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Essays on Housing and Health in Cities: Past and Present

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Introduction

After centuries of civilization dominated by rural activity, the development of cities shifted dramatically with the advent of the industrial revolution, and that shift only accelerated at the turn of the 20th century. While different countries have followed different timelines, the inevitable outcome was that most of the world now lives in an urban setting. The United States had a larger rural population than urban population until 1920, while France and Germany had a majority urban population in beginning 1950. By 2007, the UN estimated that half the world's population lived in cities, and that rate has only increased since then (Nationen (2007)). This seismic shift in how society was organized resulted in equally significant changes in the livelihoods of its citizens.

The growth of cities went hand in hand with with a drastic change in both the structure of the economy and the conditions in which people lived. Agglomeration of industry resulted in improved economies of scale and induced a concentration of economic activity in cities (Krugman (1991), Fujita, Krugman and Venables (1999)). The development of cities also often went hand in hand with increased incomes of the citizens who lived and worked there and for the nation as a whole (Nationen (2020)), in addition to playing host to a variety of important social and cultural movements (Castells (1983), Harold (2018), van Haperen (2022)). At the same time, cities were not always a positive force. Most urban areas experienced what is known as a "mortality penalty", with higher rates of disease and death relative to rural areas (Feigenbaum, Hoehn-Velasco, Lauren and Wrigley-Field, Elizabeth (2020)). The growth of the population of cities was also due in large part to immigration, whether it was from within the country or across other countries, or both. While cities provided these immigrants opportunity, higher density also meant that the risk and scale of conflict between various groups was often more severe. Tensions between nationalities, races, religions, or labor groups resulted in acute events such as the race riots of 1967 and 1968 in the United States. At the same time, chronic trends in the structure of cities also emerged, such as social and residential segregation Massey and Denton (1988). In many of these cases, governments decided to get involved to either promote or counteract many of these trends throughout history.

Governments and institutions often tried to play a role to either alter the direction of or ride this tidal wave of change to achieve ends they saw as desirable. Policies, laws, and regulations were used to attempt to harness, expand, or adapt to various developments of cities, with differing levels of success. For example, departments of public health were created in cities around the world to track the development of cases of infectious disease and invest in infrastructure to stem the spread of water-borne disease (Rosen (1993), Anderson, Charles and Rees (2022)). Zoning laws were created to confine the development and expansion of certain types of social groups or economic activity to specific areas of the city (Shertzer, Twinam and Walsh (2016), Shertzer, Twinam and Walsh (2018), Trounstine (2020), Einstein (2021)). Policies were introduced and expanded to promote homeownership in the United States, but at the same time encoded a system of segregation and discrimination (Jackson (1987)). While the repercussions of many of these policies have been well studied, others are more difficult to ascertain due to a confluence of factors. To that end, this thesis is concerned with how three specific policies impacted the health and housing of the residents of cities, with both a historical and a modern lens. Over the course of the 20th century, the urban-rural mortality in many countries inverted, so that living in cities was associated with a lower rate of mortality relative to living in a rural setting, but there is still much to understand in terms of why that happened and how these lessons can be applied to countries in the midst of persistent urban mortality today. In addition, scholars are continuing to understand the various factors behind why blacks have lower rates of wealth and homeownership relative to whites in the United States, and how government policies influence both the rate of homeownership and where residents of cities choose to live. The chapters in this thesis seek to improve our understanding of these questions by addressing the impacts of the three policies under consideration.

The first chapter of this thesis considers the consequences of a far-reaching reform to the political structure of a metropolitan area on housing and demographic outcomes. City-county government consolidation, the policy in question, is a reform many central cities around the United States are considering as they confront problems associated with local government fragmentation, such as a lack of affordable housing and racial segregation. Taking the case of the consolidation of Louisville and Jefferson County into Metro Louisville in 2003, I study how consolidation differentially affected housing and demographic outcomes in previously-unincorporated areas, which were brought directly under Metro Louisville's jursidiction, and independent municipalities, which remained independent within the county. To explore this question, I estimate a geographic-boundary difference-in-differences model, where I compare the evolution of outcomes in Louisville relative to outcomes in both previously-unincorporated areas and independent municipalities. I limit these comparisons to blocks within less than one kilometer of the Louisville city border using fine-grained census block data. The results indicate consolidation caused house prices in independent municipalities (unincorporated areas) to rise (fall) by 5% (6%) relative to old-Louisville. Further, consolidation induced differential changes in the racial composition of these jurisdictions - the white population decreased in previously unincorporated areas by about eight persons per census block, and they were largely replaced by African Americans. The population in independent municipalities did not change, remaining almost wholly white. Unpacking potential mechanisms behind these changes, I find that the increase in house prices is concentrated in smaller independent municipalities with less land zoned for multi-family housing, providing evidence the shift in housing demand was driven by preferences for single-family zoning.

In the second chapter, I investigate the impact of the creation of mortgage "redlining" maps on homeownership rates by race. In the midst of the Great Depression, President Roosevelt created the HOLC to purchase over one million underwater mortgages in an effort to stave the housing industry's decline. To assess its new investments in mortgages around the country, the HOLC adopted a systematic appraisal methodology for valuing its residential investments in cities and produced what are now infamously referred to as "redlining" maps for the largest cities in the United States. In these maps, each neighborhood was given a color and a grade that showed its future potential, with the worst colored red. While the age and quality of the housing was a primary determinant for a neighborhood's "grade", the presence of even a few minorities was justification for an immediate downgrade of the neighborhood. The question is, how were these maps used, if at all, in making mortgage lending decisions among the FHA and/or private lenders? There is some historical evidence of information sharing between the HOLC and other parties, but the lack of sufficient records makes it difficult to determine the extent of their use in mortgage lending decisions. In order to investigate the impact of the HOLC redlining maps on city homeownership rates, I use a regression discontinuity design, taking advantage of the fact that maps were only supposed to be created for cities with a population above 40,000. The results provide suggestive evidence the maps increased the white homeownership rate by 5 percentage points, while the impact on the black homeownership rate is unclear.

The last chapter explores the effects of a nationwide public health educational campaign in France in Post World War I. Mortality in France and many other Western countries experienced a dramatic decline between the late 1800s through the first half of the 20th century, but the root cause of this decline is still debated today. One of the primary hypotheses is that the decline was due to the introduction of public health programs. While there is evidence the construction of clean water infrastructure led to a considerable reduction in water-borne disease mortality, there is less research about whether the spread of the knowledge of germ theory and changes in hygienic behavior played a role in the decline of mortality from infectious diseases. To address this question, I study the effect of an educational public health campaign conducted by the Rockefeller Foundation in France after World War I on age-group mortality in a difference-in-differences framework. The results show the campaign reduced mortality of middle-aged adults, with men aged 20-39 experiencing a decline of about 0.592 per 1,000, and those aged 40-59, a decline of 1.04 per 1,000.

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Chapter 1

Fleeing the City Once More? The Impact of City-County Consolidation on Residential Choice

The segmentation of the American metropolis into a myriad of local governments began in earnest with the spread of the automobile in the early 1900s, and continues unabated to the present day. Proponents of this segmentation often cite the seminal work of Tiebout (1956), claiming fragmentation allows agents to sort into areas whose spending on public goods is best aligned with their preferences. In addition, supporters argue small local governments induce higher political participation because elected representatives often represent a close-knit population and can stay in touch with the needs of citizens on the street. However, detractors of segmentation believe it's infeasible to confront problems of a metropolitan-nature within a fragmented system (Rusk, 2013). Among many issues, the loss of residents to outlying suburbs, a lack of affordable multi-family housing, and increased racial residential segregation are often prominent in the public discourse. The debate between pro- and anti-fragmentation still continues today, yet there is sparse empirical evidence on whether a metropolitan government results in what detractors fear or what supporters hope, likely because so few places have ever adopted this form of government.

City-county government consolidation is often cited as the most straightforward mechanism to achieve metropolitan government (Rusk, 2013). In its purest form, consolidation combines both the functions and jurisdictions of a central city and its containing county, creating a single new government that assumes the responsibilities of both entities. Other types of metropolitan-style government that span multiple jurisdictions exist across the United States, such as shared water and sanitation districts, yet few areas have ever adopted a city-county consolidated government, mostly due to opposition from residents outside the central city.[1] However, as places like St. Louis, MO and Pittsburgh, PA seriously consider city-county consolidation to better confront metropolitan-level issues, it is imperative to understand its repercussions at the local level.

In this paper, I investigate within-metropolitan area changes of city-county consolidation using the Louisville-Jefferson County consolidation into Louisville Metro in 2003 as a case study. To do so, I leverage fine-grained GIS data in a geographic-border difference-in-differences framework to study the evolution of outcomes near Louisville's old city borders over time. Concretely, I use census blocks in Louisville within 250-750 meters of the city border in 2000 as controls, comparing the evolution of outcomes in these blocks to outcomes in census blocks within 250-750 meters outside of the same border. In this consolidation, as in all recent city-county consolidations of note in the preceding century, political circumstances necessitated that municipalities other than the central city within the county

[1] State laws typically require approval from residents of both the central city and outlying areas to vote for consolidation in order for it to take effect.

remained independent after consolidation.[2,3] On the other hand, unincorporated areas previously under the jurisdiction of the county were brought directly under the jurisdiction of the new consolidated government. In my analysis, I consider independent municipalities and unincorporated areas as two distinct treatments to take these features of the consolidation into account.

The Louisville-Jefferson County consolidation had important repercussions on the housing market and on residential choice within the metropolitan area. In particular, consolidation caused house prices to rise by 5% in independent municipalities, while prices in previously unincorporated areas declined by 6%. Exploring why residents were drawn to independent municipalities, I find that the house price increase in independent municipalities is largely concentrated in smaller municipalities, which tend to have few (if any) parcels zoned for multi-family housing, and a largely white population. As for the supply of housing, there is no evidence of any impact on land-use policy, nor on the provision of new housing units. Turning to the demographic consequences of consolidation, the evidence shows consolidation caused differential residential choice by race - the white population declined by about 8 persons per census block in previously unincorporated areas, being replaced in almost equal number by African-Americans. Consolidation does not shift the racial demographics of municipalities, which remain largely white. Finally, in exploring the mechanisms behind these shifts in demand and population, I provide suggestive evidence they were partly driven by preferences for zoning; in particular, the observed house price increases in independent municipalities are concentrated in those which have virtually no multi-family housing within their jurisdiction. In sum, the Louisville-Jefferson county consolidation caused a differential shift in the demand for housing in Jefferson county depending on jurisdiction type - independent municipalities became more attractive, while previously-unincorporated areas lost. These changes appear to be driven at least in part by differential preferences by race. In particular, a significant number of whites shifted their choice of residence away from unincorporated areas after it was absorbed into the Metro government.

The consolidation of Louisville and Jefferson county is an ideal setting with features that allow me to robustly identify the impact of consolidation. In particular, an "Urban Services District" (USD) was created that mimicked the borders, enforced identical taxes, and provided the same public goods as pre-consolidation Louisville. For example, garbage pickup

[2] In every consolidation of central cities with a population above 250,000 in the past century, previously-independent municipalities were allowed to stay independent. These are Indianapolis and Marion County IN in 1970; Jacksonville and Duval County, FL in 1968; Nashville and Davidson County, TN in 1958.

[3] While these independent municipalities still enjoyed the same municipal powers endowed by state law, they maintained the same relationship to the consolidated government as they previously bore to the county government.

and fire services previously provided exclusively to Louisville were not extended to other areas outside of the USD. Thus, the initial consequence of consolidation was solely on the governing structure within the county: residents of both Louisville and previously unincorporated areas were under the direct jurisdiction of the newly formed Metro government, and independent municipalities considered the Metro government as its county government. Any decisions about whether or not services would be altered or tax rates changed was left up to the future Metro government. This element of the consolidation plan enables me isolate the impact of city-county consolidation and the corresponding change in governance itself, helping me to avoid confounders such as changes in service provision.

This article makes contributions to three main strands of literature. First, it shows there are important repercussions of a change in the structure of municipal governments within a metropolitan area and patterns of residential choice. The seminal works in this literature (Tiebout (1956), Ellickson (1971)) argue residents "vote with their feet" by moving to jurisdictions that most closely align with their preferences. In my setting, the evidence suggests city-county consolidation shifts demand for housing away from previously-unincorporated areas and toward independent municipalities, implying some residents indeed voted with their feet. There is also a burgeoning literature quanitifying the willingness-to-pay of residents to live in certain jurisdictions relative to others. For example, Boustan (2013) shows that residents of high-income suburbs bordering large central cities are willing to pay a significant amount to live close to, but not within, central city jurisdictions. Conversely, Schönholzer and Zhang (2017) finds that residents of annexed areas are willing to pay more to live in municipalities, although this study considers the universe of annexations, not just in metropolitan areas.[4]

Second, my paper contributes to previous research exploring the relationship between race and residential location. In particular, there is a robust literature documenting the interplay between racial population movements within metropolitan areas. Boustan and Margo (2013) finds that whites leaving Northern central cities after the Great Migration opened up housing opportunites for minorities. Similarly, my findings suggest consolidation induced a shift in demand for housing among white residents, thereby having repercussions on residential choice for African-Americans. In related work, many researchers have studied the arrival of southern African-Americans into Northern central cities in the early 20th century and the corresponding "white flight" away from central city neighborhoods. In particular,

[4] Almost all state laws require a majority vote of the annexed area before annexation can occur, implying their sample of annexations is likely "biased" toward those which were desired by the population being annexed. In my setting, the absorption of the unincorporated areas into Louisville Metro only required the majority vote of the entire county, so even though unincorporated areas as a whole voted against consolidation, they were still forced to join Metro's jurisdiction.

they show this movement induced high levels of racial residential segregation, with African-Americans increasingly isolated in central cities (e.g. Boustan (2010), Shertzer and Walsh (2019)). In my case, the flight of whites away from unincorporated areas is driven not by incoming migrants, but by the absorption into the new Metro government.[5]

Finally, this paper contributes to the literature that studies the effects of city-county consolidation and municipal cooperation more broadly. City-county consolidation has been discussed widely in the urban studies literature, although empirical work largely focuses on across-metropolitan comparisons. Their focus is typically outcomes such as economic growth (Hall, Matti and Zhou, 2020) or government spending (Faulk and Grassmueck (2012), Taylor, Faulk and Schaal (2017); Martin and Schiff (2011)). However, the paucity of consolidations generally makes it difficult to undergo across-metropolitan area analyses. Within an international context, Tricaud (2020) studies the repercussions of a French reform that forced municipalities to join an inter-municipal agreement. They find municipalities experienced increased housing construction but also increased revenues; in addition, rural municipalities lost access to some public services, while gaining better public transportation access.[6] On the theoretical side of city-county consolidation, some proponents in the literature argue consolidation will lead to better management of suburban sprawl by permitting higher density land-use and multi-family housing outside the city center (Downs (1994)).[7] They posit this will lead to lower levels of racial segregation as housing outside central cities becomes more accessible to those with lower incomes (Rusk (2013), Jepson (2008)). While I find higher numbers of African-Americans in previously unincorporated blocks after consolidation, it does not appear to be due to an increase in housing supply, but rather a shift in the residential patterns of whites. This shift opens up opportunities for African-Americans to settle outside Louisville.[8]

The remainder of the paper is organized as follows. Section 1Institutional Backgroundsection.1 gives a brief explanation of how consolidation imapcted the governing structure and provision of public goods within the county. Section 2Data and Empirical Designsection.2 describes the data used and the empirical strategy I follow. Finally, 3Resultssection.3 presents the results a discussion of the potential mechanisms driving the results.

[5]To be clear, housing demand may have decreased in unincorporated areas because of expected influxes of minorities from the central city.

[6]See also Bel and Warner (2015), Blom-Hansen et al. (2016), Reingewertz (2012).

[7]There is an expansive literature highlighting the outsize influence of a few individual actors on localized zoning regulations Einstein (2021). By putting zoning power in the hands of a metropolitan government, supporters of consolidation believe it could potentially remove this influence and empower the new government to take a more regional approach in how to attribute land to different uses.

[8]Trounstine (2020) shows that land-use policy can indeed be responsible for enforcing racial residential segregation, and that white communities in California are more supportive of restricting additional residential development.

1 Institutional Background

A typical metropolitan area in the United States consists of a large central city within a county, surrounded by a number of independent municipalities as well as unincorporated areas, who are under the jurisdiction of the county. Each provides their own set of public goods and raises funds through varying means, often dependent on state law. In turn, residents choose where to reside within the county, often balancing their preferences over taxes levied and public goods provided by the different jurisdictions with other factors such as the distance to their place of work or local amenities. Louisville and Jefferson county is not unique in this regard; in fact, in many ways it is representative of the median metropolitan area in the United States.

The consolidation of Louisville and Jefferson County is an ideal candidate to study for a variety of reasons. First and foremost, it is the largest city-county consolidation in recent history. Second, Louisville in 2000 was on many dimensions representative of a median large metropolitan area (MSA) in the United States. It was the 50th largest MSA by population, and was similar to a median MSA on a variety of dimensions, including urbanization rate, age, homeownership rate, and poverty rate (see Figure 11Louisville MSA vs. Median Top 100 MSAtable.caption.29).[9] The most significant difference between the Louisville metropolitan area and the median large MSA is the age of the housing stock, where it is significantly older in Louisville than in others.[10] Louisville's adults are also slightly less educated and more likely to be employed in manufacturing relative to the median MSA. Excepting these minor differences, Louisville provides an ideal setting to study the impact of city-county consolidation. Of course, the lessons taken from studying Louisville may not apply to all metropoles in the United States. In the largest areas, such as Los Angeles, California or Cleveland, Ohio, the county holding the center city is entirely incorporated by municipalities, and the urban area spans well beyond the county's border. Since Louisville's consolidation with Jefferson County primarily involved unincorporated areas, the effects we identify may not be applicable to these metropolitan areas. In addition, those same areas are also surrounded by independent municipalities large enough to be fully-fledged cities in their own right, providing services like schooling and policing; on the other hand, most of the municipalities surrounding Louisville are very small and purely residential. While we may not expect the effects from this city-county consolidation to replicate in every metropolitan area, Jefferson County bears a resemblance to many. Residents of most metropolitan areas face the same tradeoffs as in Jefferson County when chosing where to reside, balancing preferences

[9] I define large MSAs as the one hundred largest MSAs in the country by population.

[10] Half of this difference is due to MSAs in the Sunbelt, where the housing stock is relatively new.

on jurisdiction-type, commuting costs, as well as current and expected policies such as tax burdens and land-use patterns.

In the following section, I will briefly summarize the institutional details about governance, public goods and taxes in the various jurisdictions of Jefferson county, before explaining which aspects of these changed and which did not under consolidation.

1.1 Pre-Consolidation Governance

Governments and Public Goods. Before consolidation, the city of Louisville was governed by a 12-member Board of Aldermen along with a mayor. Each member of the Board was elected from a geographic district representing about twenty one thousand residents, with the mayor elected city-wide. Louisville provided a number of public goods and services to its citizens, including its own police, fire and emergency services, street cleaning, waste disposal, recycling, among others.

There were 81 independent municipalities in Jefferson county in 2000, with as few as 154 residents (Lincolnshire) and as many as 17,283 (Saint Matthews). Depending on their population size, municipalities were given a designation ranging from sixth-class to first-class.[11] With this designation came certain rights as well as obligations to their respective residents. For example, only fourth and fifth-class municipalities were permitted to enact a restaurant tax[12]; second and third-class municipalities were required to have a police and fire department, but for other municipalities this was optional (2010).

Jefferson county was governed by a Fiscal Court consisting of three commissioners and an executive. While the commissioners were responsible for representing one of three districts of over 200,000 people, they were elected at-large. In other words, a district might not be represented by the commissioner they voted for if the other districts voted differently. The county executive was elected from and represented the entire county. While the Fiscal Court's jurisidiction was technically the entire county, most of the decisions regarding public services in Louisville and independent municipalities were made by their respective governments. However, unincorporated areas were under the direct jurisidiction of the county government. The county was first and foremost was responsible for carrying out duties tasked by the state, such as the management of elections, maintenance of health records and state property tax collection. Besides these functions, Jefferson county also had its own police department, emergency services, and public works department.

[11] Typically, the state would only change the status at the request of the city, so there were instances in which the population of a city did not match its designation.

[12] Cities of the fourth and fifth classes that have established a tourist and convention commission may levy a restaurant tax not to exceed three percent (3%) of gross retail sales of restaurants within the city to fund the commission.

Finally, some public goods were provided according to a joint agreement between Louisville and Jefferson county. These included the Library System, Metropolitan Sewer Division and the Parks Department, among others. One of particular relevance to homeowners was the Louisville-Jefferson County Planning Commission, who were tasked with making recommendations for the zoning code of the county. For a zoning change to occur, the Planning Commission would first receive a request for a zoning change from the requestor (e.g. developer). Property owners in the immediate vicinity were notified of this request and the Commission held a public hearing where these owners could support or protest the zoning change. After the hearing, the Commission made a yes-no recommendation based on the comments from the hearing and according to the state-mandated "Comprehensive Plan," (*Kentucky Revised Statutes (KRS), Chapter 100*).[13] This recommendation was then sent to the respective governing body to be accepted or rejected, whether it was Louisville, an independent municipality, or the county government in the case of unincorporated areas. However, not all independent municipalities had this veto power - state law dictated that only fourth-class municipalities and above were given the power to accept or reject the recommendation. Rezoning in fifth and sixth-class municipalities was delegated to the county Fiscal Court.[14] Only twelve of the eighty-one independent municipalities within the county were of fourth-class or higher designation.

Taxes. The residents of Jefferson county paid two principal taxes before consolidation - property taxes and payroll taxes.

Property taxes were levied by essentially all jurisdictions within Jefferson county, at varying rates. The county government levied a property tax to fund their government operations, although these were relatively low ($0.129 per $100 assessed value in 2000). This tax was applicable to the entire county, including properties in unincorporated areas, Louisville, and independent municipalities. Louisville also levied a property tax, which was typically the highest among municipalities within the county ($0.394 per $100 assessed value in 2000), although not as high as the county-wide school district property tax.[15] Finally, independent municipalities levied property taxes ranging from as low as $0.12 per $100 assessed value (Houston Acres) to as high as $0.39 per $100 (Strathmoor Manor) in 2000. However, all

[13]Kentucky state law required that counties adopt a Comprehensive Plan in order to have the right to set zoning laws.

[14]This requirement was exceptionally granted to Jefferson county in 1966, when the legislation for the requirement for a Comprehensive Plan first passed.

[15]The Jefferson County Public School system (JCPS) levied the highest property tax among taxing jurisdictions in the county ($0.532 per $100 assessed value in 2000). JCPS was an independent entity with their own elected leaders and tax-raising power, and had jurisdiction of the entire county. Their property tax was paid by all residents in the county. JCPS was merged with the previous Louisville School District in the 1970s to combat racial segregation.

jurisisdictions in Kentucky were limited in their ability to increase their property tax revenues by state law. A bill passed in 1979 (commonly referred to as HB44) dictated that no city, county, or other taxing jurisdiction could adopt a tax rate which implied property tax revenues in excess of 4% of the previous year's property tax receipts.[16] If any jurisdiction wanted to set a rate whose implied revenues were in excess of 4% of the previous year revenues, residents could seek the signatures of 10% of the population of the taxing jurisdiction to take the tax increase to a recall vote (*KRS 68.245*, *KRS 132.010*, *1978 Ky Acts ch. 197, sec. 4*).

The payroll tax was levied on wages paid to employees by a business located within the taxing jurisdiction, regardless of where the employee resided. Both Louisville and Jefferson county set their respective payroll tax rates at 1.25%, the legal maximum set by Kentucky state law for cities and counties (*KRS 91.200, 1942 KY Acts ch. 208, sec. 1*).[17] Independent municipalities set their own occupational tax rates, although this was only applicable to three municipalities in the county, as the majority of them were purely residential.[18]

1.2 Post-Consolidation Governance

In the latter half of the 1990s, city and civic leaders began a movement to consolidate Louisville and Jefferson county with the objective to better coordinate business attraction and increase economic development in the region (Wachter, 2013). After commissioning various studies and passing through multiple legal complications with the state legislature, city-county consolidation was put on the ballot for the residents of the county to vote on in November 2000. The measure passed by a margin of 54-46, with the consolidation taking effect in January 2003.[19]

The crafters of the city-county consolidation plan wrote it purposefully so that its only immediate impact was to merge the city and county governments. In other words, it only affected *how* decisions were made - any other choices related to changes in service provision or taxes would be left to the new consolidated government. The following section briefly summarizes how consolidation affected the structure of governance changed, as well as the

[16]The 4% rule only applied revenues from current properties, so if new properties were built, the property tax revenues garnered from these were not included in the 4% calculation, but would then be included in the following year's calculation.

[17]Since 1985, Louisville and Jefferson county were under a tax-sharing agreement to split the revenues they each earned from the occupational tax. See Appendix AFragmentation and Consolidationappendix.1.A for more details.

[18]The three independent municipalities who levied occupational taxes were Saint Matthews, Jeffersontown, and Shively.

[19]For more details regarding the process leading up to consolidation, see Appendix AFragmentation and Consolidationappendix.1.A.

eventual changes in taxes and services made by the new government after consolidation.

Governments and Public Goods. City-county consolidation the consolidation of the Louisville and Jefferson county governments into the new Louisville Metro government. Louisville Metro had two simultaneous functions - they were a municipal government for the former city of Louisville and previously unincorporated areas of Jefferson county, while they were a county government to the still-independent municipalities. Thus, while everyone in the county had a newly structured county government, Louisville and the unincorporated areas were under a new municipal government, and independent municipalities kept their respective municipal governments.

The new Metro government had a mayor-council structure: the Council was made up of 26 representatives, each elected from newly formed jurisdictions of about twenty five thousand residents, while the Mayor was elected from the county as a whole.[20]. Since Metro acted as a county government, the representatives were elected from jurisdictions that covered the entire county, including independent municipalities.

While the governance of the county and Louisville underwent change after consolidation, public goods and services were provided in almost precisely the same manner as before consolidation. This was embodied in the creation of a special taxing and services district, known as the Urban Services District (USD). The geographic boundaries of the USD mimicked the boundaries of Louisville before consolidation, and provided identical services (e.g. fire protection, street cleaning, waste disposal, recycling, etc.). Residents of previously unincorporated areas and independent municipalities did not receive these services from the USD (as it was before consolidation), but continued to contract the services they desired. If any neighborhood sought to join the USD, this would be put to a vote of the Metro Council.[21] The services provided by independent municipalities to their own jurisdictions were maintained according to their own governments.

The agencies previously managed jointly between the city and county were simply brought under the umbrella of the new Metro government. As for the Planning Commission, the process by which rezoning decisions took place was the same. However, the new Metro government replaced the Louisville and Jefferson county governments' role in accepting or rejecting recommendations from the Planning Commission. Thus, zoning decisions in what was previously Louisville, unincorporated, and fifth/sixth class cities was within Metro government's realm. Independent municipalities that previously held zoning powers (fourth-class and above) retained that right in the new governing structure.

[20]The jurisdictions were drawn by an independent geopgrapher from the University of Louisville whose stated objectives were for the jurisdictions to be geographically contiguous (e.g. not gerrymandered) and containing similar populations (Dakan, 2000)

[21]Since its creation in 2003, the USD boundaries have never changed.

Finally, one unxpected change that occurred was the consolidation of the city and county police forces. The county police union was opposed to city-consolidation and contributed funds to campaign against it (Shafer, 2000*c*). Their opposition was largely attributed to the fact that the county police union had negotiated better benefits versus the city police union, including more paid holidays and higher salaries, and the county police were afraid of losing these benefits. However, after consolidation was approved by the voters, the police unions agreed amongst themselves to consider merging departments. Louisville city overcame the county union's reticence by renegotiating its union contract with the city police department in 2002 to replicate the agreement between the county government and its police force. This paved the way for the departments' merger in 2003.

Taxes. The consolidation plan that went into effect in 2003 did not have any impact on tax rates in the county. The creation of the Urban Services District meant residents inside the USD paid identical property taxes as those in Louisville before consolidation to fund the extra services provided. Residents in independent municipalities continued to pay the rates set by their government, while everyone county-wide (including previously unincorporated areas) paid the county property tax. However, by changing the goverance structure in the county consolidation did inherently change who set property tax rates. The new Metro government had the power to change both the USD and the county property tax rate, although this would have required a vote of the Metro Council, and they were still limited to enact any significant increases by the HB44 legislation. For these reasons, USD and county property tax rates continued their downward trend after consolidation (see Appendix Figure 9Property Taxes in Jefferson countyfigure.caption.27). As for the payroll tax rates, these remained at their legal maximum of 1.25% countywide.

2 Data and Empirical Design

2.1 Data

Sources. The first set of outcomes looking at the impact of consolidation on house prices uses data collected from the Jefferson County Property Value Assessor's website (PVA). For every parcel of land in Jefferson County, the PVA has detailed data on each sale from 1997-2010, including the sale date, amount, and the name of the purchaser of the parcel. It also contains details about the structure of the building(s) on the parcel (e.g. the number of bathrooms, the year it was built, the floor area). With these detailed data, I create a house-price index at the census block-year level using a hedonic price model, as in Baum-Snow and

Han (2020), among others.[22,23]

To complement the data on house prices, I retrieved data from the National Historic Geographic Information Systems (NHGIS) and the Louisville/Jefferson County Information Consortium (LOJIC) to investigate the evolution of housing supply and land-use. The NHGIS contains geo-coded (GIS) U.S. Census data available at the block level, a small geographic area typically of about 100 people, for the census years of 1990, 2000, and 2010. From here, I obtained the total number of housing units per census block.[24] To further investigate potential changes housing supply and land-use regulations, I obtained current and archived GIS maps that contain the location and zoning designation of each contemporary parcel. These maps encompass Jefferson County and are available for the years of 1996, 1999, 2003, 2005, and 2011. With these data, I measure the number of parcels dedicated to multi-family uses.

For analyzing the impact of consolidation on population patterns, I collected geo-coded (GIS) data on population by race at the census block level from the NHGIS.

Data Construction. The final dataset is a panel of outcomes at 2010 census block geographies (i) observed at varying years (t), depending on the data availability. Since census blocks change over time, I use a crosswalk provided by the NHGIS to map blocks and their associated outcomes from 1990 and 2000 to 2010 blocks. This allows me to use geographically-consistent units across years for the analyses below.[25]

As part of my empirical strategy, it is essential to identify where census blocks are geographically located in terms of their jurisdiction and distance to the Louisville border. To identify these key attributes, I use GIS census block polygons from the NHGIS.[26]. I map the the centroid of each polygon to its respective jurisdiction, and calculate the distance of the centroid to the closest point on the Louisville border as it stood in 2000. I also use this closest point on the Louisville border to assign each census block to a "neighborhood", or in other words, a shared border that is common to census blocks in different jurisdictions. To do so, I split up the Louisville border into 1 kilometer border segments and assign each

[22]I do not use a repeat-sales index as many homes in my sample period are not sold more than once, reduing the available sample considerably. See Appendix B.1House Price Data and Index Constructionsubsection.1.B.1 for more details on the construction of the house price index.

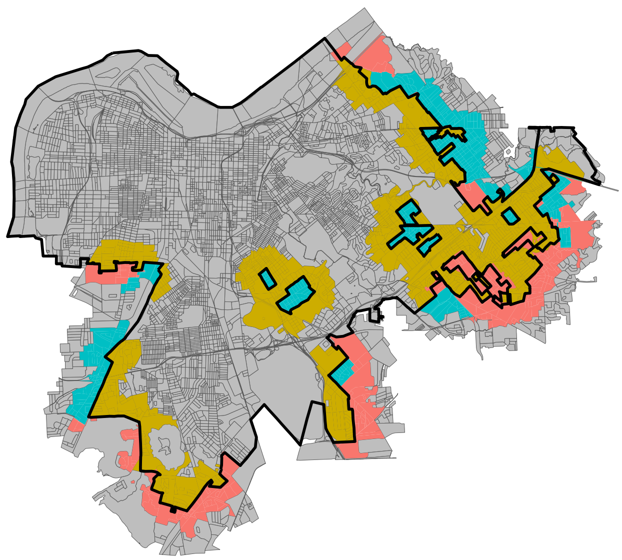
[23]For the purposes of my analysis, I consider sales of single-family homes and omit parcels used for multi-family, commercial and industrial purposes.

[24]While it would have been advantageous to have the number of units split into single family vs. multi-family units, this is unfortunately not available at the census block level. This data is available at the census block group level, but block groups have considerable overlap with jurisdictional borders, introducing significant measurement error in assigning outcomes to their corresponding jurisdiction.

[25]The crosswalk as it's currently provided performs well at matching most blocks across years, although it did require manual changes in some cases. See Data Appendix B.2NHGIS Census Block Crosswalksubsection.1.B.2 for more details.

[26]In almost all cases, census blocks were drawn to respect jurisdictional borders.

Figure 1: Sample Census Blocks



Notes: This is a map of 2010 census blocks in Jefferson county, including blocks inside Lousiville and those within 1500 meters of the Louisville border in 2000. The sample of census blocks used throughout the analysis is contained within the colored blocks: yellow blocks are the control blocks inside Louisville, blue blocks are those in independent municipalities, and red blocks are unincorporated areas. Census blocks within large industrial areas, railroad yards, natural parks, and airports are omitted from the sample.

segment a unique identifier b - then, I assign each census block i to the border segment b to which said closest point belongs. When calculating the distance of census block centroids to its closest point on the Louisville border, I omit border segments that are encompassed by large industrial areas, railroad yards, natural parks, and airports from my sample.[27] Buffer sizes around the Louisville border of 250, 500, or 750 meters generate samples of 877, 1386, or 1857 census blocks per year, respectively. Figure 1Sample Census Blocksfigure.caption.1 shows the sample using the largest buffer of 750 meters.

Table 1Summary Statistics, 2000table.caption.2 presents summary statistics of our main variables of interest in 2000 by census block, separating them by jurisdiction type (either Louisville, independent municipalities, and unincorporated areas). The first three columns present mean statistics of the entirety of the respective jurisdiction, and the latter three

[27] In other words, I seek to compare census blocks across jurisdictions that share the same set of spatial characteristics. Census blocks that are separated by features such as industrial areas likely do not share characteristics such as accessibility or distance to schooling, etc. This practice is used elsewhere when utilizing similar empirical strategies across borders, e.g. Boustan (2013).

Table 1: Summary Statistics, 2000

	Full			500m Buffer		
Outcome	Louisville	Muni.	Unincorp.	Louisville	Muni.	Unincorp.
House Prices	104200	165600	140800	116400	136100	105400
% Homes Sold	6.6	6.8	5.9	6.3	6.7	6.2
% Single-Fam Zoning	59	67	69	70	74	74
% Multi-Fam Zoning	34	27	25	23	18	20
% Renters	40	18	18	24	22	26
% Black Pop	30	7	9	12	6	15

columns contain the same statistics but limited to areas within 500m of the Louisville border in 2000, in the spirit of my geographic-border empirical strategy. Comparing the different jurisdictions' as a whole, independent municipalities had the highest house prices relative to Louisville and unincorporated areas. On other dimensions, the municipalities and unincorporated areas were very similar - they both had high levels of single-family zoning, a low proportion of residents who were renters, and a low proportion of African-American residents. On the other hand, the proportion of households that rented and the proportion that were African-American was much higher in Louisville than in both the municipalities and unincorporated areas. Louisville also had more area zoned for multi-family housing.

The differences in outcomes become less pronounced when limiting the comparisons to areas within five hundred meters of the Louisville border. In particular, zoning patterns and the proportion of renting households are almost equal across jurisdiction types. However, house prices are still significantly higher in independent municipalities, and the proportion of African-American households is much lower in municipalities than in Louisville and unincorporated areas. Overall, limiting the sample diminishes differences in our variables of interest before consolidation across jurisidictions, although disparities clearly still remain. I will next present my empirical strategy, where I take these differences into account when identifying the impact of consolidation.

2.2 Empirical Strategy

The main strategy used to study the impact of Louisville's consolidation with Jefferson County is a geographic-boundary difference-in-differences, similar to the framework seen in Aaronson, Hartley and Bhashkar (2021) and Dube, Lester and Reich (2010), among others. In other words, I compare outcomes of areas just inside the old Louisville borders with areas just outside these borders, before and after consolidation.

The baseline model estimated is as follows:

$$y_{ibjt} = \alpha + \beta \text{Muni}_j \times \text{Post}_t + \phi \text{Unincorp}_j \times \text{Post}_t + \delta_i + \gamma_{bt} + \epsilon_{ibjt} \quad (1)$$

where i is a census block closest to Louisville border segment b in jurisdiction-type j in time t within a buffer distance (e.g. 500 meters) around the border. j, defines whether i was in the center city, an independent municipality, or an unincorporated area using pre-consolidation geography. δ_i are census block fixed effects, soaking up any time invariant characteristics specific to each block, such as the distance to the city center. α_{bt} are border segment-year fixed effects. These sweep out time-specific shocks unique to each 1 kilometer border segment, b, such as the construction of a new highway or the introduction of a new bus line. Muni_j and Unincorp_j are indicators for jurisdiction type, which can be viewed as treatment indicators (where Center City_j is the control (omitted category)). These are interacted with the post treatment indicator Post_t to quantify the impact of consolidation on our outcomes, captured by the coefficients β and ϕ. With the inclusion of δ_i and γ_{bt}, β and ϕ are identified off of variation within-unit i and within border segment-years bt. Standard errors are clustered at the census block level.

In terms of the timing of the treatment, there is little concern residents acted preemptively against consolidation, as the outcome of the vote was not a foregone conclusion. First, previous consolidation attempts in 1954, 1982, and 1983 failed. Second, in the years running up to the vote, there were multiple legal hurdles the city and county had to overcome before even being able to present consolidation to the voters. Finally, as the vote came closer, polls showed the vote was going to be close. For example, a poll released on 28 September 2000 indicated the consolidation opponents had a slight advantage, 46 to 41, with 13% undecided (McDonough, 2000*b*). After a promotional campaign led by political leaders from both sides of the political spectrum that outspent anti-consolidation forces by 15 to 1, consolidation was eventually approved by a margin of 54-46 on 7 November 2000.[28]

A crucial element of my empirical strategy is to estimate this model using census blocks within narrow bounds around the Louisville border, testing buffers of 250, 500, and 750 meters. Restricting comparisons to units within such a close distance absolves most concerns about any spatial factors that may influence outcomes across borders, such as access to local neighborhood amenities. If there are no other time-varying factors (t) within common border segments (b) that differentially influence outcomes (y) across jurisdictions (j), then β_{jt} can be interpreted as the causal effect of consolidation on my measured outcomes. In the current

[28]Voter turnout for merger question was 67% vs 69% for the election as a whole, showing widepspread interest in the issue among voters (McDonough, 2000*a*).

setting, there are two crucial elements of this assumption to address: first, the use of units on the inside of the pre-consolidation Louisville border as a control group; second, that outcomes in treatment and control units would follow the same trend absent consolidation (parallel trends).

Control Units. The use of census blocks inside the pre-consolidation Louisville border as a valid control group rests on the assumption that consolidation did not affect residential choice inside Louisville. Since the city itself played a role in the consolidation, it's possible the consolidation changed current (future) residents' preferences to remain living inside (move into) the city limits. In particular, I highlight three potential avenues through which consolidation may have influenced the choice to live inside pre-consolidation Louisville: taxes and the provision of goods and services, political alignment, and the consolidation of the city and county police forces.

The provision of public goods and services and corresponding tax rates do not appear to shift as a result of consolidation, supporting the use of areas inside Louisville as control units in my analysis. First, as discussed previously, the creation of the Urban Services District meant that services previously provided by the city remained the same after consolidation. Similarly, goods and services jointly provided by the city and county before consolidation continued under the jurisdiction of the Metro Council, which was composed of representatives from both city and county. Services provided independently of either the city and the county governments, such as fire protection and public transportation, remained independent after consolidation.[29] Thus, from a citizen's perspective, the provision of services post-consolidation was virtually identical to pre-consolidation. In terms of tax rates post-consolidation, while the payroll tax was already set at its legal limit inside Louisville, property taxes would be set by the Metro Council.[30] If residents' expected property tax rates to change as a consequence of consolidation and this correspondingly influenced their location and house-purchasing decisions, this would invalidate my assumption. However, the Kentucky state law HB44 required a vote of a jurisdiction's residents if the jurisdiction set a property tax rate that increased their property tax revenue by more than 4% relative to the previous year. For this reason, actual property tax rates in Louisville were on a downward trajectory in the years before the consolidation (since home values were rising), and the rates continued declining both in the interim period of 2001-2003 and from 2004 onward.

[29] If consolidation changed the quality of the provision of any of these of goods or services (or expectations about quality) to the extent that it changed residents' decisions, this would invalidate the use of these census blocks as controls. Unfortunately, this is not possible to rule out, as data on the provision of services at a disaggregated geographic level is not available.

[30] Of course, it was possible for the new Metro Council to lower the payroll tax rate. However, payroll tax revenue was the largest source of revenue for the city and county governments before consolidation, and it was widely expected the government would continue to set the payroll tax rate at the legal maximum.

While residents may base their residential choice based on political considerations, Democrats fulfilled the expectation of continued dominance of Louisville's mayor and city council into the Metro government. Thus, if a resident had preferences over the political party leading the jurisdiction, there was no change before and after consolidation, implying consolidation would not impact residential choice inside of Louisville along this margin. Indeed, as early as March 2000, there was speculation that pro-merger advocate Jerry Abramson, a well-liked Democrat who served an unmatched three terms as Mayor of Louisville from 1986-1999, would run for mayor of Metro should consolidation pass (Shafer, 2000*b*).[31] By the time the actual election for mayor came around in 2002, his candidacy was a "foregone conclusion," (Greenblatt, 2002).[32] As for Metro Council, while it was unsurprising for there to be Republicans elected as representatives given the inclusion of representatives from the entirety of the county, Democrats were elected to a majority of the Council seats at the first election in 2003, and only strengthened their majority in following elections.

In terms of the closeness of a residents' political representation, the number of constituents per representative in Louisville proper increased only slightly after consolidation, from about twenty-one thousand residents per representative on the Louisville city council to about twenty-six thousand residents per representative on the Metro Council. For perspective, the change was much more drastic for residents of unincorporated areas. They went from one of two-hundred thousand residents per county-commissioner to twenty-six thousand per Metro Council representative. One potential factor in the new Metro Council that may have changed residential choice inside Louisville was the urban-suburban composition of the Metro Council. In particular, representatives from jurisdictions which were outside of old Louisville made up a slight majority of the Metro Council, as the county population outside of Louisville was slightly higher than the population inside Louisville. If residents believed these suburban representatives would cross political party lines to pass policies that were detrimental to old Louisville, to the point where it impacted residents' housing choices, this would invalidate my approach. While I cannot definitively rule this out, the result of the vote for/against the consolidation in the election itself suggests this was not the driving factor in the public's opinion of consolidation. If the city-county consolidation was a matter of city versus suburbs, we should expect to see voters inside Louisville vote against consolidation at higher rates relative to independent municipalities. However, Louisville and independent municipalities voted for consolidation at about the same rate, while unincorporated areas voted against consolidation in higher numbers (McDonough, 2000*d*).

[31] Abramson ran into the three-term limit under previous Louisville statutes, but the creation of the new Metro government meant he could run once more, as the term limits no longer applied.

[32] The successor to Abramson as Louisville city Mayor in 1999, David Armstrong, claimed Abramson had been "running [for Metro mayor] since the day he left" as mayor two years prior (McDonough, 2000*d*).

Parallel Trends. The structure of the empirical design coupled with parallel pre-treatment trends in outcomes together suggest the parallel trends assumption likely holds in this setting. First, the specification in Equation 1Empirical Strategyequation.2.1 identifies the impact of consolidation using variation within a tight buffer area around the Louisville border, and even further, it restricts the comparisons across jurisdictions to the same border segment (γ_{bt}). In particular, γ_{bt} allows for shocks across time to vary by border segment, absorbing any common factors such as the construction of a new grocery market to the addition of a public transporation bus line. In order for a confounding factor to bias my coefficients of interest β and ϕ, it would have to systematically vary within these border segments b and differentially impact one side of the border. While it is not possible to definitively rule the presence of a confounder, I test for pre-existing trends in my outcomes by estimating a flexible specification as shown Equation 2Empirical Strategyequation.2.2.

$$y_{ibjt} = \alpha + \sum_{t \in T, \neq 2000} \beta_t \text{Muni}_j \text{Year}_t + \sum_{t \in T, \neq 2000} \phi_t \text{Unincorp}_j \text{Year}_t + \delta_i + \gamma_{bt} + \epsilon_{ibjt} \tag{2}$$

The impact of consolidation on independent municipalities and unincorporated areas, captured by the coefficients β_t and ϕ_t, is allowed to vary by year. If a confounding factor existed prior to consolidation, these coefficients should be significantly different from zero when $t < 2000$. I present the results of the flexible estimation along with the main results in Section 3Resultssection.3.

3 Results

In the following section, I present the results from estimating the baseline Equation 1Empirical Strategyequation.2.1 at the three different buffer sizes in table form, while I present results from the flexible specification of Equation 2Empirical Strategyequation.2.2 in graphical form, using only the 500m buffer sample (results for the buffer sizes of 250 meters and 750 meters are displayed in Appendix EMain Results: Figuresappendix.2.E). The figures contain the point estimates and 95% confidence intervals of the coefficients of interest, β_t and ϕ_t, which represent the impact of consolidation on the given outcome in independent municipalities and unincorporated areas, respectively. I will first present the results on the housing market - the impact of consolidation on house prices, housing supply (housing units and zoning), and home sales. Then, I present results describing the impact of consolidation on demographic outcomes, before going on to discuss potential mechanisms.

3.1 Housing Market

Table 3Impact on House Pricestable.caption.3 presents results estimating Equation 1Empirical Strategyequation.2.1 where the outcome y_{ijbt} is the census block house price index. The impact of consolidation on independent municipalities (Muni x Post) and unincorporated areas (Unincorp x Post) is consistent across the three bandwidths. In particular, consolidation caused house prices to rise in independent municipalities by about 4.6% (using the 500m buffer). At the same time, house prices fell in unincorporated areas by around 6.3% immediately after consolidation. These estimates are statistically significant at the 1% level.

Tracing out the impact of consolidation on prices over time, Figure 2Impact on House Pricesfigure.caption.5 presents the estimates of the impact of consolidation on the house price index using the flexible specification in Equation 2Empirical Strategyequation.2.2. The results are consistent with the timing of consolidation beginning to take hold in 2003, the year consolidation was implemented. In particular, the point estimates immediately shift in the positive (negative) direction in independent municipalities (unincorporated areas) in 2004, and become statistically significant by 2006. Additionally, Figure 2Impact on House Pricesfigure.caption.5 confirms that house prices in independent municipalities and unincorporated areas were not differentially changing compared to prices in the center city prior to consolidation. In particular, all estimated coefficients on the treatment variables interacted with year indicators before 2000 (β_t and ϕ_t, $t < 2000$) are close to and not significantly different from zero. This confirms there were no pre-existing confounding factors causing trends to diverge before the treatment, and lends credence to the parallel trends assumption.

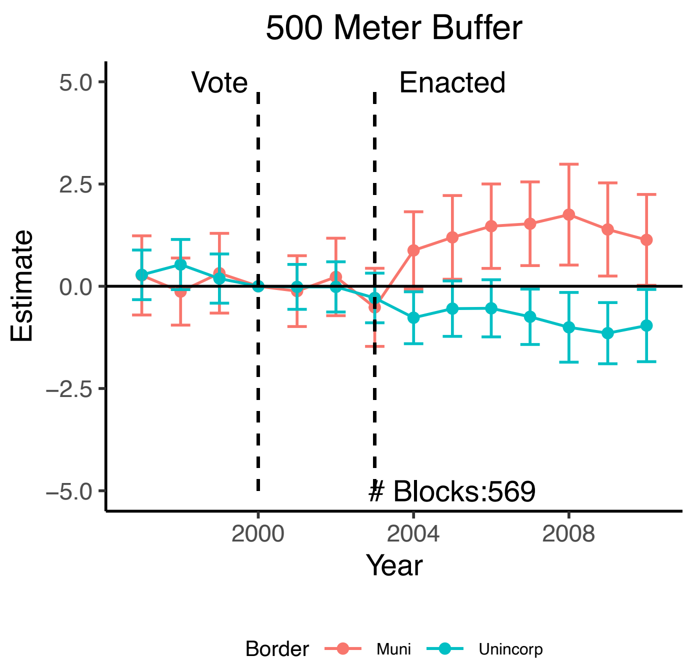
To explore whether this shift in prices may have been due to a change in housing supply, Table 4Impact on Number of Housing Unitstable.caption.6 presents results estimating Equation 1Empirical Strategyequation.2.1 where the outcome y_{ijbt} is the total number of housing units. The evidence strongly suggests supply was not influenced by consolidation. None of the estimates using any buffer size in either independent municipalities or unincorporated areas are significantly different from zero. This is reinforced by estimating Equation 2Empirical Strategyequation.2.2 with the total number of housing units as the outcome, the results of which are presented in Figure 3Impact on Number of Housing Unitsfigure.caption.8. Again, no estimates show a significant difference in the number of housing units available in either unincorporated areas or independent municipalities. However, due to the wide prevalence of single family housing in Jefferson county, using the total number of housing units as my dependent variable may mask differential shifts in the provision of multi-family housing. While my dataset does not allow me to delineate housing units by type, I now explore how the zoning code changed after consolidation. Specifically, if multi-family zoning practices within the county changed as a result of consolidation, housing supply likely reacted in a

Table 3: Impact on House Prices

	House Price Index		
	250m	500m	750m
Muni x Post	1.128	0.7822	0.5606
	(0.3069)	(0.2540)	(0.2335)
Unincorp x Post	-1.221	-0.8440	-0.9616
	(0.2396)	(0.1955)	(0.1781)
Observations	3,230	5,230	6,934
R^2	0.841	0.852	0.860
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni Index	17.41	16.89	16.67
Mean Unincorp Index	13.70	13.37	13.15

Notes: This table shows estimates of Equation 1, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is a house-price index, which is estimated at the census block-year level as described in the Data Appendix. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

Figure 2: Impact on House Prices



Notes: The above figure presents from estimating Equation 2Empirical Strategyequation.2.2 where the outcome y is the census block house price index. The red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). The sample includes census blocks within a 500m buffer of the pre-consolidation Louisville border. Standard errors are clustered at the census block level.

similar manner.[33]

Table 4: Impact on Number of Housing Units

	Number of Housing Units		
	250m	500m	750m
Muni x Post	0.8577	0.7722	0.6888
	(0.6564)	(0.4995)	(0.6078)
Unincorp x Post	6.122	4.319	3.187
	(4.134)	(2.846)	(2.419)
Observations	1,990	3,025	3,962
R^2	0.982	0.984	0.985
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni Units	22.20	25.51	28.99
Mean Unincorp Units	67.41	64.75	62.83

Notes: This table shows estimates of Equation 1, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the total number of housing units in a census block. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

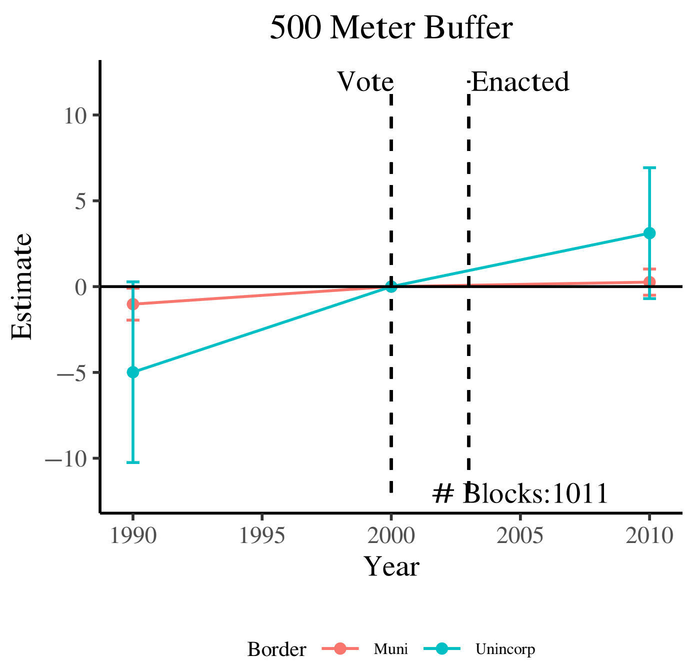
Table 5Impact on Multi-Family Zoningtable.caption.9 presents results estimating Equation 1Empirical Strategyequation.2.1 where the outcome y_{ijbt} is an indicator for the presence of a parcel zoned for multi-family housing within the census block.[34] The estimates for β across all bandwidths show we cannot reject a null hypothesis of consolidation having no impact on multi-family zoning in independent municipalities. Estimates are statistically indistinguishable from zero (e.g. using the 500 meter bandwidth, β: -0.0196; se: 0.0154). Similarly, the estimates for the impact of consolidation on zoning in unincorporated areas, ϕ, are indistinguishable from zero. All point estimates are quantitatively small and within a standard error of zero. This is consistent with an almost completely unchanged zoning code in Jefferson county over this period. This is visually verified in Appendix Figure 10Zoning Maps of Jefferson Countyfigure.caption.28, which displays zoning maps for the years available. .

The evidence thus far strongly suggests consolidation caused demand for housing near

[33] Zoning a parcel of land for a certain use does not necessitate any construction to actually occur.

[34] Area zoned for multi-family housing includes any parcel zoned for residential use that is not zoned for single-family housing, e.g. duplexes and triplexes, as well as apartment buildings.

Figure 3: Impact on Number of Housing Units



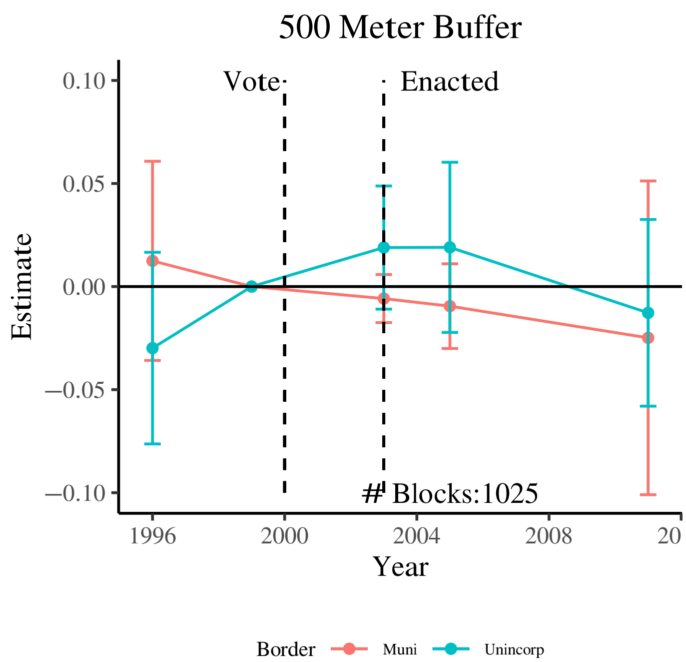
Notes: The above figure presents from estimating Equation 2Empirical Strategyequation.2.2 where the outcome y is the number of housing units. The red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). The sample includes census blocks within a 500m buffer of the pre-consolidation Louisville border. Standard errors are clustered at the census block level.

Table 5: Impact on Multi-Family Zoning

	Multi-Family Zoning		
	250m	500m	750m
Muni x Post	-0.0280	-0.0196	-0.0200
	(0.0169)	(0.0154)	(0.0146)
Unincorp x Post	-0.008	0.0232	0.0179
	(0.0231)	(0.0210)	(0.0222)
Observations	3,370	5,119	6,708
R^2	0.781	0.781	0.782
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni MFH Zoning	0.0954	0.1020	0.1317
Mean Unincorp MFH Zoning	0.1875	0.2014	0.2054

Notes: This table shows estimates of Equation 1, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the total number of housing units in a census block. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

Figure 4: Impact on Multi-Family Zoning



Notes: The above figure presents from estimating Equation 2Empirical Strategyequation.2.2 where the outcome y is an indicator for the presence of a parcel zoned for multi-family housing within the census block.. The red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). The sample includes census blocks within a 500m buffer of the pre-consolidation Louisville border. Standard errors are clustered at the census block level.

the Louisville border to shift away from unincorporated areas and move toward housing in independent municipalities. Table 6Impact on Home Salestable.caption.12 and Figure 5Impact on Sale Frequencyfigure.caption.14 investigate migration as one potential response to consolidation that results in the observed shift in demand. While I cannot measure migration directly, I observe the frequency of sales in each census block over time. If consolidation made some jurisdictions less desireable (e.g. unincorporated areas), we should except to observe at least a temporary increase in sales in those areas, as residents re-sort into their preferred jurisdiction.

Table 6Impact on Home Salestable.caption.12 shows results from estimating Equation 1Empirical Strategyequation.2.1 where the outcome y_{ijbt} is the number of home sales in census block i in year t. While the results are noisy across bandwidths, this specification considers consolidation's impact over the entirety of the post-treatment period, potentially masking significant heterogeneity over time. Estimating Equation 2Empirical Strategyequation.2.2 allows a better investigation of how the impact of consolidation played out over time. Figure 5Impact on Sale Frequencyfigure.caption.14 presents these results. While the estimations of the parameters of interest (β_t and ϕ_t) are often imprecise, there is a distinct upward shift in home sales in unincorporated areas after 2000 that is seen across the different buffer sizes (Appendix Figure 12Impact on Home Salesfigure.caption.31). For example, in 2001, the coefficient on unincorporated areas implies sales increased by about 38%, although this is not statistically significant. While the imprecision of these estimates in general suggests a cautious interpretation, the next section investigating the impact of consolidation on demographic outcomes also points to a shift in residential location patterns.

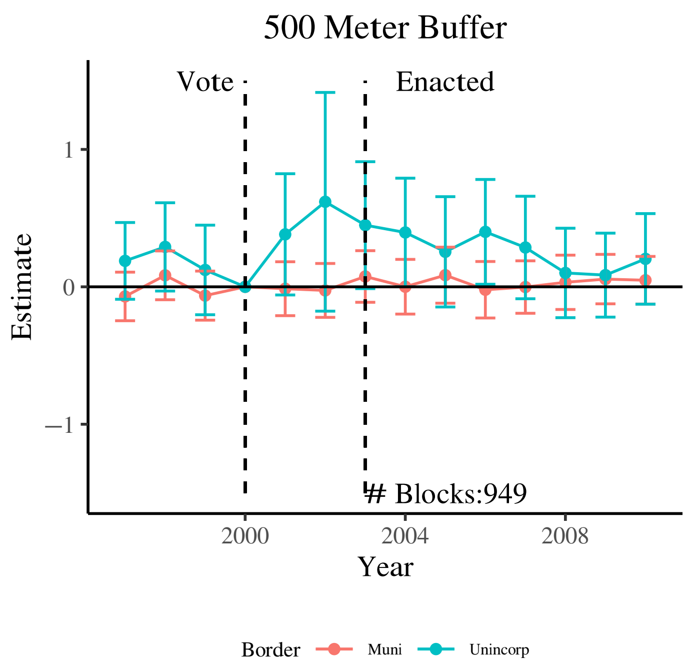
3.2 Population by Race

Table 7Impact on Population by Racetable.caption.15 presents results estimating Equation 1Empirical Strategyequation.2.1 where the outcomes y_{ijbt} are the number of African-Americans residents and the number of white residents.[35] The results show consolidation caused a significant increase in the African-American population in unincorporated areas, and this increase is almost one-for-one accounted for by a decrease in the white population. At the same time, the racial population composition went unchanged in independent municipalities, who stayed largely populated by whites.

Consolidation has important implications on the racial composition of unincorporated areas, with significant increases in the African-American population and corresponding decreases in the white population. The estimates imply the the total African-American pop-

[35] Results using the percent of population who is African-American as the outcome comes to the same conclusion.

Figure 5: Impact on Sale Frequency



Notes: The above figure presents from estimating Equation 2Empirical Strategyequation.2.2 where the outcome y is the number of home sales. The red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). The sample includes census blocks within a 500m buffer of the pre-consolidation Louisville border. Standard errors are clustered at the census block level.

Table 6: Impact on Home Sales

	Home Sale Frequency		
	250m	500m	750m
Muni x Post	0.0403	0.0360	-0.0013
	(0.0487)	(0.0428)	(0.0426)
Unincorp x Post	0.1773	0.1670	0.0620
	(0.1564)	(0.1186)	(0.1021)
Observations	8,792	13,286	17,150
R^2	0.549	0.534	0.527
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni sales	0.5791	0.6680	0.6997
Mean Unincorp sales	1.095	1.157	1.187

Notes: This table shows estimates of Equation 1, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the frequency of home sales. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

ulation in unincorporated census blocks increased by about 9 persons (using the 500 meter buffer).[36] This increase is matched by a similar decrease in the white population. Using the 500 meter buffer, the average number of white residents in census blocks in unincorporated areas decreased by about 8 persons.

Independent municipalities were largely white before Louisville's consolidation with Jefferson county, and it continued that way afterward. The estimates in Table 7Impact on Population by Racetable.caption.15 show that the impact on the African-American population in independent municipalities, β, vary significantly across the different buffer sizes. While the coefficient using the 250 meter buffer sample (-) implies consolidation reduced the African-American population in these jurisdictions, the coefficient decreases in magnitude and is indistinguishable from zero in the estimations using the 500 meter and 750 meter samples. On the other hand, the impact of consolidation on the number of white residents in independent municipalities is never statistically different from zero. Considering the average block in an independent municipality before consolidation was over 90 % white (both within 500 meters of the old Lousville border and countywide) this convincingly shows the racial

[36]Estimating a model where the outcome is the proportion of African-American residents per census block produces almost identical results.

composition of independent municipalities was unchanged by consolidation.

Table 7: Impact on Population by Race

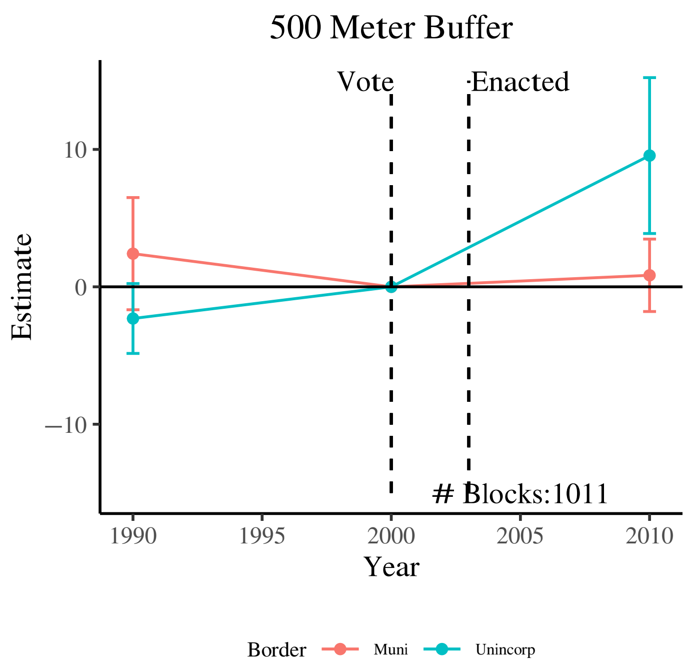
	African American			White		
	250m	500m	750m	250m	500m	750m
Muni x Post	-1.239	-0.3674	0.6418	3.633	1.730	1.688
	(0.6411)	(0.8907)	(1.056)	(1.204)	(1.238)	(1.256)
Unincorp x Post	9.136	9.317	8.405	-6.082	-8.346	-7.412
	(4.163)	(3.754)	(3.612)	(5.441)	(4.103)	(3.509)
Observations	1,990	3,025	3,962	1,990	3,025	3,962
R^2	0.878	0.923	0.941	0.931	0.955	0.963
Census Block FE	✓	✓	✓	✓	✓	✓
Year x Border FE	✓	✓	✓	✓	✓	✓
Mean Muni Pop	1.483	4.021	5.262	46.04	49.68	53.91
Mean Unincorp Pop	18.66	18.96	18.75	120.14	121.07	120.30

Notes: This table shows estimates of Equation 1, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcomes are the total number of African-American residents (the first three columns) and the total number of white residents (the last three columns) in a census block. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

3.3 Discussion

The results above show that consolidation had meaningful effects on the housing market and on the geographic dispersion of minorities in Louisville, but these results still beg the question of why. That is, why does housing demand rise in independent municipalities and fall in unincorporated areas, and why did the proportion of the population who were white in unincorporated areas decline? Was the shift in demand toward independent municipalities widespread, or were particular attributes more attractive? Recall, the provision of almost all public goods remained the same in the different jurisdictions of Jefferson county before and after consolidation, as has been discussed in detail. While there were two changes of note – the consolidation of the Louisville and Jefferson county police forces, and the ability to veto zoning proposals from the Metro planning commission – I provide evidence these factors likely did not play a role. Instead, I show that the increase in housing demand in independent municipalities is focused in small municipalities who had less zoning for multi-family housing,

Figure 6: Impact on Number of African-Americans



Notes: The above figure presents the results from estimating Equation 2Empirical Strategyequation.2.2 where the outcome y is the population of African-American residents. The red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). The sample includes census blocks within a 500m buffer of the pre-consolidation Louisville border. Standard errors are clustered at the census block level.

suggesting zoning played a role in the mechanism behind the headline results.

3.3.1 Police

Preferences over the size or type (central city vs. county vs. independent municipality) of one's police department may influence a resident to choose to live in one jurisdiction over another.[37] The fact that some municipalities within Jefferson county collected tax revenue in order to fund a local police department is a testament to this fact. At the same time, the remaining independent municipalities, as well as unincorporated areas, relied on the Jefferson county police force before consolidation. If the fact that these jurisdictions "lost" their police force to the new Metro government was responsible for the decline in housing demand in those jurisdictions, we should expect to see demand shift away (e.g. prices decline) in these jurisdictions and toward those with their own police forces. Utilizing the heterogeneity in the provision of a police force by independent municipalities, I can test whether preferences over police departments played a role in my findings.

To investigate whether the increase in the demand for housing was focused in independent municipalities with their own police force, I estimate Equation 3Policeequation.3.3, a variant of Equation 1Empirical Strategyequation.2.1 wherein I allow consolidation to have differential effects on those municipalities who provide their own police force Muni(Police)_j and those who do not Muni(No Police)_j.[38] My outcome of interest y_{ibjt} is the housing price index. Table 8Impact on House Pricestable.caption.18 presents the results from this estimation using samples of bandwidths of 250, 500, and 750 meters, as above.

$$y_{ibjt} = \alpha + \beta_1 \text{Muni(Police)}_j \times \text{Post}_t + \beta_1 \text{Muni(No Police)}_j \times \text{Post}_t + \phi \text{Unincorp}_j \times \text{Post}_t + \delta_i + \gamma_{bt} + \epsilon_{ibjt} \quad (3)$$

The evidence suggests the consolidation of the Louisville and Jefferson county police forces is not responsible for the impact of city-county consolidation on the housing market. Prices do not differentially rise between municipalities who provide their own police force and municipalities that received their police services from Metro after consolidation. Furthermore, prices rose more in independent municipalities that *do not* have their own police force. For example, using a 500 meter bandwidth, the results show prices rose by 4% in

[37] For example, some residents may prefer having an officer from the local area who they can count on to call, as opposed to a more anonymous officer from somewhere else in the county.

[38] Of the twenty four independent municipalities in the largest sample used in my analysis (those with land within 750 meters of the old Louisville border), eight of them had their own prioprietary police force. These were Audubon Park, Indian Hills, Lincolnshire, Lynnview, Saint Matthews, Shively, Strathmoor Village, and West Buechel.

municipalities who furnished their own police force, and 5% for municipalities without their own force. Thus, the shift in housing demand observed above does not appear to be driven by residents who prefer to have their own police department.

Table 8: Impact on House Prices

	House Price Index		
	250m	500m	750m
Muni (Police) x Post	1.048	0.6446	0.4560
	(0.3463)	(0.3237)	(0.2758)
Muni (No Police) x Post	1.198	0.9055	0.6823
	(0.4763)	(0.3580)	(0.3433)
Unincorp x Post	-1.215	-0.8382	-0.9561
	(0.2395)	(0.1956)	(0.1782)
Observations	3,230	5,230	6,934
R^2	0.841	0.852	0.860
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni (Police) Ind.	16.53	15.57	15.32
Mean Muni (No Police) Ind.	18.81	19.12	19.29
Mean Unincorp Index	13.70	13.37	13.15

Notes: This table shows estimates of Equation 3, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is a house-price index estimated at the census block-year level, as described in the Data Appendix. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

3.3.2 Zoning

Zoning is often referred to as one of the most powerful tools to preserve home values, as it can be harnessed to control how land is put to use in a neighborhood, and by extension who resides in a neighborhood (e.g. Jackson (1987), Fischel (2004)). For example, an owner of a single-family home likely does not want a meat factory as a next-door neighbor, as this would almost surely lead to a decline in the value of their home. Further, some homeowners may not want multi-family housing in their neighborhood, since it would likely result in increased road traffic and attract residents from a lower socioeconomic status - also known as "NIMBYs" (Not-In-My-Back-Yard). By zoning a neighborhood to only allow certain uses (e.g. residential) and certain structures (e.g. single-family detached housing), homeowners can effectively set a price floor for residing in a neighborhood, and by extension an income floor for its residents. Thus, how land use is attributed in a neighborhood and who has the power to change land use likely plays a significant role in a prospective buyer's decision to locate in a neighborhood.

Zoning Veto. Recall, before consolidation, the county-wide Planning Commission made recommendations for zoning changes, and the final decision to accept or reject that recommendation was in the hands of the jurisdiction of where the proposed rezoning would take place - with the caveat that most independent municipalities ceded this decision power to the county Fiscal Court by state law. After consolidation, the Metro government took on the decision-making power previously held by the Louisville city government and the Fiscal Court, while the few independent municipalities who held that power previously maintained that power.[39] Thus, the zoning decisions in previously-unincorporated areas and in most independent municipalities were now made by the new Metro Government. If a current or prospective resident believes that the Metro Government acts differently than the Fiscal Court in regards to zoning decisions, this may have induced the change residential choice I observe. To investigate the role of veto power, I run a similar exercise as in Section 3.3.1Policesubsubsection.3.3.1, using the fact that only select municipalities had the right to accept or reject zoning plans.[40] I estimate Equation 4Zoningequation.3.4, splitting the municipality treatment into municipalities that have the ability to veto the Planning Commission recommendations versus those that do not, instead relying on Metro Louisville to make that decision. Table 9Impact on House Pricestable.caption.20 displays these results.

$$y_{ibjt} = \alpha + \beta_1 \text{Muni(Veto)}_j \times \text{Post}_t + \beta_1 \text{Muni(No Veto)}_j \times \text{Post}_t + \phi \text{Unincorp}_j \times \text{Post}_t + \delta_i + \gamma_{bt} + \epsilon_{ibjt} \quad (4)$$

The power of municipalities to veto recommendations from the Planning Commision does not appear to be driving the observed changes in the housing market. The increase in home prices seen in independent municipalities after consolidation is larger in municipalities who do *not* have the ability to reject zoning recommendations of the joint Louisville/Jefferson county Planning Commission (5 % vs. 3% using the 500 meter bandwidth). This result may not be surprising given we saw in Section 3.1Housing Marketsubsection.3.1 that *trends* in zoning did not change as a result of consolidation.[41] However, this result does not preclude

[39] Four of the twenty four municipalities within the largest buffer of 750 meters have zoning power. These are Shively, Saint Matthews, Indian Hills, and St. Regis Park.

[40] Once more, this power was attributed according to a municipalities class designation - only municipalities of fourth class and above had this power.

[41] In particular, this could suggest that the Metro government made similar decisions as the county Fiscal Court, or that veto power was not used as an active tool to prevent changes in zoning, or that rezoning is in

Table 9: Impact on House Prices

	House Price Index		
	250m	500m	750m
Muni (Veto) x Post	0.6264	0.4923	0.1696
	(0.3879)	(0.3331)	(0.2920)
Muni (No Veto) x Post	1.304	0.9162	0.7967
	(0.3831)	(0.3192)	(0.2986)
Unincorp x Post	-1.203	-0.8345	-0.9454
	(0.2395)	(0.1954)	(0.1783)
Observations	3,230	5,230	6,934
R^2	0.841	0.852	0.860
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni (Veto) Ind.	16.32	15.61	15.41
Mean Muni (No Veto) Ind.	18.14	18.04	18.08
Mean Unincorp Index	13.70	13.37	13.15

Notes: This table shows estimates of Equation 4, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is a house-price index estimated at the census block-year level, as described in the Data Appendix. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

the *levels* of zoning regulation as a potential explanation for why housing demand shifted to municipalities and away from unincorporated areas.

Zoning Levels. While most municipalities did not have the ability to veto the Planning Commission's zoning recommendations directly, they still experienced few changes in zoning, if any, in their neighborhoods throughout the sample period.[42] If residents wanted to live in a neighborhood with certain characteristics (e.g. no mulit-family housing) and they were cognizant that zoning changes were a slow-moving process, then the level of zoning may help explain why they chose to live where they did. To explore this idea, I look at whether the effect of consolidation on house prices in independent municipalities differs by the level of zoning of multi-family housing within the independent municipality. I estimate Equation 5Zoningequation.3.5, where I split up the independent municipality treatment indicator in Equation 1Empirical Strategyequation.2.1 into separate indicators for whether the municipality contained multi-family housing (measured as 1% or more of the total number of parcels within the municipality) or not. The results are presented in Table 10Impact on House Pricestable.caption.22.

$$y_{ibjt} = \alpha + \beta_1 \text{Muni(MFH)}_j \times \text{Post}_t + \beta_1 \text{Muni(No MFH)}_j \times \text{Post}_t + \phi \text{Unincorp}_j \times \text{Post}_t + \delta_i + \gamma_{bt} + \epsilon_{ibjt} \quad (5)$$

The results show that consolidation's impact on housing prices in independent municipalities is concentrated in areas with effectively no land zoned for multi-family housing. Using the 500 meter bandwidth, house prices rose in municipalities zoned with multi-family housing by 2% (not statistically significant), whereas they rise by 6% (statistically significant) in municipalities without multi-family housing. This suggests that avoiding multi-family housing was potentially a driving force behind why residents shifted their residential preferences toward independent municipalities. However, this evidence is suggestive, as there are likely other factors at play besides zoning (e.g. neighborhood quality, tight-knit communities) which contributed to the observed shift in demand.

4 Conclusion

City-county consolidation has long been proposed as a form of metropolitan governance to solve issues of suburban sprawl, land use, and racial residential segregation. However, there is little empirical evidence showing the effects of consolidation on local outcomes. Using the

general a slow and arduous process.

[42] To be clear, this was not unique to just independent municipalities.

Table 10: Impact on House Prices

	House Price Index		
	250m	500m	750m
Muni (MFH) x Post	0.2524	0.2610	0.2092
	(0.3812)	(0.2979)	(0.2666)
Muni (No MFH) x Post	1.724	1.182	0.9035
	(0.4319)	(0.3513)	(0.3283)
Unincorp x Post	-1.097	-0.7891	-0.9226
	(0.2428)	(0.1959)	(0.1788)
Observations	3,230	5,230	6,934
R^2	0.842	0.852	0.860
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni (MFH) Ind.	16.60	15.79	15.50
Mean Muni (No MFH) Ind.	18.41	18.45	18.60
Mean Unincorp Index	13.70	13.37	13.15

Notes: This table shows estimates of Equation 5, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is a house-price index estimated at the census block-year level, as described in the Data Appendix. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000, inclusive.

consolidation of Louisville with Jefferson County in 2003 as a case study with a geographic border difference-in-differences empirical framework, I find that consolidation had important effects on the housing market and population sorting in Jefferson county. In particular, consolidation caused housing prices in independent municipalities (unincorporated areas) to rise (fall) by about 5% (6%). Finally, consolidation caused a shift in the distribution of the racial population of Jefferson County. The white population fell in unincorporated areas, resulting in the share of the African-American population in these areas to rise by 3-7%.

Taken together, the results seem to suggest the consolidation made the previously unincorporated areas less desireable for a segment of the white population, resulting in a decline in their population in the years after consolidation. I show these housing market results are not driven by the consolidation of the city and county police forces or by the zoning decision-making power within the county. Instead, the evidence seems to point to city size as an important factor - increases in house prices are largely focused in relatively smaller independent municipalities. These smaller municipalities tended to have little, if any, areas zoned for multi-family housing and few, if any, black residents, suggesting demand in these municipalities may have been driven by preferences for land use or racial homogeneity. As more metropolitan areas around the country consider city-county consolidation, policymakers need to be cognizant of the fact that consolidation will likely lead to a shift in the composition of the population and to important changes in the housing market.

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Appendices

A Fragmentation and Consolidation

In the second half of the twentieth century, as in many central cities across the U.S., Jefferson county experienced a significant shift in the distribution of both population and business within the county. From 1950 to 1990, Louisville's population declined by one hundred thousand residents, from 369,000 to just 269,000 residents, even though the population of the county as a whole experienced a net increase of 180,000 residents. The white population in Louisville actually fell even further, by about 120,000, shifting the percent of Louisville's population that was African-American from 16% to 30%.

Large industries and other businesses followed residents to the suburbs. For example, in 1951, General Electric was one of the first large manufacturers to locate outside of Louisville, announcing it would create its entire home-appliance manufacturing in "Appliance Park", an unincorporated area southeast of Louisville. Ford Motor Company moved their operations in Jefferson county from downtown Louisville to just outside its borders into unincorporated territory in 1955, and then built another complex in 1969 on the very edge of Jefferson county, far away from Louisville's border. Large enclosed shopping malls such as Mall Street Matthews in 1963 and Oxmoor in 1971 shifted retail businesses outward as well, emptying a once prosperous downtown shopping district. These population and commercial movements meant Louisville's tax base deteriorated, with its real debt per capita burden rising threefold over from 1950 to the 1990s. Starting in the early 1980s, Louisville tried to counter this loss of fiscal stability by proposing to consolidate with Jefferson county.

In the 1980s, Louisville attempted multiple referenda on city-county consolidation to recapture the population and tax base it lost during the previous three decades. The first referendum in 1982 only lost by 1,450 votes, spurring city leaders to try again in 1983. However, this new effort was defeated by 5,600 votes, implying the momentum seemed to have shifted toward anti-consolidation forces. Realizing they had few other options, the Louisville city government threaten to simply annex the entirety of unincorporated Jefferson County in 1985.[43] After the county vehemently opposed this idea, the county and city came together and reached a revenue-sharing agreement. Before this agreement, the city and county each had their own occupational taxes set at the legal maximum of 1.25%, that was paid by every business in their respective jurisdiction. Since Louisville was losing

[43] At the time, Kentucky only required a vote for annexation to occur if the territory being annexed was a part of a municipality. Soon after this incident, the Kentucky State Legislature passed a statute requiring a vote for annexation of unincorporated areas as well.

population and business to the county, that effectively reduced Louisville's ability to raise revenues. The revenue-sharing agreement pooled these two sources of revenue together and then distributed amounts to Louisville and Jefferson county according to a ratio determined by the revenue distribution in 1985 - Louisville received 58%, while Jefferson county received 42%. This arrangement helped remove competitive pressures to attract business to their respective jurisdictions at the expense of the other. The other major arm of the compact was a compromise to keep all jurisdictions within the county at their contemporary borders. New incorporations were forbidden throughout Jefferson county, and at the same time Louisville could not annex unincorporated land without a vote in the area to be annexed. By the end of the 12-year compact, though, both government and business leaders still saw room for further coordination and pursued city-county consolidation.

In the late 1990s, leaders of the business and civic communities of Louisville sought to create a new city-county consolidated government because they believed it would attract new businesses, enhance economic development, and lead to a more effective government (Wachter, 2013). The plan put forth to the public shared many common elements with the city-county consolidations of the mid-20th century. Primarily, consolidation meant the combination of the city and county governments into a new Metropolitan government. At the time, the county had three county commissioners and an executive at its head, their jurisdiction encompassing the entirety of the county,while the city was run by a mayor with a twelve-member Board of Aldermen, and its jurisdiction was solely Louisville. The consolidation plan created one Metro Council government, with a mayor and 26 council members each representing a geographic area of the county of about twenty five thousand residents, with all residents in the county voting on a representative. Independent municipalities were permitted to remain independent, meaning they would have the same relationship with the Metro government as they previous did with their county. However, the new referendum proposed to the county in 2000 deviated from Louisville's previous attempts to consolidate in one crucial element.

One distinct component of Louisville's consolidation plan is that it would only impact the governance of the county, and any future decision regarding policy was left to the future government. For example, taxes and services would be unchanged unless a change was voted on by the new Metro Council. This was embodied in the creation of an Urban Services District (USD), which mimicked the borders of Louisville before consolidation. In the USD, taxes were levied at the same rate as under the old city of Louisville to finance the same services (e.g. garbage/recycling pickup and fire protection) that they enjoyed prior to consolidation. The previously unincorporated areas that would become a part of Louisville Metro either contracted with the Metro government to receive those services, provided their own services, or paid private companies directly - again, mimicking the status quo before consolidation. If any changes to the borders of the USD were desired, the Metro Council would have to vote to approve such measures. In sum, the actual vote for Louisville-Jefferson county consolidation would not have an immediate impact on the provision of services to the residents of Jefferson county. Even though the crafter of the consolidation bill made this decision with the express objective of making it attractive to a wider population (by avoiding antagonizing any special interests), the vote on the merger was by no means a foregone conclusion.

A poll released on 28 September, 2000 indicated opponents against merger had a slight advantage, 46 to 41, with 13% undecided: while residents of Louisville and independent municipalities were evently split for-and-against in the poll, residents of unincorporated areas were against the measure 54-36 (McDonough, 2000*b*). 44 of the 85 mayors of independent municipalities in Jefferson county came out against merger (Shafer, 2000*e*). The worries of many of these mayors is embodied in this quote: "The possible negative impact upon our cities' services, powers, and ability to control and direct our own destiny could be greatly diminished," (Shafer, 2000*a*). However, consolidation supporters had superior financial backing, outraising the opposition 15 to 1, and the benefit of popular spokespersons from across the political spectrum, including three-time Democratic mayor of Louisville Jerry Abramson and Republican U.S. Senator Mitch McConnell (Shafer, 2000*d*). In a follow-up poll released on 29 October 2000, the margin in unincorporated areas against consolidation was unchanged from a month previous at -18, while pro-merger opinion grew to a 10-point lead in Louisville and independent municipalities (McDonough, 2000*c*). Consolidation by approved by voters on 7 November, 2000 by a margin of 54-46.[44]

B Data Appendix

B.1 House Price Data and Index Construction

I use a house price index to measure the impact of consolidation on single family house prices in Jefferson county. The following section describes the steps taken to clean the sales price data and estimate the price index used in the results presented throughout the paper.

Data Cleaning. The Jefferson county property value assessment database (PVA) contains important information that allows me to construct an accurate house price index for single family homes, including the buyer and seller names. Indeed, a crucial element of constructing a house price index is to ensure the transactions are "arms-length" - that is, a

[44]Voter turnout for merger question was 67% vs 69% for the election as a whole, showing widespread interest in the issue among voters (Shafer, 2000*a*).

sale between two parties that do not have a relationship such that the sale price differs from the true market price. This is most problematic for transactions within families, whether it be parents to children, siblings, or other relatives. To mitigate this issue, I remove all transactions in which the buyer and seller share the same last name. Another common practice is to set a price floor on transactions, implicitly assuming any parcel of land has an intrinsic value that does not likely fall below a certain value. Indeed, approximately 1.4% of transactions in the baseline database have a transaction value of $0. In addition to removing these transactions, I remove any transaction of less than $5,000 from my sample. Deleting these "undervalued" sales removes about 1.7% of the total number transactions in my sample period.[45]

Another concern in constructing house price indices is to create a quality-consistent measure that does not confound increases in sales price with home improvements. In addition to sales information, the PVA database also contains data on assessment valuations and the reason for each assessment valuation (e.g. Sale, No Change, Computer Reassessment, Improvements, Additions, Change in Class/Use, Foreclosure, Exoneration).[46] I remove any parcel that has an assessment indicating any additions or improvements, as the data does not allow me to see the attributes of the property before said change.

Finally, in order to better mitigate both issues of identifying arms-length transactions and avoiding issues with changes in quality, I remove parcels which experience extreme swings in prices. I identify an extreme swing as a change of 50% in three years, 100% in five years, or 200% in ten years. In looking at the data, many of these swings can be explained by non-arms-length transactions. For example, a previous owner who was a single woman "sold" the house to her (newly-named) self and spouse; or, the property was passed onto a trust in the name of the previous owner.

Index Construction. To construct a quality-consistent house price index for each census block and year, I leverage a rich set of covariates available from the Jefferson county PVA database to estimate a hedonic-pricing model, largely following Baum-Snow and Han (2020) (among others). There are three common measures of home prices used in the literature: hedonic pricing, repeat-sales, and median value. While median value is often seen as the most problematic, hedonic pricing and repeat-sales each have strengths and weaknesses (see Hill (2011) for further exposition). Building a repeat-sales indexes inherently limits the sample to properties where I observe multiple sales in my sample period. Since my unit of analysis is geographically small (census block), constructing a repeat-sales index is not feasible as it would rely solely on homes which are sold multiple times throughout my sample

[45] Results are not sensitive to this cutoff.

[46] Each parcel of land in Jefferson County is supposed to be assessed every other year to every four years.

period. . Thus, I estimate the house-price index using a hedonic-pricing model, where I can leverage each sale in my data set, controlling for differences in quality with the detailed characteristics. The estimating equation is as follows:

$$\ln P_{pit} = \alpha_{it} + \beta X_p + e_{pit}$$

$\ln P_{ibt}$ is the natural log of the sale price of property p in census block i in year t. X_{pit} contains covariates at the parcel level, including the number of stories of the structure sold, the age and floor area of the structure, and the number of bathrooms. The hedonic price index is constructed using the census block-year fixed effects, α_{it}.

B.2 NHGIS Census Block Crosswalk

The NHGIS provides GIS crosswalks that maps the 1990 and 2000 census block geographies to 2010 census block geographies.[47] With the crosswalk, each 1990 and 2000 census block is assigned to one or more 2010 census blocks with a corresponding "interpolation" weight (of less than one). The weight gives the expected proportion of the 1990 or 2000 block's population and housing units located in the 2010 census block(s).

For the majority of census blocks, this crosswalk performs well. In many cases, there are multiple 2010 census block geographies that are a subset of a 1990 or 2000 census block geography, and the NHGIS crosswalk ultimately must assume how to distribute the 1990 or 2000 census block outcomes into the smaller 2010 census blocks. When this exercise is done in an area of uniform density such as a swath of single family housing, it provides reasonable outcomes for 1990 or 2000 outcomes in the 2010 geographies. For example, in Figure 7Successful NHGIS Census Block Crosswalkfigure.caption.25, it is clear the 1990 census block geography shown encompasses two city blocks, whereas the two 2010 geographies cover one city block each. In the NHGIS crosswalk, they correctly calculate the geographic area of overlap between the 1990 to the two 2010 census blocks at precisely 0.5, and assign interpolation weights of 0.44 and 0.56 (left to right). As shown, there are a total of 26 house addresses in the two 2010 blocks, with 12 (0.46) house addresses in the left and 14 (0.54) in the right. Thus, their interpolation weights are impressively close to the "real" weights that should be given.

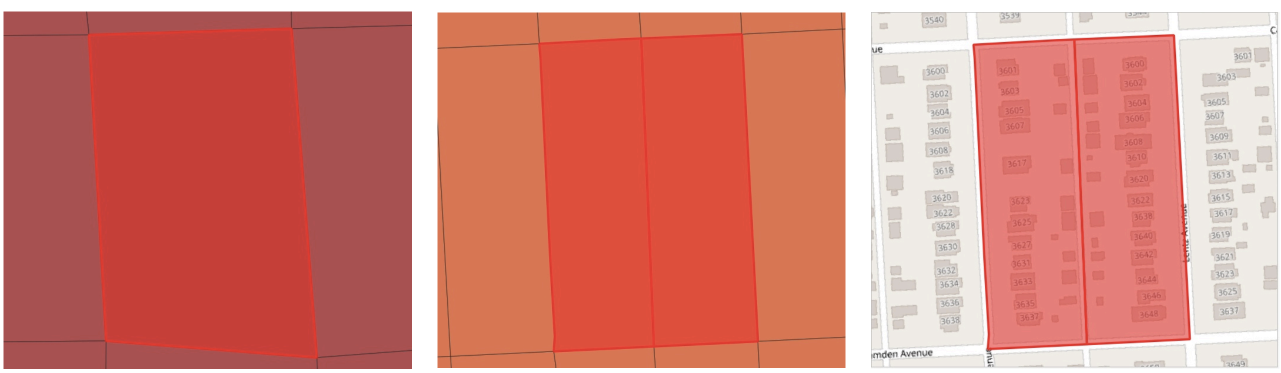
However, when the 2010 census block geography is more haphazard relative to the 1990 or 2000 census block, or the area is not uniformly populated or contains multi-family housing, the crosswalk can perform poorly. For example, in Figure 8Unuccessful NHGIS Census Block

[47] See https://www.nhgis.org/user-resources/geographic-crosswalks for files and more detailed documentation.

Crosswalkfigure.caption.26, two 2010 census blocks make up a part of one 2000 census block. In this case, while the crosswalk recognizes the larger 2010 block should receive more of the 1990 block's weight relative to the smaller 2010 block (0.86 vs. 0.09), the actual weights should be about 0.75 and 0.2.[48] To remedy this issue, I re-aggregate problematic 2010 block geographies back to their 2000 block geography.

To find these poorly matched blocks, I run a number of manual exercises. The first is to simply use the crosswalks as given, apply the weights to construct a panel of outcomes of 2010 census blocks from 1990 to 2000 to 2010, and plot this data. Potentially problematic blocks are easily spotted using this method. Then I use a few different tools to further investigate whether there is an actual mismatch. First, I can use the Jefferson County PVA website to verify the age of the structures within the census block, e.g. a spike in housing units is likely valid if a structure or structures was built in the interim. I can also look at outcomes in neighboring blocks to see if they "mirror" each other; if one block changes drastically one way and a neighboring block changes by a similar magnitude in the other direction, there is likely a mismatch occurring in the crosswalk. The other method I use to detect mismatches is simply a visual check - using QGIS and mapping the shapefiles for each year on top of one another, it is very clear where the block geographies change, and whether the change is potentially problematic (e.g. Figure 8Unuccessful NHGIS Census Block Crosswalkfigure.caption.26 versus Figure 7Successful NHGIS Census Block Crosswalkfigure.caption.25).[49] Once I find these, I can use the aforementioned tools to verify whether there may be a problematic crosswalk.

Figure 7: Successful NHGIS Census Block Crosswalk

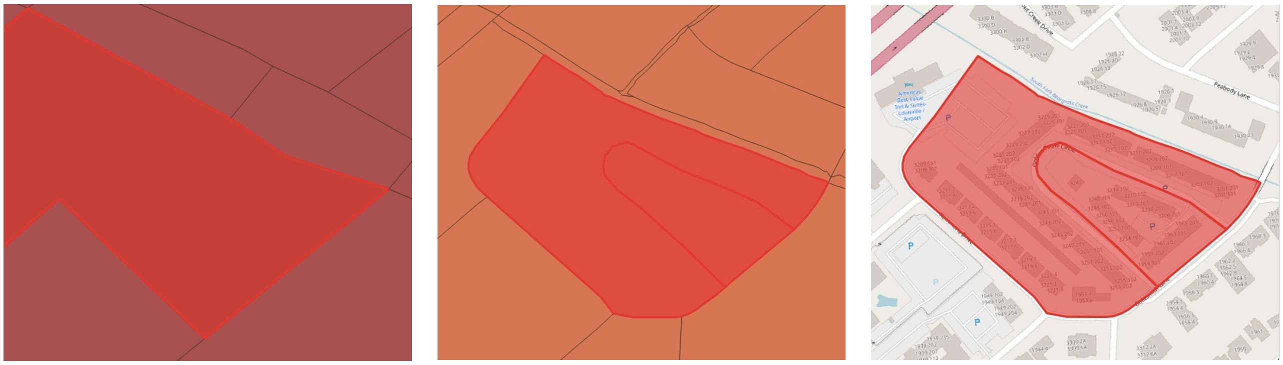


(a) 1990 Census Block (b) 2010 Census Block (c) Satellite Map

[48]I verify this by checking whether the structure(s) in the block existed in 1990 in the Jefferson County PVA. This implies the number of housing units should stay almost constant over time. The number of housing units in these blocks in 2010 are 153 and 32 (the population in 2010 holds almost precisely the same ratio).

[49]In general, the pattern seems to be that census blocks in Jefferson county stay relatively consistent from 1990 to 2000, and then become finer in 2010.

Figure 8: Unuccessful NHGIS Census Block Crosswalk



(a) 1990 Census Block (b) 2010 Census Block (c) Satellite Map

C Descriptive Figures

Figure 9: Property Taxes in Jefferson county

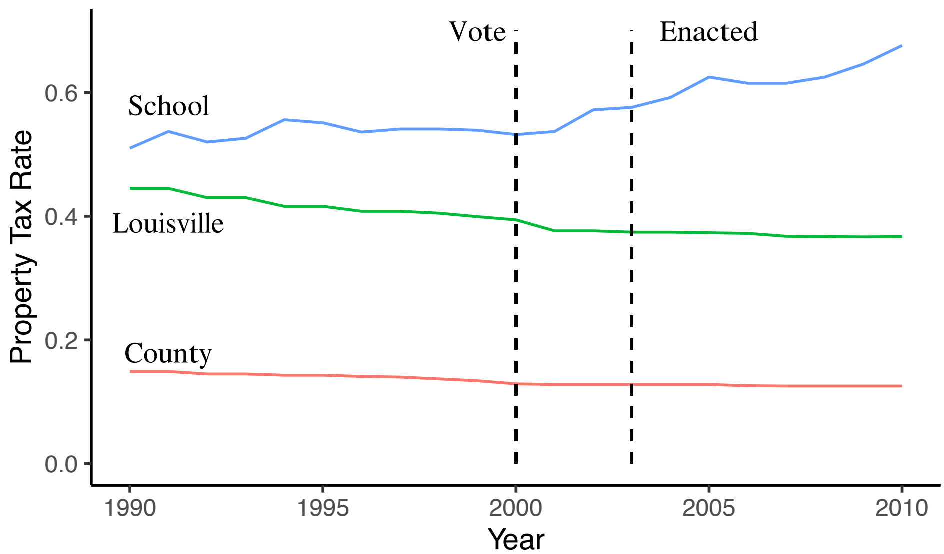
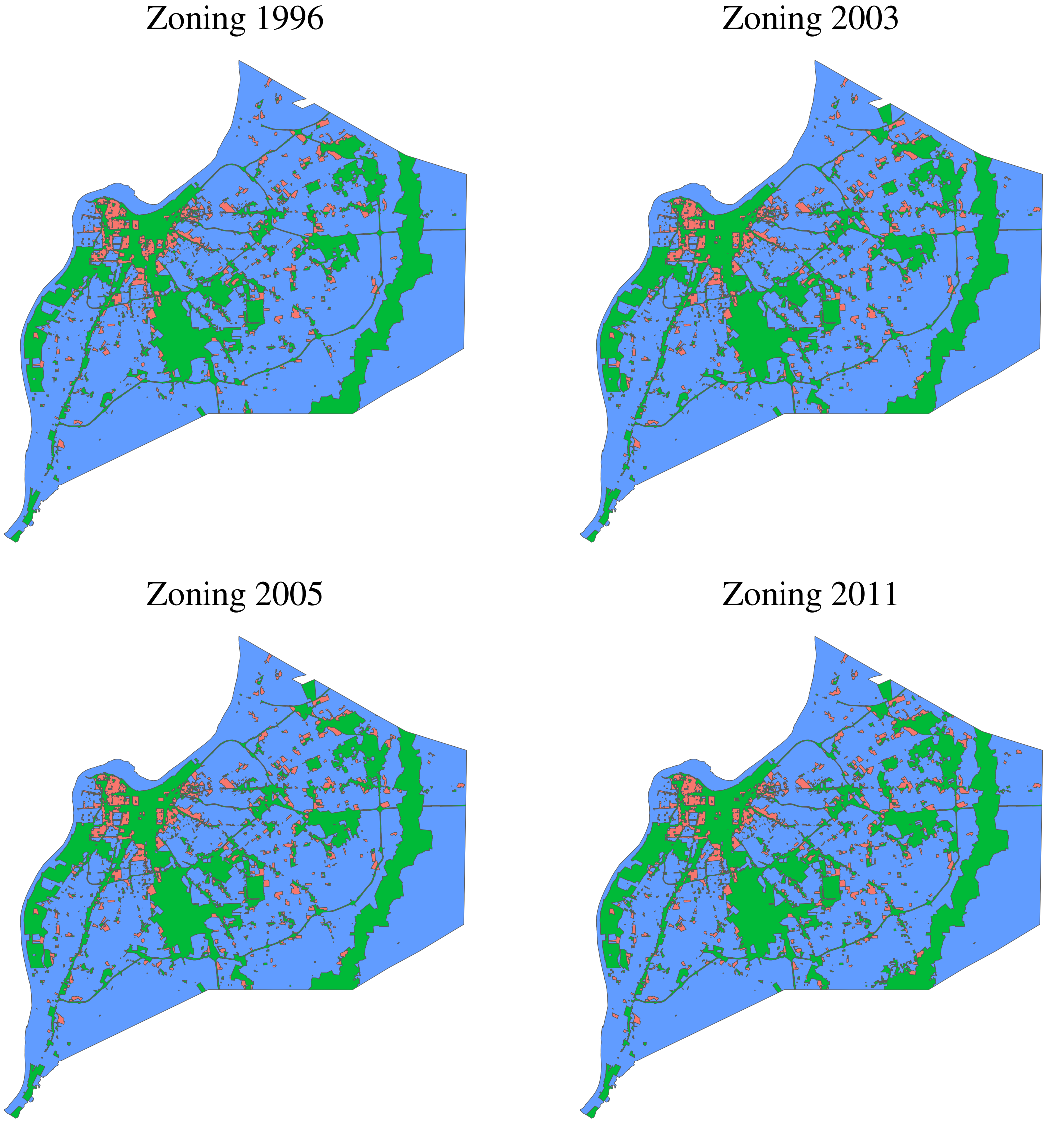


Figure 10: Zoning Maps of Jefferson County



Notes:This figure plots the zoning maps for Jeffreson county for 1996, 2003, 2005, and 2011, where the blue areas are zoned for single family housing, red areas are zoned for multi-family housing, and green areas are zoned for any other use.

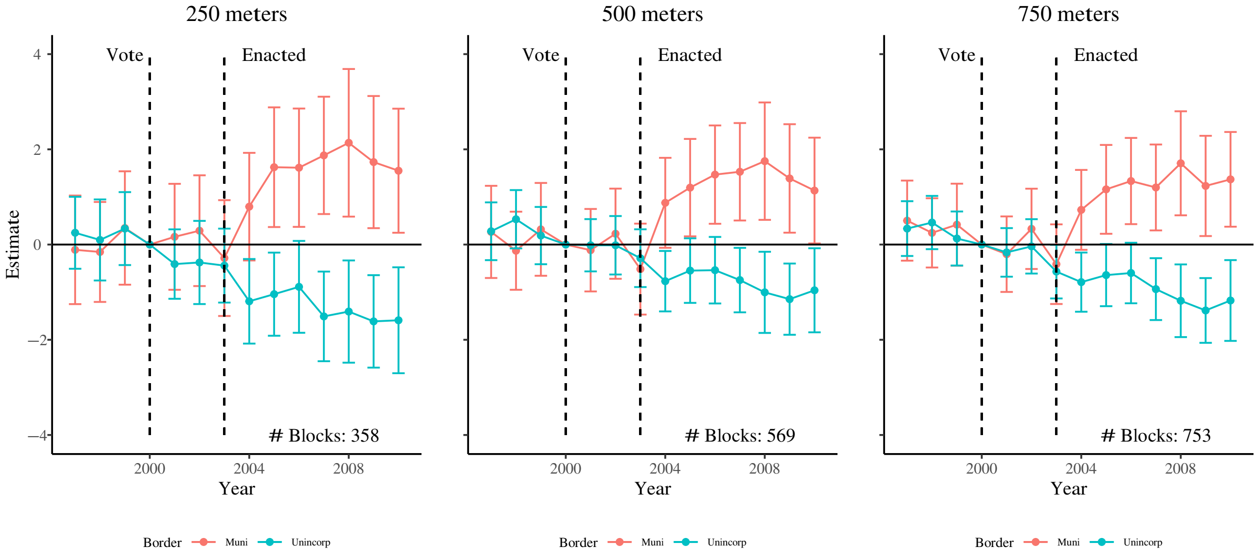
D Descriptive Tables

Table 11: Louisville MSA vs. Median Top 100 MSA

Variable	Louisville	Median
Median Income P/C	21756	21234
% Poverty	10.90	10.95
% > 18 yrs. old	75.20	74.40
% Bachelors	22.20	24.70
% Manufact Emp.	15.70	12.40
% Urban	87.30	87.85
Mean HH Size	2.44	2.52
% Black	13.90	9.25
% Homeowners	68.60	67.30
% New Housing	21.20	26.50

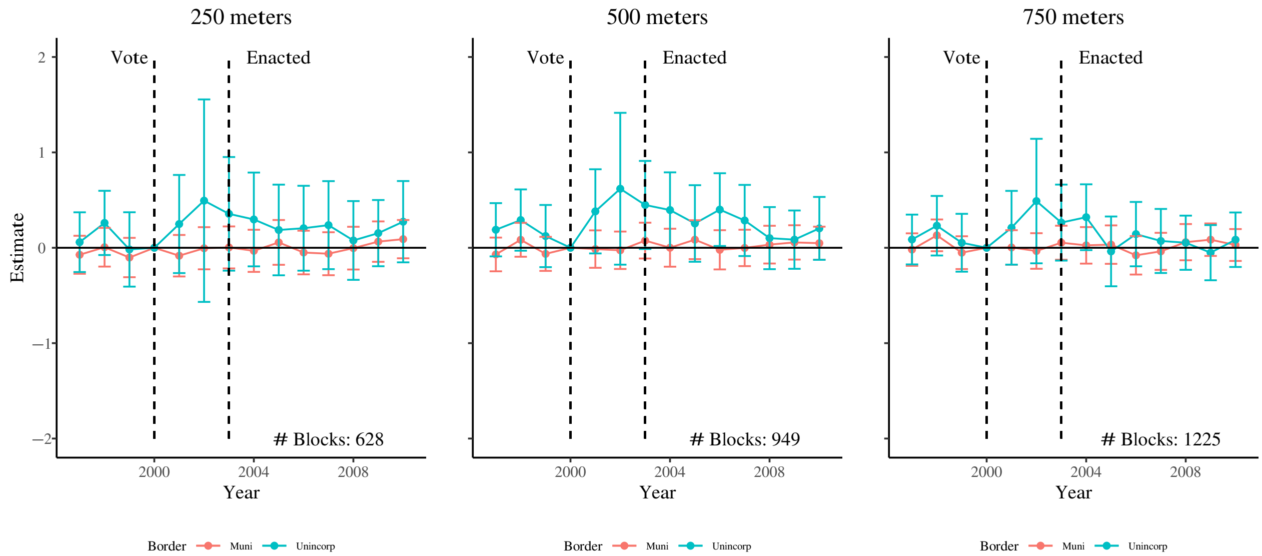
E Main Results: Figures

Figure 11: Impact on House Prices



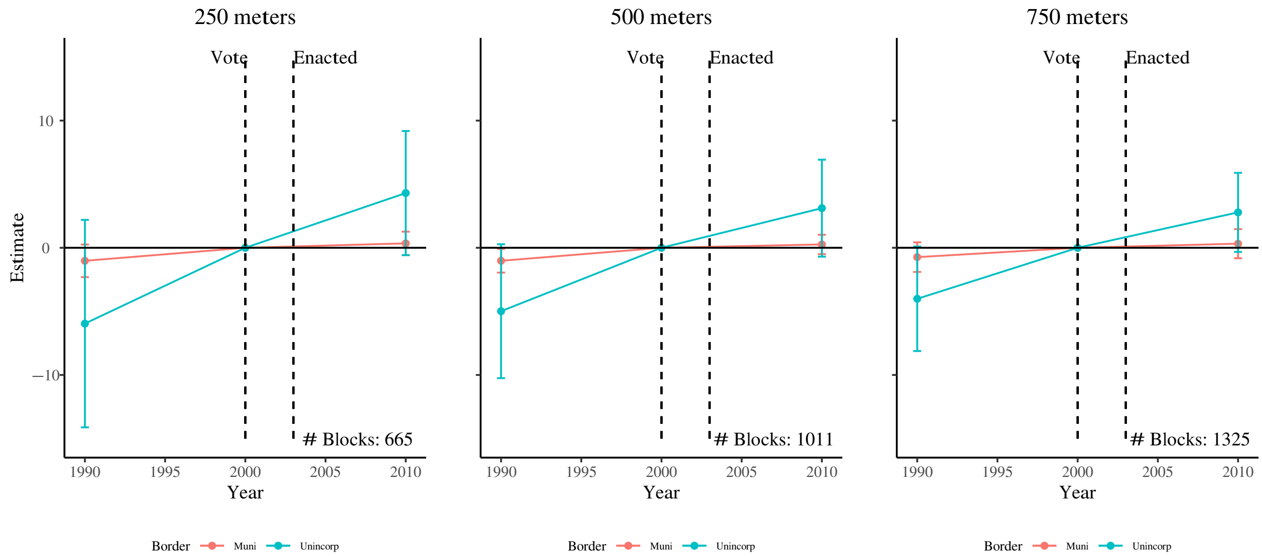
Notes: The above figures present results from estimating Equation 2Empirical Strategyequation.2.2 at three different bandwidths (250, 500, and 750 meters) where the outcome y is the house price index. In each figure, the red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). Standard errors are clustered at the census block level.

Figure 12: Impact on Home Sales



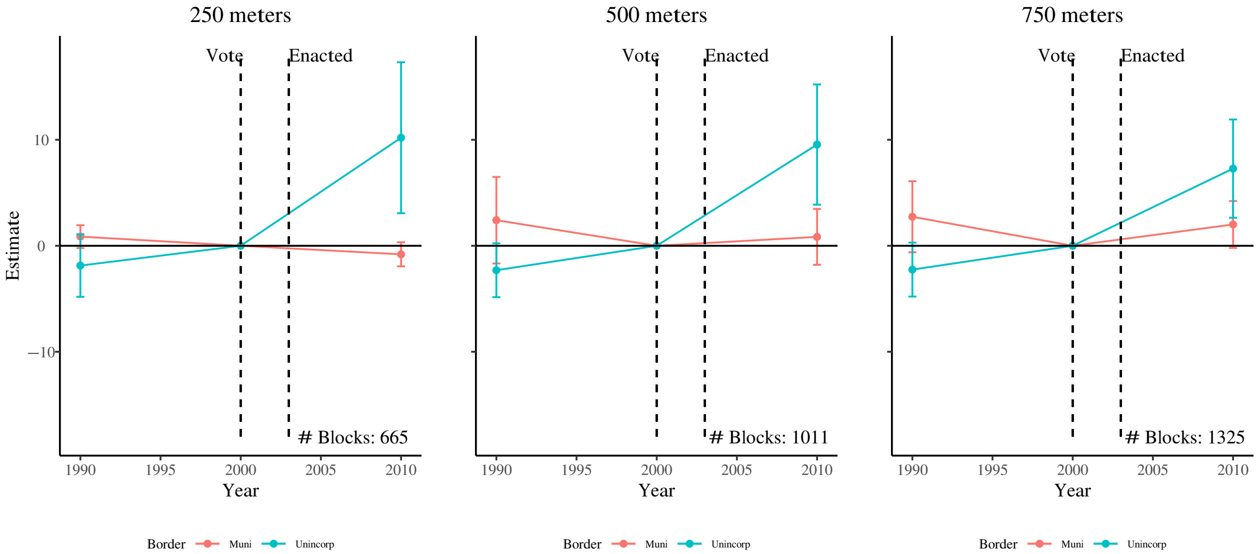
Notes: The above figures present results from estimating Equation 2Empirical Strategyequation.2.2 at three different bandwidths (250, 500, and 750 meters) where the outcome y is the number of home sales. In each figure, the red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). Standard errors are clustered at the census block level.

Figure 13: Impact on Number of Housing Units



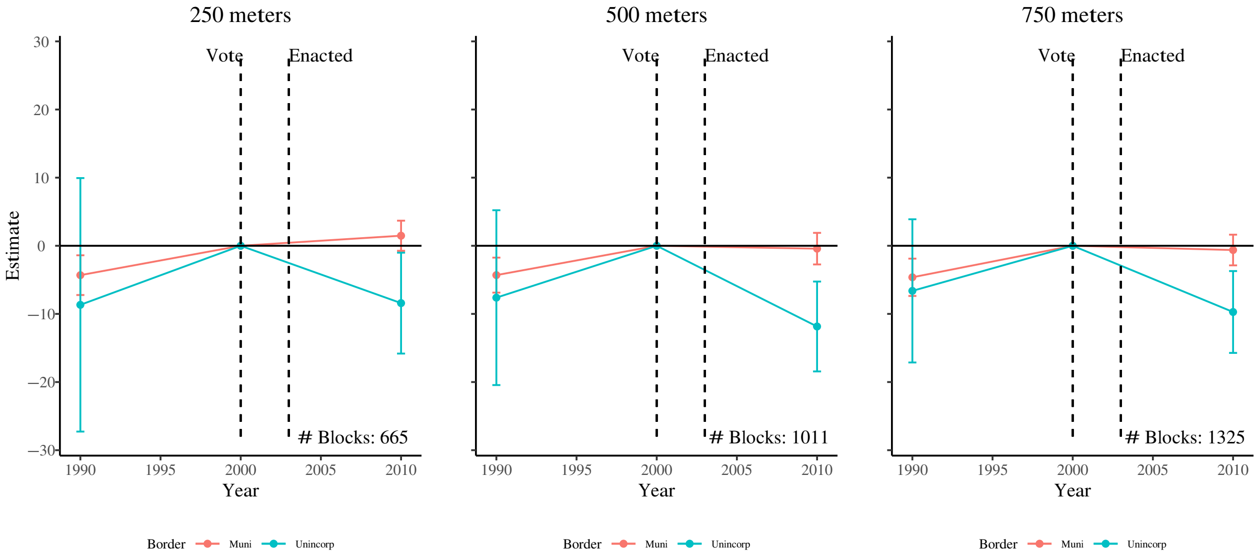
Notes: The above figures present results from estimating Equation 2Empirical Strategyequation.2.2 at three different bandwidths (250, 500, and 750 meters) where the outcome y is the number of housing units. In each figure, the red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). Standard errors are clustered at the census block level.

Figure 14: Impact on Number of African-Americans Residents



Notes: The above figures present results from estimating Equation 2Empirical Strategyequation.2.2 at three different bandwidths (250, 500, and 750 meters) where the outcome y is the number of African-American residents. In each figure, the red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). Standard errors are clustered at the census block level.

Figure 15: Impact on Number of White Residents



Notes: The above figures present results from estimating Equation 2Empirical Strategyequation.2.2 at three different bandwidths (250, 500, and 750 meters) where the outcome y is the number of white residents. In each figure, the red series depicts the impact of consolidation on y in independent municipalities (the point estimates and 95 % confidence intervals of β). The impact of consolidation on y in unincorporated areas is depicted in blue (the point estimates and 95 % confidence intervals of ϕ). Standard errors are clustered at the census block level.

F Main Results: Tables

Table 12: Impact on House Prices, Flexible Specifications

	House Price Index		
	250m	500m	750m
Muni × 1997	-0.1108	0.2657	0.5027
	(0.5818)	(0.4939)	(0.4291)
Muni × 1998	-0.1553	-0.1296	0.2455
	(0.5356)	(0.4184)	(0.3711)
Muni × 1999	0.3480	0.3192	0.4177
	(0.6075)	(0.4973)	(0.4393)
Muni × 2001	0.1628	-0.1184	-0.2021
	(0.5679)	(0.4419)	(0.4046)
Muni × 2002	0.2914	0.2280	0.3301
	(0.5934)	(0.4831)	(0.4303)
Muni × 2003	-0.2838	-0.5156	-0.4122
	(0.6208)	(0.4872)	(0.4268)
Muni × 2004	0.7960	0.8769	0.7284
	(0.5767)	(0.4821)	(0.4281)
Muni × 2005	1.625	1.196	1.159
	(0.6413)	(0.5220)	(0.4760)
Muni × 2006	1.614	1.469	1.334
	(0.6341)	(0.5267)	(0.4619)
Muni × 2007	1.873	1.529	1.199
	(0.6288)	(0.5222)	(0.4604)
Muni × 2008	2.137	1.752	1.707
	(0.7910)	(0.6291)	(0.5575)
Muni × 2009	1.732	1.389	1.232
	(0.7081)	(0.5812)	(0.5372)
Muni × 2010	1.551	1.134	1.370
	(0.6648)	(0.5668)	(0.5075)
Unincorp × 1997	0.2477	0.2794	0.3355
	(0.3855)	(0.3095)	(0.2937)
Unincorp × 1998	0.0965	0.5316	0.4626
	(0.4343)	(0.3126)	(0.2853)
Unincorp × 1999	0.3355	0.1883	0.1270
	(0.3908)	(0.3069)	(0.2895)
Unincorp × 2001	-0.4099	-0.0147	-0.1646
	(0.3719)	(0.2801)	(0.2603)
Unincorp × 2002	-0.3763	-0.0150	-0.0381
	(0.4457)	(0.3132)	(0.2913)
Unincorp × 2003	-0.4419	-0.2862	-0.5681
	(0.3962)	(0.3093)	(0.2877)
Unincorp × 2004	-1.192	-0.7694	-0.7892
	(0.4531)	(0.3239)	(0.3171)
Unincorp × 2005	-1.042	-0.5471	-0.6416
	(0.4454)	(0.3459)	(0.3331)
Unincorp × 2006	-0.8882	-0.5386	-0.5978
	(0.4922)	(0.3565)	(0.3246)
Unincorp × 2007	-1.509	-0.7463	-0.9359
	(0.4804)	(0.3456)	(0.3314)
Unincorp × 2008	-1.407	-1.004	-1.181
	(0.5477)	(0.4349)	(0.3894)
Unincorp × 2009	-1.614	-1.147	-1.384
	(0.4952)	(0.3816)	(0.3470)
Unincorp × 2010	-1.590	-0.9607	-1.175
	(0.5675)	(0.4505)	(0.4329)
Observations	3,230	5,230	6,934
R^2	0.845	0.854	0.861
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni Index	17.41	16.89	16.67
Mean Unincorp Index	13.70	13.37	13.15

Notes: This table shows estimates of Equation 2, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is a house-price index, which is estimated at the census block-year level as described in the Data Appendix. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000m inclusive.

Table 13: Impact on Home Sales, Flexible Specifications

	Home Sales		
	250m	500m	750m
Muni × 1997	-0.0733	-0.0699	-0.0189
	(0.1016)	(0.0902)	(0.0868)
Muni × 1998	0.0058	0.0836	0.1304
	(0.1035)	(0.0906)	(0.0849)
Muni × 1999	-0.1023	-0.0639	-0.0513
	(0.1053)	(0.0911)	(0.0878)
Muni × 2001	-0.0833	-0.0137	0.0032
	(0.1110)	(0.1001)	(0.0924)
Muni × 2002	-0.0050	-0.0262	-0.0336
	(0.1127)	(0.1001)	(0.0952)
Muni × 2003	0.0037	0.0753	0.0533
	(0.1122)	(0.0958)	(0.0908)
Muni × 2004	-0.0318	0.0007	0.0253
	(0.1127)	(0.1015)	(0.0977)
Muni × 2005	0.0563	0.0848	0.0333
	(0.1197)	(0.1040)	(0.1028)
Muni × 2006	-0.0497	-0.0213	-0.0790
	(0.1168)	(0.1049)	(0.1028)
Muni × 2007	-0.0624	-0.0017	-0.0374
	(0.1149)	(0.0973)	(0.0991)
Muni × 2008	-0.0039	0.0329	0.0588
	(0.1143)	(0.1009)	(0.0969)
Muni × 2009	0.0647	0.0562	0.0849
	(0.1081)	(0.0918)	(0.0869)
Muni × 2010	0.0904	0.0480	0.0291
	(0.1024)	(0.0884)	(0.0852)
Unincorp × 1997	0.0587	0.1888	0.0867
	(0.1597)	(0.1425)	(0.1332)
Unincorp × 1998	0.2606	0.2907	0.2314
	(0.1717)	(0.1638)	(0.1593)
Unincorp × 1999	-0.0175	0.1229	0.0527
	(0.1988)	(0.1663)	(0.1548)
Unincorp × 2001	0.2490	0.3819	0.2099
	(0.2624)	(0.2251)	(0.1972)
Unincorp × 2002	0.4939	0.6189	0.4897
	(0.5415)	(0.4060)	(0.3330)
Unincorp × 2003	0.3557	0.4488	0.2641
	(0.3039)	(0.2356)	(0.2033)
Unincorp × 2004	0.2970	0.3955	0.3204
	(0.2509)	(0.2016)	(0.1760)
Unincorp × 2005	0.1866	0.2548	-0.0379
	(0.2425)	(0.2048)	(0.1867)
Unincorp × 2006	0.2050	0.4001	0.1431
	(0.2270)	(0.1946)	(0.1721)
Unincorp × 2007	0.2373	0.2862	0.0713
	(0.2354)	(0.1903)	(0.1713)
Unincorp × 2008	0.0767	0.1011	0.0531
	(0.2106)	(0.1661)	(0.1449)
Unincorp × 2009	0.1534	0.0852	-0.0513
	(0.1771)	(0.1559)	(0.1476)
Unincorp × 2010	0.2734	0.2034	0.0844
	(0.2174)	(0.1681)	(0.1458)
Observations	8,792	13,286	17,150
R^2	0.550	0.535	0.528
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni Sales	0.5791	0.6680	0.6997
Mean Unincorp Sales	1.095	1.157	1.187

Notes: This table shows estimates of Equation 2, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the frequency of home sales. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000m inclusive.

Table 14: Impact on Number of Housing Units, Flexible Specifications

	Number of Housing Units		
	250m	500m	750m
Muni × 1990	-1.025	-1.022	-0.7363
	(0.6533)	(0.4743)	(0.5938)
Muni × 2010	0.3452	0.2616	0.3212
	(0.4701)	(0.3887)	(0.5838)
Unincorp × 1990	-5.962	-4.987	-4.011
	(4.161)	(2.686)	(2.097)
Unincorp × 2010	4.301	3.114	2.784
	(2.490)	(1.946)	(1.586)
Observations	1,990	3,025	3,962
R^2	0.983	0.985	0.985
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni Units	22.20	25.51	28.99
Mean Unincorp Units	67.41	64.75	62.83

Notes: This table shows estimates of Equation 2, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the total number of housing units. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000m inclusive.

Table 15: Impact on African-American Population, Flexible Specifications

	African-American Population		
	250m	500m	750m
Muni × 1990	0.8642	2.414	2.739
	(0.5481)	(2.083)	(1.710)
Muni × 2010	-0.8064	0.8394	2.011
	(0.5797)	(1.345)	(1.128)
Unincorp × 1990	-1.867	-2.305	-2.247
	(1.505)	(1.297)	(1.299)
Unincorp × 2010	10.19	9.552	7.282
	(3.635)	(2.894)	(2.365)
Observations	1,990	3,025	3,962
R^2	0.885	0.928	0.935
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni AA Pop	1.483	4.021	5.262
Mean Unincorp AA Pop	18.66	18.96	18.75

Notes: This table shows estimates of Equation 2, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the number of African-American residents. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000m inclusive.

Table 16: Impact on White Population, Flexible Specifications

	White Population		
	250m	500m	750m
Muni × 1990	-4.313	-4.307	-4.626
	(1.488)	(1.310)	(1.398)
Muni × 2010	1.477	-0.4223	-0.6233
	(1.127)	(1.182)	(1.150)
Unincorp × 1990	-8.668	-7.615	-6.620
	(9.491)	(6.546)	(5.362)
Unincorp × 2010	-8.415	-11.85	-9.719
	(3.778)	(3.365)	(3.065)
Observations	1,990	3,025	3,962
R^2	0.979	0.975	0.973
Census Block FE	✓	✓	✓
Year x Border FE	✓	✓	✓
Mean Muni White Pop	46.04	49.68	53.91
Mean Unincorp White Pop	120.144	121.07	120.30

Notes: This table shows estimates of Equation 2, with each estimation using samples that include census blocks within 250, 500, and 750 meters of the pre-consolidation Louisville border, respectively. The outcome is the number of white residents. Standard errors are clustered at the census block level. The mean of the dependent variable for each of the jurisdiction types (treatments) is the average value for years prior to 2000m inclusive.

Chapter 2

U.S. Housing Policy during the Great Depression: An RDD Analysis of HOLC Redlining

The black-white wealth gap in the United States is one of the most enduring facts of the economic history of the United States (Derenoncourt et al. (2022)). Many point to the racial homeownership gap as a significant factor in this persistence (Ray et al. (2021)). Homeownership is widely seen as an effective method of personal and intergenerational transfer of wealth, because it forces households to save through paying down their mortgage principal (Di, Belsky and Liu (2007); Dietz and Haurin (2003); Turner and Luea (2009); Coulson and Li (2013)). While slavery and its abolition can likely explain the low initial level and corresponding convergence in the racial wealth and homeownership gap in the late 1800s, it is less clear why this progress slowed dramatically after 1900. Various factors, such as the institution of Jim Crow laws, have been suggested (Althoff and Reichardt (2022)). Another oft-cited explanation is redlining (Rothstein (2017)).

Redlining is the outright denial or the issuance of unjustifiably harsher terms of mortgage applications on a property based on certain characteristics of its neighborhood, such as the race of its inhabitants. While racial redlining by the private sector was targeted in the press in the 1950s and 60s, redlining by the public sector did not receive attention until the seminal work of Jackson (1987)[1] In his book, he describes how he found maps created in the late 1930s by the Home Owner's Loan Corporation, a government agency which worked with local real estate actors to assign neighborhoods in cities a color-coded grade based on various attributes, one of which was the race of its inhabitants. He cites anecdotal evidence showing these maps were shared with the private sector and the Federal Housing Administration (FHA), and argues the private sector and the FHA then used these maps to redline mortgage loans and insurance in neighborhoods with minority inhabitants, preventing them from attaining homeownership. Since then, there has been a robust debate among historians, sociologists and economists about whether the historical evidence supports Jackson's claim. However, systematic records on the loan behavior of the private sector and the FHA in the aftermath of the creation of these maps do not exist, making it difficult to investigate empirically (Fishback et al. (2022)).

In this paper, I investigate whether there is evidence the HOLC maps had an impact on homeownership rates by race in 1950, about ten years after the maps were created. If the HOLC maps were leveraged by the private sector and/or the FHA and consequently used in the loan/insurance decision-making process, then we would expect homeownership rates to differ in cities where an HOLC map was created compared to those without an HOLC map, all else equal. To examine whether this was the case, I utilize the fact that the maps were only created for cities with a population of 40,000 or more in a regression discontinuity

[1] While redlining was not brought up, discrimination by government institutions influencing the housing industry was widely discussed and targeted by many Civil Rights activists.

framework. In order to implement this strategy, I digitized over 30,000 individual records from the Census of Housing and Population documents from the decennial censuses of from 1920 to 1950.

My results provide suggestive evidence the HOLC maps increased the white homeownership rate by 5 percentage points. On the other hand, the point estimates measuring the impact of the maps on black homeownership lack statistical precision, making it difficult to draw concrete conclusions. Tying these results back with the broader initial question, my findings suggest these maps, or the factors behind their creation, had important implications for the housing market. These findings align with theoretical predictions that relaxing information constraints can lead to an increase in credit provision (Stiglitz and Weiss (1981)), with the additional caveat that discrimination may have still played a part in limiting credit provision to potential black homeowners. In particular, while black homeseekers may have been consistently discriminated against regardless of neighborhood, these maps may have provided information to promote the offer of loans or homebuyers insurance to white homeseekers in places where information was scarce, or where there was uncertainty about risk.

To reinforce these findings, I conduct a battery of robustness checks to verify whether these results are consistent across different specifications and placebo tests. Specifically, first, I test a range of bandwidths from 50% to 150% of the MSE-optimal bandwidths, using one or two-sided MSE-optimal bandwidths, using different order polynomials, and different kernel weighting procedures. Second, I test for placebo effects on our outcomes of interest in 1950 using different cutoffs where other policies may have been influential (e.g. 50000 instead of 40000). Finally, I test for placebo effects on our outcomes of interest in 1930, before the policy took place, to test for pre-existing differences in homeownership at the threshold. These robustness checks broadly agree with the headline findings, although some caution is warranted given some sensitivity to kernel choice.

The main contribution of this paper is to inform the historical debate surrounding the question of whether the existence of the HOLC maps lead to redlining. Jackson (1987) sparked a significant amount research in his discovery of the HOLC maps that continues until today. Generally speaking, there have been two approaches to directly test Jackson's claims: first, finding further qualitative evidence of sharing of HOLC maps with private lenders or the FHA, or second, by finding quantitative evidence to determine whether the HOLC maps were drawn in a discriminatory manner or whether the maps depicted a pattern of neighborhood patterns that already existed. For the former, while some have found evidence suggesting there was some sharing with the private sector (Howell (2015)), others have found explicit cases where the maps were requested and apparently denied (Winling and Michney (2021)). The general consensus seems to be that the HOLC by and large kept by its objective to prevent the sharing of the maps to the general public (Winling and Michney (2021)). Hillier (2003) combines qualitative and quantitative data to show that, at least in Philadelphia, HOLC grades could not explain differences in lending patterns across neighborhoods, outside of differential interest rates. In addition, she cites qualitative evidence to show redlined neighborhoods were being avoided by lenders before the HOLC made their maps. Greer (2013) takes data from a group of large cities with HOLC maps to show that age, upkeep, and the price of housing were stronger predictors of the HOLC grades compared to race. Most of the work in this literature recognizes that neighborhood characteristics and "lifecycle" theory were prominent factors in housing market decision-making prior to the HOLC maps (Fishback et al. (2021)).

More recently, scholars in economics have tried to test Jackson's claim using a few different approaches. Fishback et al. (2021) aligns with Greer (2013) in finding that race was only a minor driver for grading relative to the more prominent factors, in addition to showing that there were important pre-existing differences immediately across the borders different-graded neighborhoods, supporting the idea that the HOLC maps reflected patterns in the housing market that already existed. Aaronson, Hartley and Bhashkar (2021) use a border regression discontinuity approach combined with difference in differences, comparing outcomes on different sides of the borders between different-graded neighborhoods (e.g. C vs. D). While they acknowledge there were pre-existing differences across many of these borders, they leverage a few different strategies to try and assuage those concerns, and find that these maps had important implications for the housing market both in the short and long run. In recent work using loan-level FHA activity in Baltimore, Peoria, and Greensboro before the HOLC maps existed, Fishback et al. (2022) present strong evidence that the FHA was already avoiding insuring mortgages in certain neighborhoods before the HOLC maps, and this behavior did not change after the HOLC maps were created.

My findings complement these works in two ways: first, it takes a different empirical approach that avoids many of the potential pitfalls of using within-city variation in border assignment, leveraging the arbitrary population cutoff the HOLC chose in its creation of the maps; second, my empirical strategy speaks about the impact of the HOLC maps on outcomes in cities with a population near the cutoff of 40,000. Most of previously cited works explore data from large cities for their analyses (with the exception of Fishback et al. (2022), where one of the three cities they study). In particular, I present evidence that the creation of these HOLC maps had an important impact on the housing market for relatively smaller cities. Unfortunately, the exact mechanism behind *how* that process influenced housing market decisions is difficult to ascertain.

This paper also contributes more broadly to literature on the persistence of the black-white wealth gap, the black-white homeownership gap, and the role of homeownership in intergenerational wealth transfer. Derenoncourt et al. (2022) construct a series of the white-black wealth gap from 1860-2020, showing that gap falling dramatically from 1860 to 1900, before decreasing at a much slower rate. Homeownership is widely seen as an effective method of wealth accumulation and intergenerational transfer, because it forces households to save through paying down their mortgage principal (Di, Belsky and Liu (2007); Dietz and Haurin (2003); Turner and Luea (2009); Coulson and Li (2013)). Collins and Margo (2011) shows that the black-white homeownership gap decreased by 50% between 1870 and 1900 to about 26 percentage points, after which the gap persisted for 60 years. Althoff and Reichardt (2022) find that Jim Crow laws played an important role in long run black economic progress. My findings help inform one of the potential drivers of the persistence in the homeownership gap and consequent impacts on wealth.

Finally, this paper relates to the literature on the role of information in decision-making, specifically as it relates to discrimination. Many scholars have pointed out that, absent relevant information surrounding the decision-making process, decision-makers may rely on informal, potentially discriminatory heuristics (Phelps (1972); Arrow (1973); Aigner and Cain (1977); Fang and Moro (2011)). In the context of removing information, initiatives to ban the inclusion of criminal records on employment applications actually leads to more discrimination, if anything (Doleac and Hansen (2020)). Studying how the introduction of more information through a skills test impacted retail hiring, Autor and Scarborough (2008) find the test did not impact the race gap in hiring even though black applicants scored lower, implying employers were likely discriminating before the test's implementation. Relatedly, my findings show that while the impact of the HOLC maps on black homeownership rates are not statistically different from zero, white homeownership rates increased. Thus, while black homeseekers may have been consistently discriminated regardless of the presence of the HOLC maps, the maps may have provided information to promote the offer of loans or homebuyers insurance to white homeseekers in places where information was scarce and there was uncertainty about the risk of a loan or loan insurance.

The remainder of the paper is organized as follows. In Section 1, I will describe the historical context behind who created these maps and why they were created, along with how they may have been used by other actors in the home lending industry. Section 2 details the digitization undergone to build the dataset used and the regression discontinuity design used to study the impact of HOLC maps on homeownership. Section 3 shows the results of this design and explores potential explanations for these findings.

1 Background

1.1 The Origin of the HOLC Redlining Maps

Before the Great Depression, the average rate of home ownership in the United States was steady around 45% (Census (2011), Collins and Margo (2011)). Homeownership was generally reserved for those in middle and upper income classes, mainly because the development of mortgage instruments was still relatively primitive. Mortgages were generally of short duration (5-10 years), required significant down payments, and weren't fully amortizing such that "balloon" payments were often due at the end of the mortgage. Thus, the ability to own a home was contingent on a household's ability to save for both the down and balloon payment, and to sustain the high monthly payments for the duration of the mortgage. In addition, the availability of such credit instruments varied significantly with geography, with the South generally having the least access (Collins and Margo (2001)). However, the structure of the housing market and mortgage instruments changed dramatically with the onset of the Great Depression.

The Great Depression decimated the housing industry. Between 1928 and 1933, residential construction plummeted 95%, while home repair expenditures dropped 90%. In 1926, approximately 68,000 homes were foreclosed on, while in 1932, this number rose to 250,000. (Jackson (1987), Hillier (2003)). In response, Congress passed acts to create two institutions to reverse the decline of the housing industry: the Home Owner's Loan Corporation was responsible for providing immediate relief to homeowners under foreclosure, while the Federal Housing Administration focused on changing the structure of the mortgage industry to promote higher homeownership among the population over the long-term.

The initial task set for the Home Owner's Loan Corporation was to purchase mortgages from homeowners whose property was in foreclosure and refinance them into long-term (15-25 years), low interest, fully amortizing mortgages. For the three years following the passage of the act, from July 1933 to June 1936, homeowners could apply for this mortgage assistance, although there were some minor restrictions.[2].

Overall, the HOLC received 1.8 million applications and approved slightly over one million, or about 20 percent of all *eligible* cases (mortgaged homes in the entirety of the United States), were accepted with a total value of $3 billion (Harriss 1951). It is generally accepted that this policy was successful in arresting the freefall in foreclosures, although 20% of those

[2] "... the Corporation could exchange its bonds for mortgages (and other obligations and liens) on homes or homesteads provided that (a) no loans were made for more than 80 percent of the HOLC property appraisal or for more than $14,000, (b) the property contained dwelling facilities for not more than four families, and (c) the total value of the property did not exceed $20,000," (Harriss 1951)

assisted eventually foreclosed on their property (Harriss 1950, Rose 2011). As this lending activity came to a close, HOLC and FHLB officials realized that problems in the housing industry reached far beyond foreclosures - one official at the time estimated that almost a half of all urban home mortgages were in some degree of default (Greer (2013)). In response, the FHLB created the Mortgage Rehabilitation Unit, which was tasked with assisting and advising private corporations (who held the majority of these troubled mortgages) in processing and refinancing many of these mortgages, and in making safe investments in the future. Given that the HOLC had hundreds of offices around the country from the initial lending program, it was well equipped to fulfill that mission. As a part of these responsibilities, the MRU was assigned to "make careful detailed studies of communities or areas wherein there has been a general breakdown of functioning of mortgage lending institutions... and work with local lending institutions to relieve the situation," (Greer (2013)).

From 1935 to 1940, HOLC officials worked alongside local realtors and map-makers to produce what they called *Residential Security Maps* for the largest cities in the United States, assigning one of four grades to every area within the city. The grade represented the stability of the property's value (citations). To determine a neighborhood's grade, the HOLC used an appraisal methodology that borrowed many ideas from the private industry and the FHA (citations) The methodology they used in assigning grades considered the age, quality, mortgage availability, and public good accessibility (e.g. buses, parks) of the housing as well as the demographics, race, and income of the residents in the neighborhood. While the age and quality of the housing were primary determinants of these grades, the presence of even a few minorities often lead surveyors to downgrade a neighborhood's grade (Greer (2013), Fishback et al. (2021)). However, as many scholars have noted (Hillier (2003), Rose (2011), Greer (2013), Winling and Michney (2021), Fishback et al. (2021)), these maps were created after the HOLC's loan outlays, and thus were not drivers of HOLC's lending activity. The question remains whether the maps were shared with the Federal Housing Administration or private lenders who then used those maps to discriminate in their decision-making.

1.2 HOLC-FHA Redlining

The FHA only insured mortgages which were structured according to many of the leading trends in residential real estate during that period: they had to be long-term, low interest, fully amortized loans. By significantly reducing the risk faced by lenders and changing the structure of mortgages to make them more accessible, the government hoped lenders would be more willing to lend money for mortgages and citizens would be better able to purchase them. To that end, they only insured mortgages which they perceived were a "safe and sound investment," and enforced this by requiring mortgages to follow guidelines in the FHA's Underwriting Manual.

Two important aspects of these guidelines are worth highlighting in this context. First, these guidelines effectively limited the FHA insurance program to new construction, meaning that any FHA redlining that occurred would've likely been in newly constructed neighborhoods. Second, a crucial aspect of the approval process was the "neighborhood appraisal," grounded in the idea that a mortgage rating of a property is not only dependent on the specific person and house characteristics, but also on the people and homes surrounding the property. At the time, it was generally accepted that the presence of minorities almost surely leads to a severe decline in the value of the house, and the FHA reinforced these ideas by including discriminatory language in its first *Underwriting Manual*. For example (emphasis added):

> "Some adverse influences may be immediately noticeable while others arise gradually or are destined to occur after a certain number of years. The estimated time of such occurrence must, therefore, be compared to the life of the mortgage to arrive at a proper rating. *The more important among the adverse influential factors are the ingress of undesirable racial or nationality groups... all mortgages on properties in neighborhoods definitely protected in any way against the occurrence of unfavorable influences obtain a higher rating.*" (FHA, 1934).

For those who were involved in new construction, the ability to be backed by FHA-insured loans was a significant factor in how they constructed and financed housing. In an interview in 1939, an assistant secretary at a building and loan association in New Jersey acknowledged, "the most desirable lending areas are considered to be those having the approval of the FHA, and in all probability no loans will be made in areas not approved by that agency," (Hillier (2003)).

Given the FHA's influence on the housing and the aspects of its requirements regarding neighborhood appraisals on mortgage insurance decisions, then its knowledge of neighborhood conditions was crucial in its ability to make mortgage decisions, and there is evidence the HOLC maps were a source of information the FHA may have leveraged. The HOLC shared its maps with multiple government agencies after their completion, including the FHA: "FHLBB minutes show that board members approved the request of Ernest Fisher, director of FHA's Economics and Statistics Division, that Corwin Fergus, the director of FHLBB's Division of Research and Statistics, 'be permitted to cooperate with him in the exchange of information gathered by their respective divisions,'" (Hillier (2003)). However, as others have argued, the FHA had were creating versions of redlining maps before the

HOLC's program, and they had other sources of information for housing statistics (e.g. the 1942 Census of Housing Block Statistics), suggesting that even if the HOLC maps were used at the FHA, they would not have been the only source of information (Fishback et al. (2021)). In addition, given that the FHA requirements effectively limited their insurance program to newer construction, this inherently limited the scope of any redlining. In all, the evidence suggests that while the FHA may have been responsible for redlining (Fishback et al. (2022)), the HOLC maps were likely not a pivotal source of information without which they would have changed their practices.

1.3 HOLC-Private Sector Redlining

The other potential channel through which the HOLC maps could have had an impact was through the private sector. Some argue the HOLC shared their maps with the private sector, and they used these maps to redline prospective homeowners in these area, while others argue the private sector was already using neighborhood appraisal and redlining tactics in their decision-making before the HOLC maps even existed, casting doubt on any influence the HOLC maps may have had.

To understand if and how the HOLC maps influenced the private sector, a primary question is whether they were aware of these maps. By and large, the evidence suggests the existence of these maps and what they depicted were widely known. First, the maps themselves were created in consultation with local real estate institutions, who were aware of the objective of the mapmaking program. Second, for those who were not directly involved, the HOLC actively advertised the mapmaking program in various forums, including publications, public lectures, and newspaper articles (Winling and Michney (2021)).

The next question is whether the private sector was interested in viewing these maps. Many have argued that the private sector was already actively engaged in neighborhood appraisal, and thus implying the HOLC maps would make little difference in decision-making (Hillier (2003)). While neighborhood appraisal may have been gaining in popularity, it's clear that the maps were highly sought after by the private sector, suggesting the private sector believed the maps contained valuable information.[3] According to HOLC meeting minutes, there was "a constant demand for copies from that part of the public which was familiar with the theme of them." One letter found from an official on the Chicago Real Estate Board to the HOLC stated: "Incidentally, I hope to 'borrow' a map from your portfolio when you are not looking during your journey in Chicago." (Hillier (2003)).

The final question is whether the maps were shared with private interests. Internal HOLC

[3]One possibility is that private industry pursued the maps purely to find out the opinion of the government in relation to loan conditions.

correspondence suggests their policy was to limit the sharing of its maps to itself and other relevant government entities, even asking field agents to prevent the institutions that helped provide the information for the maps creation from viewing them (Winling and Michney (2021)). However, there are multiple documented instances where it's clear private real estate actors viewed or obtained the maps in various places around the country (San Francisco, CA Howell (2015); Waco, TX and Dayton, OH Winling and Michney (2021); Milwaukee, WI Detroit, MI and Massachusetts Michney (2022)). Jackson (1987) based his judgment primarily on questionnaires he found in HOLC records. Savings and loans associations from New Jersey were asked to which areas they thought were the most desirable to lend, and it is written that almost every response corresponded to "A and B" neighborhoods, although it is unclear whether these written responses were paraphrases (Hillier (2003)). In addition to these cited examples, my own research uncovered an article from a newspaper in Tacoma, WA that makes it clear the maps were directly shared with realtors: "a map of the city... was shown to the Tacoma Real Estate board members to obtain the realtors' reaction... to see if [the board's] rating of the different sections of the city coincided with [the HOLC] survey" (Tacoma (1937)).

In the end, it's difficult to say whether the maps were systematically shared with the real estate sector. What is clear is that private actors were aware of these maps, there was high demand to view them, and there are multiple documented instaces of maps being shared with private interests.

2 Empirical Strategy

2.1 Data

The bulk of the data for the analysis comes from Census documents published by decade from 1920 to 1950. These documents contain statistics on the number of households, renters, and owners, by race and by city. Over 30,000 records were transcribed by hand to generate the statistics necessary for the analysis. Most importantly, city population in 1930, which acts as the running score in our regression discontinuity analysis, and the homeownership rate by race by city in 1950, which is our primary outcome.

I supplement the digitized data with the complete count IPUMS data from 1930. While our primary outcomes will focus on statistics from 1950, the IPUMS data is used to verify the transcribed statistics from 1930, and to conduct robustness checks related in the pre-treatment period. From IPUMS, I retrieve the race, homeownership status, and occupation of each head of household for the years 1930[4]

To determine which cities were redlined, Mapping Inequality and Hillier (2003) , who cites a document from the FHLBB archives, for the list of cities for which a residential security map was created. We drop cities for which there was seemingly no map was made: Wilmington, DE; Washington, DC, Charleston, SC. For the purposes of this study, the removal of these cities is inconsequential, since our estimation strategy is local to the population threshold of 40,000.

Finally, the analysis focuses on central cities, where a central city is defined as the largest city within a urban area according to 1930 census definitions. This is done in order to best enforce like-for-like comparisons between units. To be concrete, the following cities are dropped: suburban cities with a population greater than 40,000 that had an HOLC map; suburban cities with a population of less than 40,000 but were graded as part of a larger city's map (e.g. Orange, New Jersey had a population of 35,399 in 1930 but was graded as part of the larger New York area map); suburban cities with a population of less than 40,000 that were not graded but were part of a metropolitan area where the center city was graded are dropped. See Appendix A for more details surrounding the construction of the data.

Table 1 displays the average homeowership rate by race, census region, and population, for the cities in our sample. A few notable patterns emerge from this table. First, within each race and region, smaller cities almost always have higher rates of homeownership. Second, within each region and population subgroup, the white homeownership rate is higher than the black homeonwership rate. Finally, there is also significant variation in homeownership rates within race and across regions. For all but the largest cities, the black homeownership rate in the North is almost half of the Midwest, with the South and West in between. The white homeownership rate shows less variation across regions, but are generally higher in the South and West relative to the North and Midwest.

To motivate our empirical strategy, Figure 1 plots the 1950 black and white homeownership rates for cities in our sample with a population of around 40,000. The variance in black homeownership rates is significantly greater than the variance in white homeownership rates.

2.2 Model

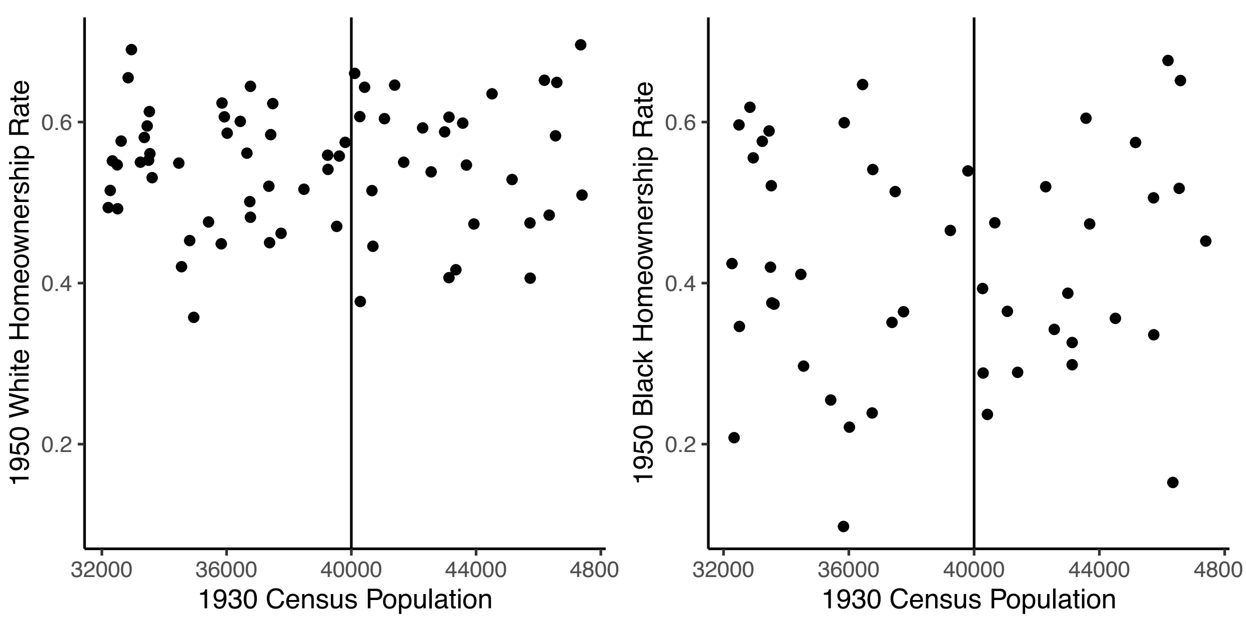
To measure the impact of HOLC maps on our outcomes, I employ a regression discontinuity design. Intuitively, since maps were created for cities with a population above 40,000 but not below, we can form two estimates our outcome of interest at the cutoff c: one estimated

[4]Unfortunately income and education are both only available for 1940 and not 1930, so I use occupation as it is consistently reported and can be seen as a rough proxy for income or education.

Table 1: Homeownership Rates by Race, Region, and Population

		Region			
Race	Population	North	Midwest	South	West
White					
	Under 40K	59	65	55	60
	Between 40K and 400K	44	60	52	58
	Over 400K	40	44	49	43
Black					
	Under 40K	35	55	42	40
	Between 40K and 400K	29	50	34	42
	Over 400K	20	24	23	27

Figure 1: Homeownership Rate around Cutoff



using only data from below the cutoff, τ_{-}, and the other estimated using only data from below the cutoff, τ_{+}. The difference in these estimated outcomes will be capture the impact of the maps on our outcomes of interest at the cutoff (e.g. a "local" average treatment effect). Following Calonico, Cattaneo and Titiunik (2015), Cattaneo, Idrobo and Titiunik (2019):

The baseline model estimated is as follows:

$$Y_i = \alpha + \tau T_i + \mu_{-1}(X_i - c) + \mu_{+1} T_i (X_i - c) + \epsilon \qquad (1)$$
$$\forall i \;\; s.t. \;\; c - h_l \leq X_i \leq c + h_r$$

where for each city i, Y is the outcome of interest in the city (homeownership rate by race), X is the city's population, c is the cutoff of 40,000, and T is an indicator that equals one when the city's population X is greater than 40,000. h_l, h_r are the bandwidth that determines the "locality" of the regression on each side of the cutoff. τ represents the treatment effect of the HOLC maps at the cutoff point. When Y is the black homeownership rate, the sample of cities is limited to those with at least 100 black households in 1950. The bandwidths are determined by Calonico, Cattaneo and Titiunik (2015) two-sided MSE-optimal procedure. To be explicit, the above model is linear in the score variable (city population), and observations weighted by a triangular kernel (both package defaults). Results using variations on these choices are discussed below as well as in the appendix.

In some specifications, we also add indicators for Census region in a linear fashion, where we omit the North from all estimations.

$$Y_i = \alpha + \tau T_i + \mu_{-1}(X_i - c) + \mu_{+1} T_i (X_i - c) + region_i + \epsilon_i \qquad (2)$$
$$\forall i \;\; s.t. \;\; c - h_l \leq X_i \leq c + h_r$$

3 Results

Table 2 presents the model estimates with homeownership rate by race in 1950 as the outcome. Columns 1 and 2 estimate Equation 1 and Equation 2, respectively, where Y_i is the black homeownership rate, while columns 3 and 4 estimate Equation 1 and 2 where Y_i is the white homeownership rate.

Looking at the estimated impact of the HOLC maps on black homeownership rates in Columns 1 and 2, the point estimates are small and of opposite sign, while their standard errors are large, making it difficult to draw any conclusions from these estimates. One

Table 2: Baseline Results

	1950 Black Homeownership		1950 White Homeownership	
	(1)	(2)	(3)	(4)
τ	-0.041	0.035	0.011	0.056
	[-0.1536, 0.0556]	[-0.0528, 0.1388]	[-0.0363, 0.0598]	[0.0175, 0.0998]
h_l, h_r	7929, 132070	11920, 138717	10199, 129389	5366, 79490
n_l, n_r	26, 123	50, 123	57, 141	23, 123
N_l, N_r	311, 165	311, 165	2711, 184	2711, 184

Notes: Columns 1 and 2 estimate Equation 1 and Equation 2, respectively, where Y_i is the black homeownership rate, while columns 3 and 4 estimate Equation 1 and 2 where Y_i is the white homeownership rate. τ represents the treatment effect of the HOLC maps, while the brackets immediately below contain the 95% confidence intervals. h_l and h_r are bandwidth sizes for the left and right side of the threshold, respectively, while n_l and n_r are the effective number of observations included in the estimation on each side of the threshold, using those bandwidths. N_l and N_r are the total sample sizes on either side of the threshold.

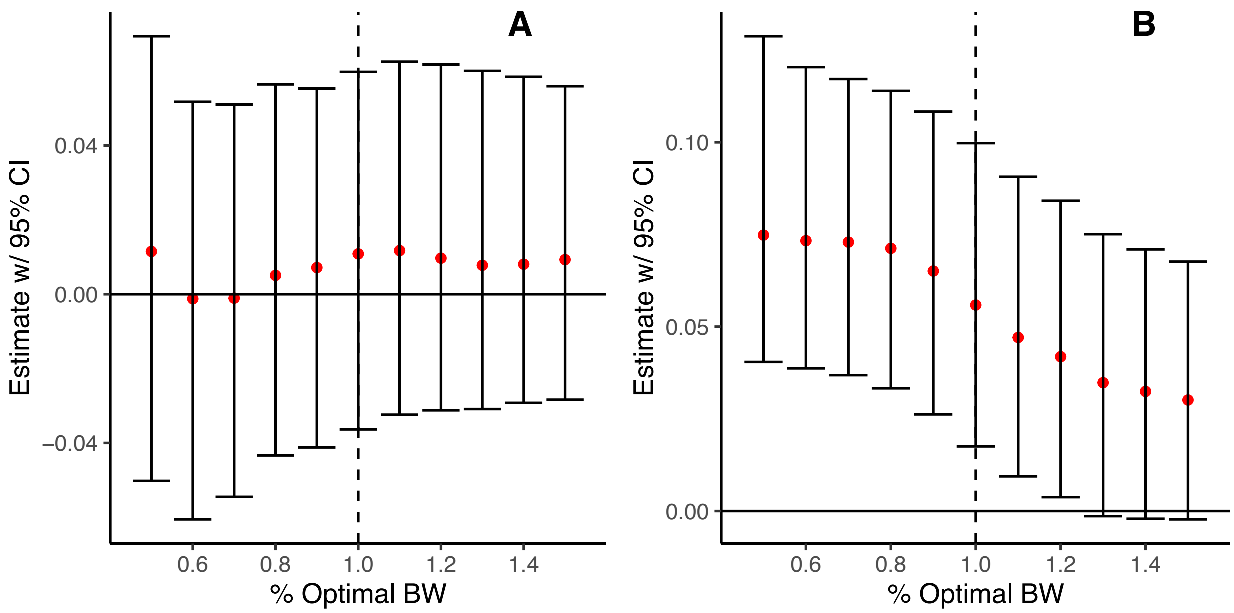
notable aspect of these estimations is that by limiting the analysis to cities with a sizeable black population, the sample of cities shrinks considerably relative to the the total number of cities. This "attrition" largely occurs below the threshold - while the number of cities below the threshold in the full sample is 57, the number of cities with a sizeable black population below the threshold is 26. Statistical precision is problematic for all analyses where Y is the black homeownership rate, so for sake of brevity, the remainder of the analysis will focus on where Y is the white homeownership rate. All further analyses for the former is available in Appendix B.

Moving to the estimations where Y is the white homeownership rate, Column 3 shows a small, positive, and statistically insignificant impact of the HOLC maps on white homeownership. However, Column 4 suggests the HOLC maps increased white homeownership rates by a statistically significant 5 percentage points. Comparing the estimation with region indicators (Column 4) with the baseline estimation (Column 3), we can see that adding the region indicators implies much smaller bandwidths and corresponding effective sample sizes (198 vs 146). While the smaller bandwidths are not unexpected, the fact that the there is a meaningful difference in the point estimates between Columns 3 and 4 does suggest some caution is warranted in interpreting these results, as adding covariates should generally increase precision in a correctly specified model. We explore the strength of this result in further robustness checks.

To further check the strength of the result of an impact on white homeownership, Figure

2 explores whether Columns 3 and 4 are sensitive to bandwidth choice, testing bandwidths between 50% and 150% of the two-sided MSE-optimal bandwidth in the original specification (represented by the vertical dotted line).[5]

Figure 2: 1950 White Homeownership, Testing Bandwidths



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the white homeownership rate in 1950. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

The left panel of Figure 2 shows the point estimate and confidence interval re-estimating Column 3 with varying bandiwdths. The point estimates across the different bandwidths are consistent, staying within +/- 1 percentage points of the baseline specification. At the same time, the right panel of Figure 2 replicating Column 4 shows the estimates keep statistical significance across bandwidths, while the point estimates are within 2 percentage points of the baseline. These findings are encouraging in the sense that the flexible estimations are broadly consistent with the baseline estimations. Further robustness checks on whether these results are sensitive to polynomial choice, bandwidth selection, and kernel choice are

[5]When conducting inference on a point estimate that leverages an MSE-optimal bandwidth b, one must estimate the bias related to that point estimate. That involves using a different, data-driven bandwidth h. However, when we force the estimation to use a non-MSE-optimal bandwidth, the proper procedure to select h is unclear. For the purposes of testing these different bandwidths, I keep the ratio of h/b consistent across the different estimations.

available in Appendix Section C. These checks are broadly consistent with those presented here - most specifications including region show the HOLC maps cause about a 5% point increase in the white homeownership rate. The results of the estimations using a uniform kernel are less consistent, suggesting some caution in interpreting these results (Cattaneo, Idrobo and Titiunik (2019)).

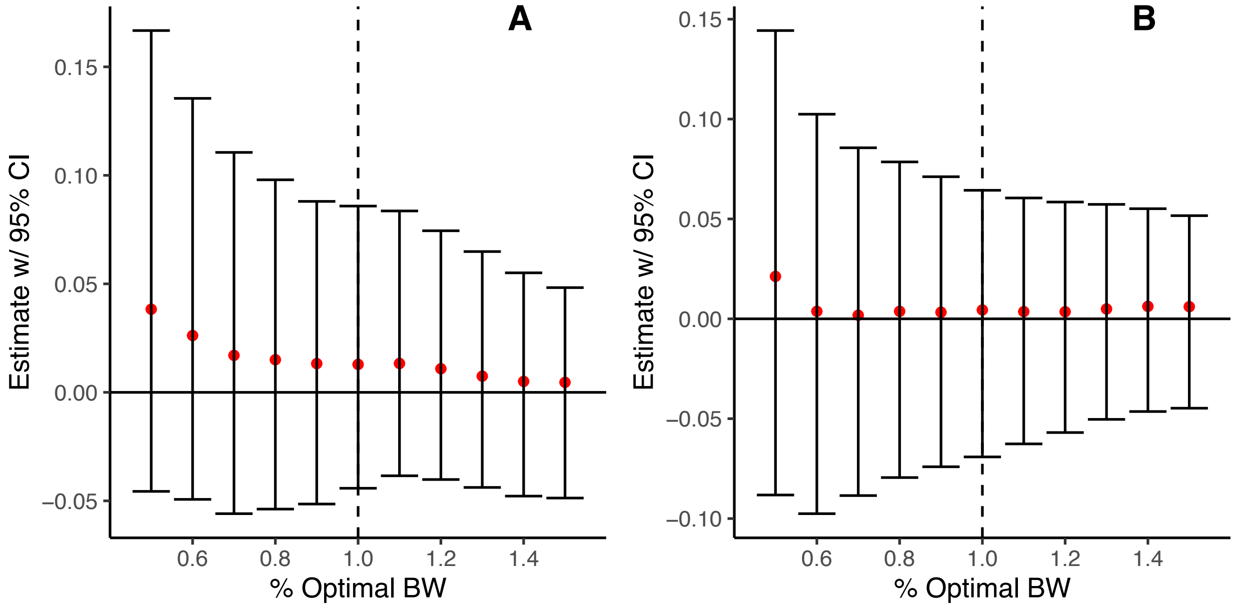
One potential threat with any regression discontinuity designs is if there was another concurrent (or historic) policy which used the same cutoff point to deliver a treatment. To the best of my knowledge, no other public policy either at the time or in the past used a threshold of 40,000 city size as a cutoff. However, the Census published block-level housing statistics for cities with a population above 50,000 in 1942 - if these reports were systematically leveraged in any way by actors in the housing market, our design may pick up variation induced by this publication (to be clear, any private actor would still have had to translate the statistics from these publications into a risk score, a task that may not have been feasible for smaller institutions). To check whether this publication confounds the interpretation of the regression discontinuity design, I re-estimate the baseline model (and with covariates) using an alternative cutoff at 50,000 instead of 40,000.

These findings are encouraging and support the fact that our design is not contaminated by the Census publication in 1942. In particular, the point estimate using the optimal bandwidths are are small (less than 1%) and not significantly different than zero. Furthermore, the estimations using bandwidths near the optimal bandwidth show a similar pattern - small point estimates and no statistical significance.

Another potential threat to the empirical strategy is if there was any knowledge of the treatment assignment rule before it was assigned, such that units try to sort themselves along the running variable in order to sort into or out of treatment. Given our running variable is based on city size, and the program was conceived and implemented in the mid-1930s based on historic data (1930 census), it seems unlikely there was any sorting of cities across the cutoff. To confirm this is the case, we run a nonparametric density test of cities around the threshold, displayed in Figure 4 (Cattaneo, Idrobo and Titiunik (2019)). This confirms the density of cities around the threshold is smooth.

As a final robustness check, I test whether there were any pre-existing differences at the threshold in the outcomes of interest (pre-treatment outcomes) by changing the outcome of interest to homeownership rates by race for 1930. The results of these tests are presented Figure 3 and 5, and give further credence to my findings. Using the optimal bandwidth, the point estimates are small and not statistically different from zero. Similarly, the point estimates for both figures across the specifications using different bandwidths near the optimal bandwidth show that zero is well within the 95% confidence intervals.

Figure 3: Alternate Cutoff Placebo Test



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the white homeownership rate in 1950, and c is the placebo threshold of 50,000. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

Figure 4: Cutoff Density Test

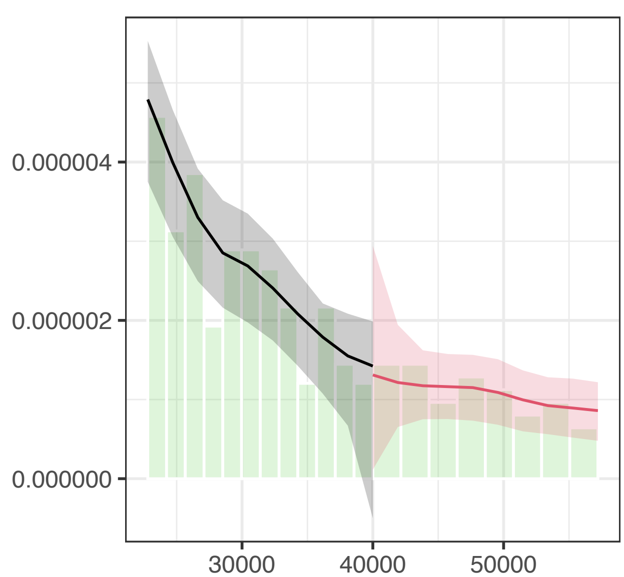
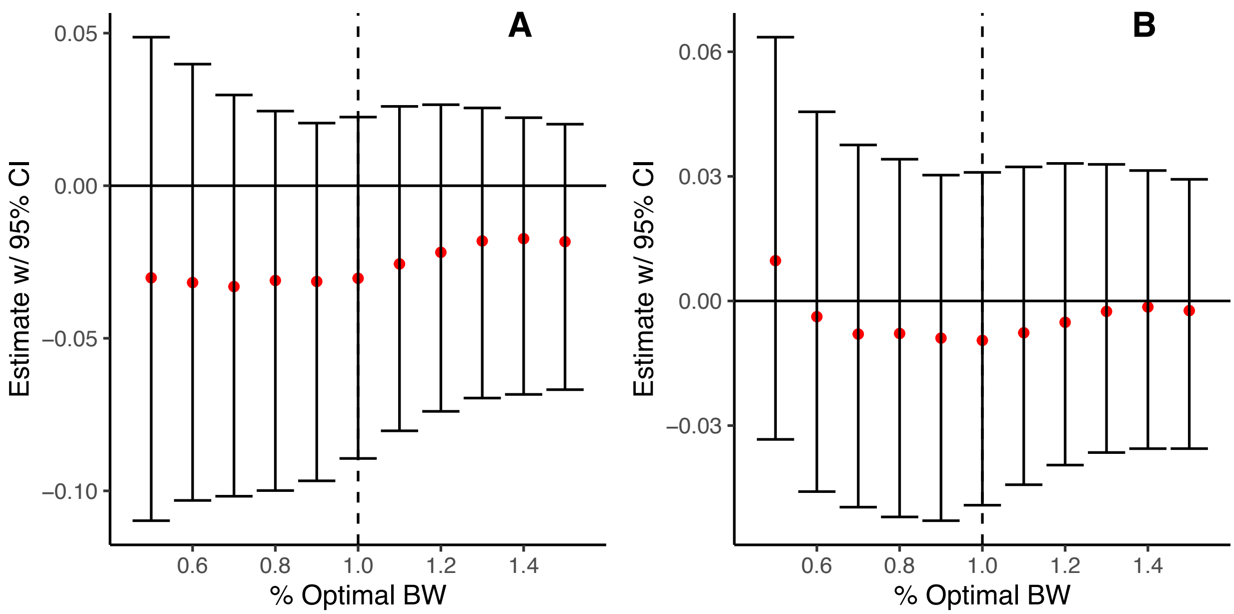


Figure 5: Pre-Treament Outcomes Placebo Test



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the white homeownership rate in 1930. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

3.1 Discussion

The results presented provide suggestive evidence that cities where HOLC maps were created with a population above 40,000 had 5 percentage points higher white homeownership rates. These findings support the hypotheses that either the maps were shared with the private sector more widely than initially thought, or that the process the HOLC used to create the maps changed the practices of local real estate institutions. For the former hypothesis, given the multitude of instances where HOLC maps were shared with private industry, it seems plausible the maps were more widely shared than the literature suggests (Aaronson, Hartley and Bhashkar (2021)). As for the latter hypothesis, the fact that the HOLC actively involved local private actors in the map-making process may well have institutionalized the practice of neighborhood appraisal and redlining, regardless of whether the HOLC maps were actually shared with the local decisionmakers. Due to the nature of the mapmaking process and the empirical methodology, it is difficult to distinguish between these two hypotheses without further qualitative evidence.

An important aspect of the creation of the HOLC maps that is less discussed in the literature is that the HOLC maps were created for cities of vastly different populations. The potential mechanisms for how the HOLC impacted homeownership may be more or less relevant depending on the size of the city. For example, some scholars point to the fact that there were thousands of private lenders in a city like Philadelphia, and thus it is extremely unlikely that the involvement of the private sector and any sharing of the maps would have been sufficiently widespread to meaningfully impact the local housing market (Hillier (2003)). While this may be true for larger cities[6], cities with a population near the threshold of 40,000 surely had fewer lenders, and the influence of a few important real estate actors would have likely been more pivotal. By the same token, while neighborhood appraisal and redlining may have been more established in larger cities, local real estate institutions in smaller cities may not have followed these practices in a counterfactual world where the HOLC did not work with these institutions to collect information to make their maps.

[6] It's not necessarily clear this is true for large cities: in San Francisco an assistant vice president of a local lending institution shared, "[r]eferring to the Security Area Map of San Francisco shown us, we would say that we will make residential loans upon our maximum terms in all the 'green' areas shown except A-4 and A-6,"... "[t]he institutions interviewed accounted for nearly 100 percent of the non-HOLC mortgages in San Francisco" Howell (2015). Thus, the lending institutions' knowledge of these maps, and their importance in the market, means the HOLC maps had the potential to have a significant impact on the housing market.

4 Conclusion

Understanding the impact of the government policies on homeownership is essential given its implications on the wealth of its citizens. One policy that has received increased attention was the creation of redlining maps by the HOLC in the late 1930s. To investigate whether there is evidence the HOLC maps had an impact on homeownership rates, I use a regression discontinuity design with homeownership by race in 1950 as the outcomes of interest, taking advantage of the fact that the maps were only created for ciites with a population above 40000. If the HOLC maps were leveraged by the private sector and/or the FHA and consequently used in the loan or insurance decision-making process, then we would expect homeownership rates to differ in cities where an HOLC map was created compared to those without an HOLC map, all else equal. As part of this project, I digitized over 30,000 individual records from the Census of Housing and Population documents from the decennial censuses of 1920, 1930, 1940, and 1950.

While the estimates measuring the impact of the maps on black homeownership lack statistical precision making it difficult to come to any conclusion, my results provide suggestive evidence the HOLC maps increased the white homeownership rate by 4-6 percentage points. Thus, it appears the creation of these maps had important implications for the housing market, whether it was through providing better information about the risks of real estate investments, or by institutionalizing a practice of redlining throughout the housing market. The channel through which this effect happened is difficult to deduce, although the existing evidence points to the private sector as opposed to the FHA.

The recent scholarship surrounding the HOLC and the redlining maps has made great strides gaining a fuller understanding the historical background of the HOLC redlining maps. More qualitative and quantitative work is needed before we can understand the full impact of the HOLC maps on local real estate markets.

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Appendices

A Data Appendix

The bulk of this project relies on housing data digitized by hand in 2016. The universe of available data for housing statistics by Census year, race, and city are contained in Table 3, along with the name of the fields digitized to construct the main outcome of interest, homeownership by race and by city. This effort required 3 months of full-time transcribing, with the number of individual statistics transcribed totaling over 30,000.

Table 3: Universe of Housing Statistics by Census Year, Race, City

Year	**Document**	**Universe**	**Fields**
1920	1920 Census of	Population > 10,000	
1930	1930 Census of Population Volume 6	Population > 2,500	
1940	1940 Census of Housing Volume 1	Population > 2,000	
1950	1950 Census of Housing Volume 1 Tables 17, 22	Population > 2,000	Occupied Units

Some census years contained the same statistics from the previous Census year. For example, the 1940 Census contained the population for cities for both 1940 and 1930. There were some differences between the Census years. In the dataset used for the analysis, the population from 1930 is taken from the 1930 Census, as that is what was available to policymakers at the time and leveraged to determine which cities to create an HOLC map. If there are any differences in the homeownership by race statistics (outcome variables), the data is taken from the latest Census year.

At the time of transcription, there was no publicly available data of homeownership by race for smaller cities for the Census years of 1920, 1930, 1940, or 1950. The 1920 Census 1% IPUMS sample does not contain statistics for cities with a population smaller than x. The 1930 Census 1% IPUMS sample does not contain statistics for cities with a population smaller than x. The 1940 Census 1% IPUMS sample does not contain statistics for cities with a population smaller than x. The 1940 Census 100% IPUMS extract did not contain statistics for cities with a population smaller than x persons. The 1950 Census 1% IPUMS sample extract does not contain statistics for cities with a population smaller than x persons.

More recently, the 100% IPUMS extracts of the Census years 1930 and 1940 have been updated with statistics on smaller cities. This allowed me to verify the validity of my transcriptions, as well as note some differences in the statistics between the two sources.

Table 4 contains shows the cities where the difference in homeownership rate by race differed by more than 2 percentage points.

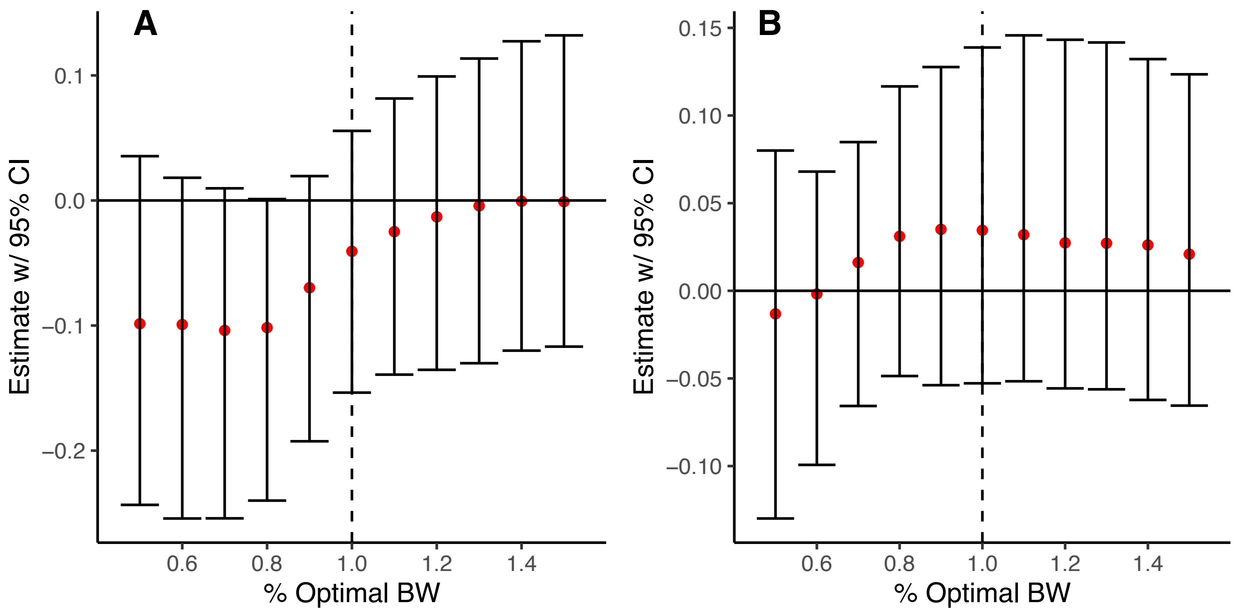
Table 4: Differences in 1930 Housing Statistics between Census and IPUMS

Year	City	1930 Population	Race	Census	IPUMS
1940	Meriden, CT		Black	15%	24%
	Dubuque, IA		Black	46%	32%
	North Bergen, NJ		Black	19%	14%
	Lacrosse, WI		Black	60%	50%
	Oshkosh, WI		Black	37%	44%

After consultation with IPUMS, the difference likely stems from how the Census treated group quarters.

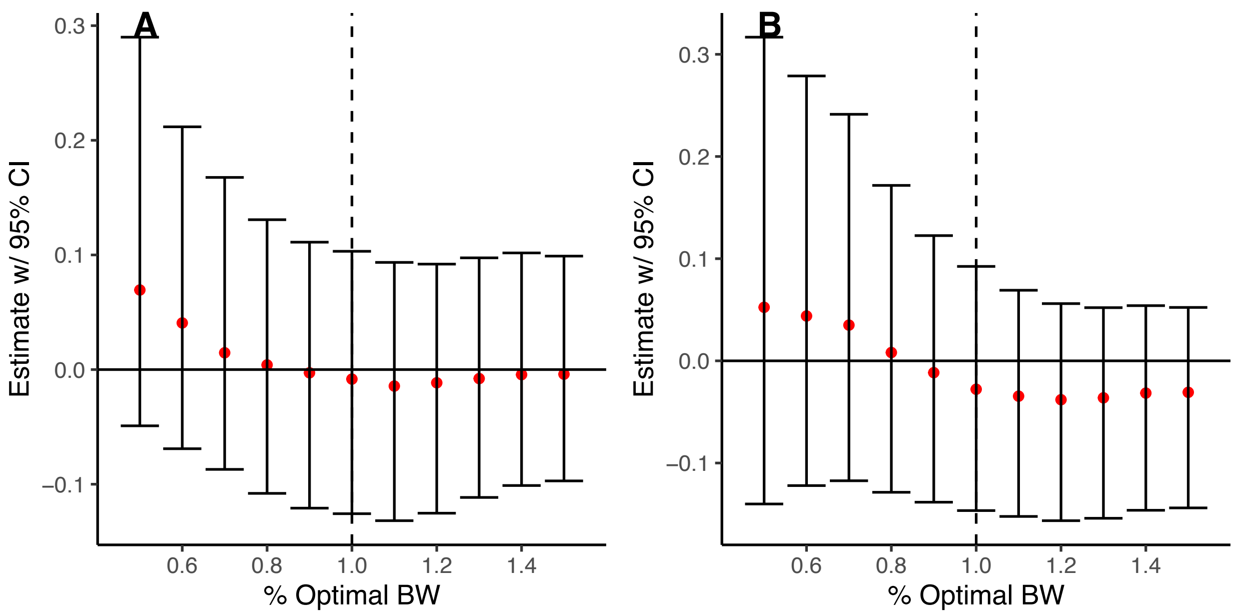
B Results for Black Homeownership

Figure 6: 1950 Black Homeownership, Testing Bandwidths



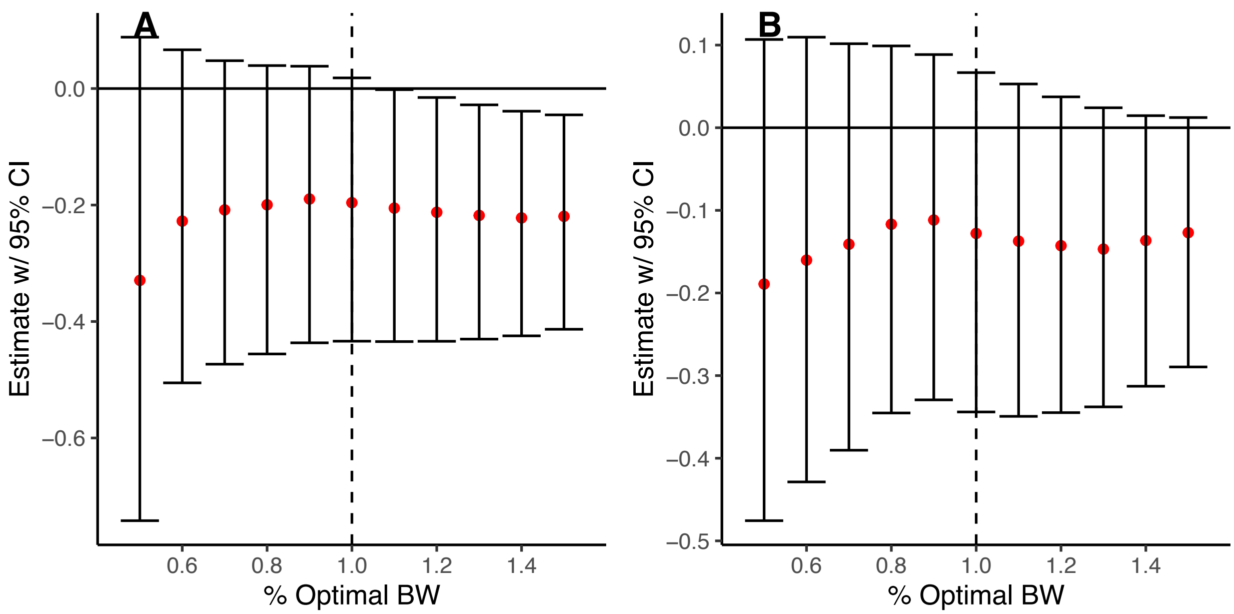
Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the black homeownership rate in 1950. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

Figure 7: 1950 Black Homeownership, Alternate Cutoff



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the black homeownership rate in 1950, and c is the placebo cutoff of 50,000. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

Figure 8: 1950 Black Homeownership, Pretreatment Placebo



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C RDD Sensitivity Analyses

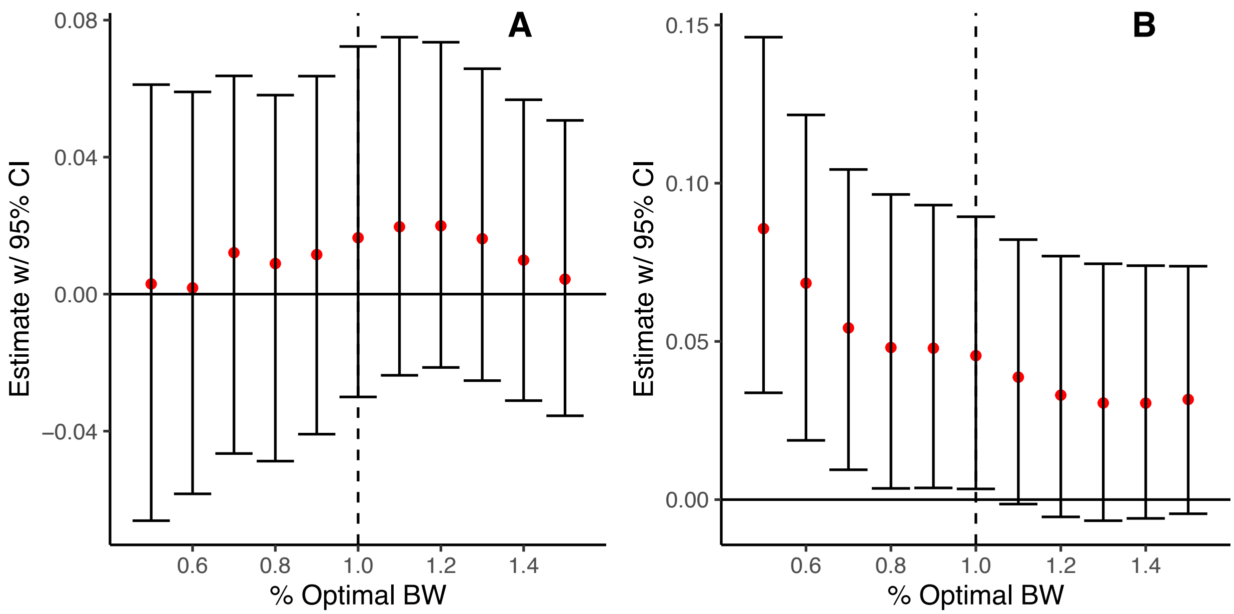
C.1 Sensitivity to Polynomial Choice

Here we present the sensitivity of the main results to polynomial choice, by estimating a local quadratic regression instead of a local linear regression. Specifically,

$$Y_i = \alpha + \tau T_i + \mu_{-1}(X_i - c) + \mu_{+1} T_i (X_i - c) + \mu_{-2}(X_i - c)^2 + \mu_{+2} T_i (X_i - c)^2 + \epsilon_i \quad (3)$$
$$\forall i \;\; s.t. \;\; c - h_l \leq X_i \leq c + h_r$$

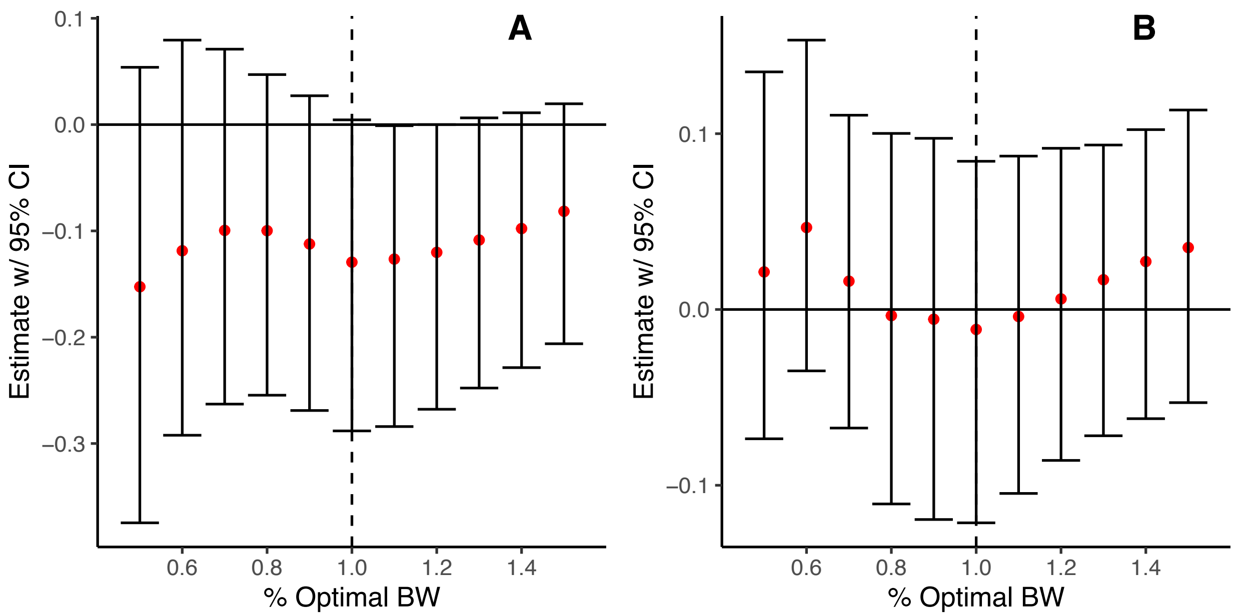
$$\begin{aligned} Y_i = \alpha + \tau T_i + \mu_{-1}(X_i - c) + \mu_{+1} T_i (X_i - c) + \\ \mu_{-2}(X_i - c)^2 + \mu_{+2} T_i (X_i - c)^2 + region_i + \epsilon_i \\ \forall i \;\; s.t. \;\; c - h_l \leq X_i \leq c + h_r \end{aligned} \quad (4)$$

Figure 9: 1950 White Homeownership, Quadratic



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 3, where Y is the white homeownership rate. Each point estimate and corresponding confidence interval comes from different estimations of Equation 3 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 4.

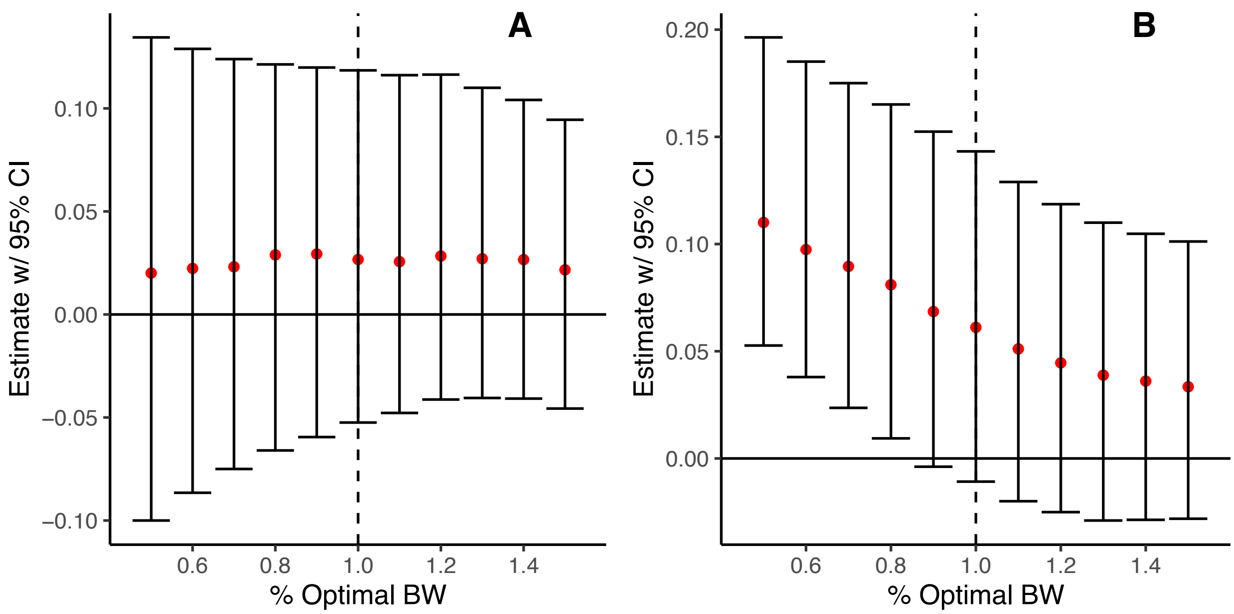
Figure 10: 1950 Black Homeownership, Quadratic



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 3, where Y is the black homeownership rate. Each point estimate and corresponding confidence interval comes from different estimations of Equation 3 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 4.

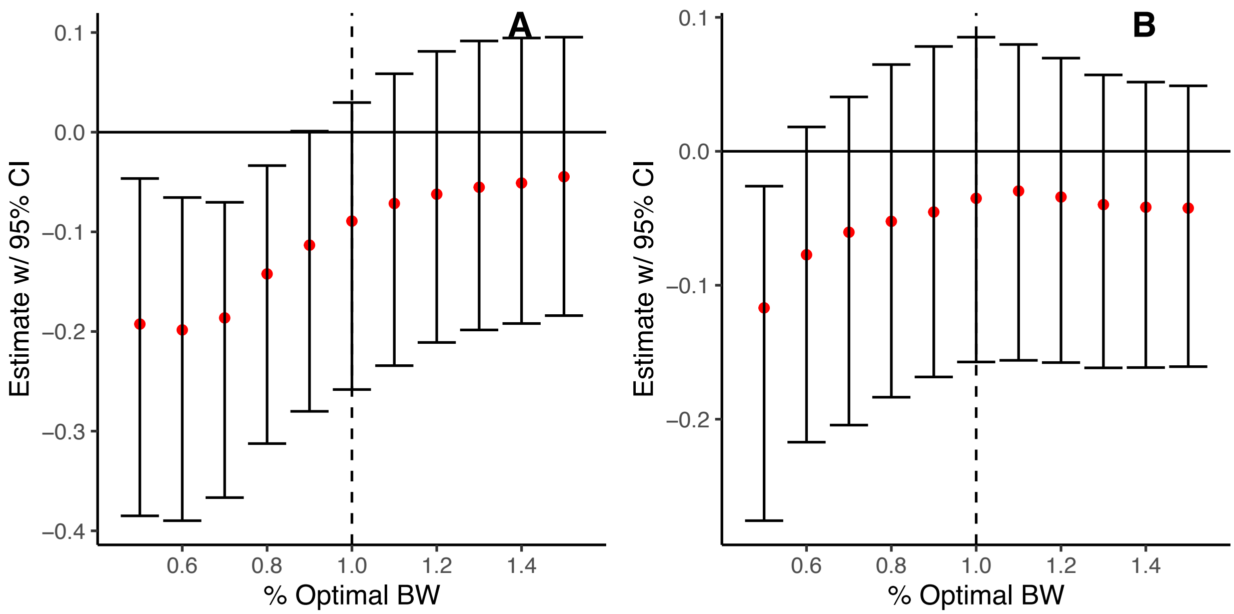
C.2 Sensitivity to Bandwidth Type

Figure 11: 1950 White Homeownership, $h_l = h_r$



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the white homeownership rate. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal one-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

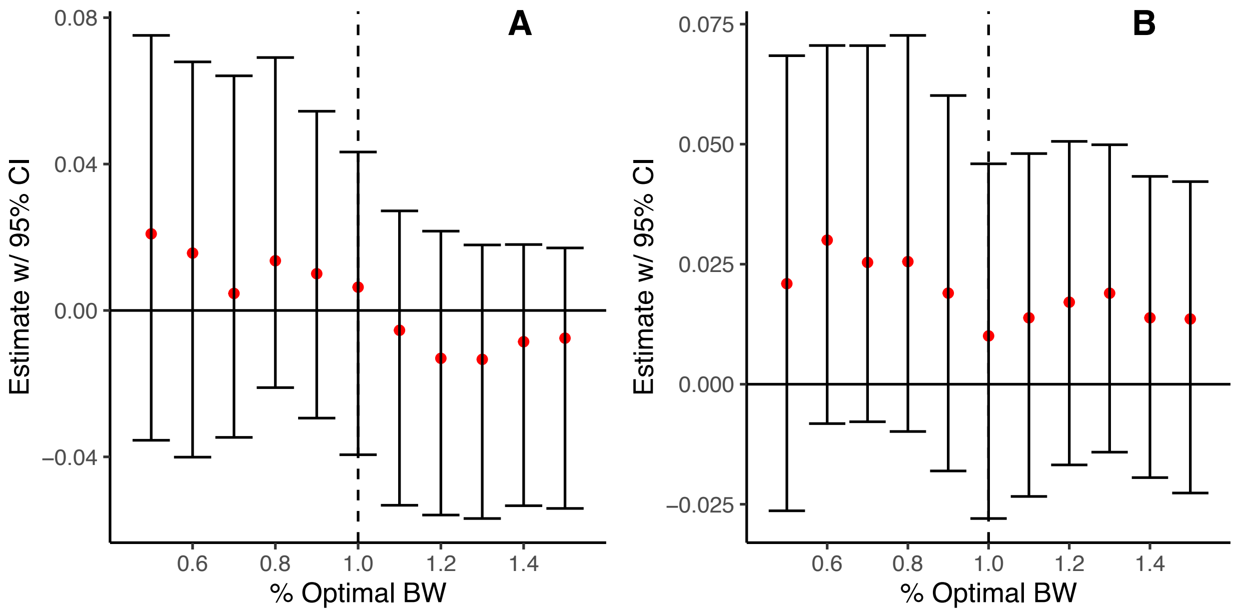
Figure 12: 1950 Black Homeownership, $h_l = h_r$



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the black homeownership rate. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal one-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a triangular kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

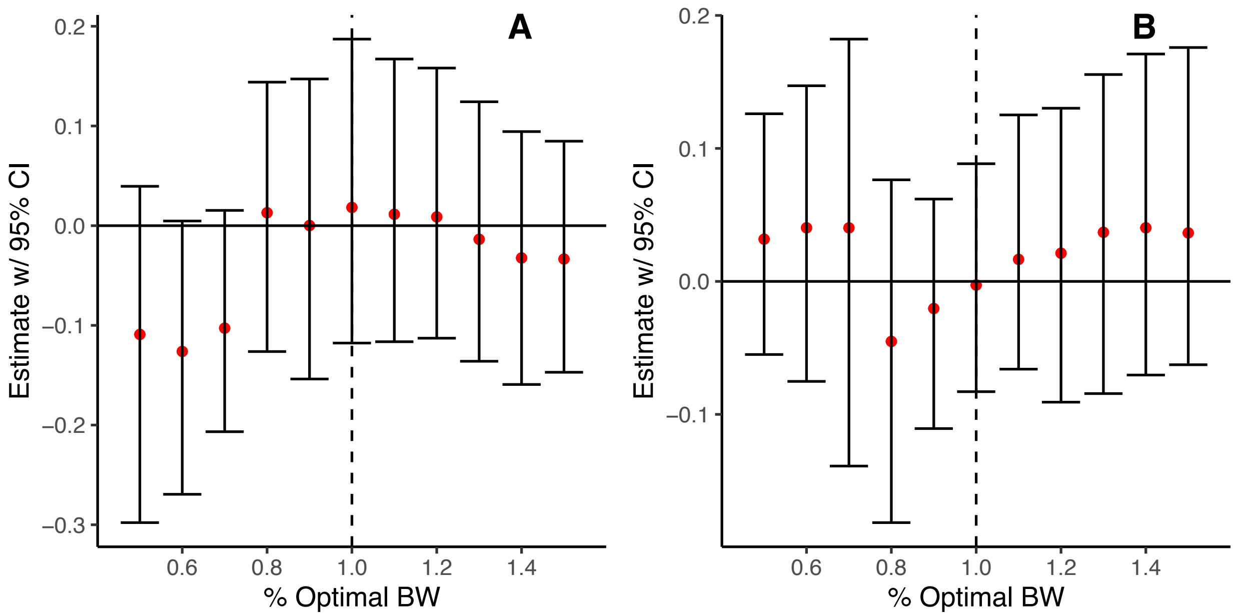
C.3 Sensitivity to Kernel Choice

Figure 13: 1950 White Homeownership, Uniform Kernel



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the white homeownership rate. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a uniform kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

Figure 14: 1950 Black Homeownership, Uniform Kernel



Notes: Subplot A plots point estimates (red dots) and 95% confidence intervals (black bars) for τ in Equation 1, where Y is the black homeownership rate. Each point estimate and corresponding confidence interval comes from different estimations of Equation 1 using different samples. The samples are determined by using different values of the bandwidths, h_l and h_r, where these bandwidths are calculated using the optimal two-sided data-driven bandwidth procedure from Calonico, Cattaneo and Farrell (2020). Observations are weighted by a uniform kernel. The vertical dashed line (where x=1) shows the point estimate and confidence interval for the optimal bandwidth. Subplot B plots the same data points from estimations of Equation 2.

Chapter 3

Does Public Health Education Help Explain the Great Escape? The Rockefeller Foundation in France

Unprecedented increases in life expectancy during the first of half of the 20th century in many now-developed countries led Angus Deaton to label the event as “The Great Escape." In the United States, the mortality rate per 1,000 fell from 17.1 to 9.8 from 1900 to 1950. A large part of that decline was due to a vast improvement in mortality for children under 5, which declined from 238 per 1,000 to 40 per 1,000. As to the causes of this drastic decline, according to Deaton, “if there is one single factor that was primarily responsible for the great escape, it was the discovery of the germ theory of disease and its implementation through public health measures," (Deaton, 2006). While the pendulum has recently swung in favor of public health measures playing the most significant role in the decline, the degree of public health's role versus other social and economic factors is still an ongoing argument in the literature.

In the late 1960s and 70s, McKeown (1976) ignited a lively conversation around the causes of the mortality decline, hypothesizing that higher incomes and the corresponding improvement in nutrition was the main contributor to the reduction. He showed that reductions in disease mortality in England occurred long before medical treatments were invented or implemented to ameliorate those diseases. On the other hand, England experienced a significant increase in GDP per capita at the same time as the reduction in mortality, and this increase in income was accompanied by an improvement in nutrition. McKeown used this “process of elimination" to deduce that public health could have only played a minor role in the “Great Escape". While his work initially sent the fields of public health and medicine into an intense period of self-reflection, it began to receive push back almost immediately. Szreter (1988) and Mitchell (1990) posited that the public health efforts of England and Germany were the main reasons behind their respective successes reducing mortality, especially relative to other countries such as France, where public health progress was more stagnant. More recently, works using modern econometric techniques find that various forms of public health measures can help explain some of the mortality decline, but the majority is still unexplained.

This paper studies the impact of a nationwide public health campaign held in post-WWI France by the Rockefeller Foundation. The main goal of the campaign was to educate French citizens about hygienic behavior, with particular focuses on addressing infant and tuberculosis mortality, due to the prominent roles both played as a part of total mortality. As part of the campaign, multiple teams from the Rockefeller Foundation traveled to communes around the country, holding *soirées* at city halls and theaters where state-of-the-art film projectors were used to educate the populace about hygienic behavior. In addition, the teams went to schools and large places of work, such as factories, to present their material and hand out pamphlets and other educational material. To study the impact of this campaign, I leverage the variation in treatment across departments in a difference-in-differences design, using total mortality by age group as the primary outcome. The campaign has the largest impact on the 40-59 age group, reducing mortality by about -1.0403 per 1,000 residents. For those aged 20-39, the campaign reduced mortality by about -0.3594 per 1,000 residents.

The main contribution of this paper is to inform the historical literature studying the decline in mortality seen in many countries in the early 1900s. In particular, the results shows that public health information lead to improved health outcomes during the Great Escape. Most modern work thus far has focused on studying the physical investment side of public health, specifically in regards to water and sewer investment, along with the construction of dispensaries.[1] Examining the experience of Chicago from 1850-1925, Ferrie and Troesken (2008) used time series regressions to show that 30-50% of the decline in the crude death rate over that time period was due to water purification. Using mortality data on 25 U.S. municipalities

[1] As Egedesø, Hansen and Jensen (2020) explore, education through these dispensaries could have been a potential channel they had an impact.

from 1900-1940, Anderson, Charles and Rees (2022) re-examined a case first studied by Cutler and Miller (2005), and showed that water filtration led to an approximate 11% decline infant mortality. Alsan and Goldin (2019) leveraged mortality data from Massachusetts to provide evidence that the combined effects of clean water and sewage infrastructure account for approximately one-third of the decline in log child mortality from 1880-1920. Similarly, Kesztenbaum and Rosenthal (2017) studied Paris' 80 neighborhoods from 1880-1914 to show that the introduction of sewer infrastructure led to an increase of one year of life expectancy at age one. Moving toward non-water public health measures, Anderson et al. (2019) studied public health measures such as sanatorium construction against tuberculosis in the United States from 1900-1917, and found these measures only explain a modest proportion in the decline in tuberculosis mortality. In Denmark, Egedesø, Hansen and Jensen (2020) focused specifically on anti-tuberculosis dispensaries and determine they are responsible for 16% of the decline in tuberculosis mortality from 1890 to 1939. The authors propose preventative action and information provision as the main mechanism for the reduction. Clay et al. (2020) use a synthetic control approach to study a comprehensive anti-tuberculosis public health program in Framingham, Massachusetts, which included both physical infrastructure investment as well as educational campaigns. They find no evidence the campaign had any impact on tuberculosis mortality. Finally, in a closely related work, Bhalotra, Karlsson and Nilsson (2017) look how an information campaign related to maternal nutrition/sanitation in Sweden in 1931–1933 impacted future mortality risk. They find a 1.56 percentage point decline in the risk of infant death relative to a baseline risk of 24%.

Second, this paper is related more broadly to the literature surrounding public health efforts in countries today. The effectiveness of informational campaigns from modern studies is mixed, often depending on the context and outcome. In this paper, I show that a nationwide educational campaign which taught citizens about public health and hygienic behavior can have meaningful impacts on mortality, encouraging policymakers to consider it as one option to help reduce mortality in their respective countries. Infectious and water-born diseases are still prevalent in many areas around the world, and policymakers have struggled to bring down mortality to levels seen in other countries. Much work has been undergone to show the effectiveness of water infrastructure investment on outcomes, such as Bhalotra et al. (2021) and Kremer et al. (2023). In terms of how residents of developing countries respond to information/education, there is a large body of work driven by randomized controlled trials that explore different information treatments in a wide variety of settings. Meredith et al. (2013) use multiple experiments across different regions and find no effect of providing information on preventative health behaviors, even though genuine learning about the benefits of the behavior occurred. While Jalan and Somanathan (2008) and Madajewicz et al. (2007) find mortality-saving changes in behavior after an information treatment related to water purity, Bennear et al. (2013) find a precise zero effect, ruling out large positive effects. In a modern version of Bhalotra, Karlsson and Nilsson (2017), Fitzsimons et al. (2016) find that an intervention in Malawi which educated mother's about infant nutrition improved health outcomes.

The remainder of the paper is organized as follows. In Section 1, I will describe the historical context behind movements in mortality in the early 1900s, highlighting the specific case of France and the Rockefeller campaign. Section 2 details the digitization undergone to build the dataset used and the empirical framework used to study the impact of the Rockefeller campaign on mortality. Section 3 shows the results of this design and explores potential explanations for these findings. Section 4 concludes.

1 Background

1.1 Public Health in France

In the beginning of the 1900s, France lagged behind its neighbors in combating diseases and promoting public health measures. Germany adopted mandatory smallpox vaccination in 1874, whereas France did not do so until 1902. For almost thirty years, France suffered significant losses of life, while in Germany, smallpox was almost completely eliminated (Mitchell, 1990). Another sphere where France fell behind was in combating tuberculosis, where the lack of action was more consequential. Just before the start of World War I, England's tuberculosis mortality rate was 1.35 per 1,000, while Germany's was 1.48 per 1,000. On the other hand, France had a tuberculosis mortality rate of somewhere between 2 and 3 per 1,000. In Paris, where more reliable numbers could be attained, the tuberculosis mortality rate was 3.78 per 1,000, compared to 2.08 in Berlin and 1.50 in London (Biggs, 1917). Looking at the 1911 *Statistique Sanitaire*, even though the numbers are likely underreported, tuberculosis was still the leading cause of death for every age group besides infants and those over 60 years old, impacting young adults the hardest (Statistique Sanitaire, 1911). Overall, from 1896 to 1913, France's mortality rate declined approximately 11%, from 20 to 17.7 per 1,000, while Germany's declined 27% from 20.8 to 15.0 per 1,000.

Much of this lack of progress in combating diseases has been attributed to ineffective laws and institutional structures that were unable to coordinate a systematic effort to combat infectious diseases (Mitchell, 1990). There were laws in place to improve health outcomes, from the Medical Assistance Law of 1893, which promised free medical care to the indigent and poor, to the 1902 Public Health Law, which gave local authorities various powers over enforcing sanitary conditions and reporting mechanisms, but they were plagued by a lack of enforcement (Nord, 1994). As for institutions, the Commission for the Prevention of Tuberculosis in 1903 was formed with the goal to help build out the medical infrastructure (e.g. sanatoriums, dispensaries) necessary to fight the disease. However, they were largely ineffectual - in 1913, 12 sanatorium and 40 dispensaries existed with approximately 6000 beds across the entirety of the country. During the same year in Germany, there were 1500 sanatoriums.[2] When the war struck, France was ill-prepared for the wave of soldiers who would return home from the front with tuberculosis.

By mid-1916, over 60,000 soldiers were remanded of their duty due to tuberculosis, with almost no public support on which they could rely on their return home. In response, a central committee was set up in Paris to help coordinate departmental committees to aid tuberculose soldiers. While it did make some strides in assisting soldiers, it was unable to make a significant impact due to a combination of financial constraints and political ineffectiveness. According to the Rockefeller Foundation representatives, France did not lack the scientific knowledge to reduce mortality, but rather an "efficient, cooperative, centralized organization" for the implementation of their knowledge. (Vincent, 1918). For this purpose, the Rockefeller Foundation thought they could combine their funding capacity and logistical know-how to have a positive impact on the French public health infrastructure.

1.2 The Rockfeller Foundation

In the midst of World War I, France struggled to accommodate the millions of wounded men returning home from the front. Many were not only injured but also infected with a variety of diseases, the most prominent being tuberculosis. However, with the French state mired in debt financing the war, any internal

[2] "in Prance these large committees, organized by political men, and having a long list of celebrities, political and medical, on their "Comités d'Honneur", do very little work, and do that badly."

initiative to conduct such an operation struggled to get off the ground. As the war continued and the plight of these wounded soldiers worsened, prominent figures within French society called for larger investments in health care infrastructure to better transition these soldiers back into society. On April 4, 1916, the Director of the Rockefeller Foundation War Relief Commission received a letter which expressed the wishes of the Duchesse de Richelieu as well as Edith Wharton, then an expatriate in France, petitioning the Foundation to assist France with their situation.

At the time, the Rockefeller Foundation was a global aid organization focused on improving health and nutrition in communities without the proper funding or know-how to succeed. In terms of the War, they focused their efforts on contributions to Red Cross activity, careful not to antagonize either side in their goal to treat the wounded on both sides of the battlefield. When the Foundation first received the requests to assist the public health efforts in France, the Foundation hesitated to come to their aid for a few different reasons. First, they feared any effort to give special assistance to wounded French soldiers would antagonize the Central Powers of Germany and Austria-Hungary. Second, they judged that the French public health system needed long-term, systematic investment to address their problems: "It is recognized that tuberculosis in France can be brought under control only by French agencies rooted in the life and traditions of the people and working over long periods of time." Thus, they wanted assurances from the French government that any outlay on the part of the Rockefeller Foundation would require the cooperation and support of the French authorities, and that the investment the Rockefeller made would persist after their departure (Vincent, 1917).

Although the initial request was for the Foundation to aid in combating tuberculosis in war veterans, they decided that by approaching the situation with a broader public health perspective, they could avoid the ire of the Central Powers and get strong buy-in from the French government. To determine how exactly the Foundation would provide aid, they sent two exploratory teams to France to meet with officials there. During those visits, they saw an opportunity to impact public health at large in France: "in view of war losses and appreciate attitude towards Americans, the present is an unprecedented opportunity to promote sound public health work in France," (Pearce, 1917). The Foundation took advantage of this opportunity and tailored their efforts toward both tuberculosis and public health more generally. Under the title of the *Comité pour la Prévention de la Tuberculose en France*, the Rockefeller launched its campaign in 1917.

While the Rockefeller Foundation's efforts were multifaceted, I focus on the impact of the educational arm of the campaign. I do this for two reasons: first, according to to the Foundation, it had the greatest potential to have an immediate impact on mortality across a wide swath of the population, and second, much of the success of the other parts of the campaign was due to the educational arm: "The units obtained particularly excellent results in increasing attendance at the dispensaries, stimulating interest in departmental organization, and creating cordial relations with departmental authorities," (Vincent, 1918) The other components of the Foundation's effort were the Departmental Organization arm, which worked with the French government to organize public health committees in each département and to coordinate with them to build local public health centers within their jurisdictions, and the Public Health Visiting arm, which created a traveling nurse school and funded many students to pass through the school to send them to staff many of the dispensaries being built under the direction of the Departmental Organization.

1.3 Rockefeller Educational Campaign

The objective of the educational campaign of the Foundation was to educate the populace on the general tenets of hygiene, infectious diseases, and public health. To accomplish their goal, they took an on-the-ground approach aimed at spreading their message to as much of the public of France as they could manage, within their time and budget constraints. They formed four teams over the course of the four years they were in operation, with each team going department to department to share their message. The teams were composed of representatives from the Foundation's public health team, along with at least one French nurse or public health representative. The teams traveled by car, moving from commune to commune, staying for one or more evenings depending on the commune's size and the Foundation's schedule. In each commune it visited, the teams had a routine they deployed throughout the day(s) they were stationed in the commune. During the day, they focused their efforts on the local institutions where the campaign would make the most difference. In practice, this usually meant going to local schools and factories, where large gatherings were conducive to spreading their message to the most citizens, and where infectious disease was most likely to spread. There, the teams gave lectures and handed out small booklets ("propaganda") that exhorted the virtues of hygienic behavior. In the evenings, the *mairie* (city hall) would host a nighttime soirée for the entire commune. These were typically advertised with posters throughout the commune when possible, which were hung upon their arrival that morning or the evening prior to give the populace ample time to learn of the event. The soirées were complete with films shown on state-of-the-art projectors, songs, and speakers from the Foundation team and the local governments. Throughout the event, which often lasted more than two hours, the Foundation and their partners educated the populace about hygiene and infectious disease.

The following description of this traveling exhibit appeared in the French newspaper, Le Matin, one of the four largest daily French newspapers in the lead up to World War I, on November 30th of 1918: "This tank rolls over the roads of France, bearing signboards... Behold this tank entering a city - or a village. it does not come unannounced... a delegate arrives. He pays visit to all the newspapers and to the municipal, military, prefectoral, and religious authorities... After the hall has been obtained the delegate covers the city with posters. And such posters! Barnum and Bailey would not be ashamed of them... these are accompanied by another lady, the 'demonstrator,' whose task it is to comment on the posters in the exposition room," (Vincent, 1918).

From the onset, the Foundation was aware of the potential pitfalls of a foreign entity entering a country to advise citizens about changing their behavior: "it may well be asked whether it was not presumptuous for Americans to go crusading against tuberculosis in the land of Louis Pasteur," (Vincent, 1918). They dealt with this potential roadblock by hiring French employees whenever possible, creating literature and exhibits "in the mold of the French mind" (Rose, 1917), and meeting with local figureheads to receive official approval and endorsement. While this was a potential risk, there is a large amount of qualitative evidence supporting the fact that their efforts were successful in attracting attendance to their events.

Newspaper articles, comments from public officials, and documents from the Foundation archives all attest that the educational events were well-attended. For example, some members of the Rockefeller teams kept extensive journals in which they provide this qualitative evidence: "in Romoratin, 'the theater has 500 seats and after 800 people had entered, the police department closed the door to prevent any more crowding in,'"; similarly, "at Montfort, 'we had 1250 people at our conference out of a population of 3,000,'"(Gunn, 1918). Newspaper articles told a similar story. For example, an article published on 23 March 1922 details the large crowds attending two evening soirées: "a crowd even more numerous than the first night filled the large room," (Le Courrier de l'Aude, 1922*b*). I also found evidence seeming to confirm the Foundation's belief that the educational campaign also helped spur the initiation of other complementary activities. In an article published the following week, the same newspaper published an article stating "the Prefecture held a meeting to organize an association to combat tuberculosis," (Le Courrier de l'Aude, 1922*a*). In a tribute to their efforts some years later, one publication wrote: “We have not a department, and doubtless few communes, where the name of Rockefeller is not respected and venerated as a symbol of benevolent safeguarding of human life and helpful cooperation for sanitary welfare in France," (Viborel, 1937). Thus, the evidence suggests the campaign events were well attended and had the potential to have a meaningful impact on hygienic behavior and mortality. However, the campaign was unable to visit every commune within France, and had to come up with some sort of framework of how to spread their message most effectively.

According to the archival evidence from the Rockefeller Foundation, the educational teams traveled through 65 of the 89 départements in France from 1918 to 1922. During that time, they visited over 1,400 communes that contained about 12 million of the 39 million people in France. In those communes, they gave 4,928 lectures to children in schools, 2,585 lectures to groups of adults, and over 5,000 evening *soirées* with over 2.3 million recorded attendees.

2 Data and Empirical Strategy

2.1 Data

The outcome data consists of the total mortality rate per 1,000 persons by department, gender, and 20-year age bracket for the census years of 1906, 1911, 1921, and 1926. The mortality data primarily comes from the *Mouvement de la Population*, a series of data published by the *Statistique Générale de la France*. Most of this data was transcribed from original documents from the *Archives Nationales* and the *Bibliothèque Nationale de France* in Paris. While the *Statistique Sanitaire* series does contain cause of death data, the reliability of these numbers during this time period is uncertain (Mitchell, 1990). For example, physicians were not required to report tuberculosis for many of the years before the war, and given the societal stigma surrounding the disease (among others), many scholars hesitate to use the cause of death statistics. To standardize the mortality numbers by the population, I collect population by department, gender, and 20-year age bracket from the censuses of the respective years.

For the education department data, I consider the extensive margin of whether the Rockefeller Foundation's educational campaign had ever visited the department in a previous year. This information was collected from records kept in the Rockefeller Foundation Archives. Since the final dataset corresponds to the census years, this effectively splits the treated departments into two groups - those which were visited before the 1921 Census, and those which were visited after the 1921 Census. For the former group, the 1921 data point associated with their departments is a “treated" period, while for the latter group, 1921 is still an untreated period.

One important aspect of the campaign was that it was rolled out department by department - there were very few instances where a team crossed a border into a different department from the one in which they were currently visiting. Even if that did occur, it was only for a short period of time, with the motivation that when the campaign eventually visited the different department, they could avoid traveling to that area. Thus, departments have very clear start and end dates for when the education campaign was visiting. While different teams spent different lengths of time in various departments, it's not clear from the archival documents if those differences implied the campaign was more active in those departments, or whether there were alternate reasons for the longer stays (e.g. technical difficulties with transportation, staff turnover). For this reason, I do not consider the length of time the team stayed in the department.

Departments in the northeast which were badly affected by the war and those which were not part of France prior to World War I were excluded from the analysis (Rhin-bas, Rhin-haut, Nord, Pas-de-Calais, Somme, Aisne, Ardennes, Meuse, Meurthe-et-Moselle, Moselle, Vosges, Belfort, Marne, Oise). Corse was also excluded due to the unique nature of being an island department, although the results do not change on its inclusion.

Figure 1 shows a map of the included departments in the sample with the mortality rate plotted by age group (vertically) and year (horizontally). Focusing first on 1911, a few different patterns emerge. First, infant mortality rate is extremely high, a fact that is replicated across the other countries who go on to experience the "Great Escape". However, there is significant variation in the infant mortality rate across departments, with some departments experiencing a rate more than double others. Second, mortality rates plummet and are lowest for those aged 1-19, with an increasing gradient as age increases. Within those aged 1-19 and over 60, the mortality rate is more geographically uniform, besides a hotspot in the southeast for the younger group. In contrast, there is more geographic variation in the mortality rate experience by those aged 20-39 and 40-59. The north/northwest consistently has very high mortality rates for both age groups, while the eastern departments experience rates close to the mean, followed by the southwest and central regions having the lowest rates.

Moving to 1926, the infant mortality rate across all departments declines drastically, and the penalty experienced by the north and southern regions is almost, if not completely, diminished. For those aged 1-59, the general pattern from 1911 to 1926 is more of a compression of the worse-off departments coming closer to the better-off departments. For example, the southeastern penalty for those aged 1-19 is no longer present, while the decline in other regions is much less prominent (from an already low level). For those aged 20-39 and 40-59, the north/northwest and eastern regions experience drops in mortality, while southwest and central mortality rates decline at a lower pace. Finally, the mortality rate falls across effectively all departments in a similar fashion for those above 60 years old.

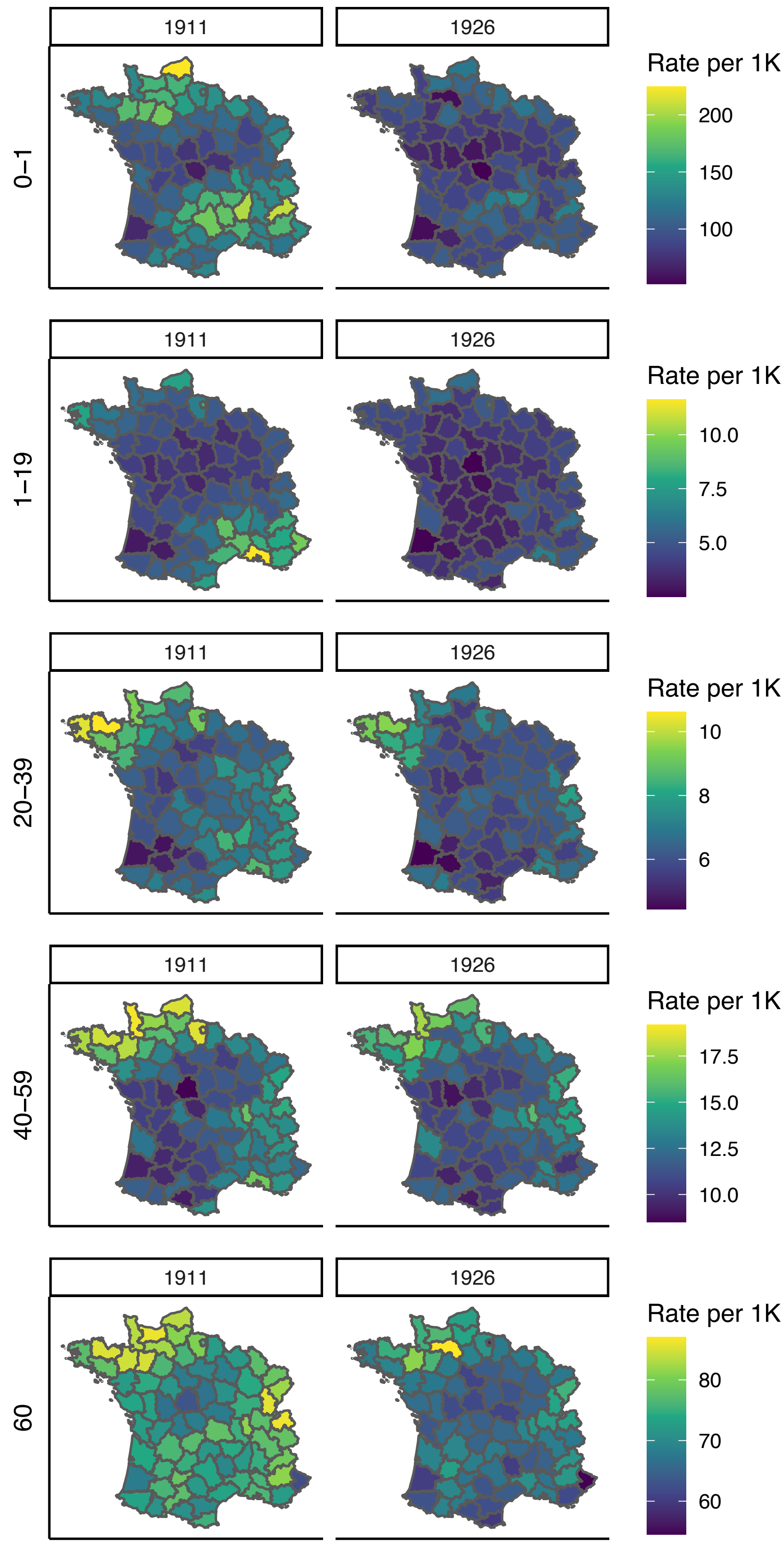
In sum, these maps show that the largest declines in mortality occurred for those aged 0-1 and those aged 60+, aligning with the well documented fact the Great Escape was largely driven by those age groups (Deaton, 2006). In addition, they show there was significant amounts of variation across departments in both the levels and changes in levels in mortality across age and year.

Figure 2 shows the path the various teams from the Rockefeller Foundation's education campaign took around the country. The general pattern of their movements was from the central/northwest region to the north and east, before hitting the south and west. Within that trend, there is some notable variation - in the northern half of France especially, there are many adjoining departments where one department was treated in 1918, while departments on its border were not treated until 1921 or 1922. For the empirical strategy, the variation in both the extensive margin (whether the department was ever treated), as well as the variation in timing of the visit, will be leveraged to identify the impact of the foundation's educational campaign.

2.2 Empirical Strategy

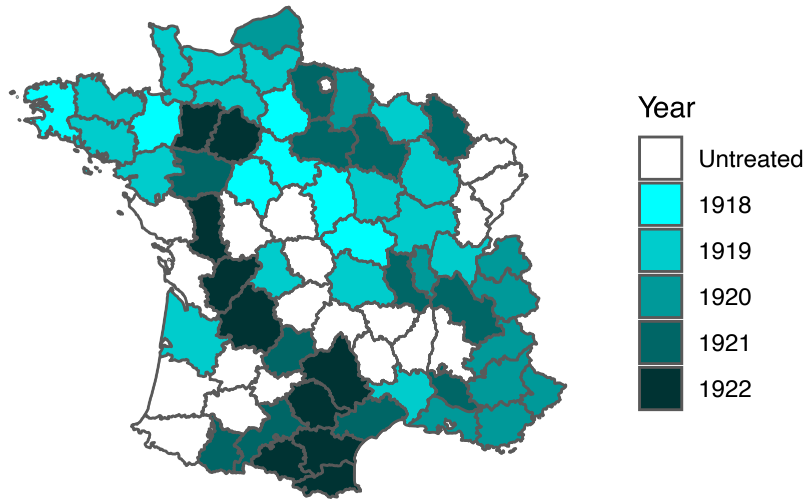
To analyze the impact of the Rockefeller Campaign, I utilize the variation in the implementation of the educational campaign over space and time to estimate its impact on mortality by age group in a multi-period difference-in-differences framework, following Callaway and Sant'Anna (2020). In particular, I estimate group-average treatment effects on the treated (ATTs), where I split the departments in the treatment into two groups - those treated before the 1921 Census, and those treated after the 1921 Census. I also show some results disaggregated into group-time ATTs.

Figure 1: Mortality by Age Group, 1911 and 1926



Notes: Each map shows the mortality rate per 1,000 by department for the different age groups (0-1, 1-19, 20-39, 40-59, 60+) for the years of 1911 and 1926. Mortality data was digitized by hand from the *Mouvement de la Population*, while the population data is from the census for the respective years. The shapefile used is from Gay (2021).

Figure 2: Rockefeller Education Campaign



Notes: This map plots the year the Rockefeller Foundation's education campaign visited each department in the sample for the analysis. The shapefile used is from Gay (2021).

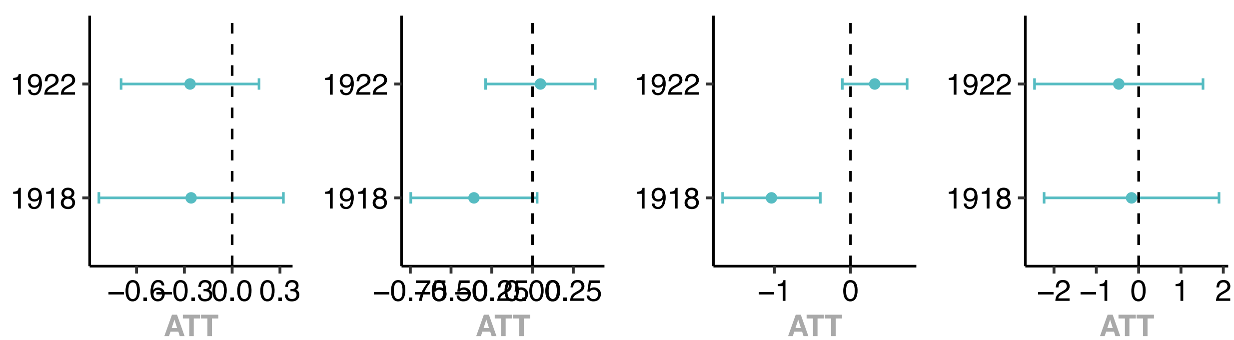
In the context of a difference-in-differences framework, one important factor is the decision behind which units are treated and which units are not treated. As described previously, not all departments were visited by the educational campaign, and some departments were chosen to be visited before others. For the first couple years of the campaign, the Rockefeller Campaign kept detailed records of the reason why a department was targeted. In Section C.1, you can see the reasons the education campaign chose to visit a particular department. There is no mention of choosing to treat or not to treat, or to prioritize treatment timing, based on the severity of the disease situation in the departments. The most common reason was because of an invitation from the prefecture, oftentimes at the behest of the Departmental Organization arm of the Rockefeller campaign. Thus, while the Foundation's activity were not driven by disease directly, if the departments who were worse off from a mortality standpoint were more likely to invite the Foundation to their department, that may invalidate the design. To address this potential concern, we show that pre-trends are never statistically different from zero. In addition, after the different teams began in their respective departments, they typically visited an adjoining department. Section C.1 shows there were many times a department was selected purely due to geographic proximity relative to where the team was in the moment.

A few other aspects of the campaign are worth mentioning in the context of how our empirical strategy will perform. First, the campaign did not "treat" all communes within the departments they visited. They went to each prefecture to establish their presence in the department, and they generally favored visiting larger communes relative to small communes, but also seemed to value geographic spread and convenience over attempting to visit as many large communes as possible. There were numerous instances where a smaller commune was visited while a nearby larger commune was omitted. For example, in Indre-et-Loire, the educational campaign stopped in La-Have-Descartes and le-Grand-Pressigny, communes of 1,697 and 1,559 residents in 1911, yet did not visit nearby Sainte-Maure, a commune with 2,527 residents. Similarly, the campaign was a purely urban effort - they did not spend time in rural areas. Since the entirety of the population within departments were not treated, if the treatment did not spill over to other communes and regions with the departments, this will attenuate the treatment effects we find toward zero.

3 Results

Figure 3 plots the results of the group ATTs, where each column shows the estimation with the mortality rate per 1,000 for age groups from 1-19, 20-39, 40-59, and 60+, left to right.[3] The results show the education campaign had a statistically negative impact on those aged 40-59 in the group of departments treated before the 1921 census (Group 1918), while the result on those aged 20-39 is negative and bordering on statistical significance. Specifically, the mortality rate per 1,000 decreased by about -0.359 for the 20-39 age group, and decreased by about -1.04 for the 40-59 age group. These are economically significant point estimates, with the mean mortality rate for the 20-39 age group in 1911 of 6.97, and that of the 40-59 age group 12.88. Conversely, the campaign does not appear to have an impact on the mortality rate for any age group for the departments treated after the 1921 census (Group 1922). I discuss potential reasons for why this is the case after first presenting results split by year and by gender.

Figure 3: Group Average Treatment Effects



Notes: Each plot shows the results estimating group ATT for the two groups (treated before or after the 1921 Census), for four different outcomes, the mortality rate per 1,000 for the different age groups (1-19, 20-39, 40-59, 60+). Each plot is one estimation with the respective mortality rate per age group, with two point estimates and confidence intervals corresponding to the two groups.

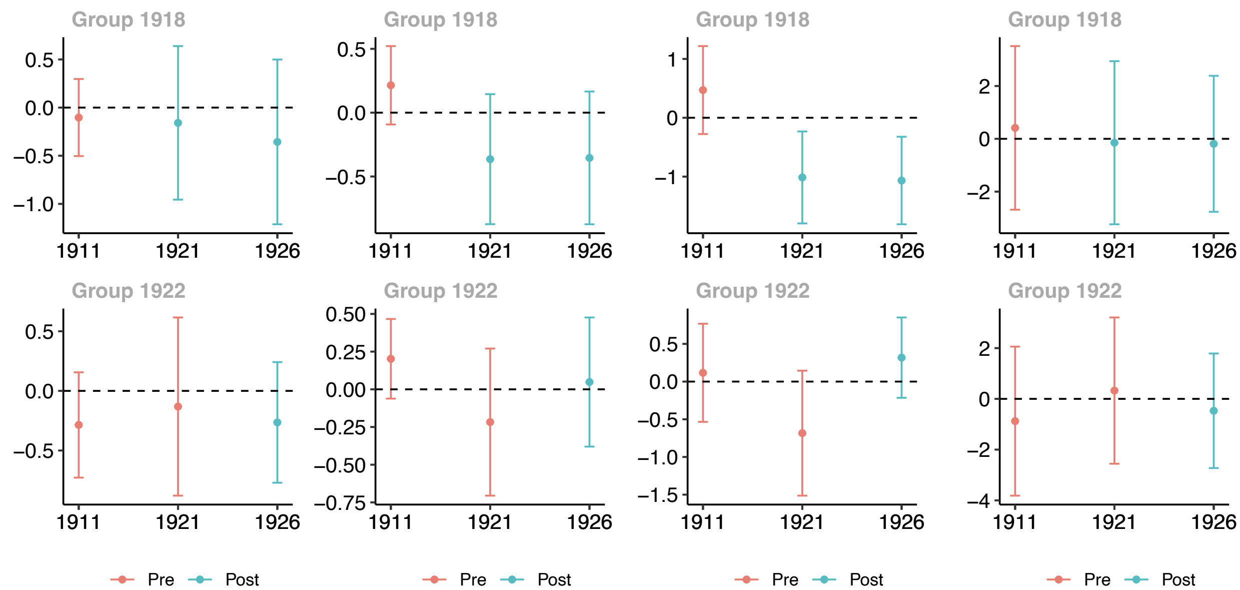
Figure 4 plots the results estimating the group-time ATTs. Each column shows the estimation with the mortality rate per 1,000 for the age ranges of 1-19, 20-39, 40-59, and 60+, left to right. The top panel of each column shows the effects for the group treated before the census (Group 1918), while the bottom panel of each column shows the effects for the group treated after the census (Group 1922). These findings align with those in Figure 3: while the post-Census treatment group does not show any differential trends in mortality, the pre-Census treatment group shows reduced mortality for the age group of 40-59, and the point estimates for the 20-39 are negative and consistent. For the former, the effect appears in the first treatment period in 1921 for both age groups, and persists to 1926. The point estimates for the 20-39 results are not individually statistically significant during the post-treatment period, but they are both close to each other and align with the estimate in Figure 3.

To investigate whether there was any differential impact of the campaign on mortality by gender, Figure 5 plots the group-specific ATT, by gender. Importantly, the point estimate for the age group of 20-39 is negative and now statistically significant for men, while the point estimate for women is attenuated and not statistically different from zero. The effect on the 40-59 age group is consistent for both genders.

Bringing the results together, there are two important aspects of these results to address in detail. First, the campaign appears to only impact middle-aged adults, with the impact skewed toward men for those

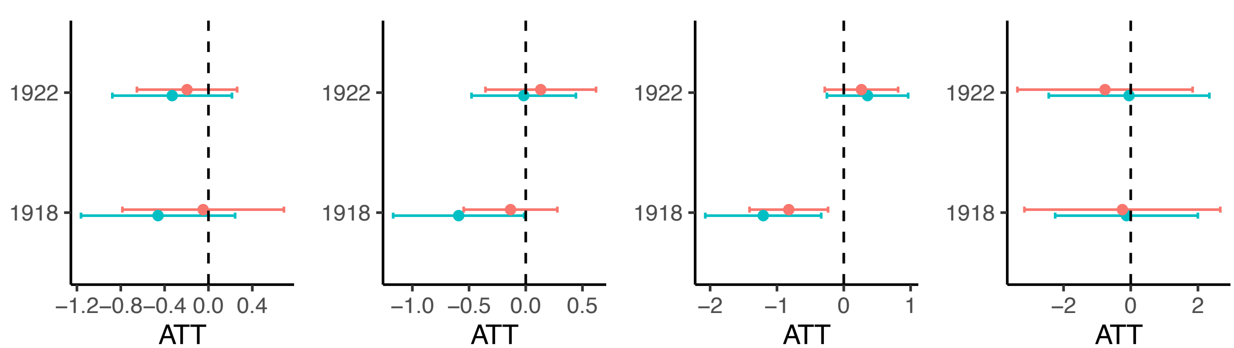
[3]The 0-1 age group results are omitted here for brevity, results are noisy and never statistically different from zero.

Figure 4: Group-Time Average Treatment Effects



Notes: Each plot shows the results estimating group-time ATTs for the two groups (treated before or after the 1921 Census), for four different outcomes, the mortality rate per 1,000 for the different age groups (1-19, 20-39, 40-59, 60+). The first row shows the results for the group treated before the 1921 census, with the age group increasing from left to right. The second row shows results for the group treated after the 1921 census. Note that each column shows results from the same estimation.

Figure 5: Group Average Treatment Effects, by Gender



Notes: Each plot shows the results estimating group ATTs for the two groups (treated before or after the 1921 Census), for four different outcomes, the mortality rate per 1,000 for the different age groups (1-19, 20-39, 40-59, 60+). Each plot shows two estimations, one for each gender (female in red, male in blue), with the respective mortality rate per age group, with two point estimates and confidence intervals corresponding to the two groups.

aged 20-39. Second, the impact only appears on the group treated during the first half of the Rockefeller's campaign - point estimates across all age groups are not statistically distinguishable from zero for the group of departments treated during the second half of Rockefeller's tenure in France.

Given the results above, the mechanism for how the educational campaign impacted mortality on middle-aged adults is difficult to decipher, but the most likely explanation seems to be through the campaign's interaction with the influenza pandemic from 1918-1919. For example, the presence of the pandemic meant hygiene and cleanliness would have been at the top of mind for many, and were highlighted in newspapers and various forms of propaganda from the government (Bar-Hen and Zylberman (2015)). Especially since the pandemic was particularly harsh for seemingly otherwise healthy middle-aged adults, this was a significant shift from previous pandemics and mortality patterns in general. If that segment of the populace was more open to hearing the campaign's message and changing their behavior due to the pandemic unfolding before them, this would explain why we see results for the pre-Census group and not the post-Census group. If the change in behavior was persistent, we would expect reduced mortality in those departments going forward. If the change in behavior was not persistent, but the temporal behavior change during the pandemic conditions resulted in a differential change in the composition of the population in the pre-Census departments relative to the post-Census and control departments (e.g. age, co-morbidities), that could also have a lasting impact on mortality experienced in later years. For example, if the educational campaign helped preserve the life of otherwise healthy middle-aged adults, this would result in a relatively healthier population in those departments moving forward.

At the same time, one might be concerned that the Spanish flu is itself driving the results seen in Figures 4 and 3 and not the educational campaign. If the flu impacted the composition of the population in departments where it had a larger impact on mortality, this could potentially explain my findings. However, for this to influence the results above, the Spanish flu would need to have had a differential impact on the pre-Census group compared to both the post-Census group and the control group. Given the ubiquity of the disease and the way in which the Foundation chose where and when to visit, it seems unlikely the Foundation happened to visit departments in a way that was correlated with pandemic severity. In terms of qualitative evidence, I found no mention of the pandemic in any documents, letters, or reports in the Rockefeller Archives. Unfortunately, it's difficult to analyze this question empirically due to a dearth of population data between the 1911 and 1921 census, and due to the fact that the number of deaths as a result of the war in each department may have influenced the number of deaths from the pandemic.

Another confounding factor that has the potential to influence my results is mortality from the war or its after-effects. We saw that the effect is only present for men for those aged 20-39, so if the war had a differential impact on treated departments, that may taint the previous results. For this to be the case, similar to above, the impact of the war would have needed to have a different effect on departments treated before the 1921 census, compared to those which were treated afterward and those which were never treated. Figure 7 plots the military death rate experienced by each department in France, showing there is a significant amount of geographic variation even within regions. To further assuage any concerns about the influence of the war, Appendix Figure 6 plots estimates of group ATTs on the male mortality rate while controlling for the military death rate in the respective department, using Gay and Boehnke (2020). The results actually show an increase in precision in the estimate on those aged 20-39 such that the effect of the campaign on mortality is more strongly significant, increasing the confidence in these findings.

4 Conclusion

The first half of the nineteenth century saw a remarkable decline in mortality across many countries, with nothing else close to it recorded in modern history. Understanding the mechanisms behind why the decline occurred is essential for countries who still experience relatively high mortality today can take action to reduce their mortality burden. While there has been ongoing debate about the relative importance of public health measures versus other social and economic factors in contributing to the mortality decline, the literature thus far still cannot explain the majority of the decline. From a public health perspective, the effects of investment in water purification and sanitation has been studied extensively, but less is known about the impact of information campaigns related to hygienic behavior. This study adds to the growing body of literature that suggests that information campaigns also likely played a significant role in improving health outcomes.

In this paper, I take advantage of a unique and unexpected public health educational campaign in France in the aftermath of World War I by the Rockefeller Foundation in which they spread messages about hygienic practices and the dangers of tuberculosis. The campaign visited 65 of the 89 departments in France, traveling to almost 1,500 communes across the country to give their presentations and providing films, posters, and pamphlets to the populace. Using a difference-in-differences framework, I show the campaign reduced mortality of middle-aged adults, with those those aged 40-59 experiencing the largest decline of about -1.04 per 1,000, and men younger aged 20-39 experienced a decline of about -0.592 per 1,000. While these are quantitatively important results, the majority of the dramatic decline seen during the Great Escape was driven by a decline in infant mortality (0-1), followed by those above the age of 60, and I cannot find evidence this campaign had an impact on those segments of the population. Overall, this research highlights the importance of public health interventions, particularly in providing information and education to the public, as a critical tool for improving health outcomes and reducing mortality rates for middle-aged adults, while still leaving room for further research into the main drivers behind the Great Escape.

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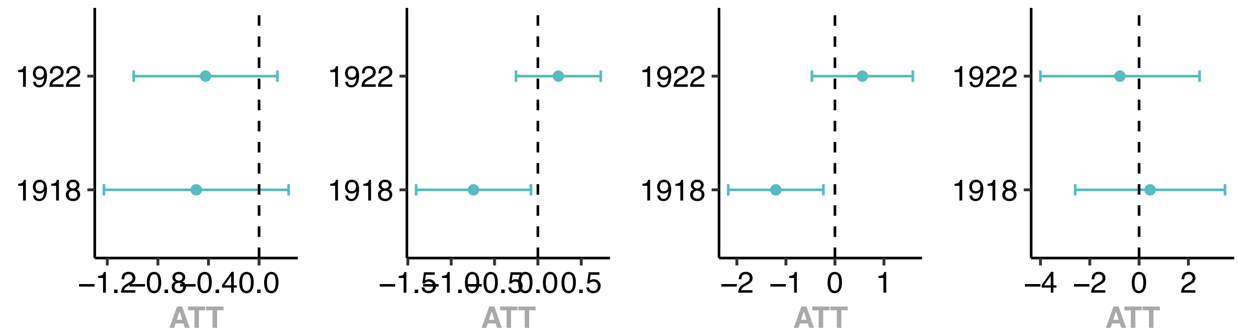
Vincent, George. 1918.

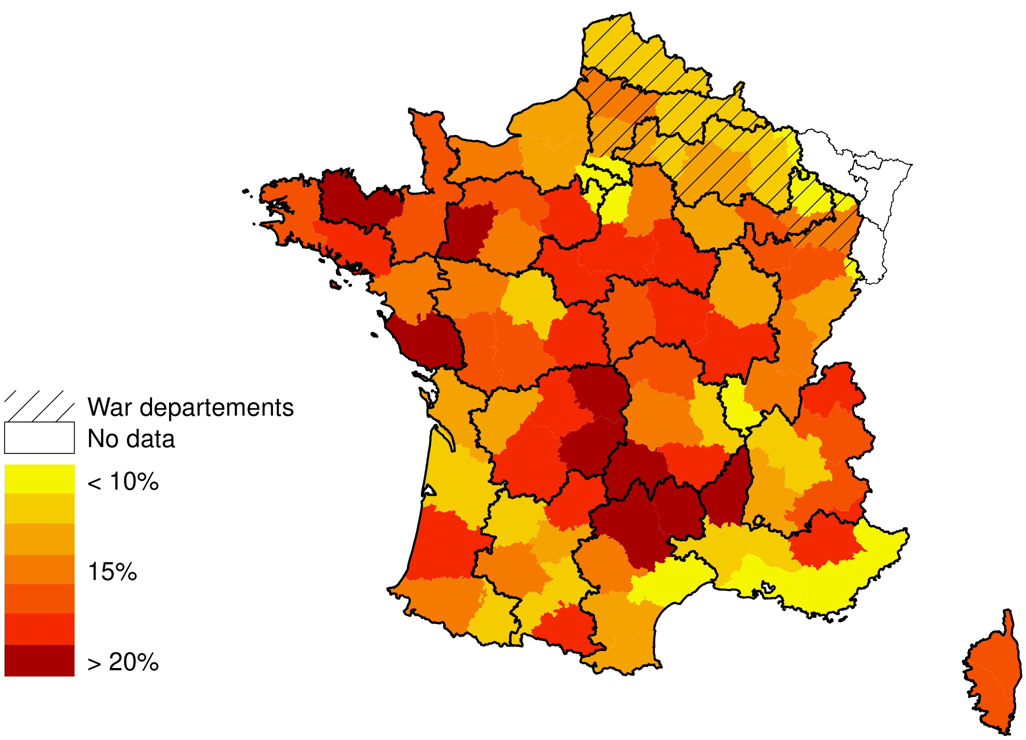
A Miscellaneous Tables and Figures

Figure 6: Group Average Treatment Effects, Male, Controlling for War Deaths



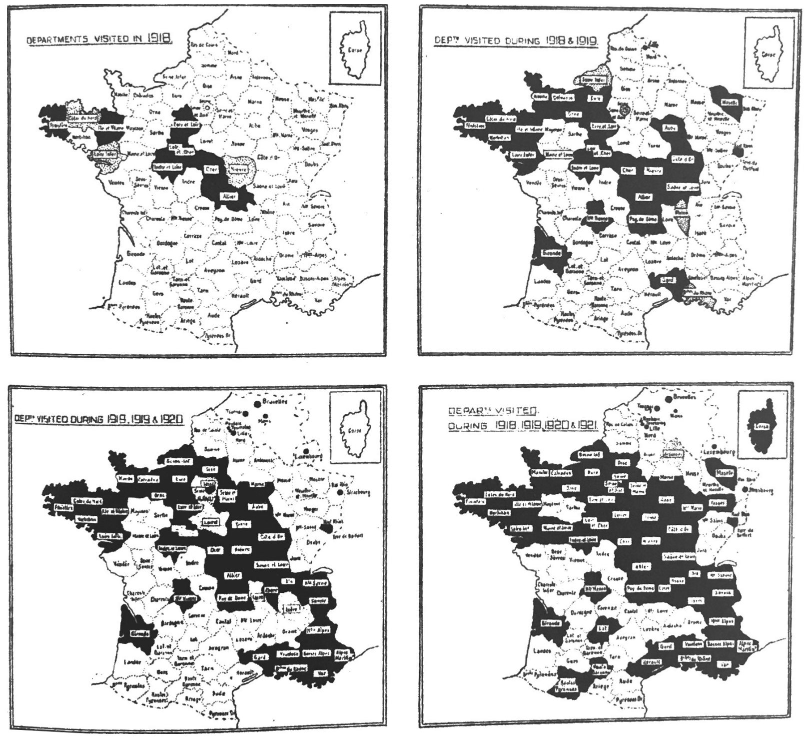
Notes: Each plot shows the results estimating group ATT for the two groups (treated before or after the 1921 Census), for four different outcomes, the male mortality rate per 1,000 for the different age groups (1-19, 20-39, 40-59, 60+). Each plot is one estimation with the respective mortality rate per age group, with two point estimates and confidence intervals corresponding to the two groups.

Figure 7: Mortality Rate from World War I



Source: Gay and Boehnke (2020)

Figure 8: Departments visited by Educational Campaign, 1918-1921



B Data Construction

B.1 Rockefeller Foundation Educational Campaign

The Rockefeller Foundation hosts an archive center where it keeps its records regarding the Rockefeller Foundation's various philanthropic efforts. To collect qualitative and quantitative data regarding the Rockefeller Foundation's campaign in France, I visited the archives on two occasions and took scans of various relevant documents. In regards to the educational campaign, the Foundation collected data on the communes it visited for about a year in France when it first arrived, after which it noted just the departments it visited.

B.2 Mortality Data Construction

Total mortality by age group and gender was collected from the *Mouvement de la Population* series published by the *Statistique Générale de la France* over the course of the study period. An example of what that looks like is shows in Figure 10. Population by age group and gender was collected from the *Résultats statistiques du recensement générale de la population* series published by the same office. Both statistics were manually transcribed into a digital format by hand.

C Miscellaneous Archival Items

C.1 Reasons for Visiting

Department	Unit	Beginning Date	End Date	# of Days	Reason
Eure-et-Loir	1	1/1/18	3/1/18	34	This is the department selected by Commission for general demonstration.
Ille-et-Vilaine	2	3/11/18	6/12/18	53	Invited by Departmental Committee and relations established by Prof. Gunn.
Loir-et-Cher	1	4/1/18	5/20/18	27	Selected in conjunction with American Red Cross work there.
Indre-et-Loire	1	5/29/18	7/21/18	27	Urged by Local Tuberculosis Committee and by the Count and Countess de Pourtalès.
Finistère	2	6/29/18	9/20/18	50	Drs Farrand and Miller established relations. Later extension Department.
Cher	1	7/25/18	8/22/18	19	Convenience of Geographical location.
Allier	1	8/4/18	10/6/18	38	Convenience of Geographical location.
Loire-Inférieure	3	9/28/18	2/8/19	132	Extension department started work and asked Educational department to go.
Côtes-du-Nord	2	10/4/18	2/10/19	97	Invited by Departmental Tuberculosis Committee.
Nièvre	1	12/1/18	2/6/19	56	Convenience of Geographical location.
Morbihan	2	2/24/19	3/27/19	27	Selected by Educational Department on account of geographical location, being next to Côtes-du-Nord.
Gironde	3	2/28/19	6/20/19	66	Invited by Local Tuberculosis Committees.
Saône-et-Loire	1	2/21/19	3/31/19	35	Asked by Extension Department and Local Committee.
Manche	4	4/5/19	5/25/19	36	Relations with Committee established by Prof. Gunn.
Aube	1	4/13/19	5/7/19	22	Invited by Departmental T.B. Committee.
Puy-de-Dôme	2	4/14/19	7/5/19	64	Invited by Departmental Inspector of Hygiene representing local Tuberculosis Committee.
Côte-d'Or	1	5/5/19	7/13/19	38	Selected by Educational Dept. on account of convenience of geographical location.
Ain	1	6/1/19	6/29/19	29	Invited by the Mayor of Bourg and urged by the Local Tuberculosis Committee.
Calvados	4	6/2/19	7/13/19	41	Invited by the Prefet.
Vienne-Haute	3	7/9/19	9/26/19	69	Relations established by Extension Department.
Nord	2	7/11/19	8/10/19	31	Urged to come by Dr. Calmette head of the Institut Pasteur of Lille and relations established by Mr. Stuart.
Orne	4	8/4/19	8/28/19	22	Invited by Prefet. Relations established by Mr Stuart.
Meuse	1	8/11/19	9/18/19	38	Urged to come by Social Insurance Department of local Government.
Moselle	1	8/11/19	9/18/19	38	Urged to come by Social Insurance Department of local Government.
Gard	2	9/1/19	11/19/19	70	Relations established by American Red Cross and invited by Local Committees. Relations established by Mr Stuart.
Eure	4	9/5/19	10/7/19	32	Selected by Educational Dept. on account of convenience of geographical location.

Rhin-Haut	1	10/2/19	11/5/19	34	Urged to come by Social Insurance Department of local Government.
Rhin-Bas	1	10/2/19	11/5/19	34	Urged to come by Social Insurance Department of local Government.
Paris-19th	3	10/9/19	2/22/20	57	Selected by Educational Department for inauguration of work in Paris.
Seine-Inférieure	4	10/19/19	2/29/20	114	Relations established by Extension Department. Selected by account of its importance.
Belfort	1	11/8/19	11/24/19	17	Relations established by Prof. Gunn.
Bouches-du-Rhone	2	11/28/19	5/3/20	114	Relations established by Prof. Gunn.
Rhone	1	12/1/19	3/30/20	95	Relations established by Prof. Gunn.
Paris 11th	3	1/19/20	4/26/20	40	
Paris 12th	3	3/15/20	5/26/20	36	
Marne	4	3/15/20	3/28/20	14	
Alpes-Maritimes	1	4/15/20	6/21/20	68	
Belgium	4	4/17/20	5/24/20	22	
Luxemburg	4	5/10/20	5/20/20	10	
Var	2	5/5/20	6/27/20	53	
Paris 17th	3	5/5/20	7/16/20	15	
Seine-et-Oise	3	6/1/20	3/3/21	209	
Seine-et-Marne	4	6/7/20	7/31/20	39	
Hautes-Alpes	1	6/25/20	7/26/20	32	
Basses-Alpes	2	7/1/20	7/31/20	31	
Savoie	2	8/30/20	9/29/20	31	
Haute-Savoie	1	8/30/20	9/30/20	31	
Oise	4	8/30/20	11/6/20	68	
Loire	2	10/11/20	2/21/21	109	
Isere	1	10/26/20	2/3/21	73	
Loiret	4	11/28/20	1/21/21	43	
Yonne	4	1/26/21	3/7/21	50	
Herault	1	2/9/21	6/9/21	124	
Vaucluse	2	2/27/21	5/7/21	46	
Maine-et-Loire	3	3/7/21	7/8/21	101	
Corse	4	5/6/21	5/31/21	26	
Marne Haute	2	5/25/21	7/1/21	38	
Pyrenees Hautes	1	6/20/21	7/23/21	34	
Vosges	2	7/7/21	11/7/21	90	
Garonne Haute	1	9/9/21	11/19/21	66	
Ardennes	2	11/11/21	1/29/22	55	
Lot	1	11/21/21	12/22/21	32	
Pyrenees Orientales	1	1/19/22	3/7/22	44	
Sarthe	2	2/6/22	4/8/22	62	
Aude	1	3/13/22	4/12/22	31	
Mayenne	2	4/24/22	6/12/22	42	
Tarn	1	4/26/22	6/12/22	48	
Ariege	1	6/16/22	7/6/22	22	
Meurthe-et-Moselle	2	6/26/22	12/6/22	109	
Aveyron	1	7/10/22	10/12/22	34	
Dordogne	1	9/1/22	10/28/22	26	
Charente	1	9/11/22	11/14/22	29	
Sevres-Deux	1	11/15/22	11/26/22	12	

Figure 9: Departments visited by Educational Campaign, 1918-1922

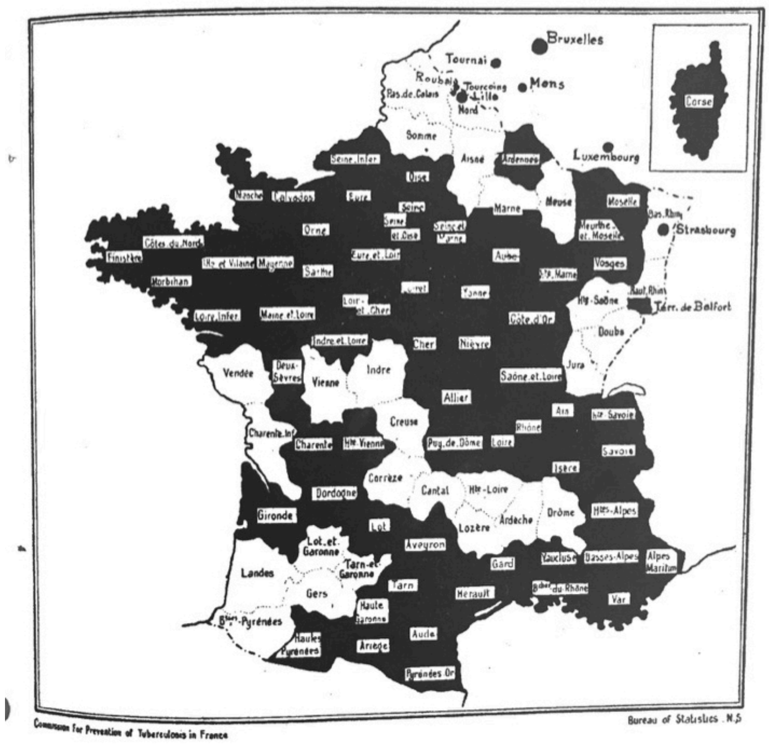


Figure 10: Example of Mouvement de la Population Scan

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DECES EN 1921.

TABLEAU XXXI. (Suite.) — *Décès*

DÉPARTEMENTS.	MOINS DE 1 AN.	DE 1 À 4 ANS.	DE 5 À 9 ANS.	DE 10 À 14 ANS.	DE 15 À 19 ANS.	DE 20 À 24 ANS.	DE 25 À 29 ANS.	DE 30 À 34 ANS.	DE 35 À 39 ANS.	DE 40 À 44 ANS.	DE 45 À 49 ANS.	DE 50 À 54 ANS.	DE 55 À 59 ANS.	DE 60 À 64 ANS.	DE 65 À 69 ANS.	DE 70 À 74 ANS.	DE 75 À 79 ANS.	DE 80 À 84 ANS.	DE 85 À 89 ANS.	DE 90 À 94 ANS.	DE 95 À 99 ANS.	DE 100 ANS ET AU-DESSUS.	ÂGE NON DÉCLARÉ.	TOTAL.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Ain	342	78	33	20	58	123	54	61	50	86	115	138	212	239	318	344	400	245	80	20	3	2	2	3.023
Aisne	636	85	46	45	74	100	96	92	107	139	160	194	249	309	296	334	303	221	71	20	″	″	3	3.580
Allier	357	49	25	22	76	97	72	63	79	102	127	145	200	263	318	399	425	344	205	63	3	1	.	3.435
Alpes (Basses-)	113	38	4	6	21	24	19	17	14	12	33	44	38	82	102	107	116	95	37	48	2	″	″	974
Alpes (Hautes-)	155	37	9	6	9	27	20	20	34	17	23	22	45	53	80	96	87	43	28	7	8	″	″	826
Al-es-Maritimes	433	109	18	26	69	102	112	105	103	131	126	153	206	244	278	261	290	180	104	21	5	″	2	3.108
Ardèche	490	96	30	41	43	62	47	44	47	69	83	120	166	203	237	318	311	276	115	32	3	″	″	2.833
Ardennes	350	45	33	23	26	76	54	48	66	79	95	117	157	200	201	220	214	157	75	15	2	.		2.253
Ariège	148	33	14	17	26	35	36	38	37	40	62	50	91	121	129	196	216	191	114	34	4	″		1.632
Aube	357	47	16	22	41	60	42	48	43	58	62	98	138	175	212	277	248	205	138	43	4	″	″	2·334
Aude	310	100	30	27	39	61	58	61	57	69	73	108	150	210	221	319	308	272	134	36	5	′	″	2.648