AN ABSTRACT OF THE DISSERTATION OF

Alexander David Natanson for the degree of Doctor of Philosophy in Applied Economics presented on June 9, 2022.

Title: Applied Economic Strategies to Evaluate Policies and Environmental Outcomes: Case Studies from Immigration and Ecosystem Services

Abstract approved: ____

Jennifer Alix-Garcia

My dissertation title is, “Applied economic strategies to evaluate policies and environmental outcomes: case studies from immigration and ecosystem services.” The first two chapters of this dissertation concern the relationship between local immigration policies and societal welfare. The third chapter examines the impact of human activity on the environment and the valuation of ecosystem services. The first two chapters examine the outcomes accompanying the institutional adoption of inclusive and exclusive immigration policies. I am studying sanctuary cities as an example of inclusive institutions, where I seek to understand the benefits that inclusivity confers on cities, as well as the costs of exclusive approaches to immigration. In these studies, I test whether economic outcomes vary according to a county's openness to immigrants and determine whether these economic indicators drive the choice of immigration policy. To the extent that sanctuary cities exemplify inclusivity, sanctuary cities' economic characteristics may inform the transition of other institutions to greater inclusivity. The third chapter documents the economic consequences of eutrophication in one of North America's Great Lakes. It also seeks to quantify the broader value of the ecosystem services the lake provides using revealed preference methods. This strategy may, in turn, influence decision-making and incentivize conservation.

The first manuscript exploits quasi-experimental variation in the time and space of policy implementation to isolate the effects of local immigration policies on U.S. counties. To assess the

impact of a county's openness to immigrants on the local economy, I use county-level data from the American Community Survey on economic indicators and Immigration and Customs Enforcement (ICE) data detailing policy intensity. These policies range from areas where immigrants are strictly regulated via collaboration with ICE compared to those that provide protections. The study finds evidence that providing protections to immigrants increases overall per capita income, wages, GDP, and total employment, while unemployment experienced a decline. Meanwhile, the data show that punitive measures have no statistically significant effect on income and unemployment but adverse effects overall on GDP, total employment, and the proportion of the foreign-born population. These results support a model of immigration policy as an institution that can either support or suppress productivity. They confirm that immigrant labor is a positive driver of economic well-being at the local and regional levels.

The second manuscript builds upon the first study data to determine whether the selection of pro- or anti-immigrant policies is consistent with the narrative that economic conditions drive the choice of immigration policy. While also exploiting the variation in economic indicators across time and space to isolate the effects of these indicators on immigration policy choice by counties, this study finds no correlations between included economic variables and selecting immigrant-friendly policies. When using cross-sectional variation, the probability of choosing anti-immigrant policies increases with unemployment, non-citizen population, voting for a Republican president, and wages decline. All correlations between economic variables and policy outcomes disappear once fixed effects at the county level are included. This suggests that policy choice has a stronger relationship with longer-term trends and unobservables. Hence, the policy choice is not driven by short-term fluctuations in per capita income, unemployment, wages, and immigrant presence.

In the third manuscript, I examine the economic consequences of eutrophication in one of North America's Great Lakes by measuring whether property values decline due to harmful algal blooms in the urban and suburban areas of the city of Toledo, Ohio. I quantify the broader value of the ecosystem services that the lake provides. With housing sales from around the city of Cleveland, Ohio as a control group, I apply a difference-in-difference model to pre-matched data on housing prices, control for census tract as well as time fixed effects, and in one of the specifications, limit the data to houses sold repeatedly over the study period. Pre-trend tests suggest valid counterfactuals. Estimates show a decline in house prices of 13 and 25 percent in 2008 and 2011, respectively, and comparisons between locations near and far from HABs in Toledo show 9 and 24 percent declines in 2008 and 2011. Results imply an overall loss due to HABs, totaling close to 3 billion dollars during the study period.

Three Essays on Applied Economics on the Valuation of Ecosystem Cervices and the Economic Outcome of Local Immigration Policies

By
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APPROVED:

Major Professor, representing Applied Economics

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I understand that my dissertation will become part of the permanent collection of Oregon State University libraries. My signature below authorizes release of my dissertation to any reader upon request.

Alexander David Natanson, Author

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My priorities in life are shaped around people that I love, learning from life's experiences, and having a positive impact on the world. These priorities have shaped how I see the world and choose to experience it. Therefore, in the tree of my life, there are people I would like to recognize who unknowingly led me to this trajectory before and during my doctoral studies. Starting in my years at Orange Coast College, Azareen Van der Vliet Oloomi taught me to love learning and introduced me to the fascinating world of literature. Juan Carlos Contreras inspired me to have further curiosity about social issues and spirituality. My uncle Ricky Rocca taught me what it is like to be an awesome dad. In an English as a Second Language class and later the Freshmen English Composition class, Professor Donavan Gaytan helped me find the beauty in writing. My dear friends Kristin Shelton and Sara Travison, from my first ESL class to my Ph.D., were the faithful and loving editors of my complicated and confusing papers, always with much joy, many laughs, cigarettes, and wine. At UC Santa Cruz, Professor Alan Richards gave me a purpose for my career after taking his inspiring "Ecological Economics" and "Blood and Oil" classes. Katherine Hagan taught me the power of words and much more. Most recently, Katie Sue Kallal taught me that the little actions and things in life are the ones that matter the most.

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1. General introduction

This dissertation research concerns, first, the ways in which inclusive policies on the part of institutions can address social and society-level challenges. Second, it examines the value of a clean environment.

Narratives around immigration in the US suggest that it comes at a cost by driving down wages for native workers and straining the national economy through the costs associated with social services. For the same reason, economic studies are often interested in measuring the impacts of immigration shocks on economic outcomes. However, identifying these impacts also depends upon understanding what drives the decision to choose immigration policies. Therefore, in the first two chapters, I first test whether economic outcomes vary according to a county's openness to immigrants as measured by their policy choice. Second, I determine whether these economic indicators drive the selection of immigration policy. In doing so, I examine the outcomes accompanying the adoption of inclusive and exclusive policies. Ultimately, in the first manuscript, I am studying sanctuary cities as an example of inclusive institutions, where I seek to understand the benefits that inclusivity and diversity confer on cities. The second manuscript aims to determine whether the selection of pro- or anti-immigrant policies is consistent with the narrative that economic conditions drive the choice of immigration policy.

Results of the first manuscript endorse a model of immigration policy as an institution that can either support or suppress productivity. They confirm that immigrant labor is a positive driver of economic well-being at the local and regional levels. However, if the economic factors influence immigration policy preferences in this paper, we will inevitably find a reverse causality problem. Hence, the second manuscript strengthens the results of the first paper by finding that the choice of immigration policies is uncorrelated with economic indicators. Results indicate that the year-to-year fluctuations in per capita income, wages, and unemployment seem independent of an immigration policy selection. The choice appears to be more likely to reflect long-term preferences toward immigration that are distinct and rigid at each location, even though economic anxiety and narratives may shape feelings about immigration. These results are robust given that no existing study uses a data set comparable in scope on two fronts. First, all the existing studies focus only on punitive measurement, while this paper adds to the current literature by examining inclusive and exclusive local immigration policies simultaneously. Second, the comprehensive data set in the present study accounts for 85.1% of the U.S. population by the end of our study period.

The third chapter documents the economic consequences of eutrophication in one of North America's Great Lakes. It also seeks to quantify the broader value of lakes' ecosystem services in quantifiable and economic terms using revealed preference methods. These results may, in turn, influence decision-making and incentivize conservation. Eutrophication has reemerged as a significant water quality issue in Lake Erie. Policy changes successfully reduced phosphorus inputs from regulated point sources. However, the amount of phosphorus entering Lake Erie from unregulated nonpoint sources such as residential and urban, but mainly agricultural and cattle runoff now exceeds by far the amount discharged from regulated point sources. The nutrient loading has led to annual summertime explosions of cyanobacteria growth concentrated throughout the Western Lake Erie Basin. They are malodorous, unattractive, and dangerous blooms that negatively impact a broad range of the ecosystem services provided by Lake Erie.

This study's ultimate goals are to contribute to methodologies for the economic valuation of ecosystem services and to provide an assessment of the damages caused by HABs in Lake Erie. It differs from previous studies by introducing a robust two-stage scenario to identify the housing areas close to the pollution source but unaffected by HABS. The approach detects water quality-related damages to ecosystem services in the absence of water quality measurements and limits the data to houses sold repeatedly over the study period. This is crucial to quantify the value of ecosystem services, not in order to commodify nature but to give currency to natural resources in settings where such resources are either invisible or assumed to be inexhaustible.

2. Economic Impacts of Sanctuary and ICE Policies Inclusive and Exclusive Institutions

2.1 Introduction

Opinions on the economic effects of immigration are political and controversial. Economic studies generally concentrate on the negative aspect of increasing the supply of labor, thus excluding native workers, and in politics fear drives the narrative. This paper focuses on the effect of sanctuary and ICE policies on local economies. "Sanctuary cities" refers to municipal jurisdictions that limit their cooperation with the federal government efforts to enforce immigration. Opponents of sanctuary policies allege that they come at an economic cost by arguing that they drive down wages for native workers and strain taxpayers and the national budget through immigrant utilization of social services. In contrast, proponents of sanctuary policies argue that anti-immigrant policies (ICE policies) only harm immigrant rights through surveillance and the threat of deportation because immigrants only respond to the availability of jobs (Harris 2006).

This paper examines the local effects of policy towards immigrants on economic outcomes. The investigation is accomplished by simultaneously studying the policies' effects in counties where immigrants' families are persecuted via collaboration with Immigration and Customs Enforcement (ICE), in contrast to counties that provide protections. To assess the impact of a county's openness to immigrants on the local economy, I use U.S. American Community Survey data on income, GDP, unemployment, and employment combined with newly digitized information on county-level immigration policies from 2006 to 2018. The econometric approach uses quasi-experimental variation in adopting policies that are both welcoming and restricting to undocumented immigrants. The circumstances for analysis create a staggered difference in difference environment. The analysis includes fixed effects, time-variant covariates, and time trends. Results are robust to nearest-neighbor matching, random assignment of treatment, and a regression discontinuity model comparing bordering counties with opposite policies.

I begin by classifying all counties by sanctuary, ICE, or neutral counties. Then, I estimate the impact of policies comparing sanctuary and ICE counties to neutral counties for each year in the data. In separate regressions using only those counties that ever end up with a sanctuary city or ICE designation, I restrict the sample to counties that ever chose to adopt either policy. This approach uses the variation in policy timing to address the possibility that sanctuary counties might be fundamentally different from non-sanctuary counties or ICE counties might be different from non-ICE counties. I also examine heterogeneity by urban, rural, educational attainment, gender, white, black, Latino population, and economic quintiles. Pre-trends suggest that the counterfactual groups in each setting are plausible, and various robustness checks confirm the results. Finally, I repeat this analysis using a geographical regression discontinuity model with counties that share a common border with opposing policies.

The evidence demonstrates that providing protections to undocumented immigrants increases economic activity. The estimates show increases in per capita income ranging from 3.1 to 7.2, median wages between 1.7 to 2.6, and GDP between 2.4 to 4.1 percent. In terms of labor, sanctuary counties saw increases in total employment between 2.3 to 4 percent, and the decline in unemployment rate ranged from 12 to 17 percent. The data further shows that punitive measures have no statistically significant effects on income, median wages, or GDP, but adverse effects on total employment with declines from 1 to 2 percent, mostly in rural counties, and an increase in unemployment of around 7 percent in urban counties. In addition, I find a decline in the foreign-born population in ICE counties, but no changes in sanctuary counties. The study also finds similar results for sanctuary counties when separating the data between urban, rural, educational attainment, gender, ethnic groups, and economic quintiles. Meanwhile, most ICE counties show no significant effects except for the foreign-born population who appear to leave these areas.

To summarize, inclusive policies show positive effects on economic outcomes with no evident increase in population. In order to make sense of these results, I propose that inclusive immigration policies play an essential role in conditioning the effect of immigration by decreasing uncertainties and constraints for immigrants' interaction in their communities. By doing so, policies reduce the cost from fear of deportation or the constant fear of criminalization, optimize their human capital, and increase efficiency in the economy.

Studies on the economic effects of immigration in local economies have mainly concentrated on the effects on wages and income for native workers. Evidence on the impact of immigration on the U.S. economy is mixed. Theory suggests that an increase in the labor supply will increase total employment, decrease wages, and increase unemployment. Empirical evidence shows lower wages for certain subgroups, including high school dropouts (Borjas 2003, 2006) and workers in the below the 20th percentile (Dustmann et al. 2013),While others show no impact (Card 1990, Lalonde and Topel 1991). Studies on the economic effects of local immigration policies, such as this paper, are scarce. All the existing studies focus only on punitive institutions (to my knowledge). Bohn et al. (2017), examine the effect of 287(g) policy on employment and wages, and found no effect on all industries combined. 287(g) is an ICE immigration policy which turns local police into immigration agents, and it is included in this study.

This paper also contributes to the discussion around the mechanisms through which immigration could increase productivity. The general equilibrium narratives suggest that as immigrants increase diversity and consumption, they supply work that natives are less willing to supply and provide a renovated entrepreneurial spirit. This view suggests that immigration to the

United States is associated with economic development due to productivity growth (Peri 2012, Model 2008). Further, immigration impacts productivity per worker because migrant skills often complement the existing populations. Immigration increases the percentage of working-age people in a country because migrants tend to fall within this age bracket and increase the employment to working-age population ratio (Jaumotte et al. 2016). The immigrant advantage is also explained by the circumstances of migration because not all people migrate; instead, only individuals who self-select themselves due to their exceptional internal drive for success, resilience, and resourcefulness (Model 2008, Borjas 1987, Bencivenga et al. 1997).

When studies look at productivity growth given local immigration policies, Ifft et al. (2017) found that after the 287(g) implementation, farms experienced statistically significant increases in labor and fuel expenses, while adjacent counties experienced lower costs. Hence, the 287(g) policy is driving a decline in farm profitability in implemented counties, while adjacent counties benefit. Likewise, Pham et al. (2010) found that local anti-immigration laws reduce employment from 1 to 2 percent and a payroll drop between 0.8 to 1.9 percent. The findings of the current paper are consistent with these stories.

One explanation for the small, measured effects of immigration is the small number of immigrants relative to the entire population. According to the U.S. Census Bureau, the net foreign-born migration into the U.S. averaged 790,000 people per year for the last ten years for both authorized and unauthorized immigrants. 2019 added only 595,000 people, and the rate has been declining since 2016. This number represents only a 0.15 percent increase in the total U.S. population per year, and only a portion of that becomes part of the U.S. labor market. The number of foreign-born individuals entering the U.S. labor market each year introduces the question of whether the addition of these individuals indeed constitutes a shock to the labor market. This question notwithstanding, it is worth noting that immigrants are not distributed uniformly across the U.S. landscape; some areas have a much higher concentration of immigrants, as is the case in sanctuary counties, shown in Table 1 in the appendix. Immigrants' location choice can be driven by border enforcement (Bohn and Pugatch 2015), local policies (Watson 2013), or economic opportunities (Cadena 2013).

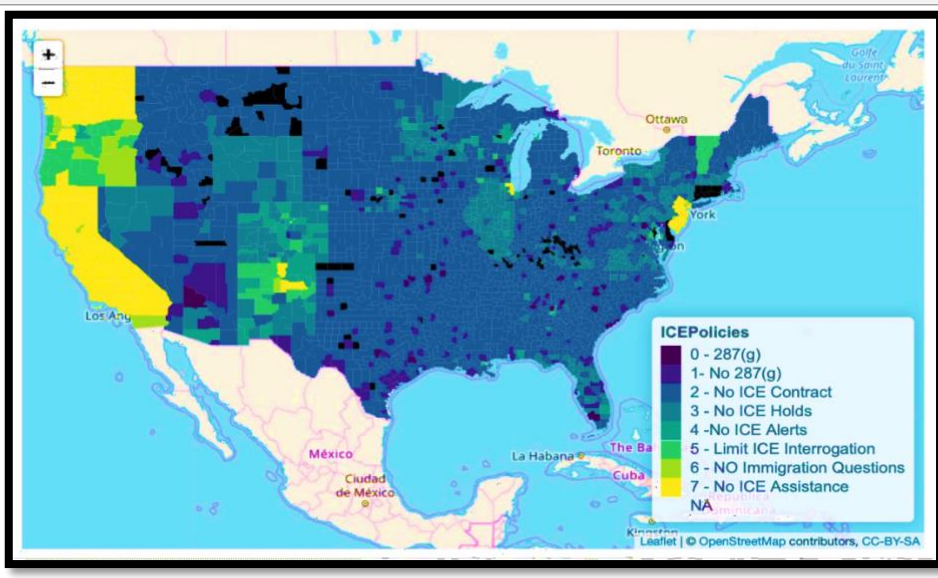
Watson (2013) finds that the 287(g) ICE policy nearly doubles the propensity for immigrants to relocate within the United States; however, the most significant effects are observed among non-citizens with college or higher education. Similarly, the data used for my analysis shows no statistically significant evidence of an increase in the foreign-born population after counties adopt sanctuary policies, and finds a 4 percent decline in the foreign-born population when countries adopt policies to criminalize undocumented immigrants. In sum, this paper adds to the current literature by examining both inclusive and exclusive local immigration policies, and no existing study uses a data set comparable in scope. The comprehensive data set in the present study accounts for 85.1% of the U.S. population by the end of our study period. The remainder of the paper proceeds as follows. Next section describes the data and the following section the empirical model. Section 2.4 tests for the parallel pre-trend assumption, Section 2.5 relays the results of this study, and Section 6 provides the discontinuity model with results. Finally, section 2.7 offers a theoretical model explanation, and the last section concludes.

2.2 Data and Summary Statistics

This study compiles data on sanctuary policies from 2006 to 2018 from the Immigration and Customs Enforcement Agency (ICE) and the Immigrant Legal Resource Center (ILRC). The data integration helps characterize counties as sanctuary counties, neutral counties, or counties cooperating with ICE to identify and detain undocumented immigrants. Our sample of 797 counties consists of all U.S. counties with a population of 65,000 or more, accounting for 85.1% of the U.S. population by the end of our study period.

For clarity, "sanctuary city" is the commonly used term, but there can be either sanctuary cities or counties in terms of jurisdictions. The term "sanctuary counties" will be used in this paper to include both sanctuary cities and counties. While some cities designate themselves as sanctuary cities, the term "sanctuary city" is, in many cases, more symbolic than actual. Stated differently, "sanctuary city" is an umbrella term for locations with an expressed pro-immigrant stance. However, sanctuary cities differ in the extent to which the city's sanctuary status reflects the city's resource allocation and formal policies regarding collaboration with ICE. Consequently, our sanctuary city definition is based on the ILRC classification of seven policies.

Figure 2.1
ICE Policies



The ILRC policy classification in 2019

The ILRC has been tracking counties' policy data on immigration since 2013 and created an index based on the extent of local, county-level assistance to immigration enforcement across the country, shown in Figure 2.1. The ILRC defines sanctuary cities by county jails' policies regarding assistance with deportations; these policies govern how immigrants may be profiled and funneled into the deportation pipeline (ILRC report) (Avila et al. 2018). Seven central policies characterize county-level cooperation with immigration enforcement along an eight-point spectrum from zero to seven. The assignment of a "zero" on this spectrum indicates that county-level authorities go out of their way to spend local resources on immigration enforcement. Conversely, a "seven" on the spectrum denotes the counties with the most comprehensive immigrants' protection. Since not all are immigrant-friendly policies, the index regards the non-adoption of a policy, as a policy itself, as in the case of counties' non-adoption of 287(g) contracts and declination of a No ICE Detention policy. The descriptions of the seven policies are as follows:

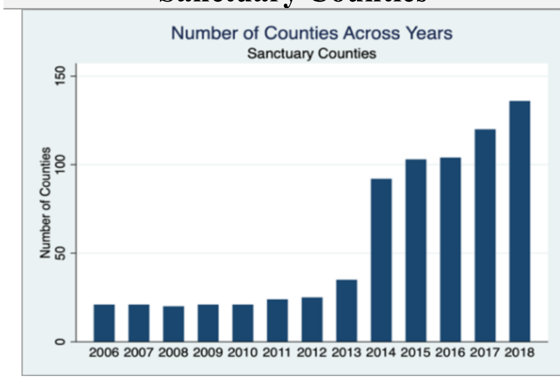
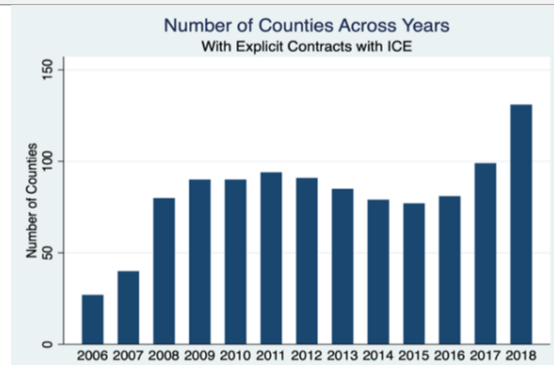
Table 2.1
Regularity of immigration county policy throughout our study period

7 Policies	Description	Out of 10166 Observations & 797 Counties		
		Observations	Counties	Percentage
No 287(g)	The non-adoption of the 287(g) agreement with ICE. This agreement turns local police into immigration agents; hence local public safety officials become a direct route to deportation.	9687	782	95.3%
No ICE Detention	The non-adoption of detention contract. This contract between ICE and a local jail where ICE pays the jail to hold immigrants in detention during their deportation Proceedings.	8917	746	87.6%
Limiting ICE Detainers (No ICE Holds	ICE hold is a request from ICE to a local jail or law enforcement agency to hold a person for longer than what is lawful to allow ICE to come and take custody.	3207	338	32.2%
Restrictions to ICE about the release dates or other information	ICE asks local agencies to give them advance notice of when immigrants will be released from custody so that ICE can come and arrest them upon release.	698	127	6.75%
Limits on ICE access to local jails and ICE interrogation of detainees	Requires ICE to have a judicial warrant to access limited areas, and enact procedural protection for immigrants, so they can refuse to be interrogated by ICE agents.	431	101	4.13%
Prohibitions on Inquiries into immigration status	Prohibits their officers or employees from inquiring into immigration status or place of birth.	322	99	3.1%
General prohibitions on participating in immigration enforcement	Prohibits the use of local resources in assisting with immigration enforcement, such as joint task forces with ICE.	248	95	2.38%

Since the data consists of 797 counties and 13 years, we have a total of 10166 observations. The observations column represents the number of observations that each policy has. The counties column represents the number of counties that ever ended up with that policy throughout our study period.

Table 2.1 gives the name of the seven policies, their description, the number of observations (a data point at a specific county and a specific year) that have adopted that policy, the numbers of counties that adopted the related policies at any point in the thirteen years under study, and the percentage of times that the policy appeared in our sample size. When a county attains at least four of these policies in a given year, I assign it a 1 for sanctuary status. Notably, the seven policies that make up the ILRC system did not emerge simultaneously. While ICE detention contracts and the 287(g) policies began in 2006, many of the sanctuary-relevant policies that make up the ILRC spectrum were introduced to different counties before or throughout our sample period. Nevertheless, there was an inflection in the data in 2014 (Figure 1), as many counties adopted those policies that year, and the number of sanctuary counties more than doubled. According to our sanctuary county definition and by using this sample, 134 counties ended up with a sanctuary city designation throughout our study period, 132 counties were counties that ultimately endorsed explicit contracts to collaborate with ICE, and 531 counties where always assigned as neutral counties (NC) during the same period. Out of 134 sanctuary counties, 36 obtained the urban definition, and 106 the rural. Similarly, out of 132 ICE counties, 28 were defined as ICE counties, and 112 were rural throughout the period. The numbers do not match since few went from rural to urban after crossing the established population density threshold across the 13 years.

Given that the ILRC has been tracking sanctuary policy information since 2013, and our data started in 2006, I used ICE Declined Detainer Reports (DDR) to supplement ILRC data for the seven years that preceded ILRC's data collection beginning in 2013. DDR reports a list of jurisdictions that enacted policies restricting cooperation with ICE, including the type of restrictions and the years and months when counties enacted the policies. Hence, ICE information was crucial to ascertain changes in policy adoption from 2006 to 2013. The ICE-authored DDR reports continue from this span through 2018, and this data overlaps with comparable ILRC data from the same timeframe. In cases where counties appeared in documentation by both organizations, the ILRC's characterization coincided with ICE. Such corroboration was possible in most cases; for a small number of counties, data were available from only one source. In short, I synthesized information from both sources to construct the information detailed in Figures 2.2 and 2.3.

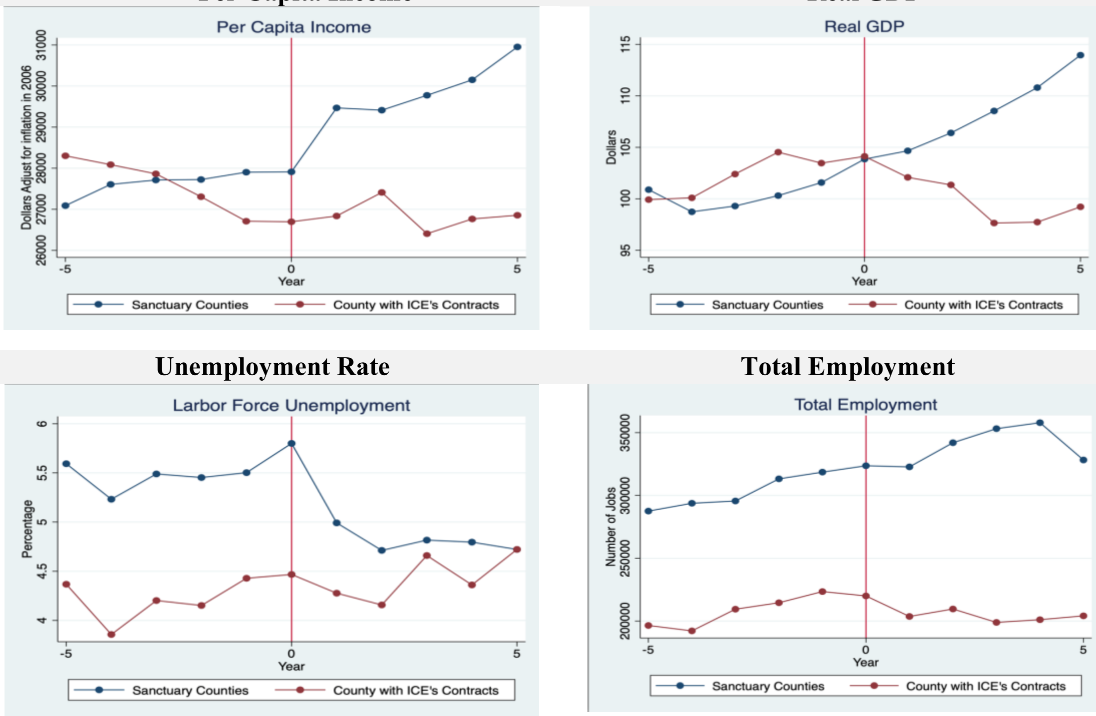
Figure 2.2 **Sanctuary Counties**	**Figure 2.3** **ICE Counties**
	

Ultimately, the study uses the American Community Survey data (ACS), combined with this newly digitized information on county-level immigration policy, to test whether development outcomes vary according to immigration policies. To summarize the data, Table 2.1-A in the appendix explores the differences in demographics that do not change significantly over time and shows the difference in economic indicators. These comparisons between SC, ICE, and NC, use the mean before and after 2013. The mean population among sanctuary counties suggests that such counties are mostly in metropolitan areas. Sanctuary counties show a much higher population density and a lower ratio of rural counties than ICE and neutral counties (NC) counties, but for all designations, rural counties are predominant with around 70 to 80 percent of all observations.

Figures 2.4

Per Capita Income **Real GDP**

Unemployment Rate **Total Employment**



All event graphs show 5 years before and after the adoption of the immigration policy for SC and ICE counties.

Additionally, sanctuary counties have a more diverse community with a higher rate of Latino-origin and foreign-born residents. However, all these indicators are also higher in ICE counties compared to NC. Some features of ICE counties may be attractive to immigrants (despite the ICE status of these counties). More likely, the higher presence of immigrants in SC and ICE counties motivate ICE initiatives to implement collaboration agreements in the first place. Similarly, ICE justifies its presence by the higher presence of immigrants in a county from a cost-benefit analysis perspective, which explains the absence of county-level policies concerning immigration enforcement in NC counties. Interestingly, ICE counties are the only designation with an observable drop in the mean-level population of the foreign born.

To visualize the difference between counties, I aligned the change in policies at a fixed period for all counties when a county became SC or ICE, as shown in Figure 2.4, describing the change in economic indicators over immigration policies' impact at the beginning of the fixed year zero. Sanctuary counties perform better than ICE counties starting at year zero according to the per-capita income and the unemployment rate. However, they performed better across time according to real GDP and total employment.

In Figure A2.1 in the Appendix, data show similar results according to the unemployment rate for women, white and Latino population, and similar results according to the average family income. The initial visualization of the data concurs with the assumption that inclusive institutions that invest in people and allow people to mobilize their talents and skills harness their potential human capital into the social system.

2.3 Empirical model

The basic strategy for this study is a panel difference in differences approach with fixed effects. The outcomes of interest are income, real GDP, the unemployment rate, median wages, and total employment. The specification equation has three approaches to reduce the problems of selection. First, I estimate the impact of policies comparing sanctuary and ICE counties versus neutral counties. Second, using only those counties that ever end up with a sanctuary city designation and only the variation in sanctuary designation timing, I addressed the criticism that sanctuary counties might be fundamentally different from non-sanctuary counties. In the third subsample, I repeat the last procedure with only counties that ever collaborate with ICE. I test for differential pre-trends between ever-sanctuary and never-sanctuary counties, as well as between early and late implementers, and economic attributes. I repeat these processes with each economic indicator and separate rural and urban areas. Finally, I estimate the impact of policies on median family income across education levels and economic quintiles, and the unemployment rates by gender and ethnic categories. The first estimation that includes the full sample is:

$$EI_{it} = \alpha + \beta_1 SC_{it} + \beta_2 ICE_{it} + \gamma_i + \sum_{i=1}^{8} \varphi_i Reg_i * Y_t + Y_t + \theta' X_{it} + \epsilon_{it} \qquad (1)$$

For only sanctuary counties, we use:

$$EI_{it} = \alpha + \beta_1 SC_{it} + \gamma_i + \sum_{i=1}^{8} \varphi_i Reg_i * Y_t + Y_t + \theta' X_{it} + \epsilon_{it} \quad for\ all\ i \in SC \qquad (2)$$

And for only ICE counties the estimation is:

$$EI_{it} = \alpha + \beta_2 ICE_{it} + \gamma_i + \sum_{i=1}^{8} \varphi_i Reg_i * Y_t + Y_t + \theta' X_{it} + \epsilon_{it} \quad for\ all\ i \in ICE \qquad (3)$$

EI_{it} is the dependent variable that stands for economic indicator (EI) at county i and in period t. The following two treatment groups, SC and ICE, are dummy variables for sanctuary cities and counties with explicit contracts with ICE, where 1 is a treatment county, and 0 is a control county or neutral county (NC). These treatment groups will also be time-variant, as counties become SC or ICE. The omitted group will be neutral counties in equation 1. The county fixed effects control for the time-invariant effect of county-specific characteristics is γ_i. The equations include eight economic region time trends (Reg*Y_t), and a continuous year time trend, and time-varying controls X_{it} . The latter includes political variables that would influence sanctuary counties' assignments, such as diversity, population density, rural or not, percentage of the foreign population, and an education index.

The study also explores specific labor markets according to people's educational attainment, gender, ethnicities, and economic quintiles. As a robustness check, we pre-process the data using nearest neighbor matching based on counties' economic attributes, demographics, regions, whether the county is rural or urban, education index, and the percentage of minority populations. Moreover, for an additional robustness check, the study analyzes the results using rural or urban counties, and a geographical regression discontinuity using only counties that share a border. All regressions use robust standard error clustered at the state level. Finally, the study applies a randomization inference test for the main regressions with each dependent variable. Randomization inference takes the set of study subject as fixed and regards only the treatment assignment as a random draw. Hence, it is based on resampling the variable of interest. Then, randomization inference tests the estimate β_1 obtained by comparing the means of the coefficient estimates from the regressions, by randomly changing the treatment status SC or ICE a thousand times.

2.4 Parallel Pre-Trend Assumption

Each of these models relies on different versions of the parallel trend assumption. Equation 4 requires that sanctuary, ICE, and neutral counties would have maintained parallel trends in the absence of policies change, while equations 5 and 6 use late policy-adopters as the counterfactual for early ones. This section presents tests on the pre-policy implementation to see if they suggest that this assumption holds. The estimation equations are:

$$EI_{it} = \alpha + \sum_{t=1}^{8} \delta_t Y_t * Ever_{SC_i} + \sum_{t=1}^{8} \theta_t Y_t * Ever_{CEC_i} + X_{it}\Gamma + \gamma_i + \epsilon_{it} \qquad (4)$$

For only sanctuary counties,

$$EI_{it} = \alpha + \sum_{t=1}^{8} \delta_t Y_t * Ever_{SC_i} + X_{it}\Gamma + \gamma_i + \epsilon_{it} \qquad (5)$$

For only ICE counties,

$$EI_{it} = \alpha + \sum_{t=1}^{8} \theta_t Y_t * Ever_{CEC_i} + X_{it}\Gamma + \gamma_i + \epsilon_{it} \qquad (6)$$

This approach allows us to see the differences in the change in economic indicators between the affected and counterfactual groups across time. As above, the dependent variable is $\boldsymbol{EI_{it}}$, and the treatment groups are ever sanctuary or ever ICE county. The interaction of the treatment groups with $\boldsymbol{Y_t}$ becomes the test for the difference in trends prior to the change in policies, which are the year-specific dummy variables from 2006 to 2013 for equation 4. When using late policy adopters as the counterfactual for early ones for equations 5 and 6, the equation uses every year, and the first year becomes the base.

Table 2.2
Pre-Trend Test using Per Capita Income

	Ordinary Least Squares		Fixed Effects	
Sanctuary County in 2007	0.021**	(0.01)	-0.0019	(0.01)
Sanctuary County in 2008	0.032**	(0.01)	0.0089	(0.01)
Sanctuary County in 2009	0.0075	(0.02)	-0.017	(0.01)
Sanctuary County in 2010	0.0030	(0.02)	-0.011	(0.01)
Sanctuary County in 2011	-0.000043	(0.02)	-0.021	(0.02)
Sanctuary County in 2012	0.012	(0.03)	-0.012	(0.03)
Sanctuary County in 2013	-0.0054	(0.03)	-0.017	(0.02)
ICE County in 2007	0.019	(0.01)	0.0093	(0.01)
ICE County in 2008	0.033	(0.02)	0.035***	(0.01)
ICE County in 2009	0.028	(0.03)	0.0028	(0.02)
ICE County in 2010	0.023	(0.03)	-0.011	(0.02)
ICE County in 2011	0.036	(0.03)	-0.00021	(0.01)
ICE County in 2012	0.034	(0.03)	0.018	(0.02)
ICE County in 2013	0.029	(0.02)	-0.0062	(0.01)
Observations	9351		9351	
Adjusted R-squared	0.437		0.240	
Pre-trend test for sanctuary counties, using early adopter as counterfactual				
Sanctuary County in 2007	-0.048	(0.03)	0.00058	(0.03)
Sanctuary County in 2008	-0.020	(0.05)	-0.00064	(0.03)
Sanctuary County in 2009	0.016	(0.06)	0.0023	(0.04)
Sanctuary County in 2010	0.075	(0.06)	0.024	(0.03)
Sanctuary County in 2011	0.089	(0.05)	0.025	(0.04)
Sanctuary County in 2012	0.036	(0.07)	-0.00084	(0.04)
Sanctuary County in 2013	0.024	(0.07)	-0.036	(0.03)
Observations	1846		1846	
Adjusted R-squared	0.525		0.338	
Pre-trend test for ICE counties, using late policy adopter as counterfactual				
ICE County in 2007	0.011	(0.03)	-0.011	(0.02)
ICE County in 2008	-0.0064	(0.05)	-0.021	(0.02)
ICE County in 2009	-0.0081	(0.06)	-0.0058	(0.02)
ICE County in 2010	0.0022	(0.04)	0.012	(0.03)
ICE County in 2011	-0.030	(0.04)	-0.016	(0.02)
ICE County in 2012	-0.021	(0.04)	-0.020	(0.02)
ICE County in 2012	-0.0062	(0.04)	0.017	(0.02)
Observations	1779		1779	
Adjusted R-squared	0.465		0.279	

Standard errors in parentheses * p<0.10, ** p<0.05, *** p<0.01 All regressions contain robust standard errors clustered by county ID, time trends and time variant covariates.

Using per capita income, Table 2.2 suggests that all ordinary least squares, fixed effects, and time-varying controls do support a parallel pre-trend assumption: most years show no statistically significant change in per capita income between the treatment and the control groups. These results are also consistent when using late policy adopters as counterfactuals for sanctuary and ICE counties. I also estimate the same test for the labor unemployment rate, real GDP, median wages, and the total employment per county, shown in Tables A2.2, A2.3, and A2.4 in the appendix. These results are also consistent except for the real GDP when comparing sanctuary counties to neutral counties, and total employment only for late policy adopters in sanctuary (using the OSL regression) and ICE counties (using the fixed effects regression). All of them show a statistically significant change in at least three years out of seven.

This study was written with a companion paper that examines the choice of pro- or anti-immigrant policies by county. This paper's goal is to find out whether the economic circumstances predict county's policy choice. The policies are the same, the collaboration with Immigration and Customs Enforcement (ICE) and the sanctuary policies that provide legal protections (Natanson 2022). However, by placing the independent variable as the dependent one, if the economic factors influence immigration policy preferences, we will inevitably have a problem of reverse causality. However, the comparison analysis shows that, in a fixed effects setting, economic factors do not determine adoption of local immigration policies.

2.5 Results

All regressions contain eight regional time trends, a time dummy, and a matrix of control for population density, foreign population, elections results per county, voting turn out, rural or urban, and an education index. They include robust standard errors and are clustered by county. The first column shows the ordinary least square regression, the second column shows the fixed effects model, and the third column combines fixed effects and the nearest neighbor matching. The nearest neighbor matching is based on counties' economic attributes, an education index, family income, and region. The fixed effect model in the second column is the primary regression since it contains the most variation. Table 2.3 presents results using the log of per capita income as the dependent variable. The omitted group is the neutral counties. Using equation 1, the point estimate for sanctuary counties is statistically significant in most models with magnitudes ranging from

3.14 to 7.46 [1] percent increase in per capita income in sanctuary counties. There are no significant effects of ICE counties in any of the models.

Table 2.3
Natural log of Per Capita Income

Model	(1) Ordinary Least Squares,	(2) Fixed Effects	(3) Fixed Effects & Matching	
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1				
Sanctuary County	0.072***	0.031***	0.043***	
	(0.02)	(0.01)	(0.01)	
P- value from Randomization Inference	0.014	0.000	0.008	
ICE County	0.00044	-0.0013		-0.0015
	(0.02)	(0.01)		(0.01)
P- value from Randomization Inference	0.51	0.324		0.569
Observations	10147	10147	3705	3095
Adjusted R-squared	0.444	0.234	0.347	0.283
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2				
Sanctuary County	0.0045	0.017*	0.013	
	(0.02)	(0.01)	(0.02)	
Observations	1846	1846	1326	
Adjusted R-squared	0.762	0.355	0.409	
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3				
ICE County	-0.0051	-0.0061	-0.0086	
	(0.01)	(0.01)	(0.01)	
Observations	1778	1778	1751	
Adjusted R-squared	0.654	0.298	0.329	

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are in parentheses. All regressions contain robust standard errors clustered by county, time trends, and time variant covariant. Nearest – neighbor matching is based on counties' economic attributes, education index, family income, and region.

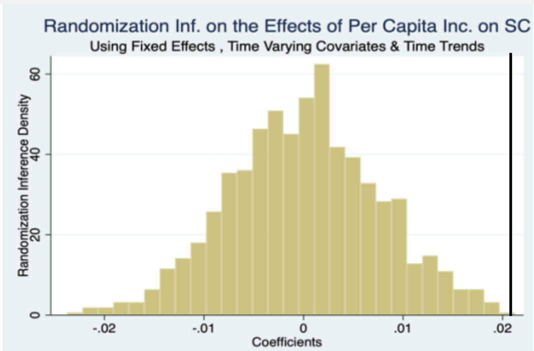
All statistically significant results are also significant after testing for randomization inference (RI) regarding the treatment assignment as random. Using RI, I regressed all the models, randomly assigning the treatment a thousand times. The results for RI are the P-values below the standard of errors for each regression using Equation 1. The significance is based on the statistical difference between the mean of all thousand coefficients using RI and the main results. The bell distributions of the randomization inference for the fixed effects models and the matching models are shown in Figure 2.5. The vertical line in each graph is the estimated coefficient from the relevant regression from Table 2.3, and it is visible to see that the sanctuary counties' significant

[1] Since the log-dependent variable is a dummy variable, the marginal effect is calculated by 100(exp(c) -1). However, given that the coefficients are small and the differences in the results are negligible, I don't do this calculation for every coefficient.

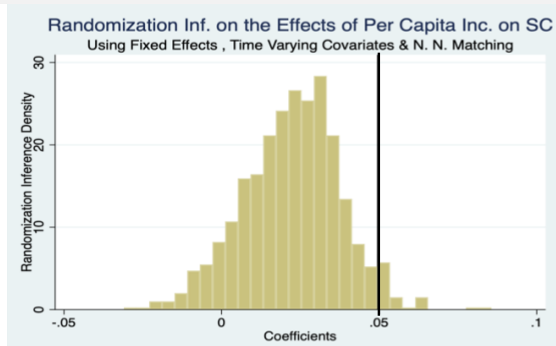
results are located further away from the median of the bell. The results are the same for all other outcomes.

Figure 2.5
Randomization Inference for Per Capita Income
The vertical lines are the estimated coefficient from the relevant regression in Table 2.3
Sanctuary Counties

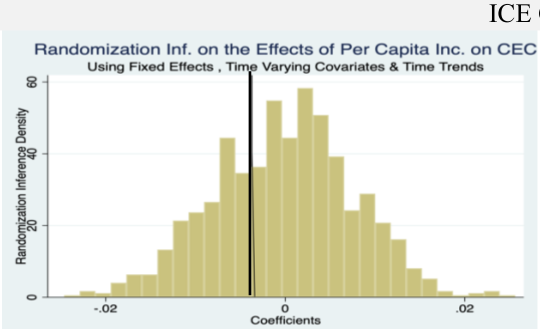
Fixed Effect6

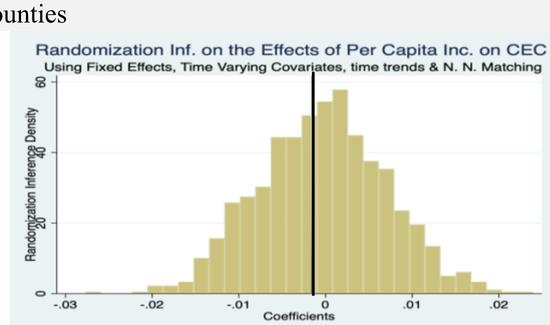


Fixed Effects & Matching



ICE Counties





In the second section, using only those counties that ever end up with a sanctuary city designation (equation 2), and therefore using only the variation in sanctuary designation timing, I addressed the possible criticism that sanctuary counties might be fundamentally different from non-sanctuary counties. Here, results are similar but with less variation in magnitude, with a 1.7 percent increase in per capita income in Sanctuary counties using our primary regression. With Equation 3, ICE counties again show no significant effects in any of the models. The contrast in the results between the sanctuary and ICE counties is interesting. It suggests that while providing protections to immigrants increases economic activity, punitive measures do not improve economic outcomes for natives.

In Table 2.4, I repeat all three equations using only the fixed effect model, but now I separate urban and rural counties. In this way, I address the likelihood that urban counties may be different from rural counties. Starting by using the urban and rural neutral counties as control groups, urban sanctuary counties show an increase in per capita income of 2.5 percent and a 3.2 increase in rural counties, while there is no effect in ICE counties for rural or urban. In addition, using late policy adopters as controls, I find an increase in per capita income of almost 2 percent in rural sanctuary counties.

Table 2.4
Natural log of Per Capita Income

Mode 2: Fixed Effects	Equation 1		Equation 2	Equation 3
	Urban	Rural	Urban using only Sanctuary Counties	Rural using only ICE Counties
Sanctuary County	0.025**	0.032***	0.0057	0.019*
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1292	7811	472	1374
Adjusted R-squared	0.415	0.225	0.513	0.361
ICE County	0.0015	-0.0030	0.0023	-0.0044
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1286	8118	301	1478
Adjusted R-squared	0.370	0.226	0.445	0.302

* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

Table A2.5 in the appendix shows the impact of immigration policies on the labor force unemployment rate. All models are statistically significant, with magnitudes ranging from 12 to 17 percent declines in the unemployment rate in sanctuary counties but no significant effect on ICE counties. The same result appears when dividing the data amount late policy adopters and rural counties in Table A2.6, with a decrease in unemployment in rural sanctuary counties by around 12 percent. However, the data shows an increase in unemployment in urban ICE counties ranging between 6.4 to 8.1 percent. Table A2.7 shows the impacts on real GDP. The results in most models are statistically significant positive for sanctuary counties with magnitudes ranging from 2.5 to 4.1 percent increase. This time for ICE counties, the study detects a small but significant decline in GDP using only the OLS model. When using late policy adopters as the control groups (with equations 2 and 3), I find consistent results, and that is also the case when separating the data between rural and urban counties in Table A2.8.

Table A2.9 shows the total employment growth after adopting sanctuary policies between 2.3 to 4 percent, while the adoption of ICE policies decreases the total employment per county. Results are the same after I restrict the data to later policy adopters, and rural and urban counties as shown in Table A2.10. Finally, with similar results, Table A2.11 and A2.12 shows an increase in median wages after adopting sanctuary policies and after restricting the data to rural, urban, and late policy adopters between 1.7 to 2.6 percent. There are not policy effects on ICE counties.

Thus, using per capita income, unemployment rate, real GDP, total employment, and median wages, the study finds strong evidence that protecting people increases efficiency in the economy. Hence, the results show evidence supporting the hypothesis that immigrants' human capital benefits ought to be more prominent in regions where institutions are inclusive, and conversely, punitive measures are detrimental to economic outcomes.

How do immigrants respond to these policies? In Table A2.13, I test for the effects of local migration policies on immigrants' population or mobility due to policy changes, and using Equation 1, I find no effect for sanctuary or ICE policies. These results contradict the basic intuition that the protection of immigrants would increase the immigrant population in sanctuary counties. No change in the foreign-born population enforces the idea that local immigration policies only harm immigrant rights because immigrants only respond to the availability of jobs. However, when comparing counties with similar characteristics using only the variation in ICE county designation timing, I find a decline in the foreign-born population by 4 percent. In this case, ICE policy institutions do produce their intended effect by creating some incentive for immigrants to leave ICE counties.

Lastly, I examine heterogeneity in impact across different populations by educational attainment, the economic quintiles, gender, and ethnic groups. Using the fixed effects model, Table 2.5 shows the effect on median earnings among educational attainment. Results are positive and significant at all educational attainment in sanctuary counties except at the college level. However, more interesting is that contrary to the literature, I obtain favorable outcomes for workers without a high school diploma. Similarly, punitive measurements in ICE counties show no significant effects.

Table 2.5
Natural log of Median Earning by Educational Attainment

	No High School	High School	College	Bachelor	Graduate School
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1					
Sanctuary County	0.038*	0.027**	0.0097	0.026***	0.019*
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
ICE County	0.015	0.0056	-0.0048	-0.0053	-0.0038
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	10147	10147	10147	10147	10147
Adjusted R-squared	0.049	0.085	0.082	0.111	0.084
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2					
Sanctuary County	0.023	0.021**	0.0038	0.023**	0.025**
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1846	1846	1846	1846	1846
Adjusted R-squared	0.116	0.119	0.102	0.141	0.156
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3					
ICE County	0.012	0.0028	-0.0048	-0.0097	-0.0042
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1779	1779	1779	1779	1779
Adjusted R-squared	0.058	0.106	0.097	0.156	0.123

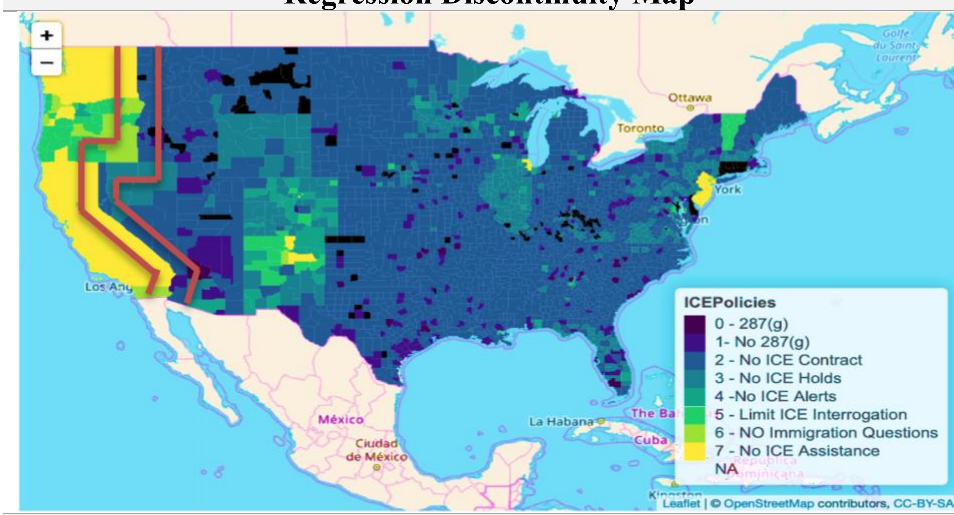
* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

Then, by dividing the population among quintiles using average household income in Table A2.14, results show positive results on all quintiles in sanctuary counties ranging from 2 to 3.8 percent increase, but no effect on ICE counties. Finally, in Table A2.15, by dividing the labor force unemployment by gender and race, I find statistically significant positive results for women, men, whites, and Latinos in sanctuary counties. However, this time results show a positive effect on ICE counties only on the African American population.

2.6 Regression Discontinuity

The final robustness check applies a geographical regression discontinuity analysis. Here we select only counties that share a border with other counties with distinct policies. The map shows a clear contrast in immigration policies between bordering counties alone California, Oregon, and Washington that share borders with Arizona, Nevada, and Idaho. Here, the former offers better protections to immigrants, represented by the lighter colors between the red lines in Picture 2.6.

Figure 2.6
Regression Discontinuity Map



The ILRC policy classification in 2019

After restricting the data to only those counties along the border, results are still consistent and robust. Table 2.6 gives the results using the natural log of per capita income, labor force unemployment, GDP, total employment, and the median wages as the dependent variables. Here, I only use the OLS and fixed effects model due to the decrease in observations to 169, using equation 1. Nonetheless, this strategy confirms initial results with favorable outcomes for sanctuary counties across all dependent variables, and adverse effects for ICE counties. In the regression discontinuity model the per capita income increases by 5.9 percent in sanctuary counties. In addition, the unemployment decreases 22 percent (only using OLS), the GDP increases by 6 percent, the total employment increase by 3.5 percent, and the median wage increases by 7 percent. In contrast, ICE counties obtain an 8.5 percent decline in GDP, a 7.2 percent decrease in total employment, a 3 percent increase in wages, and no significant results for per capita income, and unemployment.

Table 2.6
Regression Discontinuity Model

	Ordinary Least Squares		Fixed Effects	
Natural log of Per Capita Income				
Sanctuary County	0.19**	(0.08)	0.059**	(0.01)
ICE County	0.015	(0.06)	0.049	(0.03)
Observations	169		169	
Adjusted R-squared	0.694		0.426	
Natural Log Labor Force Unemployment				
Sanctuary County	-0.22***	(0.02)	-0.14	(0.19)
ICE County	0.035	(0.03)	0.046	(0.04)
Observations	169		169	
Adjusted R-squared	0.499		0.292	
Natural log of Real GDP				
Sanctuary County	0.065**	(0.02)	0.064*	(0.03)
ICE County	-0.089***	(0.00)	-0.085***	(0.01)
Observations	169		169	
Adjusted R-squared	0.528		0.553	
Natural log of Total Employment				
Sanctuary County	0.35	(0.29)	0.035*	(0.02)
ICE County	-0.18	(0.18)	-0.072***	(0.01)
Observations	169		169	
Adjusted R-squared	0.742		0.561	
Natural log Median Wages				
Sanctuary County	0.17*	(0.06)	0.073***	(0.01)
ICE County	0.009	(0.02)	0.032**	(0.01)
Observations	169		169	
Adjusted R-squared	0.553		0.415	

* p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses, all regressions contain robust standard errors clustered by county ID, time trends & time varying covariates

2.7 Theoretical Model

The Solow human-capital augmented growth model (Mankiw, Romer, and Weil 1992) emphasizes that human capital stock increases through physical investments in human capital.

These investments are measurable and accessible to analyze since they are rival to consumption and are excludable. Hall and Jones (1999) accommodate institutional differences with a framework that posits that output per worker is driven by differences in institution and government policies. In their framework, greater social infrastructure improves input productivity and increases output per worker, in that order. The previous section, however, shows positive effects of inclusive policies on economic outcomes with no evident increase in population. This research extends Hall and Jones framework to include institutional constraints on people's realization of their potential human capital in the contexts of immigration policies.

The notation is standard: Y is output, K is capital, H is human capital, L is labor, and the A term reflects knowledge and technology. Then the production function is

1) $$Y_i(t) = F(K, H, AL) = K^{\propto} H^{\beta} (AL)^{1-\alpha-\beta} \qquad (0 < \alpha < 1,\ 0 < \beta < 1)$$

The growth rates of depreciation (δ), population (n), and productivity *(g)* are assumed to be constant across countries (in our case, counties). After deriving for all market factors, the evolution of capital and human capital in the economy, in equations 2a and 2b, grow by physical investment S_k and S_h. The evolution of labor (L) is assumed to grow exogenously and constantly at rate n, in equation 3a. In equation 3b, the total factor productivity should be a function of human capital and is assumed to grow by knowledge (g). The standard equations of motion are:

2) $$a)\ \dot{h}(t) = S_h y(t) - (n + g + \delta)h(t) \qquad b)\ \dot{k}(t) = S_k y(t) - (n + g + \delta)h(t)$$

3) $$a)\ L(t) = L(0)e^{nt} \qquad b)\ A(t) = A(0)e^{gt}$$

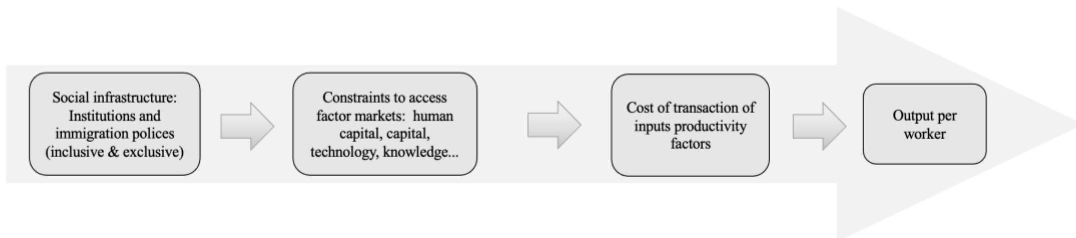
Following Eicher, Garcia, and Teksoz (EGT 2006), I allow the elasticity of output with respect to input to depend on the quality of institutions *(I)* at every location *(i)*. Total factor productivity A depends on institutions such that $A_i = Ae^{pI_i}$. Hence, local immigration policies can be represented by the combination of the MRW and EGT models, which allow the total factor of productivity A to depend on not just the advancement of knowledge (g) but also institutions according to $A_i = A(e^{gt} + e^{pI_i})$. However, the advancement of knowledge is non-excludable and non-rival across counties in our case; therefore, we can simplify the model by excluding (g) and describe the total factor of productivity as $A_i = A(e^{pI_i})$. Then, under the assumptions that $\alpha + \beta < 1$, the model converges to a steady state. By substituting h and k at the steady state into the production function and taking logs, equation 4, for income per capita, includes the total factor productivity (A) that depends on the quality of institutions, such that:

$$4)\ ln\left[\frac{Y(t)}{L(t)}\right] = \ln A(0) + pI + \frac{\beta}{1-\alpha-\beta} ln(s_h) + \frac{\alpha}{1-\alpha-\beta} ln(s_k) - \frac{\alpha+\beta}{1-\alpha-\beta} \ln(n+g+\delta)$$

Thus, social infrastructure (institutions) affects input productivity, which in turn affects output per worker. In addition, human capital is therefore constrained and shaped by institutions' excludability function, represented by pI, as well as by investment in human capital $ln(s_h)$. This constraint occurs because institutions constrain all access to factor markets such as capital, technology, knowledge, or human capital, as North (1991) argued that institutions are "the humanly devised constraints that structure political, economic and social interactions."

The case of undocumented workers and their families is fragile because they contend with a level of risk that is hard to understand. In everyday activities, undocumented immigrants risk losing all possessions, their children, their families, their livelihoods, or the household breadwinners. Vulnerability to deportation forces undocumented individuals to take risks, such as taking their kids to school, driving to work, maintaining vigilance while grocery shopping, or going to the hospital. At the same time, there is considerable evidence that high risk and uncertainty brings chronic stress, and the high levels of cortisol produced in the body among immigrants due to stress reduce economic productivity and impair human capital (Squires et al. 2012, Mewes et al. 2017, Martinez et al. 2018, Yim et al. 2019, Garcini et al. 2019, Keinan 1987; Keinan et al. 1987, Arnsten 1998). Hence, depression, stress, and uncertainty about the future restrict people's optimal contribution to society and inhibit their potential human capital.

Figure 2.7
Institutional Theory



With restrictive local immigration policies, immigrant families experience more economic insecurity, emotional stress, discrimination, racial profiling, detentions, and deportations (Androff et al., 2011; Ayon, 2014, 2015). Here, immigration policies play an essential role in ameliorating or exacerbating the consequences of risk to immigrants' human capital (Woodland et al., 2006). Hence, we can think of total factor productivity (A_i) in the Solow Model as the depression, stress, and uncertainty regulator for immigrants because they are (or are not) allowed to live their life, work, and send their children to school without hesitation. Similarly, emerging work suggests that institutions play an essential role in constraining the effects of immigration, as Kemeny et al. (2017) present evidence supporting the hypothesis that urban immigrant diversity's benefits should be broader in regions where institutions are inclusive.

In sum, institutions regulate transaction cost of the total factor productivity by increasing or decreasing uncertainty, which in principle determines whether human capital is fully optimized as it is represented by Ae^{pI_i} in the model. As described in Figure 2.7, institutions decrease uncertainty and constrain human interaction (the access to factor markets), decrease transaction cost (the inputs of productivity factors), and increase efficiency in the economy (the output per worker) (Coase 1960; Williamson 1987; North 1990; Milgrom and Roberts 1992; Hall and Jones 1999; Acemoglu and Robinson 2008, 2013; David 2017). Analogously, sanctuary cities reduce uncertainty, constraints, and risk for immigrants' interaction in their communities, decrease the cost from fear of deportation or the constant fear of criminalization, and optimize their human capital. These benefits, largely unquantifiable, are separate from the production process; nonetheless, they are socially and economically fruitful due to the gains from human capital and the subsequent increase in productivity factors.

2.8 Conclusion

This study contributes to the economics of migration literature by uncovering positive effects of inclusive policies on economic outcomes with no evidence of increase on population, and by seeking to understand the mechanism through which inclusive policies affect society. Given that sanctuary cities constitute an example of inclusive policies, characterizing sanctuary cities' economic features may have implications for inclusive institutions more generally (Sokoloff et al. 2000; Sokoloff 2003; Acemoglu & Robinson, 2013). Coase (1960) argues that uncertainty in human behavior is the reason for increased costs resulting from market transactions. Hence, the decrease in uncertainty and risk increases coordination and market exchange, improves the information flow, decreases transaction costs in society, and increases productivity. In the process, it strengthens social trust and cooperation. In many ways, that is the purpose of sanctuary cities. Fear of deportation or a constant fear of criminalization, a separate cost from the production process, is socially and economically costly for people and all businesses.

The takeaway from this study is that institutional inclusion creates the dynamic nature of the U.S. economy, as inclusion allows for an economic expansion due to the extension of fundamental freedoms to newcomers. Inclusive policies enable new immigrants to increase consumption, supply hard work, provide a renovated entrepreneurial spirit, thus creating more jobs, inventing new industries, and reviving uncompetitive sectors that would otherwise be sent abroad. Providing protections to undocumented immigrants increases economic activity. In addition, immigration policies or institutions play an essential role in conditioning the effect of immigration, and Sanctuary policies specifically yield economic benefits for counties that adopt them.

A future research plan to close the gap in this literature should investigate the labor market impacts within destination areas, such as comparing the differences in productivity between undocumented Bracero Workers and DACA recipients who work in areas where they are persecuted compared to those who work in areas with basic protections. In addition, the labor force population has started to fall in the United States, reflected in 86 percent of counties, and as a population shrinks, people are also aging faster (Ozimek et al., 2019). Future research could investigate whether sanctuary policies have had any impact on the population and age structure in the counties where they exist and if their application in other counties could help address the hollowing out of many non-sanctuary locations. This study and future research should further help policy makers to produce inform local immigration policies, and those decisions should let to future research.

3. Do Economic Indicators Influence Immigration Policies Choice?

3.1 Introduction

Many political and economic narratives around immigration in the US suggest that it comes at an economic cost by driving down wages for native workers and straining both taxpayers and the national economy through the costs associated with social services. Hence, economists are often interested in measuring the impacts of immigration shocks and new policies on economic outcomes. Identifying these impacts depends upon understanding what drives the decision to choose particular policies. If the drivers of this decision are economic, then reverse causality biases the estimated policy impact on economic factors.

Popular narratives dictate that areas with high wealth or large immigrant populations are more likely to protect immigrants' rights. Here, the perception is that in affluent locations with high employment, native jobs are not at risk, and there will be a demand for a low-income labor force. Further, areas with a high percentage of immigrants may be compelled to protect immigrants due to a robust social network. On the other hand, those jurisdictions that adopt punitive measures against undocumented immigrants do it because their economic difficulties would encourage them to protect their workers. Consequently, the drivers of these policy choices may be based on economic factors, again producing reverse causality. This paper aims to determine whether the selection of pro- or anti-immigrant policies is consistent with the narrative that economic conditions drive the choice of immigration policy.

To assess the impact of economic indicators on a county's choice of immigration policy, I use American Community Survey data on per capita income and employment combined with newly digitized information on county-level immigration policies. The econometric approach exploits the variation in economic indicators across time and space to isolate the effects of these indicators on immigration policy choice by counties. The immigration policy choice variable is based upon a County Policy Classification Index (CPC). To understand how cross-sectional and temporal variation drive policy choice, I estimate models that introduce fixed effects at the unit and then the time level, comparing these to estimates without such effects. I also use a hybrid model that allows for the simultaneous estimation of within versus between effects of covariates.

Evidence demonstrates that the selection of sanctuary policies is not correlated with per capita income, median wages, or unemployment. Results show that the selection of sanctuary policies correlates to population density, non-citizen population, and election outcomes. However, the significance of population density and election outcomes go away once we control for fixed effects at the county level. These results reflect mostly long-term preferences towards immigration by location, regardless of the temporal variation in economic circumstances. The selection of ICE policies correlates with per capita income, median wages, the unemployment rate, population density, non-citizen population, and the election outcomes across space. However, these significances also go away once we control for fixed effects at the county level. This difference between controlling for the state and county-level fixed effects also suggests that the choice of the policy does not depend on the year-to-year changes in unemployment, wages, or per capita income but again on long-term preferences.

Narratives around immigration preferences are strongly linked to economic theory. Standard economic theory suggests that in a perfectly competitive labor market, an increase in the labor supply will increase total employment, decrease wages, and may increase unemployment (Borjas 2013, 2015, Camarota 1998, 2011). Policymakers and communities who believe or have experienced this dynamic may fear that the arrival of immigrant workers will directly compete for a finite number of American jobs. If this is the case, then decreases in per capita income and unemployment or the increase of migrant population would lead to policy preferences that support punitive measurements against immigrants. Meanwhile, other narratives and theories that examine other facets of immigration suggest that immigration to the United States is associated with increased human capital and economic development due to productivity growth, which may lead to inclusive policy preferences for immigrants. (Peri, 2012, 2016; Model 2008).

Historically, the drivers of immigration policy have also been related to racism. Immigration restriction policies in the United States began by limiting Chinese and other 'non-whites', and they evolved to achieve race-based policies without direct reference to it through legal transfers such as landing taxes or literacy tests (Ghezelbash 2017). In addition, there seems to be a consensus in the literature that whereas non-economic drivers have a significant and independent effect on individual preferences, economic characteristics of the location systematically shape attitudes towards immigration (Espenshade 1996, Citrin et al. 1997, Kessler 2001, Scheve and Slaudhter 2001). Most studies are based on surveys capturing people's beliefs, as Espenshade (1996) found a close connection between possessing restrictionist immigration attitudes and having an isolationist perspective regarding international issues. Citrin et al. (1997) found that economic anxiety and generalized feelings about immigrants are significant determinants of restrictionist sentimentsFacchini and Mayda (2010) find a positive correlation between actual migration policy and public opinion. As a result, it may be the case that regardless of the economic circumstances, there might be implicit biases that shape attitudes toward immigration that are distinct and rigid at each location, influencing immigration policy adoption.

This paper is also related to work on specific immigration policies in the United States. In particular, 287(g) is an ICE immigration policy that turns local police into immigration agents, and it is included in this study. Bohn et al. (2017) examined the economic effect of policy 287(g) on employment and wages and found no beneficial effect on all industries combined. Ifft et al. (2017) also found that after the 287(g) implementation, farms experienced statistically significant labor and fuel expense increases. In addition, Natanson (2022) finds that protective policies for immigrants have positives economic outcomes, and punitive policies (including 287(g)) have mostly no statistically significant effects. This paper uses the same data as Natanson (2022). If the economic factors influence immigration policy preferences, all these papers would suffer a reverse causality problem. However, the results presented here imply that threats to identification identified in Natanson (2022) do not come from reverse causality between year-to-year variation in economic outcomes and changes in policy. Hence, the present work strengthens the results of these papers by finding that the choice of immigration policies is uncorrelated with short term changes in economic indicators.

This study contributes to the economics of migration literature by showing that there is no clear evidence of autocorrelation or reverse causality between short term economic fluctuations and the choice of immigration policy. The evidence gathered in this research suggests an incongruency between people's preference for immigration policies and counties' economic characteristics, specifically regarding policies that provide essential legal protections for immigrants. The year-to-year fluctuations in per capita income, wages, and unemployment seem independent of an immigration policy selection. The selection seems to be more likely to reflect long-term preferences toward immigration that are distinct and rigid at each location, even though economic anxiety and narratives may shape feelings about immigration.

Hence, this analysis leads to a better understanding of the determinants of local migration policies in the United States and could guide future policy implementation. In addition, examining the links between public preference toward immigration, economic indicators, and governments' policy decisions is crucial to understanding the system, and it serves as a guide for researching the impact of immigration policy. Section 3.2 describes the data, section 3.3 presents the empirical model, section 3.4 gives the results, and section 3.5 concludes.

3.2 Data and Summary Statistics

3.2.1 Outcome variable

The study combines data on sanctuary policies from 2013 to 2018 from the Immigration and Customs Enforcement Agency (ICE) and from 2006 to 2013 from the Immigrant Legal Resource Center (ILRC). This data helped characterize counties as sanctuary counties, neutral counties, or counties cooperating with ICE to identify and detain undocumented immigrants. Our sample of 797 counties consists of all U.S. counties with a population of 65,000 or more, accounting for 85.1% of the U.S. population by the end of our study period.

"Sanctuary cities" refers to municipal jurisdictions that limit their cooperation with the Immigration and Customs Enforcement (ICE) agency, a federal agency that enforces immigration policies. While some cities designate themselves as sanctuary cities, the term "sanctuary city" is, in many cases, more symbolic than actual. Stated differently, in broader policy discussions, "sanctuary city" is an umbrella term for locations with an expressed pro-immigrant stance. However, sanctuary cities differ in the extent to which the city's sanctuary status reflects the city's resource allocation and formal policies regarding collaboration with ICE. There have been jurisdictions that have called themselves sanctuary cities without related policies in place. For this paper, I use a more specific definition of sanctuary policy based on the ILRC classification of seven policies, and our Immigration Policy Index (IPI), an ordered variable ranking from 0 to 7.

The ILRC has been tracking counties' policy data on immigration since 2013 and created an index based on the extent of local, county-level assistance to immigration enforcement across the country, shown in Figure 2.1. The ILRC defines sanctuary cities by county jails' policies regarding assistance with deportations; these policies govern how immigrants may be profiled and funneled into the deportation pipeline (ILRC report) (Avila et al. 2018). Seven central policies characterize county-level cooperation with immigration enforcement along an eight-point spectrum from zero to seven. The assignment of a "zero" on this spectrum indicates that county-level authorities go out of their way to spend local resources on immigration enforcement. Conversely, a "seven" on the spectrum denotes the counties with the most comprehensive immigrants' protection. From this ranking, one or below is consider an ICE county, four or above a sanctuary county, and two or three are neutral counties. They are classified this way if they ever achieve a category. Since not all are immigrant-friendly policies, the index regards the non-adoption of a policy, as the policy itself, as in the case of counties' non-adoption of 287(g) contracts and declination of a No ICE Detention policy.

Given that the ILRC has been tracking sanctuary policy information since 2013, and our data started in 2006, I used ICE Declined Detainer Reports (DDR) to supplement ILRC data for the seven years that preceded ILRC's data collection beginning in 2013. DDR reports a list of jurisdictions that enacted policies restricting cooperation with ICE, including the type of restrictions and the years and months when counties enacted the policies. Hence, ICE information was crucial to ascertain changes in policy adoption from 2006 to 2013. The ICE-authored DDR reports continue from this span through 2018, and this data overlaps with comparable ILRC data from the same timeframe. In cases where counties appeared in documentation by both organizations, the ILRC's characterization coincided with ICE. Such corroboration was possible in most cases; for a small number of counties, data were available from only one source.

Table 2.1 gives the name of the seven policies, their description, the number of observations that have adopted that policy, the numbers of counties that adopted the related policies at any point in the thirteen years under study, and the percentage of times that the policy appeared in our sample. According to the ILRC sanctuary county definition and by using this sample, 134 counties ended up with a sanctuary city designation throughout our study period, 132 counties were counties that ultimately endorsed explicit contracts to collaborate with ICE, and 531 counties where always assigned as neutral counties (NC) during the same period. When a county attains at least four of these policies in a given year, I assign it a 1 for sanctuary status. Notably, the seven policies that make up the ILRC system did not emerge simultaneously. While ICE detention contracts and the 287(g) policies began in 2006, many of the sanctuary-relevant policies that make up the ILRC spectrum were introduced to different counties before or throughout our sample period. Nevertheless, there was an inflection in the data in 2014 (Figure 1), as many counties adopted those policies that year, and the number of sanctuary counties more than doubled. Figures 2.2 and 2.3 present the number of counties classified as having ICE or sanctuary policies over time.

3.2.2. Covariates

I combined data from the American Community Survey (ACS) with this newly digitized information on county-level immigration policy to test whether economic characteristic influence the choice of sanctuary or ICE policies at the country level. Table 3.1 summarizes the variables extracted from the ACS for each type of county. These comparisons between SC, ICE, and NC, use the mean before 2013, when significant changes began to occur in the presence of these types of policies. The mean-level mean population among sanctuary counties suggests that such counties are mostly in metropolitan areas. Sanctuary counties show a much higher population density and a lower ratio of rural counties than ICE and neutral counties (NC) counties.

Table 3.1
Descriptive statistics among sanctuary counties (SC), counties with explicit contracts with ICE (ICE) and neutral counties (NC)

	Mean Before 2013		
Variable	**SC Mean**	**ICE Mean**	**NC Mean**
Observations	994	958	3549
Total Mean Population	606652.90	363969.50	245357.40
% Rural Population	0.74	0.83	0.89
% White Population	73.46	80.30	81.05
% Latin Population	21.57	15.32	11.02
% Citizen by Birth	85.45	91.35	94.09
% Foreign Born	14.55	8.65	5.91
% Unemployment	5.43	4.45	4.56
% Women Unemployed	4.79	4.00	4.06
% White Unemployed	7.77	6.14	6.47
% Latino Unemployed	7.22	4.04	2.30
$ Med. Family Income	68582.80	65977.90	63575.23
$ White Ave. Income	79853.49	74651.13	70469.21
$ Latino Ave. Income	58211.46	53930.03	56108.42
$ Med. Earnings	36337.13	35193.91	34292.18
$ Per Capita Income	28773.84	27312.25	26105.04
% Working Poor	13.81	12.62	13.45
Gini Index	0.45	0.44	0.44

Sanctuary counties have a more diverse community with a higher Latino-origin and foreign-born residents. However, all these indicators are also higher in ICE counties compared to N.C. Some features of ICE counties may be attractive to immigrants. It is possible that the higher presence of immigrants in S.C. and ICE counties motivates ICE initiatives to implement collaboration agreements in the first place. Similarly, ICE could justify its presence by the higher presence of immigrants in a county from a cost-benefit analysis perspective, which may explain the absence of county-level policies concerning immigration enforcement in N.C. counties. In addition, economic and political factors influence immigration views and shape people's preferences over immigration policies.

This study is interested in the way people's preferences on immigration policy are linked to economic indicators and governments' policy decisions, which their voting choice could reflect. Hence, we control for voting outcome per county. The dummy variable on the presidential election outcomes with a Democratic choice candidate per county is intended to be used to proxy for voter preferences regarding immigration policies. This variable may change every four years starting in 2005, 2008, 2012, and 2016, but its variation is minimal. However, it reflects who won in each county, not who won the presidential election. Other explanatory variables include an education index, population density, rural, percentage of the noncitizen, and the percentage of households that receive public assistance income (PAI). Education and demographic indicators are likely to have some influence on people's preferences regarding immigration policy, and PAI controls the assumption that localities with poor populations will be more incline to favor anti-immigration policies. Finally, there may be a risk of reverse causality, or if the dependent variable's present value depends on its past values, we will end up with autocorrelation. Therefore, the independent variables include lagged per capita income values, median wages, unemployment rate, and voting outcomes per county.

3. Empirical model

The model regresses policy choice on economic, demographic, and political variables. The linear probability model with fixed effects is given by:

$$\boldsymbol{y_{it} = \alpha + \beta_1 X_{it-1} + \beta_2 X_i + \gamma_i + \sum_{i=1}^{n=50} \varphi_i States_i * T + Y_t + \epsilon_{it}} \qquad \textit{1)}$$

Using panel data, I begin by considering the linear probability model with county-specific effects. I use a number of dependent variables, $\boldsymbol{y_{it}}$, including a dummy variable for sanctuary cities (SC), a dummy variable for ICE counties, and a County Policy Classification (CPC), an ordered binary variable classifying ICE counties, neutral counties, and sanctuary counties ranking from 1 to 3 in that order. The CPC will be estimated by the ordered logistic probability model. All regressions use 50-state time effects (RT), county fixed effects, time effects, and time-varying and nonvarying covariates. The subscript (it) denotes level 1 variables and (i) denotes level 2 variables. Level 1 variables refer to the variables that varied between and within clusters, and level 2 variables only varied between clusters and don't change much overtime. $\boldsymbol{X_{it-1}}$ is a lagged level 1 variable, and $\boldsymbol{X_i}$ is a level 2 variable. Among other variables, $\boldsymbol{X_{it-1}}$ includes the per capita income lag, the unemployment lag, an education index, the population density, the percentage of non-citizen population, and the percentage of population with public assistance income lag. $\boldsymbol{X_i}$ includes a dummy variable for rural or urban county and a dummy variable on election outcomes with a Democratic party win. $\boldsymbol{\gamma_i}$ is the state or county fixed effects control for the time-invariant effect. **States*T** is the fifty-states time effects and $\boldsymbol{Y_t}$ are years times dummies.

From equation 1, the FE model (or "within") provides unbiased and consistent effect estimates of $\boldsymbol{\beta_1}$ assuming that $\boldsymbol{X_{it-1}}$ is strictly exogenous. But this comes at the cost of removing all variables that do not vary at level 1, and fixed effects models cannot estimate the effect of level 2 variables. Hence, we use the hybrid model (Allison 2009). The hybrid model is given by:

$$\boldsymbol{y_{it} = \alpha + \beta_1(X_{it-1} - \overline{x}_i) + \beta_2 X_i + \beta_3 \overline{x}_{it} + \gamma_i + \sum_{i=1}^{n=50} \varphi_i States_i * T + Y_t + \epsilon_{it}} \qquad \textit{2)}$$

The hybrid model estimates within effects in random-effects models by decomposing level 1 variables into between ($\boldsymbol{\overline{x}_i = n_i^{-1} \sum_{t=1}^{n_i} X_{it-1}}$) and cluster ($\boldsymbol{X_{1it-1} - \overline{x}_i}$) components by assuming no correlation between level 1 and 2 variables. From equation 2, $\boldsymbol{\beta_1}$ gives the within-effect estimate or the fixed-effects estimates, and $\boldsymbol{\beta_2}$ gives the estimate effect of level 2 variables. $\boldsymbol{\beta_3}$ estimates the between effect (Mundlak 1978), and its inclusion ensures that effect estimates of level 2 variables are corrected for between cluster differences in $\boldsymbol{X_{it}}$.

3.4 Result

All regressions contain robust standard errors and are clustered by county. Table 3.2 uses sanctuary counties as the dependent variable and the linear probability model. The first column includes only all-time varying controls. The rest of the columns show a combination of between and within variation. The second column, using time effects only, helps us identify where these policies are located while controlling what happens over time. The third column uses state fixed effects; hence, we can identify in each state where these policies are concentrated and how they change over time. Then by adding the time effects and the state fixed effects in the fourth column, we can hold constant the regionalization about each state over time and space and see which counties adopt these policies. In the fifth column, we are only looking at the things that change over time at the county level by adding county fixed effects and time effects. Hence, using the hybrid model in the sixth column, we can look at the variables that change over time and the variables that stay constant. In the appendix for Tables A3.1 and A3.1, I include a seventh column with the correlated random effects estimator, which relaxes the assumption of zero correlation between the variables that change over time and those that do not. In addition, I exclude the median wages and the public assistance variables to address the criticism that they might be highly correlated with per capita income and unemployment. In both tables, results are statistically the same in contrast to Tables 3.2 and 3.3 below.

Table 3.2
Sanctuary County policy selection as the depended variable: linear probability model

	1 OLS	2 Time Effects (TE)	3 State Fixed E. (SFE)	4 TE & SFE	5 County Fixed E.	6 Hybrid
Per Capita Income Lag	0.190* (1.80)	0.0207 (0.37)	0.0395 (0.67)	-0.0251 (-0.45)	-0.00587 (-0.18)	-0.00470 (-0.14)
Between & between - within effects						-0.0247 (-0.34)
Median wages lag	-0.0921 (-0.92)	0.0177 (0.26)	0.0308 (0.46)	0.0217 (0.32)	0.0106 (0.30)	0.0105 (0.30)
Between & between - within effects						0.0119 (0.16)
Unemployment Lag	0.0125 (1.53)	-0.00325 (-0.46)	-0.00619 (-0.83)	-0.000320 (-0.05)	0.00544 (0.97)	0.00547 (0.97)
Between & between - within effects						0.00387 (0.47)
Mean years of schooling	0.0271 (1.07)	0.0251 (1.55)	0.0256 (1.52)	0.0274* (1.82)	0.0188* (1.87)	0.0194* (1.93)
Between & between - within effects						0.0308* (1.83)
Population density	0.000934*** (4.95)	0.00155*** (11.40)	0.00155*** (11.47)	0.00152*** (12.15)	-0.000296 (-0.17)	-0.0000141 (-0.01)
Between & between - within effects						0.00152*** (8.84)
Rural county	0.109** (2.03)	0.0179 (0.83)	0.0154 (0.71)	0.0236 (1.11)	0.0329 (1.35)	0.0323** (2.02)
Percentage of Non-Citizens	0.0098*** (2.74)	-0.00297 (-1.40)	-0.00382 (-1.65)	-0.000662 (-0.32)	-0.000595 (-0.63)	-0.000532 (-0.56)
Between & between - within effects						-0.00132 (-0.64)
Democratic Win Lag	0.0605* (1.91)	0.0603*** (2.68)	0.0628*** (2.71)	0.0545** (2.45)	0.00478 (0.30)	0.00341 (0.21)
Between & between - within effects						0.0574*** (4.07)
Public assistance income (PAI)	0.0363*** (2.81)	0.00529 (0.87)	0.00935 (1.25)	-0.0000439 (-0.01)	-0.00315* (-1.69)	-0.00315* (-1.69)
Between & between - within effects						0.00238 (0.34)
Obs.	8491	8491	8491	8491	8491	8491
Linear Prob.	Yes	Yes	Yes	Yes	Yes	Yes
Yean Time Effect	No	Yes	No	Yes	Yes	Yes
States Time Trend	No	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	No	No	Yes	Yes

County Between Effects	Yes	Yes	Yes	Yes	No	Yes
County Between – Within Effects	No	No	No	No	Yes	Yes
*** p<0.10, ** p<0.05, *** p<0.01 - All Regressions includes robust standard errors clustered by county ID**						

Using the linear probability model and sanctuary counties as the dependent variable, Table 3.2 shows that the only statistically significant variables are population density, the non-citizen population, public assistance, and the election outcomes. However, the significance of population density and election outcomes go away once we control for fixed effects at the county level in columns 5 and 6. The education and all economic variables are statistically insignificant, except with the percentage of families with public assistance income, which only matters when controlling for the year-to-year variation. Hence, a recent increase in public assistance income (PAI) is associated with a decline in becoming a sanctuary county but with a minimum magnitude of 0.003 percent probability of decline. Given that the PAI mean is 0.73 percent of the population, and one standard deviation change it to 0.9 percent, the variation in PAI is minimal in relation to the outcome variable, which has a mean of .172. This is also reflected by excluding PAI from the regression. As shown in Table (A.3.1) in the appendix, API does not impact the rest of the economic variables.

These results suggest an incongruency between people's preference for immigration policies and the counties' economic characteristics. The election results variable also only matters across space; hence, it is likely to reflect long-term preferences toward immigration that are distinct and rigid at each location, regardless of the economic circumstances.

Table 3.3
ICE county policy selection as the depended variable: linear probability model

	1 OLS	2 Time Effects (TE)	3 State Fixed E. (SFE)	4 TE & SFE	5 County Fixed E.	6 Hybrid
Per Capita Income Lag	0.0257 (0.32)	0.173*** (2.72)	0.171*** (2.69)	0.177*** (2.79)	-0.00840 (-0.27)	-0.00875 (-0.28)
Between & between - within effects						0.166

						(1.16)
Median wages lag	-0.0676 (-0.80)	-0.139* (-1.78)	-0.137* (-1.77)	-0.138* (-1.75)	-0.00691 (-0.20)	-0.00606 (-0.18)
Between & between - within effects						-0.154 (-1.02)
Unemployment Lag	0.0230** (2.32)	0.0328*** (2.96)	0.0327*** (2.94)	0.0328*** (2.94)	0.00351 (0.64)	0.00354 (0.65)
Between & between - within effects						0.0297* (1.89)
Mean years of schooling	0.0102 (0.50)	-0.00152 (-0.07)	-0.00195 (-0.09)	-0.00180 (-0.08)	0.00590 (0.60)	0.00549 (0.56)
Between & between - within effects						0.00809 (0.24)
Population density	-0.000308* (-1.72)	-0.000367** (-2.03)	-0.000364** (-2.01)	-0.000372** (-2.05)	0.000524 (0.30)	0.000500 (0.29)
Between & between - within effects						-0.000452 (-1.31)
Rural county	0.00126 (0.03)	0.0374 (0.91)	0.0369 (0.90)	0.0372 (0.90)	0.0262 (1.11)	0.0313 (1.52)
Percentage of Non-Citizens	0.00762* (1.97)	0.00892** (2.39)	0.00893** (2.40)	0.00891** (2.36)	0.000507 (0.55)	0.000497 (0.54)
Between & between - within effects						0.0122*** (2.95)
Democratic Win Lag	-0.0732*** (-3.86)	-0.0504*** (-2.68)	-0.0501*** (-2.67)	-0.0512*** (-2.69)	0.0154 (0.98)	0.0158 (1.01)
Between & between - within effects						-0.0634** (-2.25)
Public assistance income (PAI)	-0.0149*** (-2.66)	0.00266 (0.61)	0.00190 (0.47)	0.00359 (0.79)	0.00193 (1.07)	0.00195 (1.08)
Between & between - within effects						0.00597 (0.42)
Obs.	8491	8491	8491	8491	8491	8491
Linear Prob.	Yes	Yes	Yes	Yes	Yes	Yes
Yean Time Effect	No	Yes	No	Yes	Yes	Yes
States Time Trend	No	Yes	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	No	No	Yes	Yes
County Between Effects	Yes	Yes	Yes	Yes	No	Yes
County Between – Within Effects	No	No	No	No	Yes	Yes

*** p<0.10, ** p<0.05, *** p<0.01 - All Regressions includes robust standard errors clustered by county ID**

The following analysis uses the selection of ICE policies in a county as the dependent variable. Table 3.3 shows that per capita income, median wages, the unemployment rate, population density, non-citizen population and the election outcome are significant when controlling for spatial and temporal effects at the state level. However, the significance goes away once we control for fixed effects at the county level, as well as when using the hybrid model. This difference between controlling for the state and county level fixed effects also suggests that it is not the year-to-year changes in the indicators that are influencing the choice of policy but a long-term process. In other words, the choice of the policy does not depend on the last year change in unemployment, wages, or per capita income. When looking at counties we are looking at short term changes, but when we are controlling for fixed effects at the state levels, we are looking across counties within in a state, allowing variation from one county to the next within the same state.

Table 3.4
ICE, neutral and sanctuary county: Order logistic probability model – marginal effects

		1 Logit		2 Time Tend		3 State Fixed E.		4 TT & SFE	
Log Per capita income lag	ICE	-0.108	(-0.97)	0.228***	(2.70)	0.221***	(2.68)	0.228***	(2.70)
	NEU	-0.00390	(-0.15)	-0.143**	(-2.48)	-0.138**	(-2.47)	-0.143**	(-2.48)
	SC	0.112	(0.88)	-0.085***	(-2.88)	-0.082***	(-2.85)	-0.08***	(-2.88)
Wages lag	ICE	-0.00286	(-0.02)	-0.149	(-1.58)	-0.144	(-1.56)	-0.149	(-1.58)
	NEU	-0.000103	(-0.02)	0.0937	(1.53)	0.0899	(1.51)	0.0937	(1.53)
	SC	0.00296	(0.02)	0.0557	(1.63)	0.0540	(1.61)	0.0557	(1.63)
Log unemploy ment lag	ICE	-0.00537	-0.38)	0.0215	(1.39)	0.0211	(1.39)	0.0215	(1.39)
	NEU	-0.000194	(-0.13)	-0.0135	(-1.36)	-0.0132	(-1.36)	-0.0135	(-1.36)
	SC	0.00556	(0.36)	-0.00804	(-1.40)	-0.00793	(-1.40)	-0.00804	(-1.40)
Mean years of schooling	ICE	0.00598	(0.21)	-0.0246	(-0.93)	-0.0243	(-0.94)	-0.0246	(-0.93)
	NEU	0.000216	(0.13)	0.0154	(0.94)	0.0152	(0.95)	0.0154	(0.94)
	SC	-0.00619	(-0.21)	0.00916	(0.90)	0.00913	(0.91)	0.00916	(0.90)
Pop. density	ICE	-0.00161	(-1.53)	-0.004***	(-3.89)	-0.004***	(-3.89)	-0.004***	-3.89)
	NEU	-0.00005	(-0.17)	0.002***	(3.36)	0.002***	(3.36)	0.002***	(3.36)
	SC	0.00167*	(1.88)	0.001***	(4.31)	0.001***	(4.31)	0.001***	(4.31)
Rural county	ICE	-0.102**	(-2.16)	-0.0120	(-0.20)	-0.0133	(-0.22)	-0.0120	(-0.20)
	NEU	-0.00367	(-0.16)	0.00751	(0.20)	0.00832	(0.22)	0.00751	(0.20)
	SC	0.105*	(1.72)	0.00447	(0.20)	0.00500	(0.22)	0.00447	(0.20)
% of NON-citizens	ICE	-0.0119*	(-1.76)	0.00727	(1.51)	0.00716	(1.52)	0.00727	(1.51)
	NEU	-0.000431	(-0.15)	-0.00455	(-1.47)	-0.00448	(-1.49)	-0.00455	(-1.47)
	SC	0.0124	(1.39)	-0.00271	(-1.52)	-0.00269	(-1.54)	-0.00271	(-1.52)
Dem. Party win lag	ICE	-0.0829**	(-2.54)	-0.074**	(-2.17)	-0.0746**	(-2.21)	-0.0744**	(-2.17)
	NEU	-0.00299	(-0.16)	0.046**	(2.31)	0.0466**	(2.35)	0.0466**	(2.31)
	SC	0.0859**	(2.33)	0.0278*	(1.89)	0.0280*	(1.92)	0.0278*	(1.89)
% Public assistance income	ICE	-0.042***	(-3.31)	0.00532	(1.05)	0.00463	(1.03)	0.00532	(1.05)
	NEU	-0.00152	(-0.16)	-0.003	(-1.06)	-0.00289	(-1.04)	-0.00333	(-1.06)
	SC	0.0437**	(2.45)	-0.001	(-1.02)	-0.00174	(-1.01)	-0.00198	(-1.02)
Obs.			9280		9280		9280		928
OLS			Yes		Yes		Yes		Yes
Time effects			No		Yes		No		Yes
State fixed effects			No		No		Yes		Yes
County fixed effects			No		No		No		No

*** p<0.10, ** p<0.05, *** p<0.01 - All regressions include robust standard errors clustered by county ID**

Finally, Table 3.4 uses a County Policy Classification dependent variable with an ordered logit probability model. This binary variable classifies ICE counties, neutral counties, and sanctuary counties ranked from 1 to 3 in that order. Results show that the only strong statistically significant covariant are per capita income, population density, and the results of the elections. However, I don’t control for year-to-year effects at the county level since the regression does not converges. Nonetheless, the result for per capita income is paradoxical to the assumptions. Namely, an increase in the per capita income increases the probability of becoming an ICE county, and the increase in per capita income decreases the likelihood of becoming a sanctuary county. Other economic variables, median wages, unemployment, and public assistance are not statistically significant. Finally, the most internally consistent results are found in the probability of becoming a sanctuary county if population density increases and if the Democratic party wins. Election outcomes indicate that a Democratic win increases the likelihood of becoming a sanctuary city by almost 3 percent, and it reduces the chances of becoming an ICE county by 7 percent.

3.5 Conclusion

Despite the persistence of claims and economic narratives that immigrant labor deprives native workers of jobs and drives down the standard of living, this study inquiry whether those claims will translate into immigration policies in U.S. counties or if they will follow local economic indicators. This study contributes to the literature by informing economic analysis that the county's choice of pro- or anti-immigrant policies is not correlated with local economic indicators. Results show that per capita income, wages, and unemployment are independent of a sanctuary policy choice. This evidence suggests an incongruency between counties' economic characteristics and people's preference for immigration policies.

All economic variables are statistically insignificant when using sanctuary counties as the dependent variable. Preferences for punitive measurement are slightly correlated to fluctuations in unemployment, wages, per capita income, and non-citizen populations when not controlling for county fixed effects. Significantly, the probability of becoming an ICE county increases when per capita income increases. This contradiction suggests that the choice for ICE county policy is still politically driven, given that the election variable is also the strongest predictor of ICE policy choice. In addition, the fact that the significance goes away once we control for fixed effects at the county level suggests that it is not the year-to-year changes in the indicators that influence the choice of policy but a long-term process. In other words, the choice of the policy does not depend on the last year's change in unemployment or per capita income, but perhaps only in long-term narratives and beliefs.

This study also contributes to the literature by rejecting the hypothesis that reverse causality and autocorrelations may be present. These results are essential for research papers that find distinct fluctuations in unemployment and per capita income after adopting immigration policies, as studies find that the ICE policies reduce employment and per capita income. In contrast, the partner paper finds that adopting sanctuary policies is associated with increased per capita income and declines in unemployment. Hence, this research encourages future research to further close the gap in this literature to investigate the labor market impacts within destination areas.

4. Impacts of Harmful Algal Blooms on Property Values in Western Lake Erie

4.1 Introduction

With the increased global use of chemical fertilizers and the difficulty of regulating nonpoint sources, land use is increasingly harming downstream economies, neighborhoods, and ecosystem services hundreds of miles from its origin. On the Chinese coast in 2013, fish farms were struck by organisms that turned the water red and resulted in a loss of half of the annual production. In Toledo, OH, in 2014, a toxic green bloom threatened aquatic life and resulted in a water shutdown for the city. Global analysis suggests a geographic expansion of cyanobacteria, with recorded blooms in at least 108 countries (O'Neil et al., 2012; Paerl and Paul, 2012). Runoff toxins are affecting aquatic ecosystems as well as public health across the world (Esposito et al. 2016; Paerl et al. 2008; Boyer et al. 2007; LaRoche et al. 1997), and where people make their living from tourism or fisheries, toxic algae blooms are doing damage with a high social price tag (Wolf et al. 2017, 2019; Keeler et al. 2015; Hanley et al. 2003). It is crucial to quantify the value of ecosystem services, not in order to commodify nature, but to give currency to natural resources in settings where such resources are either invisible or assumed to be inexhaustible. This strategy may, in turn, influence decision-making and incentivize conservation. This study examines and documents the economic consequences of eutrophication in North America's Great Lakes by measuring if property values decline due to harmful algal blooms in the urban and suburban areas of western Lake Erie.

To assess the impact of HABs on property values in western Lake Erie, I compiled data on property transactions from 2001 to 2015. The econometric approach uses quasi-experimental variation in both time and space comparing urban areas in the city of Toledo that are affected by

HABs to the urban areas in Cleveland that are not. The nature of the circumstances yields a continuous difference-in-difference environment in which I pre-match data on housing prices, control for census tract fixed effects, and in one specification, limit the data to houses sold repeatedly over the study period. Furthermore, this study applies a two-stage strategy to estimate the impact of HABs. In the first stage I compare the change in housing prices between Toledo and in and around Cleveland counties (1) before and after the blooms, (2) as a whole, and (3) conditional on distance from the pollution source. In the second stage I repeat the same model by using the areas within Toledo not affected by HABs as the control group. Pre-trend tests comparing the areas effected by HABs and counterfactuals groups find no statistically significant differences in the price trends before HABs first appeared in 2008, suggesting that the counterfactuals are valid.

Results show an overall decline in house prices with treatment effects of 13 and 19 percent in 2008 and 2011 respectively, using Cleveland (Cuyahoga County) as a control group, and a decline of 14 percent (in 2008) and 24 percent (in 2011) when using Lake County as a control group. Using unaffected areas within Toledo as control groups, the data show an overall range decline of 9 and 21 percent in 2008 and 2011, respectively. Additionally, results find statistically significant price declines of at least 7 percent among houses located within the first 14 kilometers from the lake, as well as among houses located in the first 9 kilometers from the tributaries. These estimates suggest that the total present value of all residential properties in Toledo, due to HABs, decreased from $25 billion to $22.2 billion.

This study contributes to the literature on hedonic pricing in general and seeks to assess the cost of HABs. There is a significant volume of empirical literature that utilizes housing prices to measure the value of the land attribute and ecosystem services such as air quality (Anselin et al.

2008; Kim et al. 2003; Murdoch et al. 1988), natural amenities (Gibbons et al. 2014; Cavaihes et al. 2009), environmental disamenities (Anderson et al. 2010; Day et al. 2007; Cameron 2006), and water quality (Wolf et al. 2017, 2019; Walsh et al. 2011; Leggett and Bockstael 2000, Muehlenbachs et al. 2015). Studying the impact of HABs in particular, Wolf et al. (2017) use data for four inland lakes in Ohio to show the effects of HABs on surrounding property prices. They found capitalization losses associated with near-lake homes between 12 and 17 percent. In their research design, the author's measure of lake quality in their difference-in-difference model is decomposed by the interaction of distance variables and a treatment effect. This study differs from Wolf et al. in that it introduces a control group from a similar city not affected by HABs, rather than of a lake quality variable.

Relatedly, Leggett et al. (2000) used hedonic techniques to estimate the impact of water quality on property values along the Chesapeake Bay in Maryland. They calculated the potential benefits from an illustrative water quality improvement and derived an upper bound to the benefits from a more widespread improvement. They worried about the endogeneity created by waterfront homeowners self-selecting themselves into high-price markets. Their study also added that hedonic studies of environmental quality are particularly vulnerable to omitted variable bias because emitters of pollution often have direct effects on the value of nearby properties for reasons unrelated to air or water quality. To address these issues, the present study limits the data to houses sold repeatedly over the study period in one of the specifications.

Equally important for this research, Muehlenbachs et al. (2015) estimated the positive and negative impact of new shale gas development on surrounding properties by affecting groundwater sources and bringing private investment to the area. In their research design, they had three distance measures: 0-2 km; 2-20 km; and over 20 km. The last category served as the macro effects region – the area that would be susceptible to state-level or national-level macro effects. In effect, the macro-effects region serves as a control group in their regressions. In the second stage of this study, I follow this example by using Toledo's unaffected area as its control after identifying the areas not affected by HABs.

In sum, this study's ultimate goals are to contribute to methodologies for the economic valuation of ecosystem services and to provide an assessment of the damages caused by HABs in Lake Erie. It differs from previous studies by introducing a robust two-stage scenario to identify the housing areas close to the pollution source but unaffected by HABS. The approach detects water quality-related damages to ecosystem services in the absence of water quality measurements and limits the data to houses sold repeatedly over the study period. The remainder of the paper is structured as follows: The next section briefly reviews the history of eutrophication in Lake Erie with the regulatory steps taken to stop the runoff of phosphorus into the lake and explains the current challenges. Section 3 gives the summary statistics of our data for the three counties surrounding Lake Erie. Section 4 proposes the experimental design methods, provides the econometric identification equations of the house price impact and delivers covariate balance tests and pre-trends results. Section 5 gives the results from this study, and Section 6 concludes.

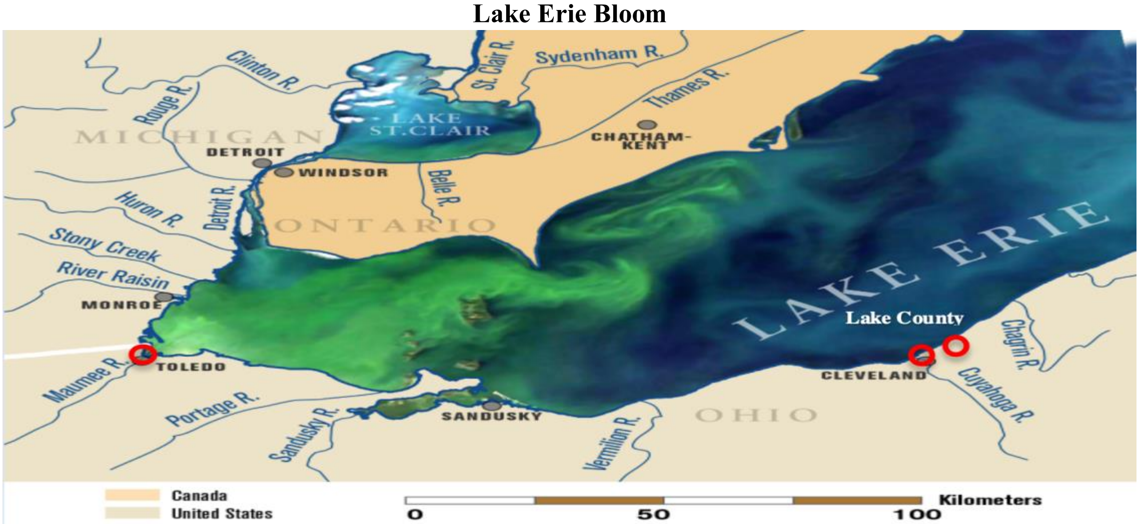
4.2 Eutrophication in Lake Erie

Lake Erie is in an intensively developed catchment that includes major cities, heavy industry and some of the most productive farmland in North America. The Lake also provides a range of ecosystem services to over 11.6 million people living within its Basin, including drinking water, swimming and beachfront recreation, and commercial and recreational fisheries. According to the International Joint Commission (IJC 2014) the Canadian American agency responsible to manage the waters of the Great Lakes, HABs could affect between 24,000 and 210,000 properties if effects on properties extend between zero and 16 km inland from the Lake Erie coastline.

Summer hypoxia has occurred naturally in the Lake for thousands of years. However, beginning in the 1950s, increased inflows of limiting nutrients, primarily phosphorus from industrial sources and urban wastewater, led to far more extensive algal blooms and severe hypoxia (Zhou et al., 2013). Starting with the Clean Water Act and the Great Lakes Water Quality Agreement in 1972, legislation to limit the flow of phosphorus and other pollutants into Lake Erie from sewage systems and industrial sources successfully created the institutional tools and regulations to limit those point sources of pollution. Lake Erie responded rapidly with decreases in phytoplankton biomass and hypoxia and the recovery of several fisheries throughout the 1980s (Scavia et al., 2014).

Figure 4.1

Lake Erie Bloom



The cities of Toledo, Cleveland and Lake County are each marked by a red circle in that order. The Maumee River is located in the lower-left corner. Image source: GreatlakesNow.org

Since the early 2000s, eutrophication has reemerged as a significant water quality issue in Lake Erie. Although policy changes successfully reduced phosphorus inputs from regulated point sources, the amount of phosphorus entering Lake Erie and its tributaries from unregulated nonpoint sources such as residential, urban, but mainly agricultural and cattle runoff now exceeds by far the amount discharged from regulated point sources (Kilbert et al., 2012). Combined with climatic factors of yearly precipitation and climate warming, this nutrient loading has led to annual summertime explosions of cyanobacteria growth concentrated around the mouth of the Maumee River and extending throughout the Western Lake Erie Basin, shown in Figure 4.1. These malodorous, unattractive, and dangerous blooms negatively impact a broad range of the ecosystem services provided by Lake Erie (Wells et al. 2015, Pearl et al. 2011, Azevedo el at 2002).

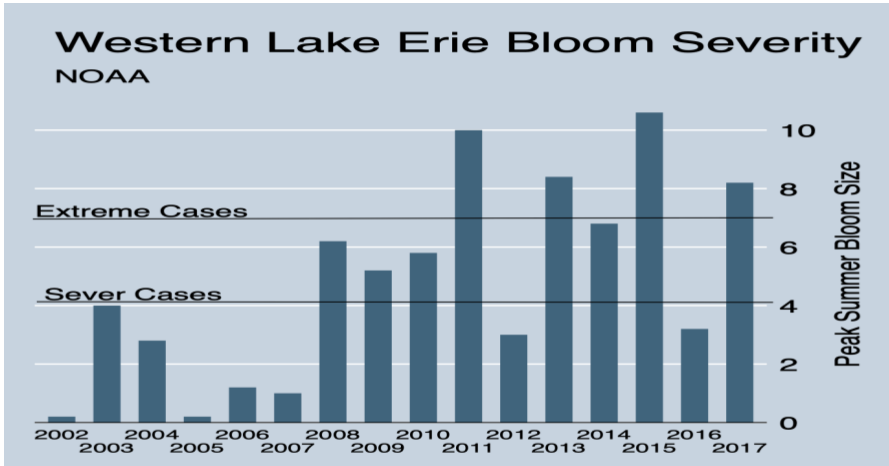
Table 4.1
Bloom Severity by Year

Cyano Index	Bloom Severity Category	Year(s)	March to June TP Load (MT)	March to June DRP Load (MT)	Annual TP Load (MT)
Maumee River					
< 1	None/Mild	2002, 2005, 2006, 2007, 2012	< 800	< 150	< 1,600
1 - 2.4	Moderate	2003, 2004	800 – 1,250	150 – 225	1,600 – 2,500
2.4 - 6	Severe	2008, 2009, 2010	1,250 – 1,750	255 – 315	2,500 – 3,500
> 6	Extreme	2011	> 1,750	> 315	> 3,500
Western Lake Erie					
< 1	None/Mild	2002, 2005, 2006, 2007, 2012	< 1,600	< 300	< 3,200
1 - 2.4	Moderate	2003, 2004	1,600 – 2,500	300 – 450	3,200 – 5,000
2.4 - 6	Severe	2008, 2009, 2010	2,500 – 3,500	450 – 630	5,000 – 7,000
> 6	Extreme	2011	> 3,500	> 630	> 7,000

Target are for the March to June period and annually, Harmful algal bloom extent is expressed as the Cyanobacterial Index, CL.
Image source: International Joint Commission Report (IJC 2014)

This study focuses on the formations of HABs in the past nine years that have drawn increasing attention to the problem in Western Lake Erie. From the year 2000 to 2007, some mild cases appeared that did not cause alarm. However, starting from 2008 to 2010, the Basin experienced its first severe cases, and since 2011, there have been five years of extreme cases. As the first IJC report on HABs categories from 2002 to 2011 shows, the total phosphorus loading is reported to come from the Maumee River and the Western Lake Erie Basin, shown in Table 1 (IJC 2014). A similar report was done from the year 2002 to 2017 by The Great Lakes Environmental Research Laboratory (Spring 2017). GLERL's report is shown in Graph 1. In August 2014, a smaller bloom contaminated Toledo's water supply, leading to a two-day drinking water shutoff that affected nearly 500,000 people (Wines 2014; Chapra et al. 2017).

Graph 4.1
Harmful Algal blooms from 2002 to 2017



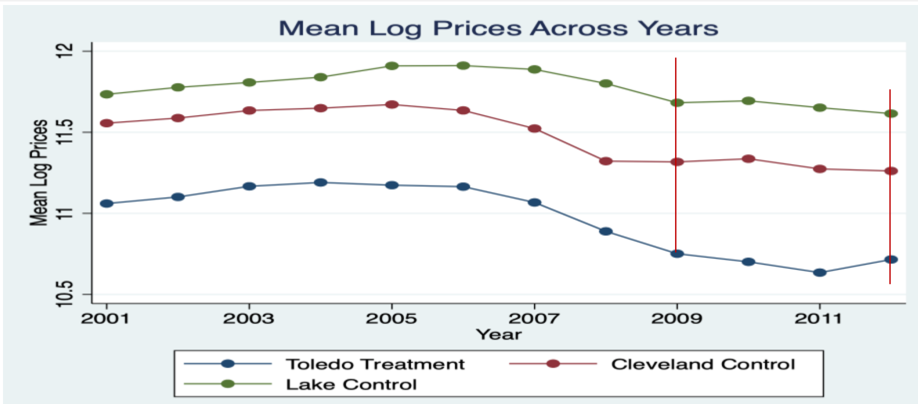
The evolution of harmful algal bloom levels in Lake Erie across years
Source: National Oceanic and Atmospheric Administration

There is randomness in the way that HABs spread around the lake that is associated not with housing prices, but with water currents. The amount of phosphorus entering the lake and its tributaries are from unregulated nonpoint sources mainly from upstream agriculture and cattle runoff. HABs are also supported by climatic factors of yearly precipitation and climate warming, resulting in summertime explosion of cyanobacteria growth concentrated in the shallow warm waters of Western Lake Erie (Wells et al. 2015, Pearl et al. 2011, Azevedo el at 2002).

4.3 Summary statistics

In the first stage of the analysis, the study areas consist of three counties near the shore of Lake Erie, Ohio. The treatment group is the city of Toledo, and the control groups are Lake County and the city of Cleveland. Figure 1 shows the locations of each county, and the green regions within the water are indicative of a summer hypoxia. Lake County and Cleveland were selected because these locations lie outside of the Western Lake Erie Basin, just east of the furthest reach of recent HABs. Furthermore, prior to the HABs outbreaks, the real estate markets in Cleveland, Lake and Toledo exhibited similar trends, although mean prices differ from one location to the next (Graph 4.2).

Graph 4.2
Annual Price Trends



Graph contains mean log yearly prices across years for each county studied. Vertical red lines indicate years of HABs outbreaks from severe in 2008 to extreme cases in 2010.

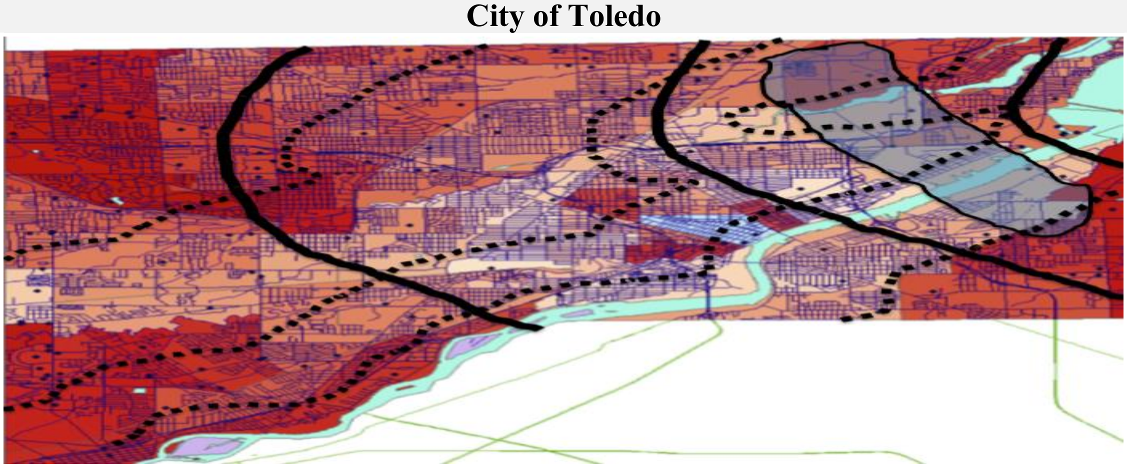
The study compiles data on property transactions between 2001 and 2015 from the county assessor's offices of the city of Toledo (Lucas County), the city of Cleveland (Cuyahoga County) and Lake County. Each lot in the database is georeferenced, so that I can measure the distance of each property to the lake and to river tributaries, which are the primary source of pollution runoff.

In the housing price data, I eliminated observations labeled annexes, combined property, and split property. I also eliminated observations for foreclosed properties, where the buyer and the seller were the same people, and when properties were sold more than once in a year or labeled as "house flip." I dropped observations from parcels that did not match a transaction, such as transactions that occurred in the year of construction, houses built before 1800, houses with 23 rooms or more, and houses with zero bedrooms, bathrooms, and living areas. To eliminate outliers in the price data, I dropped observations with prices that were two standard deviations away from the mean.

The final sample includes 369,041 total transactions, shown in the summary statistics of Table A4.1 in the Appendix. Table A4.1 shows that houses cost $127,000 on average; they were an average of 9.35 kilometers from the Lake and 6.8 kilometers from the tributaries. On average, the houses sold were built in 1954 and had 6.5 rooms, 1.6 stories, 3.1 bedrooms, and 1.5 bathrooms. The mean common living area was 1,652 sq. ft. and the average lot size was 10,843 sq. ft. In Tables A4.2.2, A4.2.3, we can see similar statistics divided into the treatment group, Toledo City, and the control groups, Cleveland City and Lake County.

While we might assume that housing prices would increase closer to the lake and the tributaries, in Toledo house prices are heterogeneous along the Tributary shore and the lake, and there is an industrial area located between the second and the sixth kilometers away from the lake. Figure 4.2 shows higher prices depicted by darker colors, and a circle depicts the industrial area. Controlling for those mismatches in the data is essential to pick up the time-invariant effects of each location.

Figure 4.2
City of Toledo

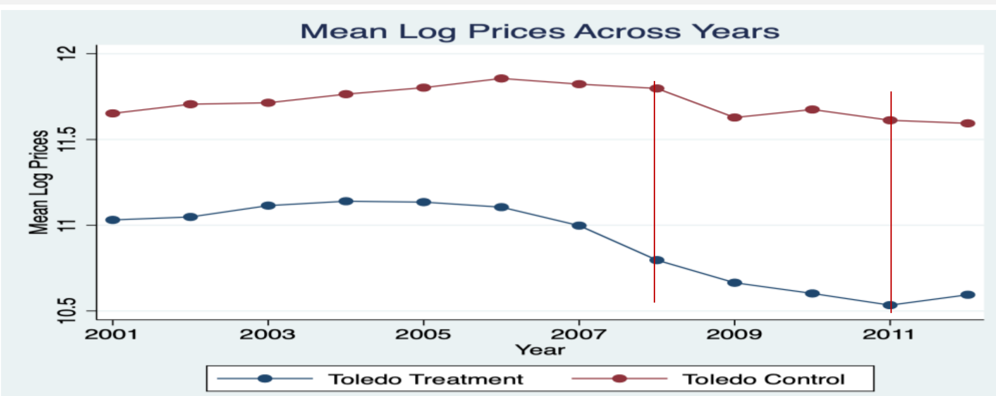


Lake Erie is located in the upper-right corner. The tributary above is the Ottawa River, and the tributary bellow is the Maumee River. Higher house prices are depicted by a darker red color, and the shaded, cicumscribed area closer to the lake is an industrial area.

The second stage of the analysis uses the area within Toledo not affected by HABs as a control group. The data from Toledo, as shown in Table A4.1, has 29 kilometers measured from the lake and 23 kilometers measured from the lake and the tributaries. Hence, I divided the data for the Treatment and Control groups by these two different measurements into two different analyses. Table A4.2 and A4.3 show the first control group starts at Kilometer 14 when the analysis uses the distance from the lake, and the second control group starts at Kilometer 9 when the analysis uses the distance from the lake and the Tributary.

Within the same city, the treatment and control groups are more comparable. Graph 4.3 shows that the treatment and control price trends using both the lake and tributary measurements are very similar. However, this does not mean that unobservable are the same. There can be essential differences in neighborhoods within a city that are associated with their distance to the Lake. For this reason, I pre-process the data using matching. Finally, we can also look at the trends by the average housing prices relative to the distances to the Lake and the tributaries. Graph A4.1 in the Appendix shows the average price trends before and after HABs started and the comparison between Toledo, Cleveland, and Lake County.

Graph 4.3
Annual Price Trends



Graphs contain mean log yearly prices across years for the areas affected and unaffected by HABs within Toledo. Vertical red lines indicate years of HABs outbreaks from severe cases starting in 2008 and extreme cases in 2011.

4.4 Identification strategy

4.4.1 Estimation strategy

In the first stage, I compare the change in housing prices of Toledo (exposed to algae blooms) before and after the blooms occurred relative to the change in housing prices in Cleveland (Cuyahoga County) and Lake County (not exposed to algal blooms) across the same period, and by calculating the spatial effect of prices, as houses are located farther away from the pollution source. In the second stage, I use the areas within Toledo not affected by HABs as the control group and use the same econometric techniques to calculate the change in price over time in the affected areas. The basic specification compares Toledo City to Cuyahoga and Lake County as a whole, using the following equation:

1) $\boldsymbol{LnP_{hit} = \alpha + \beta_1 WLEC_{hi} + \beta_2 HAB_t + \beta_3 WLEC_{hi}\, HAB_t} + X_{it}\Gamma + \boldsymbol{Y_t + \mu_i + \epsilon_{hit}}$

The dependent variable $\boldsymbol{ln}$ $\mathbf{P_{hit}}$ is the log of residential housing prices for the house characteristic index by (h), census tract fixed effect index by (i), and the time effect by (t). The first three variables reflect the basic difference-in-differences approach to estimating an average treatment effect where $\boldsymbol{\beta_3 WLEC_{hi}\, HAB_t}$ is the variable of interest that identifies the treatment effect. The treatment group is the dummy variable for Western Lake Erie County $\boldsymbol{WLEC_{hi}}$, where 1 is a treatment county, and 0 is a control county. $\boldsymbol{HAB_t}$ is the treatment effect dummy variable for the period before and after HABs appeared, where 1 represents the post-treatment period starting on September 1, 2008, or 2011, or both in the same regression, and 0 indicates all other periods before. Two-way fixed effects at the census tract and year levels are present in all regressions, and $\boldsymbol{Y_t}$ are multiple parameters of year-specific dummy variables from 2001 to 2015 to be estimated. This fixed effect is primarily a price trend estimator used to control for any macro-level shock in the system, as it is crucial to control for the financial crisis that started in 2007. $\mathbf{X_{it}}$ gives house-specific structural attributes, and the covariates controlling for neighborhood-specific characteristics and spatial mismatches between groups are controlled by census-tracts or house fixed effects given by $\boldsymbol{\mu_i}$. $\boldsymbol{\epsilon_{hit}}$ is the idiosyncratic error term with robust standard errors clustered by census tracts, while $\alpha, \boldsymbol{\beta}, \boldsymbol{and}\ \boldsymbol{\Gamma}$ are the vectors of the parameters to be estimated.

The first estimation uses the difference of the average change over time of the county as a whole for the treatment and control groups but does not allow us to see the effect of variation in the level of exposure to HABs. For the second estimation, I introduce a triple difference by finding the different effects at different distances, as houses are located farther away from the Lake and the tributaries, using the following sample:

$$\text{2)}\quad \ln \boldsymbol{P^{j}_{hit}} = \boldsymbol{\alpha} + \boldsymbol{\beta_1 WLEC_{hi}} + \boldsymbol{\beta_2 HAB_t} + \boldsymbol{\beta_3 WLEC_{hi}\, HAB_t} + \sum_{j=1}^{29} \boldsymbol{\rho_J\, d^{j}_{hi} WLEC_{hi}\, HAB_t}$$
$$+ \sum_{j=1}^{29} \boldsymbol{\delta_J\, d^{j}_{hi}\, WLEC_{hi}} + \sum_{j=1}^{29} \boldsymbol{\theta_J\, d^{j}_{hi}} + X_{it}\boldsymbol{\Gamma} + \boldsymbol{Y_t} + \boldsymbol{\mu_i} + \boldsymbol{\epsilon_{hit}}$$

This approach allows us to see the difference in price among houses as the distance from the lake increases by adding a vector of distance-specific dummy variables to the first equation. Instead of treating distance as a continuous variable, I use distance categories to denote specific ranges. Each distance category is made up of a one-kilometer region on the periphery of the Lake at successively greater distances from the Lake, with a total of 29 categories. The distances variables are $\sum_{j=1}^{29} \boldsymbol{\theta_J}\, d^{j}_{hit}$ where distance category is indexed by *j* kilometers, and (i) denotes whether the distance is measured from the Lake or the Tributary. The 29 parameters $\sum_{j=1}^{29} \boldsymbol{\delta_J}\, d^{j}_{hi}\ WLEC^{j}_{hi}$ is the interaction between the distance vector and the treatment dummy variable. Most importantly, the interaction between the dummy treatment group, the dummy before and after treatment effect, and the dummy distance-vector, $\sum_{j=1}^{29} \boldsymbol{\rho_J}\, d^{j}_{hi}\ WLEC_{hi}\, HAB_t$ become our key variables of interest created by the difference-in-difference-in-difference effect at every distance point.

4.2 Covariate Balance

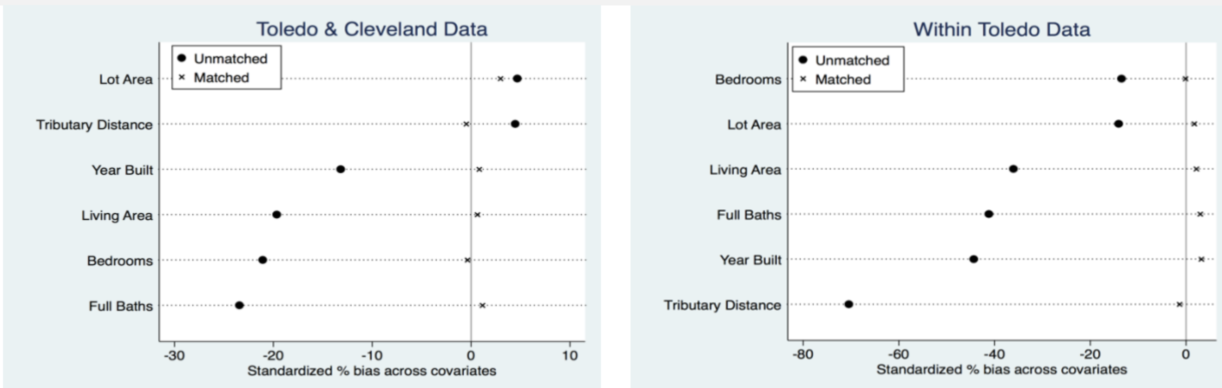
The three counties share geopolitical, geographic, demographic, and housing characteristics. Nonetheless, some characteristics are not comparable among those groups. As Figure 4.2 illustrates, the waterfront zoning characteristics are not utilized in the same way; there is a large industrial area close to the short in Toledo and a large port in Cleveland. Demographically, there are observable differences in the crime index, unemployment, and poverty rates, shown in Table 4.2. One significant challenge in the study is that random assignments are not feasible because waterfront homeowners self-select themselves into a high-priced market. Hence, systematic differences between groups may arise and may be partially reflected in the covariance. I use propensity score weighting and matching methods to address this issue (Austin 2009, Cattaneo 2010; Guo & Fraser 2015).

Table 4.2
City demographic characteristics from city-data.com 2014

	Toledo	Cleveland	Lake
Population	278,512	385,810	19840
Median resident age	35.4	36.4	32.8
Median household income	$35,301	$27,551	$42.980
Median house value	$79,100	$66,800	$101,375
High school or higher	86.3%	81.2%	79.2%
Unemployed	4.9%	6.3%	4.2%
Crime index	567.1	847.4	199.8
Density	3,455 p/m	4,973 p/m	3,320 p/m
Poverty	26.3%	35.0%	21.7%
Median gross rent	$650	$669	$753
Cost of living index	92.4	94.0	94.3

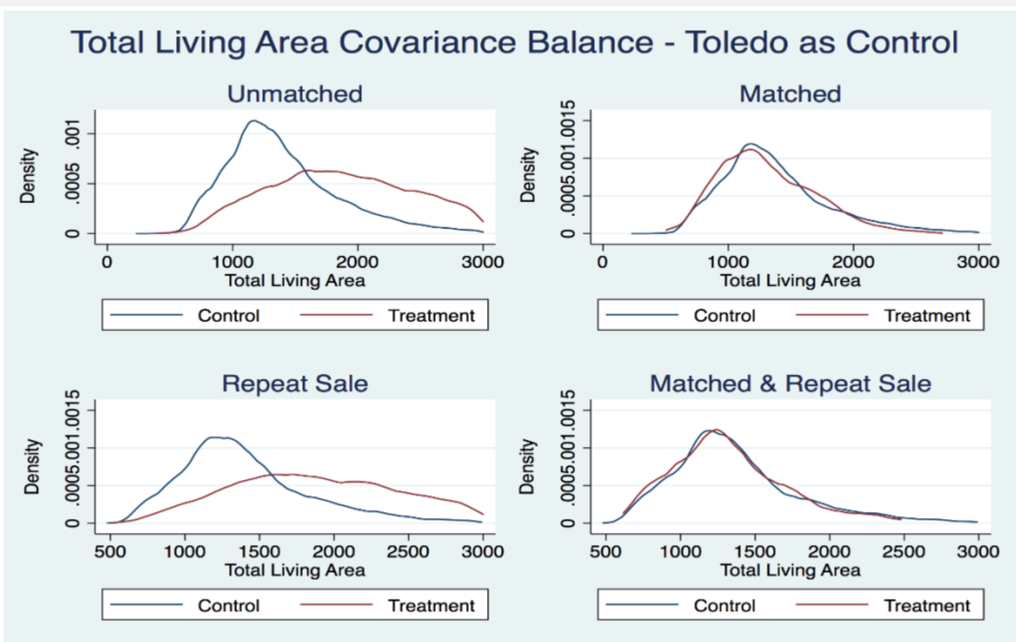
The matching covariates are the number of bedrooms, baths, year build, total living area, distance to the river and lake, and prices. I use four different methods to test for covariate balance. First, I apply a standard generalized method of moments test (Imai and Ratkovic, 2014). This test rejects the null hypothesis that indicates that the treatment model balanced the covariates. The matching model improves the level of balance as weight-standardized differences appear closer to zero, and the variance ratios are mostly closer to one (Table A4.4). Second, the matched data shows a substantial bias reduction for all variables used (Graphs 4.4 and Table A4.5).

Graph 4.4
Test for Covariate Balance



Graphs contains the standard generalized method of moments test which show the bias across covariates before and after matching

Graph 4.5
Covariance Balance



Graphs contains the differences in covariance balance between unmatched, matched, repeat sale, and matched and repeat sales simultaneously.

Third, the normalized differences in means are all less than 0.25 (Imbens and Wooldridge, 2009) in the post-matched dataset (Table A4.5 and A4.7). Finally, the improvement of covariate balance is also shown graphically in Graph 4.5 below and Graph A4.2.

4.4.3 Parallel Price Pre-Trend Assumption

This design assumes that the control groups would be comparable to the treatment groups in the absence of treatment. This section presents tests on the pre-HABs data to see if they suggest that this assumption holds. The estimation equation is:

3) $$\boldsymbol{LnP_{hit} = \alpha + \beta_9\, WLEC_{hi} + \sum_{t=1}^{8} \delta_t\, Y_t * WLEC_{hi}} + X_{it}\boldsymbol{\Gamma + Y_t + \mu_i + \epsilon_{hit}}$$

This approach allows us to see the differences in the change in prices between the affected and counterfactual groups across time using only data prior to the first extreme outbreak. As above, the dependent variable is $\boldsymbol{ln}$ $\mathbf{P_{hit}}$ the log of residential housing prices, the treatment group is again the Western Lake Erie County $\boldsymbol{WLEC_{hi}}$, but $\boldsymbol{Y_t}$ becomes the test for the difference in trend prior to the treatment, which are the year-specific dummy variables from 2001 to 2008. In the regression 2001 is the omitted year. Then $\boldsymbol{\sum_{t=1}^{8} \delta_t\ Y_t\ WLEC_{hi}}$ are the terms of intertest that identify if there was a statistically significant change in prices before 2008

Table 4.3

Parallel pre-trend test using Cleveland area as control

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
Toledo price effect in 2002	-0.063***	-0.24**	-0.073***	-0.031	0.41**
	(0.02)	(0.11)	(0.03)	(0.04)	(0.18)
Toledo price effect in 2003	-0.069**	-0.15	-0.081**	-0.050	-0.19
	(0.03)	(0.12)	(0.03)	(0.04)	(0.17)
Toledo price effect in 2004	-0.047	-0.21**	-0.029	-0.019	-0.62**
	(0.03)	(0.10)	(0.02)	(0.04)	(0.28)
Toledo price effect in 2005	-0.059***	-0.041	-0.054***	-0.043	0.050
	(0.02)	(0.12)	(0.02)	(0.04)	(0.31)
Toledo price effect in 2006	-0.0075	0.010	-0.010	0.000092	-0.075
	(0.01)	(0.14)	(0.03)	(0.03)	(0.24)
Toledo price effect in 2007	0.042*	0.040	0.064*	0.13***	-0.23
	(0.02)	(0.13)	(0.04)	(0.03)	(0.17)
Observations	303551	39158	303551	89178	13371
Adjusted r-squared	0.447	0.480	0.135	0.194	0.605
Ordinary least squares (OSL)	Y	Y	N	N	N
Fixed effects (FE)	N	N	Y	Y	Y
Repeat sales (RS)	N	N	N	Y	Y
Nearest-neighbor matching(nm)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & year dummies time trend. Nearest-neighbor matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* $p<0.10$.
** $p<0.05$.
*** $p<0.01$.

Table 4.3 suggests that the Ordinary Least Squares (OLS) does not support a parallel pre-trend assumption given that 5 out of 7 years show a statistically significant change in prices between the treatment and the control groups. The FE and the Repeat Sale models suggest a great improvement by exposing only one or two year with a significant change in prices. Nevertheless, the Nearest Neighbor Matching model and the combination of all models fully support the assumption at every year. Hence, these results suggest that the use of FE, Repeat Sales, and Nearest Neighbor Matching provides better counterfactuals than OLS. This observation notwithstanding, when looking at the difference in prices among affected and non-affected areas within Toledo City in Table A4.8, I find that only the combination of all models does not support a parallel pre-trend assumption on their own, with 4 out of 6 years exhibiting statistically significant discrepancies in prices before 2008. The rest of models fully support the parallel trend assumption at every year. In this way, the pre-trend test suggests that matching is required to eliminate discrepancies in pre-trends.

4.5 Results

Tables 4.4, 4.5, and 4.6 show the results from the comparison across all three cities, while Table 4.7 shows the estimation that uses Toledo's non-affected areas as the control group. They are each divided into five columns. Column 1 uses OLS. Column 2 uses the matched data with OLS. Column 3 introduces the fixed effects estimator, and Column 4 restricts the data to houses sold repeatedly during the study period. In the last column, I combine all models. The data is trimmed at each step, starting with 233,234 observations using the OLS and using Cleveland as a control group and ending with 19,379 observations when using a combination of all the models with Lake County as the control group. All regressions contain robust standard errors clustered by census tracts, house-specific variables, two-way fixed effect, and a time trend.

4.5.1: Cross-city results

Table 4.4 contains the change in housing prices as a whole, between Toledo and Cleveland. Results using Lake County are in the Appendix in Table A4.9. Both tables are divided into three sections to show the results when the treatment effect is in 2008, 2011, and both years simultaneously. The point estimates range from an 8 to 19 percent decline in house values in Toledo City. The results are similar across all models, when changing the control group, and when [altering or accounting for] the year the relevant HAB was detected.

Table 4.4

Cleveland as control with treatment effects in 2008 & 2011

	(1) Ordinary least squares	(2) Nearest neighbor matching (NNM)	(3) Fixed effects	(4) Fixed effect & repeat sales	(5) Fixed effects, repeat sales & NNM
	Treatment effect in 2008				
Treatment effect in 2008	-0.10**	-0.14***	-0.12***	-0.19***	-0.16***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Observations	233234	38863	233234	67955	19379
Adjusted r-squared	0.458	0.503	0.272	0.269	0.659
	Treatment effects in 2011				
Treatment effect in 2011	-0.057	-0.095***	-0.084**	-0.14***	-0.13***
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)
Observations	233234	38863	233234	67955	19379
Adjusted r-squared	0.457	0.502	0.271	0.266	0.658
	Treatment effects in 2008 & 2011				
Treatment effect in 2011	-0.082*	-0.15***	-0.11***	-0.19***	-0.19***
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
Treatment effect in 2008	-0.13***	-0.12***	-0.13***	-0.18***	-0.13***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Observations	233234	38863	233234	67955	19379
Adjusted r-squared	0.458	0.503	0.272	0.269	0.659
Ordinary least squares (OSL)	Y	Y	N	N	N
Fixed effects (FE)	N	N	Y	Y	Y
Repeat sales (RS)	N	N	N	Y	Y
Nearest-neighbor matching(nm)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-neighbor matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* p<0.10.
** p<0.05.
*** p<0.01.

Tables 4.5 and 4.6 show results using binary measures of distance, which exploits the spatial treatment effect of prices in distance categories, conditional on distance from the pollution source. Tables 4.5 uses the distance from houses to Lake Erie. The distances are depicted in Figure 4.2 by the lines starting from the right-hand side of the picture. Starting from the first kilometer away from the Lake, the triple difference model finds statistically significant declines in prices in the first two kilometers and after the fifth kilometer. The statistically insignificant areas after the second to the fourth kilometer are marked in Figure 4.2 and Graph A4.1 in the Appendix as the industrialized zone. Anywhere else, I find price declines to be highly statistically significant between 4 to 43 percent. I also find that the negative impact of HABs declines rapidly at the fourteenth kilometer, and the HABs' effects stay insignificant after that. Finding the non-affected areas within Toledo is essential in order to use these areas as a control group in the second section of the study.

Table 4.5
Cleveland as control with treatment effects in 2008 per distance away from Lake Erie

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
Treatment effects at 1 km	-0.11*** (0.02)	-0.040 (0.04)	-0.070*** (0.02)	-0.096** (0.04)	-0.14*** (0.05)
Treatment effects at 2 km	-0.014 (0.03)	-0.017 (0.04)	0.022 (0.03)	-0.026 (0.05)	-0.090* (0.05)
Treatment effects at 3 km	0.19*** (0.07)	0.13 (0.09)	0.19*** (0.05)	-0.041 (0.10)	-0.068 (0.11)
Treatment effects at 4 km	0.014 (0.05)	0.060 (0.08)	-0.038 (0.03)	-0.055 (0.07)	-0.057 (0.09)
Treatment effects at 5 km	-0.16*** (0.04)	-0.22*** (0.05)	-0.21*** (0.03)	-0.41*** (0.05)	-0.43*** (0.07)
Treatment effects at 6 km	0.0022 (0.03)	-0.14*** (0.04)	-0.069*** (0.02)	-0.14*** (0.04)	-0.19*** (0.05)
Treatment effects at 7 km	-0.22*** (0.02)	-0.31*** (0.03)	-0.24*** (0.01)	-0.39*** (0.03)	-0.41*** (0.03)
Treatment effects at 8 km	-0.29*** (0.02	-0.33*** (0.03)	-0.30*** (0.01)	-0.37*** (0.03)	-0.41*** (0.04)
Treatment effects at 9 km	-0.31*** (0.02)	-0.43*** (0.03)	-0.32*** (0.02)	-0.46*** (0.03)	-0.53*** (0.04)
Treatment effects at 10 km	-0.19*** (0.02)	-0.36*** (0.03)	-0.26*** (0.02)	-0.36*** (0.03)	-0.42*** (0.04)
Treatment effects at 11 km	-0.081*** (0.02)	-0.19*** (0.02)	-0.13*** (0.01)	-0.16*** (0.02)	-0.21*** (0.03)
Treatment effects at 12 km	-0.16*** (0.02)	-0.20*** (0.02)	-0.16*** (0.01)	-0.17*** (0.02)	-0.24*** (0.03)
Treatment effects at 13 km	-0.16*** (0.02)	-0.18*** (0.02)	-0.16*** (0.02)	-0.18*** (0.03)	-0.23*** (0.03)
Treatment effects at 14 km	-0.082*** (0.02)	-0.051** (0.02)	-0.058*** (0.02)	-0.045 (0.03)	-0.076** (0.03)
Treatment effects at 15 km	0.010 (0.02)	-0.023 (0.03)	0.040** (0.02)	0.020 (0.03)	-0.038 (0.03)

Treatment effects at 16 km	-0.031*	-0.041	0.015	-0.00022	-0.069**
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Treatment effects at 17 km	-0.031*	-0.036	-0.0093	0.0046	-0.052
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Treatment effects at 18 km	0.036*	0.059**	0.082***	0.062*	0.00027
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Treatment effects at 19 km	0.12***	0.14***	0.17***	0.11***	0.054*
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Observations	324646	53866	300208	89178	24639
Adjusted r-squared	0.484	0.572	0.253	0.257	0.682
Ordinary least squares (OSL)	Y	Y	N	N	N
Fixed effects (FE)	N	N	Y	Y	Y
Repeat sales (RS)	N	N	N	Y	Y
Nearest-neighbor matching(nm)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-neighbor matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake
* p<0.10.
** p<0.05.
*** p<0.01.

Tables 4.6 use the distance from houses to the lake and the tributary as if they were a single body mass. This measurement is depicted in Figure 4.2 by the dotted line going away from all bodies of water. From waterfront houses to the ninth kilometer, the triple difference model finds a highly statistically significant decline at every kilometer away from the tributaries and the lake, with an exception of the eighth kilometer. I also find that the significant negative impact disappears after the tenth kilometer, giving us the area not affected by HABs in Toledo when using the distance from the houses and the tributaries. In sum, under all models, I find statistically significant prices declines between 7 to 49 percent. Similar results using Lake County as the control group are in Tables A410 and A4.11 in the Appendix.

Table 4.6

Cleveland as control with treatment effects in 2008 using distance from lake and its tributaries

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
Treatment effects at 1 km	-0.086***	-0.13***	-0.14***	-0.22***	-0.22***
	(0.02)	(0.02)	(0.01)	(0.02)	(0.04)
Treatment effects at 2 km	-0.36***	-0.40***	-0.34***	-0.47***	-0.47***
	(0.02)	(0.03)	(0.01)	(0.03)	(0.05)
Treatment effects at 3 km	-0.10***	-0.26***	-0.18***	-0.26***	-0.29***
	(0.02)	(0.03)	(0.01)	(0.03)	(0.05)
Treatment effects at 4 km	-0.023	-0.12***	-0.096***	-0.11***	-0.10**
	(0.02)	(0.03)	(0.01)	(0.03)	(0.05)
Treatment effects at 5 km	-0.10***	-0.20***	-0.13***	-0.25***	-0.28***
	(0.02)	(0.02)	(0.01)	(0.03)	(0.04)
Treatment effects at 6 km	-0.29***	-0.29***	-0.26***	-0.34***	-0.36***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.04)
Treatment effects at 7 km	-0.16***	-0.16***	-0.15***	-0.16***	-0.18***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.04)

Treatment effects at 8 km	-0.017	0.0064	0.021	0.022	-0.00047
	(0.01)	(0.02)	(0.01)	(0.02)	(0.04)
Treatment effects at 9 km	-0.058***	-0.048**	-0.025*	-0.055**	-0.072*
	(0.01)	(0.02)	(0.01)	(0.02)	(0.04)
Treatment effects at 10 km	0.012	0.028	0.043***	0.020	0.0025
	(0.01)	(0.02)	(0.02)	(0.03)	(0.04)
Treatment effects at 11 km	0.053***	0.048**	0.11***	0.018	0.0049
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Treatment effects at 12 km	0.15***	0.17***	0.19***	0.21***	0.18***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Observations	324646	53866	300208	89178	24639
Adjusted r-squared	0.488	0.554	0.248	0.249	0.662
Ordinary least squares (OSL)	Y	Y	N	N	N
Fixed effects (FE)	N	N	Y	Y	Y
Repeat sales (RS)	N	N	N	Y	Y
Nearest-neighbor matching(nm)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-neighbor matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* p<0.10.
** p<0.05.
*** p<0.01.

4.5.2 Within-city results

This section compares prices within Toledo, using as control those properties farther than 10 kilometers from the lakeshore. Table 4.7 shows point estimates of HABs-related declines in house values from 9 to 21 in Toledo City when using the distance to Lake Erie, and from 12 to 25 percent when using the distance from the tributaries and Lake Erie simultaneously, shown in the Appendix Table A4.12. These results are also similar across all models, when changing the distance used in both parts, and the year HAB started, making our results robust across all examples. Most importantly, these results are similar to the overall declines found in section one shown in Table 4.4, validating both results.

Table 4. 7

Distance from Lake Erie using the non-affected areas within Toledo city as control with treatment effects in 2008 & 2011

	(1) Ordinary least squares	(2) Nearest neighbor matching (NNM)	(3) Fixed effects	(4) Fixed effect & repeat sales	(5) Fixed effects, repeat sales & NNM
		Treatment effect in 2008			
Treatment effect in 2008	-0.38***	-0.14***	-0.43***	-0.57***	-0.10**
	(0.09)	(0.04)	(0.09)	(0.12)	(0.05)
Observations	78114	38544	78114	22889	19325
Adjusted r-squared	0.462	0.467	0.227	0.256	0.642
		Treatment effects in 2011			
Treatment effect in 2011	-0.32***	-0.044	-0.36***	-0.44***	-0.037

	(0.07)	**(0.04)**	**(0.07)**	**(0.09)**	**(0.05)**
Observations	**78114**	**22986**	**78114**	**22889**	**10905**
Adjusted r-squared	**0.456**	**0.472**	**0.212**	**0.224**	**0.656**
	Treatment effects in 2008 & 2011				
Treatment effect in 2011	**-0.40*****	**-0.22*****	**-0.46*****	**-0.60*****	**-0.18*****
	(0.09)	**(0.05)**	**(0.09)**	**(0.12)**	**(0.06)**
Treatment effect in 2008	**-0.35*****	**-0.12*****	**-0.39*****	**-0.52*****	**-0.077**
	(0.08)	**(0.04)**	**(0.08)**	**(0.11)**	**(0.05)**
Observations	**78114**	**38572**	**78114**	**22889**	**19333**
Adjusted r-squared	**0.462**	**0.467**	**0.227**	**0.256**	**0.642**
Ordinary least squares (OLS)	**Y**	**Y**	**N**	**N**	**N**
Fixed effects (FE)	**N**	**N**	**Y**	**Y**	**Y**
Repeat sales (RS)	**N**	**N**	**N**	**Y**	**Y**
Nearest-neighbor matching(nm)	**N**	**Y**	**N**	**N**	**Y**

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-neighbor matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
*** p<0.10.**
**** p<0.05.**
***** p<0.01.**

4.8 Conclusion

After testing the data for pre-trends and carefully isolating the effects of harmful algal blooms by restricting the data from different locations, with distinct models, and with distinct control groups, the study finds robust results across all data configurations and methods. Estimates show a decline in house prices of 9 to 21 percent in 2008 and 2011, respectively. These results here provide evidence in support of the International Joint Commission, which estimated that between 24,000 and 210,000 properties could be affected by HABs between 1.6 and 16 km inland (IJC 2014). A small bloom that contaminated Toledo's water supply in 2014 required a shutoff of potable water over a two-day period. The shutoff affected nearly 500,000 people and demonstrated that the impact of algal blooms is not confined to individuals who live close to contaminated sources. The results also suggest that if 210,000 houses have been affected by HABs, and the average present value per home is 105,071 dollars, then the total current value of the residential properties in Toledo is 22.2 billion dollars. However, in the absence of HABs, the total present value would be around 13 percent higher, with a value of 25 billion dollars. This suggests that the overall loss in house values is close to 3 billion dollars.

Externalities produced by land use runoff are harming downstream ecosystems, neighborhoods, and livelihoods; therefore, it is crucial to quantify their cost to provide evidence-based recommendations to stakeholders. However, this analysis only considers impacts on a single indicator, housing prices. The study does not consider the full range of ecosystem services losses, which include impacts to resources like drinking water and fisheries, the compromising of tourism and recreation, the loss of biodiversity, and the loss of existence value of an unpolluted lake. Nonetheless, these results accurately reflect the decrease in housing values due to HABs, and this value loss serves as an estimate for the value placed by homeowners in Toledo on the location-specific ecosystem services compromised by the HABs. Furthermore, this study contributes to methodologies for quantifying the cost of externalities by using multiple control groups to establish counterfactual trends. It provides a step towards understanding the impacts of eutrophication on the broader value of the ecosystem services. Finally, estimates of the housing value impact of environmental damages can help guide policymakers in their development of systems to pressure polluters to limit contamination of the watershed.

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6 Appendix

Appendix A

Table A2.1 starts by exploring the differences in demographics that do not change significantly over time, such as the percentage of the rural population, ethnic composition of counties, and relative percentages of citizen and foreign-born populations. Then the table shows the differences in unemployment, average income by ethnicities, median earnings by school attainment, per capita income, percentage of working poor, and the Gini Index of Inequality. These comparisons between SC, ICE, and NC, use the mean before and after 2013.

Table A2.1:
Descriptive statistics among sanctuary counties (SC), ICE counties, and neutral counties (NC).

	Mean Before 2013			Mean After 2013		
	SC	ICE	NC	SC	ICE	NC
Variable	Mean	Mean	Mean	Mean	Mean	Mean
Observations	994	958	3549	710	718	2,790
Total Mean Population	606652.90	363969.50	245357.40	625667.70	356486.50	232287.80
% Rural Population	0.74	0.83	0.89	0.74	0.81	0.89
% White Population	73.46	80.30	81.05	72.76	80.20	80.90
% Latin Population	21.57	15.32	11.02	22.54	15.73	14.36
% Citizen by Birth	85.45	91.35	94.09	85.37	91.42	93.98
% Foreign Born	14.55	8.65	5.91	14.63	8.58	6.02
% Unemployment	5.43	4.45	4.56	4.66	4.17	4.32
% Women Unemployed	4.79	4.00	4.06	4.17	3.85	3.96
% White Unemployed	7.77	6.14	6.47	6.70	5.82	6.18
% Latino Unemployed	7.22	4.04	2.30	6.35	3.84	2.15
% Black Unemployed	6.63	7.13	6.41	5.90	6.50	5.89
$ Med. Family Income	68582.80	65977.90	63575.23	72910.59	68377.13	65288.18
$ White Ave. Income	79853.49	74651.13	70469.21	85676.80	77561.83	72562.27
$ Latino Ave. Income	58211.46	53930.03	56108.42	63659.53	56254.11	57810.13
$ Black Ave. Income	26742.61	28631.59	21766.83	27799.75	27091.27	20197.11
$ Med. Earnings	36337.13	35193.91	34292.18	37980.18	36084.53	35216.59
$ Med. Ear. No High School	20529.61	20765.23	20391.03	21767.37	21222.81	21296.95
$ Med. Ear. High Sch.	28442.09	27939.58	27861.09	29451.17	28612.71	28402.48
$ Med. Ear. Some College	34474.61	33594.31	32919.90	35294.28	33848.65	33545.93
$ Med. Ear. College	48901.69	46877.18	45294.58	50968.28	47931.52	46308.68
$ Med. Ear. Grad	64876.84	59941.41	58358.18	67352.04	61574.88	59636.48
$ Per Capita Income	28773.84	27312.25	26105.04	30656.21	28141.94	26901.12
% Working Poor	13.81	12.62	13.45	14.02	12.97	13.87
Gini Index	0.45	0.44	0.44	0.46	0.45	0.44

Table A2.2
Pre-Trend Test using Natural Log of Labor Force Unemployment

	Ordinary Least Squares		Fixed Effects	
Sanctuary County in 2007	0.034	(0.06)	0.047	(0.07)
Sanctuary County in 2008	-0.016	(0.04)	0.0044	(0.03)
Sanctuary County in 2009	-0.031	(0.05)	-0.011	(0.04)
Sanctuary County in 2010	-0.0021	(0.07)	0.0071	(0.07)
Sanctuary County in 2011	0.058	(0.09)	0.074	(0.09)
Sanctuary County in 2012	0.046	(0.12)	0.064	(0.12)
Sanctuary County in 2013	0.052	(0.10)	0.055	(0.11)
ICE County in 2007	-0.11*	(0.06)	-0.093	(0.06)
ICE County in 2008	-0.13**	(0.06)	-0.14**	(0.06)
ICE County in 2009	-0.028	(0.08)	-0.0100	(0.07)
ICE County in 2010	0.038	(0.06)	0.075	(0.06)
ICE County in 2011	-0.032	(0.05)	-0.0014	(0.05)
ICE County in 2012	-0.065	(0.07)	-0.048	(0.06)
ICE County in 2013	-0.017	(0.04)	0.016	(0.05)
Observations	9296		9296	
Adjusted R-squared	0.112		0.068	
Pre-trend test for sanctuary counties, using early adopter as counterfactual				
Sanctuary County in 2007	-0.15	(0.16)	-0.19	(0.16)
Sanctuary County in 2008	-0.058	(0.15)	-0.079	(0.14)
Sanctuary County in 2009	-0.0050	(0.14)	-0.00029	(0.13)
Sanctuary County in 2010	-0.070	(0.14)	-0.025	(0.13)
Sanctuary County in 2011	-0.082	(0.11)	-0.035	(0.11)
Sanctuary County in 2012	-0.12	(0.15)	-0.10	(0.13)
Sanctuary County in 2013	0.064	(0.15)	0.087	(0.13)
Observations	1841		1841	
Adjusted R-squared	0.165		0.099	
Pre-trend test for ICE counties, using late policy adopter as counterfactual				
ICE County in 2007	-0.037	(0.08)	-0.0024	(0.07)
ICE County in 2008	0.070	(0.11)	0.10	(0.08)
ICE County in 2009	0.028	(0.10)	0.030	(0.08)
ICE County in 2010	-0.095	(0.11)	-0.080	(0.10)
ICE County in 2011	0.016	(0.10)	0.039	(0.08)
ICE County in 2012	0.056	(0.09)	0.076	(0.07)
ICE County in 2012	-0.066	(0.08)	-0.071	(0.07)
Observations	1773		1773	
Adjusted R-squared	0.078		0.067	

Standard errors in parentheses * p<0.10, ** p<0.05, *** p<0.01 All regressions contain robust standard errors clustered by county ID, time trends and time variant covariates.

Table A2.3
Pre-Trend Test using Natural Log of Real GDP

	Ordinary Least Squares		Fixed Effects	
Sanctuary County in 2007	0.0012	(0.01)	-0.00011	(0.01)
Sanctuary County in 2008	-0.0083	(0.01)	-0.0099	(0.01)
Sanctuary County in 2009	-0.0022	(0.02)	-0.0021	(0.02)
Sanctuary County in 2010	-0.027	(0.02)	-0.027	(0.02)
Sanctuary County in 2011	-0.043**	(0.02)	-0.044**	(0.02)
Sanctuary County in 2012	-0.051**	(0.02)	-0.054**	(0.02)
Sanctuary County in 2013	-0.053**	(0.03)	-0.055**	(0.02)
ICE County in 2007	0.0031	(0.01)	0.00096	(0.01)
ICE County in 2008	0.0059	(0.01)	-0.00088	(0.01)
ICE County in 2009	0.016	(0.02)	0.0089	(0.02)
ICE County in 2010	0.0052	(0.01)	-0.00078	(0.01)
ICE County in 2011	-0.0022	(0.01)	-0.010	(0.01)
ICE County in 2012	-0.014	(0.01)	-0.021	(0.01)
ICE County in 2013	-0.0093	(0.01)	-0.017	(0.01)
Observations	9206		9206	
Adjusted R-squared	0.303		0.360	
Pre-trend test for sanctuary counties, using early adopter as counterfactual				
Sanctuary County in 2007	0.025**	(0.01)	0.022*	(0.01)
Sanctuary County in 2008	0.041*	(0.02)	0.038*	(0.02)
Sanctuary County in 2009	0.038	(0.02)	0.035	(0.02)
Sanctuary County in 2010	0.047	(0.03)	0.043	(0.03)
Sanctuary County in 2011	0.039	(0.03)	0.031	(0.03)
Sanctuary County in 2012	0.043	(0.03)	0.038	(0.03)
Sanctuary County in 2013	0.040	(0.03)	0.029	(0.03)
Observations	1846		1846	
Adjusted R-squared	0.478		0.546	
Pre-trend test for ICE counties, using late policy adopter as counterfactual				
ICE County in 2007	0.024	(0.01)	0.027**	(0.01)
ICE County in 2008	0.023	(0.02)	0.029	(0.02)
ICE County in 2009	0.019	(0.02)	0.027	(0.02)
ICE County in 2010	0.019	(0.02)	0.026	(0.02)
ICE County in 2011	0.025	(0.02)	0.032*	(0.02)
ICE County in 2012	0.036*	(0.02)	0.043**	(0.02)
ICE County in 2012	0.038**	(0.02)	0.043**	(0.02)
Observations	1753		1753	
Adjusted R-squared	0.387		0.448	

Standard errors in parentheses * p<0.10, ** p<0.05, *** p<0.01 All regressions contain robust standard errors clustered by county ID, time trends and time variant covariates.

Table A2.4
Pre-Trend Test using Natural Log of Total Employment

	Ordinary Least Squares		Fixed Effects	
Sanctuary County in 2007	0.039	(0.02)	-0.0021	(0.00)
Sanctuary County in 2008	0.0029	(0.03)	-0.0044	(0.01)
Sanctuary County in 2009	0.052*	(0.03)	-0.0014	(0.01)
Sanctuary County in 2010	0.058*	(0.03)	-0.0077	(0.01)
Sanctuary County in 2011	0.034	(0.05)	-0.013	(0.02)
Sanctuary County in 2012	0.045	(0.05)	-0.014	(0.01)
Sanctuary County in 2013	0.026	(0.06)	-0.0074	(0.02)
ICE County in 2007	0.024	(0.03)	-0.0013	(0.00)
ICE County in 2008	0.030	(0.07)	-0.0044	(0.01)
ICE County in 2009	0.030	(0.08)	-0.0044	(0.01)
ICE County in 2010	-0.00092	(0.07)	-0.0020	(0.01)
ICE County in 2011	-0.012	(0.08)	-0.0052	(0.01)
ICE County in 2012	0.014	(0.07)	-0.0017	(0.01)
ICE County in 2013	0.026	(0.07)	0.000024	(0.01)
Observations	9206		9206	
Adjusted R-squared	0.570		0.517	
Pre-trend test for sanctuary counties, using early adopter as counterfactual				
Sanctuary County in 2007	-0.035	(0.11)	0.011*	(0.01)
Sanctuary County in 2008	-0.014	(0.12)	0.014	(0.01)
Sanctuary County in 2009	0.13	(0.14)	0.013	(0.02)
Sanctuary County in 2010	0.13	(0.14)	0.014	(0.02)
Sanctuary County in 2011	0.28***	(0.06)	0.0084	(0.02)
Sanctuary County in 2012	0.32***	(0.07)	0.011	(0.03)
Sanctuary County in 2013	0.44***	(0.09)	0.0068	(0.03)
Observations	1846		1846	
Adjusted R-squared	0.544		0.683	
Pre-trend test for ICE counties, using late policy adopter as counterfactual				
ICE County in 2007	-0.039	(0.07)	0.018***	(0.01)
ICE County in 2008	-0.078	(0.11)	0.029***	(0.01)
ICE County in 2009	-0.048	(0.11)	0.032***	(0.01)
ICE County in 2010	-0.054	(0.10)	0.027**	(0.01)
ICE County in 2011	-0.050	(0.11)	0.030**	(0.01)
ICE County in 2012	-0.053	(0.11)	0.028***	(0.01)
ICE County in 2012	-0.045	(0.11)	0.029***	(0.01)
Observations	1753		1753	
Adjusted R-squared	0.555		0.636	

Standard errors in parentheses * p<0.10, ** p<0.05, *** p<0.01 All regressions contain robust standard errors clustered by county ID, time trends and time variant covariates.

Table A2.5
Natural log of Labor Force Unemployment

	Ordinary Least Squares	Fixed Effects	Fixed Effects & Matching	
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1				
Sanctuary County	-0.12***	-0.12***	-0.17***	
	(0.04)	(0.03)	(0.04)	
P- value from Randomization Inference	0.014	0.000	0.008	
ICE County	0.0025	-0.00065		-0.00085
	(0.03)	(0.02)		(0.02)
P- value from Randomization Inference	0.51	0.324		0.569
Observations	10087	10087	3650	3406
Adjusted R-squared	0.105	0.058	0.073	0.067
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2				
Sanctuary County	-0.25**	-0.12***	-0.12***	
	(0.12)	(0.03)	(0.03)	
Observations	1841	1841	1841	
Adjusted R-squared	0.474	0.036	0.036	
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3				
ICE County	-0.083	-0.024	-0.024	
	(0.07)	(0.02)	(0.02)	
Observations	1773	1773	1773	
Adjusted R-squared	0.455	0.025	0.025	

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are in parentheses. All regressions contain robust standard errors clustered by county, time trends, and time variant covariant. Nearest – neighbor matching is based on counties' economic attributes, education index, family income, and region.

Table A2.6
Natural log of Labor Force Unemployment

	Equation 1		Equation 2	Equation 3
	Urban	Rural	Urban- Early vs Late Adopter	Rural - Early vs Late Adopter
Sanctuary County	-0.071	-0.13***	-0.052	-0.12***
	(0.04)	(0.03)	(0.05)	(0.03)
Observations	1292	7755	472	1369
Adjusted R-squared	0.057	0.021	0.062	0.050
ICE County	0.064*	-0.0059	0.081*	-0.016
	(0.04)	(0.02)	(0.05)	(0.03)
Observations	1286	8059	301	1472
Adjusted R-squared	0.052	0.014	0.052	0.009

* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

Table A2.7
Natural log of Real GDP

	Ordinary Least Squares	Fixed Effects	Fixed Effects & Matching	
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1				
Sanctuary County	0.029***	0.024**	-0.0061	
	(0.01)	(0.01)	(0.02)	
P- value from Randomization Inference	0.014	0.000	0.253	
ICE County	-0.012**	-0.0087	-0.0033	
	(0.00)	(0.01)	(0.01)	
P- value from Randomization Inference	0.51	0.324		0.569
Observations	9991	9991	3583	3395
Adjusted R-squared	0.264	0.318	0.477	0.388
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2				
Sanctuary County	0.022***	0.041***	0.057***	
	(0.01)	(0.00)	(0.01)	
Observations	1846	1846	1131	
Adjusted R-squared	0.400	0.461	0.571	
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3				
ICE County	-0.015***	-0.0016	-0.016	
	(0.00)	(0.01)	(0.01)	
Observations	1752	1752	1749	
Adjusted R-squared	0.355	0.446	0.403	

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are in parentheses. All regressions contain robust standard errors clustered by county, time trends, and time variant covariant. Nearest – neighbor matching is based on counties' economic attributes, education index, family income, and region.

Table A2.8
Natural log of Real GDP

	Equation 1		Equation 2	Equation 3
	Urban	Rural	Urban- Early vs Late Adopter	Rural - Early vs Late Adopter
Sanctuary County	0.035**	0.019**	0.032*	0.041***
	(0.02)	(0.01)	(0.02)	(0.01)
Observations	1264	7697	472	1374
Adjusted R-squared	0.556	0.278	0.553	0.477
ICE County	0.0072	-0.014*	-0.012	-0.014*
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1247	8001	288	1465
Adjusted R-squared	0.501	0.271	0.669	0.363

* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

Table A2.9
Natural log of Natural Log of Total Employment

	Ordinary Least Squares,		Fixed Effects		Fixed Effects & Matching	
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1						
Sanctuary County	0.23*		0.036***		0.023**	
	(0.12)		(0.01)		(0.01)	
P- value from Randomization Inference	0.014		0.000		0.008	
ICE County	-0.073		-0.0098*			-0.013**
	(0.09)		(0.01)			(0.01)
P- value from Randomization Inference	0.51		0.324			0.569
Observations	9991		9991		3583	3395
Adjusted R-squared	0.568		0.407		0.513	0.453
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2						
Sanctuary County	-0.19*	Not past	0.040***		0.026*	
	(0.11)	P-test	(0.01)		(0.01)	
Observations	1846		1846		1131	
Adjusted R-squared	0.545		0.533		0.700	
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3						
ICE County	-0.12*		-0.017***	Not past P-test	-0.020**	
	(0.06)		(0.01)		(0.01)	
Observations	1753		1753		1750	
Adjusted R-squared	0.558		0.518		0.489	

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are in parentheses. All regressions contain robust standard errors clustered by county, time trends, and time variant covariant. Nearest – neighbor matching is based on counties' economic attributes, education index, family income, and region.

Table A2.10
Natural Log of Total Employment

	Equation 1		Equation 2	Equation 3
	Urban	Rural	Urban- Early vs Late Adopter	Rural - Early vs Late Adopter
Sanctuary County	0.045***	0.026***	0.038***	0.040***
	(0.01)	(0.01)	(0.01)	(0.00)
Observations	1264	7699	472	1374
Adjusted R-squared	0.663	0.367	0.738	0.493
ICE County	0.0070	-0.015**	-0.011	-0.018***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1247	8001	288	1465
Adjusted R-squared	0.591	0.367	0.733	0.493

* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

Table A2.11
Natural log of Median Wages

Model	(1) Ordinary Least Squares,	(2) Fixed Effects	(3) Fixed Effects & Matching	
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1				
Sanctuary County	0.052**	0.024***	0.026***	
	(0.02)	(0.01)	(0.01)	
ICE County	0.0016	-0.00098		0.0013
	(0.02)	(0.00)		(0.00)
Observations	10147	10147	3705	3095
Adjusted R-squared	0.296	0.218	0.330	0.256
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2				
Sanctuary County	0.0045	0.017*	0.013	
	(0.02)	(0.01)	(0.02)	
Observations	1846	1846	1326	
Adjusted R-squared	0.762	0.355	0.409	
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3				
ICE County	-0.0051	-0.0061	-0.0086	
	(0.01)	(0.01)	(0.01)	
Observations	1778	1778	1751	
Adjusted R-squared	0.654	0.298	0.329	

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are in parentheses. All regressions contain robust standard errors clustered by county, time trends, and time variant covariant. Nearest – neighbor matching is based on counties' economic attributes, education index, family income, and region.

Table A2.12
Natural Log of Wages

	Equation 1		Equation 2	Equation 3
	Urban	Rural	Urban- Early vs Late Adopter	Rural - Early vs Late Adopter
Sanctuary County	0.022**	0.023***	0.0043	0.014
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1292	7811	472	1374
Adjusted R-squared	0.381	0.214	0.468	0.311
ICE County	0.0024	-0.0018	0.0037	-0.0021
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1286	8118	301	1478
Adjusted R-squared	0.340	0.219	0.416	0.270

* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

Table A2.13
Natural log of Foreign-Born Population

	Ordinary Least Squares	Fixed Effects	Fixed Effects & Matching
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1			
Sanctuary County	-0.14	-0.0021	0.0091
	(0.08)	(0.01)	(0.01)
ICE County	0.051	-0.013	-0.013
	(0.07)	(0.01)	(0.01)
Observations	10158	10158	3064
Adjusted R-squared	0.513	0.052	0.052
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2			
Sanctuary County	-0.070	0.010	0.0051
	(0.08)	(0.01)	(0.01)
Observations	1846	1846	1079
Adjusted R-squared	0.554	0.031	0.059
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3			
ICE County	0.044	-0.014	-0.042**
	(0.07)	(0.01)	(0.02)
Observations	1786	1786	2108
Adjusted R-squared	0.437	0.052	0.065

* p<0.10, ** p<0.05, *** p<0.01. Standard errors are in parentheses. All regressions contain robust standard errors clustered by county, time trends, and time variant covariant. Nearest – neighbor matching is based on counties' economic attributes, education index, family income, and region.

Table A2.14
Natural log of Average Household Income by Quintile

	Lowest Q	Second Q	Third Q	Fourth Q	Highest Q
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1					
Sanctuary County	0.020**	0.028***	0.032***	0.033***	0.038***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ICE County	-0.0030	0.0015	0.00068	-0.0017	-0.0028
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	10150	10150	10150	10150	10150
Adjusted R-squared	0.068	0.181	0.236	0.281	0.227
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2					
Sanctuary County	0.011	0.021**	0.024**	0.025***	0.030***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1846	1846	1846	1846	1846
Adjusted R-squared	0.073	0.224	0.307	0.359	0.337
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3					
ICE County	-0.0063	-0.0024	-0.0023	-0.0069	-0.0082
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1779	1779	1779	1779	1779
Adjusted R-squared	0.096	0.226	0.286	0.340	0.274

* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

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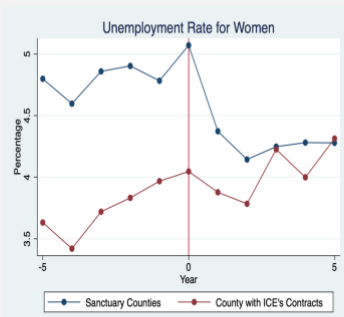
Table A2.14
Natural log of Labor Force Unemployment by Gender and Race

	Women	Men	White	Black	Latino
Treatment Effect between sanctuary, ICE, and neutral counties - Equation 1					
Sanctuary County	-0.12***	-0.12***	-0.12***	-0.063	-0.11***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
ICE County	0.013	0.0010	0.0058	-0.052*	0.018
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Observations	9442	9442	10143	4793	3611
Adjusted R-squared	0.040	0.042	0.040	0.050	0.071
Treatment Effect for sanctuary counties, using late policy adopter as counterfactual – Equation 2					
Sanctuary County	-0.14***	-0.16***	-0.14***	-0.088***	-0.17***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Observations	1800	1800	1841	882	1308
Adjusted R-squared	0.054	0.061	0.048	0.036	0.062
Treatment Effect for ICE counties, using late policy adopter as counterfactual – Equation 3					
ICE County	0.0058	-0.0081	-0.0011	-0.051*	0.030
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Observations	1694	1694	1779	983	803
Adjusted R-squared	0.037	0.034	0.032	0.076	0.095

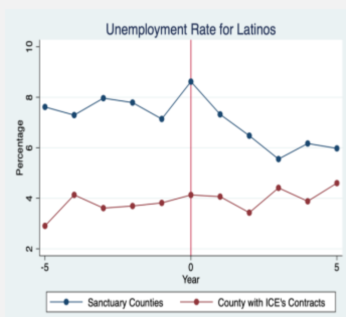
* p<0.10, ** p<0.05, *** p<0.0, Standard errors in parentheses, all regression use FE, Time Trends, and Time Variant Covariant, Robust Standard errors clustered by county ID.

FIGURES A2.1
UNEMPLOYMENT RATE

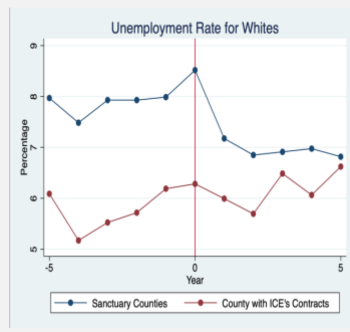
WOMEN



Latino

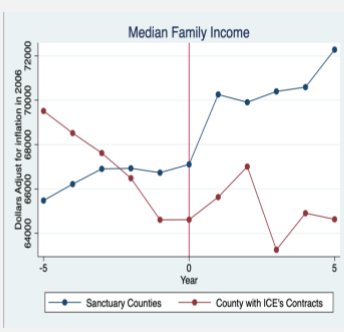


White

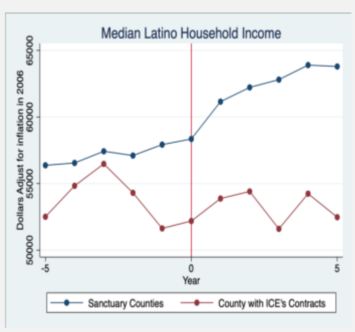


MEDIAN HOUSEHOLD INCOME

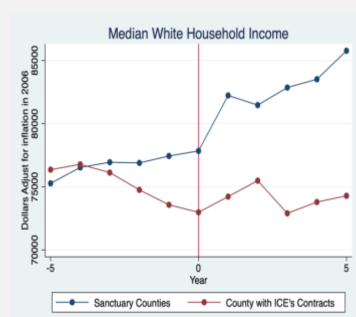
MEDIAN FAMILY



Latino



White



ALL EVENT GRAPHS SHOW 5 YEARS BEFORE AND AFTER THE ADOPTION OF THE IMMIGRATION POLICY FOR SC AND ICE COUNTIES.

Appendix B

TABLE A.3.1
Sanctuary county policy selection as the depended variable: linear probability model

	(1) OLS	(2) Time Trends (TT)	(3) State Fixed Effects (SFE)	(4) TT & SFE	(5) TT & County Fixed Effects	(6) TT & Hybrid	(7) TT & Correlated RE
Per capita income lag	0.0813 (0.97)	0.0253 (0.65)	0.0475 (1.01)	-0.0137 (-0.41)	0.00920 (0.49)		0.00945 (0.50)
Within						0.00945 (0.50)	
Between & Between - within						-0.0239 (-0.49)	-0.0333 (-0.63)
Unemployment lag	0.0160 (1.59)	-0.00278 (-0.40)	-0.00494 (-0.68)	-0.00136 (-0.20)	0.00534 (1.02)		0.00531 (1.01)
Within						0.00531 (1.01)	
Between & Between - within						0.00443 (0.57)	-0.000879 (-0.09)
Mean years of schooling	0.147 (0.31)	0.341 (1.37)	0.324 (1.22)	0.441* (1.93)	0.359** (2.32)		0.362** (2.34)
Within						0.362** (2.34)	
Between & Between - within						0.490* (1.81)	0.129 (0.41)
Population density	0.0010*** (6.94)	0.0015*** (10.84)	0.0015*** (10.61)	0.0014*** (12.42)	-0.00102 (-0.61)		-0.000940 (-0.56)
Within						-0.000940 (-0.56)	
Between & Between - within						0.00150*** (8.94)	0.00244 (1.46)
Rural county	0.106** (2.30)	0.0175 (0.71)	0.0146 (0.59)	0.0248 (0.99)	0.0404* (1.80)	0.0358** (2.33)	0.0358** (2.33)
% Of non-citizens	0.00846* (1.86)	-0.00326 (-1.48)	-0.00427 (-1.66)	-0.000154 (-0.07)	-0.00138 (-1.52)		-0.00134 (-1.48)
Within						-0.00134 (-1.48)	
Between & Between - within						-0.00122 (-0.60)	0.000121 (0.05)
Democrats win lag	0.0819** (2.25)	0.0609** (2.57)	0.0647** (2.61)	0.0522** (2.26)	0.00457 (0.32)		0.00450 (0.31)
Within						0.00450 (0.31)	
Between & Between - within						0.0558*** (4.02)	0.0513** (2.56)
Obs.	9280	6049	6049	4108	9280	9280	9280
Linear prob.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	No	Yes	No	Yes	Yes	Yes	Yes
State fixed Effects	No	No	Yes	Yes	No	No	No
County fixed effects	No	No	No	No	Yes	Yes	Yes
County between Effects	No	No	No	No	No	Yes	No

*** p<0.10, ** p<0.05, *** p<0.01; All regressions include robust standard errors clustered by county ID**

TABLE A3.2
ICE county policy selection as the depended variable: linear probability model

	(1) OLS	(2) Time Trends (TT)	(3) State Fixed Effects (SFE)	(4) TT & SFE	(5) TT & County Fixed Effects	(6) TT & Hybrid	(7) TT & Correlated RE
Per capita income lag	-0.00632 (-0.14)	0.0649* (1.77)	0.0672* (1.89)	0.0708* (1.79)	-0.0161 (-0.82)		-0.0162 (-0.82)
Within						-0.0162 (-0.82)	
Between & Between - within						0.0691 (0.72)	0.0852 (0.88)
Unemployment lag	0.0198** (2.17)	0.0310*** (3.20)	0.0306*** (3.16)	0.0312*** (3.05)	0.00373 (0.68)		0.00372 (0.68)
Within						0.00372 (0.68)	
Between & Between - within						0.0267* (1.82)	0.0230 (1.47)
Mean years of schooling	0.305 (0.89)	-0.0387 (-0.11)	-0.0291 (-0.09)	-0.0552 (-0.15)	-0.0641 (-0.40)		-0.0680 (-0.42)
Within						-0.0680 (-0.42)	
Between & Between - within						0.0391 (0.07)	0.107 (0.19)
Population density	-0.00632 (-0.14)	-0.000311 (-1.66)	-0.000310 (-1.65)	-0.000323 (-1.67)	0.00103 (-0.59)		-0.00102 (-0.59)
Within						-0.00102 (-0.59)	
Between & Between - within						-0.000384 (-1.18)	0.000635 (0.36)
Rural county	0.00231 (0.06)	0.0344 (0.82)	0.0342 (0.81)	0.0351 (0.81)	0.0323 (1.38)	0.0354* (1.74)	0.0354* (1.74)
% Of non-citizens	0.00713 (1.57)	0.00813** (2.13)	0.00805** (2.13)	0.00847** (2.09)	0.000758 (0.80)		0.000737 (0.78)
Within						0.000737 (0.78)	
Between & Between - within						0.0115*** (2.91)	0.0108*** (2.65)
Democrats lag	-0.0783*** (-4.04)	-0.0477** (-2.24)	-0.0473** (-2.22)	-0.0492** (-2.21)	0.0194 (1.28)		0.0194 (1.28)
Within						0.0194 (1.28)	
Between & Between - within						-0.0567** (-2.10)	-0.0761** (-2.46)
OBS.	9280	9280	9280	9280	9280	9280	9280
Linear prob.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	No	Yes	No	Yes	Yes	Yes	Yes
State fixed Effects	No	No	Yes	Yes	No	No	No
County fixed effects	No	No	No	No	Yes	Yes	Yes
County between Effects	No	No	No	No	No	Yes	No

*** p<0.10, ** p<0.05, *** p<0.01; All regressions include robust standard errors clustered by county ID**

Appendix C

Table A4.1
Housing summary statistics from all counties

	A - total observations N = 369,041				B - Toledo city (Lucas County) N = 78,129			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Sale price $	127052.70	84780.41	8825	505000	105701.40	75628.33	8825	415000
Sale year	2007.18	4.39	2001	2015	2006.98	4.36	2001	2015
Lake distance k.	9348.07	6626.07	0	29122.54	13330.20	6631.30	0	29122.54
Tributary distance k.	6779.95	5367.16	0	26854.48	7379.90	4936.70	0	23070.1
Stories number	1.57	0.46	1	4	1.44	0.47	1	3
Year build	1954.02	30.01	1800	2015	1953.59	32.06	1827	2015
Rooms number	6.54	1.80	1	20	6.39	1.52	2	19
Bedrooms number	3.12	0.86	1	12	3.09	0.77	1	11
Bathrooms number	1.45	0.61	1	11	1.42	0.60	1	9
Half bathrooms	0.43	0.54	0	5	0.40	0.53	0	4
Total living area	1652.44	704.67	2	71323	1626.47	689.99	232	14076
Total lot area	10842.81	19176.71	0	1346094	12133.96	21929.15	0	429100
Control locations	C-Cleveland city (Cuyahoga County) N = 249,888				D- Lake County (Lake) N = 41,024			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Sale price $	132443.20	89559.56	14500	505000	134880.90	61661.76	23000	375000
Sale year	2007.25	4.39	2001	2015	2007.16	4.37	2001	2015
Lake distance k.	9054.80	6200.69	0	28602.77	3375.82	2723.53	0	14981.58
Tributary distance k.	7379.25	5544.97	0	26854.48	2344.02	2062.69	0	11212.32
Stories number	1.65	0.44	1	4	1.33	0.47	1	3
Year build	1951.68	29.23	1800	2015	1969.16	26.00	1803	2014
Rooms number	6.66	1.92	1	20	6.10	1.32	2	18
Bedrooms number	3.15	0.90	1	12	2.98	0.70	1	8
Bathrooms number	1.45	0.61	1	11	1.50	0.58	1	5
Half bathrooms	0.43	0.54	0	5	0.50	0.54	0	3
Total living area	1678.83	727.68	2	71323	1541.09	562.88	280	9211
Total lot area	10586.51	19426.61	0	1346094	9945.09	9441.53	0	107910

Table A 4.2
Housing summary statistics from Toledo using the areas affected and non-affected by HABs measured in kilometers away from the lake

	Treatment group: first 14 km. Observations = 44,999				Control group: starting at the 14 km. Observations = 34,283			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Sale price $	74102.80	52959.95	8000	500000	151272.20	90263.86	8000	508000
Sale year	2006.87	4.29	2001	2015	2007.18	4.45	2001	2015
Lake distance k.	8433.74	3508.14	0	13999.53	19803.15	3575.45	14000.03	29122.54
Tributary distance k.	4462.27	3103.72	0	11975.94	11221.36	4266.12	1922.462	23367.7
Stories number	1.43	0.46	1	3	1.45	0.48	1	3
Year build	1936.95	26.26	1852	2014	1975.34	25.87	1827	2015
Rooms number	6.18	1.54	2	19	6.70	1.50	2	18
Bedrooms number	2.98	0.77	1	11	3.24	0.76	1	10
Bathrooms number	1.22	0.46	1	5	1.72	0.67	1	9
Half bathrooms	0.26	0.46	0	4	0.59	0.56	0	4
Total living area	1396.66	529.33	232	14076	1956.08	787.37	400	8830
Total lot area	8426.58	16691.83	900	409602	17161.37	26848.70	0	466092

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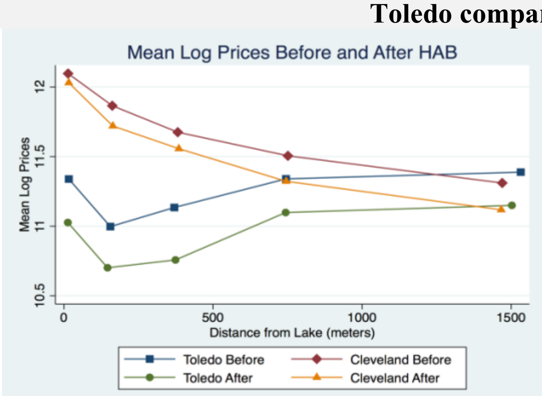
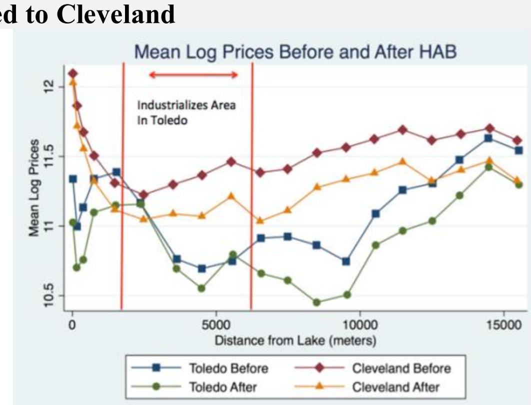
Table A4.3
Housing summary statistics from Toledo using the areas affected and non-affected by HABs measured in kilometers away from the lake and tributary

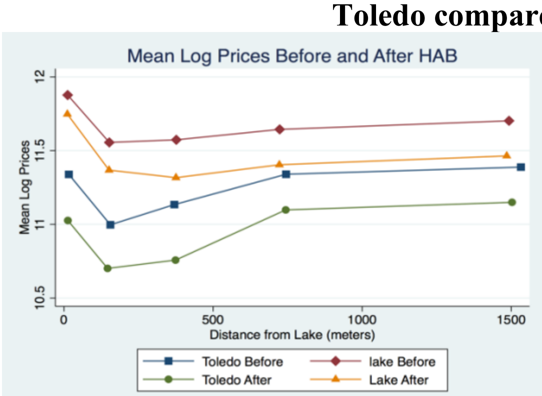
	Treatment group: first 10 km. N = 51,625				Control group: starting at the 11 km. N = 27,657			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Sale price $	80494.43	56589.57	8000	506000	157829.50	94767.19	8000	508000
Sale year	2006.92	4.31	2001	2015	2007.17	4.45	2001	2015
Lake distance k.	9829.18	4729.48	0	21136.42	19922.25	4327.84	10834.11	29122.54
Tributary distance k.	4334.34	2632.94	0	8999.146	13079.48	2763.39	9000.559	23367.7
Stories number	1.42	0.46	1	3	1.47	0.49	1	3
Year build	1939.99	26.88	1827	2014	1978.87	25.60	1830	2015
Rooms number	6.20	1.52	2	19	6.78	1.51	2	18
Bedrooms number	2.99	0.77	1	11	3.28	0.76	1	10
Bathrooms number	1.24	0.49	1	6	1.78	0.67	1	9
Half bathrooms	0.28	0.48	0	4	0.61	0.55	0	4
Total living area	1428.89	545.52	232	14076	2029.95	808.57	400	8500
Total lot area	8784.02	16154.88	900	409602	18586.82	29169.07	0	466092

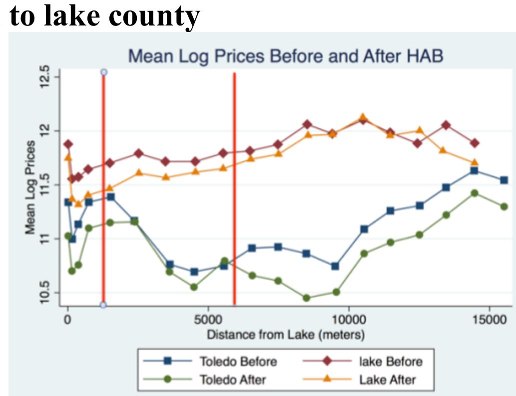
Graph A4.1
Prices trend away from the lake
Toledo compared to Cleveland





Toledo compared to lake county





Graphs contains mean log yearly prices trend across space away from the lake and before and after HABs. Vertical red lines indicate the areas within the industrial zone of Toledo.

Within Toledo Data	**Table A4.4 Standard generalized method of moment test is a test for covariate balance**			
	Standardized differenced		Variance Ratio	
	Raw	Weighted	Raw	Weighted
Bedrooms	-0.13	-0.08	0.92	0.95
Full bathrooms	-0.41	-0.20	0.57	0.84
Year built	-0.44	-0.32	0.59	0.55
Living area	-0.36	-0.20	0.57	0.83
Lot area	-0.14	-0.11	0.74	0.70
Tributary distance	-0.70	-0.40	0.36	0.34

The model improves the level of balance as weight-standardized differences appear closer to zero, and the variance ratios are mostly closer to one.

Table A4.5
Bias reduction

		Toledo City & Cleveland City				Toledo City affected and non-affected Areas			
Unmatched & matched (U & M)		**Mean**			**Reduce Bias %**	**Mean**			**Reduce Bias %**
		Treated	**Control**	**% Bias**		**Treated**	**Control**	**% Bias**	
Bedrooms	U	3.00	3.17	-21.10		3.01	3.08	-10.50	
	M	3.00	3.00	-0.40	98.20	3.01	2.99	1.40	86.50
Full baths	U	1.25	1.37	-23.40		1.26	1.43	-31.90	
	M	1.25	1.24	1.10	95.10	1.26	1.24	3.70	88.40
Year built	U	1943.80	1947.30	-13.20		1945.90	1954.10	-28.20	
	M	1943.80	1943.60	0.80	93.80	1945.90	1944.20	6.10	78.30
Living area	U	1456.10	1571.60	-19.70		1464.00	1622.40	-26.00	
	M	1456.10	1452.40	0.60	96.80	1464.00	1444.10	3.30	87.40
Lot area	U	9886.70	9145.40	4.70		9941.50	12256.00	-11.70	
	M	9886.70	9416.70	3.00	36.60	9941.50	9224.20	3.60	69.00
Tributary distance	U	5207.40	5050.20	4.50		5112.70	7635.20	-61.60	

The standardized residual is first calculated on the total unadjusted sample dividing difference in means by the pooled standard deviation. The standardized residual is then calculated on the reduced subset matched set of treatment vs. Controls using the new difference in means as the numerator and the original raw sample pooled standard deviation in the denominator. Holding the variability constant, the difference in the first and second calculations represents the bias reduction.

Table A4.6
Using Cleveland and lake as the control groups

Variable	Unmatched Mean treatment	Mean control	Normalized difference	Matched Mean treatment	Mean control	Normalized difference
Bedrooms	3.00	3.17	0.15	3.00	3.00	0.00
	0.75	0.84		0.75	0.75	
Full bathrooms	1.25	1.37	0.17	1.25	1.24	-0.01
	0.49	0.56		0.49	0.47	
Year built	1943.80	1947.30	0.09	1943.80	1943.60	-0.01
	24.18	28.26		24.18	25.43	
Living area	1456.10	1571.60	0.14	1456.10	1452.40	0.00
	565.13	608.80		565.13	533.72	
Lot area	9886.70	9145.40	-0.03	9886.70	9416.70	-0.02
	18220.87	13114.88		18220.87	17469.31	
Tributary distance	5207.40	5050.20	-0.03	5207.40	5224.80	0.00
	3301.51	3728.27		3301.51	3599.86	
Log sale price	10.86	11.45	0.55	10.86	11.38	0.51
	0.81	0.71		0.81	0.63	

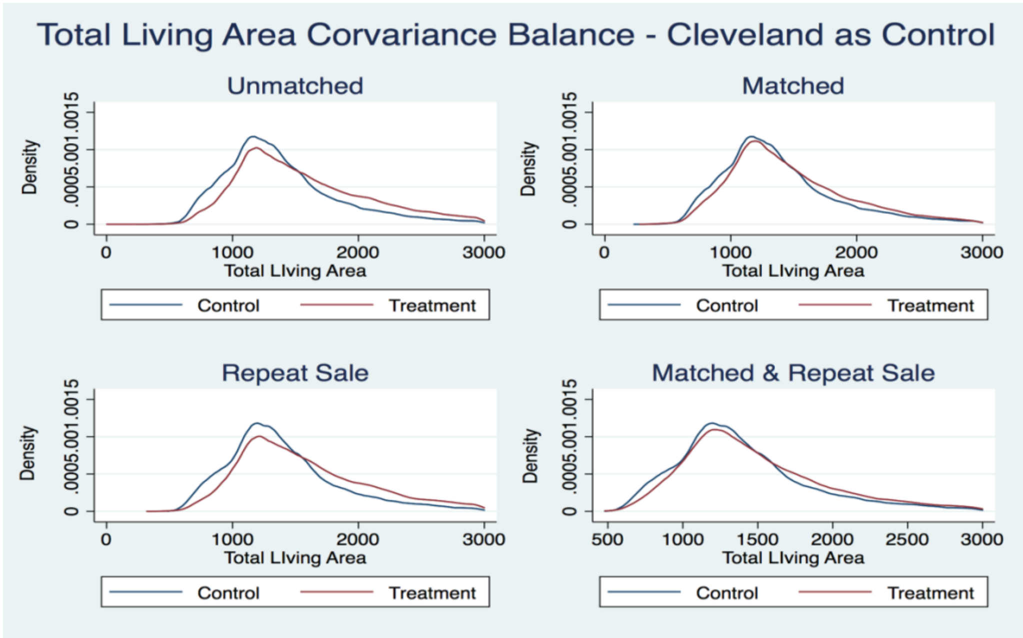
Using Imbens and Wooldridge (2009), this table reports the difference in averages by treatment status, scaled by the square root of the sum of the variances, as a scale-free measure of the difference in distributions that is the normalized difference calculated as $\frac{\bar{X}_{treat} - \bar{X}_{Control}}{\sqrt{Van(X_{Treat}) + Van(X_{Control})}}$. Normalized difference in means greater than 0.25 indicates that linear regression methods will be sensitive to the linear specification

Table A4.7
Using Toledo as its own Control

Variable	Unmatched Mean Treatment	Mean Control	Normalized Diff	Matched Mean Treatment	Mean Control	Normalized Diff
Bedrooms	3.01	3.08	0.07	3.01	3.00	-0.01
	0.74	0.77		0.74	0.75	
Full Bathroom	1.26	1.43	0.23	1.26	1.24	-0.03
	0.49	0.59		0.49	0.46	
Year Built	1945.94	1954.11	0.20	1945.94	1943.91	-0.06
	24.72	32.62		24.72	26.63	
Living Area	1464.01	1622.39	0.18	1464.01	1443.33	-0.03
	534.64	674.26		534.64	535.37	
Lot Area	9941.49	12256.36	0.08	9941.49	9094.68	-0.04
	16970.56	22315.91		16970.56	15016.54	
Tributary Dist.	5112.67	7635.20	0.44	5112.67	5083.84	-0.01
	2818.95	5059.64		2818.95	3014.77	
Log Sale Price	10.92	11.31	0.34	10.92	11.23	0.29
	0.83	0.78		0.83	0.68	

Using Imbens and Wooldridge (2009), This table reports the difference in averages by treatment status, scaled by the square root of the sum of the variances, as a scale-free measure of the difference in distributions that is the normalized difference calculated as $\frac{\bar{X}_{treat} - \bar{X}_{Control}}{\sqrt{Van(X_{Treat}) + Van(X_{Control})}}$. Normalized difference in means greater than 0.25 indicates that linear regression methods will be sensitive to the linear specification

Graph A4.2:
Covariance Balance



Graphs contains the differences in covariance balance between unmatched, matched, repeat sale, and matched and repeat sales simultaneously.

Table A4.8
Parallel Pre-trend test using Non-Affect Areas within Toledo as control

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
Toledo price effect in 2002	0.081	0.054	0.082	0.11	-0.20**
	(0.05)	(0.16)	(0.08)	(0.09)	(0.09)
Toledo price effect in 2003	0.049	-0.15	0.095	0.082	0.068
	(0.06)	(0.13)	(0.08)	(0.10)	(0.09)
Toledo price effect in 2004	0.062	-0.13	0.12*	0.11	-0.29***
	(0.06)	(0.15)	(0.07)	(0.09)	(0.11)
Toledo price effect in 2005	0.046	-0.084	0.10*	0.085	-0.39**
	(0.05)	(0.17)	(0.06)	(0.09)	(0.16)
Toledo price effect in 2006	0.066	-0.26**	0.16***	0.14**	-0.19
	(0.04)	(0.12)	(0.05)	(0.06)	(0.25)
Toledo price effect in 2007	0.035	0.19	0.062	0.15***	0.61***
	(0.03)	(0.14)	(0.04)	(0.05)	(0.18)
Observations	119137	71520	119137	34723	36631
Adjusted R-squared	0.450	0.466	0.106	0.144	0.615
Ordinary Least Squares (OSL)	Y	Y	N	N	N
Fixed Effects (FE)	N	N	Y	Y	Y
Repeat Sales (RS)	N	N	N	Y	Y
Nearest-Neighbor Matching (NM)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-Neighbor Matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* P<0.10.
** P<0.05.
*** P<0.01.

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Table A4.9
Lake County as control with treatment effects in 2008 & 2011

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
	Treatment Effect in 2008 alone				
Treatment Effect in 2008	**-0.23*** **	**-0.22*** **	**-0.30*** **	**-0.34*** **	-0.20***
	(0.04)	**(0.04)**	**(0.04)**	**(0.04)**	(0.04)
	88271	**30358**	**88271**	**24973**	16689
Adjusted R-squared	**0.531**	**0.508**	**0.302**	**0.338**	0.658
	Treatment effects in 2011 alone				
Treatment Effect in 2011	**-0.16*** **	**-0.12*** **	**-0.22*** **	**-0.22*** **	-0.12***
	(0.04)	**(0.02)**	**(0.03)**	**(0.03)**	(0.03)
	88271	**30358**	**88271**	**24973**	16689
Adjusted R-squared	**0.528**	**0.507**	**0.291**	**0.321**	0.657
	Treatment effects in 2008 & 2011				
Treatment Effect in 2011	**-0.22*** **	**-0.26*** **	**-0.29*** **	**-0.33*** **	-0.25***
	(0.04)	**(0.04)**	**(0.04)**	**(0.05)**	(0.04)
Treatment Effect in 2008	**-0.25*** **	**-0.19*** **	**-0.30*** **	**-0.35*** **	-0.16***
	(0.04)	**(0.04)**	**(0.03)**	**(0.04)**	(0.04)
Observations	**88271**	**30358**	**88271**	**24973**	16689
Adjusted R-squared	**0.531**	**0.508**	**0.302**	**0.338**	0.658
Ordinary Least Squares (OSL)	**Y**	**Y**	**N**	**N**	N
Fixed Effects (FE)	**N**	**N**	**Y**	**Y**	Y
Repeat Sales (RS)	**N**	**N**	**N**	**Y**	Y
Nearest-Neighbor Matching (NM)	**N**	**Y**	**N**	**N**	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-Neighbor Matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* P<0.10.
** P<0.05.
*** P<0.01.

Table A4.10
Lake County as control with treatment effects in 2008 per distance away from Lake Erie

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
Treatment Effects at 1 km	-0.21***	-0.091**	-0.22***	-0.22***	-0.097*
	(0.02)	(0.04)	(0.02)	(0.04)	(0.06)
Treatment Effects at 2 km	-0.12***	-0.087*	-0.12***	-0.15***	-0.067
	(0.03)	(0.05)	(0.03)	(0.05)	(0.06)
Treatment Effects at 3 km	0.10	0.094	0.051	-0.16	-0.0023
	(0.07)	(0.10)	(0.05)	(0.10)	(0.11)
Treatment Effects at 4 km	-0.084	0.028	-0.17***	-0.17***	0.0072
	(0.05)	(0.08)	(0.04)	(0.06)	(0.10)
Treatment Effects at 5 km	-0.27***	-0.32***	-0.36***	-0.54***	-0.43***
	(0.04)	(0.06)	(0.03)	(0.05)	(0.07)
Treatment Effects at 6 km	-0.100***	-0.19***	-0.21***	-0.26***	-0.14***
	(0.03)	(0.04)	(0.02)	(0.04)	(0.06)
Treatment Effects at 7 km	-0.32***	-0.38***	-0.39***	-0.51***	-0.38***
	(0.02)	(0.03)	(0.01)	(0.03)	(0.04)
Treatment Effects at 8 km	-0.39***	-0.39***	-0.45***	-0.50***	-0.37***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 9 km	-0.41***	-0.49***	-0.47***	-0.58***	-0.50***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 10 km	-0.30***	-0.42***	-0.41***	-0.49***	-0.39***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 11 km	-0.18***	-0.24***	-0.28***	-0.29***	-0.17***
	(0.02)	(0.03)	(0.01)	(0.02)	(0.04)
Treatment Effects at 12 km	-0.26***	-0.25***	-0.31***	-0.30***	-0.19***
	(0.02)	(0.03)	(0.01)	(0.02)	(0.04)
Treatment Effects at 13 km	-0.25***	-0.24***	-0.30***	-0.30***	-0.20***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Treatment Effects at 14 km	-0.18***	-0.11***	-0.21***	-0.16***	-0.046
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Treatment Effects at 15 km	-0.10***	-0.091***	-0.12***	-0.11***	-0.011
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 16 km	-0.14***	-0.098***	-0.14***	-0.12***	-0.035
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Treatment Effects at 17 km	-0.14***	-0.084***	-0.16***	-0.12***	-0.0088
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 18 km	-0.067***	0.010	-0.066***	-0.060*	0.052
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 19 km	0.025	0.085***	0.026	-0.011	0.095**
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Observations	115805	43877	115794	34723	22961
Adjusted R-squared	0.528	0.566	0.273	0.310	0.671
Ordinary Least Squares (OSL)	Y	Y	N	N	N
Fixed Effects (FE)	N	N	Y	Y	Y
Repeat Sales (RS)	N	N	N	Y	Y
Nearest-Neighbor Matching (NM)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-Neighbor Matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* P<0.10.
** P<0.05.
*** P<0.01.

Table A4.11
Lake County as control with treatment effects in 2008 per distance away from Lake Erie and its tributaries

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
Treatment Effects at 1 km	-0.19***	-0.21***	-0.28***	-0.34***	-0.22***
	(0.02)	(0.03)	(0.01)	(0.02)	(0.04)
Treatment Effects at 2 km	-0.46***	-0.48***	-0.48***	-0.59***	-0.47***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.05)
Treatment Effects at 3 km	-0.20***	-0.34***	-0.32***	-0.38***	-0.29***
	(0.02)	(0.03)	(0.01)	(0.03)	(0.05)
Treatment Effects at 4 km	-0.12***	-0.20***	-0.24***	-0.23***	-0.100**
	(0.02)	(0.04)	(0.02)	(0.03)	(0.05)
Treatment Effects at 5 km	-0.21***	-0.28***	-0.28***	-0.38***	-0.28***
	(0.02)	(0.03)	(0.01)	(0.03)	(0.04)
Treatment Effects at 6 km	-0.40***	-0.37***	-0.41***	-0.46***	-0.36***
	(0.02)	(0.03)	(0.01)	(0.02)	(0.04)
Treatment Effects at 7 km	-0.27***	-0.23***	-0.30***	-0.28***	-0.18***
	(0.02)	(0.03)	(0.01)	(0.02)	(0.04)
Treatment Effects at 8 km	-0.12***	-0.074***	-0.13***	-0.11***	0.00058
	(0.01)	(0.02)	(0.01)	(0.02)	(0.04)
Treatment Effects at 9 km	-0.17***	-0.13***	-0.17***	-0.18***	-0.070*
	(0.01)	(0.02)	(0.02)	(0.03)	(0.04)
Treatment Effects at 10 km	-0.097***	-0.053*	-0.11***	-0.10***	0.0043
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Treatment Effects at 11 km	-0.049***	-0.030	-0.041**	-0.100***	0.0072
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Treatment Effects at 12 km	0.032*	0.093***	0.035**	0.080***	0.19***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
Observations	115805	43877	115794	34723	22961
Adjusted R-squared	0.518	0.552	0.262	0.292	0.658
Ordinary Least Squares (OSL)	Y	Y	N	N	N
Fixed Effects (FE)	N	N	Y	Y	Y
Repeat Sales (RS)	N	N	N	Y	Y
Nearest-Neighbor Matching (NM)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-Neighbor Matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* P<0.10.
** P<0.05.
*** P<0.01.

Table A4.12
Distance from the Lake and the Tributaries Non-Affected areas within Toledo City as control with treatment effects in 2008 and 2011

	(1) Ordinary Least Squares	(2) Nearest Neighbor Matching (NNM)	(3) Fixed Effects	(4) Fixed Effect & Repeat Sales	(5) Fixed Effects, Repeat Sales & NNM
	Treatment Effect in 2008				
Treatment Effect in 2008	**-0.41*** **	**-0.15*** **	**-0.46*** **	**-0.58*** **	-0.17***
	(0.09)	**(0.03)**	**(0.09)**	**(0.12)**	(0.04)
	78114	**45088**	**78114**	**22889**	22294
Adjusted R-squared	**0.456**	**0.466**	**0.229**	**0.254**	0.657
	Treatment effects in 2011				
Treatment Effect in 2011	**-0.34*** **	**-0.12*** **	**-0.38*** **	**-0.44*** **	-0.11***
	(0.08)	**(0.03)**	**(0.08)**	**(0.09)**	(0.04)
	78114	**26835**	**78114**	**22889**	12439
Adjusted R-squared	**0.449**	**0.474**	**0.213**	**0.223**	0.667
	Treatment effects in 2008 & 2011				
Treatment Effect in 2011	**-0.44*** **	**-0.18*** **	**-0.50*** **	**-0.62*** **	-0.22***
	(0.10)	**(0.04)**	**(0.10)**	**(0.13)**	(0.05)
Treatment Effect in 2008	**-0.38*** **	**-0.13*** **	**-0.41*** **	**-0.52*** **	-0.13***
	(0.09)	**(0.03)**	**(0.08)**	**(0.12)**	(0.04)
Observations	**78114**	**44801**	**78114**	**22889**	22049
Adjusted R-squared	**0.457**	**0.474**	**0.230**	**0.255**	0.661
Ordinary Least Squares (OSL)	Y	Y	N	N	N
Fixed Effects (FE)	N	N	Y	Y	Y
Repeat Sales (RS)	N	N	N	Y	Y
Nearest-Neighbor Matching (NM)	N	Y	N	N	Y

All regressions contain robust standard errors clustered by census tracts, house specific variables, two-way fixed effect & county specific time trend. Nearest-Neighbor Matching is based upon houses' structural attributes such as number of bedrooms, baths, year built, total living area, and distance to the river and lake.
* P<0.10.
** P<0.05.
*** P<0.01.