Essays in Public and Environmental Economics

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My dissertation explores how citizens and local governments make decisions regarding the provision of public goods and the implementation of public policy. I particularly focus on how environmental conditions interact with constituent demands in the context of American democracy. In the first chapter, I show how individual demands for government policies are influenced by environmental shocks related to climate change. In the second chapter, I use the example of crime to show voter policy demands are also responsive to short-term, highly localized changes in neighborhood conditions. Finally, in the third chapter, I show that one of the most expensive infrastructure projects ever undertaken by an American municipality proved to be a worthwhile investment even as it reshaped the urban environment.

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Preface

I would like to thank my advisor Allison Shertzer, who first introduced me to public economics as an undergraduate and provided thoughtful guidance and direction throughout my graduate education. Her excellent advising included, among many other things, the encouragement I needed to pursue a PhD. I would also like to thank my co-chair Randall Walsh, Andrea La Nauze, Daniel Jones, Andreas Ferrara, Osea Giuntella, and the rest of the economics faculty at the University of Pittsburgh for challenging me to grow as a researcher. I owe a debt of gratitude to my late advisor Werner Troesken, whose creativity and enthusiasm were a constant inspiration and who provided a vision for the third chapter of this dissertation. Thanks also to Pun Winichakul, Ning Zhang, and my fellow economics graduate students for always being there to collaborate and commiserate when needed. I am appreciative of the many students, teachers, administrators, and friendly faces who have made Pitt feel like home for the past decade of my education.

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1.0 Climate Risk and Preferences over the Size of Government: Evidence from California Wildfires

How does exposure to risk shape individual preferences for an expanded state? I examine this question in the context of a source of risk prominently featured in the public discourse: climate change. I use variation in California wildfire activity to study how demand for government services evolves following exposure to climate change-associated disaster events. I find that Census block groups experiencing a large fire in the two years preceding a biennial Congressional election increase support by 0.8 percentage points for ballot initiatives which expand government spending and taxation. Preference for a more activist state is stronger on the issues rendered most salient by fire exposure, as I document a larger increase of 2.4 percentage points in support for ballot initiatives endorsed by pro-environment interest groups. The effect of fire exposure is stronger in more Republican areas and decays with distance from a fire. The effect does not appear to be driven by shifts in voter registration or turnout, suggesting that the mechanism is indeed changes in individual preferences rather than compositional changes in the electorate.

1.1 Introduction

How do we expect attitudes toward the size of government to evolve in a world increasingly impacted by climate change? According to the United Nations, "there is overwhelming evidence that [global warming] is resulting in profound consequences for ecosystems and people" (IPCC 2019). As the profound consequences of climate change become more widespread, will individuals support a larger role for government in society as a form of insurance against heightened climate risk?

There is reason to believe that exposure to climate change-associated natural disasters may lead individuals to prefer a larger, more redistributionist government, primarily through the channel of altering voters' risk aversion. An extensive literature examines the role of natural disasters in shaping risk preferences.[1] Although this literature lacks consensus, many of these studies find that exposure to natural disasters increases individuals' risk aversion, and it is further known that risk averse individuals are more likely to express support for state-facilitated redistribution (Gärtner et al. (2017)). If natural disasters indeed lead to greater risk aversion, voters may desire expanded social insurance and a more robust safety net as they directly experience climate impacts.

There is also evidence that personal experience may shape individuals' attitudes toward climate change specifically. A broad survey-based literature finds that extreme weather anomalies affect respondents' views on climate change.[2] Less studied is the degree to which enhanced awareness of climate change translates to preferences for expanded state capacity. In the context of California wildfires, Hazlett and Mildenberger (2020) show that proximity to fires increases voter support for pro-climate positions on four ballot initiatives over the 2006, 2008, and 2010 elections. While it is perhaps unsurprising that voters exposed to fires shift in a limited way toward supporting climate-relevant policy, the broader question remains whether climate-change associated natural disasters are capable of inducing a more

[1] See Kahsay and Osberghaus (2018), Brown et al. (2018), Hanaoka et al. (2018), Gallagher (2014), Timar et al. (2014), Henderson and Turner (2020), Bourdeau-Brien and Kryzanowski (2020), Cameron and Shah (2015), Dessaint and Matray (2017).

[2] See Zanocco et al. (2018), Sisco et al. (2017), Konisky et al. (2016), Zaval et al. (2014), Deryugina (2013), Egan and Mullin (2012), Borick and Rabe (2010), Brulle et al. (2012).

fundamental shift in the relationship between individuals and government.

In this paper, I provide estimates on the extent to which individuals' preferences over the size of government evolve in response to climate-change associated extreme events using evidence from California. Specifically, I look at how exposure to large wildfires impacts support for ballot initiatives in two categories: 1) redistributive initiatives that expand the size of government and 2) initiatives that are favored by environmental advocacy groups. I use election returns provided by California's Statewide Database to create a panel of consistent geographies spanning nine statewide biennial elections from 2002-2018. I define initiatives that expand the size of government as those that increase either government spending or taxes. I define environmental initiatives as those endorsed by both the Sierra Club of California and the California League of Conservation Voters, two prominent environmental interest groups. Each set of initiatives encompasses at least 27 individual propositions. Finally, I use local variation in wildfire activity to execute a difference-in-differences strategy which identifies changes in policy support in response to fires. The strategy allows me to control for general time trends in policy support and for unobserved differences across small geographies.

The California setting is advantageous for several reasons. First, California notably has a lengthy and often severe wildfire season which is worsening over time. While there are many causes for California's wildfires including past fire suppression techniques and development of the wildland-urban interface, California wildfires have been linked to climate change both scientifically (Williams et al. (2019)) and in popular opinion (Yale Climate Survey 2014). Second, California has extremely lax rules governing ballot initiatives. California voters have faced an average of more than 11 statewide ballot initiatives per even-year election since 2002. The frequency of ballot initiatives allows me to disentangle support for policy from support for a political party. This is an important distinction in a polarized political culture like the United States where opinions on redistributive and environmental policy are highly correlated with political party identification.

My results show that exposure to wildfire bolsters support for policies that expand the size of government and offer greater redistribution. The vote share on expanded-government ballot initiatives increases by about 0.8 percentage points in Census block groups that have directly experienced a large fire. In accordance with previous findings, I show support for environmental policies increases by 2.4 percentage points. The effect in both cases decays with distance from the fire perimeter. Using an event study framework, I show that the effect is transitory with voting patterns quickly returning to baseline. The impact of fire exposure is stronger in areas with more registered Republicans. I find no effect on voter turnout and a statistically significant but economically small increase in the share of registered Democrats in burned areas. I also find no effect on support for Democrats in US House races. Taken together, the results suggest the observed increases in support for big government and pro-environment policies are driven by changes in voter preferences rather than compositional changes in the electorate.

As with any difference-in-differences identification strategy, the main threat to identification is violation of the parallel trends assumption; in this case, that burned and non-burned areas are trending differently with respect to support for the policies of interest. My empirical specification includes a full set of county-by-election-year time dummies to flexibly control for time trends at a local level. However, the concern may exist that burned and non-burned areas are trending differently even within county. I employ several approaches to provide evidence that the parallel trends assumption is upheld. First, evidence for the existence of differential pre-trends is limited in the event study framework. Next, I show that future fires have no ability to predict voting patterns in the current year. Finally, a number of recent papers have demonstrated concern about heterogeneous treatment effects and negative weighting arising from erroneous comparisons between treated and not-yet-treated units in two-way fixed effects models like the one used here.[3] I show there is no evidence of pre-trends and the results remain consistent when using the difference-in-differences imputation estimator of Borusyak et al. (2021).

The main result that exposure to wildfire increases both environmental and redistributive voting is consistent with previous studies which show one of the following: a direct effect of natural disasters on insurance takeup; a direct effect of natural disasters on climate beliefs; or a spillover effect related to consistent voting on partisan-aligned issues. This spillover effect is documented by Brunner et al. (2011) who use California ballot initiatives to show a decrease in support for redistributive policies in response to favorable economic conditions. Moreover, they show voters exhibit a preference for cognitive consistency as the effect of economic shocks extends to non-economic policies supported by the same party.

This desire for ideological consistency has been explored in theoretical and survey-based studies as a source of the partisan divide around climate change in the United States (Van Boven et al. (2018), Czarnek et al. (2021)). It is therefore unsurprising that a shock of sufficient strength to shift voter preferences on environmental policies produces a smaller but meaningful shift on politically-aligned economic policies; however, my study is the first to demonstrate this pattern in a unified setting related to climate change. The existence of this spillover effect has implications for the political trajectory of the United States, because voters may move toward embracing liberal policies across multiple dimensions as climate change impacts increase in severity.

[3]see: de Chaisemartin and D'Haultfœuille (2020), Borusyak et al. (2021), Goodman-Bacon (2021), Abraham and Sun (2018), Callaway and Sant'Anna (2021)

I find that individuals increase support for initiatives that expand government and offer greater redistribution by about 0.8 percentage points. These results suggest that experience with the impacts of climate change produces a risk-aversion mediated increase in demand for social insurance and that demand may become stronger with the growing salience of climate events. My results are also largely consistent with both the survey-based literature on climate beliefs and the literature that looks at political activity. In a more precisely identified setting that directly measures preferences for government size and environmental policy, I find voters are responsive to climate change-associated wildfires and that part of the responsiveness is driven by ideology. My results differ in that I do not observe a movement toward support for Democrats in US House races. Instead, the finding that the effect of fires is generally stronger in areas with more registered Republicans while Democratic vote share does not increase illustrates how voter preferences for singular issues can change even while party support remains constant.

1.2 Related Literature

This paper is related to previous studies which have demonstrated that risk exposure, particularly exposure to natural disasters, leads people to increase insurance takeup in various forms. Demand for additional insurance in experimental settings follows rainfall shocks (Cole et al. (2014)) and floods (Said et al. (2015)) in the developing world. Weather shocks have also been shown to alter marriage behavior in ways that allow individuals to smooth consumption in the absence of complete markets (Kumala Dewi and Dartanto (2019), Khanna and Kochhar (2020)). In the developed context, insurance takeup increases following floods (Gallagher (2014)) and earthquakes (Lin (2019)), though these responses may be short lived as with the housing price response to wildfire found by McCoy and Walsh (2018). I use a novel setting to explore the degree to which insurance-seeking behavior induced by disaster experience extends to a desire for the government to take a stronger redistributive posture in society.

Another strand of literature deals with the political activity of voters and politicians following climate change-associated weather anomalies. Li et al. (2011) find increased contributions to an environmental charity while Liao and Ruiz Junco (2022) find increased contributions to Democratic candidates and support for pro-environment candidates following extreme weather events. Other work has found that politicians vote for greener bills following weather anomalies and hurricanes (Herrnstadt and Muehlegger (2014), Gagliarducci et al. (2019)). The main advantages of my study are that I can measure policy support directly from a voter perspective instead of through the proxy of a candidate who represents a bundle of policy positions; I can measure how the effect of a disaster varies spatially; and I can provide evidence that the mechanism involves changes in voter preferences rather than activation of already environmentally-aligned individuals. Hazlett and Mildenberger (2020) find support for pro-climate ballot initiatives in California increases in response to wildfire exposure using a sample of four ballot initiatives across three elections. I confirm their pro-environment findings in my sample of 27 ballot initiatives across nine elections, although in contrast I find that the pro-environment shift is strongest in areas with more registered Republican voters. That the strongest shift occurs amongst Republicans comports with Margalit (2013) who shows Republicans, with lower baseline levels of support for redistribution, move more strongly than Democrats toward pro-redistributive positions in response to economic shocks.

Finally, this paper is related to the studies that use voting on referenda as a way to elicit preferences over public goods, particularly in the context of California. Previous estimates using referenda to measure willingness to pay can be found in Vossler et al. (2003) and Vossler and Watson (2013). More recent work by Burkhardt and Chan (2017) estimates willingness to pay for policy outcomes associated with 13 California ballot initiatives. My use of ballot initiatives is most similar to Brunner et al. (2011). Their work uses the mean vote share across all ballot initiatives in California supported by Democrats as a measure of preference for redistribution; I also use the mean vote share across relevant initiatives as a measure of policy support. Furthermore, Brunner et al. document a preference for political consistency. They show that positive economic shocks lead not only to a decrease in demand for redistribution but also to decreases in support for liberal positions on social issues. I show voters exhibit a similar pattern of consistency as highly salient wildfire shocks produce both an increase in voting for pro-environment positions and also for politically-aligned fiscal measures.

1.3 Background and Data

1.3.1 Fires in California

My identification strategy depends on plausibly exogenous variation in wildfire activity over small geographies. I therefore use high-resolution data on historical fires from the California Department of Forestry and Fire Protection's (CAL FIRE's) Fire and Resource Assessment Program (FRAP). FRAP maintains an ongoing database of historical fire perimeters which is updated each year.[4] The included GIS shapefile contains incidents from CAL

[4] Available online from CAL FIRE at https://frap.fire.ca.gov/frap-projects/fire-perimeters/

FIRE, the United States Forest Service Region 5, the Bureau of Land Management, and National Park Service jurisdictions to create "most complete digital record of fire perimeters in California" (FRAP). Information includes fire perimeters, the size of fires, start and end dates, and other notes for each incident. Figures 1.1 and 1.2 show the generally increasing severity of fires both in acreage burned and in suppression costs. Large wildfires are highly salient events in California; the record 2018 fire season saw over 1.6 million acres burned, tens of thousands of people evacuated, and $773 million in suppression costs.

1.3.2 Ballot Initiatives and Voting Data in California

The primary appeal of utilizing ballot initiatives as an outcome variable is that a vote for an initiative provides a more direct measure of support for a policy stance than a vote for a politician who represents a bundle of policy stances. California has a long history with ballot initiatives. Since the initiative process was adopted in 1911, Californians have voted on more than 1,200 propositions. These propositions range from both citizen-initiated and legislatively-referred state statutes and constitutional amendments to bond measures authorizing the state to take on debt earmarked for specific projects.

I focus on initiatives placed on the ballot during November even-year general elections from 2002 to 2018. Californians voted on 102 initiatives over the nine elections. To define policies that increase the size of government, I examine the fiscal impact statement accompanying each ballot measure for initiatives which definitively increase taxation and spending or, following Brunner et al. (2011), those that could be considered redistributive by the initiatives' expansion of public goods related to education, health, or welfare. To define pro-environment policy positions, I use initiatives that have been endorsed by both the Sierra Club of California and the California League of Conservation Voters (LCV). Defining the pro-environment positions in this way comports with the findings by Brulle et al. (2012) that climate beliefs may be primarily driven by elite cues. I obtain information on endorsed positions for the Sierra Club and LCV from each group's historical website captures hosted on the Wayback Machine Internet Archive.[5]

My outcome variable of interest is the average share of the vote for the endorsed positions across all initiatives endorsed in a given election. Of the 102 initiatives in the study period, 45 are included in the big government category and 27 are in the environmental category. At least two initiatives are included in each of the nine elections except for 2014, when the League of Conservation Voters endorsed one initiative. Some initiatives are directly related to climate change such as Proposition 23 in 2010 (suspension of AB 32, which limited greenhouse gas emissions) while others are less related, such as bond issues funding clean water projects. Given the political alignment between environmental advocates and advocates for more government spending, there is overlap across samples. 15 ballot initiatives appear in both samples, 13 of which have the same endorsed ballot position. A complete list of endorsed initiatives can be found in Appendix A.

I use data on election returns and voter registration compiled by California's Statewide Database which is maintained by law at the University of California, Berkeley for use in redistricting projects. The data hosted online contains election returns and voter registration information at the precinct level for biennial elections dating back to 2002. The Statewide Database also provides crosswalks to convert precincts, the boundaries of which may change every election, to constant Census geographies. I use 2010 Census block groups as the unit of observation in this study. I combine the Statewide Database election returns with Cal Fire

[5] Sierra Club: https://www.sierraclub.org/california; California League of Conservation Voters: https://www.ecovote.org/elections/

incident perimeters using the US Census bureau's TIGER/line shapefiles for block groups in California.

1.4 Empirical Strategy

To estimate the effect of fire exposure on policy, I use a standard difference-in-differences strategy conducted on a panel of 2010 Census block groups. Each of the more than 23,000 block groups has one observation per election. There are nine elections in the study from 2002-2018. The outcome variable is the mean share of the vote for the designated positions on ballot initiatives across all initiatives in the category in that election. I define treated block groups as those groups in which the perimeter of a large fire ($\geq$5000 acres) overlaps some part of the block group in the two years preceding an election. Figure 1.3 illustrates this treatment definition. Specifically, I estimate forms of the following equation:

$$Y_{st} = \alpha + \beta_1 Fire\_Treatment_{st_{-2}} + \delta_s + \gamma_{ct} + \epsilon_{st} \tag{1.1}$$

Where Y_{st} is the mean share of the vote for big government or environmental positions on ballot initiatives in block group s in election year t measured in percentage points, $Fire\_Treatment_{st_{-2}}$ is an indicator equal to 1 if the block group experienced a large fire in the two years preceding the election, δ_s is a block group fixed effect, and γ_{ct} is a county-by-year fixed effect. The coefficient of interest is β_1 measuring the responsiveness of big government or pro-environment voting to fire exposure. A positive β_1 indicates movement toward the endorsed positions. The block group fixed effect captures time-invariant unobservable determinants of voting across block groups. The county-by-year fixed effect flexibly controls for time trends and shocks common to all block groups within a county such as

TV media campaigns. I use heteroskedasticity-robust standard errors clustered at the block group level for all regressions.

Figure 1.4 shows the importance of including the block group fixed effect. While block groups experiencing a fire appear to be trending in the same manner as the state as a whole, block groups which experience a large fire at any point in the study are on average wealthier than the rest of California and have a much lower share of registered Democrats. Table 1.1 presents the summary statistics. Treated areas are also less likely to support big government or pro-environment initiatives at baseline.

Having laid out the main specification, I run variants of Equation 1 to test additional hypotheses. Equation 1 takes a very rigid view of treatment; Census block groups must actually experience a fire in order to be counted as treated. Large fires in California are highly salient and as such are likely to have effects beyond the immediate burn scar. To the extent nearby communities are also impacted by the fire, we would expect 1) the effect of fire exposure to decay with distance from the fire perimeter and 2) β_1 of Equation 1 to underestimate the true effect of fire exposure since partially treated spillover areas are included in the control group. To test these hypotheses, I estimate the following equation:

$$Y_{st} = \alpha + \beta_1 Fire\_Treatment_{st_{-2}} + \beta_2 5kbuff_{st_{-2}} + \beta_3 20kbuff_{st_{-2}} + \delta_s + \gamma_{ct} + \epsilon_{st} \quad (1.2)$$

Where $Fire\_Treatment_{st_{-2}}$ is an indicator equal to 1 if the block group experienced a large fire in the two years preceding the election, unchanged from Equation 1; $5kbuff_{st_{-2}}$ is an indicator equal to 1 if the block group is less than 5 kilometers from a burn scar but did not itself experience a fire, and $20kbuff_{st_{-2}}$ is an indicator equal to 1 if the block group is less than 20 kilometers but greater than 5 kilometers from a burn scar. If large fires have spillover effects on voting beyond the fire perimeter, we would expect $\beta_1 > \beta_2 > \beta_3$ and β_1

from Equation 2 larger than the corresponding estimate from Equation 1. Figure 1.6 shows the extent of buffer zones in California for large fires occurring prior to the 2018 election.

Next, I attempt to test whether changes in environmental voting are driven by changes in voter preferences or by compositional changes in the electorate. An increase in fire-driven environmental voting could, among other explanations, be a result of 1) an increase in turnout among voters predisposed to support big government or environmental policies 2) an increasing trend of Democrats, who support the interest group-endorsed positions at higher rates, moving into burned areas or 3) existing voters changing their minds from small- to large-government positions. To test these hypotheses, I run Equation 1 with the outcome variable Y_{st} being either the share of registered Democrats or total voter turnout.

To further examine which voters are driving the effect, I estimate a fully interacted version of Equation 1 in which the treatment indicator is interacted with a continuous measure of the share of registered Democrats or Republicans.

$$\begin{aligned} Y_{st} = \alpha + \beta_1 Fire\_Treatment_{st_{-2}} + \beta_2 5kbuff_{st_{-2}} + \beta_3 20kbuff_{st_{-2}} + \beta_4 PartyReg_{st} \\ + \beta_5 PartyReg^{*} Fire\_Treatment_{st_{-2}} \\ + \beta_6 PartyReg^{*} 5kbuff_{st_{-2}} + \beta_7 PartyReg^{*} 20kbuff_{st_{-2}} + \delta_s + \gamma_{ct} + \epsilon_{st} \quad (1.3) \end{aligned}$$

Where $PartyReg$ can be the share of registered voters in a Census block group who are registered as Democrats or Republicans. Given the current alignment of political parties in the United States, the expectation is that β_4 is positive when $PartyReg$ represents Democrats and β_4 is negative when $PartyReg$ represents Republicans.

Equation 3 allows me to test which concentrations of ideological voters are driving changes in policy support. Because previous literature has focused on contributions and votes for Democratic candidates, I then ask whether changes in voter preferences are limited to single-issue ballot initiatives or if they extend to changes in overall party support. I therefore run the Equation 1 model using the vote share for Democrats in US House races and vote share for incumbents as outcome variables.

I next explore how the effect of fire exposure evolves over time through an event study framework. Again, the identifying assumption is that block groups in which a large fire occurred would have experienced the same trend in big government and environmental voting as untreated block groups absent the event. Equation 4 is the standard event study specification:

$$Y_{st} = \alpha + \delta_s + \gamma_{ct} + \sum_{i \in PRE} \beta_i 1\{t - t_s^* = i\} + \sum_{i \in POST} \pi_i 1\{t - t_s^* = i\} + \epsilon_{st} \tag{1.4}$$

Where δ_s is again a block group fixed effect and γ_{ct} is again county-by-year time fixed effect. The coefficients β_i capture trends in voting in the pre-period prior to exposure to large fire occurring in block group s at time t_s^*. The post-period coefficients π_i capture the responsiveness of big government and environmental voting to fires over time.

It is important to account for multiple treatments in this setting, as nearly 30% of ever-treated block groups experience more than one fire. Schmidheiny and Siegloch (2019) show that a panel of the dependent variable ranging from $[\overline{t}, \underline{t}]$ for an event window spanning $[\overline{j}, \underline{j}]$ requires observation of events until time $[\overline{t} + \underline{j} - 1]$ in order to properly bin the endpoints allowing for testing of pre-trends. In my context with biennial periods and $\overline{t} = 2018$, an event study with three leads in the pre-period would require the observation of wildfires through 2022 for proper identification of pre-trends. This limitation means I cannot run an event study in the entire panel, but I can run a three-lag, three-lead event study on a panel restricted to the years 2006 through 2014. I therefore limit the observation window to this time frame.

While I am interested in categorizing a shift in voter preferences, another mechanism to explain a change in voting outcomes may be that voters experience changes in beliefs in response to fire exposure rather than changes in preferences. Since I only observe voting outcomes, I cannot separately identify the two channels. However, to interpret the findings as a change in voter preferences, it may be more reasonable to model the decision of voters at the ballot box as a discrete choice for or against each policy. I therefore run the following equation where the outcome is the difference in log vote share:

$$ln(Yes_{st}) - ln(No_{st}) = \alpha + \beta_1 Fire\_Treatment_{st_{-2}} + \delta_s + \gamma_{ct} + \epsilon_{st} \tag{1.5}$$

Where Yes_{st} is the vote share for big government or environmental positions in block group s in election year t and No_{st} is the vote share against. The error term then follows a type-I extremum distribution which facilitates interpretation of the results as a shift in voter preferences.

I wish to address the main threat to the empirical approach, which is that burned and unburned areas could be trending in different directions regarding big government or environmental support. Figure 1.5 shows that, on a broad level, ever-treated and never-treated areas are experiencing the same pattern in vote share. To provide further evidence that the parallel trends assumption is upheld in this setting, I run a falsification test of Equation 1 using future fires as the treatment variable:

$$Y_{st} = \alpha + \beta_1 Treatment\_Future_{st_{+2}} + \delta_s + \gamma_{ct} + \epsilon_{st} \tag{1.6}$$

Equation 6 is the same as Equation 1 with the exception that $Treatment\_Future$ is now an indicator equal to 1 if a block group experiences a large fire in the two years following an election rather than in the two years prior. This specification tests whether future fires are predictive of current year voting patterns. A positive coefficient on β_1 implies the results are driven by trends unrelated to fires. A null result indicates the fixed effects are adequately capturing all differential trends between burned and non-burned areas. I limit the falsification test sample to block groups only ever within 5km from a fire and include only those block groups which have not yet experienced a fire. I then show the main results are largely unchanged in this sample.

Finally, the use of a two-way fixed effects model with variation in treatment timing introduces concerns about weighting of treatment effects arising from an implicit assumption of treatment homogeneity as highlighted by a number of recent papers (de Chaisemartin and D'Haultfœuille (2020), Borusyak et al. (2021), Goodman-Bacon (2021), Abraham and Sun (2018), Callaway and Sant'Anna (2021)). There are particular reasons for concern in my setting, as treatment effects are likely not homogeneous given the political heterogeneity of California, and the existence of multiple treated units with variations in timing creates the potential for erroneous comparisons between treated and not-yet-treated units. I therefore show the results are robust to allowing for heterogenous treatment effects using the efficient DiD-imputation method of Borusyak et al. (2021).

1.5 Results

My aim is to understand what role climate change-associated disasters play in shifting preferences over the size of government and over environmental policies along with the mechanism behind any shifts that do occur. Table 1.2 shows the estimates of Equation 1. Column 1 shows the effect of large fires on big government voting as defined by the share of the vote on ballot initiatives that increase either spending or taxes. Column 2 shows the effect for environmental initiatives endorsed by both environmental interest groups. The results indicate that Census block groups experiencing a large fire in the two years preceding an election increase support for big government positions by around 0.5 percentage points. Support for environmental policies increases by 1.8 percentage points, indicating that voters are shifting most heavily on the issues made most salient by fire exposure. There is no effect on the share of registered Democrats in the Census block group or on overall voter turnout in Columns 3 and 4 respectively. Together these results suggest that the results in Columns 1 and 2 are not purely a result of the activation of sympathetic voters.

I next look at how the effect of fire exposure varies spatially. Table 1.3 presents the results from Equation 2. Once again, Columns 1 and 2 tell a similar story. Consistent with the hypothesis that large fires are highly salient and have effects beyond the immediate burn scar, the coefficients in Table 1.3 are positive but decreasing with distance from the fire perimeter. A large fire increases big government voting by around 0.8 percentage points and increases pro-environment voting by around 2.4 percentage points in the areas most affected by the fire. The effect decays with distance such that support for the relevant initiatives is only around 0.3-0.5 percentage points higher for block groups between 5 and 20 kilometers from the fire. Again, the fact that the coefficient on $Fire\_Treatment$ is larger in Table 1.3 is in line with expectations that some partially treated block groups are included in the control group in Equation 1. The results of Table 1.3 are my preferred estimates because the distance specification more accurately captures the dynamic spatial effect of fire exposure on voting. Column 4 shows a statistically significant but economically small increase in the share of registered Democrats in burned block groups and for those 5 to 20 km away, though not within 5 kilometers. Once again, there is no meaningful effect on turnout.

My next step is to explore which voters are driving the increases in big government and environmental voting. Table 1.4 shows the results of the model in which indicators for fire proximity are interacted with party share of registered voters in each block group. Columns 1 and 3 run the model using the share of registered Democrats in the block group and Columns 2 and 4 use share of Republicans. Each share variable has been standardized about its mean, so the coefficient on $Demreg$, for example, should be read as a one standard deviation increase (9.3 pp increase) in the share of registered Democrats in a block group is associated with an increase of 3.5 percentage points in the vote share for big government positions. When interpreting the model, note that the share of registered Republicans in block groups directly experiencing a large fire ($Fire\_Treatment = 1$) is generally one standard deviation higher than in California as a whole. The share of registered Democrats is around one standard deviation lower while the share of independents is the same.

Table 1.4 confirms prior expectations that Democrats tend to support big government and pro-environment positions while Republicans oppose them in the state as a whole. The effect of large fires on big government and pro-environmental voting is increasing in the share of Republicans in burned areas and decreasing with the share of Democrats. That the effect of fires increases in more heavily Republican areas combined with lack of an effect on voter turnout suggests the mechanism for the increase is that exposure to fires causes a change in voter preferences from small- to big-government positions.

A logical question to ask is whether the change in voter preferences in burned, largely Republican areas is limited to initiatives or emblematic of a larger partisan shift toward Democrats. As mentioned previously in Table 1.2, there is little increase in Democratic registrations following a fire. I next look at vote shares for Democrats and incumbents in US House races. Because California moved to a "jungle" primary in 2012 through which the two highest vote getters in the primary advance to the general election, I restrict the sample to elections from 2002-2010 to avoid the selection problems created by the change in election structure. Included in the regression are any US House races in which a Democrat faced a Republican opponent. Results are shown in Table 1.5. I find fire exposure has no effect on the vote share for Democratic candidates. Combined with the results from Table 1.4, the null effect on Democratic vote share implies a limited shift of voter preferences toward support of issues rendered most salient by nearby fires. It also illustrates the usefulness of having independent measures of big government and environmental policy support that are not explicitly connected to party identification. Finally, comparing Columns 2 and 4, support for incumbents increases in unburned areas near a fire but does not show an increase in burned areas. Most incumbents in burned areas are Republicans. The results on incumbent share are consistent with the story that partisan shifts are not occurring, but that residents may wish to reward their representatives for suppressing fires near their neighborhoods.

To interpret the results as a change in preference in a discrete choice model, I regress the difference in the log share for and against the initiative on an indicator for fire exposure and the fixed effects. Results are shown in Table 1.6. Fire exposure results in a significant increase in voting for both big government and environmental initiatives. At the mean of 48% support for big government initiatives in ever-treated block groups, the coefficient of 0.02 represents a 0.5 percentage point increase in support for these positions. Likewise, fire exposure results in a 1.8 percentage point increase in pro-environment support at the mean.

To explore the dynamic effect of fire exposure, I run an event study as described by Equation 4. Here the time period is restricted to 2006 to 2014 such that the data requirements are met so as to properly bin the endpoints according to the process laid out by Schmidheiny and Siegloch (2019). While there are only three pre- and post- periods, the window spans over a decade of voting outcomes given the two year interval between observations. Figure 1.7

shows the effect of fire exposure is to increase big government voting by just over 1 percentage point for big government initiatives and just over 2 percentage points for environmental initiatives. Pre-period coefficients are not significantly different from zero in the four years prior to a fire. If anything, treated areas are trending away from both pro-environment and pro-redistributive voting. That the effect dies out rapidly is consistent with prior literature which finds transitory responses to wildfires (McCoy and Walsh (2018)) and floods (Gallagher (2014)). There is reason to expect the effect would be even more transitory in this setting. The stakes attached to a single individual's vote on a ballot initiative are much less than the cost involved in buying a home (McCoy and Walsh) or adding a flood insurance policy (Gallagher). Furthermore, as fire exposure has no effect on turnout, a change in position on an initiative is relatively costless conditional on having already made the decision to vote for other reasons.

I provide additional support for the identifying assumption through a falsification test asking whether future fires can predict voting patterns in the current year. That is, instead of $Fire\_Treatment = 1$ if a Census block group experiences a large fire in the two years preceding an election, I instead define $Treatment\_Future = 1$ if a Census block group experiences a fire in the two years following the election. A positive coefficient on $Treatment\_Future$ would reveal that burned areas are trending differentially toward big government and pro-environment voting in ways erroneously attributed to fire exposure. A null result suggests that it is indeed fire exposure, and not long-term differential trends in voter composition or ideology, that drive the main results in Tables 1.2 through 1.5. Here I restrict the sample to block groups only ever within 5 kilometers of a fire and, to avoid issues from previously treated block groups, I restrict the sample to only those block groups yet to experience a fire. Table 1.7 shows that future fires have no predictive power regarding big government or environmental voting. There is no effect on democratic registrations, though a there is a negative coefficient on turnout. Table 1.8 reproduces the main results of Equation 1 on the restricted sample of block groups used for the falsification test; the effect of fire exposure on big government and environmental voting remains positive and significant.

Finally, I use the difference-in-differences imputation estimator developed by Borusyak et al. (2021) to provide estimates that are robust to the presence of heterogeneous treatment effects. Because the approach assumes that, once treated, a block group is treated forever, I again limit the sample as in the falsification test of Table 1.7. There is no evidence of pre-trends and the effect of fire exposure is comparable to the main results with an increase of 0.7 percentage points for positions that increase the size of government and an increase of 1.3 percentage points for pro-environment policy positions.

1.6 Discussion and Conclusion

As global emissions continue to increase, the UN warns that nations will have to quintuple their commitments beyond those agreed to at the Paris climate summit in order to limit warming to 1.5°C. Nations will have to decide whether or not to make those commitments during a time in which climate-associated extreme events are becoming more severe and more frequent. Thus the question of how voters in a democracy respond to climate-associated disasters is crucial for understanding the roadmap forward for climate activists, skeptics, politicians, and activist groups alike. Those hoping to curb emissions using bold policy solutions enacted through the democratic process would wish to know whether public support can be expected to increase as climate change becomes more salient or if efforts should be focused in areas farther removed from voters. Skeptics may argue that the climate change trajectory is already highly salient and that voters are already rationally deciding what action is appropriate to take or not take.

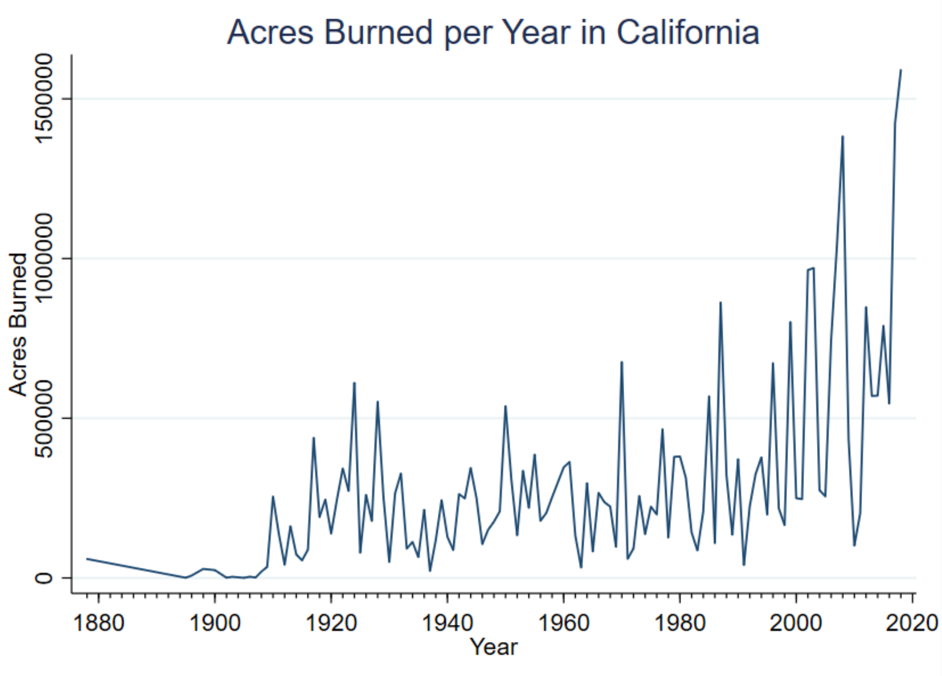
My study injects new evidence into the discussion by examining how voters respond to climate change-associated natural disasters using evidence from California. The California setting offers two distinct advantages: its frequent, severe, and climate-linked fire season, and its use of ballot initiatives that allow me to more directly measure support for big government and environmental policies than had been done previously. I leverage yearly variation in California wildfire activity across small geographies to explore the extent to which fire exposure shapes voter preferences. I find exposure to fires results in a statistically and economically meaningful shift of around 0.8 percentage points toward initiatives that expand government spending or taxation and a larger shift of 2.4 percentage points toward highly salient pro-environment positions. The effect decays with distance and is strongest for highly Republican areas. My result showing that voting for Democrats does not increase suggests voters are shifting preferences along those dimensions most closely related to their experience with fire without shifting wholesale party support.

The results presented here largely corroborate findings from previous literature while expanding on those findings in a more precise setting. I provide evidence that voters prefer the expansion of government services in the wake of disasters which is consistent with a risk aversion-driven desire for increased social insurance. Like the large survey-based literature on climate change, I find exposure to climate change-related anomalies impacts how people view the issue and like Deryugina (2013) I find the effect is largest among those who identify as more conservative. As in Liao and Ruiz Junco (2022), I find that exposure translates into political action, although my setting has the advantage of allowing me to disentangle support for an issue from support for a political party. The ability to disentangle support for a singular issue from the bundle of policy positions represented by a candidate is important in a setting like the United States in which political parties are polarized along many dimensions including size-of-government and environmental policies. My results confirm work by Hazlett and Mildenberger (2020) showing shifts in pro-environment voting following wildfire exposure using a broader definition of environmental support and an expanded panel of elections, although in contrast I find the effect is driven by Republicans rather than Democrats.

Taken together, my findings show salient climate change-associated events have the capacity to alter the relationship between individuals and government, moving people toward preferring a larger role for government in society. As climate change impacts become increasingly widespread, we can anticipate that individual demands of government will continue to evolve accordingly.

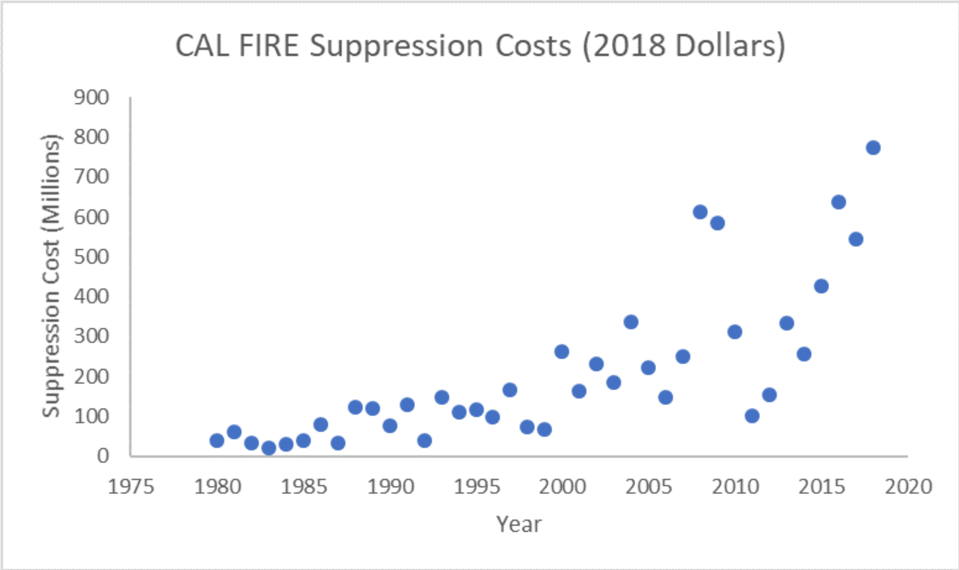
1.7 Figures and Tables

Figure 1.1: Fire Season Severity by Year



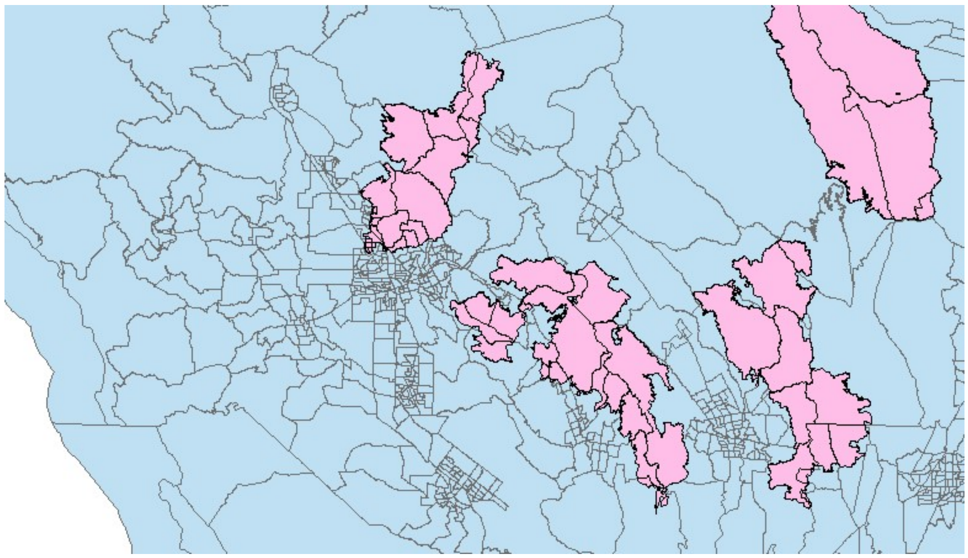
Number of acres burned per year in California. Source: California Fire and Resource Assessment Program (FRAP) fire perimeters data.

Figure 1.2: Fire Suppression Costs by Year



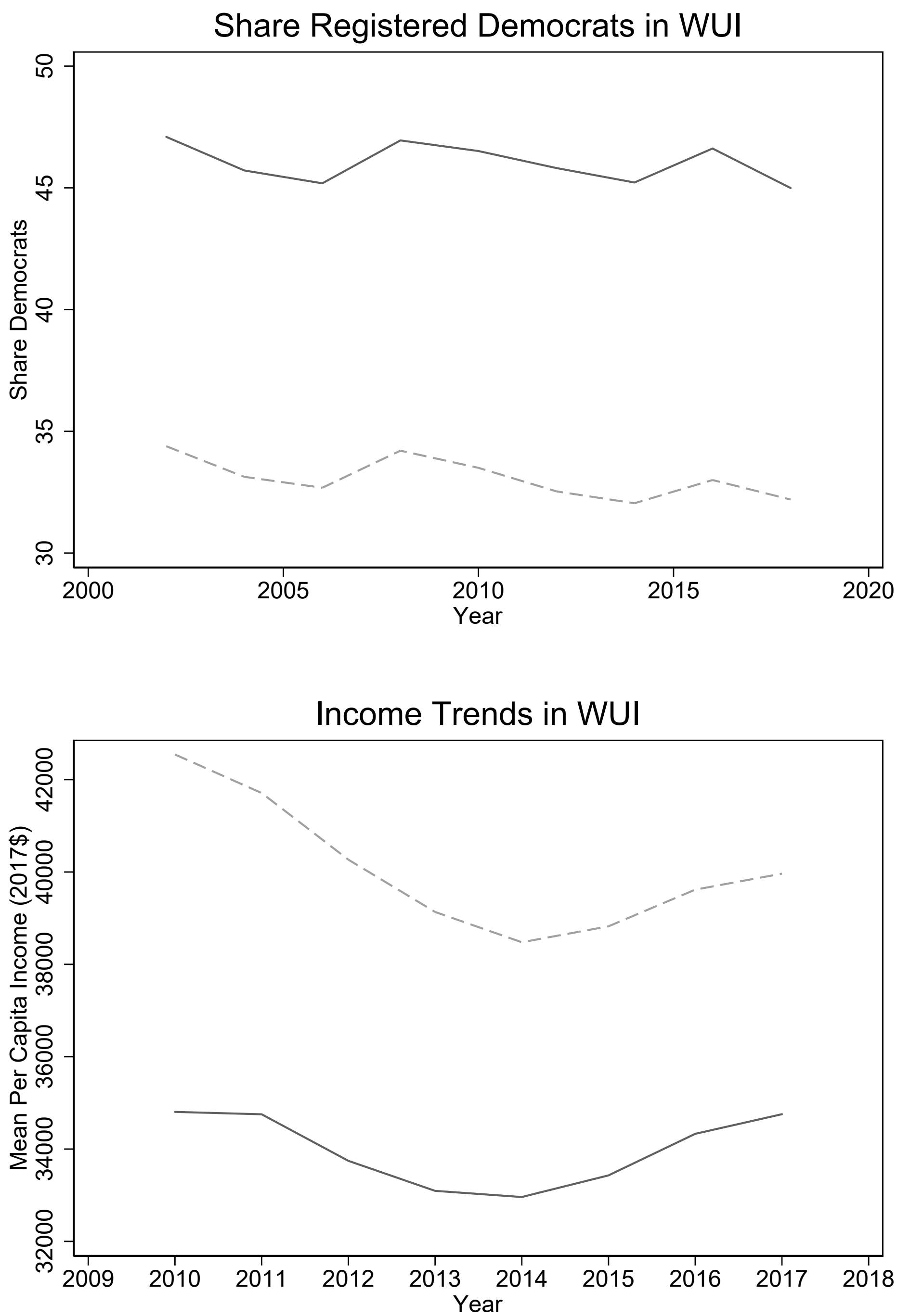
Emergency fire suppression costs by year. Source: CAL FIRE at https://www.fire.ca.gov/media/8641/suppressioncostsonepage1.pdf

Figure 1.3: Identifying Variation, Fire Perimeters Overlaying Block Groups



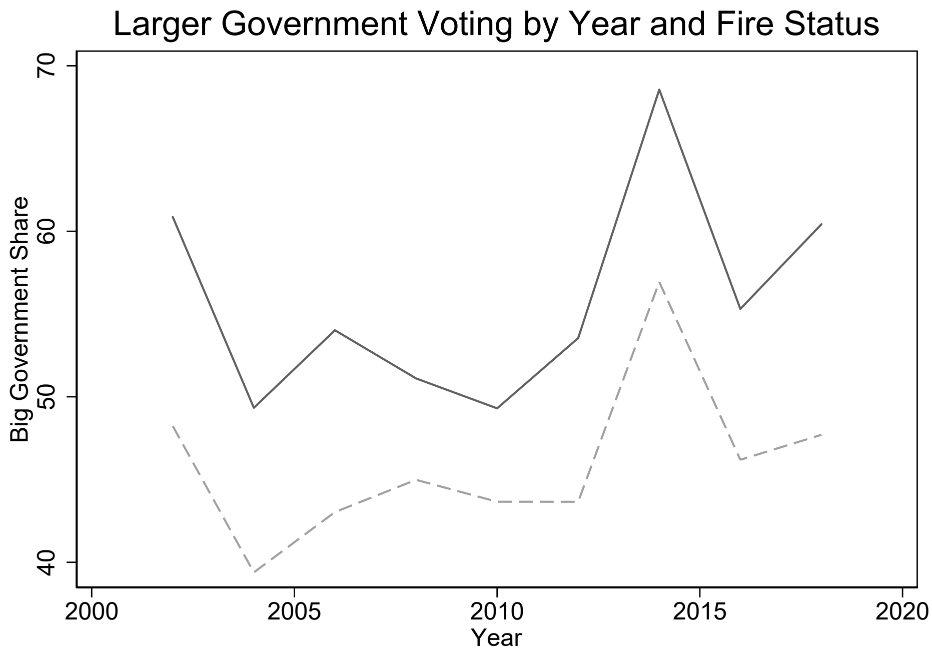
Large fires over 5000 acres (pink) in the Napa valley from 2017-2018. Block groups are considered treated for the 2018 election if any part of the fire perimeter overlaps any part of the block group (blue).

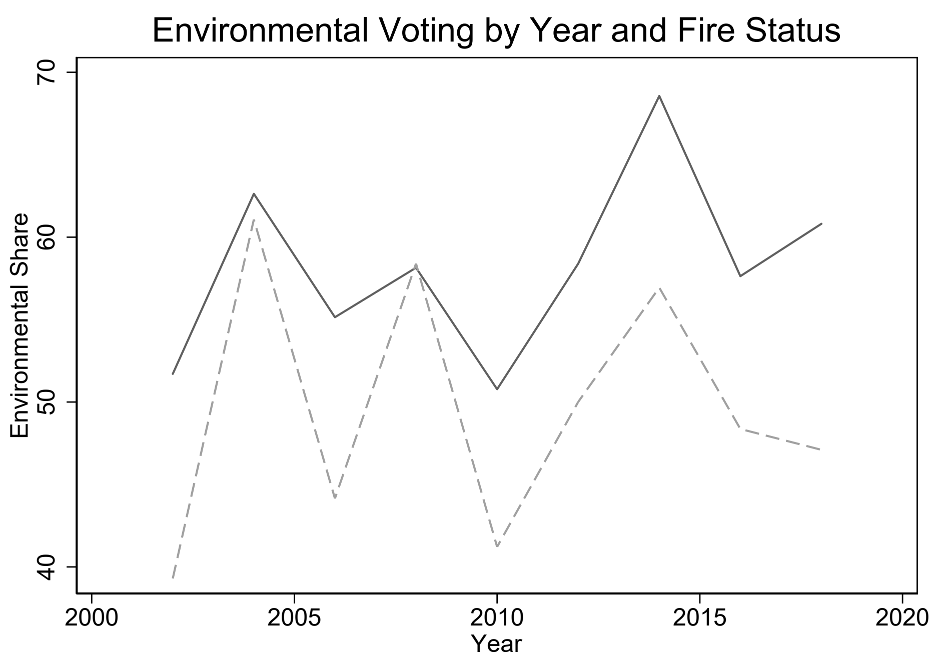
Figure 1.4: Trends in Share Democrats and Income in the Wildland-Urban Interface (WUI)



Trends in the mean share of registered Democrats (top) and mean per capita income (bottom). Pure control areas are represented by the solid line; ever-treated areas are represented by the dashed line. Areas that experience fires are wealthier and more Republican on average.

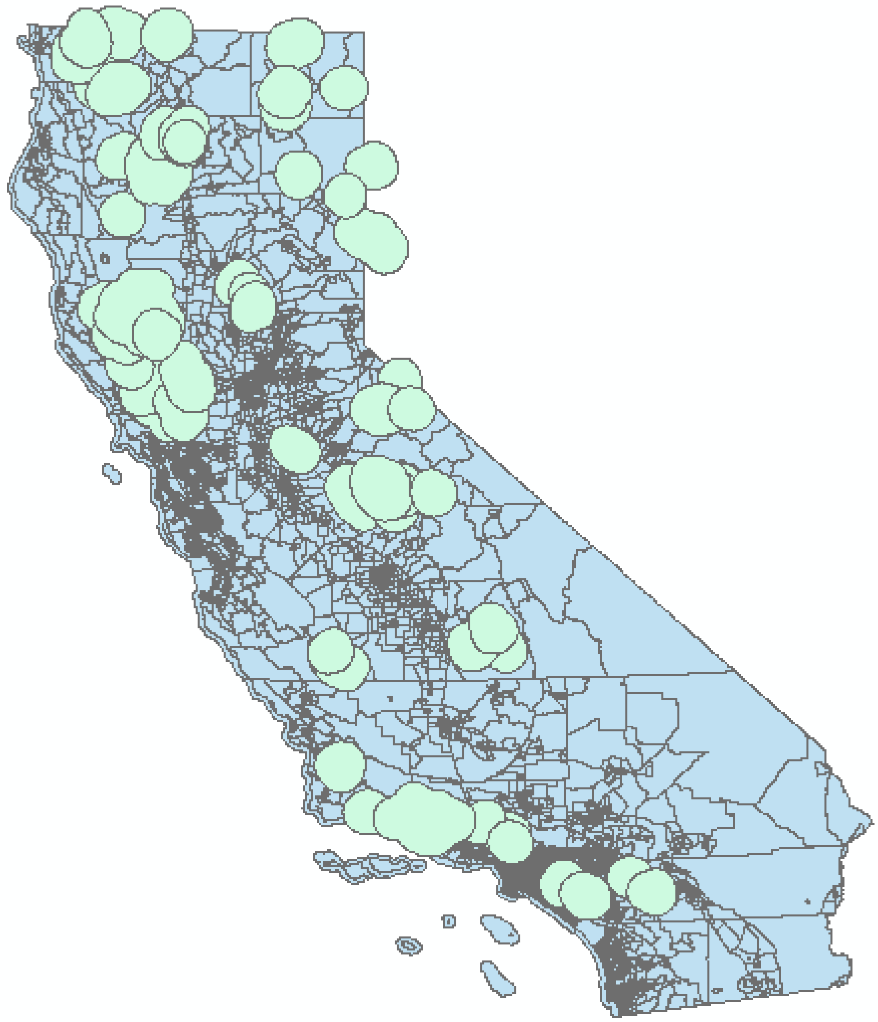
Figure 1.5: Voting Patterns in the Wildland-Urban Interface (WUI)





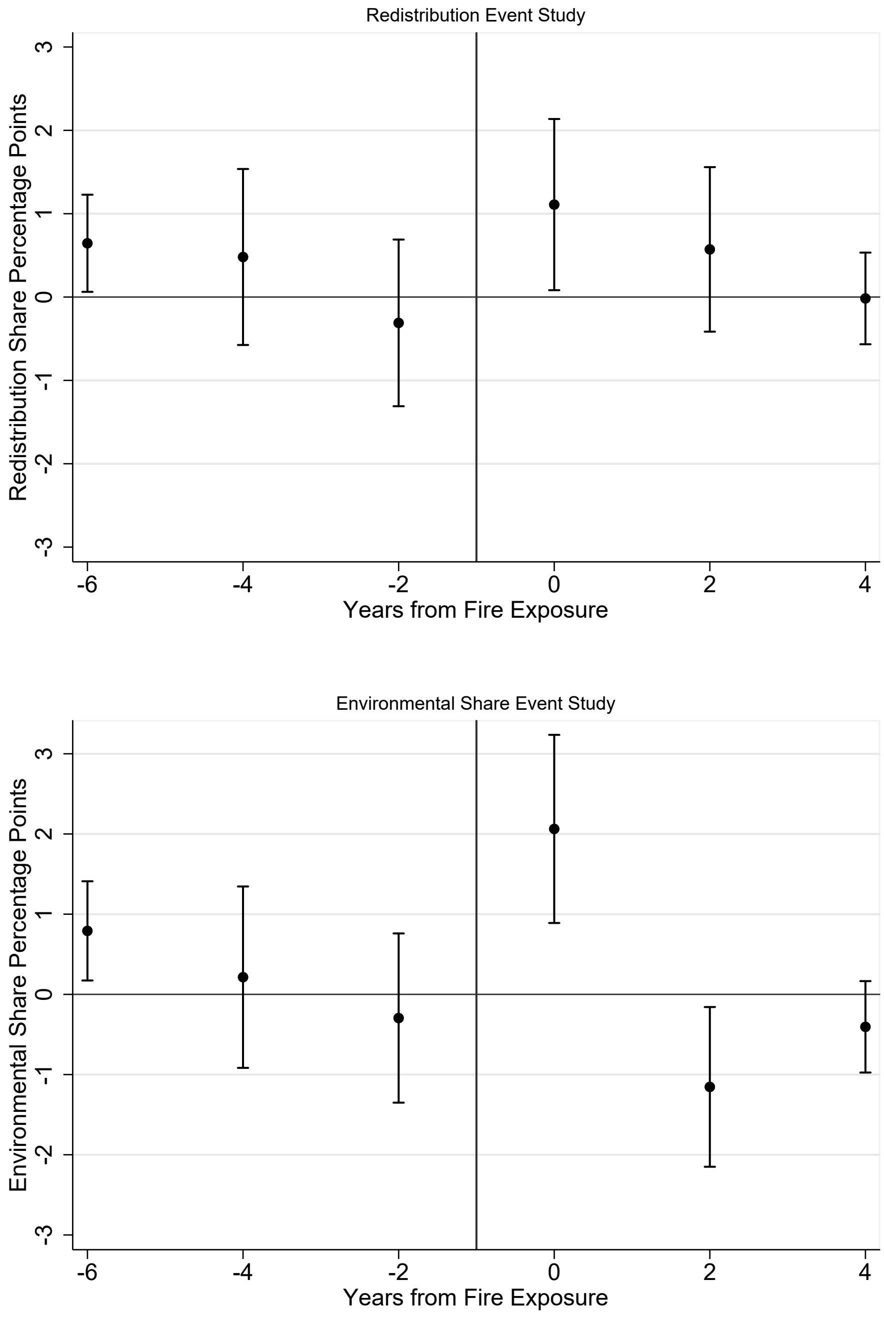
Trends in the mean vote share for big government positions on ballot initiatives (top) and pro-environment initiatives (bottom). Pure control areas are represented by the solid line; ever-treated areas are represented by the dashed line. Areas that experience fires are less supportive of both types of measures on average.

Figure 1.6: Example Coverage within 20km of a Large Fire



20km buffers around fires greater than 5000 acres in the state of California from 2017-2018. Block groups are considered with the 20km perimeter if any part of the buffer zone overlaps the block group.

Figure 1.7: Event Study Coefficients



Plot of event study coefficients. Endpoints are binned according to the multiple treatment approach described in Schmidheiny and Siegloch (2019). To satisfy the data requirements for the binning process, the observation window is limited to 2006 until 2014. Error bars represent 95% confidence intervals. Top: Big government sample Bottom: Environmental sample.

Table 1.1: Summary Statistics

	Areas with No Fires	Areas with Fires
Mean Share of Vote on Initiatives that Expand Government	58.84	47.98
	(10.83)	(9.706)
Mean Share of Vote on Environmental Initiatives	58.21	49.58
	(11.04)	(10.78)
Registered Republican Share	24.90	39.35
	(13.63)	(10.77)
Registered Democrat Share	45.67	32.43
	(12.68)	(9.380)
Per Capita Income	32154.8	37712.6
	(21000.2)	(17836.3)
Observations	208,805	208,805

Summary statistics for block groups. Column 1 includes block groups that never experience a large fire during the study period. Column 2 includes block groups that are included in the treatment group at some point during the study period; that is, block groups which have some portion directly coinciding with the burn scar of a large fire. Areas that experience a fire are generally wealthier, have a larger share of Republican voters, and have lower baseline support for initiatives that expand government or enact pro-environment positions.

Table 1.2: Effect of Large Fires on Initiative Voting and Electoral Composition

	(1)	(2)	(3)	(4)
VARIABLES	Big_Gov	Env_Share	Dem_Reg	Turnout
Fire_Treatment	0.462***	1.845***	0.127	-0.156
	(0.125)	(0.207)	(0.0963)	(0.135)
Observations	208,085	208,085	207,853	207,782
R-squared	0.900	0.848	0.963	0.942

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Fire_Treatment is an indicator equal to 1 if the block group experiences a large fire in the two years preceding the biennial election. The outcome variable is expressed in percentage points. Column 1 reports the effect on the vote share for initiatives that expand government. Column 2 reports the effect on environmental interest group-endorsed initiatives. Columns 3 and 4 report the effect on Democratic registrations and voter turnout. Regression includes block group fixed effects and county-by-election-year fixed effects. Robust standard errors are clustered at the block group level.

Table 1.3: Spatial Variation in the Effect of Large Fires

	(1)	(2)	(3)	(4)
VARIABLES	Big_Gov	Env_Share	Dem_Reg	Turnout
20kbuff	0.289***	0.498***	0.204***	-0.0799*
	(0.0326)	(0.0452)	(0.0267)	(0.0427)
5kbuff	0.786***	1.607***	0.0183	0.0713
	(0.0637)	(0.0915)	(0.0499)	(0.0784)
Fire_Treatment	0.780***	2.448***	0.234**	-0.180
	(0.128)	(0.211)	(0.0986)	(0.138)
Observations	208,085	208,085	207,853	207,782
R-squared	0.900	0.849	0.964	0.942

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Fire_Treatment is an indicator equal to 1 if the block group experiences a large fire in the two years preceding the biennial election. 5k buff is an indicator equal to 1 if a block group is less than 5km from a fire perimeter but did not itself experience a fire. 20k buff is an indicator equal to 1 if the block group is greater than 5km but less than 20km from a fire perimeter. The outcome variable is expressed in percentage points. Regression includes block group fixed effects and county-by-election-year fixed effects. Robust standard errors are clustered at the block group level.

Table 1.4: Effect of Large Fires by Party Affiliation

VARIABLES	(1) Big_Gov	(2) Big_Gov	(3) Env_Share	(4) Env_Share
20kbuff	0.224***	0.321***	0.313***	0.374***
	(0.0320)	(0.0319)	(0.0464)	(0.0464)
5kbuff	0.567***	0.641***	0.718***	0.684***
	(0.0785)	(0.0702)	(0.104)	(0.0965)
Fire_Treatment	0.268	0.275	0.191	-0.0341
	(0.187)	(0.179)	(0.248)	(0.225)
Dem_reg	3.524***		3.743***	
	(0.0714)		(0.0678)	
20kbuff*Dem_reg	-0.0434		-0.533***	
	(0.0312)		(0.0513)	
5kbuff*Dem_reg	-0.363***		-1.546***	
	(0.0819)		(0.121)	
Fire_Treatment*Dem_reg	-0.486***		-2.375***	
	(0.167)		(0.287)	
Rep_reg		-5.099***		-4.765***
		(0.0838)		(0.0894)
20kbuff*Rep_reg		-0.122***		0.528***
		(0.0294)		(0.0502)
5kbuff*Rep_reg		0.160**		1.602***
		(0.0678)		(0.107)
Fire_Treatment*Rep_reg		0.472***		2.725***
		(0.154)		(0.254)
Observations	207,753	207,753	207,753	207,753
R-squared	0.904	0.907	0.853	0.854

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Fire_Treatment is an indicator equal to 1 if the block group experiences a large fire in the two years preceding the biennial election. 5k buff is an indicator equal to 1 if a block group is less than 5km from a fire perimeter but did not itself experience a fire. 20k buff is an indicator equal to 1 if the block group is between 5km and 20km from a fire perimeter. Dem reg is the share of registered Democrats and Rep reg is the share of registered Republicans in the block group, both variables standardized around the mean.

Table 1.5: Effect of Large Fires on Voting for US House Candidates

VARIABLES	(1) Dem_Share	(2) Incum_Share	(3) Dem_Share	(4) Incum_Share
Fire_Treatment	-0.185	-0.573*	-0.0975	0.149
	(0.271)	(0.306)	(0.274)	(0.310)
5kbuff			0.138	0.801***
			(0.119)	(0.144)
20kbuff			0.107*	0.996***
			(0.0614)	(0.0741)
Observations	106,715	101,654	106,715	101,654
R-squared	0.963	0.911	0.963	0.911

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Fire_Treatment is an indicator equal to 1 if the block group experiences a large fire in the two years preceding the biennial election. 5k buff is an indicator equal to 1 if a block group is less than 5km from a fire perimeter but did not itself experience a fire. 20k buff is an indicator equal to 1 if the block group is greater than 5km but less than 20km from a fire perimeter. Dem Share is the share of votes received by Democratic US House candidates in races contested against a Republican. Incum Share is the vote share received by incumbent US house candidates. The study period is limited from 2002-2010, after which California adopted jungle primaries. The outcome variable is expressed in percentage points. Regression includes block group fixed effects and county-by-election-year fixed effects. Robust standard errors are clustered at the block group level.

Table 1.6: Effect of Large Fires, Difference in Log Share Specification

VARIABLES	(1) Log Difference Big_Gov	(2) Log Difference Env_Share
Fire_Treatment	0.0214***	0.0781***
	(0.00536)	(0.00875)
Observations	208,076	208,076
R-squared	0.889	0.847

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Fire_Treatment is an indicator equal to 1 if the block group experiences a large fire in the two years preceding the biennial election. The outcome variable is $ln(share\_yes) - ln(share\_no)$. In ever-treated areas, the mean share of support is 48% for big government initiatives and 49.5% for environmental initiatives. Regression includes block group fixed effects and county-by-election-year fixed effects. Robust standard errors are clustered at the block group level.

Table 1.7: Effect of Large Fires on Initiative Voting: Future Fire Test

	(1)	(2)	(3)	(4)
VARIABLES	Big_Gov	Env_Share	Dem_Reg	Turnout
Treatment_Future	0.180	-0.00109	-0.0780	-0.744***
	(0.156)	(0.203)	(0.132)	(0.251)
Observations	34,181	34,181	34,183	34,156
R-squared	0.912	0.865	0.960	0.945

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Treatment_future is an indicator equal to 1 if the block group experiences a large fire in the two years following the biennial election. The sample is limited only to block groups ever within 5 kilometers of a fire that have not yet experienced a fire. Column 1 presents the effect on big government positions, Column 2 for environmental initiatives, Column 3 for share of Democratic registrations and Column 4 for turnout. Robust standard errors are clustered at the block group level.

Table 1.8: Effect of Fires on Initiative Voting: Within 5kms, Treated Once

	(1)	(2)	(3)	(4)
VARIABLES	Big_Gov	Env_Share	Dem_Reg	Turnout
Fire_Treatment	0.690***	0.986***	-0.00511	-0.253
	(0.137)	(0.195)	(0.104)	(0.168)
Observations	35,214	35,214	35,217	35,190
R-squared	0.913	0.866	0.960	0.945

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table presents the results of Equation 1 limited to the sample used in the falsification test of Table 7; that is, only block groups ever within 5 kilometers of a fire and no previously treated units. Column 1 presents the effect on big government positions, Column 2 for environmental initiatives, Column 3 for share of Democratic registrations and Column 4 for turnout. Robust standard errors are clustered at the block group level.

Table 1.9: DiD Imputation

VARIABLES	(1) Big_Gov	(2) Env_Share
Fire_Treatment	0.720***	1.293***
	(0.134)	(0.182)
Pre Period t-1	0.289	-0.0466
	(0.198)	(0.269)
Pre Period t-2	0.218	-0.114
	(0.203)	(0.227)
Pre Period t-3	-0.00846	-0.405*
	(0.148)	(0.210)
Observations	35,286	35,286

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Results using the difference-in-differences estimator of Borusyak et al. (2021), robust to heterogeneous treatment effects. The sample is limited to block groups ever within 5 kilometers of a fire that have not been previously treated. There is no evidence of pre-trends across three pre-treatment periods representing six years of lead time.

2.0 Crime and Demand for Police

This paper studies how exposure to crime affects demand for policing using a unique setting where both crime and demand can be measured at the neighborhood level. Specifically, I use precinct level returns from ballot measures in San Francisco to provide novel evidence on how individuals' support for pro-police policies responds to exposure to crime. Using variation in criminal activity across neighborhoods around election day, I find that each additional violent crime leads to an increase in support for police union-endorsed ballot positions ranging from 2.9 percentage points for homicides to 0.4 percentage points for lesser crimes. The effects are present during biennial congressional elections but not during municipal elections, suggesting the results are driven by lower-propensity voters. The effects are also largest in areas with high shares of white residents.

2.1 Introduction

What is the relationship between crime and demand for police? Policing is a major public good provided by local governments. Spending on police accounts for around 0.5% of GDP in the United States and up to 30% of the municipal budget in several major American cities. Moreover, resource allocation for law enforcement has been at the center of a heated debate following several high profile incidents of police violence. Although an extensive literature in economics has studied the effect of the number of officers on crime (Levitt (2002), McCrary (2002), Klick and Tabarrok (2005), Evans and Owens (2007), Chalfin and McCrary (2018), Mello (2019)), there is little evidence on how crime – one manifestation of law enforcement effectiveness – shapes individuals' demand for police. The primary challenge in answering this question is that experience with crime is a highly localized phenomenon while the most concrete measures of public support for police are broad national-level surveys.

In this paper, I exploit a unique setting to provide novel evidence on the extent to which individuals' demand for police responds to changes in crime in their neighborhoods. Causal evidence on this question is crucial as the relationship between crime and demand for police is theoretically ambiguous. On one hand, it is reasonable to expect increases in crime could diminish demand for police because people believe police are ineffective at preventing crime; on the other hand, increases in crime could lead people to believe the police are under-resourced and need more support.

I bring new insight to this question by exploring how incidents of violent crime impact electoral support for positions on ballot measures which are supportive of police. Defining pro-police preferences using initiatives gives me a time-varying, revealed-preference measure which, when obtained at the precinct level, captures changes in demand for pro-police policies over neighborhoods approximately one-eighth of a square mile in size. The additional advantage of using ballot measures for this purpose is that a vote for a ballot initiative more directly captures support for a particular policy stance than a vote for a politician who represents a bundle of policy positions. Furthermore, the issue of support for policing does not cleanly break down ideological lines in the US political system; candidates from both parties routinely seek the endorsement of law enforcement.

The use of ballot measures as the outcome motivates San Francisco as the setting for this study because the city has a long and voluminous history of placing law enforcement-relevant propositions on the ballot. I use election returns provided by the San Francisco Department of Elections to create a panel of consistent geographies spanning 16 municipal, primary, and statewide elections from 2003 to 2018. During this time period San Francisco voters decided 37 individual propositions with endorsements by the police union. The panel allows me to leverage two sources of variation. First, I can explore how pro-police support varies spatially in response to crime. Second, I can exploit temporal variation in crime occurring within small windows around election day. This salience shock to citizens' perception of crime is plausibly exogenous to overall crime trends in the city. I utilize this variation to execute a difference-in-differences strategy which identifies changes in pro-police policy support in response to idiosyncratic shifts in crime. This strategy allows me to control for general time trends in demand for police and for unobserved differences across neighborhoods.

The results show that close proximity to occurrences of violent crime prior to election day leads to an increase in support for policies that have been endorsed by the police union. The effect is most striking for homicide. Voters residing in a census block group experiencing a homicide within a month of election day increase their vote share for pro-police positions on ballot initiatives by 2.9 percentage points. Crimes such as aggravated assault lead to a smaller increase of 0.4 percentage points in the vote share for pro-police positions. The effect of an additional crime decays with distance from the crime and with time until the election. The response is strongest in areas with high shares of white residents. I find the effects are only present in even-year November elections; there is no effect of violent crime on voting in primary or municipal elections. Given the substantial differences in the composition of the electorate across types of elections, the results suggest the effect is driven by lower propensity voters responding to a salience shock in the prevalence of crime.

As with any difference-in-differences identification strategy, the main threat to identification is violation of the parallel trends assumption; in this case, that areas with and without violent crimes occurring before the election may be trending differently with respect to demand for police. My empirical specification includes a full set of election-specific dummies to flexibly control for time trends in pro-police voting. However, I also address the concern that crime and non-crime areas are trending differently by asking whether future crimes can predict voting patterns in the current election. I find crimes occurring in the month after the election have no effect on pro-police voting. I provide a further falsification test using police warrant incident reports instead of violent crimes as the treatment variable. The null result of this test provides additional assurance that the identifying assumption is upheld in this setting. Additionally, I show the findings are robust to accounting for heterogeneous treatment effects using the DiD-imputation method of Borusyak et al. (2021).

Because the contemporary debate around support for police centers on the role of officer-involved violence, I next explore the effect of these incidents on demand for police. I geocode incidents of police violence from the San Francisco Police Department's Officer-Involved Shooting Investigations dataset. Using an event study framework, I find that police shootings, including those in which the person died, do not appear to produce changes in voting; however, caution is warranted when interpreting these results as confidence intervals are wide. Given over 80% of residents of San Francisco are either white or Asian, a null finding comports with Ang and Tebes (2020), who shows responses to incidents of police violence are limited to black and Hispanic individuals.

The finding that exposure to crime increases demand for pro-police policies is consistent with prior work on public service provision at the local government level. Burnett and Kogan (2017) use the case of road repair to show that voters in municipal elections engage in retrospective voting on the quality of public goods under the purview of city authority. Additionally, the election of nonwhite city council members is associated with a decline in the share of nonwhite citizens arrested (Beach et al. 2019). Like road repair, policing is predominantly a local public good, so voters at the municipal level have reason to expect their decisions at the ballot box will translate into meaningful change. That the response is strongest in areas with high concentrations of white voters reflects survey evidence showing differences in satisfaction with public goods provision by race, with white survey respondents expressing satisfaction with police 20 percentage points higher than their black counterparts (Hajnal and Trounstine (2014)).

This work is related to previous studies which address the role personal experience, particularly exposure to crime, plays in shaping individuals' political participation (Bateson (2012), Sønderskov et al. (2020), Blattman (2009), Bellows and Miguel (2009)). While the results of this literature are mixed, these survey-based studies generally show that crime victimization increases civic engagement. Most closely related is Ang (2021) who show that incidences of police violence increase voter turnout in black and Hispanic communities in subsequent elections and increase support for certain criminal justice-related referenda. Ang and Tebes (2020) documents the negative effect of police violence on student performance more generally, an effect which exists only for black and Hispanic students. My contribution is to look directly at how demand for police responds to incidents of crime more generally rather than focusing solely on incidents of police violence.

This paper also complements the literature on the electoral accountability of law enforcement. Facchini et al. (2020) show that increases in political power for black citizens following the Voting Rights Act led to decreases in black arrest rates in counties with elected sheriffs. Nowacki and Thompson (2021) document patterns of law enforcement behavior that more closely align with preferences of the electorate regarding, for example, drug arrests after police are exposed to elected oversight. Other work shows the responsiveness of voters to crime rates in gubernatorial elections (Cummins (2009)). Together, these studies illustrate the fact that law enforcement officials and public officials more generally are sensitive to the electoral consequences regarding their performance as it relates to crime. My finding that voters are responsive to crime complements this previous literature showing police are responsive to voters. My setting offers the particular advantage of precluding reverse causality by identifying the effect of crime on pro-police voting off of idiosyncratic changes in the timing of violent crimes over which police have little discretion.

In a precisely identified setting that directly measures preferences for pro-police policy positions, I show that voters react to shocks to the salience of crime at the neighborhood level by increasing demand for police. This contribution sheds light on the ever-evolving relationship between police and the community by illustrating a link between law enforcement and the citizens who ultimately decide the allocation of public resources through the ballot box.

2.2 Background and Data

2.2.1 Ballot Initiatives and Voting Data in San Francisco

What constitutes demand for policing? I define demand for policing as voting in favor of positions on ballot initiatives endorsed by the police union; in other words, voter demand for policies that the majority of police offers would prefer to have enacted by the city government. I aim to capture whether localized experiences with crime act to shift police and voter preferences over policies into greater alignment.

This study is one of several to utilize ballot initiatives as a means of eliciting preferences over public goods, particularly in the context of California. Previous estimates using referenda to measure willingness to pay for public goods can be found in Vossler et al. (2003) and Vossler and Watson (2013). More recent work by Burkhardt and Chan (2017) estimates willingness to pay for policy outcomes associated with 13 California ballot initiatives. My use of ballot initiatives is most closely related to that of Brunner et al. (2011). Their work uses the mean vote share across all ballot initiatives in California supported by Democrats as a measure of preference for redistribution; Hazlett and Mildenberger (2020) and Coury (2021) likewise use mean share across ballot initiatives as a measure of support for environmental policies. I use mean vote share across police union-endorsed ballot initiatives to isolate those positions subject to review by voters that most directly reflect law enforcement policy interests.

The use of ballot initiatives as the outcome motivates San Francisco as the choice of setting. San Francisco has a long history with ballot initiatives. The initiative and referendum process in the city dates back to 1898. Citizens may vote on issues referred by the city council or may initiate a measure having obtained signatures equal to 5% of the turnout in the preceding mayoral election. It is not uncommon for San Francisco voters to decide upwards of a dozen measures per election; for example, 25 initiatives were listed on the November 2016 ballot. Few if any other cities in the United States vote on such a diverse and voluminous array of ballot measures. This is in part a consequence of the low threshold needed to place initiatives on the ballot in San Francisco. Around 9,000 signatures were needed to place an initiative on the ballot in 2020; by comparison, Los Angeles required around 60,000 signatures (10% of the votes cast in the preceding mayoral election). San Francisco's long, consistent, and high-frequency use of ballot initiatives allows me to construct a 16 year panel with a stable definition of local demand for policing.

To determine which of these initiatives reflect support for policing, I turn to the San

Francisco Voter Guide that accompanies each election, available online through the San Francisco Public Library's Ballot Propositions Database.[1] The Voter Guide provides a synopsis of important election information for voters. Details include the date of the election, instructions on how to fill out a ballot, and statements from candidates. For ballot initiatives, the Voter Guide explains the current law as it relates to the measure in question, estimates the potential cost to taxpayers from enactment, and offers arguments for and against each proposition from interested groups.

I define a policy position on a ballot initiative as pro-police if the Voter Guide indicates an endorsement on the initiative from the San Francisco Police Officers Association (SFPOA) or an organization sponsored by SFPOA such as the San Francisco Police Activities League (SFPAL). The SFPOA is the largest police union in San Francisco with a membership over 2,000; union members themselves comprise nearly one quarter of the signatures required to place an initiative on the ballot. The Voter Guide frequently includes arguments from the SFPOA advocating their preferred positions on ballot initiatives; an example can be seen in Figure 2.1. My outcome variable of interest is the mean share of the vote for SFPOA-endorsed positions on ballot measures across all measures that received an SFPOA endorsement in a given election. The resulting sample consists of 37 ballot initiatives across 16 general, municipal, and primary elections from 2003 to 2018 out of 213 total local ballot measures during the study period. Endorsed propositions often concern modifications to pension and retirement benefits for police offices (for example, Proposition A 2013), or changes in laws that police officers enforce (rules for homeless tent encampments, Proposition Q 2016). Appendix A contains the full list of endorsed propositions.

[1] Ballot Propositions Database: https://sfpl.org/locations/main-library/government-information-center/san-francisco-government/san-francisco-1/san-1

I use data on election returns from the San Francisco Department of Elections' Statement of Vote.[2] The Statement of Vote contains results by precinct for all of the local ballot initiatives. I obtain precinct shapefiles for statewide elections from California's Statewide Database which is maintained by law at the University of California, Berkeley for use in redistricting projects. The Statewide Database also provides crosswalks to convert precincts, the boundaries of which may change every election, to constant census geographies. I use 2010 census block groups as the unit of observation in this study to maintain constant geography. For municipal elections not hosted at the Statewide Database, I obtained precinct shapefiles through the San Francisco Department of Elections, then constructed a crosswalk to 2010 census blocks using areal interpolation. I combine the election returns with geocoded incidents of crime, homicides, and police shootings using TIGER/line shapefiles for block groups in San Francisco County. I also draw demographic data from the 2010 census (Manson et al. (2021)).

Table 2.1 presents summary statistics. Pro-police policies tend to be popular with average support around 60%. The violent crime rate is approximately 400 per 100,000, where violent crimes include aggravated assault, attempted homicide, and homicide. This crime rate is typical for similarly sized cities in the United States (FBI UCR). However, San Francisco is not demographically representative of a major American city. The share of Asian residents is much larger at 32% while the city has a smaller share of white residents (44%) and especially a smaller share of black residents (5%). The share of Hispanic residents is comparable to other urban areas in the US at 14%.

Figures 2.2 and 2.3 show the general trend of pro-police voting as it relates to neighborhood characteristics. Figure 2.2 shows how pro-police support varies with respect to the

[2]available: https://sfelections.sfgov.org/past-election-results

racial composition of neighborhoods. Pro-police support is increasing in the share of white residents and decreasing in the share of Asian and Hispanic residents. It is also increasing in the share of black residents, but as San Francisco is less than 6% black, there is a lack of support across the distribution of share black from which to draw a concrete conclusion. Figure 2.3 shows that pro-police voting tends to be slightly higher in areas with higher crime rates. While there is variation in demand for police, the main takeaway is that pro-police positions enjoy consistent and broad-based support in the vicinity of 60% across neighborhoods. Inclusion of a block group fixed effect captures any remaining difference in pro-police voting determined by persistent demographic or crime characteristics.

2.2.2 San Francisco Crime Data

To capture the effect of crime on electoral support for police over small geographies, I use geocoded crime incident reports available from the *Police Department Incident Reports: Historical 2003 to May 2018* dataset published by the San Francisco Police Department (SFPD).[3] This dataset contains over two million police incident reports and includes the geographic coordinates of each incident, the date of occurrence, category of crime, and a brief description of the incident.[4]

One limitation of this data is that the *Incident Reports* do not contain information on homicides prior to the changes in 2018. I therefore supplement the *Incident Reports* with homicide data compiled by the *Washington Post* for their “Murder with Impunity” project.[5]

[3]Access available through the DataSF website for the city and county of San Francisco, https://datasf.org/

[4]I restrict my analysis to the time period covered by the 2003 to 2018 dataset, as DataSF notes fundamental changes occurred in the reporting system after 2018.

[5]noa (2018)

This dataset spans 2007 to 2017 and includes geographic coordinates of the homicide in addition to information on the victim, date of occurrence, and whether the homicide was cleared via an arrest of a suspect. Figure 2.4 compares the count of observations in the *Washington Post* data with the aggregate number of homicides per year in San Francisco reported by the SFPD; it is evident that very few homicides occurred which are not reflected in the data from the *Post*. San Francisco averaged 60 homicides per year from 2007 to 2017 with a maximum of 99 homicides in 2007 and a minimum of 44 in 2014.

2.2.3 Police Shooting Incidents Data

I further explore the effect of police performance on electoral support for police by examining the role of police shootings. I obtain data on police shootings from the SFPD's Officer-Involved Shootings Data.[6] The department maintains a list of police shooting incidents dating back to 2000. The file contains date of occurrence, location of the shooting or location where the incident began (typically in the form of an address block, which I geocode to match voting data), a short description of the encounter, and whether the person died. There were 84 people shot by SFPD during the main study period from 2003 to 2018; 40 of those people died. It is important to note that this data presents only a one to two sentence description of the incident, not the results of a full investigation. While it appears, for example, that most incidents occurred with a crime in progress or after officers were fired upon, I lack detailed insight into the circumstances surrounding each incident.

[6] available online: https://www.sanfranciscopolice.org/your-sfpd/published-reports/officer-involved-shootings-ois-data

2.3 Empirical Strategy

To estimate the effect of crime and police performance on demand for policing, I use a standard difference-in-differences strategy conducted on a panel of 2010 census block groups. Each of the 579 block groups has one observation per election. There are 16 elections in the study from 2003 to 2018 that contain SFPOA-endorsed ballot initiatives: six November even-year general elections, six odd-year municipal elections, and four even-year primary elections. The outcome variable is the mean share of the vote for the designated positions on ballot initiatives across all SFPOA-endorsed measures in that election. I define treated block groups as those in which a violent crime or homicide occurs within j days of election day. Figure 2.5 shows the distribution of violent crimes in San Francisco in 2016 highlighting those which occur prior to election day. I estimate forms of the following equation:

$$Y_{st} = \alpha + \beta_1 CountViolent_{st_{-j}} + \delta_s + \gamma_t + \epsilon_{st} \tag{2.1}$$

Where Y_{st} is the mean share of the vote for SFPOA-endorsed positions on ballot initiatives in block group s in election t measured in percentage points, $CountViolent_{st_{-j}}$ is the count of homicides or violent crimes experienced in the block group within j days preceding the election, δ_s is a block group fixed effect, and γ_t is an election-specific time fixed effect. The coefficient of interest is β_1 measuring the responsiveness of pro-police voting to variations in local crime. A positive β_1 indicates movement toward the endorsed positions. The block group fixed effect captures time-invariant unobservable determinants of voting across block groups. The election specific fixed effect flexibly controls for time trends and shocks common to all block groups within San Francisco such as the overall crime rate or TV media campaigns. Additionally, the election-specific fixed effect captures baseline support for the particular set of initiatives in that election. I use heteroskedasticity-robust standard errors clustered at the block group level for all regressions.

Having laid out the main specification, I run variants of Equation 1 to test additional hypotheses. In particular, given the average size of a census block group in San Francisco is less than 0.25 square miles, it is reasonable to expect nearby block groups to be partially treated by an event in a neighboring block group particularly for an incident as notable as a homicide. To isolate the effect of these spillovers, I run a variation of Equation 1 that allows the effect of a homicide on pro-police voting to vary with distance from the incident. To the extent that neighboring block groups are also impacted, we would expect 1) the effect of a nearby homicide to decay with distance from the homicide and 2) β_1 of Equation 1 to underestimate the true effect of homicide since partially treated spillover areas are included in the control group. To test these hypotheses, I estimate the following equation:

$$Y_{st} = \alpha + \beta_1 Homicide_{st_{-j}} + \beta_2 1000buff_{st_{-j}} + \beta_3 2000buff_{st_{-j}} + \delta_s + \gamma_t + \epsilon_{st} \tag{2.2}$$

Where $Homicide_{st_{-j}}$ is the count of homicides within 500 ft of the block group within j days preceding the election; $1000buff_{st_{-j}}$ is the count of homicides less than 1000 ft from the block group but the block group itself did not experience a homicide, and $2000buff_{st_{-j}}$ is the count of homicides less than 2000 ft but greater than 1000 ft from the block group within j days prior to the election. If homicides have spillover effects on voting over small distances, we would expect $\beta_1 > \beta_2 > \beta_3$ and β_1 from Equation 2 larger than the estimate from Equation 1. Figure 2.6 illustrates this empirical approach for homicides occurring prior to the 2016 November election.

Next, I explore the effect of police shootings and police killings on electoral support for police through an event study framework. Again, the identifying assumption is that block groups in which a police shooting occurred would have experienced the same trend in pro-police voting as untreated block groups absent the event. Equation 3 is the standard event study specification:

$$Y_{st} = \alpha + \delta_s + \gamma_t + \sum_{i \in PRE} \beta_i 1\{t - t_s^* = i\} + \sum_{i \in POST} \pi_i 1\{t - t_s^* = i\} + \epsilon_{st} \qquad (2.3)$$

Where δ_s is again a block group fixed effect and γ_t is again an election-specific time fixed effect. The coefficients β_i capture trends in the pre-period prior to a police shooting or killing occurring in block group s at time t_s^*. The post-period coefficients π_i capture the responsiveness of pro-police voting to the shooting incident.

Finally, I wish to address the main threat to the empirical approach, which is that areas experiencing crime before the election could be differentially trending toward increasing pro-police support in ways that are not related to crime or captured by the fixed effects. I therefore run a version of Equation 1 using future crime as the treatment variable:

$$Y_{st} = \alpha + \beta_1 CountViolent_{st+j} + \delta_s + \gamma_t + \epsilon_{st} \qquad (2.4)$$

Equation 4 is the same as Equation 1 with the exception that $CountViolent$ is now the count of crimes or homicides that occur in the future within j days after an election rather than within j days prior. This specification tests whether future crimes are predictive of current year voting patterns. A positive coefficient on β_1 implies the results are driven by trends unrelated to crime. A null result provides suggestive evidence that the identifying assumption is upheld in this setting.

2.4 Results

2.4.1 Violent Crime

I first look at the effect of crime on demand for policing by examining the impact of homicides. Homicides are potentially the most salient incidents under consideration and therefore likely to induce the largest shifts in demand for police. Table 2.2 shows the results of Equation 1, where the treatment variable represents the count of homicides occurring within 1000 ft of the block group. Column 1 shows the effect of a homicide within 30 days of a November general election is to increase support for pro-police positions on ballot initiatives by 2.9 percentage points. Columns 2 and 3 expand the treatment window to 60 days and one year respectively. Perhaps unsurprisingly, the coefficient declines from 2.9 percentage points for homicides occurring within 30 days to 0.37 percentage points for homicides occurring as far away as one year. The declining coefficient size is consistent with a salience effect or diminishing margins in the effect of homicides-the greater the time to the election, the less impact each homicide has on voting. Column 4 presents results from the falsification test of Equation 4. Homicides that occur after the election fail to predict any change in pro-police voting which provides support for the identification assumption that the results are not driven by differential trends across treated and control groups.

I next look at how the effect of a nearby homicide varies spatially. I draw rings around each homicide event. Here *Homicide* contains block groups that directly experience a homicide prior to the election or have some portion of the block group within 500 ft of the homicide location. The 1000 ft buffer contains block groups intersecting the annulus of inner radius 500 ft and outer radius 1000 ft drawn from the homicide location. The 2000 ft buffer contains block groups between 1000 and 2000 ft from the homicide location. Table 2.3 presents the results from the distance specification of Equation 2. The coefficient on block groups nearest to a homicide is 3.4 percentage points. This larger result is expected in the presence of geographic spillover effects, with those nearby block groups experiencing spillovers being included in the control group in Table 2.2. The effect declines with distance such that a block group between 1000 and 2000 ft from a homicide increases pro-police voting by 2.4 percentage points relative to block groups more than 2000 ft from any homicide. Moving from Column 1 to Column 3 again shows the effect declines temporally, as homicides further removed from election day have less of an impact on pro-police voting. Column 4 presents the falsification test using future homicides to define the treatment variables. Results are not statistically significant and the coefficients lack a consistent sign.

I next look at the effect of homicide on pro-police voting by race. Because I cannot observe individual votes, I limit the sample to those block groups in the highest quartile of share of residents reporting the respective race in the 2010 census. Table 2.4 shows the increase in demand for policing is strongest in the areas with the highest share of white residents. These block groups are greater than 63% white and experience a movement of 3.7 percentage points toward pro-police policy positions. Smaller increases in demand for policing are seen in the highest quartile for Asian residents (2.0 percentage points toward pro-police positions among block groups with >49% Asian share), and smaller still for block groups in the highest quartiles of Hispanic and black residents. The highest share Hispanic sample includes block groups with a greater than 19% share Hispanic residents, and the highest share black sample includes block groups with a greater than 6% share of black residents. The small number of Hispanic and black citizens in San Francisco introduces an important limitation on the interpretation of the increase in demand for policing in these areas. It may be the case that the impact of crime on demand for policing among black and

Hispanic residents is zero or even negative, with the net increase observed in Table 2.4 being driven by the majority white and Asian residents of those block groups. Thus it cannot be said that black and Hispanic voters respond to crime by increasing demand for police, but only that the change in their demand for police in response to crime is lower than that of white residents.

Having explored the impact of homicides on pro-police voting, I next look at the effect of other violent crimes on support for police. I use the *Incident Reports* database which does not include homicide incidents. For this analysis, I define violent crime as attempted homicide or aggravated assault both of which are violent crimes according to the FBI Uniform Crime Reporting definition. The results of Equation 1 are presented in Table 2.5 where the treatment variable is the count of violent crimes occurring in the block group prior to the election. The sample of elections is once again limited to November general elections. Column 1 shows that each violent crime occurring within 30 days of the election increases pro-police voting by 0.4 percentage points. Again, the effect of each violent crime decays with increasing time prior to the election, such that crimes occurring one year before the election have no effect on voting. That the coefficients are smaller for other violent crimes than the coefficients for homicides in Table 2.2 is unsurprising as homicides are events with a much greater salience.

I next investigate the origins of the measured increase in demand for police by looking at the effect of homicide and other violent crimes on turnout. Results are presented in Table 2.6. A homicide occurring in the 30 days before an election reduces turnout by 0.7 percentage points. Though the effect is negative and significant, the magnitude is such that turnout alone cannot explain the increase in demand for police. Other violent crimes have no effect on turnout. The lack of impact on turnout suggests the increase in demand for police is being driven by changing preferences of voters rather than compositional changes in the electorate.

Table 2.7 is identical to Table 2.5 except the elections included in the sample are 12 primary and municipal elections instead of six November even-year elections. The effect of violent crime is eliminated; if anything, the effect is reversed in sign. The finding suggests that the very different electorates across types of elections respond differently to a spike in the salience of crime prior to the election. Turnout in a presidential November election is around 75%; turnout is 30-45% for primary and municipal elections. Voters in primary and municipal elections are the highest-information, highest-propensity voters; a priori, it is reasonable to expect these voters to be the least swayed by a police union endorsement and the least likely to change an intended vote on a ballot measure without placing singular incidents in the context of overall trends in crime.

I present another falsification test in Table 2.8. Here, I use the count of incidents in the Warrants category from the Police Incident Reports database as the treatment variable. Because warrant-related reports are much more routine than occurrences of violent crime (indeed no immediate crime is necessary for police to conduct warrant-related activity), these events should have a much smaller effect, if any, for the count of warrant incidents relative to the effect for violent crimes. Table 2.8 shows no effect of warrant incidents, providing further evidence that the main results are not being driven by trends related to overall levels of police activity.

The use of a two-way fixed effects model with variation in treatment timing introduces concerns about weighting of treatment effects arising from an implicit assumption of treatment homogeneity as highlighted by a number of recent papers (de Chaisemartin and D'Haultfœuille (2020), Borusyak et al. (2021), Goodman-Bacon (2021), Abraham and

Sun (2018), Callaway and Sant'Anna (2021)). I therefore show the results are robust to allowing for heterogenous treatment effects using the efficient DiD-imputation method of Borusyak et al. (2021). Table 2.9 presents the results; there is little evidence of differential pre-trends. The effect of homicide is much larger at 5.3 percentage points. The corresponding event study is shown in Figure 2.7. Block groups experiencing a homicide exhibit a large increase in demand for police which quickly returns to baseline within two general election cycles.

2.4.2 Effect of Police Shootings

Figures 2.8 and 2.9 show the effect of police shootings and police killings respectively on pro-police vote share using the event study specification of Equation 3. Police shooting incidents are drawn from the SFPD's Officer-Involved Shooting Investigation file; police killing incidents are those incidents from the file which indicate that the victim died either on site or at the hospital. Neither figure shows evidence of trends in the pre-period which is consistent with the identifying assumption. There also appears to be no effect of police shootings or police killings on pro-police voting in subsequent elections. However, it is important to note three important limitations of the event study analysis. First, confidence intervals are wide. It may be the case that an effect exists, but given the data I am unable to estimate the effect with sufficient precision. Second, the effect on demand for policing may be specific to the circumstances surrounding a particular incident; for example, it is likely that police shooting an unarmed individual produces an opposite response in demand for policing than the police shooting an armed individual in the process of endangering bystanders. However, I have limited ability to disentangle these circumstances in the Officer-Involved Shooting Reports. Third, it is not uncommon for a police killing to receive widespread media attention even at the national level. Identification in my study relies on differences in treatment exposure across small neighborhoods; in the event of a major story surrounding police violence, all block groups in San Francisco may be exposed to treatment. In that case, the effect of the incident will be absorbed into the election-specific fixed effect, and this study fundamentally is unable to measure the impact, if any, of these incidents on demand for police. Finally, these results should be interpreted in light of findings by Ang and Tebes (2020) and Ang (2021) who show incidents of police violence only produce responses among black and Hispanic residents. A similar effect may be present here, but I am unable to detect it given the relatively small share of black and Hispanic residents in San Francisco.

2.5 Discussion and Conclusion

The question of demand and resource allocation for policing is one of the most hotly debated topics in US public discourse. Calls for police reform and police accountability are likely to continue as the issue remains a central rallying cry for parties on all sides of the political debate. Furthermore, the prevalence of violent crime is one measure of police performance as defined by police departments such as SFPD. Many American cities experienced a spike in homicides during 2020 after several years of steady or declining rates. Understanding the convergence of these two trends- the first, an ongoing reevalution of police-community relations; the second, changes in crime- is important for guiding policy responses in an evidence-based manner.

My study offers the first evidence on the responsiveness of demand for policing to salient changes in violent crime. The setting of San Francisco offers the advantage of providing a rich, detailed measure of support for policing through its long and extensive use of ballot measures, many of which are police union-endorsed. This allows me to define demand for policing as an alignment between police and voter policy preferences. I leverage temporal and spatial variation in homicides and other violent crime across small geographies to examine the extent to which demand for policing responds to salience spikes in these incidents. I find a homicide occurring in a small neighborhood within a month of a November general election results in a statistically and economically meaningful shift of around 2.9 percentage points toward initiatives that align with police union interests. I document a smaller shift of around 0.4 percentage points for each lesser violent crime that occurs in the month preceding an election. The effect decays with time from election day and with distance from the event. There is no effect for municipal and primary elections, suggesting the effect is driven by the higher responsiveness of lower-propensity voters. Increases in demand for police are largest in areas with high concentrations of white residents.

Finally, I show there is no effect of police shootings and police killings on pro-police voting. Given the vast majority of San Francisco voters are either white or Asian, the finding that police shootings do not impact voting at the aggregate level is consistent with work by Ang and Tebes (2020).

Broadly, my findings show that while pro-police policy positions enjoy widespread support among the electorate, community demand for policing increases in response to crime. This finding does not imply, however, that law enforcement can influence electoral results by altering policing behavior prior to an election as the effect is present only for homicide and other violent crimes over which police have little discretion. Policing is a resource-intensive public good and the level at which it is provisioned is ultimately adjudicated at the ballot box. As the discourse surrounding policing in the United States continues to rapidly evolve, my study provides the first causal evidence that crime is a factor in shifting individual demand for policing.

2.6 Figures and Tables

Figure 2.1: Voter Guide Example

Paid Argument IN FAVOR of Proposition A

Proposition A Honors Our Commitment To Our Retired First Responders

Proposition A protects the commitment our city made to fund the Retiree Health Care Trust Fund for our retired city firefighters, police officers and nurses.

As first responders we make a commitment to keep San Francisco's safe, often putting our lives on the line to protect people's health and welfare.

Now we are asking the City to keep its commitment to ensure health care for our retired firefighters, police officers and nurses by voting Yes on Proposition A.

Proposition A protects the Retiree Health Care Trust Fund by preventing the city from raiding it for other purposes.

Vote Yes on Proposition A to honor San Francisco's commitment to our retired first responders.

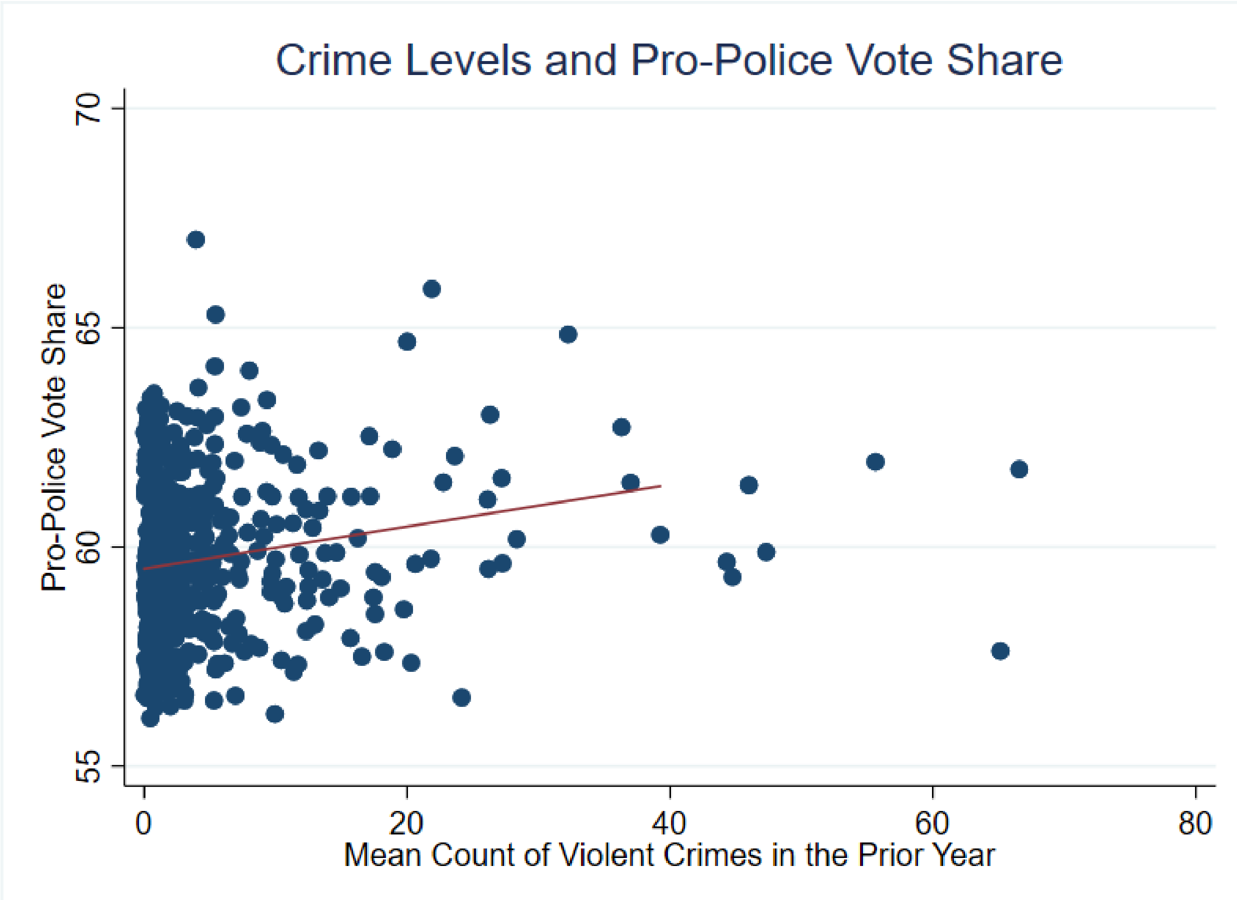
San Francisco Firefighters Local 798
San Francisco Police Officers Association

An example of a police union-endorsed ballot proposition and argument from the San Francisco Voter Guide. Proposition A 2016.

Figure 2.2: Pro-Police Vote Share and Neighborhood Demographics

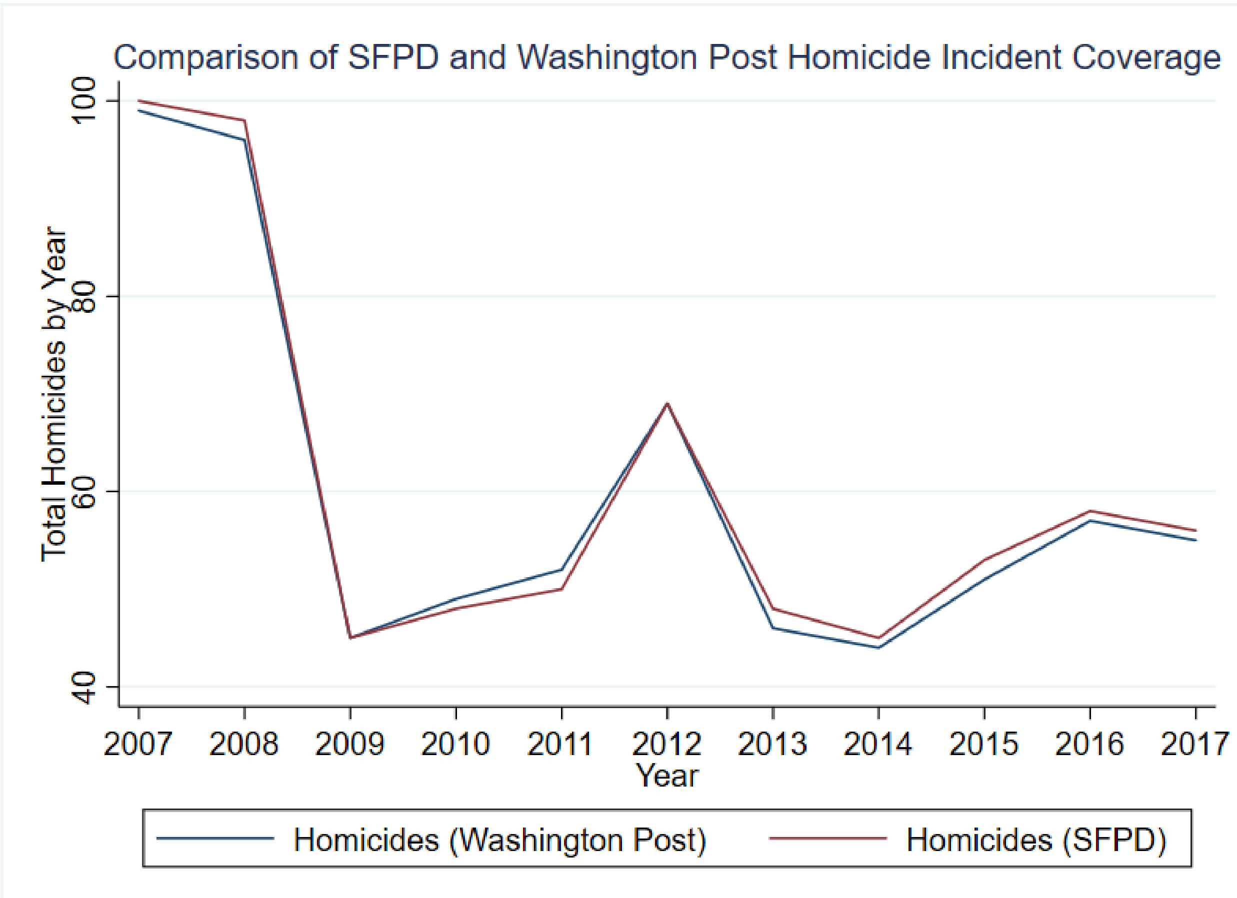
Relationship between the mean pro-police vote share in a block group and its share of residents by race. Pro-police vote share is high across all block groups at around 60%. Note pro-police support is increasing in the share of white residents and decreasing in the share of Asian and Hispanic residents. It is also slightly increasing in the share of black residents, but as San Francisco is less than 6% black, there is a lack of support across the distribution of share black.

Figure 2.3: Pro-Police Vote Share and Neighborhood Crime



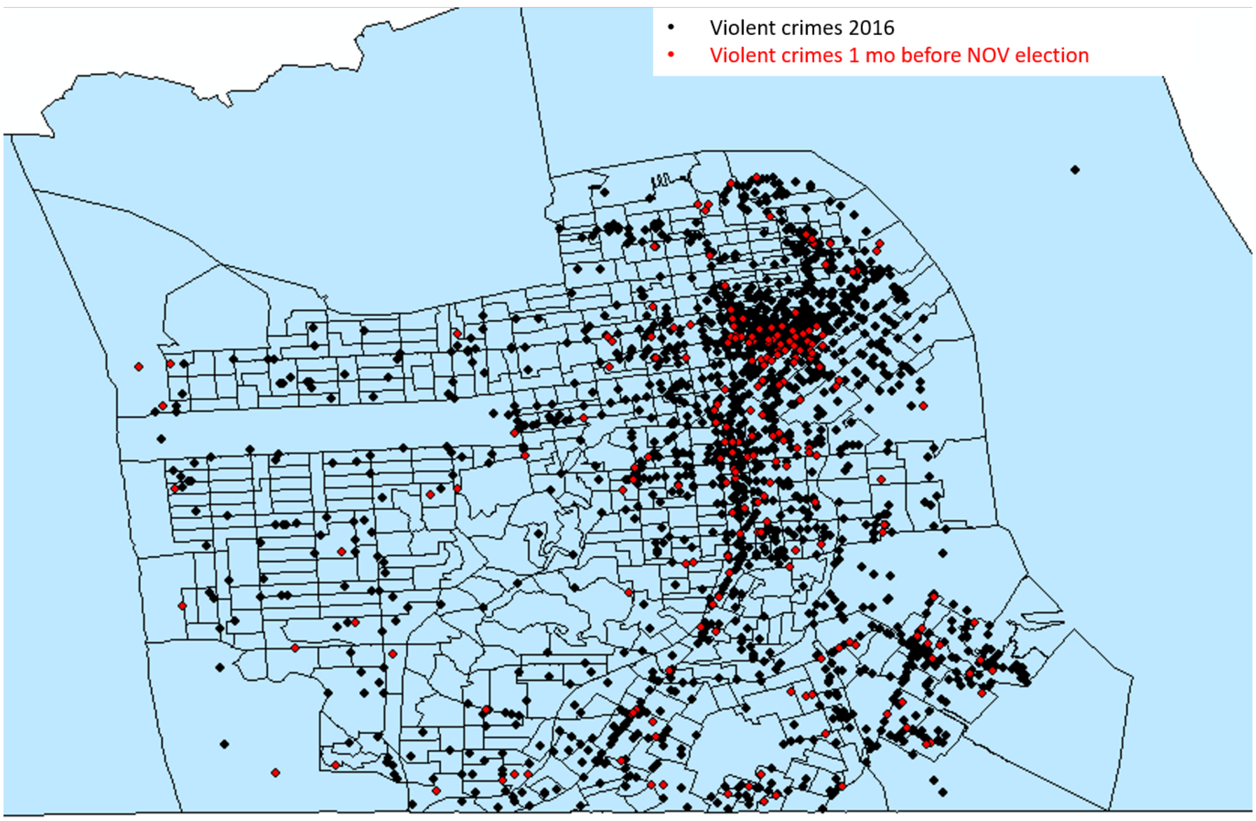
Relationship between the mean pro-police vote share in a block group and its experience with violent crime. Pro-police vote share is high across all block groups and increases in higher-crime neighborhoods.

Figure 2.4: Homicide Data Source Comparison



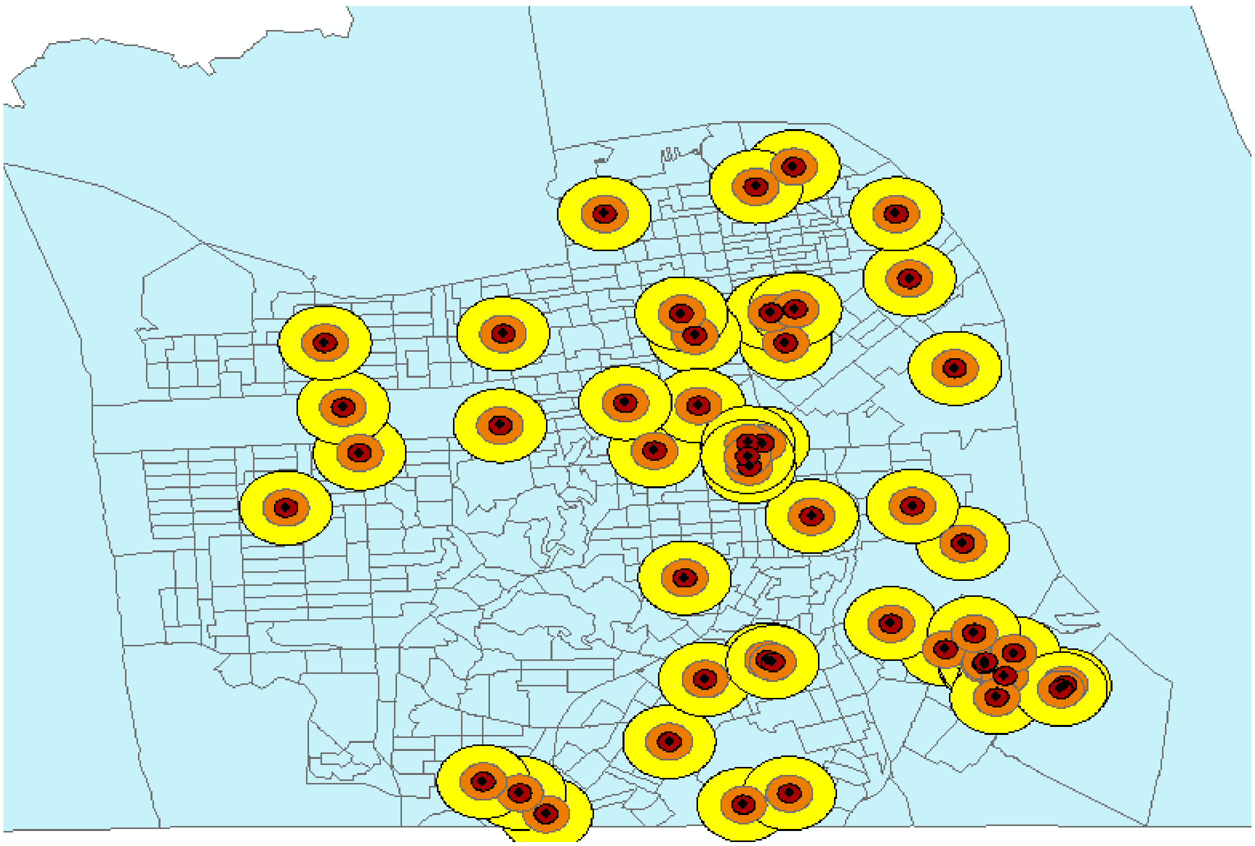
Comparison of aggregate homicide data between SFPD and *Washington Post* Datasets. The geocoded *Washington Post* data contains nearly every homicide in San Francisco from 2007 to 2017.

Figure 2.5: Distribution of Violent Crimes in San Francisco



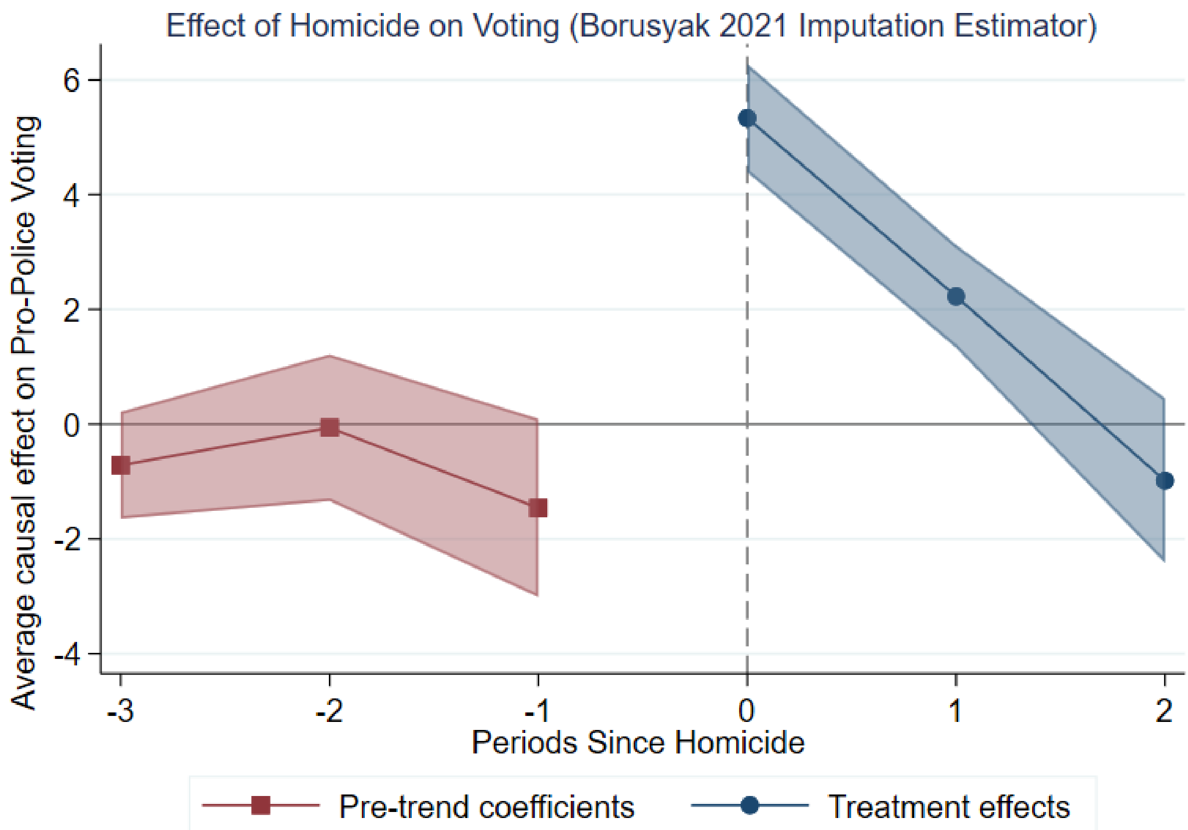
The distribution of violent crime (aggravated assaults and attempted homicide) occurring in San Francisco in 2016. Incidents occurring within a month of the election are shown in red. The treatment variable in Equation 1 is the count of violent crimes occurring within a block group within j days of the election

Figure 2.6: Buffer Approach



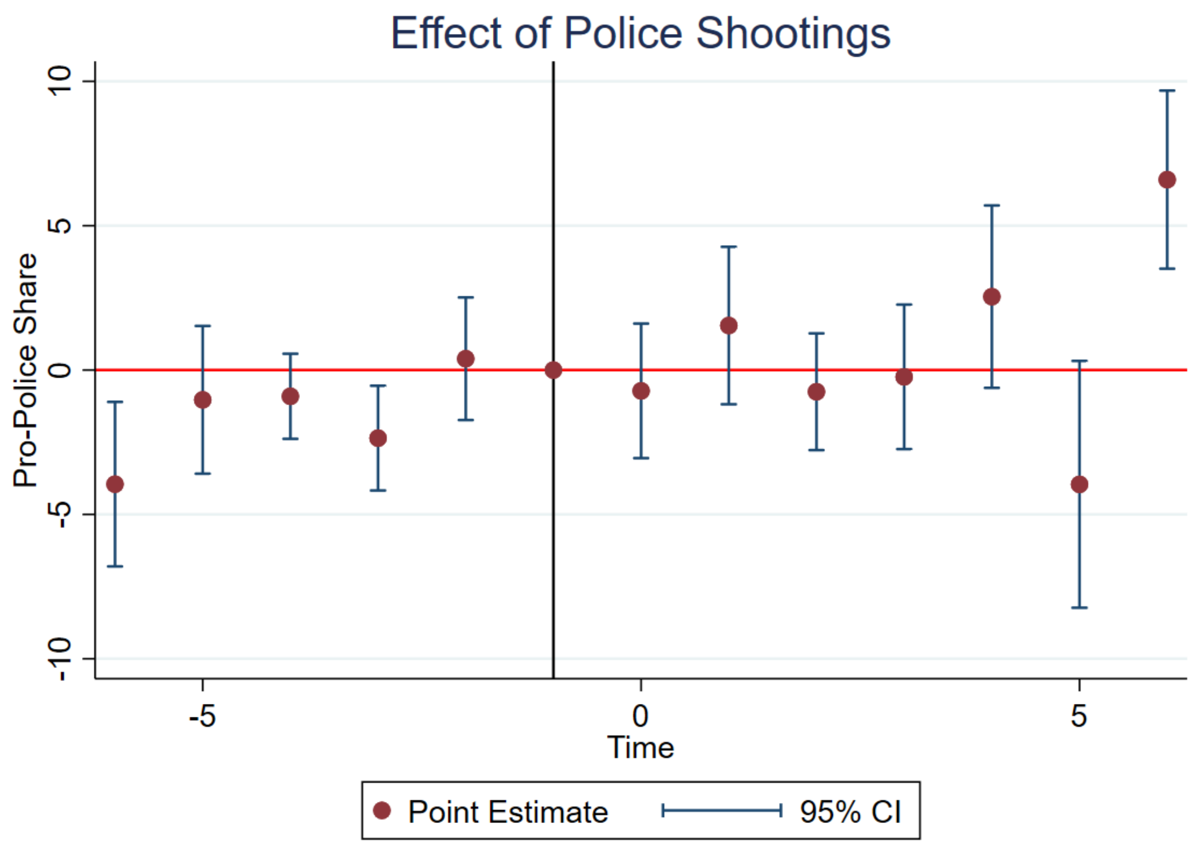
The buffer approach specified in Equation 2 for homicides occurring in 2016. The treatment variables are the counts of homicides that occur within 500 ft of a block group (red circle); greater than 500 ft but less than 1000 ft (orange annulus); and greater than 1000 but less than 2000 ft from the block group (yellow annulus).

Figure 2.7: DiD-Imputation Event Study Coefficients



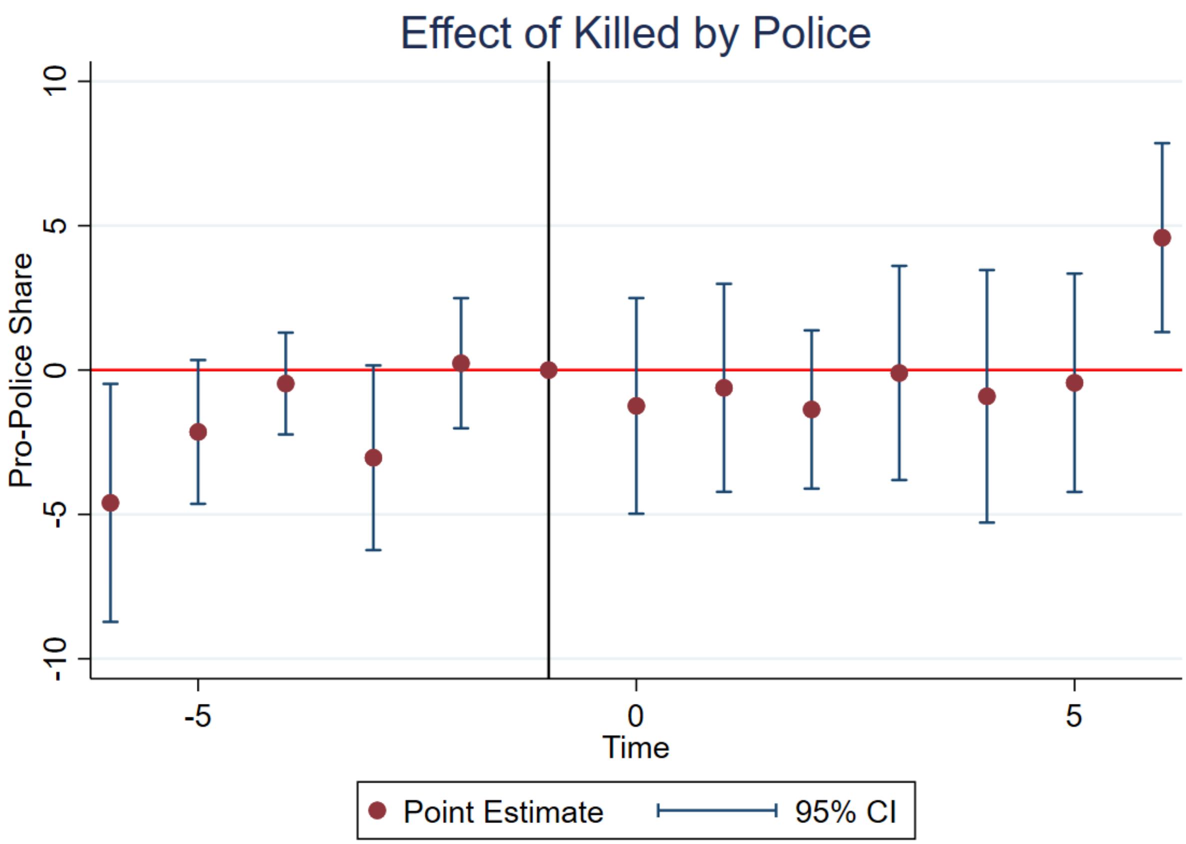
Event study coefficients using the heterogeneous treatment effect correction for two-way fixed effects models of Borusyak et. al (2021). Treatment is defined as having a homicide occur within 2000 ft of the block group. Three period leads and lags correspond to six years or three November general election cycles. The dependent variable is pro-police vote share in percentage points.

Figure 2.8: Event Study, Police Shooting Incidents



Event study coefficients of Equation 3 for incidents in which the police shot an individual. The outcome variable is the pro-police vote share in percentage points. There is no evidence of pre-trends or of an effect of these incidents on pro-police voting, though confidence intervals are large.

Figure 2.9: Event Study, Killed by Police Incidents



Event study coefficients of Equation 3 for incidents in which the police shot someone who died as a result of the interaction. The outcome variable is the pro-police vote share in percentage points. There is no evidence of pre-trends or of an effect of these incidents on pro-police voting, though confidence intervals are large.

Table 2.1: Summary Statistics

Variables	Block Groups
Pro-Police Vote Share	59.69
	(12.65)
Violent Crime Rate	408.96
	(2506)
Count of Violent Crimes within 1 month	0.37
	(0.94)
Share White Residents	44.43
	(22.61)
Share Asian Residents	31.93
	(20.16)
Share Hispanic Residents	14.10
	(12.21)
Share black Residents	5.37
	(8.83)

Summary statistics. The mean pro-police share across all 579 block groups in all elections is around 60%. Violent crimes include aggravated assault and attempted homicide. Note this is a different definition from FBI UCR, which also includes rape and robbery.

Table 2.2: Effect of Homicide on Pro-Police Voting, November General Elections

	Before Election			After Election
VARIABLES	pro_police	pro_police	pro_police	pro_police
	<30 days	<60 days	< 365 days	>30 days
Homicide	2.876***	1.548***	0.365***	-0.0395
	(0.451)	(0.291)	(0.104)	(0.465)
Observations	2,895	2,895	2,895	2,731
R-squared	0.743	0.742	0.741	0.742

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Effect of proximity of homicide on pro-police voting. Pro_police is the mean vote share on SFPOA-endorsed ballot initiatives in percentage points. $Homicide$ is the count of homicides occuring within 1000 feet of the block group within j days of the election, where j is equal to 30, 60, and 365 days in columns 1, 2, and 3 respectively. Column 4 presents the falsification test, where $Homicide$ is now the count of homicides occurring within 30 days after the election. Robust standard errors are clustered at the block group level.

Table 2.3: Effect of Homicide on Pro-Police Voting by Distance, November General Elections

	Before Election			After Election
VARIABLES	pro_police	pro_police	pro_police	pro_police
	<30 days	<60 days	< 365 days	>30 days
Homicide	3.351***	2.350***	0.824***	1.051
	(0.674)	(0.419)	(0.172)	(0.693)
1000ft buffer	2.990***	1.575***	0.301	-0.940
	(0.577)	(0.537)	(0.392)	(0.608)
2000ft buffer	2.352***	1.635***	0.721**	0.533
	(0.467)	(0.380)	(0.341)	(0.440)
Observations	2,895	2,895	2,895	2,731
R-squared	0.746	0.744	0.742	0.742

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Effect of homicide on pro-police voting changes with distance. Pro_police is the mean vote share on SFPOA-endorsed ballot initiatives in percentage points. $Homicide$ is the count of homicides occuring within 500 feet of the block group within j days of the election, where j is equal to 30, 60, and 365 days in columns 1, 2, and 3 respectively. $1000ftbuffer$ is the count of homicides occurring greater than 500 ft. but less than 1000 ft. from the block group. $2000ftbuffer$ is the count less than 2000 ft. but greater than 1000 ft from the block group. Column 4 presents the falsification test, where each treatment variable is now the count of homicides occurring in that area within 30 days after the election. Robust standard errors are clustered at the block group level.

Table 2.4: Effect of Homicide on Pro-Police Voting by Neighborhood Racial Composition

VARIABLES	(1) pro_police	(2) pro_police	(3) pro_police
	<30 days	<60 days	<365 days
All Block Groups			
Homicide	2.876***	1.548***	0.365***
	(0.451)	(0.291)	(0.104)
Highest Share White			
Homicide	3.743***	3.061***	0.338
	(0.697)	(1.010)	(0.380)
Highest Share Asian			
Homicide	2.054***	0.655	0.692***
	(0.687)	(0.562)	(0.167)
Highest Share Hispanic			
Homicide	1.626**	0.762*	0.243*
	(0.768)	(0.421)	(0.145)
Highest Share Black			
Homicide	1.398***	1.645***	0.147
	(0.389)	(0.592)	(0.142)
Observations	730	730	730

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Effect of homicide on pro-police voting by racial composition of the block group. Each regression for "Highest Share" is restricted to block groups in the highest quartile of share of residents reporting the respective race in the 2010 census. The thresholds are: Share White, >63%; Share Asian, >49%; Share Hispanic, >19%; Share black, >6%. Effects are strongest in block groups with the highest concentration of white residents. Conclusions with respect to black voters should be drawn with caution given the sparsity of black residents of the city.

Table 2.5: Effect of Violent Crimes on Pro-Police Voting, November General Elections

	Before Election			After Election
VARIABLES	pro_police	pro_police	pro_police	pro_police
	<30 days	<60 days	< 365 days	>30 days
Count_Violent	0.385***	0.213**	0.0314	0.0082
	(0.128)	(0.0883)	(0.0306)	(0.153)
Observations	3,474	3,474	3,474	3,310
R-squared	0.754	0.754	0.754	0.754

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Effect of proximity of violent crime on pro-police voting. Pro_police is the mean vote share on SFPOA-endorsed ballot initiatives in percentage points. $Count\_Violent$ is the count of violent crimes (attempted homicides and aggravated assaults) occuring in the block group within j days of the election, where j is equal to 30, 60, and 365 days in columns 1, 2, and 3 respectively. Column 4 presents the falsification test, where $Count\_Violent$ is now the count of violent crimes occurring within 30 days after the election. Robust standard errors are clustered at the block group level.

Table 2.6: Effect of Homicides and Violent Crimes on Turnout

	Before Election			Before Election		
VARIABLES	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
	<30 days	<60 days	<365 days	<30 days	<60 days	<365 days
Homicide	-0.733***	-0.0955	-0.0921			
	(0.243)	(0.188)	(0.0595)			
Count_Violent				0.0497	-0.0820	0.0284
				(0.0677)	(0.117)	(0.106)
Observations	2,895	2,895	2,895	3,474	3,474	3,474
R-squared	0.964	0.964	0.964	0.957	0.957	0.957

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Effect of proximity of homicide (columns 1 to 3) and violent crimes (columns 4 to 6; includes aggravated assault and attempted homicide, homicides dropped) on turnout. The effect on turnout is negative and significant only for the most recent homicides within 30 days of the election and zero otherwise. The decrease in turnout of 0.7 percentage points in column 1 is not enough to explain the increase in pro-police voting, so the results cannot be explained by compositional changes in the electorate alone. Robust standard errors are clustered at the block group level.

Table 2.7: Effect of Violent Crimes on Pro-Police Voting, Primary and Municipal Elections

	Before Election			After Election
VARIABLES	pro_police	pro_police	pro_police	pro_police
	<30 days	<60 days	< 365 days	>30 days
Count_Violent	-0.142	-0.122	-0.0887***	-0.012
	(0.125)	(0.0886)	(0.0331)	(0.135)
Observations	5,744	5,744	5,744	5,565
R-squared	0.866	0.866	0.867	0.867

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Effect of proximity of violent crime on pro-police voting in primary and municipal elections, as opposed to November biennial elections in Table 4. *Pro_police* is the mean vote share on SFPOA-endorsed ballot initiatives in percentage points. *Count_Violent* is the count of violent crimes (attempted homicides and aggravated assaults) occuring in the block group within j days of the election, where j is equal to 30, 60, and 365 days in columns 1, 2, and 3 respectively. Column 4 presents the falsification test, where *Count_Violent* is now the count of violent crimes occurring within 30 days after the election. Robust standard errors are clustered at the block group level.

Table 2.8: Effect of Warrant Incidents, November General Election

	Before Election			After Election		
VARIABLES	pro_police	pro_police	pro_police	pro_police	pro_police	pro_police
	<14days	<30days	<60 days	>14days	>30days	>60 days
Warrant_Incidents	0.0311	0.106	0.0319	0.0580	-0.0493	0.0113
	(0.135)	(0.0803)	(0.0397)	(0.108)	(0.0685)	(0.0454)
Observations	3,474	3,474	3,474	3,474	3,474	3,474
R-squared	0.754	0.754	0.754	0.754	0.754	0.754

Falsification test using the count of warrant-related incidents as the treatment variable instead of violent crime. Pro_police is the mean vote share on SFPOA-endorsed ballot initiatives in percentage points. $Warrant\_Incidents$ is the count of warrant incidents (a designated category in the SFPD data) occuring in the block group within j days of the election, where j is equal to 14, 30, and 60 days in columns 1, 2, and 3 respectively. In column 4 $Warrant\_Incidents$ is now the count of incidents occurring within 14 days after the election. Robust standard errors are clustered at the block group level.

Table 2.9: DiD-Imputation Estimator Event Study for Effect of Homicide

	(1)
VARIABLES	pro_police
Homicide Year	5.337***
	(0.482)
Post Period 2	2.228***
	(0.457)
Post Period 3	-0.985
	(0.733)
Pre-period 1	-1.461*
	(0.793)
Pre-period 2	-0.0612
	(0.652)
Pre-period 3	-0.715
	(0.475)
Observations	2,272

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Event study coefficients using the heterogeneous treatment effect correction for two-way fixed effects models of Borusyak et. al (2021). Treatment is defined as having a homicide occur within 2000 ft of the block group. Three period leads and lags correspond to six years or three November general election cycles.

3.0 The Value of Piped Water and Sewers: Evidence from 19th Century Chicago

with Toru Kitagawa, Allison Shertzer, and Matthew Turner

We estimate the impact of piped water and sewers on property values in mid-19th century Chicago. The cost of sewer construction depends sensitively on imperceptible variation in grade, and such variations in grade delay water and sewer service to part of the city. This delay provides quasi-random variation for causal estimates. We extrapolate ATE estimates from our natural experiment to the area treated with water and sewer service during 1874-1880 using a new estimator. Water and sewer access increases property values by more than a factor of two. This exceeds costs by about a factor of 60.

3.1 Introduction

We estimate the impact of piped water and sewers on land values in mid-19th century Chicago. To conduct this estimation, we rely on novel, purpose-collected data describing Chicago land transactions in the late 19th century and detailed annual maps of piped water and sewer networks. To identify causal effects, we exploit the fact that the construction cost for sewers varies sensitively with variations in grade that are otherwise imperceptible and, therefore, affect land values only through their effect on the timing of piped water and sewer access. We propose a new estimator to extrapolate treatment effects from the small region where we can defend our natural experiment to a region that is more relevant for cost-benefit analysis. In our most conservative estimate, we find that access to piped water and sewers more than doubles the value of residential land in Chicago. Aggregating this increase over affected parcels and comparing to construction costs, we find that the benefits of piped water and sewer infrastructure exceed costs by about a factor of 60.

These results are of interest for several reasons. First, according to the World Bank, about 15% of the world's urban population did not have access to safely managed drinking water in 2020. A larger share, about 40%, did not have access to safely managed sanitation facilities.[1] Given the likely impact of safely managed water and sanitation on health and mortality, the provision of such services would seem to be a priority. Yet, many cities also lack other basic services such as decent roads, sufficient public transit, adequate schooling and reliable electricity. Thus, trade-offs inevitably arise. By providing estimates of the benefits of piped water and sewer access, we hope to inform policy makers facing such trade-offs.

Second, our estimates inform us about an important aspect of the development of the American economy during the late 19th and early 20th centuries. Economic historians have long emphasized the importance of public health infrastructure for the development of American cities (Ferrie and Troesken, 2008). The existing literature on sanitation investments relies almost entirely on time series or panel data relating city-level changes in health and mortality to changes in the availability of particular public health interventions (e.g., Cutler and Miller (2005), Alsan and Goldin (2019)). However, this time period also saw changes in food purity laws, improvements in water and sewer access and quality, widespread acceptance of the germ theory of disease, and dramatic increases in income that could confound estimates based on time-series variation, and results in Anderson et al. (2018) suggest that this concern is not purely hypothetical. We contribute to this debate by estimating the value of piped water and sewer infrastructure using a novel cross-sectional identification strategy

[1]Source: World Bank. https://data.worldbank.org/indicator/SH.H2O.SMDW.UR.ZS. Accessed December 15, 2021.

to provide new evidence for the importance of capital-intensive public health interventions in the development of American cities.

Third, we pioneer a new identification strategy for estimating the causal effects of sewers. The effects of sewer access on the development of cities and the well-being of their inhabitants have been much less studied than have the effects of other types of infrastructure such as electrification or transportation. This partly reflects the intrinsic difficulty of observing underground pipes. But it also reflects the lack of a compelling identification strategy. We hope that our research design will prove portable, and will facilitate research on the effects of sewer and water infrastructure in cities of the modern world.

Finally, building on the marginal treatment effect model proposed by Carneiro et al. (2011), we develop a method for extrapolating treatment effects from a quasi-experimental region to a more economically relevant region. The reliance on small, carefully constructed samples to identify the effects of location specific policies is common, and our hope is that our technique will permit researchers using such designs to extrapolate their results to more relevant samples in a principled way.

3.2 Literature

The effect of late 19th and early 20th century municipal water treatments on mortality rates is well studied. Using a sample of 13 US cities between 1902 and 1936, Cutler and Miller (2005) (and subsequent reanalyses of an expanded sample of 25 cities Anderson et al. (2018), Anderson et al. (2019), and Cutler and Miller (2020)) estimate the relationship between water filtration, chlorination and various mortality rates. Ferrie and Troesken (2008) considers the effects of various public works projects to improve drinking water quality in Chicago from

1852 to 1925 on the crude death rate. Alsan and Goldin (2019) examine the effect of measures to improve water quality in the Boston Harbor watershed on infant mortality between 1880 and 1915. Beach et al. (2016), Ogasawara and Matsushita (2018), and Knutsson (2020) also study the effect of improvements in water quality on measures of mortality. Finally, Cain and Rotella (2001) consider the effect of expenditures on water works on mortality.

While the details of the particular studies differ, the results in Cutler and Miller (2020) are typical. During their 1900-1936 study period the crude death rate declined from 1935 to 287 per 100,000, the infant mortality rate declined from about 189 to about 71 per 1000, and about 13% of the decline in both mortality rates reflects the increased availability of filtered drinking water.

Expansions of sewer access during this period are less well studied. Kesztenbaum and Rosenthal (2017) examine the effect of the increasing availability of sewers in Paris between 1880 and 1915 and find that a 10% increase in neighborhood sewer connections increases neighborhood mean life expectancy, conditional on reaching age one, by 0.13 years. Beach (2021) argues that the various innovations in municipal sanitation and water supply were responsible for the elimination of typhoid in American cities between 1900 and 1930. Finally, Cain and Rotella (2001) finds that a 1% increase in sewer expenditure is associated with a 2% decrease in waterborne disease death rate.

The effects of municipal water quality improvement in the modern developing world have also been studied. Ashraf et al. (2017) find that interruptions to piped water supplies in urban Lusaka significantly increase the incidence of diarrhea and typhoid, and increase time at chores and decrease time at study for young women. Galiani et al. (2005) examine the effects of privatizing the provision of municipal water supplies in Argentina in the 1990s and conclude that the resulting improvements in service quality reduced child mortality by 8%.

Bhalotra et al. (2021) examine the effect a large expansion of water treatment in Mexico between 1991-5 and find that improved access to piped water led to a large reduction in childhood mortality from diarrheal illness. Devoto et al. (2012) find that randomly assigned help obtaining credit for piped water connections significantly increases time allocated to leisure activities in an RCT conducted in Tangiers in 2007.

Finally, Gamper-Rabindran et al. (2010) investigate the relationship between increased access to piped water and sewers in Brazil between 1970 and 2000. During this period, the share of households with piped water increased from 15% to 62% and the infant mortality rate fell from 125/1000 to 34/1000. On the basis of a panel data estimation, they conclude that each percentage point increase in piped water access decreases infant mortality by 0.48/1000. Thus, the realized expansion in piped water access decreased infant mortality by $(62-15)\times 0.48 \approx 22/1000$, about 25% of the total decrease of 91/1000. Gamper-Rabindran et al. (2010) also examine the effects of increased sewer access and find no effect.

Our analysis makes several contributions. First, while public health innovations in later 19th century US have been widely studied, the historical literature is predominantly about water treatment, not about expansions of the availability of piped water and sewers. Only Kesztenbaum and Rosenthal (2017), Cain and Rotella (2001) and and Anderson et al. (2018) explicitly analyze sewer provision, and the expansion of piped water access appears to be almost unstudied. Among papers studying the modern developing world, only Gamper-Rabindran et al. (2010) explicitly studies expansions in water and sewer availability.

Second, our analysis of the relationship between public health infrastructure and land rent appears to be unique. Public health infrastructure has complicated effects on the lives of those it touches. Not only does it affect current mortality and morbidity rates, it (at least sometimes) affects time allocated to leisure Devoto et al. (2012), and time spent at school

Ashraf et al. (2017). In light of the higher post-typhoid mortality rates (the Mills-Reinecke phenomena (Ferrie and Troesken, 2008)), public health infrastructure also affects future mortality rates. It follows that an evaluation of the benefits of public health infrastructure requires an effort to aggregate and monetize all of these different effects. This exercise is complicated by the difficulty of calculating the value of a statistical life from historical data Costa and Kahn (2004). In contrast, land rent is a revealed preference measure summarizing the value of all of the effects of piped water and sewer service to the people to whom the service is made available. As such, it provides simple basis for valuing all of the private benefits piped water and sewer service.

Third, without exceptions, the literature studying 19th century public health initiatives relies on comparisons of mortality rates before and after an innovation (as in Ferrie and Troesken (2008) or on a difference in differences design (as in Cutler and Miller (2005) or Alsan and Goldin (2019)). Given this, our cross-sectional research design is a novel basis for the estimating the effects of public heath infrastructure. Our new research design should be particularly welcome given the recent debate over the landmark estimates of Cutler and Miller (2005) of the effect of water filtration on US mortality at the end of the 19th century (see Anderson et al. (2018), Anderson et al. (2019), Cutler and Miller (2020)).

Fourth, while the disease environment in modern developing world cities is clearly different from late 19th century Chicago (see Henderson and Turner (2020) and Haines (2001)), the available evidence points to striking similarities between the 19th century US and modern developing countries. To see this, note that between 1853 and 1925, the crude death rate in Chicago declined from declined from 27/1000 to 11/1000 and between 32 and 52% of this improvement was due to improvements in water quality Ferrie and Troesken (2008). Estimates of the effect of water filtration in Cutler and Miller (2020) for a sample of US cities between 1900 and 1936 are of about the same magnitude. Gamper-Rabindran et al. (2010) investigate the relationship between increased access to piped water and sewers in Brazil between 1970 and 2000. During this period, infant mortality rate fell from 125/1000 to 34/1000 with about 25% of this decrease due to piped water. Bhalotra et al. (2021) consider the effect of expanded chlorination of Mexican drinking water from 1991-5. In their sample, the child mortality rate declines from about 28/1000 to 6/100, about half of which they attribute to chlorination. While the comparison is imprecise, both raw mortality rates and the effects of improved water quality are large in both turn of the century US and modern day Brazil and Mexico. This suggests that, in the absence of studies based on modern data, our estimates of the value of piped water and sewer in late 19th century Chicago can serve as a starting point for evaluating policies in modern day developing countries.

Summing up, the available literature on the effects of improvements to water supply or sewer access broadly supports the hypothesis that such innovations are important contributors to health, particularly of children, and to well-being more broadly defined. However, only Gamper-Rabindran et al. (2010) provides an analysis of policies to construct urban water and sewer networks, and ours is the only examination of the effect of piped water and sewer infrastructure on land prices. Given the ability of land prices to capitalize place specific benefits, this means that our estimates provide a unique foundation for the evaluation of the benefits of piped water and sewer construction projects.

In addition to our primary object of estimating the effects of piped water and sewer infrastructure on land prices, we develop a new method for extrapolating estimates based on a quasi-experiment to a more economically relevant sample for which quasi-random assignment of the treatment is not available. Our approach to this problem builds on the marginal treatment effects estimator developed by Heckman and Vytlacil (2005) and Carneiro et al.

(2010) but extrapolates to units not in the original estimation sample. Other methods for extrapolating causal effects to populations other than the sampled population include Hotz et al. (2005), Angrist and Fernández-Val (2013), Andrews and Oster (2019), and Dehejia et al. (2021). There is also a small literature (Angrist and Rokkanen (2015), Rokkanen (2015), and Cattaneo et al. (2020)) considering the related question of extrapolating treatment effects estimated using an RDD design to points away from the discontinuity. The possibility of extrapolation from quasi-experimental samples to more economically relevant samples based on marginal treatment effect estimates has not been previously considered.[2]

3.3 Data

Our main empirical exercise requires two main types of data, a measure of land values and a measure of piped water and sewer access. For econometric purposes, we also require a description of the attributes of transacted parcels. To complete our cost benefit analysis, we must also measure construction costs. We here describe the data we use for each purpose.

Between 1873 and 1889, the Chicago Tribune reports every land parcel transaction filed with the municipal title office on the previous day. We collect all transactions listed in the Sunday edition, which is usually the day of the week with the largest number of listings. This results in about 700 observations per year in the 1870s and 1000 per year in the 1880s.[3]

[2]We also note a series papers, Mogstad and Torgovitsky (2018), Mogstad et al. (2018), and Brinch et al. (2017) are also related. However, these papers consider the extrapolation of marginal treatment effect to units for which all similar observed units have the same treatment status. In contrast, we consider the problem of extrapolating marginal treatment effects to unobserved units.

[3]The Tribune still published parcel transactions after 1889, but the coverage is limited to

The Tribune consistently reports; price, parcel dimensions, either a street address or the nearest intersection, and whether the parcel is "improved." Figure 3.1 illustrates a sample of transaction listings. Because the Tribune separately indicates transactions with a "premises", that is, parcels with a structure, we are confident that our data describe land transactions only. The newspaper does not define "improved" and it is clear from the data it does not refer to water and sewer access or to the presence of a structure. Because the Annual Reports of the Chicago Department of Public works routinely refer to paved streets as "improved", we believe that improved indicates that the parcel fronts a paved road.

We geocode our sample parcels in two steps. First, we attempt to match the "nearest intersection" reported by the Tribune to an intersection in the contemporary street grid described by the Google Maps API. When we cannot match a reported intersection to the contemporary street grid, we attempt to match it to an intersection in the circa 1880 street map created by Logan et al. (2011). This process allows us to geocode about 77% of transactions by assigning them the coordinate of their nearest intersection.[4]

We rely on historical GIS maps describing the block-by-block expansion of the sewer network from 1830-1930 Fogel et al. (2014). These maps derive from the annual reports of the Chicago Department of Public Works and report both the location and opening date for each segment of the sewer network. Water and sewer service were almost always installed simultaneously, and so we rely exclusively on sewer maps.

We say a transaction "has water and sewer access" if the nearest intersection to the transaction is within 75 feet of an operating sewer line in the transaction year. Visual inspection of the matching process indicated that this rule resulted in an accurate matching

parcels with a value of at least $1000 (nominal value).

[4]Addresses are not universally reported for our transactions and Chicago undertook a complete renumbering of addresses in 1909. This rules out the geocoding of addresses.

of intersections to sewers. One can imagine situations in which a parcel without access to sewer and water matches to an intersection where access is available, though such situations should be rare.[5] False negatives are harder to imagine.

Figure 3.2a illustrates the expansion of piped water and sewer access during the post-Civil War period. In this figure, the heavy, light gray lines indicate water and sewer lines predating our 1874-1880 study period. Unsurprisingly, these lines tend to be close to the center of the city. Heavy black lines indicate water and sewer lines constructed during our 1874-1880 study period. Also unsurprisingly, these lines are mostly located on the periphery of the previous network. Finally, the fine gray lines indicate sewer and water lines built after the end of our study period; these lines are also peripheral to the 1880 network and often extend beyond the boundary of the figure.

We calculate a number of control variables from GIS data layers. For each parcel, we calculate distance to the CBD as the distance to City Hall in 1873 (now known as the Rookery Building). We calculate distance to the lake as distance to the modern lakeshore,[6] and calculate distance to the Chicago River similarly. Finally, we calculate distance to a horse car line and a major street using contemporaneous maps of the two networks.[7]

To estimate the cost of piped water and sewer expansion, we rely on reports of annual expenditures on water and sewer construction in the Annual Reports of the Chicago Depart-

[5]A parcel on a street without water and sewer service could match to an intersection where the cross-street has water and sewer access.

[6]The hydro file was obtained from Cook County Government Open Data, see https://datacatalog.cookcountyil.gov/GIS-Maps/Historical-ccgisdata-Lakes-and-Rivers-2015/kpef-5dtn.

[7]The 1880 horse-drawn streetcar routes were digitized using a map from the Illinois State Grain Inspection Department. The street network in 1880 was digitized by John Logan, see https://s4.ad.brown.edu/Projects/UTP2/39cities.htm

ment of Public Works (accessed through Hathi Trust). Expenditures vary year to year but are increasing in the early 1870s and decline during the recession of the late 1870s. Waterworks, including pumping stations, were typically the largest category of expenditure, with sewer construction second. Sewer maintenance costs, including manual flushing (discussed below), were stable and relatively small throughout the period. Expansions to the sewer and water system were primarily financed by bonds, and nineteenth-century Chicago had a large tax base of valuable land on which to levy the property taxes that were the primary source of revenue to service these bonds.[8]

3.4 Background

The Census reports Chicago's population as 300,000 in 1870 and above one million in 1890. The Great Fire of 1871 destroyed the central business district and much of the city, but barely checked this growth. The city continued to expand throughout the 1870s and 1880s, particularly in the band of mostly unsettled land a few miles from the downtown where our study area lies. This rapid growth was driven by immigrants from Europe and by internal migration. Chicago provided relatively high-wage employment opportunities for unskilled workers. The average income per laborer in the city of Chicago was as high as $650 in 1880 dollars or $17,000 in 2021 dollars.[9]

[8]Special assessments and connection fees also helped to finance sewer and piped water infrastructure. However, the Sewerage Board was reluctant to rely too heavily on fees and user charges because the resulting negotiations with building owners slowed down the expansion process (Melosi, 2000, p. 98).

[9]From estimates of wages per non-agricultural worker for the state of Illinois taken from (Easterlin, 1960, 73-140) ($627 per year) and Hoyt's (2000, pp.118-119) estimates of wages

Hoyt (2000) describes Chicago's land market between 1830 and 1930. He reports rapid growth in the value of land in the early 1870s. Farms that sold for $25 to $100 an acre were platted into town lots that sold for $400 to $1000 immediately thereafter (Hoyt, 2000, p. 108). Prices declined from their peak after the panic in 1873 and the value of the land within city limits declined 50 percent by 1877. Speculative landlords had "their cup of misery filled to the brim" in 1877 when the largest savings banks in the city of Chicago also failed (Hoyt, 2000, p. 123).[10] Economic conditions improved in the early 1880s and, by 1882, Chicago's land values had recovered to their 1873 peak (Hoyt, 2000, p. 140). Population growth and land prices were both relatively stable during the following decade. In short, our 1874-1880 study period spans a major recession (1873-1877) and recovery (1878-1882). Several years of moderate growth followed. Population growth was robust throughout the whole period from 1870-1890.

Chicago's infant mortality rate in the 1870s was 74 per 1000. This is similar to contemporaneous rates reported in other US cities, e.g., Alsan and Goldin (2019) or Haines (2001), and also current rates in poor developing countries like Sierra Leone or Somalia.[11] Most deaths were caused by infectious disease and occurred predominantly among the young (Ferrie and Troesken, 2008).

In the 1850s, the quality of Chicago's drinking water was notably poor. Most residents

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for workers in the city of Chicago during the 1870s ($3 a day for unskilled laborers). These values were inflated to 2021 price levels using CPI estimates from Sahr (2009) for 1880-1912 and the BLS CPI series for 1913-.

[10]Hoyt used 1879 prices to proxy for the bottom of the market in 1877 because it was difficult for him obtain data for this year. Our data reports transactions in 1877 and 1878.

[11]Estimate for Chicago taken from Ferrie and Troesken (2008) and for Africa from the UN Inter-agency Group for Child Mortality Estimation (UNICEF, WHO, World Bank, UN DESA Population Division) at childmortality.org.

drank from backyard wells. These wells were often near privy vaults and these vaults were seldom tight. Households with access to the city water system found it contaminated by industrial pollutants and minnows from Lake Michigan. Water quality improved as the city moved the water intakes further out into Lake Michigan and reduced the volume of waste dumped in the lake. Specifically, water quality improved with the completion of the Two Mile crib (1867), the Four Mile crib (1892), and the complete reversal of the Chicago River in 1900 (Ferrie and Troesken, 2008). Importantly, our study period (1874-1880) is located entirely within the Two Mile crib period.

The condition of the City's poorly drained streets was grim. The well-known Chicago history, (Asbury, 1940, p.23) reports that the "gutters [run] with filth at which the very swine turn up their noses in supreme disgust…". When storms washed these wastes into Lake Michigan or private wells, cholera and dysentery epidemics followed. Such events killed hundreds of people in both 1852 and 1854, prompting the city to begin planning the improvements to its water and sewer infrastructure that we discuss below.

Typical gravity fed sanitary sewers require a grade of about 1:200 to prevent suspended solids from settling and blocking the pipe. The precise required grade is sensitive to the details of the system; the rate of flow, pipe size and cross-sectional shape, and the smoothness of interior walls. For details see, e.g., Mara (1996). Importantly, variation in grade that is critical for sewer construction is practically beyond human perception. Aldous (1999) reports that people begin to perceive a playing field as sloped at a grade of about 1:70. Variation in grade is less relevant to piped water networks.

Our research design will be organized around transactions that occurred in the area around Tyler Street, currently the Eisenhower Expressway, and extending West about three miles from Halsted Street. The present day corner of Halsted and Tyler streets is about two miles from and twelve feet above the level of Lake Michigan, a grade of about 1:880. This is much too flat for conventional gravity-fed sanitary sewers. Indeed, such grades are so flat that water generally does not drain away. Rainfall either evaporates or is absorbed into the ground. Chicago's unusually flat terrain contributes to the benefits of sewers as well as to the difficulty of constructing them.

Chicago hired noted engineer Ellis Chesbrough to design a sewer system capable of operating in Chicago's flat topography, and substantially followed the proposal he submitted in 1855. Chesbrough proposed what is now known as a "combined" sewer system to manage household sewerage and street runoff. Chesbrough's plan called for continuous mechanical flushing, although the city ultimately adopted a system under which sewer mains were manually flushed using water delivered by horse-drawn carts.[12] This systematic manual flushing allowed sewer mains to operate at a grade of 1:2500, far shallower than conventional sewers.

To function, even Chesbrough's sewers require large enough flows of water that they are only practical if piped water is available. For this reason, sewers could not be installed before piped water. In fact, drainage in Chicago was so poor, that the increased volume of wastewater that accompanied piped water caused cesspools to overflow (Melosi, 2000, p. 91), so that installing piped water without sewer access was also impractical. For these reasons, the provision of piped water and sewer access almost always coincided.

Because water and sewer service are almost always provided together, we estimate their joint value. With this said, the discussion above points out that water and sewer service were highly complementary, so that providing one without the other would probably have had much less value.

[12] As late as 1940, horse-drawn tanks were still used to manually flush certain sewer lines in Chicago (Cain, 1978, p. 32).

Construction of Chesbrough's sewers required a massive program of regrading in order to raise streets to the required grades. The process for constructing sewers involved first laying sewer and water pipes at the required grade, whether above or below ground, and then filling in the space around them with earth as required. The newly raised streets were then sometimes paved over to conclude the process. Because street paving could independently contribute to property values, this raises the possibility that our estimates reflect the joint value of water, sewer and street paving. We address this possibility by controlling for improved status in our estimations.

Buildings, particularly those built out of stone and brick, were raised in the downtown to match the new street level as the sewer system expanded. These well-known feats of engineering predate our 1874-1880 study period. Our analysis focuses on vacant lots in outlying areas.

Chicago issued its original plan for sewerage in 1855. This document describes the street grades in each region of the city needed to accommodate the proposed sewer system (Plan of Sewerage, Chicago Board of Sewerage Commissioners, 1855). Subsequent ordinances were issued at regular intervals as the sewer system expanded beyond the streets covered in this initial report. The sewer ordinances describe the details of the regrading operation and list, block by block, the planned elevation of each street intersection relative to the level of the lake. The 1855 plan states, "It will be necessary to raise the grades of streets an average of eighteen inches per 2500 feet going West." To get a sense for the scale of this undertaking, it requires about 8300 cubic yards of fill to raise a 2,500 foot segment of a 20 foot wide street by 18 inches. At about 1.5 tons per cubic yard, this is almost 12,500 tons of fill per 2500 foot segment of road.

The historical record suggests that municipal authorities knew which streets had the worst drainage and were anxious to sewer them as soon as the network reached them. From the Chicago Tribune (June 25th, 1873, page 4):

> "The Mayor points out the various localities where this sewerage is the most needed. It so happens that the unsewered portion of the city is that which, of all others, most needs it. ... These neighborhoods are densely populated by people who have not the means to adopt any sanitary measures."

Thus, there is no reason to believe that the assignment of sewers to neighborhoods and streets was independent of land value.

The 1855 ordinance describes a "triangle" southwest of the downtown that was at a slightly lower elevation than the rest of the city. Chesbrough wrote of this region, South of Tyler Street (now the Eisenhower Expressway) and West of Halsted Street: "The extreme south-west part of the city [is] too low [to sewer], "as the depth of filling required to raise streets over it would average two feet" (p. 16). Recalling that the plan calls for streets to be raised "an average of eighteen inches per 2500 feet going West", this means that the marginal 6 inches of fill required in this region was decisive. Chesbrough concludes by writing, "[a]s this part of the city may not be improved for several years, it is deemed sufficient for present purposes to state the general depth of filling that would be required" (p. 15).

Figure 3.2 illustrates the expansion of the Chicago sewer system that occurred between 1870 and 1890. In both panels, thick light grey lines indicate the extent of the sewer network prior to 1874, thick black lines indicate the expansion that occurred between 1874 and 1880, and, thin light gray lines indicate post-1880 expansion. Red lines indicate the northern and eastern border of the Southwest Triangle, Tyler and Halsted streets.

While the 1855 plan refers to "a triangle", it specifies only northern and eastern borders. We draw a western boundary near the limit of the 1880 sewer network, 14,000 feet west of Halsted street, and a southern boundary at the Chicago River. We exclude parcels exactly on

Tyler street, i.e., those matching to intersections within 75' of Tyler Street, for two reasons. First, the 1855 plan is ambiguous about whether or not Tyler street lies inside or outside the Southwest Triangle. Second, our data does not allow us to determine whether parcels matching to Tyler Street lie north or south of the street. Thus, we cannot determine whether parcels matching to Tyler street are inside or outside the Southwest Triangle.

The black region in Figure 3.2b illustrates the entire region that received sewer and water access between 1874 and 1880. This is the region for which we observe construction costs and it is the economically relevant area for the purpose of policy evaluation. We often refer to a sample drawn from this area as a "Relevant sample." Our estimation of causal effects is primarily based on the region within 2000 feet of the northern boundary of the Southwest Triangle, Tyler Street. We often refer to a sample drawn from this area as a "Quasi-experimental sample". We sometimes consider the effect of sewers in the area within 2000' of the northern <u>or</u> eastern boundary of the Southwest Triangle, Tyler and Halsted streets. We often refer to a sample drawn from this area as an "Extended-quasi-experimental sample." Figure 3.2b illustrates all three regions.

Figure 3.3 highlights the evolution of the sewer network in the Quasi-experimental sample. This figure makes it clear that, even 20 years after the adoption of the 1855 sewer ordinance, the construction of sewers south of Tyler street lags the northern side of the street by several years. It is this north-south difference in sewer assignment on which we base our estimates of the causal effects of piped water and sewer access.

3.5 Description

Our Quasi-experimental sample is a set of 351 transactions occurring between 1874-1880 within 2000' of Tyler Street, west of Halsted. This is the sample where the case for quasi-random assignment of sewer and water access as a function of membership or exclusion from the Southwest Triangle is strongest.

Gray squares in figure 3.4 report mean log transaction price by year (after controlling for improved and corner status, log of parcel area, and log miles to the CBD), for all transactions falling in the Quasi-experimental region at any time between 1873 and 1880. Black points show the corresponding prices calculated for the entire city of Chicago. Whiskers indicate 95% confidence intervals. Unsurprisingly, annual means are more precise for the whole city than for the smaller sample drawn from the Quasi-experimental region.

This figure shows the same basic patterns described in Hoyt (2000). Prices fall between 1873 and 1880, before beginning a slow recovery. Figure 3.4 also shows that prices in the Quasi-experimental region follow those in the city as a whole. That is, the Quasi-experimental region is a small part of a large, liquid land market. This suggests that the assignment of sewers and piped water (or not) to parcels in the Southwest Triangle should not affect prices outside of the Southwest Triangle. On the basis of this observation, we ignore the general equilibrium price effects in our analysis of the Quasi-experimental sample.

Table 3.1 presents sample means for the Quasi-experimental sample. The first column describes transactions inside the Southwest Triangle, i.e., south of Tyler Street, the second, transactions outside the Triangle, i.e. north of Tyler Street. As the 1855 Ordinance prescribes, and as figure 3.3 shows, piped water and sewer incidence is lower inside the Southwest Triangle than outside. About half of transactions in the Southwest Triangle have water and sewer access during 1874-80 and access is almost universal outside. Consistent with a large effect of water and sewer access on value, unconditional prices are 0.72 log points or 105% higher outside of the Southwest Triangle than inside. The frequency of corner parcels is the same on both sides of the boundary. Improved parcels are more frequent outside the Southwest Triangle indicating the importance of this control. Parcels outside the Southwest Triangle are at most slightly larger than those inside. Parcels outside the Southwest triangle are on average one city block closer to the nearest horse car line, though both sides of Tyler street are well integrated with the horse car network. Major streets in Chicago occur at one mile intervals, or every eight blocks. Parcels on either side of Tyler street are on average one to two blocks from the nearest major street. The region inside the Southwest Triangle is marginally further from the CBD than the region outside, and so transactions outside are nearer the CBD than those inside by construction.

The fourth column of table 3.1 highlights one of our main econometric challenges. It reports sample means from the Relevant sample. On average, these parcels are less expensive and further from the CBD than parcels in the Quasi-experimental sample. If we are to apply estimates of the effects of water and sewer access based on the Quasi-experimental study region to this larger policy relevant area, we should consider the possibility that treatment effects may vary systematically between the two samples.

Ideally, to check that unobservable determinants of value are the same on both sides of Tyler Street, we would check land prices before piped water and sewer service was available on either side of the border. However, such data are not available.[13] Instead, we compare land prices on either side of Tyler street a short time after our study period when piped

[13]The Tribune began reporting transactions only in 1873, and 1860 census did not ask about home values or about the value of vacant land.

water and sewer access was universal.

Table C.1.1 describes transactions occurring in the Quasi-experimental region during 1886-9, six to nine years after the end of main study window. This table replicates the first three columns of table 3.1 for the later time period. This table indicates that the same basic patterns present in the data during 1874-80 largely persist into 1886-9, with two notable exceptions. Piped water and sewer access is universal during the later period, and the difference between prices inside and outside the Southwest Triangle that shows so clearly in Table 3.1 is no longer present in the later period.

Figure 3.5a illustrates piped water and sewer access in our experimental study area during 1886-9 as a function distance to Tyler Street. The x-axis of this figure is distance from Tyler Street. Negative distances indicate displacement into the Southwest Triangle, and conversely for positive values. The y-axis indicates piped water and sewer share relative to the share in the bin just inside the Southwest Triangle. Sewerage is universal across the boundary by 1886.

Figure 3.5b is similar, but reports on transaction prices. The y-axis indicates log price relative to the bin just inside the Southwest Triangle. Mean log price in each bin is calculated controlling for year indicators, ln(area), and ln(mi. to CBD). Whiskers indicate 95% confidence intervals. Table 3.1 indicates a 105% difference in prices across this boundary during 1874-80. Figure 3.5 indicates that this difference is completely erased in less than 9 years, once sewer incidence across the border equalizes. This confirms what we see in the unconditional means presented in table C.1.1.

Table 3.1 shows that parcels in the Southwest Triangle were less valuable during our study period. There is evidence that such initial disadvantages often "lock-in" and lead to long run differences between places (e.g., Bleakley and Lin (2012) or Ambrus et al. (2020)).

Poor places stay poor and rich places stay rich. Given this, our finding that price differences largely disappear with the elimination of the difference in sewer access is surprising. The available evidence suggests that path dependence works against the price equalization that we see in figure 3.5. We suspect this reflects the dynamic nature of the Chicago real estate market, the pervasiveness of cheap, short-lived structures, and our focus on vacant lots.

The descriptive evidence provided so far is consistent with the following narrative. Parcels in the Southwest Triangle are less likely to have access to piped water and sewers because of a nearly imperceptible change in elevation that affected costs of constructing gravity fed sewers. There is no a priori reason to suspect that parcels on opposite sides of Tyler street are systematically different, except that parcels inside the Southwest Triangle are slightly more remote from the CBD. This suggests that conditional on controls, a comparison of changes in prices and sewer access across Tyler street should yield an unconfounded estimate of the effect of water and sewer access on prices.

Figure 3.6 performs this comparison. Panel (a) shows changes in sewer incidence across the Tyler street border of the Southwest Triangle and panel (b) shows the corresponding changes in log price. The construction of this figure is the same as figure 3.5, except that it is based on data from our main study period, 1874-1880. Consistent with the unconditional means presented in table 3.1, we see that piped water and sewer incidence and land prices are lower in the Southwest Triangle. These figures illustrate the variation on which our estimates are based. The left panel is a first-stage regression, the right panel is a reduced form. The ratio of the two cross-boundary gaps, averaged over the four interior and exterior bins, yields (approximately) a local average treatment effect for the whole Quasi-experimental sample.

We note that Figure 3.6 suggests the possibility of implementing a fuzzy-RD design. Given our already small sample, this research design would rely heavily on a tiny set of observations. To avoid this, we abstract from the spatial structure of the data and base our estimates on an instrumental variable design using the whole Quasi-experimental sample. Note that our Quasi-experimental study region is narrow enough to walk across in less than 20 minutes and lies in an a priori homogeneous landscape. We can reasonably hope to have restricted attention to parcels with on average identical unobserved determinants of land price. Nevertheless, to the extent our sample allows, we investigate the possibility of confounding spatial trends in unobservables in our regression analysis.

3.6 Estimation

Let Y_i be the log of parcel i's transaction price observed in the data. Let X_i denote a vector of observable parcel attributes drawn from, transaction year indicators, ln(miles to CBD), ln(Parcel Area), Corner and Improved indicators, distance to horsecar line and distance to major street. Let D_i be a treatment indicator, with $D_i = 1$ if and only if parcel i has piped water and sewer access. Let Z_i be a binary variable indicating $Z_i = 1$ if and only if the parcel is not in the Southwest Triangle. We view Z_i as an instrumental variable and assume that it shifts the cost of access to piped water and sewage without directly affecting the land price, fixing the controlling covariates. By defining Z so that $Z_i = 1$ outside of the Southwest Triangle, we assure a conventional positive relationship between instrument and treatment.

We adopt the convention of indicating potential outcomes with a subscript, so that Y_{1i} is the price of parcel i in a state of the world where it is treated, and Y_{0i} is the untreated price. Let U_1, U_0, U_D denote three error terms to be defined later. Finally let P denote our Quasi-experimental sample and, abusing notation slightly, the joint distribution of (Y_1, Y_0, X, Z, D, U_1, U_0, U_D) drawn from this sample.

We are also interested in the corresponding quantities drawn from the Relevant sample, all transactions in the area receiving water and sewer access during 1874-80. We indicate these quantities with an asterisk. For example, Y_i^* is a transaction price drawn from this sample, and P^* denotes the distribution of (Y_1^*, Y_0^*, X^*, Z^*, D^*, U_1^*, U_0^*, U_D^*).

We would like to estimate the average treatment effect on the economically relevant sample, that is, $ATE^* \equiv E(Y_1^* - Y_0^*)$. This treatment effect permits an immediate evaluation of a realized policy and matches neatly to available data on costs. Estimating ATE^* requires that we address the conventional problem of estimating ATEs rather than LATEs. In addition, we must find a way to extrapolate our estimated treatment effect from the Quasi-experimental to the Relevant sample.

We first estimate local average treatment effects of piped water and sewer access with TSLS.[14] We next implement the local IV framework proposed by Carneiro et al. (2010). This framework allows the explicit calculation of an average treatment effect and tests for heterogeneity of treatment effects with respect to observable and unobservable characteristics. The LIV/MTE framework also provides a foundation for a novel, principled approach to the extrapolation of treatment effects. We develop and implement this method in the final stage of our analysis.

Table 3.2 presents four sets of estimates. For reference, Panel A presents OLS regressions

[14]In addition to instrument exclusion, exogeneity, and monotonicity (no-defier condition) conditional on X, if the conditional expectation of D given X is linear, we can interpret the estimand of TSLS as a weighted average of the local average treatment effects aggregating compliers' conditional average causal effects given X. See Abadie (2003), Kolesár (2013), and Słoczyński (2021) for further detail.

of the form,

$$Y_i = A_0 + A_1 D_i + A_2 X_i + \varepsilon_i.$$

These regressions show a significant positive association between piped water and sewer access, and transaction prices. In the first column, we control for year indicators and log miles to the CBD. In the second column, we add indicators for corner lot and improved status. In the third column, we add controls for distance to horsecar and distance to a major street. In each case, transaction prices are about 0.4 log points higher for parcels with water and sewer access. We postpone a discussion of the remaining columns.

Panel B presents the corresponding reduced form regressions of transaction price on the instrument,

$$Y_i = A_0 + A_1 Z_i + A_2 X_i + \varepsilon_i.$$

We see in column 1 that being in the Southwest triangle decreases transaction prices by about 0.6 log points. This effect is estimated precisely and varies only slightly as we add control variables in columns 2 and 3. Column 3 uses the same controls as we used in figure 3.6b, and so the estimated effect approximately corresponds to the average price difference between inside and outside parcels that we see in this figure.

Panel C presents first stage regressions,

$$D_i = B_0 + B_1 Z_i + B_2 X_i + \mu_i.$$

Conditional on control variables, being in the Southwest triangle reduces the probability of piped water and sewer access by about 40%. Again, this effect corresponds approximately to the mean difference in sewer access between inside and outside parcels in figure 3.6a. First stage F statistics are above critical values for conventional weak instrument tests (e.g., Stock and Yogo (2002)).

Panel D presents TSLS estimates of the effect of piped water and sewer access on transaction prices. IV estimates range between about 1.3 and 1.5 log points, estimated precisely. This treatment effect is enormous. A 1.3 log point increase in parcel price is a factor of 3.7.

Comparing IV to OLS results suggests that the equilibrium process assigns piped water and sewer service to parcels that are less valuable after conditioning on observable controls. This is consistent with anecdotal evidence presented earlier.

Figure 3.6 illustrates an increase in piped water and sewer access and transaction prices that occurs when we cross Tyler street to leave the Southwest triangle. These changes appear to occur sharply in the figure. Nevertheless, we are concerned that this increase may reflect a confounding trend correlated with treatment and transaction prices. To address this concern, in column 4 of table 3.2 we restrict the sample to a narrower window that includes only parcels within 1000 ft. of Tyler street. The magnitudes of the reduced form and first stage are reduced, but the IV estimate is unchanged. In column 5, we include controls for distance to Tyler street in our regression of column 2, where we allow the slope of this trend to change at Tyler street. Once again these controls reduce the magnitude of first stage and reduced form effects by about half, but leave the IV point estimate unchanged, although the standard error increases to just above the 10% significance threshold.

To refine this test, we consider the impact of a hypothetical confounding trend in land prices across Tyler Street, the trend that we observe across the Tyler Street boundary during 1886-9, after piped water and sewer access is universal on both sides of the border. Implicitly, we suppose that the entire (small) trend we observe in 1886-9 is due to confounding unobservables rather than path dependence on an otherwise homogeneous landscape. Appendix

Table C.1.2 is similar to panel D of table 3.2, and reports this trend in column 3. We then subtract this trend from transaction prices, the dependent variable, in our 1874-80 sample in column 6 of table 3.2. Unsurprisingly, this leads to a smaller estimated treatment effect, but one that is estimated precisely and is still nearly 0.7 log points.

Summing up, the validity of our research design rests on four pieces of evidence. First, the sensitivity of sewer construction costs to otherwise imperceptible changes in grade supports the a priori argument that the instrument affects outcomes only through its effect on the likelihood of treatment. Second, the near disappearance of price differences across Tyler street after water and sewer access equalizes across this boundary suggests that, except for piped water and sewer access, the distribution of parcel prices is the same on both sides of the boundary. Third, the difference between OLS and IV estimates is consistent with what one would predict from anecdotal evidence about the assignment process; the equilibrium assignment process favors cheaper parcels. Finally, the robustness of results to various permutations of control variables, and to correction for a confounding spatial trend, suggests that omitted variables correlated with the instrument and outcome are not confounding our estimates.

The estimates in panel (d) of table 3.2 are LATEs for our Quasi-experimental sample. We now turn our attention to whether this estimate differs from the ATE in this sample and whether we can extrapolate to the Relevant sample.

To begin, columns 7 and 8 of table 3.2 re-estimate the specifications of columns 1 and 2 on the Extended-quasi-experimental sample. That is, the sample of transactions drawn from within 2000' of the Northern or Eastern boundary of the Southwest Triangle.

A Local Average Treatment Effect coincides with the Average Treatment Effect if treatment effects are the same for all units. By expanding our sample, we change the set of compliers, and hence the sample of units over which the LATE is estimated. We observe that coefficients in columns 7 and 8 are statistically indistinguishable from their counterparts estimated on the smaller Quasi-experimental sample. This suggests either that treatment effects are not very heterogeneous, or that the distributions of treatment effects in the two samples of compliers are similar.

We would ultimately like to extrapolate our estimate to the Relevant sample. The Extended-quasi-experimental sample has a larger support for X and presumably, a larger support for unobservable determinants of treatment and potential outcomes. In this sense, less extrapolation is required from the Extended-quasi-experimental sample to the Relevant sample, than from the smaller Quasi-experimental sample.

We note that the validity for our research design is easier to defend on the smaller Quasi-experimental sample than the Extended-quasi-experimental. Figure C.1.2 in the appendix reproduces the border plots of figure 3.6 for the larger sample. Neither prices nor sewer access change as sharply at the boundary of the Southwest Triangle in the larger sample.[15] This increases our concern about the possibility of a confounding trend across the border and motivates our preference for estimates based on the smaller Quasi-experimental sample.

The LIV/MTE framework developed in Heckman and Vytlacil (2005) and Carneiro et al. (2010) offers a method to estimate treatment effect heterogeneity and a framework to evaluate the difference between LATEs and ATEs. Moreover, as we will show, this framework provides a foundation for extrapolating our estimates from the Quasi-experimental to the Relevant sample under a weaker assumption than "no heterogeneous treatment effects".

The LIV/MTE framework recasts the potential outcome framework described in Angrist

[15] This is because, 20 years after the 1855 ordinance, both sides of the Eastern boundary of the Southwest Triangle have sewer service, see figure 3.2.

et al. (1996) as a Roy model. Each unit selects into treated or untreated status on the basis of a third selection equation. Formally,

$$
\begin{aligned}
Y_1 &= X'\delta_1 + U_1 \\
Y_0 &= X'\delta_0 + U_0 \\
D &= 1[v(X,Z) - U_D \geq 0],
\end{aligned}
\tag{3.1}
$$

where Y_1 denotes treated potential outcome and Y_0 not treated. We assume that the controls enter the potential outcome equations linearly with coefficients δ_1 and δ_0, and make the "practical independence" assumption as in Carneiro et al. (2010),

$$
(X,Z) \perp (U_1, U_0, U_D) \tag{3.2}
$$

U_D measures unobserved "resistance to treatment," in our context, unobservable determinants of the cost of piped water and sewer access for each parcel. We assume that U_D is continuously distributed.

Let $p = F(X,Z) \equiv P(D=1|X,Z)$ be the propensity score in the Quasi-experimental sample. Let $\widetilde{U}_D$ denote U_D normalized by its cdf. That is, $\widetilde{U}_D = F_{U_D}(U_D) \sim Unif(0,1)$. This transformed unobserved heterogeneity ranks units in the population P according to the unobservable cost of access to piped water and sewage, i.e., $\widetilde{U}_D$ is smaller as unobserved costs of piped water and sewer access are smaller. On the basis of arguments presented in Carneiro et al. (2011), we state our estimating equation and subsequent derivations in terms of this transformed variable.

Define marginal treatment effects, MTE, for each conditioning covariate value X and

$\widetilde{U}_D \in [0,1]$ as

$$MTE(X, \widetilde{U}_D) \equiv E(Y_1 - Y_0 | X, \widetilde{U}_D)$$

That is, MTE describes how the causal effects vary with observable characteristics, X, and with the unobservable $\widetilde{U}_D$.

To estimate MTEs, we run the local IV regression

$$\begin{aligned} p &\equiv \Pr(D = 1 | X, Z) = F(X, Z), \\ Y &= X'\delta_0 + \widehat{p}X'(\delta_1 - \delta_0) + K(\widehat{p}) + \varepsilon. \end{aligned} \tag{3.3}$$

The first equation is a first stage binary regression of treatment status on the instrument and controls. In our case, we specify a Logit regression with linear index in (X, Z) for the first stage. The second equation is a structural equation with a control function in $\widehat{p}$, where the additive functional form follows from our specification (3.1) and the practical exogeneity restriction (3.2). In light of our small sample size, we restrict attention to the case with a parametric cubic specification for $K(\cdot)$,

$$K(\widehat{p}) = \gamma_1 \widehat{p} + \gamma_2 \widehat{p}^2 + \gamma_3 \widehat{p}^3.$$

Heckman and Vytlacil (2005) show that the derivative of the local IV regression with respect to the propensity score identifies the marginal treatment effect, and that taking the expectation of MTE over $(X, \widetilde{U}_D)$ identifies the average treatment effect. That is,

$$MTE(X, \widetilde{U}_D) = X'(\delta_1 - \delta_0) + \gamma_1 + 2\gamma_2 \widetilde{U}_D + 3\gamma_3 \widetilde{U}_D^2 \tag{3.4}$$

$$ATE = E(X)'(\delta_1 - \delta_0) + \gamma_1 + \gamma_2 + \gamma_3. \tag{3.5}$$

Equation (3.4) allows explicit tests for heterogeneity of treatment effects. If $\delta_1 - \delta_0 \neq 0$ then the marginal treatment effects vary with unit observables. If γ_3 or $\gamma_2 \neq 0$ then the marginal treatment effects vary with unobserved resistance to treatment. Rejecting both sorts of treatment heterogeneity means that LATE, any weighted average of MTEs, and ATE are all equal. In this case, we can interpret the conventional linear TSLS estimator for the coefficient of endogenous D as a consistent estimator for ATE.

We estimate equation (3.3) for specifications corresponding to those in columns 1,2,3, 7, and 8 of table 3.2. Because equation (3.3) is quite long, we relegate a complete report of parameter estimates and bootstrapped standard errors to appendix table C.1.3. Table 3.3 reports estimates of ATE derived from these regressions, along with several hypothesis tests.

The first row of table 3.3 reports a χ^2 test of significance of our instrument in the first stage Logit regression. As in our TSLS estimations, we easily reject the hypothesis that our instrument does not affect treatment.

The second row of table 3.3 reports p-values of the tests of the hypothesis that all terms involving the propensity for treatment are zero. That is, that treatment effects are different from zero. This is rejected in all specifications. Piped water and sewer almost surely affect land prices in our Quasi-experimental and Extended-quasi-experimental samples.

The third row tests the hypothesis that there is heterogeneity in treatment effects by observables. The fourth row tests whether there is heterogeneity in treatment effects by unobservables. The fifth row tests the joint hypothesis of either sort of treatment effect heterogeneity.

The results of these tests vary with sample. In our Quasi-experimental sample, columns 1,2 and 3, we see clear evidence of treatment heterogeneity on unobservables, somewhat weaker evidence for treatment effects on observables, and clearly reject the hypothesis of no treatment heterogeneity at all. Columns 4 and 5, we consider the larger Extended-quasi-experimental sample. Here, we reject the hypothesis of any treatment effect heterogeneity at the 7 or 15% level, depending on specification, but we cannot reject the hypothesis of treatment heterogeneity by observables or by unobservables alone. Inspection of appendix table C.1.3 suggests that treatment effects likely vary by year in all specifications, though there is no clear pattern in the coefficients across years.

The sixth row of table 3.3 calculates the average treatment effect given in equation (3.5) along with bootstrapped standard errors. Comparing to the LATEs estimated in table 3.2 we see that ATEs are marginally smaller than LATEs in the Quasi-experimental sample, $[0.72, 1.04]$ versus $[1.28, 1.52]$ and both are estimated precisely. In the larger Extended-quasi-experimental sample, ATE and LATE are statistically indistinguishable. Even the smallest of these ATE estimates is still very large; $e^{0.72} \approx 2$, so these estimates indicate that piped water and sewer access at least doubles land values.

The differences between between LATE and ATE estimates are consistent with other results in rows 3 to 5 of table 3.3. Heterogeneous treatment effects are necessary if ATE and LATE are to diverge.

Figure 3.7 presents a standard diagnostic for the LIV regression presented in column 2 of tables C.1.3 (a) and (b). This figure is a histogram showing the frequency of treated and untreated transactions as a function of $\widehat{p}$. As we expect from table 3.1, the distribution of parcels is heavily skewed toward "treated"; 0.47 of the Quasi-experimental sample South of Tyler street has piped water or sewer access, and this share is even higher to the North. With this said, conditional on this skewed distribution, the histograms for treated and untreated parcels are similar, although there is more mass left of 0.6 for untreated parcels. The corresponding histograms for other specifications reported in table C.1.3 (not reported) are qualitatively similar.

Figure C.1.1 is a second standard diagnostic figure. Figure C.1.1 plots marginal treatment effects as a function of resistance to treatment, $\widetilde{U}_D$, and lets us visualize the importance of treatment heterogeneity on unobservables. In light of the hypothesis test presented in column 2, row 4 of table 3.3, that this figure suggests marginal treatment effects change with unobservables is unsurprising. Because most of the probability mass of treated and untreated parcels has $\widehat{p}$ of at least 0.6, the region of figure C.1.1 to the left of 0.6 should be understood as extrapolation from the larger values.[16]

The final row of Table 3.3 presents the p-value for the instrument validity test proposed in Carr and Kitagawa (2021). This test evaluates the joint null hypothesis of practical exogeneity (3.2), instrument monotonicity, and the functional form specification for the potential outcome equations (3.1). p-values consistently above 15% indicate that the data do not reject the assumptions on which our MTE and ATE estimates rely.[17]

[16]Identification of $MTE(X, \widetilde{U}_D)$ without a parametric control function $K(\cdot)$ is possible for values of $\widetilde{U}_D$ supported by the distribution of propensity scores. Figure 3.7 indicates that observations with propensity scores near 1 largely contribute to the estimation of cubic $K(\cdot)$. MTE estimates for the range of $\widetilde{U}_D$'s without much probability mass extrapolate using the functional form of $K(\cdot)$.

[17]We also apply the IV validity test of Mourifié and Wan (2017). This test evaluates the strict exogeneity of instrument (i.e., Z is also independent of X) rather than conditional exogeneity. We do not reject the null of instrument validity at 5% significance level for the Quasi-experimental sample. However, we do reject the null at the same level for the Extended-quasi-experimental sample. Taken together with the results of the Carr & Kitagawa test reported in table 3.3, this means that we reject the strict exogeneity of of our instrument, but fail to reject conditional exogeneity. It follows that controlling for conditioning covariates is necessary for the estimation of causal effects in our model, particularly in the Extended-quasi-experimental sample.

While our LIV estimation does not offer conclusive evidence for the importance of heterogeneous treatment effects, neither does it offer much reassurance that they are not important. Given this, we consider the problem of extrapolating our ATE estimates under both assumptions, that treatment effects are heterogeneous, and that they are not.

In the absence of treatment heterogeneity, extending our treatment effect estimates from the Quasi-experimental to the Relevant sample is straightforward. Estimates in table 3.2 can be interpreted as Average Treatment Effects, and provided treatment effects remain constant on the larger support of the Relevant sample, these estimates apply immediately to units in the larger sample.

However, table 3.3 suggests that concern about treatment heterogeneity is warranted. Given this, we develop a method for extrapolating treatment effects in the presence of treatment heterogeneity.[18]

This extrapolation requires that equations (3.1) and (3.2) continue to hold on the Quasi-experimental sample. In addition, we assume

$$\begin{aligned} Y_1^* &= X^{*\prime}\delta_1 + U_1^* \\ Y_0^* &= X^{*\prime}\delta_0 + U_0^* \\ D^* &= 1[v(X^*, Z^*) - U_D^* \geq 0]. \end{aligned} \tag{3.6}$$

and that

$$P^*_{U_1^*, U_0^*, U_D^*} = P_{U_1^{**}, U_0^*, U_D^*}. \tag{3.7}$$

[18]We note that simply conducting our TSLS regressions on the Relevant sample offers a particularly simple solution to this problem. However, and unsurprisingly, our instrument is not relevant on this larger more heterogeneous sample.

In words, we assume that the same econometric model governs the effects of treatment in the Relevant sample as in the Quasi-experimental sample and that the joint marginal distribution of residuals is the same across the two samples. These conditions would be satisfied, for example, if the mechanism and magnitude of the causal effect are the same in both samples, and unobserved resistance to receiving the treatments is identically distributed between them.

In our data, the cost shock Z is observed on the Quasi-experimental sample and latent on the Relevant sample. In addition, we can credibly assume that Z is randomized in the Quasi-experimental sample, but it is probably not randomized in the Relevant sample. Our approach to extrapolation does not require that the joint distributions of observable characteristics and the instrument are identical for the Quasi-experimental and Relevant samples.

Assuming equations (3.1), (3.2), (3.6) and (3.7), we can extrapolate MTE estimates from the Quasi-experimental to the Relevant sample and use them to calculate an average treatment effect on the Relevant sample as follows,

$$ATE^{*} = E(X^{*})'(\delta_1 - \delta_0) + \gamma_1 + \gamma_2 + \gamma_3. \tag{3.8}$$

Appendix tableC.2 provides a proof.

In words, the average treatment effect for the Relevant sample is the same as for the Quasi-experimental sample, except that we must adjust for differences in the distributions of observable controls between the two samples. If the structural equations that govern treatment effects and assignment are the same across samples, and if the distribution of unobservables is the same, then we can extrapolate MTE estimates. This result holds even if the instrument is latent or dependent on the unobservables in the Relevant sample, or if the support of observable controls differs across samples. This result seems intuitive and, to our knowledge, no similar result exists in the literature.

The seventh row of table 3.3 presents our estimates of ATE^* for each of our specifications, along with bootstrapped standard errors. All are estimated precisely enough that they may easily be distinguished from zero. These estimates of ATE^* range from 0.75 to 1.04, across all samples and specifications. There is even less variation in ATE^* across samples and specifications than we saw for ATE, but in no case is the ATE^* statistically distinguishable from the corresponding ATE.

Conditional on the validity of our estimates of ATE, the validity of our estimates of ATE^* hinges on equations (3.6) and (3.7). Ideally, we would be able to test whether these equations hold in our data. We have not been able to define such a test, and our investigations suggests that a test may not exist except in the uninteresting case where there is no treatment heterogeneity. In the absence of a formal test, we provide informal evidence that the Quasi-experimental and Relevant samples are both governed by the same basic economic logic.

Figure 3.8 compares the Quasi-experimental and Relevant samples. Panel (a) of figure 3.8 reports mean log prices by year in the Relevant and Quasi-experimental samples, conditional on: ln(Area), ln(miles to CBD), improved and corner. Panel (b) reports mean log prices by parcel area in both samples, conditional on year indicators, ln(miles to CBD), improved and corner. Finally, panel (c) gives counts of transactions by year and sample. None of these figures obviously contradicts the hypothesis that the same basic economic forces are at work determining prices in the Quasi-experimental and Relevant samples.

3.7 The Value of Piped Water and Sewer Access

We can now calculate the effect of piped water and sewer access on land values in the relevant area. We proceed in four steps. First, we calculate the area affected by the piped water and sewer expansion of 1874-80. Second, we calculate average price per square foot of an untreated parcel in this region. Third, we calculate the increase in price per square foot that results from piped water and sewer access. Fourth, multiplying this increase by the area affected gives the total increase in land value resulting from piped water and sewer expansion during 1874-80.

An average residential lot in any of our samples is about 125 feet deep. If we assume that every sewer serves lots on both sides of one street, then each linear foot of sewer serves 250 ft^2 of land area. Our shapefiles of the sewer network then allow us to calculate that about 138m ft^2 of land received piped water and sewer access during 1874-80.

During 1874-80, 384 untreated parcels transacted in the Relevant sample area. The total area of these parcels was about 1.8m ft^2, and their aggregate value was about 0.81m 1880 dollars. Dividing, the average price per ft^2 of land in the Relevant area was about 0.45 dollars.

We must now decide whether to apply an estimated ATE that does or does not allow for heterogeneous treatment effects. Our LIV estimates do not strongly support either hypothesis, and so we proceed using the smallest estimates, 0.75, from column 4 of table 3.3.

Applying this treatment effect to the price per square foot of untreated land in the Relevant sample area, we calculate that piped water and sewer access increases the value of land in this area by $0.45 \times (e^{ATT^*} - 1) = 0.50\$/\text{ft}^2$. That is, using our most conservative estimate, piped water and sewer access increases the value of land by about 110%. Multiplying this increase by the area affected, the total value of the piped water and sewer expansion was slightly above 69m 1880 dollars.

This estimate requires several comments. First, this calculation reflects our smallest estimate of the average treatment effect. If, as we might do on the basis of column 8 of table 3.3, we reject the hypothesis of heterogeneous treatment effects, then the LATEs we estimate in Table 3.2 can be defended as ATEs and extended to the relevant sample. In this case, using column 7 in table 3.2 (the analog of column 8 of table 3.3) we have $ATE = 1.3$. Using this estimate to value piped water and sewer access gives about 164m 1880 dollars.

Second, an average parcel in the Quasi-experimental sample receives piped water and sewer service about four years after it is sold. Thus, our estimates reflect the flow value of four years of piped water and sewer access, not the full asset value. Hoyt (2000) reports that interest rates were about 8% during our study period. If we denote our estimated aggregate value by V^* and assume that this flow value arrives every four years for perpetuity, then the full asset value of piped water and sewer access is $\sum_{t=0}^{\infty}\left[\left(\frac{1}{1.08}\right)^4\right]^t V^* \approx 3.8V^*$. Thus, we should multiply by about 3.8 to scale up our four year flow value to an asset value. Applying this adjustment to our 69m dollar estimate of the four year flow value, we have an asset value of about 262m 1880 dollars.

Third, as we noted earlier, piped water and sewer expansions were largely paid for with bonds that were serviced by property taxes (Chicago Board of Public Works, 1873). If there is any sort of capitalization of piped water and sewer construction costs into transaction prices, then this would bias our estimates of treatment effects downward.

Finally, while it seems reasonable to ignore general equilibrium effects in our estimates of treatment effects based on the relatively small Quasi-experimental sample, this assumption seems difficult to defend when we extend our estimates to the Relevant area, the entire area that received piped water and sewer access between 1874-80. Given this, our estimates of the value of piped water and sewer expansion should be understood as a basis for evaluating a marginal counterfactual change in the extent of the Relevant area, or as being net of general equilibrium effects.

With our estimates of the value of piped water and sewer access in place, we turn to estimates of its cost. We digitize expenditures on water and sewer for the 1874-80 period (Chicago Board of Public Works, 1873). Construction costs during this time were: Sewer Construction, \$1.5m; Maintenance, \$0.4m; Waterworks construction, \$2.4m. Summing, we have a total of \$4.3m.

Our estimate of the four year flow value of piped water and sewer access was about \$69m, about 16 times as large as construction costs. Our estimate of the total asset value piped water and sewer access is \$262m, about 60 times as large as costs. Both of these calculations are based on our smallest estimate of average treatment effects. If we use one of our larger (but still defensible) estimates of ATE, these ratios approximately triple.

3.8 Conclusion

While tremendous progress has been made in providing safe water and modern sanitation for the relatively poor recent immigrants to developing world cities, access is far from universal. A large body of evidence suggests that in the absence of modern public health and sanitation infrastructure, urban density causes disease. Thus, increasing access to high quality drinking water and modern sanitation would seem to call for a crisis response. However, relatively poor developing world cities face a portfolio of crises. Not only do their residents need more and better water and sewer infrastructure, they also need more and better roads, public transit, electricity supply and distribution, education, and housing. Trade-offs will inevitably need to be evaluated and made.

With this in mind, piped water and sewer access are conspicuously understudied. There is now a large active literature evaluating various improvements to transportation infrastructure, both in the developed and developing world. Electricity generation and distribution has also received attention. The literature on piped water and sewer access is much less developed. Indeed, as a result of conflicting estimates presented in Cutler and Miller (2005) and Anderson et al. (2018), recent research has served to increase our uncertainty about the importance public health policy. In this light, our results are doubly important. We are the first to evaluate the effect of piped water and sewer access on land prices, a comprehensive revealed preference measure of value, and our results suggest a value of piped water and sewer access that is large, even relative to the large estimates of Cutler and Miller (2005).

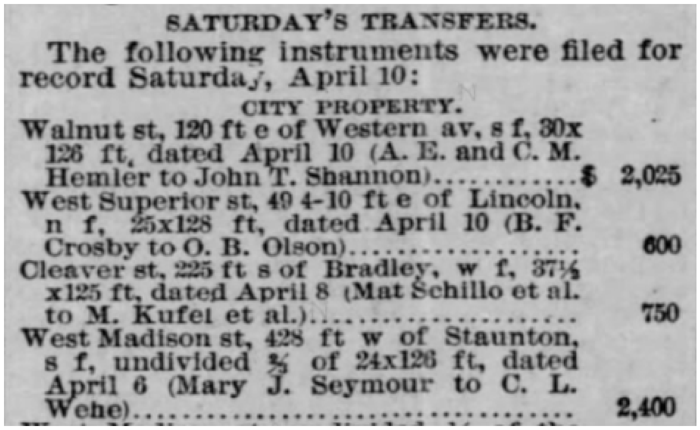
This generally supports a high priority for water and sewer infrastructure. It also highlights the importance of further research on the the issue. The disease environment in modern Latin American and African cities is clearly different than it was in 19th century Chicago (see Henderson and Turner (2020)), so the desirability of studies conducted in these places is high. An important obstacle to such research has been the absence of a credible research design for estimating causal effects. We are hopeful that some variant of the research design we develop can help to address this issue.

Our results also inform the ongoing inquiry into the development of the American economy. Up until now, almost all evidence for or against the importance of piped water and sewer infrastructure reflects changes in mortality rates, and is estimated by comparing outcomes before and after a particular intervention. By offering a novel research design and a different outcome, we provide independent evidence for the importance piped water and sewer infrastructure. Our most conservative estimate indicates that piped water and sewer access more than doubled land prices.

Finally, we propose a technique for the principled extrapolation of treatment effects from a quasi-experimental study area to an area that is more relevant for economic analysis. The practice of restricting attention to small populations or areas, carefully chosen so that a quasi-experimental research design may be defended, is a pervasive practice in applied micro-economic analyses. Thus, so to is the problem of extrapolating to more economically interesting samples. We hope that our technique for extrapolating treatment effects will, therefore, find wide use among other applied researchers.

3.9 Figures and Tables

Figure 3.1: Land Transactions in the Chicago Tribune



SATURDAY'S TRANSFERS.

The following instruments were filed for record Saturday, April 10:

CITY PROPERTY.

Walnut st, 120 ft e of Western av, s f, 30x 126 ft, dated April 10 (A. E. and C. M. Hemler to John T. Shannon)............$ 2,025

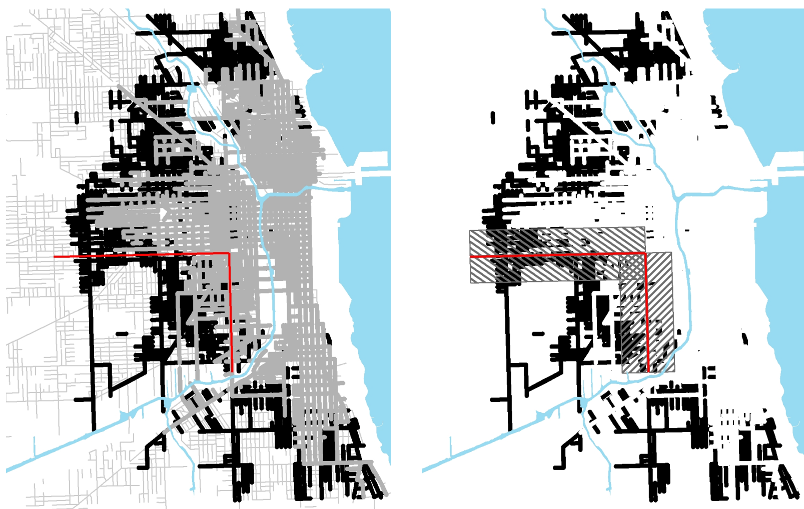
West Superior st, 49 4-10 ft e of Lincoln, n f, 25x128 ft, dated April 10 (B. F. Crosby to O. B. Olson).................. 600

Cleaver st, 225 ft s of Bradley, w f, 37½ x125 ft, dated April 8 (Mat Schillo et al. to M. Kufel et al.).................... 750

West Madison st, 428 ft w of Staunton, s f, undivided ⅝ of 24x126 ft, dated April 6 (Mary J. Seymour to C. L. Wehe)................................ 2,400

An example of listings of land transactions in the Chicago Tribune. Our land transaction data results from digitizing all transactions reported on Saturday between 1873 and 1889. Note that each record reports the nearest intersection, price, and area. Most records also report if the parcel is “improved“ or “corner.“

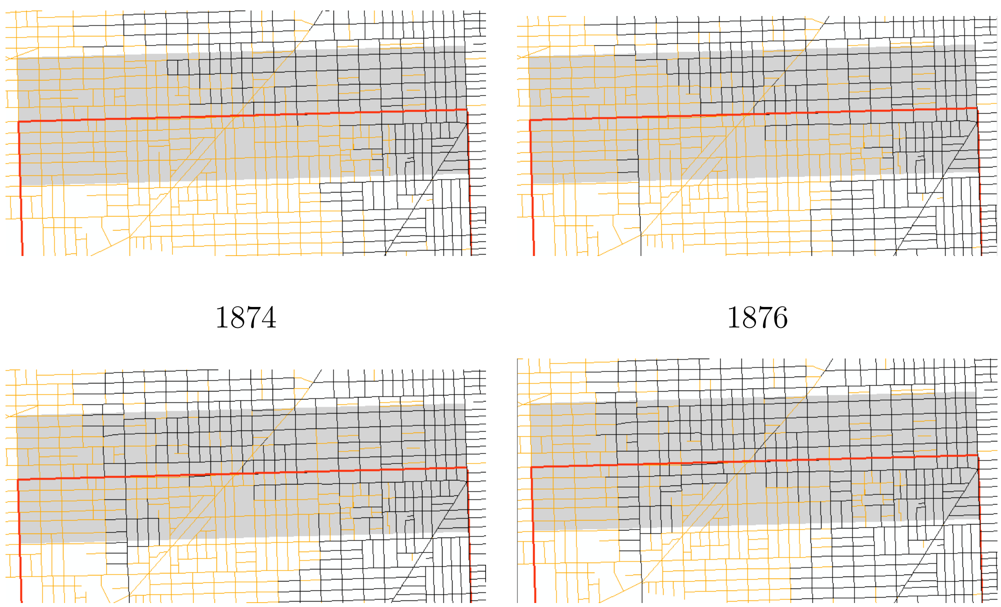
Figure 3.2: Extent of Sewer Network, Southwest Triangle, and Quasi-Experimental Samples



(a) (b)

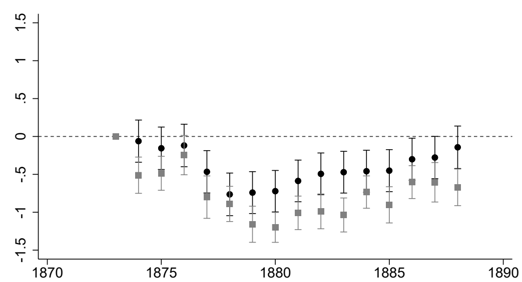
(a) Sewers before 1874, during 1874-1880, after 1880, and boundaries of the Southwest triangle. (b) "Relevant' sample area (1874-1880 expansion) and "Quasi-experimental' sample areas.

Figure 3.3: Sewer Extent in Study Area, 1874-1880



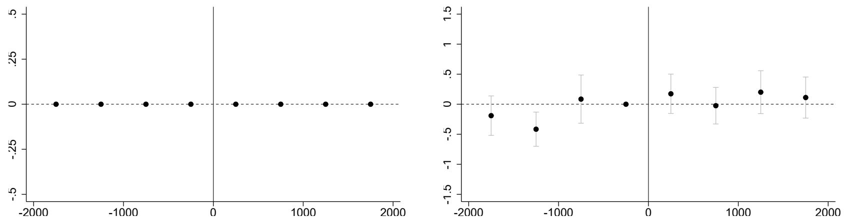
Tan indicates the 1930s street network and red indicates boundaries of the Southwest Triangle. Light gray indicates the area within 2000 feet of Tyler street running 14,000 feet West from Halsted Street. Black lines indicate the sewer network. There is more sewer coverage in the Northern half of our study area than the southern half during the 1874-80 study period.

Figure 3.4: Land Prices in Chicago and Quasi-Experimental Sample



Mean ln(Price) by year in Quasi-experimental sample (Gray) and all of Chicago (Black). Controls: ln(miles to CBD), improved, corner, ln(Area).

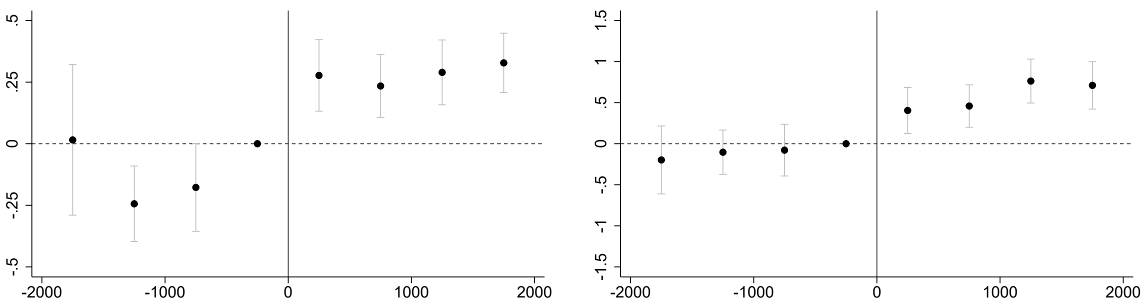
Figure 3.5: Sewer Share and Price by Distance to Boundary, 1886-9



(a) (b)

(a) x-axis is distance to Tyler Street boundary, with $x < 0$ displacement South, "inside" and conversely. y-axis is share of transactions sewered between 1886-89, controlling for year indicators, ln(Area), and ln(mi. to CBD)) by 500' long bins. (b) Same as left panel but y-axis is ln(Price), controlling for the same set of covariates. Piped water and sewer access and prices are both the same at the border after sewer and water provision is completed in the Southwest Triangle.

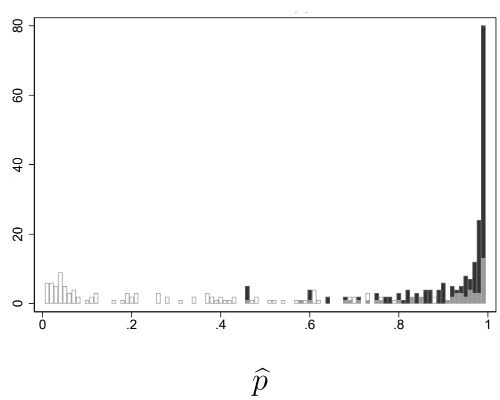
Figure 3.6: Sewer Incidence and Distance to Boundary, 1874-80



(a) (b)

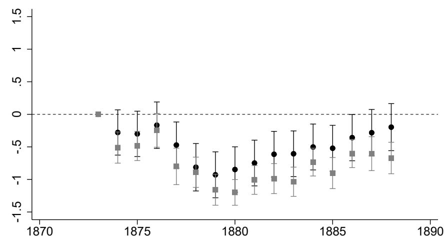
(a) x-axis is distance to Tyler Street boundary, with $x < 0$ displacement South, "inside" and conversely. y-axis is share of transactions sewered between 1874-80, controlling for year indicators, ln(Area), and ln(mi. to CBD)) by 500' long bins. (b) Same as left panel but y-axis is ln(Price), controlling for the same set of covariates. Piped water and sewer access and prices are both the same at the border after sewer and water provision is completed in the Southwest Triangle.

Figure 3.7: Density of Treatment by $\widehat{p}$

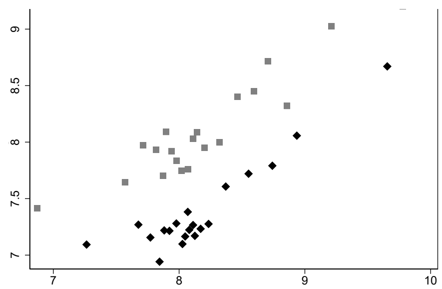


Density of treated and untreated parcels by propensity score. The propensity score distribution is skewed toward one, but conditional on a mass of propensity scores, treated and untreated parcels both occur. Based on column 2 of table 3.3.

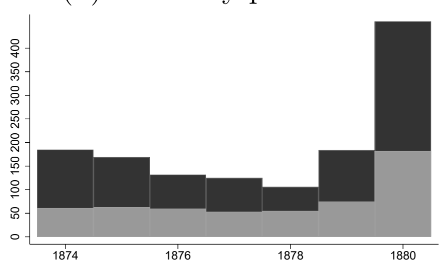
Figure 3.8: Comparison of Quasi-experimental and Relevant Samples.



(a) Prices by year and sample



(b) Prices by parcel area



(c) Transaction frequency by year and sample

(a) Mean log transaction price by year in the main Quasi-experimental (gray) sample and the Relevant (black) sample. Conditional on: ln(Area), ln(miles to CBD), improved, corner. Means and variances of Y in the two samples are similar conditional on year. (b) Mean log transaction price by parcel area. (c) Transactions by year and sample. The Relevant sample is larger, but the distribution of transactions across years is similar for the Quasi-experimental and Relevant samples. The spike in 1880 reflects a change in sampling effort, not in transaction volume.

Table 3.1: Summary Statistics 1874-1880

	(1)	(2)	(3)	(4)
	SW$\triangle = 1$	SW$\triangle = 0$	t-test	Relevant
Share Sewered	0.47	0.92	11.04	0.70
	(0.50)	(0.27)		(0.46)
Log Price	7.70	8.42	8.44	7.41
	(0.86)	(0.76)		(0.91)
Log Distance to CBD	9.13	9.10	-0.89	9.49
	(0.38)	(0.38)		(0.25)
Log Area	8.12	8.26	1.88	8.17
	(0.62)	(0.69)		(0.54)
Share Improved	0.11	0.23	2.99	0.15
	(0.31)	(0.42)		(0.36)
Share Corner	0.11	0.13	0.42	0.14
	(0.32)	(0.33)		(0.34)
Distance to Horsecar	884	427	-9.53	1757
	(573)	(335)		(1351)
Distance to Major Street	564	475	-2.13	441
	(427)	(363)		(372)
Year	1877.18	1877.45	1.14	1877.60
	(2.19)	(2.17)		(2.26)
N	150	211		1358

Means and standard deviations of parcel characteristics. Column 1 reports on parcels in the Quasi-experimental sample (within 2000' of Tyler St. west of Halsted) that are in the Southwest Triangle (south of Tyler Street). Column 2 reports on parcels that are not in the Southwest Triangle (north of Tyler Street). Column 3 reports the t-statistic for the difference between the first two columns. Column 4 presents parcel means and standard deviations for all parcels in the Relevant sample.

Table 3.2: OLS, First Stage, Reduced Form, and TSLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: OLS								
Sewer=1	.413***	.39***	.4***	.328***	-.018	.194***	.276***	.239***
	(.086)	(.082)	(.084)	(.139)	(.101)	(.08)	(.081)	(.078)
R^2	0.386	0.502	0.504	0.567	0.598	0.505	0.376	0.439
B: Red. Form								
SW$\triangle = 0$	.657***	.568***	.714***	.439***	.292*	.3***	.336***	.332***
	(.072)	(.069)	(.073)	(.093)	(.151)	(.068)	(.063)	(.059)
R^2	0.486	0.568	0.591	0.606	0.602	0.527	0.397	0.462
C. 1st Stage								
SW$\triangle = 0$	.432***	.443***	.451***	.323***	.194**	.443***	.259***	.259***
	(.039)	(.04)	(.043)	(.057)	(.097)	(.04)	(.031)	(.031)
R^2	0.451	0.455	0.455	0.456	0.474	0.455	0.333	0.335
F-stat	119.729	125.018	110.664	32.311	3.992	125.018	71.711	71.283
D. IV								
Sewer=1	1.522***	1.283***	1.582***	1.36***	1.501	.678***	1.296***	1.283***
	(.22)	(.191)	(.209)	(.352)	(1.067)	(.164)	(.277)	(.266)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y
Imp. & Corner		Y	Y	Y	Y	Y		Y
H.car & Maj. St.			Y					
Sample	Q.E.	Q.E.	Q.E.	Q.E. 1k'	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	351	172	351	351	533	533

All results based on transactions during 1874-80. Columns 1-3, 5 rely on the Quasi-experimental sample, 7 and 8 on the Extended-quasi-experimental sample, and column 4 restricts attention to the subset of the Quasi-experimental sample within 1000' of Tyler Street. (A) Reports OLS regressions of log transaction price on the treatment indicator. (B) Reports reduced form regressions log transaction price on the instrument. (C) Reports first stage regressions of treatment on instrument. (D) Reports TSLS estimate of the effect of water and sewer access on log parcel price.

Table 3.3: LIV Regression Test Statistics

	(2)	(4)	(6)	(8)	(10)
χ^2	220	221	237	243	245
H0: $\delta_1 - \delta_0, \gamma_1, \gamma_2, \gamma_3 = 0$	0	0	0	.005	.002
H0: $\delta_1 - \delta_0 = 0$	.108	.07	.074	.298	.205
H0: $\gamma_2, \gamma_3 = 0$	.002	0	.001	.656	.498
H0: $\delta_1 - \delta_0, \gamma_2, \gamma_3 = 0$	.001	.001	.001	.15	.076
ATE	1.04^{***}	$.72^{**}$	$.8^{***}$	1.31^{*}	1.31^{**}
	(.4)	(.35)	(.32)	(.69)	(.65)
ATE^*	1.04^{***}	$.75^{***}$	$.89^{***}$	1.05^{**}	$.87^{**}$
	(.31)	(.27)	(.36)	(.46)	(.41)
Carr & Kitagawa	0.156	0.154	0.434	0.792	0.916
Year FE & ln(Area)	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y
Improved and Corner		Y	Y		Y
Horsecar and Major Street			Y		
Sample	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	351	533	533

Various test statistics based on estimates of the LIV model of equation (3.3) and estimates of ATE and ATE^* based on equations (3.5) and (3.8). Complete report of coefficient estimates is in table C.1.3. All estimations based on transactions during 1874-80. Columns 2,4, and 6 rely on the Quasi-experimental sample, 8 and 10 on the Extended-quasi-experimental sample. Omitted odd numbered columns report first stage Logit coefficients in appendix table C.1.3. Bottom panel indicates controls for the regression above. Bootstrapped standard errors in parentheses. $*, **, ***$ indicate 10%, 5%, 1% significance.

Appendix A Climate Risk and Preferences over the Size of Government

A.1 List of Ballot Initiatives in Study Period

Begins next page.

Year	Name	California Secretary of State Description	Big Government Sample Position	Environmental Sample Position
2018	PROP 001	AUTHORIZES BONDS TO FUND SPECIFIED HOUSING ASSISTANCE PROGRAMS. LEGISLATIVE STATUTE.	Yes	Yes
2018	PROP 002	AUTHORIZES BONDS TO FUND EXISTING HOUSING PROGRAM FOR INDIVIDUALS WITH MENTAL ILLNESS. LEGISLATIVE STATUTE.	Yes	N/A
2018	PROP 003	AUTHORIZES BONDS TO FUND PROJECTS FOR WATER SUPPLY AND QUALITY, WATERSHED, FISH, WILDLIFE, WATER CONVEYANCE, AND GROUNDWATER	Yes	N/A
2018	PROP 004	AUTHORIZES BONDS FUNDING CONSTRUCTION AT HOSPITALS PROVIDING CHILDREN'S HEALTH CARE. INITIATIVE STATUTE.	Yes	N/A
2018	PROP 005	CHANGES REQUIREMENTS FOR CERTAIN PROPERTY OWNERS TO TRANSFER THEIR PROPERTY TAX BASE TO REPLACEMENT PROPERTY. INITIATIVE CONSTITUTIONAL	No	N/A
2018	PROP 006	ELIMINATES CERTAIN ROAD REPAIR AND TRANSPORTATION FUNDING. REQUIRES CERTAIN FUEL TAXES AND VEHICLE FEES BE APPROVED BY THE ELECTORATE. INITIATIVE	No	No
2018	PROP 007	CONFORMS CALIFORNIA DAYLIGHT SAVING TIME TO FEDERAL LAW. ALLOWS LEGISLATURE TO CHANGE DAYLIGHT SAVING TIME PERIOD. LEGISLATIVE STATUTE.	N/A	N/A
2018	PROP 008	REGULATES AMOUNTS OUTPATIENT KIDNEY DIALYSIS CLINICS CHARGE FOR DIALYSIS TREATMENT. INITIATIVE STATUTE.	N/A	N/A
2018	PROP 010	EXPANDS LOCAL GOVERNMENTS' AUTHORITY TO ENACT RENT CONTROL ON RESIDENTIAL PROPERTY. INITIATIVE STATUTE.	Yes	N/A
2018	PROP 011	REQUIRES PRIVATE-SECTOR EMERGENCY AMBULANCE EMPLOYEES TO REMAIN ON-CALL DURING WORK BREAKS. ELIMINATES CERTAIN EMPLOYER LIABILITY. INITIATIVE STATUTE.	N/A	N/A
2018	PROP 012	ESTABLISHES NEW STANDARDS FOR CONFINEMENT OF SPECIFIED FARM ANIMALS; BANS SALE OF NONCOMPLYING PRODUCTS. INITIATIVE STATUTE.	N/A	Yes
2016	PROP 051	SCHOOL BONDS. FUNDING FOR K-12 SCHOOL AND COMMUNITY COLLEGE FACILITIES. INITIATIVE STATUTORY AMENDMENT.	Yes	N/A
2016	PROP 052	STATE FEES ON HOSPITALS. FEDERAL MEDICAL MATCHING FUNDS. INITIATIVE STATUTORY AND CONSTITUTIONAL AMENDMENT.	N/A	N/A
2016	PROP 053	REVENUE BONDS. STATEWIDE VOTER APPROVAL. INITIATIVE CONSTITUTIONAL AMENDMENT.	No	N/A
2016	PROP 054	LEGISLATURE. LEGISLATION AND PROCEEDINGS. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2016	PROP 055	TAX EXTENSION TO FUND EDUCATION AND HEALTHCARE. INITIATIVE CONSTITUTIONAL AMENDMENT.	Yes	N/A
2016	PROP 056	CIGARETTE TAX TO FUND HEALTHCARE, TOBACCO USE PREVENTION, RESEARCH, AND LAW ENFORCEMENT. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	Yes
2016	PROP 057	CRIMINAL SENTENCES. JUVENILE CRIMINAL PROCEEDINGS AND SENTENCING. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2016	PROP 058	SB 1174 (CHAPTER 753, STATUTES OF 2014), LARA. ENGLISH LANGUAGE EDUCATION	N/A	N/A
2016	PROP 059	SB 254 (CHAPTER 20, STATUTES OF 2016), ALLEN. CAMPAIGN FINANCE: VOTER INSTRUCTION	Yes	Yes
2016	PROP 060	ADULT FILMS. CONDOMS. HEALTH REQUIREMENTS. INITIATIVE STATUTE.	N/A	N/A
2016	PROP 061	STATE PRESCRIPTION DRUG PURCHASES. PRICING STANDARDS. INITIATIVE STATUTE.	Yes	N/A
2016	PROP 062	DEATH PENALTY. INITIATIVE STATUTE.	N/A	N/A
2016	PROP 063	FIREARMS. AMMUNITION SALES. INTIATIVE STATUTE.	N/A	N/A
2016	PROP 064	MARIJUANA LEGALIZATION. INITIATIVE STATUTE.	N/A	N/A
2016	PROP 065	CARRY OUT BAGS. CHARGES. INITIATIVE STATUTE.	N/A	N/A
2016	PROP 066	DEATH PENALTY. PROCEDURES. INITIATIVE STATUTE.	N/A	N/A
2016	PROP 067	REFERENDUM TO OVERTURN BAN ON SINGLE USE PLASTIC BAGS.	N/A	Yes
2014	PROP 001	AB 1471 WATER QUALITY, SUPPLY AND INFRASTRUCTURE IMPROVEMENT ACT OF 2014	Yes	Yes
2014	PROP 002	STATE RESERVE POLICY.	N/A	N/A
2014	PROP 045	APPROVAL OF HEALTHCARE INSURANCE RATE CHANGES. INITIATIVE STATUTE.	N/A	N/A
2014	PROP 046	DRUG AND ALCOHOL TESTING OF DOCTORS. MEDICAL NEGLIGENCE LAWSUITS. INITIATIVE STATUTE.	N/A	N/A
2014	PROP 047	CRIMINAL SENTENCES. MISDEMEANOR PENALTIES. INITIATIVE STATUTE.	N/A	N/A
2014	PROP 048	REFERENDUM TO OVERTURN INDIAN GAMING COMPACTS.	N/A	N/A

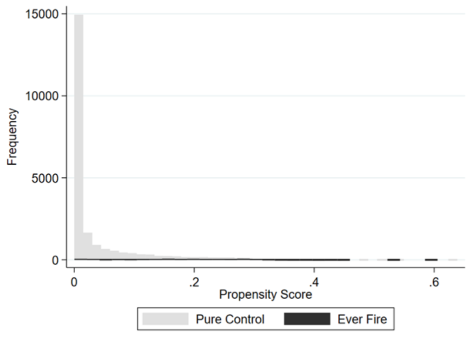
Year	Name	California Secretary of State Description	Big Government Sample Position	Environmental Sample Position
2012	PROP 030	TEMPORARY TAXES TO FUND EDUCATION. GUARANTEED LOCAL PUBLIC SAFETY FUNDING. INITIATIVE CONSTITUTIONAL AMENDMENT.	Yes	N/A
2012	PROP 031	STATE BUDGET. STATE AND LOCAL GOVERNMENT. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	No	No
2012	PROP 032	PROHIBITS POLITICAL CONTRIBUTIONS BY PAYROLL DEDUCTIONS. PROHIBITIONS ON CONTRIBUTIONS TO CANDIDATES. INITIATIVE STATUTE.	N/A	No
2012	PROP 033	CHANGES LAW TO ALLOW AUTO INSURANCE COMPANIES TO SET PRICES BASED ON A DRIVER'S HISTORY OF INSURANCE COVERAGE. INITIATIVE STATUTE.	N/A	N/A
2012	PROP 034	DEATH PENALTY REPEAL. INITIATIVE STATUTE.	N/A	N/A
2012	PROP 035	HUMAN TRAFFICKING. PENALTIES. SEX OFFENDER REGISTRATION. INITIATIVE STATUTE.	N/A	N/A
2012	PROP 036	THREE STRIKES LAW. SENTENCING FOR REPEAT FELONY OFFENDERS. INITIATIVE STATUTE.	N/A	N/A
2012	PROP 037	GENETICALLY ENGINEERED FOODS. MANDATORY LABELING. INITIATIVE STATUTE.	N/A	Yes
2012	PROP 038	TAX FOR EARLY EDUCATION AND EARLY CHILDHOOD PROGRAMS. INITIATIVE STATUTE.	Yes	N/A
2012	PROP 039	TAX TREATMENT FOR MULTISTATE BUSINESSES. CLEAN ENERGY AND ENERGY EFFICIENCY FUNDING. INITIATIVE STATUTE.	YEs	Yes
2012	PROP 040	REDISTRICTING. STATE SENATE DISTRICTS. REFERENDUM.	N/A	N/A
2010	PROP 019	CHANGES CALIFORNIA LAW TO LEGALIZE MARIJUANA AND ALLOW IT TO BE REGULATED AND TAXED.	N/A	N/A
2010	PROP 020	REDISTRICTING OF CONGRESSIONAL DISTRICTS.	N/A	No
2010	PROP 021	ESTABLISHES $18 ANNUAL VEHICLE LICENSE SURCHARGE TO HELP FUND STATE PARKS AND WILDLIFE PROGRAMS AND GRANTS FREE ADMISSION TO ALL STATE PARKS TO	Yes	Yes
2010	PROP 022	PROHIBITS THE STATE FROM TAKING FUNDS USED FOR TRANSPORTATION OR LOCAL GOVERNMENT PROJECTS AND SERVICES.	No	N/A
2010	PROP 023	SUSPENDS AIR POLLUTION CONTROL LAWS REQUIRING MAJOR POLLUTERS TO REPORT AND REDUCE GREENHOUSE GAS EMISSIONS THAT CAUSE GLOBAL WARMING UNTIL	No	No
2010	PROP 024	REPEALS RECENT LEGISLATION THAT WOULD ALLOW BUSINESSES TO CARRY BACK LOSSES, SHARE TAX CREDITS, AND USE A SALES-BASED INCOME CALCULATION TO	Yes	N/A
2010	PROP 025	CHANGES LEGISLATIVE VOTE REQUIREMENT TO PASS A BUDGET FROM TWO-THIRDS TO A SIMPLE MAJORITY. RETAINS TWO-THIRDS VOTE REQUIREMENT FOR TAXES.	N/A	Yes
2010	PROP 026	INCREASES LEGISLATIVE VOTE REQUIREMENT TO TWO-THIRDS FOR STATE LEVIES & CHARGES. IMPOSES ADDITIONAL REQUIREMENT FOR VOTERS TO APPROVE LOCAL LEVIES	Yes	No
2010	PROP 027	ELIMINATES STATE COMMISSION ON REDISTRICTING. CONSOLIDATES AUTHORITY FOR REDISTRICTING WITH ELECTED REPRESENTATIVES.	No	N/A
2008	PROP 001A	SAFE, RELIABLE HIGH-SPEED PASSENGER TRAIN BOND ACT.	Yes	Yes
2008	PROP 002	STANDARDS FOR CONFINING FARM ANIMALS. INITIATIVE STATUTE.	N/A	N/A
2008	PROP 003	CHILDREN'S HOSPITAL BOND ACT. GRANT PROGRAM. INITIATIVE STATUTE.	Yes	N/A
2008	PROP 004	WAITING PERIOD AND PARENTAL NOTIFICATION BEFORE TERMINATION OF MINOR'S PREGNANCY. INITIATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2008	PROP 005	NONVIOLENT DRUG OFFENSES. SENTENCING, PAROLE AND REHABILITATION. INITIATIVE STATUTE.	Yes	N/A
2008	PROP 006	POLICE AND LAW ENFORCEMENT FUNDING. CRIMINAL PENALTIES AND LAWS. INITIATIVE STATUTE.	Yes	N/A
2008	PROP 007	RENEWABLE ENERGY GENERATION. INITIATIVE STATUTE.	No	No
2008	PROP 008	ELIMINATES RIGHT OF SAME-SEX COUPLES TO MARRY. INITIATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2008	PROP 009	CRIMINAL JUSTICE SYSTEM. VICTIMS' RIGHTS. PAROLE. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2008	PROP 010	ALTERNATIVE FUEL VEHICLES AND RENEWABLE ENERGY. BONDS. INITIATIVE STATUTE.	Yes	No
2008	PROP 011	REDISTRICTING. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2008	PROP 012	VETERANS' BOND ACT OF 2008.	Yes	N/A

Year	Name	California Secretary of State Description	Big Government Sample Position	Environmental Sample Position
2006	PROP 1A	SCA 7 (RESOLUTION 49, 2006). TORLAKSON. TRANSPORTATION INVESTMENT FUND.	N/A	N/A
2006	PROP 1B	SB 1266 (CHAPTER 25, 2006). PERATA. HIGHWAY SAFETY, TRAFFIC REDUCTION, AIR QUALITY, PORT SECURITY BOND ACT	Yes	N/A
2006	PROP 1C	SB 1689 (CHAPTER 25, 2006). PERATA. HOUSING AND EMERGENCY SHELTER TRUST FUND ACT OF 2006.	Yes	Yes
2006	PROP 1D	AB 127 (CHAPTER 35, 2006). NUNEZ. EDUCATION FACILITIES: KINDERGARTEN UNIVERSITY PUBLIC EDUCATION FACILITIES BOND ACT OF 2006.	Yes	N/A
2006	PROP 1E	AB 140 (CHAPTER 33, 2006). NUNEZ. DISASTER PREPAREDNESS AND FLOOD PREVENTION BOND ACT OF 2006.	Yes	N/A
2006	PROP 83	SEX OFFENDERS. SEXUALLY VIOLENT PREDATORS. PUNISHMENT, RESIDENCE RESTRICTIONS AND MONITORING. INITIATIVE STATUTE.	N/A	N/A
2006	PROP 84	WATER QUALITY, SAFETY AND SUPPLY. FLOOD CONTROL. NATURAL RESOURCE PROTECTION. PARK IMPROVEMENTS. BONDS. INITIATIVE STATUTE.	Yes	Yes
2006	PROP 85	WAITING PERIOD AND PARENTAL NOTIFICATION BEFORE TERMINATION OF MINOR'S PREGNANCY. INITIATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2006	PROP 86	TAX ON CIGARETTES. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	Yes	N/A
2006	PROP 87	ALTERNATIVE ENERGY. RESEARCH, PRODUCTION, INCENTIVES. TAX ON CALIFORNIA OIL. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	Yes	Yes
2006	PROP 88	EDUCATION FUNDING. REAL PROPERTY PARCEL TAX. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	Yes	N/A
2006	PROP 89	POLITICAL CAMPAIGNS. PUBLIC FINANCING. CORPORATE TAX INCREASE. CONTRIBUTION AND EXPENDITURE LIMITS. INITIATIVE STATUTE.	N/A	N/A
2006	PROP 90	GOVERNMENT ACQUISITION, REGULATION OF PRIVATE PROPERTY. INITIATIVE CONSTITUTIONAL AMENDMENT.	N/A	No
2004	PROP 1A	SCA 4 (RESOLUTION CHAPTER 133, STATUTES OF 2004). TORLAKSON. PROTECTION OF LOCAL GOVERNMENT REVENUES.	N/A	N/A
2004	PROP 59	SCA 1 (RESOLUTION CHAPTER 1, STATUTES OF 2004). BURTON. ACCESS TO GOVERNMENT INFORMATION	N/A	N/A
2004	PROP 60	SCA 18 (RESOLUTION CHAPTER 103, 2004). JOHNSON. ELECTION RIGHTS OF POLITICAL PARTIES. SURPLUS PROPERTY. LEGISLATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2004	PROP 60A	SCA (RESOLUTION CHAPTER 103, 2004). JOHNSON. SURPLUS PROPERTY. LEGISLATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2004	PROP 61	CHILDREN'S HOSPITAL PROJECTS. GRANT PROGRAM. BOND ACT. INITIATIVE STATUTE.	Yes	N/A
2004	PROP 62	ELECTIONS. PRIMARIES. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2004	PROP 63	MENTAL HEALTH SERVICES EXPANSION AND FUNDING. TAX ON INCOMES OVER $1 MILLION. INITIATIVE STATUTE.	Yes	N/A
2004	PROP 64	LIMITATIONS ON ENFORCEMENT OF UNFAIR BUSINESS COMPETITION LAWS. INITIATIVE STATUTE	N/A	No
2004	PROP 65	LOCAL GOVERNMENT FUNDS AND REVENUES. STATE MANDATES. INITIATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2004	PROP 66	LIMITATIONS ON "THREE STRIKES" LAW. SEX CRIMES. PUNISHMENT. INITIATIVE STATUTE.	N/A	N/A
2004	PROP 67	EMERGENCY AND MEDICAL SERVICES. FUNDING. TELEPHONE SURCHAGE. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	Yes	N/A
2004	PROP 68	TRIBAL GAMING COMPACT RENEGOTIATION. NON-TRIBAL COMMERCIAL GAMBLING EXPANSION. REVENUES, TAX EXEMPTIONS. INITIATIVE CONSTITUTIONAL AMENDMENT	N/A	No
2004	PROP 69	DNA SAMPLES. COLLECTION. DATABASE. FUNDING. INITIATIVE STATUTE.	N/A	N/A
2004	PROP 70	TRIBAL GAMING COMPACTS. EXCLUSIVE GAMING RIGHTS. CONTRIBUTIONS TO STATE. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2004	PROP 71	STEM CELL RESEARCH. FUNDING. BONDS. INITIATIVE CONSTITUTIONAL AMENDMENT AND STATUTE.	N/A	N/A
2004	PROP 72	REFERENDUM PETITION TO OVERTURN AMENDMENTS TO HEALTH CARE COVERAGE REQUIREMENTS.	N/A	N/A
2002	PROP 46	HOUSING AND EMERGENCY SHELTER TRUST FUND ACT OF 2002.	Yes	N/A
2002	PROP 47	KINDERGARTEN-UNIVERSITY PUBLIC EDUCATION FACILITIES BOND ACT OF 2002.	Yes	N/A
2002	PROP 48	COURT CONSOLIDATION. LEGISLATIVE CONSTITUTIONAL AMENDMENT.	N/A	N/A
2002	PROP 49	BEFORE AND AFTER SCHOOL PROGRAMS. STATE GRANTS. INITIATIVE STATUTE.	Yes	N/A
2002	PROP 50	WATER QUALITY, SUPPLY AND SAFE DRINKING WATER PROJECTS. COASTAL WETLANDS PURCHASE AND PROTECTION. BONDS. INITIATIVE STATUTE.	Yes	Yes
2002	PROP 51	TRANSPORTATION. DISTRIBUTION OF EXISTING MOTOR VEHICLE SALES AND USE TAX. INITIATIVE STATUTE.	N/A	N/A
2002	PROP 52	ELECTION DAY VOTER REGISTRATION. VOTER FRAUD PENALTIES. INITATIVE STATUTE.	N/A	Yes

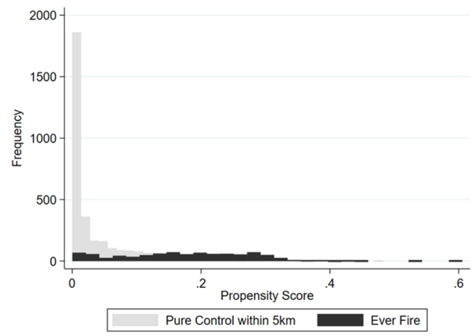
A.2 Propensity Score Approach

While the analysis passes the falsification test, it is clear from Table 1.1 and Figures 1.4 and 1.5 that ever-treated areas are demographically and ideologically different from pure controls. Block groups which experience a fire are have on average a higher share of Republican voters, lower support for the initiatives in question, have more white voters, are wealthier, and are less urban. To account these differences, I present a propensity score approach to first balance the treatment and control areas along observable dimensions. I first run a logit regression that predicts ever-treated areas as a function of the following characteristics: Share Democratic voters; change in share Democratic voters from sample start to sample end; per capita income; share white residents; share black residents; share of homeownership; and population density. The distribution of the propensity score shown in Figure A.2.1 is as expected- a mass of highly urbanized pure control areas has low propensity for treatment, while ever-treated block groups are represented across propensity score distribution.

Figure A.2.1: Propensity Score Distributions, Control and Ever-Treated Block Groups



(a)



(b)

Count of block groups over the distribution of propensity scores. Full sample at left (a), pure controls limited to within 5 kilometers of a fire at right (b).

Matching on the propensity score eliminates significant differences in observable characteristics between ever treated and control units:

Table A.2.1: Summary Statistics, Before and After Propensity Score Matching

	Treated =0 (unmatched)	Treated =1 (unmatched)	T-test	Treated =0 (matched)	Treated =1 (matched)	T-test
Change in Share Democrats	-2.08	-2.20	0.50	-2.02	-2.20	0.50
	(7.01)	(7.09)		(7.25)	(6.93)	
Share Democrats	46.53	33.41	29.54	33.27	33.39	-0.26
	(13.12)	(9.27)		(9.32)	(9.27)	
Per Capita Income	30523.44	37365.16	-10.32	35998.43	37410.47	-1.49
	(19373.72)	(18933.89)		(19234.97)	(18928.27)	
Share White	58.78	78.16	-27.26	78.56	78.39	0.26
	(21.05)	(13.45)		(14.02)	(13.00)	
Share Owned Housing	39.21	49.86	-2.98	49.20	50.05	-0.42
	(106.54)	(39.10)		(41.57)	(39.15)	
Share Black	6.25	2.31	11.21	2.39	2.29	0.55
	(10.46)	(3.80)		(3.50)	(3.69)	
Population Density	3538.04	412.54	23.73	522.46	416.50	2.97
	(3932.48)	(708.67)		(723.11)	(711.17)	

Summary statistics and balance tests between ever-treated and control block groups, before and after propensity score matching.

The effect of fire exposure, although smaller in magnitude, remains positive and significant for both big government and pro-environmental voting in the propensity score matched sample:

Table A.2.2: Effect of Large Fires, Propensity Score Matched Sample

	(1)	(2)	(3)	(4)
VARIABLES	Big Gov	Env Share	Dem_Reg	Turnout
Fire Treatment	0.294**	0.520***	0.0844	0.195
	(0.118)	(0.145)	(0.0975)	(0.184)
Observations	15,875	15,875	15,878	15,845
R-squared	0.893	0.895	0.939	0.930

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Propensity score matched sample. Fire_Treatment is an indicator equal to 1 if the block group experiences a large fire in the two years preceding the biennial election. The outcome variable is expressed in percentage points. Column 1 reports the effect on the vote share for initiatives that expand government. Column 2 reports the effect on environmental interest group-endorsed initiatives. Columns 3 and 4 report the effect on Democratic registrations and voter turnout. Regression includes block group fixed effects and county-by-election-year fixed effects. Robust standard errors are clustered at the block group level.

Appendix B Crime and Demand for Police: List of Ballot Initiatives

Begins next page.

Year	Election Type	Proposition	Voter Guide Description	SFPOA-Endorsed Position
2003	Municipal	B	Retirement Benefits for Safety Employees	Yes
2003	Municipal	H	Police Commission/Office of Citizen Complaints	No
2003	Municipal	K	Sales Tax for Transportation	Yes
2003	Municipal	M	Aggressive Solicitation Ban	Yes
2004	Primary	J	Incentive to Build Below-Market-Rate Housing	Yes
2004	General	E	Police and Fire Survivor Benefits	Yes
2004	General	H	Naming the Stadium at Candlestick Point	No
2004	General	J	Sales Tax Increase	Yes
2004	General	K	Business Tax	Yes
2005	Municipal	F	Neighborhood Firehouses	Yes
2007	Municipal	F	Authorizing the Board of Supervisors to Amend Contract for Retirement Benefits for Police Department Employees Who Were Airport Police Officers	Yes
2008	Primary (February)	B	Creating a New Deferred Retirement Option Program for Members of the San Francisco Police Department	Yes
2008	Primary (June)	B	Changing Qualifications for Retiree Health and Pension Benefits and Establishing a Retiree Health Care Trust Fund	Yes
2008	General	A	San Francisco General Hospital and Trauma Center Earthquake Safety Bonds, 2008	Yes
2008	General	G	Allowing Retirement System Credit for Unpaid Parental Leave	Yes
2008	General	H	Setting Clean Energy Deadlines; Studying Options for Providing Energy: Changing Revenue Bond Authority to Pay for Public Utility Facilities	No
2008	General	N	Changing Real Property Transfer Tax Rates	Yes
2008	General	O	Replacing the Emergency Response Fee with an Access Line Tax and Revising the Telephone Users Tax	Yes
2008	General	Q	Modifying the Payroll Expense Tax	Yes
2008	General	V	Policy Against Terminating Junior Reserve Officers' Training Corps (JROTC) Programs in High Schools	Yes
2010	Primary	B	Earthquake Safety and Emergency Response Bond	Yes
2010	General	B	City Retirement and Health Plans	No
2010	General	L	Sitting or Lying on Sidewalks	Yes
2010	General	M	Community Policing and Foot Patrols	No
2011	Municipal	C	City Pension and Healthcare Benefits	Yes
2011	Municipal	D	City Pension Benefits	No
2012	General	B	Clean and Safe Neighborhood Parks Bond	Yes
2012	General	E	Gross Receipts Tax	Yes
2013	Municipal	A	Retiree Health Care Trust Fund	Yes
2014	Primary	B	Voter Approval for Waterfront	No
2014	General	F	Pier 70	Yes
2015	Municipal	A	Affordable Housing Bond	Yes
2015	Municipal	D	Mission Rock	Yes
2015	Municipal	F	Short-Term Residential Rentals	No
2015	Municipal	I	Suspension of Market-Rate Development in the Mission District	No
2016	General	Q	Prohibiting Tents on Public Sidewalks	Yes
2018	Primary	H	Policy for the Use of Tasers by San Francisco Police Officers	Yes

Appendix C The Value of Piped Water and Sewers: Evidence from 19th Century Chicago

C.1 Supplementary Results

Begins next page.

Table C.1.1: Summary Statistics 1886-1889

	(1)	(2)	(3)
	SW$\triangle = 1$	SW$\triangle = 0$	t-test
Share Sewered	1.00	1.00	.
	(0.00)	(0.00)	
Log Price	8.35	8.56	1.56
	(0.94)	(0.78)	
Log Distance to CBD	9.08	8.98	-1.46
	(0.35)	(0.48)	
Log Area	8.29	8.19	-0.99
	(0.67)	(0.51)	
Share Improved	0.22	0.15	-1.11
	(0.42)	(0.36)	
Share Corner	0.09	0.10	0.34
	(0.29)	(0.31)	
Distance to Horsecar	751	374	-5.50
	(527)	(314)	
Distance to Major Street	512	438	-1.11
	(431)	(390)	
Year	1887.19	1887.35	0.95
	(0.95)	(1.07)	
Observations	68	86	

Means and standard deviations of parcel characteristics. Column 1 reports on parcels in the Quasi-experimental sample (within 2000' of Tyler St. west of Halsted) that are in the Southwest Triangle (south of Tyler Street). Column 2 presents corresponding values for parcels that are not in the Southwest Triangle (i.e., north of Tyler Street). Column 3 reports the t-statistic for the difference between the first two columns.

Table C.1.2: Reduced Form Regressions After Completion of Water and Sewer Network

	(1)	(2)	(3)	(4)	(5)	(6)
Reduced Form: ln(Price)						
SW $\triangle = 1$	-.174	-.233***	.165	-.183*	-.146	-.164*
	(.119)	(.096)	(.225)	(.105)	(.1)	(.09)
Miles to Boundary			1.03			
			(.539)			
R^2	0.364	0.580	0.590	0.598	0.330	0.454
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y
Improved and Corner		Y	Y	Y		Y
Horsecar and Major Street				Y		
Sample	Q.E.	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.
Observations	143	143	143	143	213	213

All results based on transactions during 1886-9. Columns 1-4 rely on the Quasi-experimental area, 5 and 6 on the Extended-quasi-experimental area. Regressions are reduced form regressions of log transaction price on the instrument and, in column (3), distance to the Tyler Street. Bottom panel of the table indicates control variables. Unlike the 1874-80 period, the entire Southwest Triangle has piped water and sewer access by 1886-9 and the price difference across the Tyler Street boundary is small economically and statistically. Robust standard errors in parentheses. $*, **, ***$ indicate 10%, 5%, 1% significance.

Table C.1.3: (a) LIV Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage
Z	3.95***		4.08***		5.55***		2.76***		2.74***	
	(.49)		(.52)		(.76)		(.36)		(.36)	
ln(Area)	-.08	.72***	.01	.63***	-.02	.63***	-.34	.72***	-.33	.67***
	(.29)	(.22)	(.33)	(.21)	(.35)	(.21)	(.23)	(.2)	(.25)	(.2)
1(Year = 1875)	.56	.45**	.6	.42**	.57	.35*	.21	.38*	.24	.42*
	(.64)	(.2)	(.65)	(.19)	(.72)	(.19)	(.54)	(.23)	(.53)	(.22)
1(Year = 1876)	.95	.39	.99	.37	.89	.29	.42	.35	.44	.38
	(.66)	(.26)	(.68)	(.27)	(.75)	(.28)	(.54)	(.32)	(.54)	(.31)
1(Year = 1877)	1.41*	.52	1.59**	.58	1.73**	.47	1*	.42	.89	.38
	(.72)	(.36)	(.74)	(.39)	(.8)	(.38)	(.57)	(.37)	(.58)	(.33)
1(Year = 1878)	3.06***	.32	3.31***	.38	3.6***	.23	1.58***	.29	1.38**	.21
	(.83)	(.43)	(.89)	(.44)	(.93)	(.38)	(.66)	(.5)	(.69)	(.43)
1(Year = 1879)	2.45***	-.08	2.66***	.03	2.86***	-.03	1.15**	-.38	1.05*	-.27
	(.73)	(.49)	(.76)	(.44)	(.81)	(.49)	(.56)	(.58)	(.57)	(.53)
1(Year = 1880)	3.65***	-.63	3.86***	-.26	4.09***	-.59	2.72***	-1.54	2.6***	-1.21
	(.71)	(.63)	(.75)	(.51)	(.79)	(.57)	(.53)	(.94)	(.54)	(.74)
ln(mi. CBD)	-5.83***	.31	-5.93***	.03	-8.3***	.09	-5.41***	.85	-5.38***	1.2
	(.91)	(.64)	(.93)	(.57)	(1.32)	(.58)	(.71)	(.79)	(.71)	(.76)
1(Improved)			-.6	.43	-.7	.51			.66	.52
			(.63)	(.52)	(.64)	(.46)			(.5)	(.66)
1(Corner)			-.52	.53*	-.6	.43			.12	.35
			(.64)	(.29)	(.7)	(.29)			(.49)	(.34)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Improved and Corner			Y	Y	Y	Y			Y	Y
Horsecar and Major Street					Y	Y				
Sample	Q.E.	Q.E.	Q.E.	Q.E.	Q.E.	Q.E.	E.Q.E.	E.Q.E.	E.Q.E.	E.Q.E.
Observations	351	351	351	351	351	351	533	533	533	533

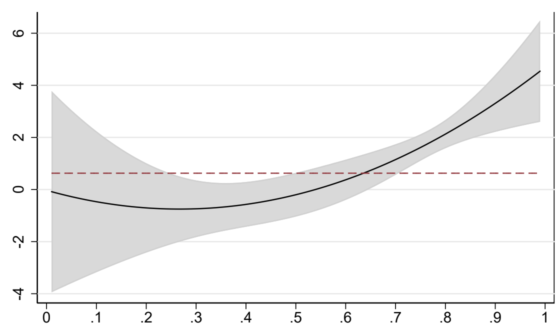
Table continued next page

Table C.1.4: (b) LIV Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage
$\widehat{p}$		.74		1.21		1.3		2.39		3.59
		(2.84)		(2.73)		(2.8)		(2.91)		(2.92)
$\widehat{p}^2$		-3.56		-3.04		-2.74		-.94		-1.71
		(4.83)		(4.41)		(4.23)		(4.51)		(4.1)
$\widehat{p}^3$		3.81		3.65		3.26		1.05		1.59
		(3.03)		(2.77)		(2.62)		(2.72)		(2.5)
$\widehat{p}\,\ln(\text{Area})$		-.1		.02		.02		.09		.16
		(.23)		(.23)		(.22)		(.23)		(.23)
$\widehat{p}1(\text{Year} = 1875)$		-.97***		-.93***		-.77***		-.66*		-.69*
		(.33)		(.32)		(.29)		(.37)		(.36)
$\widehat{p}1(\text{Year} = 1876)$		-.64*		-.6		-.39		-.35		-.38
		(.39)		(.4)		(.38)		(.46)		(.46)
$\widehat{p}1(\text{Year} = 1877)$		-1.4***		-1.66***		-1.4***		-.93*		-1.02**
		(.54)		(.56)		(.49)		(.5)		(.46)
$\widehat{p}1(\text{Year} = 1878)$		-1.24**		-1.58***		-1.18***		-1.04*		-1.19**
		(.54)		(.55)		(.44)		(.6)		(.53)
$\widehat{p}1(\text{Year} = 1879)$		-1.09*		-1.43***		-1.17**		-.36		-.64
		(.59)		(.54)		(.55)		(.67)		(.61)
$\widehat{p}1(\text{Year} = 1880)$		-.51		-1.2*		-.62		.78		.21
		(.72)		(.62)		(.62)		(1.01)		(.83)
$\widehat{p}\,\ln(\text{mi. CBD})$		-.11		.14		.07		-.57		-.92
		(.68)		(.61)		(.62)		(.85)		(.81)
$\widehat{p}1(\text{Improved})$				.38		.28				0
				(.56)		(.51)				(.69)
$\widehat{p}1(\text{Corner})$				-.14		-.01				-.05
				(.36)		(.34)				(.39)
Year FE & ln(Area)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
ln(mi. CBD)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Improved and Corner			Y	Y	Y	Y			Y	Y
Horsecar and Major Street					Y	Y				
Sample		Q.E.		Q.E.		Q.E.		E.Q.E.		E.Q.E.
Observations		351		351		351		533		533

Estimates of the LIV model of equation (3.3). Odd columns are Logit first stage coefficients and even columns are corresponding second stages. Specifications and samples match those reported in the same columns of table 3.3. Bottom panel indicates controls for the regression above. Bootstrapped standard errors in parentheses. $*, **, ***$ indicate 10%, 5%, 1% significance.

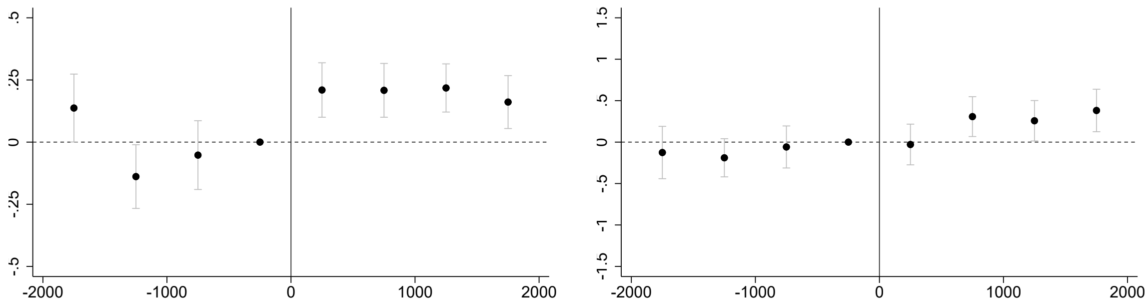
Figure C.1.1: Marginal Treatment Effect as a Function of $\widetilde{U}_D$



Marginal Treatment Effects

Expected MTE as a function of $\widetilde{U}_D$. Dashed line shows ATE for this sample/specification and sample average X's. Based on column 2 of Table 3.3.

Figure C.1.2: Incidence and Land Price by Distance to Boundary, 1874-80, Extended Sample



(a) (b)

(a) Share of parcels sewered 1874-80 by 500' bins of distance to SW△ boundary, $x < 0$ is "inside". $x \in [-500, 0]$ is y intercept. Conditional on year, ln(area), ln(mi. to CBD). (b) Same as left panel but y-axis is ln(Price).

C.2 Derivation of Equation 3.18

We maintain the MTE model with semiparametric potential outcome equations introduced in the main text; see (3.1) in the main text. We also maintain the key restriction of practical exogeneity; see (3.2) in the main text. With propensity score $p = F(x, z) = P(D = 1|X = x, Z = z)$ introduced in the main text and the normalized unobserved heterogeneity in the selection process, $\widetilde{U}_D \sim Unif[0, 1]$, the selection equation can be represented as

$$D = 1\{\widetilde{U}_D \leq F(X, Z)\}. \tag{C.1}$$

Under the cubic polynomial specification of the control function $K(p)$ in (3.3), MTE at each conditioning covariate value X and $\widetilde{U}_D \in [0, 1]$ is given as in (3.4), and averaging $(X, \widetilde{U}_D)$ for the population of the Quasi-experimental sample leads to ATE in the Quasi-experimental sample (3.5).

Our interest is to obtain an estimate for ATE for the population of the Relevant sample P^* as denoted by ATE^* in the main text. We assume that a unit in the Relevant sample admits the same structural equations (3.6) with the same parameter values as a unit in the Quasi-experimental sample. Importantly, even though we assume that a binary cost shifter Z^* is present and measures the cost of access to sewage in the same scale for each unit in the Relevant as in the Quasi-experimental sample, Z^* is not observed for any unit of the Relevant sample. In addition, unlike in the Quasi-experimental sample, Z^* need not be randomly assigned and the analogue of the instrument exogeneity assumption $Z^* \perp (U_1^*, U_0^*, U_D^*)$ may fail in P^*.

The following assumption describes what is necessary, and what is not, for feasible extrapolation from P to P^*.

Assumption EX: (The relationship between P and P^*)

1. The equations of potential outcomes and selection given in (3.1) are identical between the Quasi-experimental and Relevant samples (other than that Z^* is not observed in P^*). Furthermore, the distributions of (U_1, U_0, U_D) and (U_1^*, U_0^*, U_D^*) are common.
2. The joint distribution of observable covariates X and cost shifter (instrument) Z in the Quasi-experimental sample and the joint distribution of X^* and Z^* in the Relevant sample can be different.

Under (EX1), we can normalize U_D^* of (3.6) to define the uniform random variable $\widetilde{U}_D^* = F_{U_D^*}(U_D^*)$ such that for $\widetilde{U}_D$ defined in (C.1), $\widetilde{U}_D^* = \widetilde{U}_D$ is equivalent to $U_D^* = U_D$. In other words, a unit in the Relevant sample and a unit in the Quasi-experimental sample that share the values of $\widetilde{U}_D^*$ and $\widetilde{U}_D$ have identical unobservables in the selection equation. Assumption EX1 also implies that the control function term $K(\cdot)$ in the LIV regression (3.3) is common between the two samples, because the control function term is determined only by the distribution of $(U_1, U_0)|U_D$ and this does not vary between the two samples. As a result, for MTE in the Relevant sample $MTE^*(X^*, \widetilde{U}_D^*)$, $MTE(X, \widetilde{U}_D) = MTE^*(X, \widetilde{U}_D^*)$ holds whenever $X = X^*$ and $\widetilde{U}_D = \widetilde{U}_D^*$ hold. We hence obtain

$$MTE^*(X^*, \widetilde{U}_D^*) = (X^*)'(\delta_1 - \delta_0) + \gamma_1 + 2\gamma_2 \widetilde{U}_D^* + 3\gamma_3 \widetilde{U}_D^{*\,2}. \tag{C.2}$$

Taking the expectation with respect to X^* and $\widetilde{U}_D^* \sim Unif[0,1]$, we obtain equation of (3.8) in the main text, where $E(X^*)$ is directly identified by the data of the Relevant sample. Note that this argument does not require Z^* to be independent of the unobservables (U_1^*, U_0^*, U_D^*).

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