upcoming hurricane season by searching for information about flood insurance. Unfortunately, we do not have the data to test whether this later uptick in relative search activity is an artifact of individuals that experience flooding from a previous storm, or if interest is driven by media coverage of the fallout of a tropical storm or hurricane near the anniversary of the event.

2.4.2 The Effect of Storm Heterogeneity on Flood Insurance Search Volume

The results presented from Equation (1) treat storm strength as a continuous variable, however, there is reason to believe there could be heterogeneous effects storms have on Google search activity for *flood insurance* based on what type of storm a DMA experiences. Hurricanes may have a bigger impact on search volume than tropical storms due to more damage from flooding or from increased media coverage hurricanes have over tropical storms. Table 2.3 presents the results of Equation (2) where storm treatment is broken up into dummy variables that indicate whether a DMA experienced a storm of hurricane strength or tropical storm strength. Results corroborate those presented in Table 2.2 where storms cause an increase in the *flood insurance* search index over time in DMAs that experience storms. Coefficient estimates also follow intuition that hurricanes have a larger impact on search volume than tropical storms do. On average, a hurricane will increase the *flood insurance* search index between 13.4 and 16.9 points in a DMA during the month of the strike and between 10.9 and 14 points in the following month. Tropical storms, on the other hand, increase relative search volume between 2.9 and 4.3

points during the month of a strike, on average, and between 2 and 2.3 points the following month (although the coefficient estimate is not statistically significant in Column (3) that uses month-year fixed effects). Interestingly, results still show statistically significant longer lags, but mainly with the tropical storm dummy variables.

2.4.3 Coastal DMA Analysis

Table 2.4 and Table 2.5 recreate the results from Equation (1) and Equation (2) on the smaller sample of DMAs in states that border the Atlantic Ocean and Gulf of Mexico, respectively. Both sets of results are very similar to the results from the full sample of DMAs. Table 2.4 shows the effect maximum wind has on relative search activity for *flood insurance* in coastal DMAs. Depending on the set of fixed effects used, a 1% increase in maximum wind that a coastal DMA experiences results in an increase in the *flood insurance* search index in the range of 0.95 and 1.61 points during the month of the storm and between 0.76 and 1.12 points the following month. Results also show statistically significant coefficient estimates 11 and 12 months following a storm strike, although the results are not as robust as they were in Table 2.4.

Results for the heterogeneity of storm effects on search activity is presented in Table 2.5. Similar to the results of the full sample of DMAs from Table 2.3, hurricanes increase *flood insurance* search activity by 12.1 to 16.8 points during the concurrent month in coastal DMAs and between 10.2 and 13.9 points the next month. Tropical storms cause an increase in search activity at a smaller magnitude than hurricanes with an increase in the range of 2.4 to 4.6 points during the concurrent month and between 2.3 and 2.5 points in the following month. Again, there are signs that people in DMAs that experience a tropical storm may be anticipating the consequences of future storms 11 and

12 months afterwards (although the coefficient of the 12-month lag is not statistically significant in the model that uses month-year fixed effects).

2.4.4 Comparison of Results with Related Studies

Although we are the first to examine the effect of tropical storm and hurricane treatment on the relative popularity of *flood insurance* Google searches, it is informative to compare our results with studies that used similar settings. In a study that looks at how weather fluctuations affect information seeking about climate change and global warming, Lang (2014) does not examine the effects of individual tropical storms and hurricanes but does find that increased precipitation leads to more searches for the term *flood*. Our results show similar behavior for DMAs when the universe of searches is restricted further to the more specific term *flood insurance*.

There have been a few studies that look at how storms cause changes in internet search behavior. Both Sherman-Morris et al. (2011) and Lang and Ryder (2016) find that experience increases searches for the term *hurricane*. Lang and Ryder (2016) also find that storm experience increases searches for *climate change* and *global warming* in a delayed manner two months after the event. Our results fit in well with these studies where people may spend the time immediately following a storm strike seeking information about hurricane characteristics, flood patterns and warnings, as well as cost mitigating options such as flood insurance before turning their attention to less urgent narratives such as how weather patterns related to climate change and global warming.

2.5 CONCLUSION

A growing literature has been using Google Trends search activity as a “revealed preference” for environment and climatic concerns (Kahn and Kotchen 2010). Many studies have investigated the relationship between storm experience and information seeking about hurricanes, flooding, and climate change. We add to the literature by investigating if there is a window of opportunity after a storm strike where people are interested in ways to mitigate future environmental damages. To do so, we use a difference-in-differences model to observe how the relative popularity of the search term *flood insurance* in U.S. DMAs is affected by exogenous exposure to tropical storm and hurricane strikes from 2004 to 2014.

We find that storm strikes cause statistically significant changes to the relative popularity of *flood insurance* over time. Results indicate that a 1% increase in the strength of a storm results in an increase in *flood insurance* search index of 1.08 points the month of the strike, 0.82 points the following month, and 0.51 points two months after. Estimates remain significant when storms are split into effects from hurricanes and tropical storms separately. This indicates that there is a temporary amount of time after a storm strike (regardless of whether the storm is a tropical storm or hurricane) where there is an increased public attention on learning about flood insurance. Due to the low cost, anonymous, and revealed preference nature of internet search term behavior, we believe our results are useful for policymakers that want to propose individual or community based environmental damage mitigation policies that need an engaged and supportive stakeholder base to enact.

REFERENCES

Atreya, Ajita, Susana Ferreira, and Erwann Michel-Kerjan. "What drives households to buy flood insurance? New evidence from Georgia." *Ecological Economics* 117 (2015): 153-161.

Birkmann, Jörn, et al. "Extreme events and disasters: a window of opportunity for change? Analysis of organizational, institutional and political changes, formal and informal responses after mega-disasters." *Natural hazards* 55.3 (2010): 637-655.

Botzen, Wouter JW, Jeroen CJH Aerts, and Jeroen CJM van den Bergh. "Willingness of homeowners to mitigate climate risk through insurance." *Ecological Economics* 68.8-9 (2009): 2265-2277.

Botzen, W. J. W., J. C. J. H. Aerts, and J. C. J. M. Van den Bergh. "Individual preferences for reducing flood risk to near zero through elevation." *Mitigation and Adaptation Strategies for Global Change* 18.2 (2013): 229-244.

Burby, Raymond J., et al. "Creating hazard resilient communities through land-use planning." *Natural hazards review* 1.2 (2000): 99-106.

Carneiro, Herman Anthony, and Eleftherios Mylonakis. "Google trends: a web-based tool for real-time surveillance of disease outbreaks." *Clinical infectious diseases* 49.10 (2009): 1557-1564.

Cavanagh, Patrick, et al. "Searching for the determinants of climate change interest." *Geography Journal* 2014 (2014).

Choi, Hyunyoung, and Hal Varian. "Predicting the present with Google Trends." *Economic Record* 88.s1 (2012): 2-9.

Congressional Budget Office, *Potential Increases in Hurricane Damage in the United States: Implications for the Federal Budget* (June 2016), https://www.cbo.gov/sites/default/files/114th-congress-2015-2016/reports/51518-hurricane-damage.pdf.

Costanza, Robert, et al. "The value of coastal wetlands for hurricane protection." *AMBIO: A Journal of the Human Environment* 37.4 (2008): 241-248.

Dixon, Lloyd, Noreen Clancy, Seth A. Seabury, and Adrian Overton. "The National Flood Insurance Program’s market penetration rate: estimates and policy implications." *Santa Monica, CA: RAND Corporation* (2006).

Ficetola, Gentile Francesco. "Is interest toward the environment really declining? The complexity of analysing trends using internet search data." *Biodiversity and conservation* 22.12 (2013): 2983-2988.

Government Accountability Office, *Flood Insurance: More Information Needed on Subsidized Properties*. (July 2013), https://www.gao.gov/assets/660/655734.pdf.

Greene, William H. "Fixed and random effects models for count data." (2007).

Greene, William. "Fixed effects and bias due to the incidental parameters problem in the Tobit model." *Econometric reviews*23.2 (2004): 125-147.

Kahn, Matthew E., and Matthew J. Kotchen. *Environmental concern and the business cycle: The chilling effect of recession*. No. w16241. National Bureau of Economic Research, 2010.

Kearney, Melissa S., and Phillip B. Levine. "Media influences on social outcomes: The impact of MTV's 16 and pregnant on teen childbearing." *American Economic Review* 105.12 (2015): 3597-3632.

Kingdon, John W., and James A. Thurber. *Agendas, alternatives, and public policies*. Vol. 45. Boston: Little, Brown, 1984.

Kriesel, Warren, and Craig Landry. "Participation in the National Flood Insurance Program: An empirical analysis for coastal properties." *Journal of Risk and Insurance* 71.3 (2004): 405-420.

Lang, Corey. "Do weather fluctuations cause people to seek information about climate change?" *Climatic change* 125.3-4 (2014): 291-303.

Lang, Corey, and John David Ryder. "The effect of tropical cyclones on climate change engagement." *Climatic change*135.3-4 (2016): 625-638.

Leatherman, Stephen P., Arindam Gan Chowdhury, and Carolyn J. Robertson. "Wall of wind full-scale destructive testing of coastal houses and hurricane damage mitigation." *Journal of Coastal Research* (2007): 1211-1217.

McSweeney, Kendra, and Oliver T. Coomes. "Climate-related disaster opens a window of opportunity for rural poor in northeastern Honduras." *Proceedings of the National Academy of Sciences* 108.13 (2011): 5203-5208.

Michel-Kerjan, Erwann O., and Carolyn Kousky. "Come rain or shine: Evidence on flood insurance purchases in Florida." *Journal of Risk and Insurance* 77.2 (2010): 369-397.

Michel-Kerjan, Erwann, Sabine Lemoyne de Forges, and Howard Kunreuther. "Policy tenure under the us national flood insurance program (nfip)." *Risk Analysis* 32.4 (2012): 644-658.

Nghiem, Le TP, et al. "Analysis of the capacity of Google Trends to measure interest in conservation topics and the role of online news." *PLoS One* 11.3 (2016): e0152802.

NOAA National Centers for Environmental Information (NCEI) U.S. Billion-Dollar Weather and Climate Disasters (2018). https://www.ncdc.noaa.gov/billions/

Pelling, Mark, and Kathleen Dill. "Natural'disasters as catalysts of political action." *Media Development* 53.4 (2006): 7.

Prater, Carla S., and Michael K. Lindell. "Politics of hazard mitigation." *Natural Hazards Review* 1.2 (2000): 73-82.

Prendergast, Patrick, and Emi Ucida. “The Effect of Hurricane Strikes on County Poverty Measurements in the United States.” Working paper, 2018.

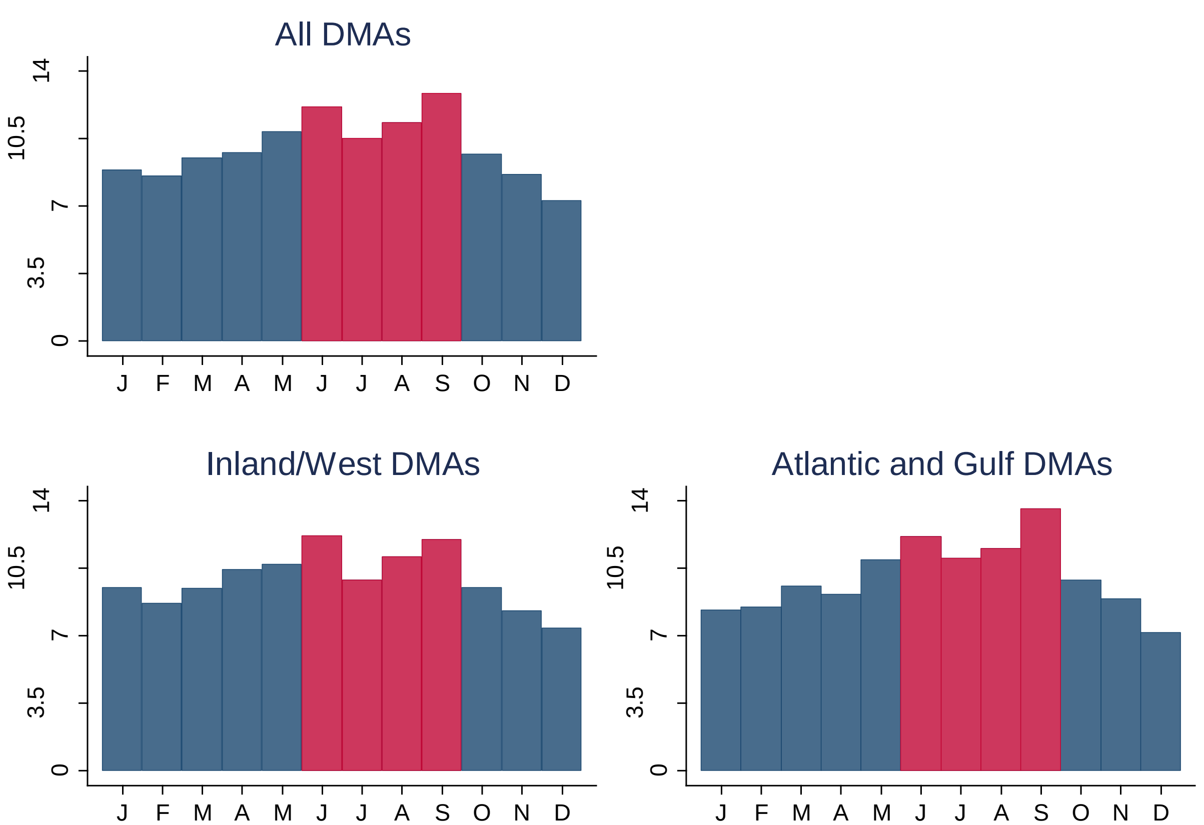
Proulx, Raphaël, Philippe Massicotte, and Marc Pépino. "Googling trends in conservation biology." *Conservation Biology* 28.1 (2014): 44-51.

Raschky, Paul A., et al. "Uncertainty of governmental relief and the crowding out of flood insurance." *Environmental and Resource Economics* 54.2 (2013): 179-200.

Sherman-Morris, Kathleen, Jason Senkbeil, and Robert Carver. "Who's Googling what? What Internet searches reveal about hurricane information seeking." *Bulletin of the American Meteorological Society* 92.8 (2011): 975-985.

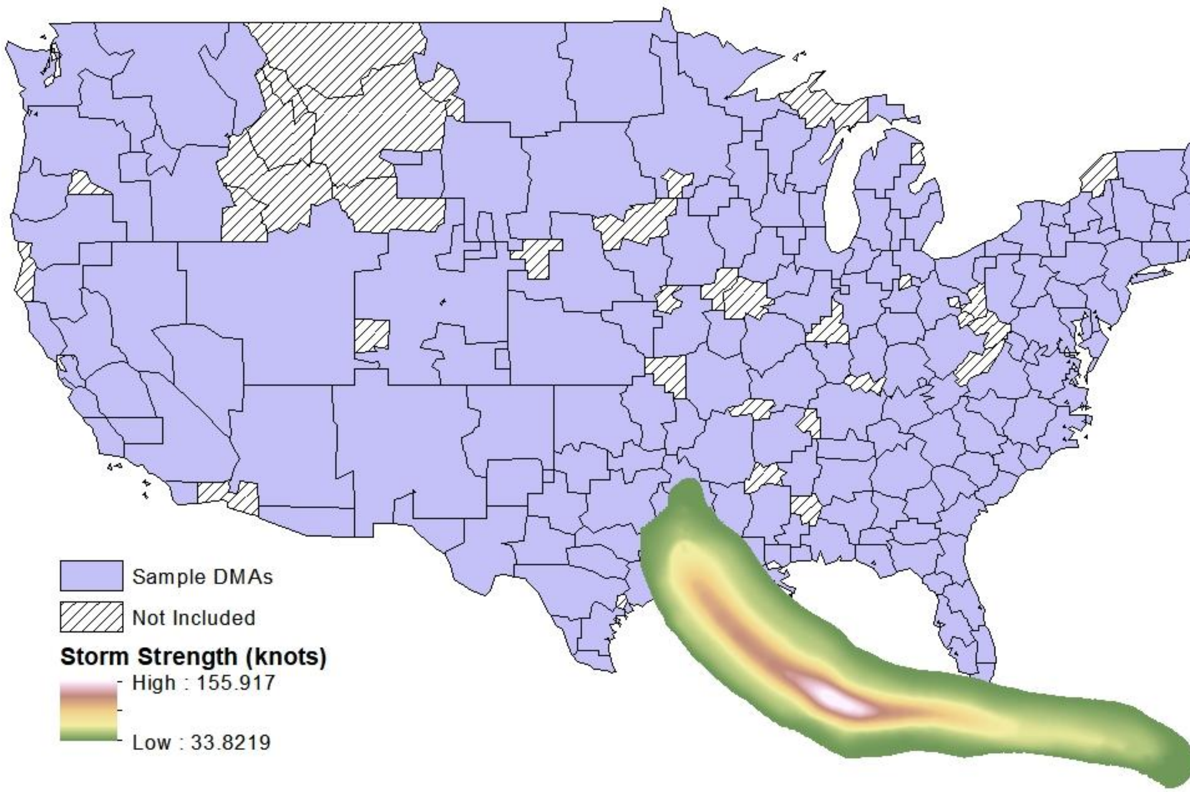
Swearingen, C. Douglas, and Joseph T. Ripberger. "Google Insights and US senate elections: does search traffic provide a valid measure of public attention to political Candidates?" *Social Science Quarterly* 95.3 (2014): 882-893

Figure 2.1: Flood Insurance Searches by Month



Notes: Bar graphs show average relative search volume for *flood insurance* by month. The top panel shows the distribution of *flood insurance* searches by month for all DMAs in the sample, while the bottom left and bottom right panels do the same for DMAs that do not border the Atlantic Ocean and Gulf of Mexico and those that do, respectively. Red colored bars indicate searches that occur during hurricane season in the North Atlantic Ocean between June and September.

Figure 2.2: Sample DMAs and Rita Storm Example



Notes: Figure shows the estimated wind field of Hurricane Rita (2005) with pixelated values of wind speed measured in knots.

Table 2.1: Summary Statistics

Continuous Variables					
Variable	Years	Mean	Std. Dev.	Min	Max
Flood insurance searches	2004-2014	9.998	17.167	0	100
Max Wind	2003-2014	46.833	14.071	33.388	116.411

Discrete Variables		
Variable	Years	Count
Hurricane	2003-2014	54
Tropical Storm	2003-2014	470

Notes: Summary statistics for Max Wind given for non-zero values

Table 2.2: Effect of Storm Experience on Flood Insurance Google Searches on all DMAs

VARIABLES	(1)	(2)	(3)
Max Wind (log)			
Concurrent Month	1.495***	1.286***	1.084***
	(0.195)	(0.198)	(0.208)
1 Month Ago	0.995***	1.057***	0.815***
	(0.195)	(0.197)	(0.208)
2 Months Ago	0.293	0.529***	0.506**
	(0.194)	(0.197)	(0.208)
3 Months Ago	0.0202	0.276	0.265
	(0.194)	(0.197)	(0.208)
4 Months Ago	-0.263	-0.0251	-0.0274
	(0.190)	(0.193)	(0.203)
5 Months Ago	-0.107	0.0727	0.136
	(0.189)	(0.193)	(0.202)
6 Months Ago	-0.0980	0.0319	0.132
	(0.188)	(0.192)	(0.201)
7 Months Ago	0.103	0.0993	0.148
	(0.188)	(0.191)	(0.200)
8 Months Ago	0.191	0.0566	0.101
	(0.188)	(0.191)	(0.200)
9 Months Ago	0.376**	0.113	0.344*
	(0.188)	(0.191)	(0.200)
10 Months Ago	0.366*	0.168	0.220
	(0.188)	(0.191)	(0.200)
11 Months Ago	0.621***	0.384**	0.516**
	(0.188)	(0.191)	(0.201)
12 Months Ago	0.988***	0.654***	0.410**
	(0.188)	(0.191)	(0.201)
Observations	22,440	22,440	22,440
DMA FE	Yes	Yes	Yes
Month FE	No	Yes	No
Year FE	No	Yes	No
Month-Year FE	No	No	Yes
Adjusted R-squared	0.147	0.158	0.176

Notes: Results are from three separate OLS regressions. Observations are at the DMA-month level. DMA Google search share of queries that include flood insurance is the dependent variable and a log transformation of monthly maximum wind strength with lags are the independent variables. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 2.3: Effect of Storm Experience Heterogeneity on Flood Insurance Google Searches on all DMAs

VARIABLES	(1)	(2)	(3)
Hurricane Dummy			
Concurrent Month	16.89***	15.48***	13.37***
	(4.089)	(4.008)	(3.967)
1 Month Ago	13.99***	13.45***	10.92***
	(3.917)	(3.935)	(3.710)
2 Months Ago	4.095	4.246	4.287
	(3.459)	(3.431)	(3.342)
3 Months Ago	1.689	2.559	2.050
	(2.337)	(2.238)	(2.186)
4 Months Ago	2.729	2.775	3.043
	(3.003)	(2.912)	(2.910)
5 Months Ago	2.309	2.175	2.408
	(2.487)	(2.580)	(2.575)
6 Months Ago	2.763	2.298	3.286
	(3.238)	(3.263)	(3.250)
7 Months Ago	5.332	4.669	5.273
	(3.464)	(3.408)	(3.346)
8 Months Ago	3.302*	2.101	2.487
	(1.854)	(1.798)	(1.813)
9 Months Ago	3.206	1.377	2.076
	(3.034)	(3.069)	(3.074)
10 Months Ago	2.142	0.955	0.764
	(2.665)	(2.657)	(2.693)
11 Months Ago	0.0184	-1.199	0.0526
	(2.059)	(2.025)	(2.101)
12 Months Ago	8.386**	6.170*	3.673
	(3.407)	(3.301)	(2.960)

Table 2.3: (continued)

Tropical Storm Dummy			
Concurrent Month	4.304***	3.576***	2.930***
	(0.867)	(0.859)	(0.942)
1 Month Ago	1.957**	2.291**	1.527
	(0.924)	(0.958)	(1.011)
2 Months Ago	0.197	1.174	1.065
	(0.901)	(0.925)	(0.964)
3 Months Ago	-0.586	0.388	0.369
	(0.785)	(0.803)	(0.822)
4 Months Ago	-1.650***	-0.634	-0.663
	(0.570)	(0.585)	(0.625)
5 Months Ago	-0.855	-0.0338	0.212
	(0.756)	(0.770)	(0.771)
6 Months Ago	-0.808	-0.153	0.166
	(0.717)	(0.710)	(0.767)
7 Months Ago	-0.395	-0.282	-0.222
	(0.786)	(0.798)	(0.859)
8 Months Ago	0.190	-0.208	-0.116
	(1.070)	(1.082)	(1.164)
9 Months Ago	1.246	0.340	1.207
	(0.797)	(0.820)	(0.902)
10 Months Ago	1.506*	0.792	1.038
	(0.806)	(0.824)	(0.886)
11 Months Ago	2.907***	2.041**	2.481***
	(0.855)	(0.867)	(0.907)
12 Months Ago	3.398***	2.234**	1.479
	(0.843)	(0.861)	(0.906)
Observations	22,440	22,440	22,440
DMA FE	Yes	Yes	Yes
Month FE	No	Yes	No
Year FE	No	Yes	No
Month-Year FE	No	No	Yes
Adjusted R-squared	0.149	0.159	0.178

Notes: Results are from three separate OLS regressions. Observations are at the DMA-month level. DMA Google search share of queries that include flood insurance is the dependent variable and dummy variables indicating hurricane and tropical storm strength wind strikes with lags are the independent variables. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 2.4: Effect of Storm Experience on Flood Insurance Google Searches on DMAs in Coastal States

VARIABLES	(1)	(2)	(3)
Max Wind (log)			
Concurrent Month	1.608***	1.399***	0.947***
	(0.259)	(0.251)	(0.284)
1 Month Ago	1.116***	1.139***	0.757**
	(0.273)	(0.295)	(0.320)
2 Months Ago	0.376	0.580**	0.464
	(0.263)	(0.276)	(0.286)
3 Months Ago	0.0495	0.254	0.334
	(0.228)	(0.236)	(0.228)
4 Months Ago	-0.312*	-0.102	-0.0687
	(0.170)	(0.172)	(0.181)
5 Months Ago	-0.137	-0.0604	0.108
	(0.199)	(0.194)	(0.196)
6 Months Ago	-0.213	-0.189	0.0224
	(0.206)	(0.200)	(0.216)
7 Months Ago	0.176	0.156	0.128
	(0.251)	(0.246)	(0.268)
8 Months Ago	0.257	0.0867	0.189
	(0.286)	(0.285)	(0.307)
9 Months Ago	0.375*	0.0669	0.210
	(0.209)	(0.216)	(0.256)
10 Months Ago	0.403*	0.153	0.162
	(0.217)	(0.220)	(0.250)
11 Months Ago	0.619***	0.351	0.456*
	(0.223)	(0.223)	(0.253)
12 Months Ago	1.088***	0.710***	0.338
	(0.232)	(0.236)	(0.254)
Observations	11,748	11,748	11,748
DMA FE	Yes	Yes	Yes
Month FE	No	Yes	No
Year FE	No	Yes	No
Month-Year FE	No	No	Yes
Adjusted R-squared	0.134	0.148	0.178

Notes: Results are from three separate OLS regressions. Observations are at the DMA-month level. DMA Google search share of queries that include flood insurance is the dependent variable and a log transformation of monthly maximum wind strength with lags are the independent variables. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 2.5: Effect of Storm Experience Heterogeneity on Flood Insurance Google Searches on DMAs in Coastal States

VARIABLES	(1)	(2)	(3)
Hurricane Dummy			
Concurrent Month	16.81***	15.52***	12.05***
	(4.089)	(3.976)	(4.003)
1 Month Ago	13.94***	13.52***	10.22***
	(3.928)	(3.953)	(3.754)
2 Months Ago	4.120	4.278	3.570
	(3.460)	(3.418)	(3.367)
3 Months Ago	1.843	2.667	2.100
	(2.342)	(2.204)	(2.166)
4 Months Ago	2.831	2.854	3.277
	(3.022)	(2.928)	(2.936)
5 Months Ago	2.493	1.980	3.012
	(2.487)	(2.616)	(2.546)
6 Months Ago	2.744	1.787	2.843
	(3.243)	(3.277)	(3.236)
7 Months Ago	5.353	4.554	4.945
	(3.473)	(3.390)	(3.290)
8 Months Ago	3.325*	1.851	2.733
	(1.859)	(1.812)	(1.836)
9 Months Ago	3.139	1.109	1.430
	(3.042)	(3.086)	(3.141)
10 Months Ago	2.071	0.467	-0.0749
	(2.673)	(2.639)	(2.659)
11 Months Ago	-0.197	-1.599	-0.608
	(2.053)	(2.036)	(2.125)
12 Months Ago	8.296**	5.799*	2.513
	(3.422)	(3.270)	(2.920)

Table 2.5: (continued)

Tropical Storm Dummy			
Concurrent Month	4.640***	3.905***	2.390**
	(0.919)	(0.893)	(1.011)
1 Month Ago	2.324**	2.469**	1.247
	(1.013)	(1.073)	(1.147)
2 Months Ago	0.467	1.270	0.868
	(0.978)	(1.027)	(1.054)
3 Months Ago	-0.544	0.172	0.568
	(0.862)	(0.900)	(0.889)
4 Months Ago	-1.986***	-1.130*	-0.988
	(0.577)	(0.595)	(0.651)
5 Months Ago	-1.091	-0.682	-0.0463
	(0.798)	(0.781)	(0.780)
6 Months Ago	-1.428*	-1.161*	-0.365
	(0.722)	(0.695)	(0.794)
7 Months Ago	-0.187	-0.0923	-0.327
	(0.850)	(0.873)	(0.967)
8 Months Ago	0.400	-0.0710	0.162
	(1.180)	(1.197)	(1.301)
9 Months Ago	1.197	0.158	0.658
	(0.828)	(0.867)	(1.013)
10 Months Ago	1.682**	0.819	0.904
	(0.846)	(0.871)	(0.985)
11 Months Ago	3.017***	2.075**	2.423**
	(0.886)	(0.882)	(0.988)
12 Months Ago	3.830***	2.546***	1.368
	(0.917)	(0.932)	(0.994)
Observations	11,748	11,748	11,748
DMA FE	Yes	Yes	Yes
Month FE	No	Yes	No
Year FE	No	Yes	No
Month-Year FE	No	No	Yes
Adjusted R-squared	0.138	0.150	0.180

Notes: Results are from three separate OLS regressions. Observations are at the DMA-month level. DMA Google search share of queries that include flood insurance is the dependent variable and dummy variables indicating hurricane and tropical storm strength wind strikes with lags are the independent variables. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix 1: Supplemental Analysis for Manuscript 2

This appendix provides information and analysis that supplements the analysis done in the main paper.

As discussed in the main paper, Google Trends search activity data for *flood insurance* is censored at zero for geographies where the raw number of searches for the population does not exceed an undisclosed threshold. With many zero values, logical estimators to use in our analysis could be the negative binomial or the tobit model. We are interested in within DMA variation over time, however, and the use of negative binomial and tobit estimators in panel data settings are controversial. For completeness, we present results from negative binomial and tobit models that included the full set of fixed effects that we use in the main analysis but refrain from exact interpretation of coefficients or marginal effects. Instead, we focus on coefficient direction and significance.

Table A1.1 serves as a robustness check to Table 2.2 in the main text that examines the effect of the max wind variable on *flood insurance* search activity in all available DMAs in the sample. Columns (1) through (3) present results using negative binomial models and Columns (4) to (6) present results using tobit models. Results confirm those found in the main paper. Both sets of models show positive and significant treatment effects during the month of the storm strike until one month after. The tobit models also show robust evidence that *flood insurance* interest also increases leading up to the one-year anniversary of a storm strike.

Table A1.2 serves as a robustness check to Table 2.3 in the main text that examines the heterogeneity of the effect of storm strikes on *flood insurance* search activity for all DMAs in the sample by separating storms into discrete indications of whether they were hurricanes or tropical storms. Columns (1) through (3) present results using negative binomial models and Columns (4) to (6) present results using tobit models. Results confirm those found in the main paper. Both sets of models show positive and significant treatment effects for hurricane strikes during the concurrent month until one month after. Both sets of models also show positive and significant treatment effects for tropical storms, although the magnitudes are smaller than for hurricane strikes.

Table A1.1: Effect of Storm Experience on Flood Insurance Google Searches on all DMAs

	Negative Binomial			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches
Max Wind (log)						
Concurrent Month	0.109***	0.0862***	0.0595***	2.110***	2.167***	1.703***
	(0.0246)	(0.0235)	(0.0220)	(0.350)	(0.361)	(0.401)
1 Month Ago	0.0802***	0.0866***	0.0653***	1.310***	1.797***	1.237***
	(0.0205)	(0.0201)	(0.0226)	(0.383)	(0.415)	(0.444)
2 Months Ago	0.00822	0.0277	0.0372	0.0135	0.818*	0.839*
	(0.0262)	(0.0270)	(0.0286)	(0.413)	(0.430)	(0.454)
3 Months Ago	-0.0105	0.0181	0.0138	-0.191	0.622	0.622
	(0.0233)	(0.0241)	(0.0250)	(0.374)	(0.395)	(0.382)
4 Months Ago	-0.0283	0.00915	0.0204	-0.623*	0.201	0.161
	(0.0208)	(0.0217)	(0.0277)	(0.322)	(0.332)	(0.343)
5 Months Ago	-0.00905	0.0184	0.0111	-0.277	0.329	0.486
	(0.0215)	(0.0227)	(0.0257)	(0.342)	(0.363)	(0.373)
6 Months Ago	0.00395	0.0206	0.0534*	-0.345	0.193	0.483
	(0.0261)	(0.0261)	(0.0304)	(0.368)	(0.375)	(0.404)
7 Months Ago	0.0301	0.0350	0.0504	0.0320	0.265	0.439
	(0.0360)	(0.0337)	(0.0374)	(0.384)	(0.394)	(0.430)
8 Months Ago	0.0521	0.0379	0.0355	0.0947	0.0941	0.160
	(0.0437)	(0.0426)	(0.0368)	(0.450)	(0.477)	(0.516)
9 Months Ago	0.0258	-0.00303	0.0361	0.414	0.167	0.586
	(0.0293)	(0.0286)	(0.0325)	(0.351)	(0.360)	(0.403)
10 Months Ago	0.0444*	0.0232	0.0267	0.302	0.164	0.250
	(0.0258)	(0.0260)	(0.0278)	(0.368)	(0.386)	(0.421)
11 Months Ago	0.0448**	0.0123	0.0362	0.901**	0.622*	0.869**
	(0.0222)	(0.0226)	(0.0241)	(0.358)	(0.377)	(0.402)
12 Months Ago	0.0696***	0.0269	-0.00843	1.417***	1.006***	0.558
	(0.0230)	(0.0234)	(0.0232)	(0.322)	(0.348)	(0.371)
Observations	22,440	22,440	22,440	22,440	22,440	22,440
DMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	No	Yes	No
Year FE	No	Yes	No	No	Yes	No
Month-Year FE	No	No	Yes	No	No	Yes
Log Likelihood	-58940	-58868	-58726	-54039	-53688	-53427

Notes: Results in Columns (1) to (3) are from three separate negative binomial regressions while results in columns (4) to (6) are from three separate tobit regressions. Observations are at the DMA-month level. DMA Google search share of queries that include flood insurance is the dependent variable and a log transformation of monthly maximum wind strength with lags are the independent variables. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A1.2: Effect of Storm Experience Heterogeneity on Flood Insurance Google Searches on all DMAs

	Negative Binomial			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches	Flood Insurance Searches
Hurricane Dummy						
Concurrent Month	1.105***	0.976***	0.763***	19.88***	21.99***	17.36***
	(0.222)	(0.218)	(0.236)	(5.508)	(5.333)	(5.314)
1 Month Ago	0.828***	0.772***	0.498***	15.93***	19.28***	14.11***
	(0.186)	(0.173)	(0.160)	(4.768)	(4.679)	(4.395)
2 Months Ago	0.335	0.337	0.351	0.966	5.914	6.144
	(0.252)	(0.246)	(0.268)	(5.397)	(5.428)	(5.429)
3 Months Ago	0.290	0.383*	0.311	0.278	6.360	5.178
	(0.216)	(0.210)	(0.232)	(4.376)	(4.094)	(4.014)
4 Months Ago	0.269	0.283	0.382	0.729	5.386	6.057
	(0.247)	(0.239)	(0.250)	(5.055)	(4.992)	(4.985)
5 Months Ago	0.220	0.344	0.231	1.081	4.866	5.790
	(0.195)	(0.223)	(0.227)	(3.810)	(4.268)	(4.225)
6 Months Ago	0.571*	0.544	0.688**	1.086	4.372	7.237
	(0.335)	(0.336)	(0.337)	(5.691)	(5.809)	(5.806)
7 Months Ago	0.761	0.707	0.769	3.695	6.740	8.526
	(0.618)	(0.578)	(0.578)	(5.411)	(5.527)	(5.460)
8 Months Ago	0.191	0.127	0.149	1.352	3.341	4.245
	(0.154)	(0.160)	(0.187)	(2.774)	(2.729)	(2.821)
9 Months Ago	0.749	0.572	0.722	0.972	2.105	3.555
	(0.617)	(0.597)	(0.569)	(5.023)	(5.089)	(5.114)
10 Months Ago	0.0491	0.00473	-0.0183	-0.725	1.257	0.489
	(0.165)	(0.171)	(0.225)	(4.062)	(4.098)	(4.185)
11 Months Ago	-0.0294	-0.146	0.0104	-1.879	-0.794	1.747
	(0.164)	(0.166)	(0.176)	(3.446)	(3.419)	(3.617)
12 Months Ago	0.452**	0.201	-0.00356	7.960*	7.600*	2.868
	(0.205)	(0.192)	(0.175)	(4.608)	(4.424)	(3.967)

Table A1.2: (continued)

Tropical Storm Dummy						
Concurrent Month	0.306***	0.230**	0.149*	6.470***	6.379***	4.938***
	(0.0946)	(0.0921)	(0.0850)	(1.330)	(1.398)	(1.533)
1 Month Ago	0.197**	0.243***	0.190**	2.847*	4.374***	2.631
	(0.0892)	(0.0892)	(0.0954)	(1.550)	(1.649)	(1.721)
2 Months Ago	-0.0450	0.0543	0.104	-0.751	2.003	2.128
	(0.108)	(0.115)	(0.119)	(1.552)	(1.666)	(1.732)
3 Months Ago	-0.150*	-0.0211	-0.0284	-1.608	1.042	1.159
	(0.0896)	(0.0948)	(0.0984)	(1.494)	(1.538)	(1.536)
4 Months Ago	-0.193**	-0.0239	0.00512	-3.073**	-0.181	-0.407
	(0.0810)	(0.0881)	(0.115)	(1.206)	(1.261)	(1.329)
5 Months Ago	-0.108	0.0152	-0.00155	-1.439	0.684	1.229
	(0.0936)	(0.0997)	(0.109)	(1.459)	(1.536)	(1.559)
6 Months Ago	-0.0784	0.00425	0.136	-1.658	0.259	1.159
	(0.0919)	(0.0955)	(0.118)	(1.301)	(1.336)	(1.500)
7 Months Ago	-0.0114	0.0243	0.0793	-0.591	0.0343	0.419
	(0.132)	(0.126)	(0.135)	(1.383)	(1.450)	(1.580)
8 Months Ago	0.151	0.106	0.0943	-0.0236	-0.308	-0.252
	(0.193)	(0.192)	(0.171)	(1.869)	(2.006)	(2.163)
9 Months Ago	0.00433	-0.0951	0.0470	1.729	0.466	1.995
	(0.105)	(0.105)	(0.127)	(1.437)	(1.485)	(1.664)
10 Months Ago	0.162	0.0768	0.102	1.748	0.839	1.281
	(0.0986)	(0.0982)	(0.106)	(1.453)	(1.553)	(1.662)
11 Months Ago	0.206**	0.0772	0.162*	4.649***	3.258**	4.055***
	(0.0857)	(0.0885)	(0.0945)	(1.395)	(1.484)	(1.550)
12 Months Ago	0.228**	0.0821	-0.0328	5.415***	3.664***	2.318
	(0.0893)	(0.0924)	(0.0934)	(1.326)	(1.422)	(1.501)
Observations	22,440	22,440	22,440	22,440	22,440	22,440
DMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	No	Yes	No
Year FE	No	Yes	No	No	Yes	No
Month-Year FE	No	No	Yes	No	No	Yes
Log Likelihood	-58927	-58859	-58719	-54029	-53676	-53417

Notes: Results in Columns (1) to (3) are from three separate negative binomial regressions while results in columns (4) to (6) are from three separate tobit regressions. Observations are at the DMA-month level. DMA Google search share of queries that include flood insurance is the dependent variable and a log transformation of monthly maximum wind strength with lags are the independent variables. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

MANUSCRIPT 3

How Does Municipal Policy Affect State and Local Actions?

Evidence from Land Conservation Spending

(To be submitted to Resource and Energy Economics, under first round of revision)

By

Corey Lang [a], Patrick Prendergast [a], and Shanna Pearson-Merkowitz [b]

[a] *Department of Environmental and Natural Resource Economics, University of Rhode Island, Kingston, RI*

[b] *Department of Political Science, University of Rhode Island, Kingston, RI*

Abstract

Understanding responses to government actions is critical for developing efficient policy. In the context of land conservation, this paper examines whether municipal policy has a crowding-in or crowding-out effect on neighboring municipalities' actions and state government actions. Importantly, we focus on municipal conservation referendums, which allow us to use a regression discontinuity framework for causal inference. Using data from Massachusetts and New Jersey, our findings suggest municipal conservation referendum decisions have no effect on neighboring local governments' or the state's conservation activity.

3.1 INTRODUCTION

One of paramount roles of government is the provision of public goods. In the United States, there are 30,000 municipal governments, and nested on top of that are county, state, and federal governments. When multiple governments can provide the same or similar public good, it is critical to understand if governments behave strategically with respect to other governments' actions. A wide variety of research focuses on how competition can cause government entities to react to the public good decisions of others which in turn affects the overall provisioning of public goods including charitable donations (Heutal, 2014), public school inputs (Millimet and Rangaprasad, 2006), and property tax rates (Bruickner and Saavedra, 2001), amongst others. This reactionary dynamic gives rise to many questions: when a government entity provides a public good, how does that affect the actions of government at different levels? How might this decision affect the decisions of neighboring governments? Do reactions have a crowd-in effect where provisioning for public goods increases, or does a crowding-out effect result?

In this paper, we address these questions in the context of land conservation. With about 2 million acres of farm, forest, and open space land being converted to development each year (Cordell et al. 2014), federal, state, and local governments have established themselves as important agents in curbing urbanization by accounting for about half of total conservation easement holdings in the United States (NCED, 2017). The overall production of conservation goals depends on, in part, the size and connectivity of conservation lands. The way governmental conservation agents react to the actions of other agents in conservation provisioning decisions holds implications for how successful we are at protecting our natural resources, supplying ecosystem services, providing outdoor recreational opportunities, and maintaining a representative sample of the full variety of biodiversity (Margules and Pressey 2000). In this paper, we test whether the passage of a conservation referendum in a municipality affects state level conservation activity in that and surrounding municipalities. We also test if there are spatial spillover effects among municipalities where local government activity influences the local government conservation activity of surrounding municipalities.

We build a panel dataset of conservation activity of multiple agents for Massachusetts and New Jersey. Both Massachusetts and New Jersey have state programs – the Community Preservation Act for Massachusetts and Green Acres for New Jersey – that incentivize municipal land conservation. This makes Massachusetts and New Jersey ideal places to study because of their substantial amount of conservation activity and available data. We collect state level conservation spending for Massachusetts from the Conservation Almanac and local government conservation referendum activity for Massachusetts and New Jersey from the Trust for Public Land, both at the municipal level.

Since residents vote on local government conservation referendums, we utilize the regression discontinuity (RD) framework developed by Cellini et al. (2010) to test whether the relationship between conservations agents among different levels of government and across space are causal.[17] Past studies in the conservation literature that test for spillovers of conservation activity typically use models that rely on correct

[17] Cellini et al. (2010) study how housing prices respond referendums authorizing school infrastructure spending in California. The dynamic RD method has been applied in a handful of papers since (e.g., Isen 2014, Martorell et al. 2016). Lang (2018) uses the same open space referendums data in this paper and examines housing price responses to authorization of conservation spending.

covariate selection to produce unbiased results (see Albers et al. 2008 and Parker and Thurman, 2011 as examples). Omission of key covariates in these instances may lead to results that are indicative of correlations instead of causal relationships. We believe we are the first to use a causal framework that controls for both observed and unobserved municipal characteristics to estimate conservation spillover effects that do not suffer from omitted variable bias. To highlight the importance of using a causal framework such as the dynamic RD model, we also produce cross-sectional (XS) and difference-in-difference (DID) estimates and contrast results.

Results from the dynamic RD framework suggest there is not a causal relationship between municipal level conservation referendum activity and state level conservation in the municipalities that pass conservation referendums and neighboring municipalities. We also do not find a causal relationship between municipal level conservation referendum activity among neighboring municipalities in both Massachusetts and New Jersey. There are two main implications of our findings. First, municipal governments may not need to be concerned about whether their conservation referendum activity crowds-out state level conservation and neighbor municipality conservation referendum activity in their town and surrounding areas. Conversely, they should not expect the state and surrounding municipalities to crowd-in additional conservation land in the area after a conservation referendum passage. Second, land conservation provisioning may be at an efficient level where surrounding towns do not need to compete with their neighbors through the allocation of conservation areas in order to attract residents. Our main results differ with results we obtain from XS and DID estimates, which show positive and statistically significant crowding-in effects between local and state conservation activity, as well as neighboring conservation activity. We interpret these differences as evidence of bias in the XS and DID estimates.

We contribute to the literature in two important ways. First, we believe we are the first to investigate whether the actions of conservation agents at different levels of government affect each other. Many papers in the public finance literature have investigated the dynamics between different levels of government in the context of setting consumption taxes (Besley and Rosen 1998), income and wealth taxes (Brülhart and Jametti 2006), and funding decisions for public schools (Cascio et al. 2013). Prominent papers in the land conservation literature have analyzed the effects public conservation activity has on private conservation activity (Parker and Thurman 2011; Albers, Ando, and Chen 2008; Lawley and Yang 2015). We extend this idea and test whether there is a reactionary dynamic between local and state governments when it comes to land conservation activity because such reactions can hold important implications for conservation efficiency.

Second, we use a causal framework to investigate the relationships between conservation agents instead of investigating spatial correlations that previous studies have identified. According to the public finance literature, public good decisions by a local government may cause a reaction to neighboring local governments because people can choose to move to a community with a level of public goods that fit their preferences (Tiebout 1956) or voters may judge their public officials based on the tax performance of politicians in surrounding areas in what is referred to as a yardstick competition (Besley and Case 1995, Bordignon et al. 2003). In addition, spatial spillovers often result due to strategic competition between neighboring jurisdictions when setting property tax rates

(Brueckner and Saavedra 2001), school inputs (Millimet and Rangaprasad 2006), and other public finances (Baicker 2005, Isen 2014). Similarly, studies in the land conservation literature find evidence of spatial clustering between conservation agents and voting outcomes (e.g., Albers and Ando 2003, Heintzelman et al. 2013, Altonji et al. 2016), but tend not to make causal claims either due to dataset limitations or the scope of the study.

We aim to add to the valuable insights provided by the land conservation literature by analyzing a novel dataset that allows us to identify conservation activity spillover effects in a quasi-experimental manner. We do not find evidence of the positive spillover effect between conservation agents that many studies find.

3.2 DATA

This section describes the four sources of data used in our analysis: 1) municipal level referendums and associated spending, 2) state government conservation spending, 3) land use characteristics, and 4) municipal demographics.

3.2.1 Land Conservation Referendum Data

Land conservation referendum data come from The Trust for Public Land's LandVote Database (The Trust for Public Land, LandVote, 2016) and spans the years 1996-2016. The data include proposed municipal level referendum information such as date, financial mechanism, total funds at stake, total funds approved, conservation funds at stake, conservation funds approved, as well as percentage of yes and no votes. Tables A2.1 and A2.2 in the online appendix show a yearly breakdown of municipal referendum activity for Massachusetts and New Jersey, respectively. On average, Massachusetts municipalities vote on 15 conservation related referendums a year, approve 10 of them, and dedicate about $28 million to conservation activities. Over the same time period, New Jersey municipalities experience more conservation referendum activity compared to Massachusetts. They vote on an average of 23 referendums a year, pass 17 of them, and dedicate about $66 million to conservation activities.

Figures 3.1 and 3.2 show the spatial distribution of Massachusetts and New Jersey municipal referendum activity, respectively. The top map in Figure 3.1 and the map in Figure 3.2 differentiate municipalities that have never passed a referendum from municipalities that have passed at least one referendum. Conservation referendum activity in Massachusetts seems to be primarily concentrated in the eastern part of the state. Though municipalities in western Massachusetts have also held referendums, many municipalities either never proposed a referendum or never passed one. Like for Massachusetts, there appears to be spatial patterns of referendum activity in New Jersey with activity being concentrated to the northern and western part of the state. Our analysis will allow us to determine if the spatial clustering of conservation referendum activity is caused by municipalities reacting to the conservation activity of their neighbors or is a function of observable and unobservable population characteristics that are spatially correlated.

3.2.2 State Conservation Spending Data

Due to data availability, we are only able to observe historical state conservation spending at the municipal level for Massachusetts. Data on Massachusetts state conservation spending come from The Trust for Public Land's Conservation Almanac (The Trust for Public Land, Conservation Almanac, 2016). The data include dollars spent on land conservation from state programs such as the Massachusetts Department of

Agricultural Resources (MDAR), Department of Conservation and Recreation (DCR), Department of Fish and Game (DFG), and others from 1998-2011. We aggregate dollar amounts by municipality by year for every year available.[18] On average, the state spends about $37 million on conservation in Massachusetts municipalities.[19]

The bottom map in Figure 3.1 shows the spatial distribution of state conservation spending per capita from 1998-2011 for Massachusetts. State conservation spending occurs throughout Massachusetts, with the heaviest concentration in the western part of the state and the least amount of activity in the eastern part of the state right before the state's peninsula, Cape Cod.

By comparing state conservation and referendum activity, we can get an initial assessment of how municipal and state conservation actions relate to one another. Referendum activity appears to have a few distinct pockets with a lot of activity in the entire eastern part of the state, where state spending is sparse, and a smaller concentration in the western part of the state right before the highest concentration of state spending. Visually, there seems to be a substitution effect of conservation vehicle where state conservation spending reacts to municipality referendum activity by increasing spending in municipalities that do not hold referenda or vice versa.[20] This may lead to the

[18] Table A2.1 in the appendix shows a yearly breakdown of state conservation spending.

[19] Massachusetts municipalities that adopt the Community Preservation Act (CPA) to preserve open space, affordable housing, and historical sites automatically receive funding from the CPA Trust Fund which disperses revenues collected from statewide real estate transactions each year. Inclusion of CPA Trust Fund revenues in state spending measurements for our models would be expected to upwardly bias the estimated relationship between local referendum passage and state conservation spending. We do not believe this is a concern with our dataset because, after multiple communications with the Trust for Public Land, it was determined that money spent from the CPA Trust Fund would most likely be reflected in local and not state expenditures in the Conservation Almanac dataset. This concern is further dispelled by the mostly negative (and insignificant) coefficient estimates from our causal framework models.

[20] This visual substitution effect holds when comparing only years where state spending and local referendum activity overlap. Figure A2.1 shows referendum activity in Massachusetts between 1998 and 2011.

conclusion that referendum activity crowds-out state conservation spending where Massachusetts conservation funds are focused on communities that may not have the resources or support to conserve on their own. Our main analysis allows us to investigate whether this relationship is causal.

3.2.3 Land Use Data

Municipalities that hold referendums are matched with land cover control variables. Acres available in each municipality for open space is calculated using GIS and the National Land Cover Database (NLCD) (Homer et al., 2015). The NLCD creates a pixelated map of the United States, available for years 2001, 2006, and 2011, where each pixel is assigned a category based on land use type. We use this information to calculate the percentage of total land within each municipality categorized as developed open space, forests, and grasslands to proxy for acres available for conservation for 2001 and 2011. We then linearly interpolate available acres for years in between and extrapolate for years before and after 2001 and 2011.

3.2.4 Demographic and Partisanship Data

Finally, municipalities that hold referendums are matched with municipal level socioeconomic data from the 2000 Census, 2010 Census, and 2010 American Community Survey. We collect data on municipal level median household income, population density, median house price, and proportion of residents under 18, over 65, white, black, and with a bachelor’s degree or higher. Sociodemographic values were interpolated for years between 2000 and 2010 and extrapolated for years before and after.

We use presidential election outcomes as a proxy for political ideology. For Massachusetts, we gathered results for each election at the municipal level between 1996 and 2016 from the Elections Division of the Secretary of the Commonwealth. For New Jersey, the same data was only available between 2004 and 2016 (from the Division of Elections). With this data, we calculate the Democrat share deviation, which equals the share of votes the Democrat candidate received in a given municipality minus the statewide Democrat vote share. This measurement accounts for changing candidate popularity and provides a better accounting of changes to partisanship over time (Lang and Pearson-Merkowitz 2015). As with census data, we interpolate Democrat share deviation for years between elections.

3.2.5 Links Between State and Local Conservation Activity

An important assumption in our analysis is that locally raised conservation funds and state conservation spending by state departments like MDAR, DCR, and DFG can be either complements (that could crowd-in each other) or substitutes (that could crowd-out each other). If the types of conservation projects that each funding source typically supports are not related to each other at all, then we would expect to see insignificant estimation results regardless of the appropriateness of methodology used. Massachusetts municipalities that adopt the Conservation Preservation Act (CPA) are incentivized to fund projects that preserve open space, affordable housing, historical sites, and recreation. Completed projects have funded agricultural preservation, bike trails, fish ladders, shellfish population preservation, among many others.[21]

There are no overt policy mechanisms that link voting behavior to state spending or neighboring municipality activity. There is very limited information about the motivations of individual towns. The Land Vote database provides the wording that is listed on the ballot and does not indicate explicit coordination with neighboring towns or with the state spending activity that we observe. Because of this, we fundamentally view municipal referendums as discrete

[21] For a full list of CPA related projects, please visit: http://communitypreservation.org/projects/new.

activities between municipalities rather than coordinated. At the state level, however, we do see evidence of coordinated efforts in the Conservation Almanac dataset. For example, the state may partner with the federal U.S. Fish and Wildlife and a municipality to purchase a land easement. In the end, however, it is unclear if state efforts are attracted or repulsed by municipal efforts in a causal way. We believe projects funded by towns using the CPA are related enough to what state departments MDAR, DCR, and DFG would focus on that it is plausible to test for crowding-in or crowding-out activity, and this is ultimately an empirical matter.

3.3 METHODOLOGY

3.3.1 Outcome Variable Construction

To assess the effect that municipal open space conservation has on other government decision making, we construct and test empirical models with four different outcome variables. The first outcome variable is the amount of state government conservation spending per capita in the municipality that passed the referendum. To form this variable, we sum state level spending for each municipality by year and normalize it by population. The second dependent variable is state government spending per capita in neighboring municipalities. To form this variable, we calculate annual state level conservation spending per capita and then calculate a weighted average of all municipalities that share a border with a given municipality with weights proportional to the length of border in common.[22] The third dependent variable is the number of open space referendums passed by neighboring municipalities. The last dependent variable is the amount of open space funding per capita approved by referendums held by

[22] The intuition behind this construction is that there is more likely to be strategic behavior between municipalities that share a longer border. Results are qualitatively similar with different weights.

neighboring municipalities. Both of these neighbor averages are similarly weighted by length of border.

3.3.2 Dynamic Regression Discontinuity Model

We begin with a simple model and build up to our preferred specification in order to build intuition. We are interested in whether municipal conservation decisions have any effect on state and neighboring municipality conservation decisions. We observe municipality *j* hold an open space referendum, and the measure passes if the vote margin, which equals the percent approval minus the percent required to pass, is greater than zero, i.e., $Pass_j = 1$ if $margin_j > 0$. We also observe our four outcome variables for government *i* that is linked to municipality *j*, denoted y_{ij}. Government *i* can be the state government or a municipality that neighbors *j*. A simple bivariate regression of the outcome on referendum passage would be:

$$y_{ij} = \alpha + \beta Pass_j + \varepsilon_{ij} \qquad (1)$$

Since voting outcomes are correlated with observable and unobservable municipality characteristics that are also likely correlated with state and neighbor actions, it is likely that $\hat{\beta}$ will be biased.

This endogeneity problem can be mitigated by applying the RD framework originally proposed by Thistlethwaite and Campbell (1960) that takes advantage of the continuous nature of vote margin. By flexibly controlling for the vote margin, we can essentially compare outcomes just below the passing threshold (the control group) and just above (the treatment group) where both observable and unobservable characteristics of municipalities holding referendums are most likely very similar. Transforming Equation (1) into an RD model, we get:

$$y_{ij} = \alpha + \beta Pass_j + f(margin_j, \gamma) + \varepsilon_{ij} \qquad (2)$$

where $f(\cdot)$ is a flexible polynomial and γ signifies the corresponding parameter. We use a cubic polynomial of vote margin in our main analysis, but also present results with linear and quadratic polynomials in the online appendix as a robustness check.[23]

Comparing outcomes for municipalities that are just below and just above the threshold results in a quasi-experiment where referendum passage is as good as randomly assigned, and the causal effect of referendum passage on other government conservation spending can be isolated. Election outcomes in an RD framework have been used to examine causal relationships between incumbency and election advantage in the House of Representatives (Lee, 2008), electoral support and legislator's voting behavior (Lee et al. 2004), political party affiliation and land use policies (Solé-Ollé and Viladecans-Marsal, 2013), legislator partisanship on city policing and fire protection expenditures (Gerber and Hopkins, 2011), and the spillover effects of incumbency in mixed election systems (Hainmueller and Kern, 2008).

While RD is a powerful research design for causal inference, we must further modify Equation (2) for this specific setting. Municipalities can and do hold more than one referendum, which necessitates incorporating dynamics into the model. Following the model developed by Cellini et al. (2010), we implement a dynamic RD estimator that conditions treatment effects on other referendums a community has held. Our preferred specification is:

[23] Gelman and Imbens (2014) argue that high order polynomials can lead to biased inference and should be avoided. We chose to use a cubic polynomial in our main specification because Cellini et al. (2010) and Lang (2018) use a cubic in similar setting. We admit this is ad hoc, which is why we present estimates using linear and quadratic polynomials in the online appendix (Tables A3 and A4). Results are similar regardless of polynomial order choice.

$$y_{ijt} = \alpha_j + \sum_{\tau=0}^{\bar{\tau}}[\beta_\tau Pass_{j,t-\tau} + f\left(margin_{j,t-\tau}, \gamma_\tau\right) + \mu_\tau Held_{j,t-\tau}] + \pi_t + \varepsilon_{ijt} \tag{3}$$

where *t* indicates the year of observation, τ is the number of years since a referendum, $Pass_{j,t-\tau}$ is a binary indicator for municipality *j* passing a referendum τ years prior to year *t*, $Held_{j,t-\tau}$ is a binary indicator for municipality *j* holding a referendum (this acts as an intercept to separate municipalities that do versus do not hold referendums in a given year), α_j is a municipality fixed effect, and π_t is a year fixed effect. Additionally, this specification allows the polynomial in vote margin to vary across lagged years. By controlling for the vote margin, past referendum activity, and municipality and year fixed effects in Equation (3), β_τ no longer suffers from the endogeneity problem that plagued Equation (1) and is interpreted as the causal effect that passing a conservation referendum has on another government τ years after the referendum is passed for municipalities that are near the vote margin threshold. Additionally, Equation (3) models time paths of government responses. Conserving land parcels or placing items on the ballot is not immediate, and thus the effect may be delayed or heterogeneous over time.[24]

3.4 REGRESSION DISCONTINUITY DIAGNOSTICS

The RD framework aims to replicate the identification of treatment effects from randomized experiments in settings where treatment is not randomly assigned. This is done by focusing regression analysis to observations just below and just above an arbitrary threshold where treatment assignment is as good as randomized due to the

[24] In the context of U.S. state capital tax policy, Chirinko and Wilson (2017) find that a dynamic specification is critical for understanding strategic responses.

similarity of observation characteristics and the inability of observations to affect the treatment outcome.

The key identifying assumption of the framework is the continuity of the conditional expectations of counterfactual observations below and above the threshold. This assumption may not be valid, however, if observations can manipulate their treatment status. Though very unlikely in our setting where municipalities use thousands of votes to determine the passage of a referendum, we can test for manipulation in a few ways. One way is to look at the density of observations around the threshold. If municipalities cannot manipulate their treatment status, we would expect a relatively smooth density of observations across the passage threshold. Another way is to analyze the similarity of municipality characteristics around the passage threshold. Municipalities can be similar in observable and unobservable ways. Although it is impossible to explicitly test for similarities in unobservable characteristics, we can compare observable municipality characteristics for municipalities that fail a referendum and municipalities that pass a referendum.

3.4.1 Referendum Vote Margin Density

Figure 3.3 shows the distribution of vote margins for all referendums held in Massachusetts from 1996-2016 in the form of a local polynomial density estimator of observations on either side of the passage threshold. Evidence of strategic behavior in voting outcomes would reveal a statistically significant difference in the frequency of vote margins just below and just above the threshold of a 0% vote margin. A visual inspection of the vote margin distribution shows an increase in frequency on the positive side of the threshold, but a density test for manipulation of the running variable proposed by Calonico et al. (2014) reveals an insignificant test statistic of 1.105 with a p-value of 0.269. This suggests manipulation of the running variable should not be an issue in our identification strategy for Massachusetts.

Figure 3.3 also shows the vote margin distribution for New Jersey referendums during the same time period. New Jersey municipality vote margins visually do not show the same jump that Massachusetts has around the threshold. Any worries of strategic behavior are further dismissed after the manipulation test reveals a statistically insignificant test statistic of -0.2975 with a p-value of 0.7661. Like for Massachusetts, this suggests that manipulation of the running variable should not be an issue for New Jersey.

3.4.2 Sociodemographic Balance

Table 3.1 presents the means and standard deviations of Massachusetts sociodemographic characteristics to investigate whether municipalities that have failed at least one conservation referendum are similar to those that have passed at least one referendum. Columns 1 and 2 show municipalities that have ever failed a referendum and municipalities that have ever passed a referendum are very similar in median income, percentage of population under the age of 18, percentage of population that is white, percentage of population that is black, population density, number of acres that are available for conservation, and median house price. Column 3 shows the results of a t-test between the means presented in Columns 1 and 2. There is a statistically significant difference between the two groups of municipalities when it comes to the percentage of population over the age of 65, the proportion of populations that have a bachelor's degree or higher, and Democrat share deviation.

RD makes a comparison at the threshold, and it is most important that there is balance, and hence no manipulation, at that point rather than across the whole distribution. Lee and Lemiuex (2010) suggest a way to test this balance, which is to estimate the RD model with the sociodemographic variables as the dependent variables and inspect for discontinuity at the threshold. Since we have many covariates, we follow Lee and Lemiuex's suggestion to perform a chi-squared test for the discontinuity to be zero for all covariates after running a Seemingly Unrelated Regression (SUR). Column (4) of Table 3.1 shows the results of the SUR model where each sociodemographic variable is a dependent variable with a dummy variable indicating a passed referendum and a cubic polynomial for vote margin as the independent variables. Individual coefficient estimates for the pass dummy variable are mostly not statistically different than zero, with the exception of proportion over age 65 (at the 10% level). However, a postestimation Chi^2 test does not allow for the rejection of the null hypothesis that each of the coefficients are equal to zero. Together with the results of the vote margin manipulation test, we are comfortable proceeding with the RD framework to analyze Massachusetts referendum data.

Table 3.2 repeats the same columns as in Table 3.1, but for New Jersey. Democrat share deviation is not included because those data are only available 2004 and after, which removes about one-third of observations. Column (3) shows that there are statistically significant differences in means between towns that have ever failed a conservation referendum and those that have ever passed a referendum in the proportion of population under the age of 18 and median house price. Estimation results of the SUR model in Column (4) show a statistically significant discontinuity for the proportion of the population over the age of 65, but the Chi^2 test shows the same conclusions as those for Massachusetts. This suggests we can use a RD framework to analyze New Jersey referendum data as well.

The strength of the regression discontinuity design is that it eliminates the endogeneity issue of omitted variable bias by analyzing outcomes in a way that makes variation in treatment exogenous. Omitted variable bias is not the only contributor of endogeneity, however. Reverse causality is also a concern for endogeneity in econometric settings. In our context, reverse causality would be a concern if regression results were being driven by the influence of state activity or neighbor conservation activity on a municipality passing a referendum instead of the other way around. Although regression discontinuity does not explicitly control for reverse causality (which is typically addressed through instrumental variables), we are not worried about it in our analysis due to the exogenous nature of treatment assignment in the regression discontinuity setting.

Reverse causality may be a concern in our setting if our dependent variables are influencing where towns fall on the vote margin spectrum. By analyzing treatment effects in a small neighborhood around the referendum passage threshold where municipalities are similar in observable and unobservable ways, the independent variables in our model are unlikely to be influenced by the dependent variables. We have already shown that town demographic characteristics do not influence vote margin outcomes around the passage threshold using SUR models. In the appendix, we use the same approach to show that prior state and neighbor conservation activity is not influencing referendum passage among municipalities close to the threshold, diminishing concerns of reverse causality.

3.5 RESULTS

Table 3.3 shows the main results from Equation (3). Columns 1-4 present results for Massachusetts with each column being a different outcome variable. Across columns, almost all of the coefficients are not statistically significantly different from zero, which suggests municipal referendum passage does not have any effect on state conservation spending, neighbor state conservation spending, or neighbor referendum activity in the year of the referendum and the years following. There is a statistically significant coefficient estimate for neighbor state spending per capita six years after a passed referendum, however, it is not robust to controlling for alternate vote margin polynomials.[25] Estimates are also inconsistent throughout time with coefficient signs switching between positive and negative magnitudes in each model.

Table 3.3 also produces the results from Equation (3) for New Jersey referendum activity. Consistent with the results for Massachusetts, nearly all coefficients are statistically insignificant, which suggests municipal referendum passage does not have a causal effect on neighbors' conservation referendum activity.

While there is no statistical evidence of strategic responses by other governments, we must caution against strong conclusions because our results are not precisely estimated zeros. Point estimates vary considerably across years and standard errors are large, meaning that within the bounds of what is statistically consistent with the data are economically meaningful strategic responses. We attempted to improve precision by including socioeconomic covariates that vary by year in Equation (3) and by combining data from Massachusetts and New Jersey, but neither are a panacea. These results are

[25] Robustness results that control for linear and quadratic polynomials of vote margin are included in the online appendix.

reported in the online appendix and have similar coefficient variation and standard errors. We proceed cautiously with the interpretation that there is no causal effect of municipal conservation on other governments' actions.

3.6 TESTING THE IMPORTANCE OF THE RESEARCH DESIGN

To better understand the importance of our dynamic RD modeling strategy, we also estimate cross-sectional (XS) and difference-in-differences (DID) models that address the same questions, and then we compare the results to our preferred results to assess bias in XS or DD models. The DID model analysis is performed on the same dataset as the dynamic RD model. The specification does not control for the referendum vote margin, but is otherwise identical to Equation (3), namely the specification still conditions on past referendum activity to account for municipalities that hold more than one referendum. For the XS analysis, we sum our outcome variables across years and the independent variable of interest is a binary indication of whether the municipality passed at least one conservation referendum over the whole time period. In the XS specification, we lose municipality fixed effects, but instead include a rich set of socioeconomic variables that are averaged across years. When the outcome variable measures actions taken in a neighboring municipality, the socioeconomic variables are averaged across neighbors, using the same weights (border length) as the dependent variable construction (see Section 3.1). Lastly, for the XS model, we include all municipalities, not just those that hold a referendum, though results are similar if we do not expand the sample in that way.

3.6.1 Cross-Sectional Analysis

Table 3.4 shows the results from XS regressions for Massachusetts (columns 1-4) and New Jersey (columns 5-6). All models regress the outcome variable (identified at the column header) on an indicator for referendum passage and a suite of socioeconomic variables.

Columns 1 and 2 of Table 3.4 show the correlations between a municipality passing a referendum and the amount of state funded conservation that happens in that municipality and in neighboring municipalities. Column 1 shows a statistically insignificant positive coefficient between a referendum passage and state conservation spending while Column 2 shows a statistically significant coefficient for the relationship between a municipality passing a referendum and state spending in neighboring municipalities.

Columns 3 and 4 show the conditional correlations between a municipality passing a referendum and the referendum activity of neighboring municipalities in Massachusetts. The results indicate a positive and statistically significant coefficient for the relationship between a municipality passing a referendum and the number of referendums their neighbors pass and the total conservation funds attached to those referendums. The results are quite similar for New Jersey, as shown in columns 5-6.

3.6.2 Difference-in-Differences Analysis

Table 3.5 presents regression results from the DD analysis for Massachusetts (columns 1-4) and New Jersey (columns 5-6). Columns 1-2 of Table 3.5 estimate the dynamic relationship between passing a conservation referendum and the amount of state conservation expenditure in the municipality that held the referendum and neighboring municipalities. These results have both positive and negative coefficients, and most are insignificant. Columns 3-4 show positive and statistically significant coefficients in the concurrent year, as well as a lag of seven years, which indicates some support for a crowd-in effect for neighboring municipalities. This finding is bolstered and more pronounced in New Jersey (columns 5-6), which shows positive and statistically significant coefficients in the concurrent year through a four year lag.

3.6.3 Comparison to the main results

The main results using the dynamic RD indicate that no causal effect of municipal open space referendums on other government conservation actions. The intuitive appeal of the dynamic RD model is that it controls for time-invariant and time-varying unobservables, which could lead to biased inference if not controlled for. However, the extent of bias is an empirical question for this given setting.

Both the XS and DD models do not find evidence of municipal actions affecting state actions in the municipality that holds a referendum, the same conclusion as the dynamic RD. Thus, in this case, we find no evidence of bias in this setting.

In contrast, XS and DD models do find evidence that municipal actions positively affect neighboring state and municipal actions, whereas the dynamic RD models indicated no effect. We interpret these differences as evidence of bias in the XS and DID estimates. We hypothesize that the XS and DD results reflect spatial correlations that are not adequately captured by socioeconomic control variables or municipality fixed effects. Supporting this idea, the DD models estimate a statistically significant positive effect in the concurrent year, which is near impossibly causal given that it takes time to strategically respond.

3.7 CONCLUSION

We use local government conservation referendum data from Massachusetts and New Jersey, two states with land conservation incentive programs, as well as state government conservation spending data from Massachusetts, to investigate the relationship between public conservation agents at different levels of government and across space. Using a RD framework, our results suggests there is not a causal relationship between the conservation referendum activity of local and state governments as well as between neighboring local governments.

By investigating whether there are spillover affects among public conservation agents at different levels of government and neighboring governments, we make two main contributions to the literature. First, we believe we are the first to investigate whether conservation agents in different layers of government react to each other. Prior literature investigates externalities between different levels of governments for other public goods, but not for land conservation. Second, our methodology allows us to investigate these relationships between public conservation agents in a more causal manner than what has been done in the past.

As urban sprawl in the United States continues to damage biodiversity and natural resources, communities can use land conservation as a tool to curb urban sprawl. The types of agents involved in conservation and how they react to each other will determine how efficient conservation actions will be. Our empirical setting is unique in that extensive municipal conservation voting allows for causal identification, however this may impact external validity. We choose to study two states that have state-level incentives for municipalities to take conservation actions. Results found here may not hold in states without these types of policies. One could imagine that state-level policies increase positive responses because municipalities face the same incentives and their state institutions see conservation as a priority. On the other hand, municipalities in states without conservation incentives may, in the face of scarcer resources, be more proactive in building off of neighbors' actions to enhance conservation benefits. Future research that examines states without strong land conservation incentive programs or uses a causal framework to examine the relationship between public conservation agents and private land trusts can also aid in the understanding of the efficiency of land conservation provisioning.

References

Albers, Heidi J., and Amy W. Ando. "Could state-level variation in the number of land trusts make economic sense?." *Land Economics* 79.3 (2003): 311-327.

Albers, Heidi J., Amy W. Ando, and Xiaoxuan Chen. "Spatial-econometric analysis of attraction and repulsion of private conservation by public reserves." *Journal of Environmental Economics and Management* 56.1 (2008): 33-49.

Albers, Heidi J., Amy W. Ando, and Michael Batz. "Patterns of multi-agent land conservation: crowding in/out, agglomeration, and policy." *Resource and Energy Economics* 30.4 (2008): 492-508.

Altonji, Matthew, Corey Lang, and Gavino Puggioni. "Can urban areas help sustain the preservation of open space? Evidence from statewide referenda." *Ecological Economics*130 (2016): 82-91.

Baicker, Katherine. "The spillover effects of state spending." *Journal of Public Economics* 89.2 (2005): 529-544.

Besley, Timothy, and Anne Case. "Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits." *The Quarterly Journal of Economics* 110.3 (1995): 769-798.

Besley, Timothy J., and Harvey S. Rosen. "Vertical externalities in tax setting: evidence from gasoline and cigarettes." *Journal of Public Economics* 70.3 (1998): 383-398.

Bordignon, Massimo, Floriana Cerniglia, and Federico Revelli. "In search of yardstick competition: a spatial analysis of *Italian municipality property tax setting." Journal of Urban Economics* 54.2 (2003): 199-217.

Brueckner, Jan K., and Luz A. Saavedra. "Do Local Governments Engage in Strategic Property—Tax Competition?." *National Tax Journal* (2001): 203-229.

Brülhart, Marius, and Mario Jametti. "Vertical versus horizontal tax externalities: An empirical test." *Journal of Public Economics* 90.10 (2006): 2027-2062.

Cascio, Elizabeth U., Nora Gordon, and Sarah Reber. "Local responses to federal grants: evidence from the introduction of title I in the South." *American Economic Journal: Economic Policy* 5.3 (2013): 126-159.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. "Robust data-driven inference in the regression-discontinuity design." *Stata Journal* 14.4 (2014): 909-946.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. "The value of school facility investments: Evidence from a dynamic regression discontinuity design." *The Quarterly Journal of Economics* 125.1 (2010).

Chirinko, Robert S., and Daniel J. Wilson. "Tax competition among US states: Racing to the bottom or riding on a seesaw?." *Journal of Public Economics* 155 (2017): 147-163.

Cordell, H. Ken, Carter Joseph Betz, and Stanley J. Zarnoch. "Recreation and protected land resources in the United States: a technical document supporting the Forest Service 2010 RPA Assessment." General Technical Report-Southern Research Station, USDA Forest Service SRS-169 (2013).

Gelman, Andrew, and Guido Imbens. "Why high-order polynomials should not be used in regression discontinuity designs." *Journal of Business & Economic Statistics* just-accepted (2017).

Gerber, Elisabeth R., and Daniel J. Hopkins. "When mayors matter: estimating the impact of mayoral partisanship on city policy." *American Journal of Political Science* 55.2 (2011): 326-339.

Hainmueller, Jens, and Holger Lutz Kern. "Incumbency as a source of spillover effects in mixed electoral systems: Evidence from a regression-discontinuity design." *Electoral studies* 27.2 (2008): 213-227.

Heintzelman, Martin D., Patrick J. Walsh, and Dustin J. Grzeskowiak. "Explaining the appearance and success of open space referenda." *Ecological Economics* 95 (2013): 108-117.

Heutel, Garth. "Crowding out and crowding in of private donations and government grants." *Public Finance Review* 42.2 (2014): 143-175.

Homer, C.G., Dewitz, J.A., Yang, L., Jin, S., Danielson, P., Xian, G., Coulston, J., Herold, N.D., Wickham, J.D., and Megown, K., 2015, Completion of the 2011 National Land Cover Database for the conterminous United States-Representing a decade of land cover change information. *Photogrammetric Engineering and Remote Sensing*, v. 81, no. 5, p. 345-354

Isen, Adam. "Do local government fiscal spillovers exist? Evidence from counties, municipalities, and school districts." *Journal of Public Economics* 110 (2014): 57-73.

Lang, Corey. "Assessing the efficiency of local open space provision." *Journal of Public Economics* 158 (2018): 12-24.

Lang, Corey, and Shanna Pearson-Merkowitz. "Partisan sorting in the United States, 1972–2012: new evidence from a dynamic analysis." *Political Geography* 48 (2015): 119-129.

Lawley, Chad, and Wanhong Yang. "Spatial interactions in habitat conservation: Evidence from prairie pothole easements." *Journal of Environmental Economics and Management* 71 (2015): 71-89.

Lee, David S. "Randomized experiments from non-random selection in US House elections." *Journal of Econometrics* 142.2 (2008): 675-697.

Lee, David S., Enrico Moretti, and Matthew J. Butler. "Do voters affect or elect policies? Evidence from the US House." *The Quarterly Journal of Economics* 119.3 (2004): 807-859.

Lee, David S., and Thomas Lemieux. "Regression discontinuity designs in economics." *Journal of economic literature* 48.2 (2010): 281-355.

Margules, Chris R., and Robert L. Pressey. "Systematic conservation planning." *Nature* 405.6783 (2000): 243-253.

Martorell, Paco, Kevin Stange, and Isaac McFarlin Jr. "Investing in schools: capital spending, facility conditions, and student achievement." *Journal of Public Economics* 140 (2016): 13-29.

Millimet, Daniel L., and Vasudha Rangaprasad. "Strategic competition amongst public schools." *Regional Science and Urban Economics* 37.2 (2007): 199-219.

NCED. (2017). All States and All Easements. Retrieved April 3, 2017, Available at: http://http://nced.conservationregistry.org/reports/easements.

Parker, Dominic P., and Walter N. Thurman. "Crowding out open space: the effects of federal land programs on private land trust conservation." *Land Economics* 87.2 (2011): 202-222.

Solé-Ollé, Albert, and Elisabet Viladecans-Marsal. "Do political parties matter for local land use policies?." *Journal of Urban Economics* 78 (2013): 42-56.

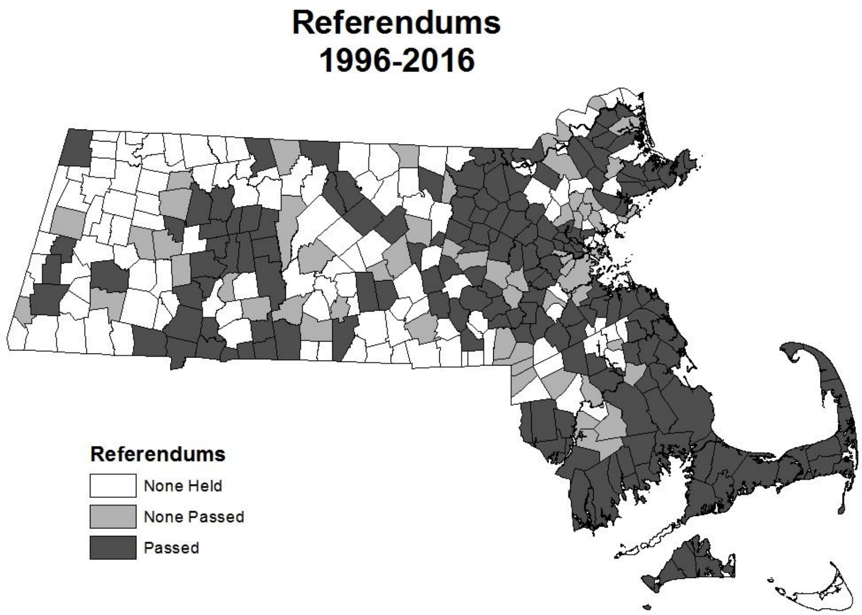
The Trust for Public Land, Conservation Almanac, 2016. www.conservationalmanac.org

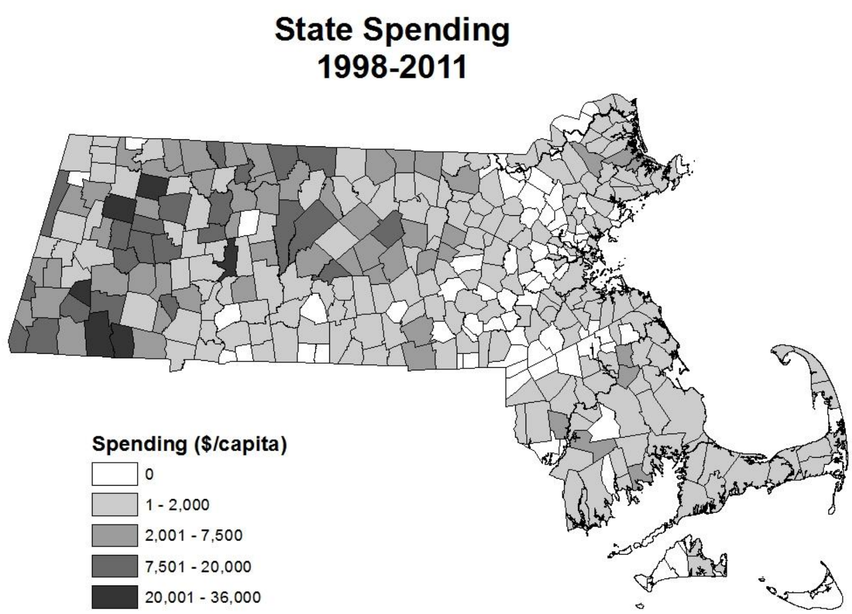
The Trust for Public Land, LandVote, 2016. www.landvote.org

Thistlethwaite, Donald L., and Donald T. Campbell. "Regression-discontinuity analysis: An alternative to the ex post facto experiment." *Journal of Educational psychology* 51.6 (1960): 309.

Tiebout, Charles M. "A pure theory of local expenditures." *Journal of Political Economy* 64.5 (1956): 416-424.

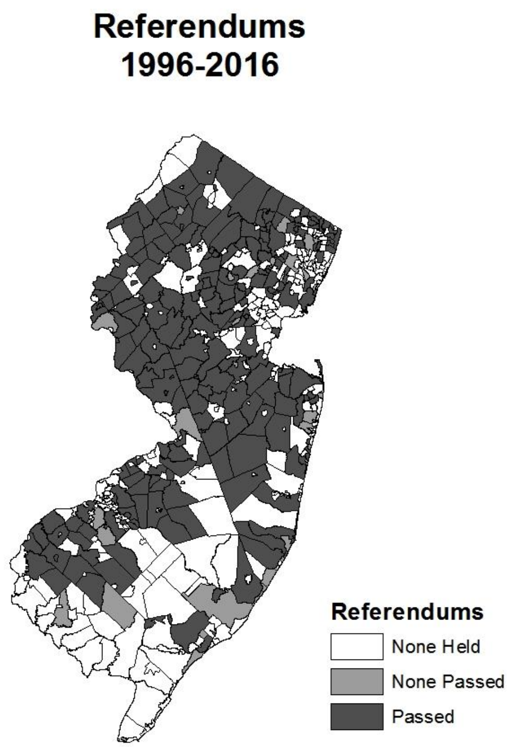
Figure 3.1: Massachusetts Land Conservation Activity





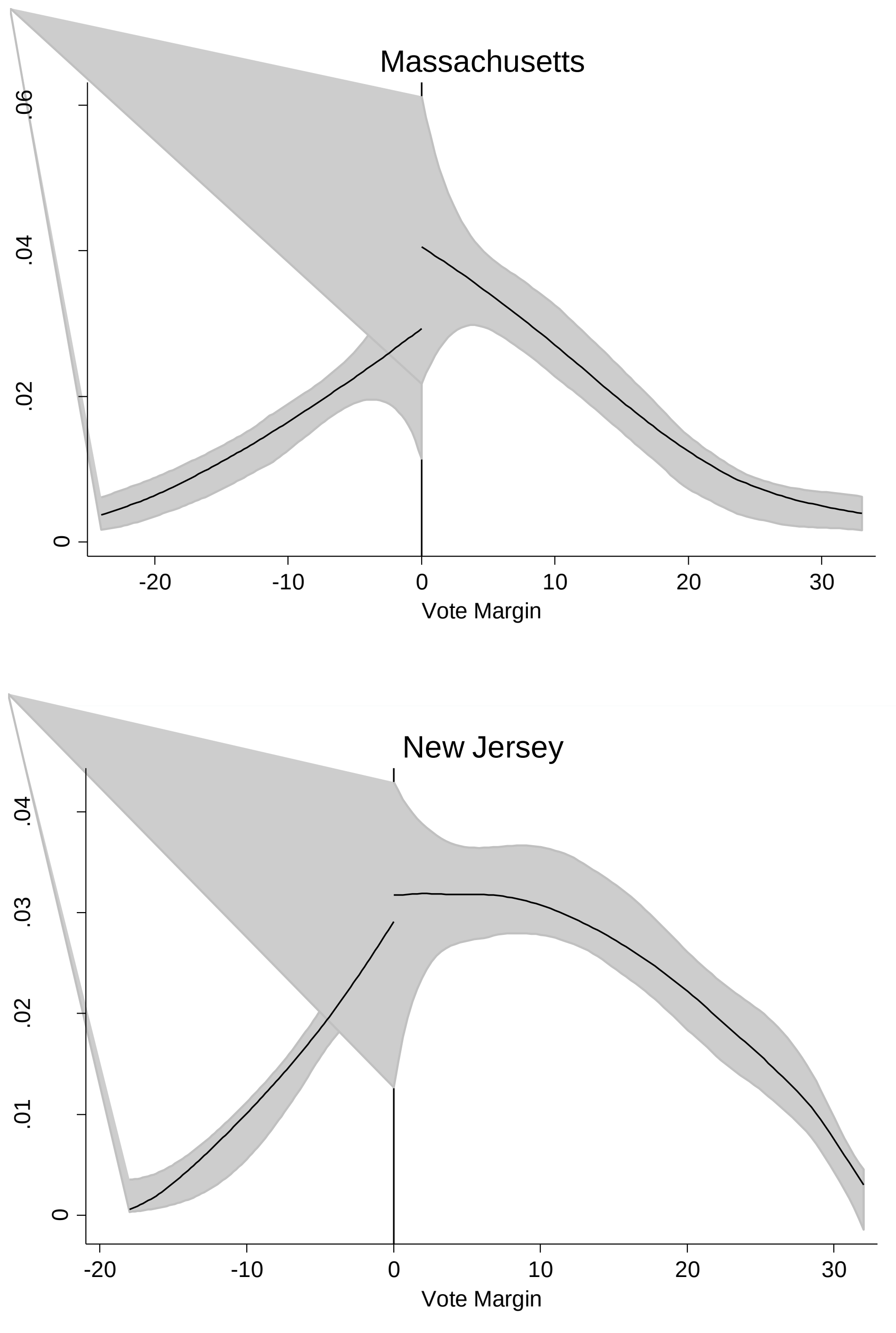
Notes: Figures show referendum and state spending activity for conservation in Massachusetts municipalities.

Figure 3.2: New Jersey Land Conservation Activity



Notes: Figure shows referendum activity for land conservation in New Jersey municipalities.

Figure 3.3: Distribution of referendum voting by margin for Massachusetts and New Jersey



Notes: Graphs are visualizations of manipulation tests for open space referendum vote margins in Massachusetts and New Jersey using a quadratic local-polynomial to construct the density point estimator and a cubic polynomial to construct the bias-corrected density point estimator. Solid lines are density point estimates and the shaded areas are 95% confidence intervals. Test statistics of density discontinuity are insignificant for both states.

Table 3.1: Sociodemographic Balance of Treatment and Control Groups for Massachusetts

	(1)	(2)	(3)	(4)
	Municipalities that ever fail a referendum (std. dev.)	Municipalities that ever pass a referendum (std. dev.)	Difference of means (t stat)	SUR model difference (std. error)
Median Income ($)	88,977	88,633	-344	-8,071
	(24,441)	(29,264)	(-0.11)	(5,955)
Bachelor's Degree or more (%)	40.06	43.27	3.22*	-1.941
	(14.11)	(15.21)	(1.82)	(3.288)
Population Under 18 (%)	23.45	22.96	-0.49	-0.837
	(4.16)	(5.05)	(-0.88)	(1.017)
Population Over 65 (%)	13.64	15.16	1.52***	2.114*
	(3.91)	(6.02)	(2.75)	(1.095)
White Population (%)	93.25	92.51	-0.74	-0.729
	(6.73)	(7.57)	(-0.87)	(1.583)
Black Population (%)	1.78	1.86	0.08	0.272
	(3.08)	(2.96)	(0.24)	(0.653)
Population Density	1,305	1,096	-210	-197.1
	(2,044)	(2,047)	(-0.88)	(446.0)
Available Acres	8,923	8,992	68	634.3
	(6,607)	(6,830)	(0.09)	(1,471)
Median House Price ($)	366,801	397,879	31,078	-17,556
	(131,672)	(171,608)	(1.68)	(34,321)
Democrat Vote Share Margin (%)	-4.80	-2.92	1.88*	1.968
	(8.52)	(9.96)	(1.70)	(2.044)
Observations	115	203	318	318
Number of Municipalities	96	171	267	267
Vote Margin Polynomial				Cubic
Chi^2 Test				8.07
Prob > Chi^2				0.6215

Notes: Demographic data is for the year the referendum was held. Values were interpolated/extrapolated from the 2000 Census and 2010 Census or ACS, NLCD database for 2001 and 2011, and the Elections Division of the Secretary of the Commonwealth of Massachusetts. Results for Column (4) are from seemingly unrelated regressions where the error terms are assumed to be correlated between individual regression equations where municipality demographics were the dependent variable and the exogenous explanatory variables were a dummy variable for a passed referendum and a cubic vote margin polynomial. Coefficient estimates for the pass dummy variable are shown. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3.2: Sociodemographic Balance of Treatment and Control Groups for New Jersey

	(1)	(2)	(3)	(4)
	Municipalities that ever fail a referendum (std. dev.)	Municipalities that ever pass a referendum (std. dev.)	t-test difference (t stat)	SUR model estimated difference (std. error)
Median Income ($)	105,423	106,291	868	-1,371
	(31,480)	(29,697)	(0.26)	(6,059)
Bachelor's Degree or more (%)	39.79	40.56	0.77	-0.312
	(15.19)	(16.23)	(0.43)	(3.212)
Population Under 18 (%)	24.47	25.23	0.77*	0.813
	(3.65)	(4.09)	(1.73)	(0.791)
Population Over 65 (%)	14.42	13.47	-0.95	-2.448**
	(4.77)	(6.22)	(-1.45)	(1.192)
White Population (%)	85.61	87.24	1.63	-0.320
	(10.31)	(12.10)	(1.25)	(2.345)
Black Population (%)	4.24	4.59	0.35	1.870
	(5.81)	(7.94)	(0.42)	(1.492)
Population Density	1,968	1,802	-166	65.60
	(1,838)	(2,830)	(-0.57)	(530.0)
Available Acres	7,765	7,965	201	2,178
	(8,963)	(8,000)	(0.22)	(1,647)
Median House Price ($)	440,913	379,184	-61,730***	-35,759
	(235,050)	(193,801)	(-2.73)	(41,022)
Observations	105	357	462	462
Number of Municipalities	77	235	312	312
Vote Margin Polynomial				Cubic
Chi^2 Test				9.53
Prob > Chi^2				0.3897

Notes: Results for Columns (1) to (3) are from t-tests between municipalities that have ever failed a conservation referendum and municipalities that have ever passed a referendum. Demographic data is for the year the referendum was held. Values were interpolated/extrapolated from the 2000 Census and 2010 Census or ACS, NLCD database for 2001 and 2011. Results for Column (4) are from seemingly unrelated regressions where the error terms are assumed to be correlated between individual regression equations. Municipality demographics were the dependent variables and the exogenous explanatory variables were a dummy variable for a passed referendum and a cubic vote margin polynomial. Coefficient estimates for the pass dummy variable are shown. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3.3: The Effect of Passing a Conservation Referendum on Own and Neighbor State Spending and Neighbor Referendum Activity

	(1)	(2)	(3)	(4)	(5)	(6)
	Massachusetts				New Jersey	
VARIABLES	State Spending per capita (log)	Neighbor State Spending per capita (log)	Neighbor Refs Passed per neighbor	Neighbor Funds Approved per capita (log)	Neighbor Refs Passed per neighbor	Neighbor Funds Approved per capita (log)
Pass Concurrent Year	0.258	-0.839	0.013	-0.343	0.040	0.091
	(0.455)	(0.547)	(0.060)	(0.547)	(0.054)	(0.669)
Pass 1 Year Ago	0.631	0.363	-0.023	0.175	-0.031	-0.946*
	(0.644)	(0.512)	(0.042)	(0.451)	(0.040)	(0.560)
Pass 2 Years Ago	-0.113	0.092	0.005	-0.042	-0.061	-0.344
	(0.525)	(0.382)	(0.026)	(0.323)	(0.040)	(0.559)
Pass 3 Years Ago	0.349	-0.183	0.023	0.005	-0.010	-0.287
	(0.416)	(0.470)	(0.048)	(0.598)	(0.035)	(0.525)
Pass 4 Years Ago	0.081	0.036	0.011	0.390	-0.024	-0.080
	(0.482)	(0.491)	(0.035)	(0.439)	(0.025)	(0.463)
Pass 5 Years Ago	0.004	-0.432	0.007	0.065	-0.029	-0.352
	(0.852)	(0.624)	(0.031)	(0.446)	(0.023)	(0.378)
Pass 6 Years Ago	-0.087	1.224***	0.049	0.287	-0.017	-0.293
	(0.456)	(0.442)	(0.042)	(0.441)	(0.028)	(0.410)
Pass 7 Years Ago	-0.369	0.950	-0.053	-0.211	0.005	0.109
	(0.660)	(0.724)	(0.055)	(0.517)	(0.022)	(0.370)
Observations	3,220	3,220	4,830	4,830	5,565	5,565
Adjusted R-squared	0.251	0.375	0.212	0.159	0.134	0.172
Vote Margin Polynomial	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column is a separate regression. Standard errors are shown in parentheses and are clustered at the town level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3.4: Cross-sectional Relationship between Passing a Conservation Referendum with Own and Neighbor State Spending and Referendum Activity

	(1)	(2)	(3)	(4)	(5)	(6)
	Massachusetts				New Jersey	
VARIABLES	State Spending per Capita (log)	Neighbor State Spending per capita (log)	Neighbor Refs Passed per neighbor	Neighbor Funds Approved per capita (log)	Neighbor Refs Passed per neighbor	Neighbor Funds Approved per capita (log)
Passed a Referendum	0.168	0.406**	0.265***	0.553***	0.159***	0.674***
	(0.230)	(0.175)	(0.039)	(0.156)	(0.049)	(0.161)
Demographics						
Median Income (log)	0.631	0.168	-0.931***	-1.325	0.720***	3.541***
	(0.986)	(1.206)	(0.267)	(1.074)	(0.268)	(0.878)
Bachelor's Degree or Higher (%)	0.012	0.031*	0.016***	0.041**	0.008	0.040**
	(0.015)	(0.018)	(0.004)	(0.016)	(0.005)	(0.018)
Under 18 Years Old (%)	-0.101**	-0.260***	-0.025*	-0.058	0.008	-0.037
	(0.042)	(0.058)	(0.013)	(0.052)	(0.014)	(0.045)
Over 65 Years Old (%)	0.006	-0.085**	0.032***	0.033	0.015**	0.027
	(0.033)	(0.038)	(0.008)	(0.034)	(0.007)	(0.024)
White Population (%)	0.013	0.021	0.006	0.047	0.011**	-0.001
	(0.027)	(0.039)	(0.009)	(0.035)	(0.005)	(0.016)
Black Population (%)	0.073	0.028	-0.008	-0.002	-0.001	0.014
	(0.054)	(0.071)	(0.016)	(0.063)	(0.005)	(0.017)
Population Density (log)	-0.827***	0.000	0.135***	0.519***	-0.047	0.268**
	(0.106)	(0.134)	(0.030)	(0.120)	(0.034)	(0.110)
Acres Available (log)	0.000***	1.058***	0.173***	0.762***	0.155***	0.645***
	(0.000)	(0.187)	(0.041)	(0.167)	(0.027)	(0.089)
Median House Price (log)	-1.134*	-0.494	0.593***	2.500***	-0.002	-1.319**
	(0.590)	(0.559)	(0.124)	(0.498)	(0.180)	(0.589)
Democratic Vote Margin	-0.021	-0.085***	-0.023***	-0.016	0.023***	-0.001
	(0.019)	(0.020)	(0.004)	(0.018)	(0.006)	(0.020)
Observations	349	349	349	349	565	565
R-squared	0.495	0.357	0.564	0.466	0.399	0.459
Years	1998-2011	1998-2011	1996-2016	1996-2016	1996-2016	1996-2016
Municipality Characteristics	Own	Avg Nbr	Avg Nbr	Avg Nbr	Avg Nbr	Avg Nbr

Notes: Each column is a separate regression where the independent variables include a dummy variable indicating whether a municipality passed at least one referendum and municipality or neighbor demographic variables. "Own" demographic characteristics are the demographic variables for the municipality that holds a referendum averaged over the years indicated. "Average neighbor" demographic characteristics are the average of demographics variables of towns that border the municipality that holds a referendum, weighted by border length. Standard errors are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3.5: Difference-in-Differences Estimates of the Effect of Passing a Conservation Referendum on Own and Neighbor State Spending and Neighbor Referendum Activity

	(1)	(2)	(3)	(4)	(5)	(6)
	Massachusetts				New Jersey	
VARIABLES	State Spending per capita (log)	Neighbor State Spending per capita (log)	Neighbor Refs Passed per neighbor	Neighbor Funds Approved per capita (log)	Neighbor Refs Passed per neighbor	Neighbor Funds Approved per capita (log)
Pass Concurrent Year	-0.072	-0.117	0.165***	1.097***	0.039***	0.425***
	(0.110)	(0.103)	(0.027)	(0.197)	(0.010)	(0.109)
Pass 1 Year Ago	0.087	0.150	0.011	0.237**	0.018**	0.305***
	(0.127)	(0.119)	(0.010)	(0.106)	(0.009)	(0.108)
Pass 2 Years Ago	0.135	-0.099	0.005	0.113	0.020**	0.427***
	(0.122)	(0.106)	(0.007)	(0.101)	(0.008)	(0.097)
Pass 3 Years Ago	-0.062	-0.088	0.002	0.092	0.020**	0.350***
	(0.112)	(0.097)	(0.009)	(0.115)	(0.008)	(0.104)
Pass 4 Years Ago	0.098	0.066	-0.001	0.127	0.005	0.133
	(0.136)	(0.115)	(0.008)	(0.100)	(0.007)	(0.090)
Pass 5 Years Ago	0.040	-0.043	-0.004	-0.036	-0.003	0.020
	(0.119)	(0.118)	(0.006)	(0.075)	(0.006)	(0.094)
Pass 6 Years Ago	-0.189*	0.058	0.009	0.047	0.006	0.115
	(0.101)	-0.108	(0.008)	(0.087)	(0.006)	(0.085)
Pass 7 Years Ago	0.077	-0.013	0.066***	0.475***	0.002	0.117
	(0.158)	(0.126)	(0.014)	(0.106)	(0.006)	(0.089)
Observations	3,220	3,220	4,830	4,830	5,565	5,565
Adjusted R-squared	0.256	0.371	0.176	0.145	0.138	0.175
Vote Margin Polynomial	None	None	None	None	None	None
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column is a separate regression. Standard errors are shown in parentheses and are clustered at the town level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Appendix 2: Supplemental Analysis for Manuscript 3

This appendix provides supplemental figures, statistics, and results to our main paper.

Figure A2.1 shows Massachusetts referendum activity from 2008 to 2011 for a more direct comparison to our state spending data. The map looks almost identical to Figure 3.1 with a visual appearance of a substitution effect between state spending and referendum activity.

Studies that use RD in their analysis typically present RD plots that fit separate lines to the relationship between a running variable, such as vote margin, and the dependent variable in question below and above a threshold to show the discontinuity in the outcome variable that results from treatment. While this is a good practice to build intuition for interpreting statistically estimated results, it is harder to do in our situation where a dynamic framework would call for multiple plots across time. The use of municipality and year fixed effects in our model further complicates the visualization of the relationships we estimate in one graph. Regardless, we present RD plots which are more akin to cross-sectional results than dynamic results for Massachusetts and New Jersey referendums that were held in the previous year in Figure A2.2.

Figure A2.2 shows regression discontinuity plots for state conservation spending per capita, neighbor state conservation spending per capita, neighbor conservation referendums passed per neighbor, and neighbor conservation funds approved per capita for Massachusetts and New Jersey. Dependent variables are grouped into 2% vote margin bins. The visualized relationships are not directly comparable with the coefficients we estimate from Equation (3) in the main paper due to the inability to present dynamic results and control for municipality and year fixed effects. The small discontinuities between municipalities that barely fail a referendum and those that barely pass a referendum for most of the dependent variables are consistent with our estimations for Massachusetts. The plots for New Jersey seem to show consistent discontinuities between treated and untreated municipalities which suggests treatment may affect neighboring municipality conservation activity which is not consistent with our New Jersey estimations.

Tables A2.1 and A2.2 show the yearly breakdown of aggregate local conservation referendum activity and state conservation spending activity where available for Massachusetts and New Jersey, respectively. These tables show the prevalence of conservation activity in each state from 1996-2016 for referendums and 1998-2011 for state conservation spending.

Table A2.3 serves as a robustness check to Table 3.3 in the main paper by controlling for different vote margin polynomials for Massachusetts. In general, these regressions confirm the insignificant results found with a cubic polynomial. The regressions that use a linear polynomial of vote margin show positive and significant crowding-in results for neighboring conservation activity 1-2 years after a passed referendum, but this result is not robust to controlling for quadratic and cubic vote margin polynomials.

Table A2.4 serves as a robustness check to Table 3.3 in the main paper by controlling for different vote margin polynomials for New Jersey. Controlling for linear and quadratic vote margin polynomials do not reveal any statistically significant results other than a significant coefficient for neighbor referendums passed two years after at the

10% level. This estimate is not robust to controlling for linear and cubic vote margin polynomials, however.

Table A2.5 serves as a robustness check to Table 3.3 in the main text by including town level sociodemographic variables in the dynamic regression discontinuity model. Sociodemographic variables add explanatory power to the model with significant coefficient estimates, however, coefficient estimates for the dynamic effect of passing a referendum remain insignificant.

Table A2.6 serves as a final robustness check to Table 3.3 in the main text by pooling together referendum activity in Massachusetts and New Jersey. Standard errors are slightly lower for pooled coefficient estimates compared to individual state results, however, coefficient estimates are still statistically insignificant and inconsistent throughout time.

Table A2.7 serves as a test of reverse causality in the dynamic regression discontinuity setting. If our dependent variables are influencing treatment status, then we would expect to see prior state and neighbor conservation activity to influence municipality vote margins around the threshold. To test this, we use SUR models to see if there are discontinuities between towns that barely fail and barely pass a conservation referendum based on prior (and concurrent year) state and neighbor conservation activity. SUR models for each state are the same models run in Table 3.1 for Massachusetts and Table 3.2 for New Jersey in the main text, but with the additional concurrent and prior year dependent variables. Coefficient estimates for town demographics are left out of the table for brevity.

Estimates for Massachusetts show that there are statistically significant discontinuities between municipalities that barely fail and barely pass conservation referendums based on the number of referendums passed by their neighbors during the year a municipality holds a referendum (Column 1) and the number of referendums passed by their neighbors the year before the municipality holds a referendum (Column 2). A postestimation Chi^2 test does not allow for the rejection of the null hypothesis that each of the coefficients are equal to zero, however. A similar story is seen with New Jersey where there is a statistically significant discontinuity between municipalities that barely fail and barely pass a conservation referendum based on the amount of conservation funds passed by their neighbors during the year they hold a referendum (Column 4). A postestimation Chi^2 test does not allow for the rejection of the null hypothesis that each of the coefficients are equal to zero, as well.

Figure A2.1: Massachusetts Referendum Activity 1998-2011

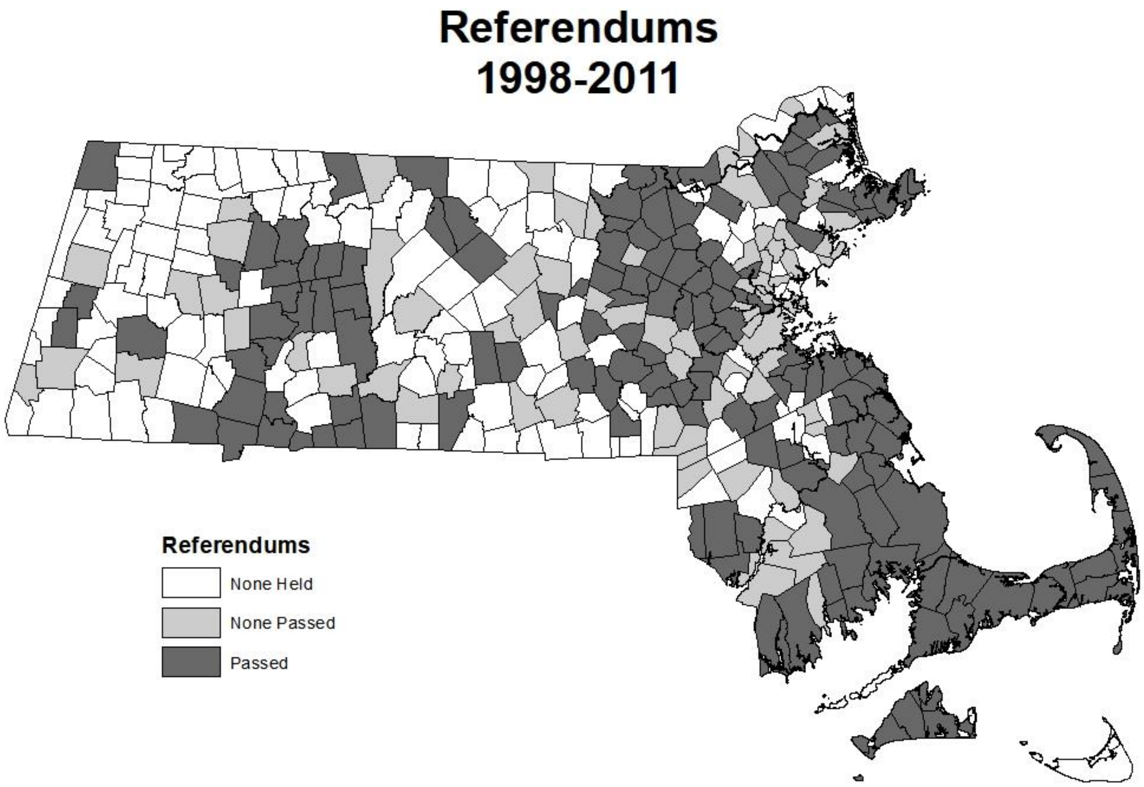
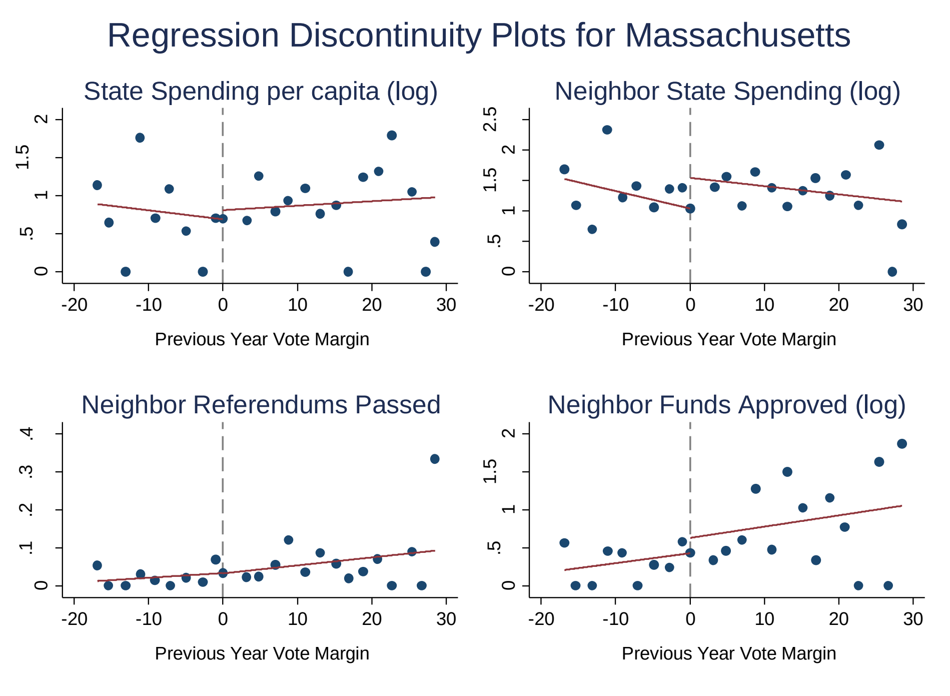


Figure A2.2: Static Regression Discontinuity Plots



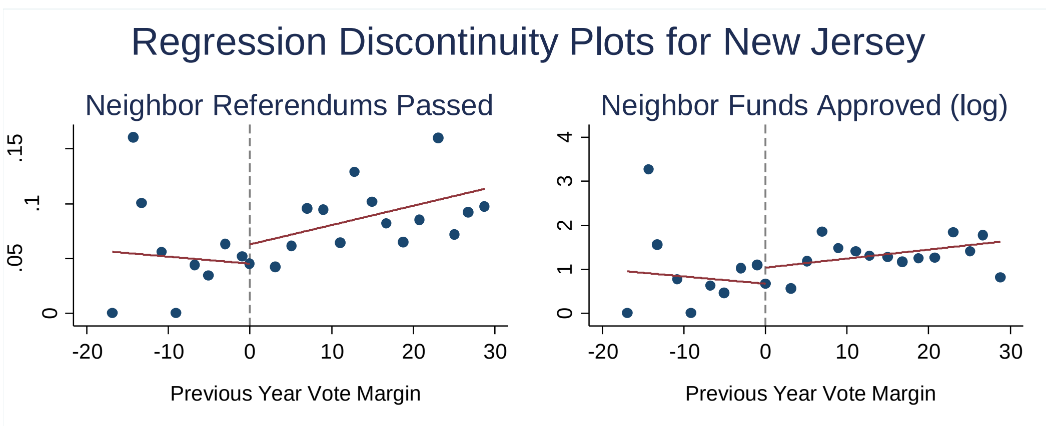


Table A2.1: Referendum and State Spending Conservation Statistics for Massachusetts

Year	Referendums Held	Referendums Passed	Conservation Funds Approved	State Spending
1996	1	0	$0	-
1997	5	5	$18,398,000	-
1998	18	17	$101,431,459	$4,758,851
1999	0	0	$0	$54,716,714
2000	3	3	$17,415,000	$43,344,154
2001	67	35	$109,830,772	$51,897,637
2002	46	22	$47,546,162	$74,827,403
2003	6	4	$6,395,006	$12,779,610
2004	18	17	$48,858,902	$13,997,265
2005	31	28	$94,592,119	$27,484,753
2006	36	18	$30,629,839	$43,118,496
2007	17	11	$23,677,068	$35,980,955
2008	21	14	$15,425,933	$47,758,589
2009	4	2	$2,747,987	$45,240,840
2010	7	6	$9,404,884	$42,221,711
2011	4	3	$1,417,860	$21,985,087
2012	11	7	$28,113,508	-
2013	2	0	$0	-
2014	13	6	$19,154,148	-
2015	4	3	$12,499,016	-
2016	4	2	$2,309,939	-
Average	15	10	$28,087,981	$37,150,862

Table A2.2: Referendum and State Spending Conservation Statistics for New Jersey

Year	Referendums Held	Referendums Passed	Conservation Funds Approved
1996	7	6	$54,383,998
1997	8	6	$28,882,368
1998	55	46	$234,091,328
1999	38	36	$116,858,202
2000	43	42	$168,405,307
2001	49	43	$115,493,091
2002	30	24	$123,695,970
2003	37	28	$98,943,898
2004	40	29	$64,091,930
2005	29	20	$45,275,606
2006	23	14	$39,362,914
2007	24	11	$82,538,428
2008	21	13	$37,753,200
2009	7	2	$3,404,981
2010	5	3	$1,724,147
2011	8	4	$10,162,888
2012	9	7	$68,695,485
2013	8	6	$28,094,534
2014	2	1	$2,616,721
2015	3	3	$2,053,726
2016	16	13	$50,149,292
Average	22	17	$65,556,096

Table A2.3: Robustness Check of the Effect of Passing a Conservation Referendum on Neighbor Referendum Activity in Massachusetts

VARIABLES	(1) State Spend per cap (log)	(2) Neighbor State Spend per cap (log)	(3) Neighbor Referendums per neighbor	(4) Neighbor Funds per cap (log)	(5) State Spend per cap (log)	(6) Neighbor State Spend per cap (log)	(7) Neighbor Referendums per neighbor	(8) Neighbor Funds per cap (log)
Pass Concurrent Year	0.175	-0.214	0.040	0.147	0.152	-0.542	0.098*	0.292
	(0.275)	(0.291)	(0.035)	(0.319)	(0.352)	(0.412)	(0.050)	(0.434)
Pass 1 Year Ago	0.102	-0.132	0.040*	0.691***	0.482	0.444	0.000	0.331
	(0.312)	(0.298)	(0.023)	(0.245)	(0.456)	(0.424)	(0.032)	(0.347)
Pass 2 Years Ago	-0.096	0.007	0.047***	0.538***	-0.248	0.029	0.004	0.082
	(0.306)	(0.223)	(0.016)	(0.201)	(0.440)	(0.315)	(0.021)	(0.256)
Pass 3 Years Ago	-0.072	-0.219	0.042*	0.364	0.259	-0.007	0.016	-0.013
	(0.266)	(0.237)	(0.022)	(0.292)	(0.343)	(0.372)	(0.035)	(0.455)
Pass 4 Years Ago	0.008	-0.337	0.019	0.312	-0.139	-0.009	-0.001	0.160
	(0.286)	(0.297)	(0.023)	(0.286)	(0.405)	(0.404)	(0.027)	(0.377)
Pass 5 Years Ago	-0.268	-0.419	-0.006	-0.217	-0.050	-0.367	-0.014	-0.197
	(0.395)	(0.356)	(0.022)	(0.263)	(0.627)	(0.510)	(0.025)	(0.338)
Pass 6 Years Ago	-0.206	0.078	0.022	0.196	0.110	0.411	0.040	0.378
	(0.317)	(0.305)	(0.023)	(0.246)	(0.436)	(0.350)	(0.033)	(0.354)
Pass 7 Years Ago	0.154	0.291	0.025	0.283	0.481	0.762	-0.040	-0.088
	(0.375)	(0.367)	(0.034)	(0.279)	(0.530)	(0.536)	(0.047)	(0.396)
Observations	3,220	3,220	4,830	4,830	3,220	3,220	4,830	4,830
Adjusted R-squared	0.252	0.373	0.201	0.156	0.251	0.375	0.205	0.156
Vote Margin Polynomial	Linear	Linear	Linear	Linear	Quadratic	Quadratic	Quadratic	Quadratic

Table A2.3: (continued)

Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column is a separate regression. Standard errors are shown in parentheses and are clustered at the municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A2.4: Robustness Check of the Effect of Passing a Conservation Referendum on Neighbor Referendum Activity in New Jersey

VARIABLES	(1) Neighbor Referendums Passed per neighbor	(2) Neighbor Funds Approved per capita (log)	(3) Neighbor Referendums Passed per neighbor	(4) Neighbor Funds Approved per capita (log)
Pass Concurrent Year	0.022	0.353	0.041	0.401
	(0.027)	(0.313)	(0.041)	(0.500)
Pass 1 Year Ago	0.015	0.038	-0.019	-0.500
	(0.025)	(0.332)	(0.032)	(0.449)
Pass 2 Years Ago	-0.013	0.152	-0.053*	-0.316
	(0.023)	(0.297)	(0.030)	(0.421)
Pass 3 Years Ago	-0.005	-0.023	-0.024	-0.371
	(0.018)	(0.263)	(0.025)	(0.402)
Pass 4 Years Ago	-0.003	0.110	-0.009	0.078
	(0.017)	(0.242)	(0.020)	(0.357)
Pass 5 Years Ago	-0.001	-0.077	-0.016	-0.224
	(0.016)	(0.257)	(0.019)	(0.297)
Pass 6 Years Ago	-0.017	-0.162	-0.019	-0.347
	(0.017)	(0.236)	(0.021)	(0.306)
Pass 7 Years Ago	0.016	0.165	-0.008	-0.136
	(0.015)	(0.244)	(0.018)	(0.293)
Observations	5,565	5,565	5,565	5,565
Adjusted R-squared	0.137	0.174	0.136	0.174
Vote Margin Polynomial	Linear	Linear	Quadratic	Quadratic
Year Fixed Effects	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes

Notes: Each column is a separate regression. Standard errors are shown in parentheses and are clustered at the municipality level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A2.5: The Effect of Passing a Conservation Referendum and Demographics on Own and Neighbor State Spending and Neighbor Referendum Activity

	(1)	(2)	(3)	(4)	(5)	(6)
	Massachusetts				New Jersey	
VARIABLES	State Spending per capita (log)	Neighbor State Spending per capita (log)	Neighbor Referendums Passed per neighbor	Neighbor Funds Approved per capita (log)	Neighbor Referendums Passed per neighbor	Neighbor Funds Approved per capita (log)
Pass Concurrent Year	0.297	-0.835	0.011	-0.394	0.038	0.048
	(0.450)	(0.548)	(0.060)	(0.544)	(0.054)	(0.658)
Pass 1 Year Ago	0.658	0.347	-0.027	0.094	-0.036	-1.010*
	(0.655)	(0.513)	(0.042)	(0.450)	(0.040)	(0.563)
Pass 2 Years Ago	-0.109	0.050	-0.001	-0.140	-0.064	-0.396
	(0.535)	(0.390)	(0.026)	(0.319)	(0.040)	(0.561)
Pass 3 Years Ago	0.346	-0.241	0.017	-0.093	-0.012	-0.320
	(0.423)	(0.469)	(0.049)	(0.594)	(0.035)	(0.525)
Pass 4 Years Ago	0.060	-0.062	0.006	0.301	-0.024	-0.088
	(0.483)	(0.497)	(0.034)	(0.432)	(0.025)	(0.461)
Pass 5 Years Ago	-0.068	-0.555	0.002	-0.017	-0.028	-0.346
	(0.865)	(0.621)	(0.031)	(0.446)	(0.022)	(0.367)
Pass 6 Years Ago	-0.150	1.121**	0.043	0.196	-0.014	-0.274
	(0.458)	(0.446)	(0.042)	(0.441)	(0.027)	(0.400)
Pass 7 Years Ago	-0.425	0.803	-0.058	-0.279	0.010	0.154
	(0.662)	(0.698)	(0.056)	(0.511)	(0.021)	(0.357)
Demographics						
Median Household Income (log)	-0.715	-2.410***	0.043	0.805*	-0.090*	-0.553
	(0.690)	(0.878)	(0.044)	(0.475)	(0.049)	(0.679)

Table A2.5: (Continued)

Bachelor's Degree (%)	0.027	0.050*	-0.002*	-0.007	0.002	0.027
	(0.019)	(0.029)	(0.001)	(0.011)	(0.002)	(0.024)
Under 18 Years Old (%)	-0.019	-0.075	-0.004**	-0.073***	-0.002	-0.034
	(0.044)	(0.071)	(0.002)	(0.021)	(0.002)	(0.033)
Over 65 Years Old (%)	-0.025	-0.040	-0.010***	-0.128***	-0.008***	-0.103***
	(0.039)	(0.054)	(0.002)	(0.021)	(0.002)	(0.027)
White Population (%)	0.025	0.027	0.004***	0.063***	0.001	0.009
	(0.026)	(0.044)	(0.002)	(0.016)	(0.001)	(0.019)
Black Population (%)	-0.005	-0.001	0.005	0.049	-0.005**	-0.053*
	(0.054)	(0.066)	(0.003)	(0.033)	(0.002)	(0.029)
Population Density (log)	-0.274	-0.261	0.070	-0.479	-0.108**	-1.129*
	(1.123)	(1.866)	(0.059)	(0.608)	(0.045)	(0.649)
Acres Available (log)	0.000	3.141	-0.249***	-4.237***	-0.158**	-2.310**
	(0.000)	(2.152)	(0.086)	(1.152)	(0.077)	(1.087)
Median House Price (log)	1.369***	1.000*	-0.038	0.171	0.006	0.143
	(0.521)	(0.599)	(0.026)	(0.259)	(0.023)	(0.348)
Democratic Vote Margin	0.023**	0.031***	0.001**	0.020***		
	(0.009)	(0.011)	(0.000)	(0.005)		
Observations	3,220	3,220	4,830	4,830	5,565	5,565
Adjusted R-squared	0.253	0.380	0.217	0.168	0.142	0.181
Vote Margin Polynomial	Cubic	Cubic	Cubic	Cubic	Cubic	Cubic
Municipality Characteristics	Own	Avg Neighbor	Avg Neighbor	Avg Neighbor	Avg Neighbor	Avg Neighbor
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column is a separate regression. Standard errors are shown in parentheses and are clustered at the town level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A2.7: Test of Reverse Causality

	(1)	(2)	(3)	(4)
	Massachusetts		New Jersey	
VARIABLES	SUR model difference (std. error)	SUR model difference (std. error)	SUR model difference (std. error)	SUR model difference (std. error)
State Spending	8.449	2.928		
	(12.43)	(14.56)		
Neighbor State Spending	-3.052	-2.842		
	(6.224)	(4.931)		
Neighbor Referendums Passed	0.103*	0.0433**	0.0542	0.0444
	(0.0596)	(0.0218)	(0.0355)	(0.0314)
Neighbor Conservation Funds	35.43	4.460	58.38**	16.85
	(34.42)	(4.860)	(25.04)	(12.03)
Observations	318	317	462	461
Conservation Activity	Concurrent Year	1-Year lag	Concurrent Year	1-Year lag
Vote Margin Polynomial	Cubic	Cubic	Cubic	Cubic
Chi^2 Test	10.42	17.24	13.65	11.68
Prob > Chi^2	0.7307	0.2436	0.2528	0.3881

Notes: Each column is a separate regression. Results are from seemingly unrelated regressions where the error terms are assumed to be correlated between individual regression equations where municipality demographics were the dependent variable and the exogenous explanatory variables were a dummy variable for a passed referendum and a cubic vote margin polynomial. SUR regressions include the same municipal demographic variables that were included in Table 3.1 and Table 3.2 in the main text.

CONCLUSION

Communities in the United States have a complex relationship with the environment. For instance, if a municipality has been experiencing rapid development, they could conserve land around the municipality to curb urban sprawl. However, this may cause a "free-rider" problem where surrounding towns are less likely to conserve land on their own and consequently miss out on the ecological benefits of connected conservation land. Another example is the physical threat that counties along the East Coast and Gulf of Mexico face from strong tropical storms and hurricanes. When a hurricane hits, both people and businesses may be affected. The policies that communities adopt will govern how successful they will be at maximizing future benefits and minimizing future costs related to both types of environment-related events.

In order for communities to efficiently manage responses to environmental events such as severe weather and land conservation decisions of surrounding communities, it is important first to quantify and understand how these events affect communities over time. In my dissertation research, I conduct three independent studies to examine how environmental events such as hurricane strikes and land conservation decisions affect communities in the United States.

In the first chapter, I examine how hurricane strikes affect the economy in U.S. counties, including poverty levels, public business accounts, and population trends. The main hypothesis tested in this chapter was that hurricane strikes increase poverty levels and the effect persists over multiple years after the strike. Interestingly, we see that hurricane strikes *decrease* poverty levels in affected counties using a difference-in-differences methodology. There are two vehicles that can be causing this decrease after a strike – an increase in business activity (including per capita personal income, wages, and employment) as well as a decrease in population. Supplementary analysis on shifts of income distribution shows that the decrease in poverty is most likely due to displacement of families around the poverty line.

In the second chapter, I examine if there is a "window of opportunity" in communities that experience a tropical storm or hurricane strike where people are more interested in taking preventative action against future storm damage costs. The main hypothesis tested in this chapter is that hurricane strikes cause an increase in interest activities used to mitigate against future hurricane damage costs – measured by relevant Google search terms – during the month of a hurricane strike and a short duration afterwards. Results using a difference-in-differences methodology reflect this relationship. Populations in media markets that experience tropical storms and hurricanes show increased popularity of internet searches of *flood insurance* during the concurrent month up to a few months after. This suggests there may be a window of opportunity in which stakeholders are more likely to be engaged and support policies aimed at reducing future damage costs from environmental events like tropical storms and hurricanes.

In the third chapter, I examine whether municipalities that pass land conservation referenda cause state and neighboring municipalities to crowd-in or crowd-out land conservation spending. The main hypothesis tested in this chapter is that municipalities that pass conservation referenda are more likely to receive additional state land conservation funding and encourage neighboring towns to pass conservation referenda (i.e. a crowd-in effect). Results from a dynamic regression discontinuity methodology do not show a consistent causal effect in any of the relationships tested – passing a referendum does not result in any patterns of state spending in the focal municipality, state spending in neighboring municipalities, or referendum activity in neighboring municipalities. Results indicate that municipalities need not worry that their own conservation activity will crowd-out state and neighboring municipality conservation spending. Conversely, they should not expect crowding-in activity as well.

Discrete environmental events such as storm experience and natural resource decisions have the potential to have widespread consequences. Most of the research in this dissertation focuses on the impacts of environmental events in directly affected communities – although neighboring counties to those that experience hurricane strength intensity are identified in the first chapter conservation activity in neighboring municipalities was examined in the third chapter. Future research can focus on examining if willingness to mitigate against future environmental damages is shown in neighboring communities. Future research can also examine individual data instead of aggregate data, which was the focus of this dissertation.