ABSTRACT

Title of Dissertation: EVIDENCE ON PUBLIC AND PRIVATE INVESTMENTS IN COMMUNITIES

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In this dissertation, I study how communities respond to local policy and investment changes, using evidence from public libraries and place-based policies to understand how public and private interventions impact community behavior, institutional outcomes, and local economic development.

In the second chapter, I estimate the effect of eliminating overdue fines on library usage and finances. Since 2010, a growing number of public libraries have eliminated fines for overdue materials, citing concerns about access. Using panel data of Illinois public libraries from 2007 to 2019 and a staggered difference-in-differences framework, I find that removing overdue fines increases library visits and circulation, with suggestive evidence that it is driven by libraries in lower-income communities. I find no evidence of negative financial impacts, as lost fine revenue is small relative to total operating income. These findings suggest that fine elimination can improve efficiency in library use without compromising financial sustainability.

In the third chapter, I study the effects of philanthropic grants from the Bill and Melinda Gates Foundation on public libraries, examining their impact on computer spending and access, library service availability and use, and library spending. Between 1997 and 2003, the Foundation awarded grants to over 4,500 library systems across the US to support public access to computers. Using panel data and a staggered difference-in-difference design, I find limited evidence of any impact to libraries across outcomes considered. The grants may have increased the number of public access computers, but consistent pre-trends in outcomes make exact interpretation of this and all outcomes challenging. At best, the results suggest that philanthropic investments of this variety may address infrastructure deficits in the short run but have more limited effects on long-term public service usage.

In the final chapter, I study the impact of the New Markets Tax Credit (NMTC) program, a place-based policy, on local development outcomes. Established in 2000, the NMTC program provides tax credits to incentivize private investment in economically distressed communities. Using comprehensive data on NMTC-financed projects and a fuzzy regression discontinuity design, I find limited evidence that the program meaningfully changed local economic conditions. While some outcomes show statistically significant changes in some years, these effects are not consistent over time or easily interpreted. Heterogeneity analyses very tentatively suggest that if there are effects, that they may be stronger in higher-vacancy and lower-income tracts, but the variability in estimates and the strength of the first stage warrant caution. Overall, the findings highlight the challenges of identifying local effects of place-based investments and suggest that NMTC may, in many cases, have limited or no measurable impact.

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by

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Dedication

To Cooper.

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Chapter 1: Introduction

A substantial share of public and private investment is made at the local level, often with the goal of improving efficiency, equity, or both. These investments, whether initiated by local institutions, philanthropic funders, or the federal government, are intended to shape access to resources and improve outcomes within communities. Assessing the impact of these interventions is critical to understanding their effectiveness and guiding future investment decisions. This dissertation contributes to that effort through three empirical studies: two focused on public libraries, a widespread yet often overlooked local institution, and one evaluating a federal place-based policy. Across these settings, I examine how community-level investments influence access to resources, institutional behavior, and broader social and financial outcomes.

The most fundamental service that public libraries provide is the ability to borrow books. In this sense, library books are as rivalrous public goods: only one patron can use a given item at a time. This creates the potential for overuse and inefficient allocation, which may require regulation or taxes to manage demand. To address this, libraries have traditionally employed checkout limits and overdue fines to discourage prolonged use and promote equitable access. However, in recent years many libraries have begun eliminating overdue fines, motivated in part by concerns that fines disproportionately affect low-income patrons and restrict access. While the goal is to promote more equitable use, there is also an efficiency argument, as low-income borrowers may have high marginal benefits of library use. Removing fines does carry the risk of book overuse, which could reduce availability for others and strain library resources. It may also negatively impact library budgets if fines constitute a meaningful revenue source. In Chapter Two, I study the consequences of eliminating overdue fines on library usage and finances using panel data of public libraries in Illinois.

As early as 2011, public libraries in Illinois began to eliminate overdue fines and forgive outstanding balances. Despite per-item per-day fine sizes being modest, many libraries block patrons from borrowing materials or using services when their unpaid fines exceed low thresholds. As a result, eliminating fines could have a relatively large impact on patron behavior, while having a modest effect on library income. To study the consequences of this reform, I compile a novel dataset documenting fine elimination policies across Illinois library districts and merge it with panel data from the Public Library Survey from 2007 to 2019. Using a staggered difference-in-differences design, I estimate the causal effects of fine elimination on library usage and finances. I find that library visits increase by 20 to 27 percent, registered borrowers by 20 to 23 percent, and total circulation by 25 to 40 percent within three years of adoption. Importantly, I find no evidence that eliminating overdue fines harms libraries financially. "Other income", the category that includes fine revenue, shows no change, consistent with the fact that fines typically represent a small share of library budgets. At the same time, expenditure on print materials rises modestly. Usage effects continue to grow over time, indicating a persistent behavioral shift. Together, these findings suggest that fine elimination improves the efficiency of library services without jeopardizing financial sustainability.

A central motivation for eliminating overdue fines was the concern that low-income patrons were disproportionately blocked from access to library services due to their inability to pay.

While individual-level demographic data are not available, I examine whether the policy may have had a greater impact in lower-income communities by looking at heterogeneity by library community characteristics. Using service area-level measures including poverty rates and median household income, I find that the largest increases in usage occur in communities with higher poverty and lower median incomes. Although these subgroup estimates are underpowered, they are consistent with the hypothesis that fine elimination reduces access barriers for those most likely to face them.

The third chapter shifts focus from local policy reforms to external investment in communities, examining how philanthropic funding shaped digital access in public libraries. In the late 1990s, the rise of the internet created new opportunities for information access but also introduced the "digital divide." Lower-income and rural communities were significantly less likely to have internet access, raising concerns about unequal availability to the economic and education opportunities available on the internet in an increasingly digital world. Public policy responses often looked to shared infrastructure to expand access, and public libraries were identified as a potential delivery channel. While most libraries had internet connectivity by the late 1990s, the investment in equipment necessary for patron access was slower, particularly in lower-income areas. These limitations presented a barrier to the effective use of libraries as digital access points and motivated efforts to subsidize their technological infrastructure.

To help bridge the digital divide, the Bill and Melinda Gates Foundation (BMGF) launched a major philanthropic initiative to expand community internet access through public libraries. Between the late 1990s and early 2000s, the Foundation awarded grants to thousands of libraries in high-poverty areas, funding the purchase of public-use computers, software, and staff training. While the specific goal differed from the fine-free reforms examined in the prior chapter, the broader logic remained consistent: leveraging libraries as community hubs to deliver targeted interventions in under-served communities. The BMGF grants aimed to strengthen digital infrastructure where it was weakest, expanding both access and opportunity. In Chapter Three, I examine the effects of these grants on libraries' technological resources and service provision, as well as other measures of library services and use using panel data covering libraries across the United States.

I construct a panel dataset by merging historical Gates Foundation grant records with Public Library Survey data from 1992 to 2007. To estimate the causal effects of the grants, I implement a staggered difference-in-differences design, comparing libraries that received grants to those that never did. I start by estimating a baseline two-way fixed effects (TWFE) regression; however, there are concerns about selection bias and the non-random timing of treatment. To address these concerns, I incorporate inverse probability weighting following Abadie (2005) in both a TWFE regression and in the estimator proposed by Callaway and Sant'Anna (2021), which under some assumptions accounts for treatment heterogeneity across cohorts and over time. I find sharp increases in capital spending in the year of grant receipt and a rise in the number of public-use computers, consistent with libraries using the funds to purchase computers and supporting equipment. Beyond the direct impacts on capital spending and technology, I find little evidence of broader changes in library services or usage. Estimated effects on visits, circulation, hours open, and operating expenditure are generally not robust across specifications. If anything, there may be evidence of a decline in operating expenditure and hours open; however, pre-treatment trends in several outcomes limit the ability to draw strong conclusions in any outcome. While the program appears to have increased the number of public-use computers, it is difficult to determine whether it has sustained effects on broader measures of library function.

The fourth chapter turns to a large federal initiative aimed at promoting community investment: the New Markets Tax Credit (NMTC). Unlike the previous chapters, which focused on local institutions as platforms for intervention, this chapter examines a federal place-based policy designed to encourage private investment in economically distressed communities. Created in 2000, the NMTC offers tax credits to investors who fund projects in low-income census tracts, supporting a range of activities including commercial real estate, community facilities, and small business financing. While place-based policies have a long history in the US and a large body of literature evaluating their effects, empirical evidence on the NMTC remains limited. In Chapter Four, I extend earlier work by Freedman (2012, 2015) by studying the effects of NMTC investments made between 2013 and 2019, focusing on changes in local economic outcomes within recipient communities.

To evaluate the impact of the NMTC, I construct a dataset that merges project-level data from the CDFI Fund with economic and demographic indicators from the Census. Using a fuzzy regression discontinuity design that compares census tracts just above and below the median family income ratio eligibility threshold, I estimate the causal effects of NMTC investment on local economic outcomes such as population characteristics, employment, and housing markets. Overall, I find limited and mixed evidence of the effect of NMTC investment. There is no consistent impact on population size, demographic composition, or labor market outcomes, and while some housing market indicators, such as rent, show statistically significant changes, these patterns are not consistent across years or easily interpreted. Heterogeneity analyses suggest potentially stronger effects in higher-vacancy and lower-income areas, but variability in significance and first-stage strength warrant caution. Taken together, the results underscore both the challenges of measuring local effects of place-based investment and the possibility that NMTC may, in some cases, have limited or no measurable impact.

Taken together, the chapters in this dissertation highlight both the promise and the complexity of evaluating local community investments. Across a range of programs, I apply empirical methods to isolate causal effects and assess observable outcomes. While some of the interventions studied yield measurable improvements in access or infrastructure, others show limited or ambiguous effects. These findings highlight the difficulty of identifying clear, measurable outcomes from community investments, even when using rigorous research designs and high-quality data. The effects of community-focused interventions may vary by local context or may be negligible altogether. Our ability to detect such impacts is often constrained by data limitations, contextual complexity, and methodological challenges. Continued empirical evaluation is essential to better understand what works, where, and why.

Chapter 2: Eliminating Overdue Fines: Effects on Library Operations and Patron Behavior

2.1 Introduction

Public libraries are a public good provided by local governments to community residents. They are intended to be inclusive spaces where individuals from all backgrounds can access resources for learning and growth. A defining feature of public libraries is their ability to lend out books, allowing for extended use. In this way, library books are rivalrous goods, as one person's use limits another's. Because of this, most libraries in the US have historically set check out limits and imposed overdue fines on individuals for keeping books beyond the due date. However, there is concern that overdue fines may function as a barrier to access for some individuals, leading to socially inefficient utilization (ALA, 2019).

More specifically, low-income households may be disproportionately impacted by overdue fines. These households are more likely to be unable to pay fines and, as a result, either avoid the library or face restrictions on their access to both book check-outs and other library services. Additionally, low-income individuals are likely to derive high benefits from library services, as they may otherwise lack access to books, the internet, or other educational resources.[1] Ideally, libraries would be able to charge differential fines across borrowers based on their marginal utility of library use, but this is infeasible in practice. Consequently, if fines are set too high, the equilibrium outcome may be a socially inefficient use of library resources (Babaioff et al., 2021). Despite these potential inefficiencies, the effects of overdue fine policies on library outcomes remain largely understudied.

In this paper, I examine the effects of removing overdue fine policies on library outcomes, focusing on both library usage and library finances. Due to privacy concerns, most libraries do not collect much personal data on borrowers, so I focus on observable outcomes at the library district level including annual visits, registered borrowers, total circulation, "other income" which includes fine income, and expenses on print materials. My analysis centers on public libraries within the state of Illinois, which includes some of the earliest adopters of fine-free policies. As early as 2011, libraries in the state began not only removing their overdue fine policies but also forgiving existing fine balances. Together, these changes have the potential to influence the behavior of both existing borrowers and individuals currently not engaging with the library. Importantly, most libraries retained charges for lost or unreturned items, meaning that while overdue fines were eliminated, patrons could still face financial consequences if they failed to return materials entirely.

One might initially assume that the potential effects of removing overdue fines on either borrowers or the library itself would be minimal, considering the small scale of fines both in terms of the per-book, per-day charge and their share of library budgets. Fines are around 5 to 25 cents

[1] For example, there is a strong positive correlation between income and literacy, with literacy strongly tied to adult SES and employment outcomes (Blanchard, 2023). Horrigan (2015) also finds that library users with annual incomes under $30,000 utilize the library for computers, Internet, and as a place to read, study, or watch media at higher rates compared to the overall library-user population.

per item per day and typically account for around 1 to 2 percent of a library's operating budget (Depriest, 2017). For example, overdue fines comprised just 0.7 percent of total income for the Chicago Public Library prior to their elimination, and only 0.2 percent of the San Francisco Public Library's operating budget (CPL, 2019; Innovative, 2020). Although this suggests that the overall effect on library budgets may be small, potential impacts on library utilization are much larger. Patrons can be blocked from borrowing materials or accessing services once their fines exceed just \$5 to \$10, meaning even minor infractions can trigger significant consequences—particularly for those least equipped to absorb them.

There is some limited data publicly available about who is affected by overdue fines. For instance, statistics from the Chicago Public Library indicate that cardholders living in economically disadvantaged neighborhoods were blocked from using the library due to unpaid fines at higher rates than those living in less disadvantaged neighborhoods (33 percent compared to 16.5 percent) (CPL, 2019). In San Diego, 40 percent of library cardholders living in low-income communities were barred from checking out materials because of overdue fines (Garske & Johnson, 2019). These patterns suggest that a substantial population, particularly economically disadvantaged communities, could be significantly affected by the removal of overdue fines. Overall, this evidence suggests removing overdue fines has the potential to increase library usage without a significant impact on library budgets.

To estimate the effect of removing overdue fines on library usage and finances, I collect a novel dataset on fine policies for the majority of library districts in Illinois. I combine this data with panel data from the Public Library Survey (PLS) from 2007 through 2019. Using a staggered difference-in-differences framework, I compare outcomes of fine-free adopter libraries to that of later- or never-treated libraries to estimate the causal effect of removing overdue fines on library usage and finances. In my main analysis, I estimate both an event study two-way fixed effect (TWFE) regression and the Callaway and Sant'Anna (2021) estimator. I present additional robustness checks in the Appendix using the methods proposed by Borusyak et al. (2024) and Arkhangelsky et al. (2021).Arkhangelsky et al. (2021).

In my TWFE results, I find that library usage dramatically increased in response to removing overdue fines: annual visits increased by between 18.9 to 26.5 percent, implying an average increase of 26,156 to 36,674 visits annually. Similarly, registered borrowers increased by 20.3 to 23.3 percent, suggesting an increase of 2,052 to 2,355 additional borrowers, and total circulation increased by 26.3 to 42.5 percent, implying an increase of 55,654 to 89,935 additional check-outs in each of the three years following the policy change. I also find smaller (16.5 to 19.9 percent) increases in print material expenses, although there are pre-trends that warrant caution in interpreting these effects. Finally, as expected, I find no statistically significant change in "other income," the revenue category that includes fine income. All usage effects are persistent and generally increasing through three years post-adoption, suggesting a ramp-up period in the policy's effect. My results are also robust to the alternative empirical methods.

Given concerns that overdue fines disproportionately burden low-income households, I also examine whether the effects of fine removal differ by library community socioeconomic status. While I lack data on individual patron or subpopulation behavior, I instead look at libraries serving different community types. Using Census-based measures of poverty and median income at the library district level, I split libraries by median poverty and median household income levels and run my analysis separately for each group. These results should be interpreted as purely suggestive, as the treatment sample sizes are too small for valid inference; however, I find that usage increases are particularly pronounced in districts with higher poverty rates or lower median incomes, consistent with the idea that fine removal most impacts under-resourced communities. These patterns suggest that eliminating overdue fines not only increases overall usage but may also enhance equitable access to library services.

Overall, the results suggest that removing overdue fines was effective in increasing visits to the library and check-out of materials from the library. The increase in registered borrowers also suggests that the effect is not purely driven by an intensive margin response from existing library users. Despite the increase in library utilization and loss of fine income, library budgets do not appear to have been negatively affected. This suggests that either the effect size is too small to detect or, if anything, libraries may be compensating for the loss of fine income through alternative revenue sources also included in "other income". While I am unable to do a full welfare calculation, my results as a whole suggest that the removal of overdue fines was beneficial to the districts that removed them.

The remainder of the paper is organized as follows. The next section provides an overview of overdue fine policies in the US and the existing literature. I then describe the data used and my empirical approach, followed by estimates of the effects of removing overdue fines on library usage and finances, as well as heterogeneity analysis. I conclude with a discussion of the results and implications for future policy design.

2.2 Institutional Details

2.2.1 A History of Overdue Fines

Public libraries are a public good designed to promote education and information sharing, with their ability to lend out books serving as a key feature of their services. In this way, library books are a rivalrous public good, where "consumption" of a book—i.e. a book being checked out—inhibits others from using it.[2] As with other rival public goods, library books can suffer from negative externalities created by book borrower overuse and a common solution is to tax or regulate their use. For library books, this has typically involved setting a due date and charging overdue fines for keeping books beyond that due date. These fines are generally small amounts charged on a per-day, per-item basis (5 to 25 cents), but they can quickly accumulate. Even relatively small accrued fine balances, often as little as \$5 to \$10, can lead libraries to block patrons from checking out materials or accessing services such as programs and public Wi-Fi.

In recent decades, libraries have shifted their focus toward inclusivity, aiming to serve all community members. Part of this shift in focus has led to a re-evaluation of fine policies, as fines—while originally intended to enforce accountability—may inadvertently discourage library use, particularly among low-income patrons. For these lower-resourced individuals, overdue fines can impose a financial burden that discourages returning to the library and accessing its resources. It is also reasonable to assume that low-income individuals may have high marginal benefits from library use, as they are less likely to have access to books at home and may have the most to gain from reading, educational resources, Wi-Fi access, and programs libraries provide.[3] This suggests that high overdue fines may be socially inefficient, as they may lead those who have much to gain from library resources to forgo library use altogether or face restrictions on

[2]Similar issues also exist for digital materials, see: https://www.marketplace.org/2024/06/20/librarians-e-book-library-publishing-industry/

[3]There is a substantial literature highlighting the importance of parental time investment (including reading time) and household characteristics in children's development, with evidence indicating that children from lower-resourced households tend to experience worse outcomes (Björklund & Salvanes, 2011; Blanchard, 2023; Bono et al., 2016; Cameron & Heckman, 1998; Cunha & Heckman, 2007; Cunha & Heckman, 2008; Cunha et al., 2006, 2010; Currie & Almond, 2011; Duncan & Magnuson, 2013; Ermisch & Francesconi, 2001; Ermisch et al., 2012; Feinstein, 2003; Francesconi & Heckman, 2016; Haveman & Wolfe, 1995; Hsin & Felfe, 2014; Justino et al., 2023; Keane & Wolpin, 1997; Nores & Barnett, 2010; Todd & Wolpin, 2007; Wang et al., 2023).

access.

As libraries began to assess the impact of overdue fines, they concluded that those living in the poorest neighborhoods were disproportionately affected, often being blocked from accessing library services due to unpaid fines.[4] This raised a broader question about the impact of fines: how can libraries balance deterring the overuse of books with ensuring access for under-resourced populations? Library fines were never intended to serve as a significant source of income, and for most libraries, they constitute a tiny fraction of total revenue. However, for some smaller libraries, fines contribute a much larger share of their operating budgets, making financial considerations an important factor in decisions about removing overdue fines. Additionally, libraries could face increased operating costs associated with increased demand for library services.[5]

The momentum for eliminating overdue fines picked up in the 2000s and 2010s, as a small but growing number of libraries adopted fine-free policies. Then in January 2019, the American Library Association (ALA) took a formal stance on the issue with its "Resolution on Monetary Library Fines as a Form of Social Inequity," arguing that monetary fines create barriers to the provision of essential library services (ALA, 2019). The formal position of the ALA against library fines, as the leading voice for public libraries in the US, inspired many libraries to reconsider their fine policies, producing a noticeable spike in libraries adopting fine-free policies in 2019. Although not used in this paper, the Covid-19 pandemic was also influential in libraries' adoption of fine-free policies: state-of-emergency orders closed all physical locations of libraries, leading to the temporary suspension of overdue fines. For many libraries, this temporary suspension be-

[4]See New York Public Library (2021) and Bowman (2019) for two examples.

[5]Public library funding comes primarily through the local government and is generally a fixed percent of local property values. State funding for public libraries tends to be linked to the population of the legal service area. This suggests that increased utilization without a population increase in the legal service area alone should not directly increase library funding. Increased utilization could indirectly lead to local financing changes if it pressures local governments to try and change the local tax percentage allotted to public libraries.

came the catalyst for fully adopting fine-free policies once the pandemic subsided. This growing adoption of fine-free policies is evidence that the policy has gained traction and is impacting more communities, underscoring the importance of understanding the effects of removing fines.

The implementation of fine-free policies in Illinois occurred in ways that simplify the analysis of their effects. In most cases, overdue fines were eliminated suddenly and without prior notice, as the decision was made by the library board, which is generally separate from the library's daily operations. This limited the possibility of anticipation effects from either librarians or borrowers. The policy also took effect all at once, impacting all borrowers, without any ramp-up period. This defined clean identification of pre- and post-policy adoption periods. Finally, the policy not only removed future fines charged, but also existing fines owed. If the policy only removed the threat of future fines, then we might expect it to have less of an effect on the extensive margin of library use. By removing existing fines, borrowers who are unable to pay or those avoiding the library out of shame about fines are also treated by the policy. Importantly, however, the policy does not eliminate all consequences for failing to return materials: after a certain period, items are still marked as lost, and borrowers are charged the full replacement cost. As a result, patrons continue to have an incentive to return items rather than keeping them indefinitely, limiting the potential impact on libraries' costs to replace materials.

2.2.2 Prior Literature

Although public libraries are widespread across the US, relatively little causal research has examined them. There is substantial work in the library sciences literature; however, this research is rarely able to establish a causal link. One notable exception is Gilpin et al. (2024)

which studies the causal effects of large capital spending shocks on library usage. They find that capital investment increases library visits, children's circulation, and children's attendance at library events by an average of 5–15 percent in the years after library investment. They also find that capital spending events may have small positive effects on children's reading test scores. In another recent paper, Bekkerman and Gilpin (2013) investigate the causal impact of increased high-speed Internet access on the use of public libraries. They find that increased access to high-speed Internet in a library's service area is associated with higher demand for library services. This implies that internet access is a complement to library services, rather than a substitute. Bhatt (2010) uses distance to the closest library as an instrument for library use and finds that using the public library is associated with 27 more minutes per day reading, 14 more minutes of parents spending time reading with young children, and an increase in homework completion rates for children in school.[6] The increased reading time is offset by a 59 minute decrease in time spent watching television.[7]

This paper also relates to a much broader literature on behavior deterrence, which examines how rules and disincentives are designed to reduce behaviors that have social costs. Examples from this literature include studies on court fines in the criminal justice system (Giles, 2023), speed limits and ticketing policies (Goncalves & Mello, 2025; Makowsky & Stratmann, 2009; Traxler & Libor, 2023), and penalties for late daycare pick-ups, as explored in the well-known

[6] To support the validity of their instrument they show that there does not appear to be sorting into distance from the library as measured by use of non-public libraries, having cable TV, having rules regarding children's TV viewing, using computers at school, receiving free/reduced price lunches, or being enrolled in a public school. They also look within movers and find prior reading habits are not predictive of future distance from a public library.

[7] There are also several other papers looking at descriptive outcomes in both economics and library sciences, and at other library related outcomes (Bhatt, 2010; Bookstein, 1981; DeBoer, 1992; Feldstein, 1977; Ferreira Neto & Hall, 2019; Ferreira Neto et al., 2023; Ferreira Neto, 2023; Getz, 1980; Goddard, 1971; Goldhor & Mc Crossan, 1966; Guryan et al., 2016; Hammond, 2006; Hemmeter, 2006; James, 1985; Japzon & Gong, 2005; Karger, 2021; Nencka, 2020; Neto, 2019; Newhouse & Alexander, 1972; Reimers & Waldfogel, 2022; Rodríguez-Lesmes et al., 2014; Vitaliano, 1997, 1998; Worthington, 1999).

work of Gneezy and Rustichini (2000). By focusing on the removal of a deterrent—in this case, overdue fines—this paper contributes to the discussion by examining whether such a policy shift improves access without introducing unintended financial or capacity challenges for libraries.

To the best of my knowledge, this paper is the first to evaluate the causal effects of fine-free policies in public libraries. The limited research in this area is in part due to the absence of a centralized data source on library adoption of overdue fine policies. I address this gap by constructing a novel dataset that compiles detailed information on fine policies for libraries in Illinois, as described in more detail in the Data section. This dataset enables me to leverage variation in policy changes over time, estimating effects using both the standard two-way fixed effect staggered difference-in-differences framework and recent methodological advancements.

2.3 Analysis of Fine-Free Policies

The removal of overdue fines has multiple potential consequences. Like other taxes on rivalrous public goods, overdue fines serve as both a mechanism to reduce overuse by encouraging timely returns and a small source of revenue for library districts. When fines are eliminated, there is a direct reduction in the price of checking out books, and for users with outstanding fines, a reduction in the cost to use any of the library's services. Theory predicts that this should lead to an increase in books checked out, length of check-outs, and overall library utilization, which is not all good. While increased utilization may potentially be associated with positive educational outcomes, longer check-out times could lead to longer wait times, particularly for high-demand books, which may reduce the service capacity of the library. These dynamics could shift the library to the socially inefficient equilibrium associated with not "taxing" book check-outs through overdue fines. However, the continued imposition of replacement charges for items not returned after a set period helps preserve an incentive to return materials and may mitigate some of these concerns.

Another potential impact of removing overdue fines is on libraries' budgets, as the elimination of fines directly reduces income. Libraries are primarily funded by local taxes, particularly property taxes, which form the core of their budgets. They also rely on "other" income sources, such as fines, fees, and donations, which collectively account for approximately 8 to 10 percent of total revenue. However, fines typically make up only 1–2 percent of total revenue, suggesting that the overall financial impact is likely to be modest. Still, libraries often operate within tight budgets that restrict their ability to expand services or collections, making even modest income losses meaningful. For smaller or lower-resourced libraries, where fines may constitute a slightly larger share of revenue, the effects could be more noticeable. To offset any revenue loss, library districts may respond by seeking alternative forms of income, such as applying for grants, soliciting donations, or increasing fundraising efforts. Alternatively, they may reduce spending on services, collections, or staff. This margin of response is important because if libraries are unable to compensate for the lost revenue, they may have to cut services or slow collection growth to maintain a balanced budget.

Ideally, I would observe how individual patrons—especially those from low-income households—respond to the removal of overdue fines, as these groups are most likely to be affected by financial barriers to access. Understanding heterogeneous effects across income levels would provide valuable insight into the equity implications of the policy. However, my data do not include individual-level measures of library utilization or disaggregated outcomes by socioeconomic status, as most libraries do not collect such information due to privacy concerns.[8] Instead, I focus on library-level outcomes that reflect different dimensions of overall utilization: annual visits, number of registered borrowers, and total circulation of materials.

Annual visits is a direct count or estimate of all visitors to the library district, even those who are not using the library to check out books. This is useful because it captures use not just associated with checking out books; however, because it is an estimate rather than a direct measure for some libraries, it likely has measurement error.[9] Registered borrowers is a more direct measurement of the number of people interacting with the library with the intention of checking out books. However, this variable does not capture actual use of the library, as it would include users who register but choose not to check out books and excludes people who use other library services that do not require registration. Finally, total circulation captures a more direct estimate of book use; however, it measures the commingled effects of increased check-outs if more people are utilizing the library, of increased renewals if people are keeping books for longer, and of decreased check-outs due to longer wait times. This means that I can only estimate the net effect and not how much each of these three channels contributes to the overall effect. These proxies provide an overview of overall library usage trends; if the policy disproportionately affects low-income patrons, as one might expect, the proxies may understate the full benefit of the effect for this subpopulation.

Similarly, I look at two financial outcomes that are related to the expected effects: "other income," which includes fine revenue as well as other sources like fees and donations, and the

[8]Most libraries keep minimal records on their patrons to ensure confidentiality, particularly to protect patrons' rights to access a wide range of materials without surveillance. See: https://www.theguardian.com/us-news/2016/jan/13/us-library-records-purged-data-privacy?CMP=twt_gu for more details.

[9]This error may also be non-classical if for example, larger libraries are more likely to have sophisticated tracking systems and thus more accurate counts.

amount spent on print materials. Changes in “other income” allow me to assess whether the removal of fines results in a net revenue loss. If, for example, libraries are compensating through other funding sources, the effect on “other income” could truly be zero. That is the primary drawback of this variable: I cannot break down the effect into more granular income sources such as overdue fines or donations. Similarly, changes in spending on print materials can indicate whether libraries are responding to increased demand by expanding their collections, mitigating reductions in service capacity from higher demand and longer checkout times. These measures provide indirect evidence of how fine-free policies affect library finances and their ability to serve patrons effectively.

2.3.1 Data

Analyzing the effect of removing overdue fines on library outcomes requires detailed information on public library overdue fine policies, library usage and finance outcomes, and local library district characteristics. In this section, I provide details on the data sources used for this analysis and my sample construction method.

2.3.1.1 Data on Public Library Outcomes

I use data on public libraries from the Public Library Survey (PLS). The PLS is a voluntary annual survey of all public libraries in the US and contains information at the library district-level including finances, collection and operation details, and utilization of library services (IMLS, 2021).[10] I use this survey to create my main dataset which is a panel of public libraries in Illinois

[10] Despite being a voluntary survey, the PLS has an average response rate of 97.7 percent between 2007 and 2021.

between 2000 and 2019.[11] The panel data on libraries from 2007 to 2019 form my estimation sample; I use data for the same libraries from 2000 to 2007 to construct pre-treatment characteristics. The main benefits of these data are their longitudinal nature and that they cover the near universe of public libraries. Observing the same districts over time allows me to compare within-library outcomes, essentially controlling for unobserved, time-invariant characteristics that would otherwise affect the estimation. My main outcome variables are annual measurements from the PLS and include the number of registered borrowers, total visits, total circulation, amount spent on print materials, and total "other income" (non-government income, including fine income).

2.3.1.2 Data on Public Library Overdue Fine Policies

For my analysis, I need a history of overdue fine policies during my study period; however, the PLS does not contain this information prior to 2022, and no other comprehensive data source provides it. To fill this gap, I conducted an email survey of library directors in Illinois, asking about major changes in fine and fee policies between 2000 and 2024. This allows me to track exactly when each district removed their overdue fines, defining the treatment year. My survey received responses from 52% of library districts, and I supplemented this data with information from library websites, social media posts, and archival records from the Wayback Machine (Archive.org, 2024). In total, I collected data on overdue policies for 528 out of 615 possible library districts.

In Table 2.1, I show the summary statistics for 2007–2010 averages of library district characteristics from the PLS by whether I was able to find data on their fine policies. There are statistically significant differences between the two groups and unsurprisingly, the districts where I was

[11] All years refer to fiscal rather than calendar years.

unable to find information are significantly smaller and less resourced. The average service area population for districts about which I could not locate information is 3,532 compared to 21,547 for the found group; there are about 1,800 registered borrowers for the not found group versus 9,900 for the found group; and the not found group received $155,366 in total annual income as opposed to $1,621,328 for the found group. This suggests that my analysis may under-represent smaller and less resourced library districts, potentially limiting the external validity of the findings to these types of library districts.

2.3.1.3 The American Community Survey and Census

The PLS has some limited information on characteristics of the library district itself. To construct additional library district-level characteristics, I overlay library district shapefiles from GeoMARC onto census tract shapefiles from IPUMS in ArcGIS (GeoMARC, 2024; Manson et al., 2024). The areas of intersection are used to construct a crosswalk linking census tracts to library districts.[12] I then calculate characteristics of the library district service area by applying my crosswalk to census tract-level data from the 2000 and 2010 Decennial Censuses, as well as the 2006–2010 5-year American Community Survey (ACS). These data include details on the racial and ethnic composition of census tracts, the proportions of renters versus owners, levels of educational attainment, poverty rates, and the percentage of households receiving public assistance. I use these data for balance tests and heterogeneity analysis.

[12] To construct the crosswalk, I calculate the proportion of each census tract's land area that overlaps with a library district boundary in ArcGIS. These proportions serve as weights for aggregating census tract characteristics to the library district level.

2.3.1.4 Sample Creation and Summary Statistics

Characteristics of Illinois Libraries

I begin by providing contextual details about libraries and summary statistics on the overall population of libraries to give a sense of scale in 2010, the first period before any libraries in my sample are treated. As shown in Table 2.2, public libraries in Illinois are diverse, varying widely in size and resources. Many of the variables examined are skewed, with a few large libraries driving the averages. To provide a clearer picture, I report the mean values, with the median in parentheses for comparison.

Among the libraries in my sample for 2010, they held on average 84,039 books (median 38,232) and were typically single-location entities, with an average of 1.31 library outlets per district. These libraries served populations that varied in size, with an average service area population of 22,448.35 (7,984) and a range from as few as 408 residents to as many as 2.896 million. Engagement with the library, as measured by registered borrowers, was moderate, averaging 10,073 (3,967). However, library usage was high. Average total circulation was 234,461 items annually (57,410) or 9.77 (8.27) items checked out per person living in the service area (per capita). Similarly, the mean number of annual visits was 158,414 (51,272), or 7.02 (6.09) annual visits per capita. Illinois libraries relied heavily on local government funding, which accounted for most of their income. The average total income for libraries in 2010 was $1,727,094 ($412,480). Local government contributions alone averaged $1,557,119 ($319,402). "Other income," which included non-government revenue sources averaged $95,735 ($33,209). State and federal funding by contrast were minor contributors, averaging $68,296 ($15,108) and $5,718 ($0), respectively. On a per capita basis, average total income was $68.44 per person living in the service area.

From my survey of libraries, I know that fines were fairly ubiquitous in 2010. Only 11 library districts, or 1.76% of libraries in Illinois, removed fines prior to 2010. Most overdue fines ranged from $0.05 to $0.15 per item per day, although for DVDs, Blu-rays, and other specialty items, fines could be as high as $2 to $10 per day. Although small, fines can add up quickly, particularly for high-volume patrons, such as parents checking out many children's books at once. Most libraries cap fines owed at the cost of the item and limit library access once a patron reaches between $5 and $20 in total fines. Data on fines collected do not exist for 2010, but according to the Library Journal's 2017 survey of fines and fees, the median monthly fee income for libraries collecting fees in 2017 across the US was $500, or $6,000 per year (Gerber, 2022). This suggests that, in general, library fines are a small contribution to total library income.

Viewed together, these data show there was substantial variation across libraries in Illinois in 2010. While the number of registered borrowers falls far short of the population eligible to utilize the library, utilization—as measured by visits and circulation—shows that libraries are highly utilized by the registered borrower population. These data also show that there was substantial variation in resources available to libraries.

Sample Creation

Although PLS data exists through 2022, I end my sample period at 2019 to avoid the confounding effects of the Covid-19 pandemic. I further limit my panel to exclude districts that opened or closed between 2007 and 2019, to maintain a balanced panel. Additionally, libraries that opened or closed in this period may bias my estimates if there were confounding factors driving library outcomes such as substantial changes in local economic conditions, poor handling of library finances, or other events exogenous to library fines. This results in the removal of 16 library districts that opened or closed. I also remove 4 districts associated with other entities such as schools or museums, as they do not reflect the average experience of a public library. I further drop 49 districts missing multiple observations of key variables.[13] This leaves 566 potential public library districts in the sample. I then exclude districts without overdue fine policy data (56 remaining districts after prior exclusions) and those that adopt fine-free policies prior to 2007 (11 districts). The final estimation sample contains information on 499 library districts or 6,487 observations across 13 years.[14]

Table 2.3 contains the average PLS characteristics for library districts that were dropped from the sample compared with those remaining in the sample. The table reveals substantial differences between the two groups. On average, the dropped districts are considerably smaller, serving fewer people (4,926 vs 22,270) with fewer employees (3.7 vs 16.5) and fewer branches. They also operate with fewer total annual hours and receive significantly less funding from federal, state, and local governments. Financially, dropped districts bring in less total income ($335,612 vs $1.66M) and have much lower capital expenditure ($20,733 vs $212,912), collection expenditure, and operating costs. In terms of library services, the remaining districts demonstrate significantly greater utilization and capacity. They experience substantially higher total visits (151,917 vs 31,125) and have more registered borrowers (10,120 vs 2,803). Their collections are larger, holding more books (83,114 vs 26,790) and significantly more e-books (1,140 vs 165). The remaining districts also offer greater technology access, with nearly three times as

[13]For cases where a single data point is missing, I interpolate the value using the data from the two surrounding years. This maximizes the number of available treatment units for analysis and should have a minimal effect on the estimates.

[14]I include the Chicago Public library in my sample to maximize the number of treated units, despite it being a large outlier in nearly every respect. In Appendix 2.A.1 I show that my results are robust to its exclusion.

many public-use computers (19.9 vs 6.8). The dropped districts are also more likely to be rural, with 82.8% classified as rural compared to 57.5% of the remaining districts.

The differences between the two groups are highly statistically significant, indicating that there are systemic differences between the two groups. Overall, this suggests that the dropped districts are smaller, less resourced, and more rural, while the remaining districts are larger, better funded, and more heavily utilized. This indicates that the remaining districts may not be representative of the broader sample of libraries in Illinois, and that some caution should be used in generalizing the findings of this paper to other districts.

Distribution of Treatment

Figure 2.1 illustrates the distribution of treatment years for library districts that eliminated fines in my sample period. The analysis includes 67 library districts that adopted fine-free policies between 2011 and 2019, along with 432 untreated districts that had not implemented the policy by the end of the sample period.[15] Among the treated districts, the majority (48 out of 67) adopted the policy prior to the American Library Association's (ALA) formal statement on overdue fines in 2019. These districts are classified as early adopters. Summary statistics are calculated separately for late adopters—libraries that implemented the policy after January 2019—since their decisions may have been influenced by the ALA's statement. This distinction could be important, as late adopters may differ fundamentally from early adopters, who acted independently of this external guidance. In my main analysis, I include both early and late adopters in the treated group and show in Appendix 2.A.2 that my results are robust to excluding those that adopt the policy

[15] 11 districts that I collected fine data on adopted fine-free policies prior to 2011. 3 went fine-free in the 1970's, 4 went fine-free between 1999 and 2002, and 4 went fine-free between 2007 and 2010. I exclude these libraries for two reasons: the first is that some key variables are not available prior to 2007, so to keep enough pre-period data I cutoff treatment at 2011. The second is that the libraries that adopted fine-free policies during and shortly after the Great Recession may be unique, as most libraries were struggling financially at the time.

after 2016.

Figure 2.2 maps the locations of library districts across the state categorized by early adopters, late adopters, and untreated districts. The maps highlight the geographic dispersion of treatment status. If treated districts were heavily clustered in a specific area, it could suggest the presence of external factors driving policy adoption in that region, potentially influencing library district outcomes and introducing bias. While these maps are not conclusive, they indicate an even distribution of treatment statuses, reducing concerns about geographic clustering as a confounding factor.

Characteristics of Illinois Libraries by Treatment Status

Next, I present summary statistics by treatment groups to assess balance. In Table 2.4, I present mean values for the pre-adoption period, separately by early treated libraries (those that adopted fine-free policies before the ALA's statement), late treated libraries (those that adopted fine-free policies after the ALA's statement, but before the Covid-19 government shutdown), and the Covid treated districts that adopted the policy following the Illinois government shutdown in the Covid-19 pandemic and who are considered never treated in my sample. The first panel shows library district averages from the PLS data for 2007 through 2010. The second panel shows average characteristics of the library district's legal service area using 2010 Census data, while the third panel uses 2006 to 2010 5-year ACS data for variables not available in the 2010 Census.

The summary statistics suggest that early treated and Covid treated districts are not substantially different from each other as measured by library characteristics or legal service area characteristics. There are more substantial differences between the late treated districts and both the early treated and the Covid treated districts. Late treated compared to early or Covid treated districts are more educated (29.7 percent with a high school diploma or less compared to 41.3 and 39.3 percent), and have fewer individuals living in poverty (poverty rate is 7.1 percent compared to 8.97 and 10.68 percent, and 4.98 percent of households have public assistance compared with 7.86 and 8.15 percent). When compared with early treated districts, late treated districts are also less rural (16.5 percent compared to 46.8 percent) and compared with Covid treated they spend a smaller share of their operating expenses on print materials (8.35 percent vs 10.23 percent).

Table 2.5 displays the average changes in outcomes between 2000 and 2010 by library district. These results allow me not only to assess whether the library districts are similar in average pre-treatment levels, but also in pre-treatment trends. The first panel of the table displays changes in outcomes between the 2000 and 2010 PLS data, the second panel shows changes between the 2000 and 2010 Census data, and the third panel has changes between the 2000 Census and the 2006–2010 five-year estimates from the ACS. There are no substantial differences in library characteristics or service area trends across the three groups. The only statistically significant differences between the groups are that early treated and Covid treated libraries had a larger increase in the share of households with public assistance compared with late treated (5.99pp for early, 3.21pp for Covid, and 6.09pp for late), and a larger reduction in the population with less than a HS diploma (-7.14pp for early, -4.84pp for Covid, and -7.28pp for late). Covid treated library districts also experienced a larger increase in poverty rates in comparison to late treated libraries (2.73pp vs 1.27pp). Overall, this suggests that the different treatment groups are similar, although late treated districts are slightly better off than the early or Covid treated districts. In Appendix 2.A.2 I re-run my analysis on a balanced panel limited to districts treated between 2011 and 2016, excluding both late-treated districts and a subset of later-treated early districts, and find that the results are robust.

2.3.2 Empirical Approach

To investigate whether the removal of overdue fines in public libraries led to changes in library utilization or finances, I take advantage of the staggered adoption of fine-free policies across library districts. Using a staggered difference-in-differences framework, I compare changes in outcomes between early adopters of fine-free policies and later adopters or libraries that did not adopt during my sample period. I focus on public library districts in Illinois, which has a large population of library districts, includes some of the earliest adopters of fine-free policies, and offers rich variation in policy timing. Limiting the sample to libraries that were continuously open between 2007 and 2019, I estimate the causal effect of removing overdue fines by comparing outcomes across early adopters, late adopters, and "never" adopters—defined as libraries that implemented fine-free policies in 2020 in response to the Covid-19 shutdown.[16]

2.3.2.1 Two-Way Fixed Effects Estimation

I first run an event study two-way fixed effects (TWFE) regression for estimating dynamic effects of removing overdue fines over time, as the effects of the policy may take time to ramp up. I include data from 2007 through 2019 and only report the coefficients for four years before and three years after adoption of the policy, as the treatment group size beyond outside this range is too small for reasonable inference. I estimate the following equation at the library district and year level:

[16] As discussed earlier in the paper, the near universe of public libraries in Illinois adopted fine-free policies during the Illinois government shutdown due to the Covid-19 pandemic in 2020. Because of this and other confounding factors related to the pandemic, I restrict my sample to the 2007–2019 period, effectively treating libraries that did not adopt prior to 2020 as "never adopters" rather than not-yet-treated.

$$Y_{i,t} = \sum_{l \neq -1} \beta_l T^l_{i,t} + \mu_i + \phi_t + \epsilon_{i,t} \tag{2.1}$$

where l is the number of periods since treatment, $T^l_{i,t}$ is a dummy variable that takes a value of 1 if library district i in period t was treated l years ago, μ_i are the library district fixed effects and ϕ_t are the year fixed effects. Standard errors are clustered at the library district level and corrected for multiple inference using the Benjamini and Hochberg (1995) method. The coefficients β_l capture the average effect of removing overdue fines on the outcome of interest l years since treatment. As mentioned previously, the main outcomes $Y_{i,t}$ considered are annual visits, registered borrowers, total circulation, "other income" (which includes fines, fees, and donations), and print material expenditures. While I am unable to directly observe the exact policy effects of interest or the effects on subpopulations such as low-income households, the outcomes studied are reasonable proxies for the outcomes of interest.

For this approach to be informative about the casual effect of going fine-free on the measured outcomes, it requires several identification assumptions. The first is that of no anticipation, or more formally:

$$\mathbb{Y}_{i,l}(0) = Y_{i,l}(1) \text{ for all } i \text{ with } D_i = 1 \text{ and } l < 0 \tag{2.2}$$

where $Y_{i,l}(.)$ are the potential outcomes under treatment (1) and no treatment (0) relative to the period of adoption (l = 0) and D_i is an indicator for whether the district is ever treated. Given my setting, this is a reasonable assumption to make. Library staff, while influential in shaping library usage outcomes, have limited say in the timing or specifics of fine-free policy decisions as the library board ultimately decides whether and when they are implemented. Additionally, library users, the primary drivers of the outcomes I measure, are even less likely to have influence over or early knowledge of the policy choice. From my discussions with librarians and reading library board minutes, I also know that libraries tended to implement fine-free policies almost immediately after board approval, leaving minimal opportunity for anticipation effects between the policy decision and its adoption. Finally, my analysis is conducted at the annual level, so any potential anticipation effect would have to occur well in advance of the policy's implementation.

The second assumption is that of parallel trends: that in the absence of treatment the average outcome for treated units would have trended in parallel to the untreated units. While not directly observable, there is some suggestive evidence that this assumption holds. First, the summary statistics in Tables 2.4 and 2.5 suggest that the two groups are similar and as such may trend similarly. Additionally, I find no evidence of pre-trends in outcomes, which supports both the no anticipation and parallel trends assumptions.

2.3.2.2 Recent Developments in Staggered DID

There is a recent and robust literature highlighting problems with the event study TWFE estimator of staggered treatment effects.[17] In particular, TWFE allows for "bad" comparisons of already treated units to later-treated units, resulting in bias when there is staggered adoption and treatment effect heterogeneity. To address these concerns, I implement the Callaway and Sant'Anna (2021) estimator as a comparison to the TWFE results. This is justified as in my setting I cannot rule out heterogeneous treatment effects by group or over time. Although there are many recent estimators proposed, I utilize Callaway and Sant'Anna (2021) as their estimator

[17] There have been several papers to note the deficiencies in the TWFE estimator with staggered adoption. See Roth et al. (2023) for an overview, and the following for a more in-depth look at some of the various proposed alternative methods: Athey and Imbens (2022), Callaway and Sant'Anna (2021), Goodman-Bacon (2021), and Sun and Abraham (2021)

relies on a looser assumption of parallel trends that is more plausible in my setting. For this estimator, I estimate the following:

$$\widehat{ATT}(g,t) = \frac{1}{N_g}\sum_{i:G_i=g}[Y_{i,t} - Y_{i,g-1}] - \frac{1}{N_{\mathbb{G}_{comp}}}\sum_{i:G_i\in\mathbb{G}_{comp}}[Y_{i,t} - Y_{i,g-1}] \tag{2.3}$$

$$ATT_l = \sum_g w_g ATT(g, g+l) \tag{2.4}$$

Equation 2.3 estimates the group-time average treatment effects, where g represents the group of library districts treated in year g and t represents the number of years since year g. In this framework, $Y_{i,t}$ represents the usage and financial outcomes for library district i in period t. N_g is the number of treated library districts in cohort g and $N_{\mathbb{G}_{comp}}$ is the number of library districts in the relevant control group $\mathbb{G}_{comp}$. My main analysis will use not-yet-treated library districts as controls, with Appendix 2.A.3 showing my results are robust to using never treated districts. Equation 2.4 takes the group-time average treatment effects and computes dynamic, time-since-treatment effects. The group-time ATT's are stacked such that t = 0 is the period before adoption for all cohorts and then averaged using treated group cohort sizes as weights. ATT_l is the weighted average of the $ATT(g,t)$ l periods after adoption across adoption cohorts. For all results I report bootstrapped standard errors clustered at the library district level, which are robust to multiple hypothesis testing.

For this approach to identify the causal effect of fine-free policies on library outcomes, I need to make three major assumptions. The first assumption is that treatment is an absorbing state, such that libraries do not revert back to having overdue fines after adopting the fine-free policy. While in theory it is possible for a library to reinstate fines, in practice, I do not observe any cases of this behavior. The second assumption is that there is no anticipation of the policy, same as for the TWFE estimator. The last assumption is parallel trends in post-treatment, similar to the TWFE. In Appendix 2.A.4 I present additional robustness checks using other estimators including the Borusyak et al. (2024) estimator, which also addresses heterogeneity in treatment timing and avoids negative weighting issues, but through a different methodology. I also run the Synthetic Difference-in-Differences (SDID) estimator, which combines matching and DID to improve pre-treatment balance and reduce reliance on parallel trends (Arkhangelsky et al., 2021). In general, my results are robust to these alternative estimators.

2.4 Results

2.4.1 TWFE Analysis

I begin my analysis by looking at measures of library utilization. Figure 2.3 presents estimation results from the TWFE specification of equation 2.4 for the library usage outcomes: log total visits, log registered borrowers, and log total circulation. Removing overdue fines leads to statistically significant increases in library utilization, with positive effects across all three measures. I estimate that adoption of a fine-free policy causes a 3.5 percent increase in visits in the year of implementation, growing to approximately 26 percent by two to three years post-adoption. Similarly, total circulation increases by 13 percent in the first year and by 43 percent after three years, while the number of registered borrowers rises by 8 percent in the first year and 23 percent by year three.

These dynamic time effects across all measures suggest a ramp-up in the effect of the policy and a gradual shift towards a new equilibrium. In particular, if the increase in visits and circulation is driven in part by people who were not actively using the library at the time of the policy change, it is reasonable to expect that it would take longer for them to learn about the new fine-free policy, as they are less likely to be directly engaged with library communications. The results also cannot rule out an increase in borrowing time, as total circulation captures the joint effect of both new check-outs and renewals. However, the increase in registered borrowers suggests that the rise in visits and circulation is not solely an intensive margin effect among existing borrowers but also reflects a broader expansion in the library's user base.

Next, I assess the effect of removing overdue fines on library finances, as this policy directly eliminates a source of library income. Recall, however, that fine income in general makes up a small fraction of most libraries' budgets, so the expected magnitude of the effect on library income is small. However, if demand increased as the utilization results suggest, we may expect larger effects on print material spending, as libraries may increase their collection size to meet demand. Figure 2.4 presents my estimation results for the two main library finance outcomes: log amount spent on print materials, a measure of expenditures, and log "other income", a measure of library income. Here "other income" encompasses fines and fees income, as well as other ad hoc sources such as donations. I find an increasing trend in print material expenses, suggesting that by three years after adoption, treated libraries spend 20 percent more on print materials when compared to untreated libraries. I caution investing too much in this conclusion, as the pre-treatment analysis shows a pre-trend. For log "other income", I find no statistically significant effect of fine-free policies.

These results suggest that libraries may be responding to the increased demand shown in the utilization results by increasing spending on print materials. However, the existing pre-trend suggests that print material expenses at treated libraries were already trending upwards compared with untreated libraries. This could reflect selection into treatment because of differences in strategic priorities for the library during this period of change for all libraries. Importantly, I find no evidence of concurrent policy changes related to library spending that could confound the findings. The results on "other income" suggest that although libraries are losing a source of income, it is either small or potentially made up through other sources such as donations. Overall, this suggests the fine-free policy increased utilization without significantly reducing income for treated library districts.

2.4.2 CSA Analysis

Figures 2.5 and 2.6 show the results when estimated using the Callaway and Sant'Anna (2021) estimator. The Callaway and Sant'Anna results are all the same sign and of similar magnitude, if not slightly larger, than the TWFE results. I find annual visits increase by between 20 and 31 percent in the years following policy adoption, while the number of registered borrowers increased by between 8 and 29 percent and total circulation increased by between 13 and 44 percent. Similarly, I find an increase of between 9 and 18 percent in print material expenses, but with a linear pre-trend. For "other income", as with the TWFE estimator, I find no statistically significant and inconsistent signed effects in the years following adoption. Overall, this suggests the results from the TWFE regression are robust to this alternative estimation strategy, with only slight differences in magnitudes of treatment and statistical significance. Table 2.6 also contains the numeric values of the coefficients from both the TWFE and Callaway and Sant'Anna estimations for comparison. In Appendix 2.A.4 I also show that my results are robust to alternative estimators including Borusyak et al. (2024) and Synthetic Difference-in-Differences (SDID)

(Arkhangelsky et al., 2021).

2.4.3 Heterogeneity Analysis

As mentioned previously, library fines are likely to disproportionately impact low-income and under-resourced households. These households are not only the most likely to struggle with paying fines but also stand to gain the most from accessing library services. However, I do not have direct measures of library utilization by household income or socioeconomic status, as such data are not collected. Instead of examining within-district differences based on household characteristics, I analyze variation across districts by service area population characteristics. Specifically, I divide the full sample into subpopulations based on 2010 Census values of poverty rates and median household income at the service area level. Districts with values of these characteristics above the median are grouped together, while those below the median form a separate group.

One potential concern with this heterogeneity analysis is the possibility of sorting in treatment timing—that is, if districts with higher or lower poverty or income levels systematically adopted fine removal earlier or later, comparisons across groups could be confounded. To explore this, Figure 2.7 shows the distribution of treatment timing by group. There is slightly more density in above-median districts by median household income in later years and more density in below-median poverty rate districts in later years. To more formally assess whether the timing of fine removal is systematically related to these service area characteristics, I estimate the following regression:

$$\mathrm{M}_i = \alpha + \beta \text{ Treatment Year}_i + \varepsilon_i \tag{2.5}$$

Where M_i is either an indicator equal to 1 if library district i has a poverty rate above the sample median, or a median household income above the sample median. Table 2.7 presents the results of equation 2.5. For both specifications, the estimated relationship between treatment year and the above-median indicator is statistically insignificant. In the poverty model, the coefficient on treatment year is 0.031 (standard error = 0.054, p = 0.566), while in the median household income model, the coefficient is 0.046 (standard error = 0.054, p = 0.394). These results indicate that treatment timing is not systematically related to whether a district is above or below the median poverty rate or median household income level. This helps alleviate concerns about potential sorting and supports the validity of the heterogeneity design based on these service area characteristics.

To assess the effects of removing overdue fines across these subpopulations, I estimate the same TWFE regression equations for each subpopulation separately. However, even with the full sample, my analysis is limited by a small number of observations. This issue becomes more pronounced when running regressions on subpopulations. As a result, I interpret these findings as suggestive evidence of potential heterogeneity in the effects of removing fines, rather than definitive conclusions.

Figures 2.8 and 2.9 summarize the results of the analysis by poverty rate groups, dividing districts based on whether their 2010 Census poverty rates were above or below the median.[18] Among districts with above-median poverty rates, the removal of overdue fines is associated with

[18] There are 31 districts treated in 2019 or earlier and 218 control districts below the median, and 25 treated and 224 control districts above the median poverty rate.

significant increases in library utilization. By three years post-adoption, annual visits increase by 45 percent and registered borrowers increase by 44 percent. In contrast, districts with below-median poverty rates show smaller and statistically insignificant changes in these outcomes. For total circulation, both poverty rate groups experience statistically significant increases, but the magnitude of the effect is larger for the above-median group, with a 67 percent increase by three years post-adoption compared to 18 percent for the below-median group. For financial outcomes, the analysis shows no statistically significant effects on "other income" for either poverty rate group. However, there is evidence of increased spending on print materials in districts with above-median poverty rates, with expenses rising by 39 percent by three years post-adoption. Districts with below-median poverty rates, on the other hand, show no significant changes in spending on print materials.

Figures 2.10 and 2.11 present the results by median household income, highlighting a similar pattern to the poverty rate analysis.[19] For library utilization, the effects are driven entirely by districts with below-median household income. By three years post-adoption, annual visits increase by 46 percent for the below-median group, while no statistically significant effect is observed for the above-median group. Registered borrowers follow a similar trend, with a 46 percent increase in the below-median group and no significant effect on the above-median group. Total circulation increases significantly for both groups, but the effect is larger for the below-median group (61 percent compared to 23 percent for the above-median group). For financial outcomes, there are no significant effects on "other income" for districts below the median, but districts above the median show a large and statistically significant negative effect, accompanied

[19] There are 27 districts treated in 2019 or earlier and 222 control districts below the median, and 29 treated and 220 control districts above the median district level of the median household income.

by a strong pre-trend. Spending on print materials overall does not have statistically significant effects for either group or any meaningful difference in levels of estimated effects.

While the sample sizes in this analysis are too small to draw definitive conclusions, the results suggest that the effects of removing overdue fines may be larger in districts that are more under-resourced, as evidenced by larger estimated effects in districts with higher poverty rates and lower median household incomes. This supports the idea that low-income households are particularly responsive to the policy change. In districts with higher poverty rates or lower incomes, the increases in visits and registered borrowers align with the hypothesis that more patrons, previously unable to pay fines, engage with the library following the policy change. Additionally, total circulation increases in all districts, reflecting a broader utilization of library resources not limited to previously blocked patrons but also including existing borrowers who respond by increasing their usage. The increased spending on print materials appears to be associated with higher library engagement, as evidenced by increased visits and registered borrowers, rather than simply supporting greater circulation. Furthermore, the lack of significant effects on "other income" suggests that the policy change could improve access and utilization without creating financial strain on library budgets across different types of communities. These patterns provide suggestive evidence that the removal of fines has the potential to enhance library accessibility and utilization, particularly in communities that stand to benefit the most.

2.5 Conclusion

Conversations about the potential access effects of overdue fines began in the late 2000s, leading many public libraries in Illinois to adopt fine-free policies starting around 2011. The rationale for this policy is primarily about social inefficiency: if we assume low-income households are the most likely to be blocked from utilizing the library due to overdue fines and also have high marginal benefits of library use, then overdue fines may lead to inefficient outcomes. To quantify the effect of removing overdue fines, I exploit the staggered adoption of fine-free policies by library districts in the state of Illinois to assess the effect on library usage and budgets. Because of data availability restrictions, I focus my analysis on outcomes at the library district level, rather than looking directly at low-income households.[20] Overall, the results suggest that fine-free policies increase library utilization without substantial losses to library income.

My estimates indicate that fine-free policies led to significant increases in library utilization, as measured by annual visits, total circulation, and registered borrowers. These effects are not only large and statistically significant, but also persistent over the three years following policy adoption. Additionally, there is suggestive evidence of increased spending on print materials, which may reflect libraries' responses to higher demand for library resources. I find no effect on "other income," the income category that includes fines and donations; this result is expected given fines were a small share of libraries' incomes prior to the policy. The robustness of these results is confirmed through alternative estimation strategies including Callaway and Sant'Anna (2021), Borusyak et al. (2024) and Synthetic Difference-in-Differences (SDID) Arkhangelsky et al. (2021), suggesting that the findings are not driven by a particular methodological choice.

In conclusion, my results suggest that removing overdue fines can lead to increased extensive and intensive library utilization. The results also suggest that the income reduction from removing fines is small, as expected. There remain several open questions, including whether

[20] If we expect that low-income households are more exposed to the policy and would have larger responses, then my estimated effects represent a lower bound for the true effect on this subpopulation.

there is treatment effect heterogeneity particularly by socioeconomic status or income groups, whether libraries are changing their fund-raising or grant applying behavior, how the recent shift towards digital media impacts the need for overdue fines, and whether the effects generalize to other states or time periods.

2.6 Tables and Figures

Table 2.1: 2007–2010 Means for Districts with/without Fine Information

Variable	With Fine Information	Without Fine Information	Adj P Val
Total Outlets	1.29	1.08	0.1816
Legal Service Area Population	21,547	3,532.2	0.0015
Total Employees	16.05	2.33	0
Hours Per Outlet	2,627.1	1,767.9	0
Total Visits	147,472	17,841	0.0001
Registered Borrowers	9,883.3	1,799.3	0
Reference Transactions	26,774	2,369.4	0.0011
Books	81,090	20,302	0
E-Books	1,101.6	72.77	0
Total Circulation	206,957	16,292	0
Public Use Computers	19.43	5.36	0.0002
Computer Use Sessions	29,922	3,768.8	0.0002
Total Programs	268.5	47.09	0
Total Program Attendance	6,878.2	963.22	0
Federal Gvt Income	6,738.3	1,555.0	0.0036
State Gvt Income	72,416	12,727	0.0028
Local Gvt Income	1,422,565	127,561	0
Other Income	119,555	13,524	0
Total Income	1,621,328	155,366	0
Print Material Exp	126,991	13,588	0.0001
Total Collection Exp	188,422	18,320	0
Total Operating Exp	1,465,535	130,585	0
N: Library Districts	528	87	

Notes: P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 2019 dollars.

Table 2.2: Summary Statistics for All Illinois Libraries

Variable	Mean	Std. Dev.	Min	Median	Max
		2010 PLS Data			
Total Libraries	1.31	3.54	1	1	79
Legal Service Area Population	22,448.35	130,959.85	408	7,984	2,896,016
Total Employees	16.36	47.08	0.3	5.13	937.93
Hours per Branch	2,640.08	862.83	624	2,704	7,668
Total Visits	158,414.40	758,879.50	1,322	51,272	16,500,327
Registered Borrowers	10,073.34	18,021.59	97	3,967	229,658
Reference Transactions	28,262.86	173,971.68	0	4,400	3,760,264
Books	84,038.82	252,319.83	2,537	38,232	5,266,209
E-Books	1,266.03	3,109.01	0	0	23,382
Total Circulation	234,461.52	616,259.16	867	57,410	9,983,590
Number of Public Computers	21.87	84.18	0	10	1,828
Computer Use Sessions	33,332.48	177,038.50	0	9,441	3,800,403
Total Programs	334.77	1,327.26	0	153	28,840
Total Program Attendance	7,883.01	16,521.87	0	2,820	262,000
Federal Government Income	5,718.37	48,747.81	0	0	1,041,814.94
State Government Income	68,295.98	451,210.97	0	15,108.03	9,852,223.24
Local Government Income	1,557,118.67	5,501,769.33	0	319,402.42	
Other Income	95,735	239,850	0	33,209	3,447,665
Total Income	1,727,094	6,159,245	5,530.4	412,480	127,210,160
Capital Expenditures	207,807	981,989	0	0	10,571,743
Print Materials Exp	142,708	944,901	0	34,624	20,869,385
% Collection Exp on Print	0.7379	0.1542	0.2856	0.7300	1
% Operating Exp on Print	0.0948	0.0402	0	0.0876	0.3503
Total Collection Exp	206,769	971,969	0	43,302	20,869,384
Total Operating Exp	1,576,820	5,898,016	6,504.7	363,608	122,720,681
		2010 Census			
Share Rural	0.3306	0.4235	0.0000	0.0533	1.0000
Share White	0.8753	0.1715	0.0563	0.9470	0.9899
Share Hispanic	0.0731	0.1108	0.0022	0.0300	0.8867
Share Renter	0.2686	0.1559	0.0289	0.2242	0.9810
		2006–2010 5 year ACS			
Share with HS diploma or less	0.3113	0.1023	0.0160	0.3252	0.6056
Share with less than HS diploma	0.0896	0.0714	0.0000	0.0751	0.5065
Share of Households with Public Assistance	0.0866	0.0702	0.0000	0.0702	0.5900
Poverty Rate	0.1119	0.0861	0.0000	0.0917	0.6397
N: Library Districts	615				

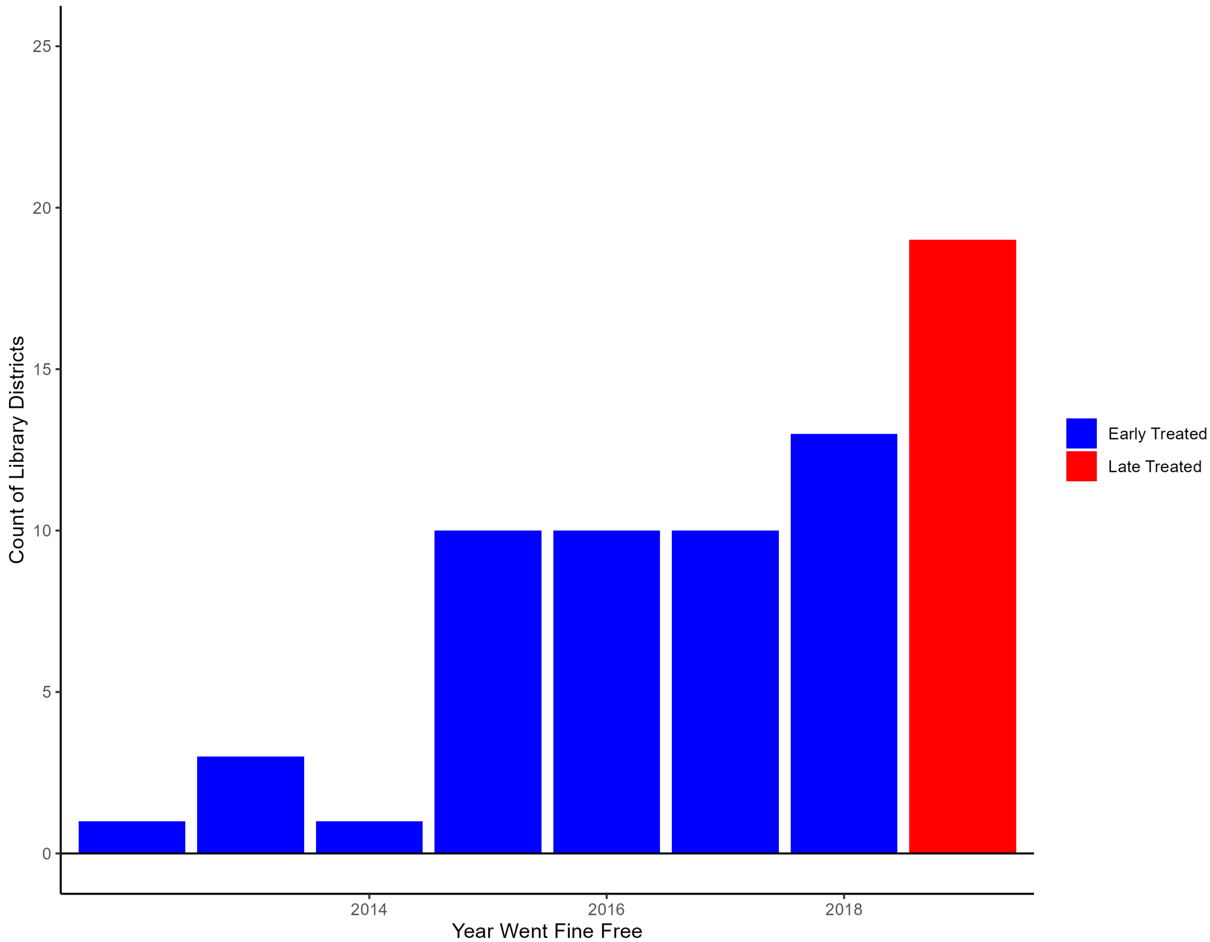
Notes: All dollar values are in 2019 dollars. Data are pooled across all libraries. Summary statistics include mean, standard deviation, minimum, median, and maximum. Local government income max value not shown due to truncation.

Table 2.3: 2007–2010 Means for Dropped/Remaining Districts, PLS Data

Variable	Dropped Districts	Remaining Districts	Adj P Value
Total Outlets	1.07	1.3	0.1474
Legal Service Area Population	4,925.7	22,270	0.004
Total Employees	3.74	16.52	0
Hours Per Outlet	1,890.6	2,648.5	0
Total Visits	31,125	151,917	0.0008
Registered Borrowers	2,803.1	10,120	0
Reference Transactions	4,317.8	27,739	0.0033
Books	26,790	83,114	0
E-Books	164.63	1,140.0	0
Total Circulation	39,896	212,551	0
Number of Public Use Computers	6.81	19.92	0.0013
Public Computer Use Sessions	6,220.9	30,872	0.0013
Total Programs	78.02	274.18	0
Total Program Attendance	1,814.5	7,024.0	0
Federal Gvt Revenue	2,016.3	6,932.3	0.0118
State Gvt Revenue	20,317	74,121	0.0118
Local Gvt Revenue	285,278	1,461,162	0
Other Income	28,001	122,352	0
Total Income	335,612	1,664,623	0
Print Material Expenses	25,971	130,703	0.0006
Total Collection Exp	38,166	193,694	0
Total Operating Exp	289,445	1,506,187	0
Share Rural	0.8276	0.5752	0
N: Library Districts	116	499	

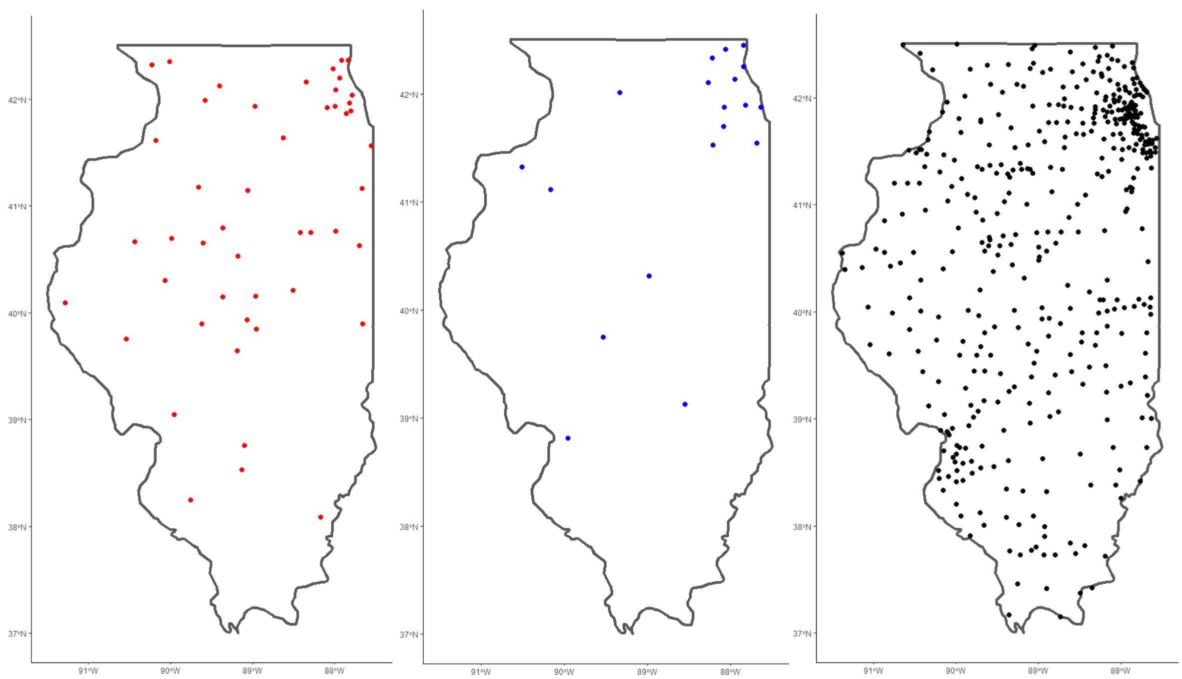
Notes: P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 2019 dollars.

Figure 2.1: Distribution of Treatment Timing



Notes: Author's own calculations using library policy data. Not shown, 443 districts that adopted during the Covid-19 pandemic, either temporarily or permanently.

Figure 2.2: Geography of Library Districts



(a) Early Treated (2011–2018) (b) Late Treated (2019) (c) Untreated

Notes: Author's own generation using PLS and the described fine policy data.

Table 2.4: Treatment Group Means by Treatment Timing

Variable	Early Treated	Late Treated	Covid Treated	P: Early/Late	P: Early/Cov	P: Late/Cov
			2007–2010 Averages using PLS Data			
Total Outlets	1.27	5.17	1.14	0.3612	0.9083	0.3651
Legal Service Area Population	16,495	174,162	16,231	0.3591	0.9775	0.3684
Total Employees	16.57	78.00	13.81	0.3674	0.9099	0.3622
Hours Per Outlet	2,470.8	3,144.0	2,646.5	0.3708	0.9134	0.0722
Total Visits	125,789	1,073,532	114,286	0.3558	0.9022	0.3611
Registered Borrowers	11,417	27,607	9,206.5	0.3603	0.9105	0.3636
Reference Transactions	20,281	220,404	20,094	0.3688	0.9744	0.3691
Books	72,680	375,307	71,422	0.3649	0.9723	0.3650
E-Books	1,081.8	1,749.1	1,119.7	0.3620	0.9758	0.3675
Total Circulation	233,220	752,250	186,518	0.3692	0.9081	0.3633
Number of Public Use Computers	17.04	121.53	15.77	0.3655	0.9107	0.3668
Public Computer Use Sessions	26,103	220,306	23,070	0.3663	0.9066	0.3629
Total Programs	218.46	1,107.4	243.73	0.3606	0.9120	0.3682
Total Program Attendance	6,604.4	19,271	6,532.0	0.3628	0.9741	0.3661
Federal Gvt Income	2,729.0	49,023	5,548.1	0.3599	0.2284	0.3737
State Gvt Revenue	49,358	587,819	54,279	0.3622	0.9112	0.3631
Local Gvt Revenue	1,622,857	7,657,000	1,170,694	0.3640	0.9090	0.3623
Other Income	130,440	432,830	107,798	0.3636	0.9101	0.3657
Total Income	1,805,971	8,726,672	1,338,319	0.3605	0.9088	0.3670
Capital Expenditures	153,504	396,188	211,452	0.3617	0.9069	0.4532
Print Materials Exp	108,945	872,285	100,504	0.3647	0.9255	0.3638
% Collection Exp on Print	0.7698	0.6703	0.7557	0.3676	0.9098	0.1401
% Operating Exp on Print	0.0924	0.0852	0.1023	0.3593	0.9109	0.0120
Total Collection Exp	182,520	1,054,082	157,094	0.3652	0.9083	0.3616
Total Operating Exp	1,716,136	8,112,689	1,192,296	0.3634	0.9074	0.3653
			2010 Census			
Share Rural	0.4681	0.1650	0.3068	0.0105	0.1014	0.2695
Share White	0.8698	0.8175	0.8637	0.3294	0.8421	0.2537
Share White non-Hispanic	0.8318	0.7610	0.8220	0.3618	0.8233	0.2779
Share Hispanic	0.0751	0.1075	0.0782	0.3567	0.8810	0.2915
Share Renter	0.2274	0.2066	0.2469	0.4772	0.4215	0.2846
			Using 2006 to 2010 5 year ACS			
Share with HS diploma or less	0.4133	0.2974	0.3930	0.0084	0.5335	0.0092
Share with less than HS diploma	0.0894	0.0706	0.0892	0.2985	0.9785	0.1966
Share of Households with Public Assistance	0.0786	0.0498	0.0815	0.0407	0.9779	0.0092
Poverty Rate	0.0897	0.0710	0.1068	0.2834	0.0962	0.0092
N: Library Districts	48	19	432			

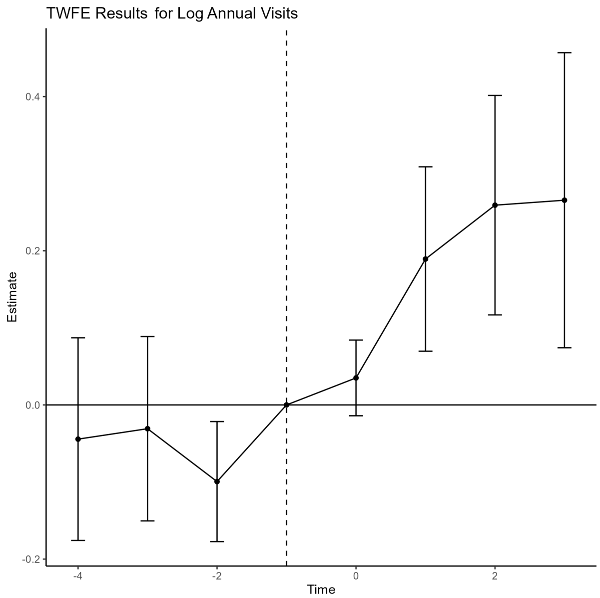
Notes: P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 2019 dollars.

Table 2.5: Treatment Group Means by Treatment Timing: Changes between 2000 and 2010

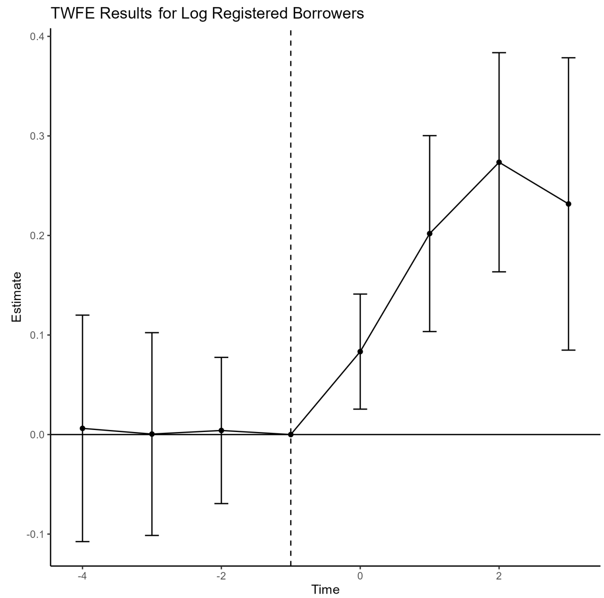
Variable	Early Treated	Late Treated	Covid Treated	P: Early/Late	P: Early/Cov	P: Late/Cov
			2000–2010 Change using PLS			
Total Outlets	0.0625	0	0	0.7251	0.6545	1.0000
Legal Service Area Population	1,547.8	8,282.2	1,939.0	0.5870	0.7132	0.5198
Total Employees	2.07	20.59	2.10	0.5293	0.9814	0.4826
Hours Per Outlet	-76.92	39.70	47.32	0.6837	0.6412	0.9817
Total Visits	42,375	519,828	26,553	0.5638	0.6215	0.5093
Reference Transactions	-476.62	-106,410	1,226.3	0.5812	0.8054	0.4772
Books	11,536	-31,607	9,092.3	0.5466	0.7210	0.4895
Total Circulation	103,387	229,103	67,407	0.5742	0.6398	0.5036
Number of Public Use Computers	15.42	92.37	13.01	0.5640	0.7356	0.5091
Federal Gvt Revenue	-88.76	36,038	-3,708.5	0.5352	0.6709	0.4887
State Gvt Revenue	-34,787	-298,079	-21,104	0.5487	0.6398	0.5042
Local Gvt Revenue	494,169	1,146,754	396,097	0.5611	0.7298	0.4824
Other Income	-90,142	28,977	-34,758	0.5396	0.6631	0.5060
Total Income	371,503	913,689	336,526	0.5675	0.8578	0.5014
Capital Expenditures	-415,649	-668,331	46,817	0.7077	0.6735	0.4879
Total Collection Exp	14,150	286,429	21,077	0.5377	0.7601	0.4918
Total Operating Exp	538,771	854,941	384,047	0.5712	0.6409	0.5084
			2000 Census to 2010 Census Change			
Change in Share Hispanic	0.0259	0.0394	0.0283	0.2952	0.7046	0.6723
Change in Share Rural	0.0124	-0.0337	-0.0092	0.2891	0.3798	0.6789
Change in Share White	-0.0321	-0.0417	-0.0347	0.4526	0.7104	0.6751
Change in Share White non-hispanic	-0.0443	-0.0654	-0.0495	0.2883	0.7325	0.6942
Change in Share Renter	-0.0009	0.0082	0.0080	0.2537	0.3214	0.9731
			2000 Census to 2006–2010 5 year ACS Change			
Change in share HS diploma or less	-0.0989	-0.0861	-0.1004	0.3670	0.8846	0.3639
Change in share less than HS diploma	-0.0714	-0.0484	-0.0728	0.0829	0.8874	0.0215
Change in Poverty Rate	0.0194	0.0127	0.0273	0.2937	0.2312	0.0284
Change in Share HH w/ Public Assistance	0.0599	0.0321	0.0609	0.0141	0.8831	0.0028
N: Library Districts	45	19	413			

Notes: P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 2019 dollars.

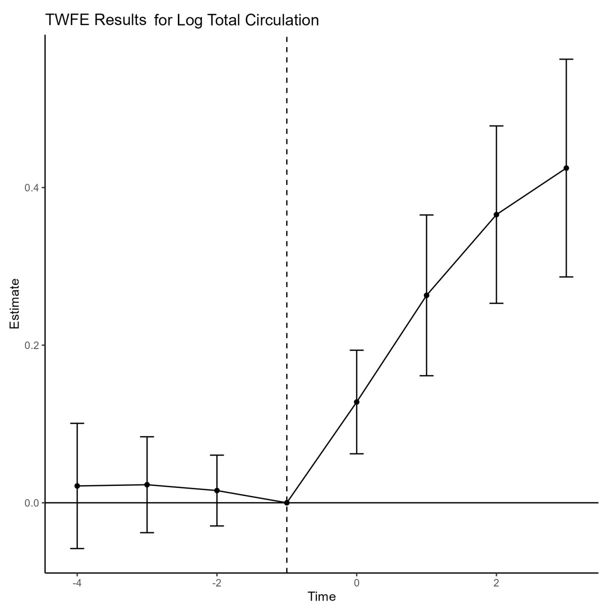
Figure 2.3: Results for Library Usage: TWFE



(a) Log Total Visits



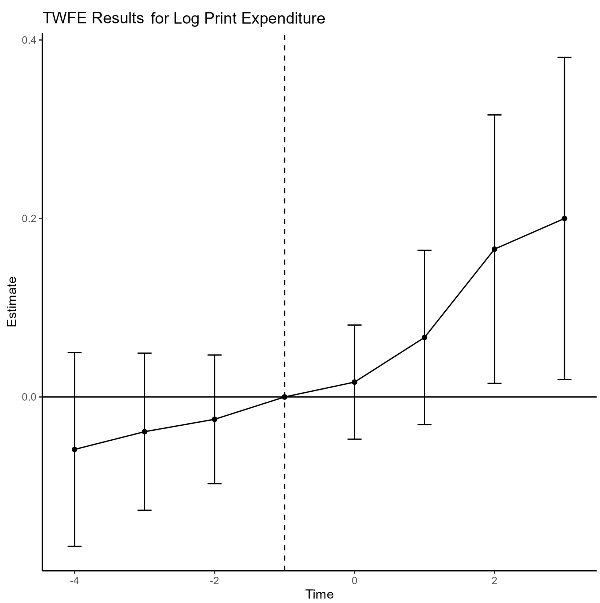
(b) Log Registered Borrowers



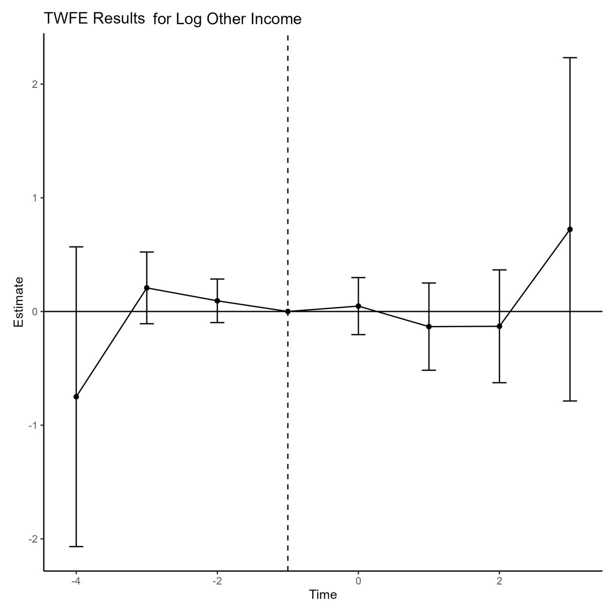
(c) Log Total Circulation

Notes: Estimated using Equation 2.1. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure 2.4: Results for Library Finances: TWFE



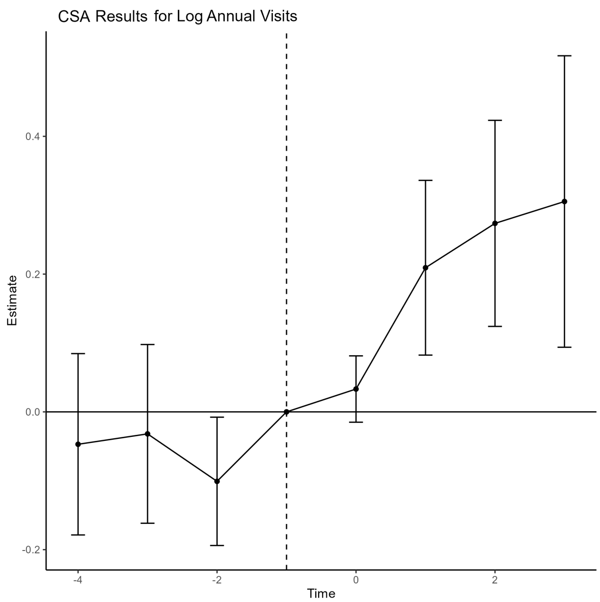
(a) Log Print Expenses



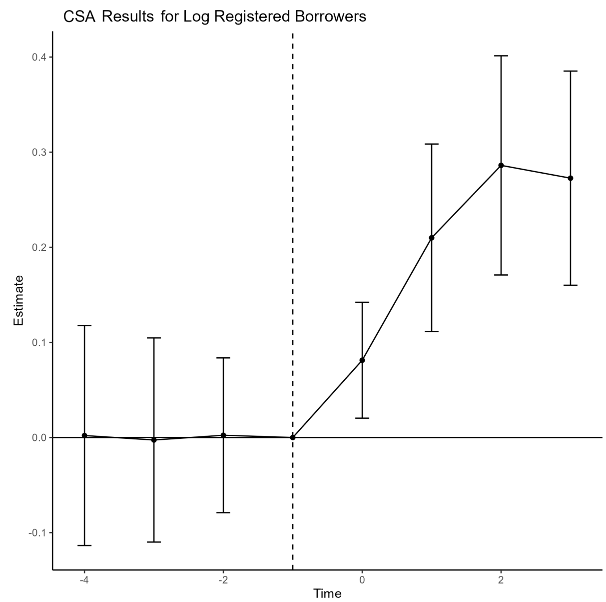
(b) Log Other Income

Notes: Estimated using Equation 2.1. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

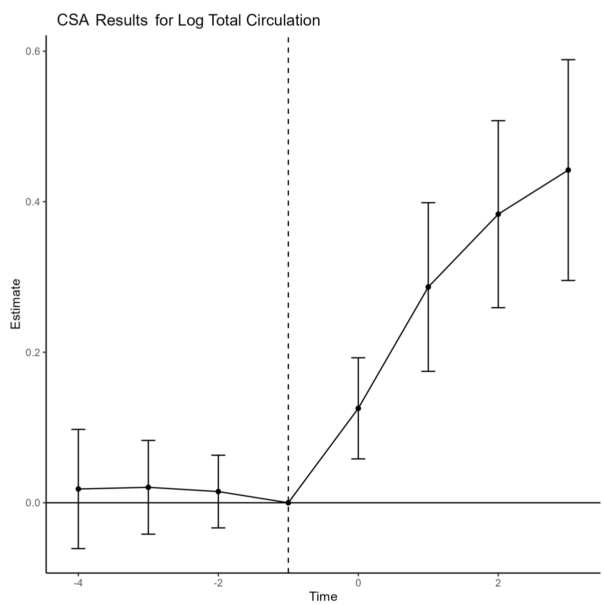
Figure 2.5: Results for Library Usage: Callaway and Sant'Anna, Not Yet Treated



(a) Log Total Visits



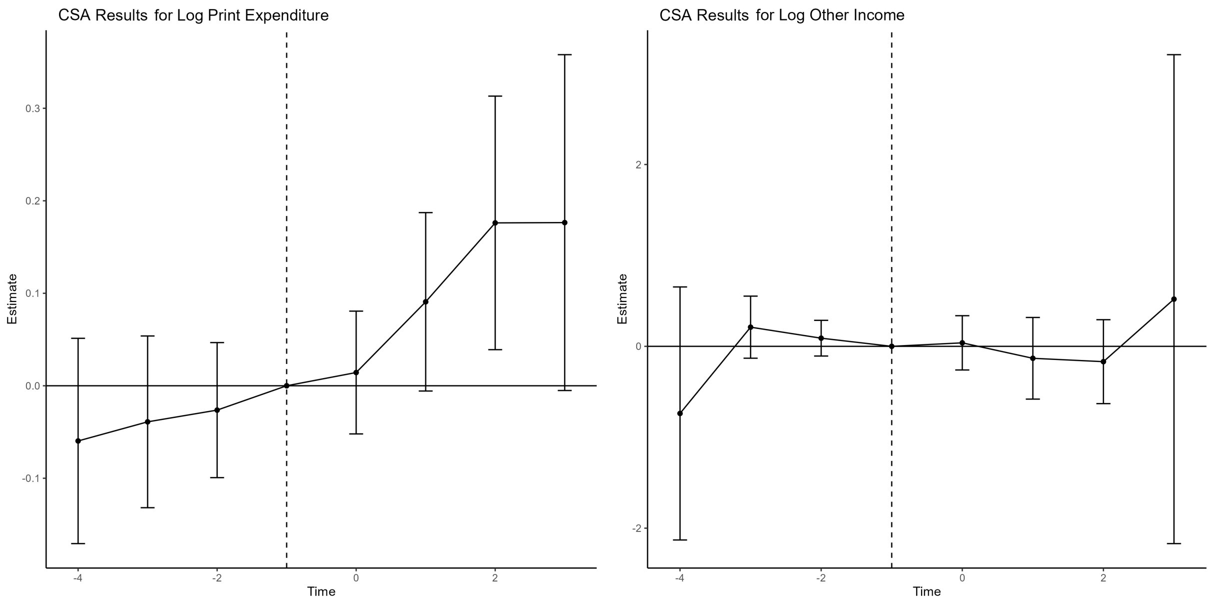
(b) Log Registered Borrowers



(c) Log Total Circulation

Notes: Estimated using Equations 2.3 and 2.4. Unbalanced panel used. 95% confidence intervals shown. Not yet treated used as control group.

Figure 2.6: Results for Library Finances: Callaway and Sant'Anna, Not Yet Treated



(a) Log Print Expenses

(b) Log Other Income

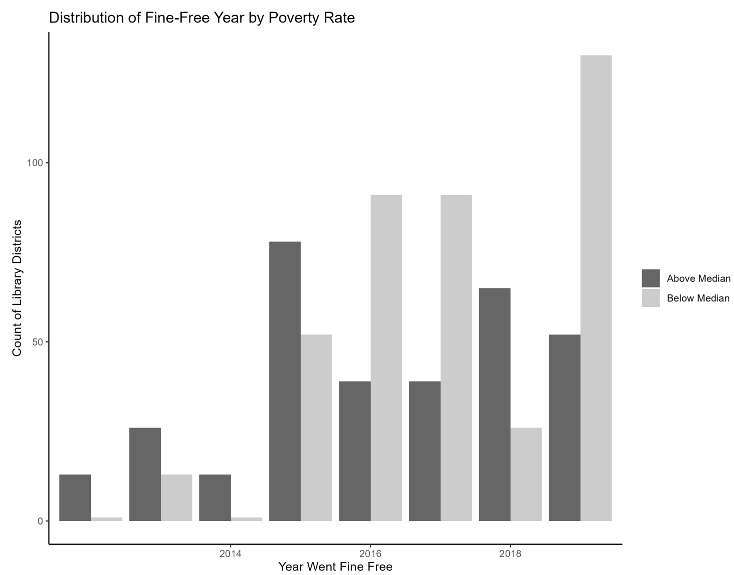
Notes: Estimated using Equations 2.3 and 2.4. Unbalanced panel used. 95% confidence intervals shown. Not yet treated used as control group.

Table 2.6: Dynamic Effects

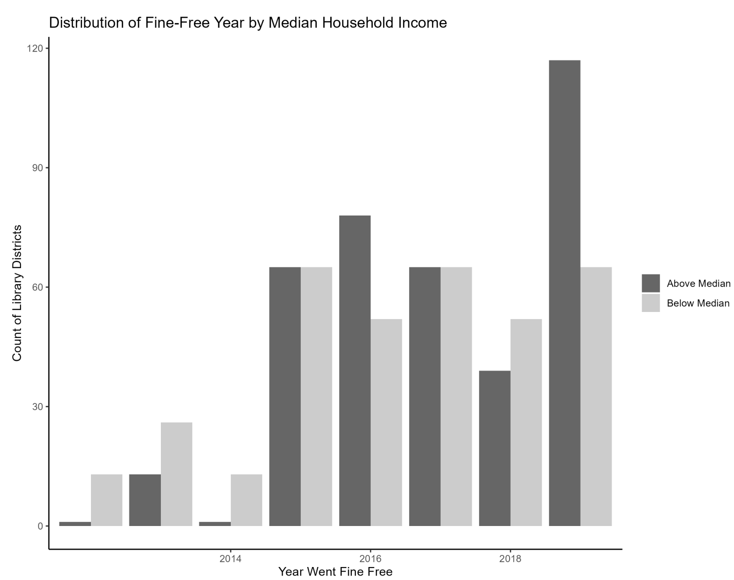
	TWFE					Callaway and Sant'Anna (Not-Yet-Treated, Unbalanced)				
Years Since Treatment	Log Annual Visits	Log Registered Borrowers	Log Total Circulation	Log Print Material Expenses	Log Other Income	Log Annual Visits	Log Registered Borrowers	Log Total Circulation	Log Print Material Expenses	Log Other Income
-4	-0.0444	0.0062	0.0214	-0.0588	-0.7498	-0.047	0.0021	0.0183	-0.0597	-0.7383
	(0.0671)	(0.058)	(0.0405)	(0.0554)	(0.6726)	(0.0657)	(0.0636)	(0.0394)	(0.054)	(0.6792)
-3	-0.0308	0.0005	0.0229	-0.0389	0.2077	-0.0319	-0.0026	0.0205	-0.039	0.2108
	(0.061)	(0.052)	(0.031)	(0.0449)	(0.1609)	(0.0617)	(0.0545)	(0.0335)	(0.0455)	(0.1647)
-2	-0.0994**	0.0041	0.0155	-0.025	0.0939	-0.1008**	0.0023	0.0149	-0.0263	0.0896
	(0.0397)	(0.0375)	(0.0229)	(0.0368)	(0.0977)	(0.0486)	(0.0381)	(0.0235)	(0.0394)	(0.1015)
0	0.035	0.0833***	0.1279***	0.0166	0.0475	0.0331	0.0812***	0.1255***	0.0143	0.0384
	(0.025)	(0.0295)	(0.0335)	(0.0326)	(0.128)	(0.0252)	(0.03)	(0.0335)	(0.0354)	(0.1466)
1	0.1893***	0.2019***	0.2632***	0.0667	-0.1332	0.2092***	0.21***	0.2867***	0.0908*	-0.1315
	(0.061)	(0.0502)	(0.052)	(0.0498)	(0.1957)	(0.0648)	(0.0478)	(0.0557)	(0.048)	(0.224)
2	0.2591***	0.2735***	0.3657***	0.1656**	-0.1302	0.2736***	0.2861***	0.3834***	0.1761**	-0.1686
	(0.0726)	(0.0562)	(0.0574)	(0.0767)	(0.2531)	(0.0704)	(0.0513)	(0.0602)	(0.0739)	(0.2106)
3	0.2656***	0.2317***	0.4247***	0.2**	0.7226	0.3054***	0.2727***	0.442***	0.1764*	0.5191
	(0.0976)	(0.0749)	(0.0705)	(0.0921)	(0.7703)	(0.1037)	(0.0629)	(0.0816)	(0.095)	(1.3719)

Notes: There are 499 library districts used in the estimation of effects. The results are unbalanced, so the actual observation count for each estimate varies. In particular, the time = 0 effect is estimated using districts treated between 2011 and 2019, while the time = 3 effect is estimated using districts treated between 2011 and 2016. The excluded reference year is -1. Standard errors clustered at the library district level. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Figure 2.7: Distribution of Treatment Timing by Heterogeneity Group



(a) Poverty Rate



(b) Median Household Income

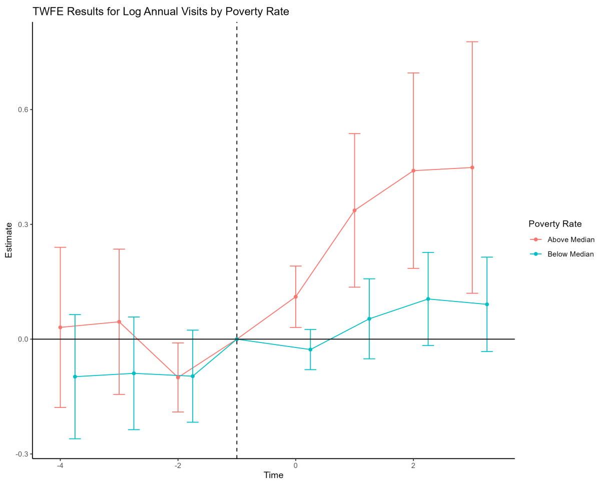
Notes: Author's own calculation using poverty rate and median household income data from the 2010 Census.

Table 2.7: Regression of Treatment Year on Flag of above-median Poverty Rate/Median Household Income

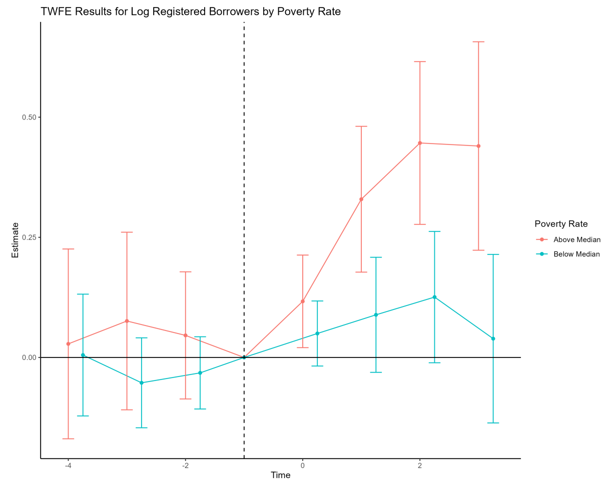
	Estimate	Std. Error	t value	Pr($> \|t\|$)
Outcome: above-median Poverty Rate				
(Intercept)	-63.3026	109.7	-0.577	0.564
Year Treated	0.031	0.054	0.574	0.566
Outcome: above-median Poverty Rate				
(Intercept)	-92.41	108.9	-0.849	0.396
Year Treated	0.046	0.054	0.852	0.394

Notes: Estimated using Equation 2.5. Poverty rate and median household income indicators from 2010 Census.

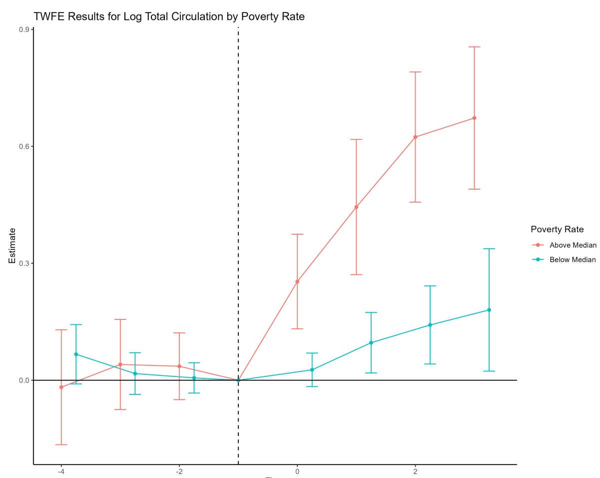
Figure 2.8: Results for Library Usage: Heterogeneity by Poverty Rate



(a) Log Total Visits



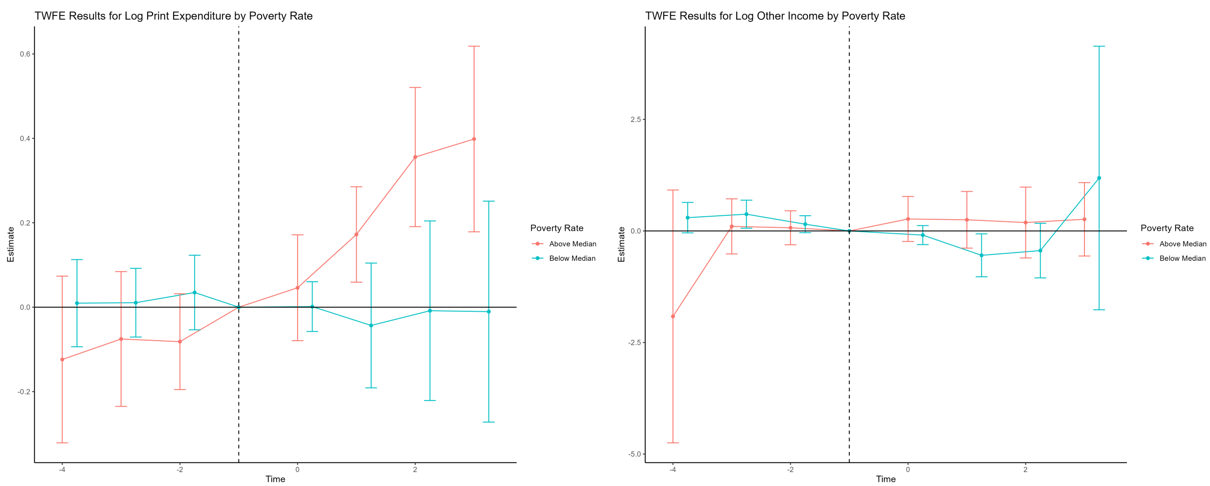
(b) Log Registered Borrowers



(c) Log Total Circulation

Notes: Estimated using Equation 2.1, only on the relevant subpopulation. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure 2.9: Results for Library Finances: Heterogeneity by Poverty Rate

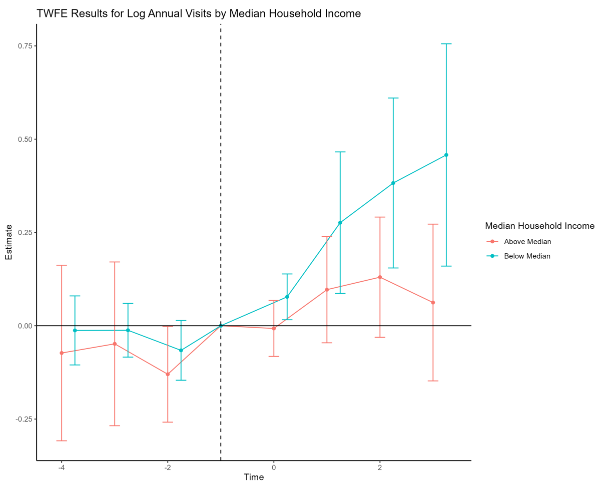


(a) Log Print Expenses

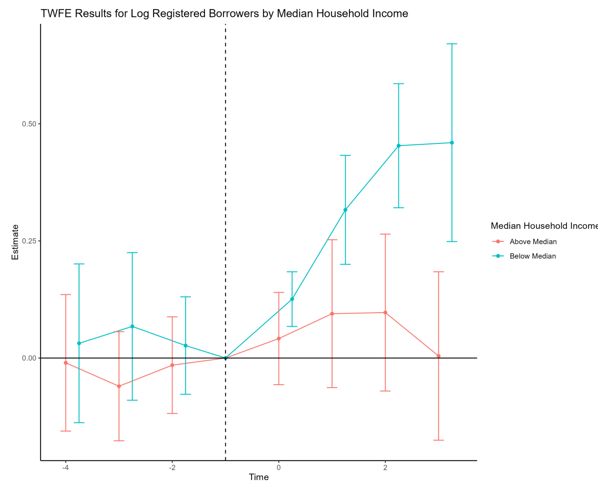
(b) Log Other Income

Notes: Estimated using Equation 2.1, only on the relevant subpopulation. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

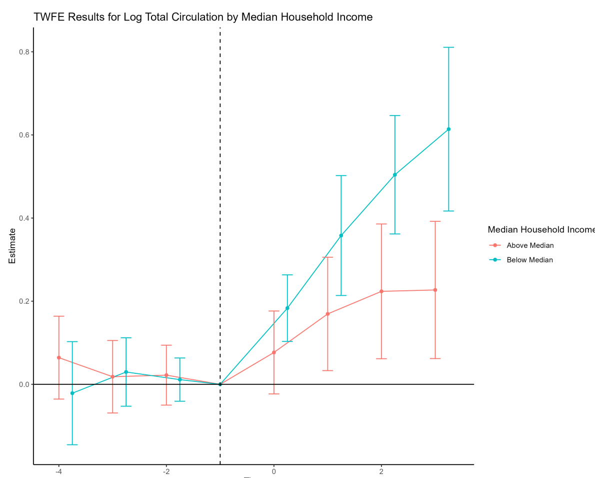
Figure 2.10: Results for Library Usage: Heterogeneity by Median Household Income



(a) Log Total Visits



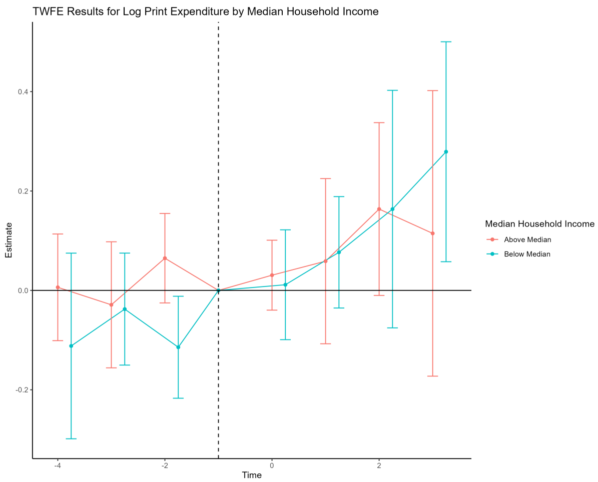
(b) Log Registered Borrowers



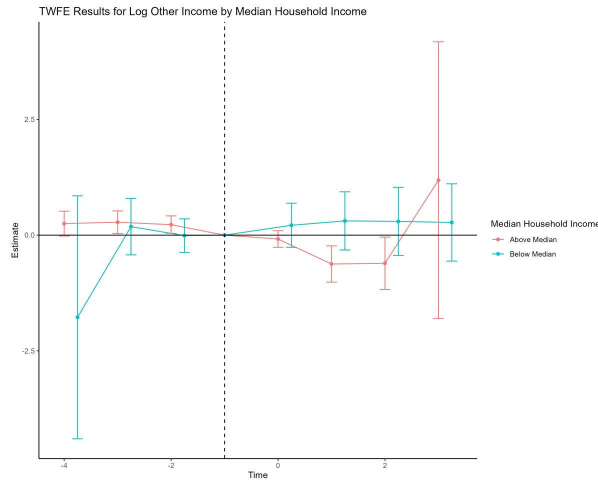
(c) Log Total Circulation

Notes: Estimated using Equation 2.1, only on the relevant subpopulation. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure 2.11: Results for Library Finances: Heterogeneity by Median Household Income



(a) Log Print Expenses



(b) Log Other Income

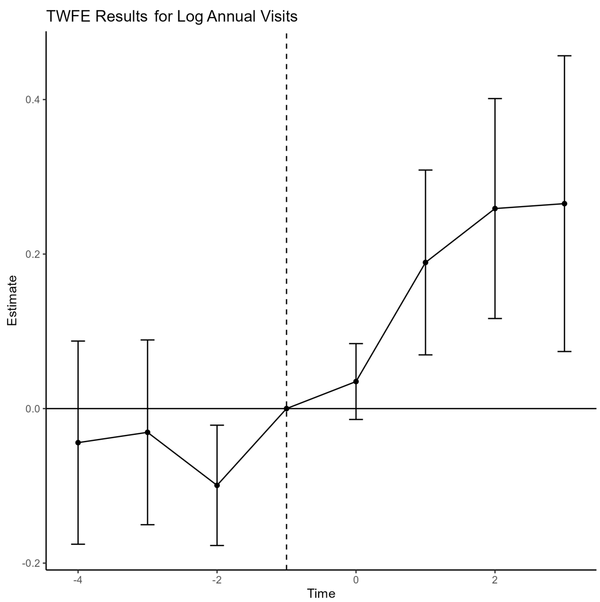
Notes: Estimated using Equation 2.1, only on the relevant subpopulation. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

2.A Appendix

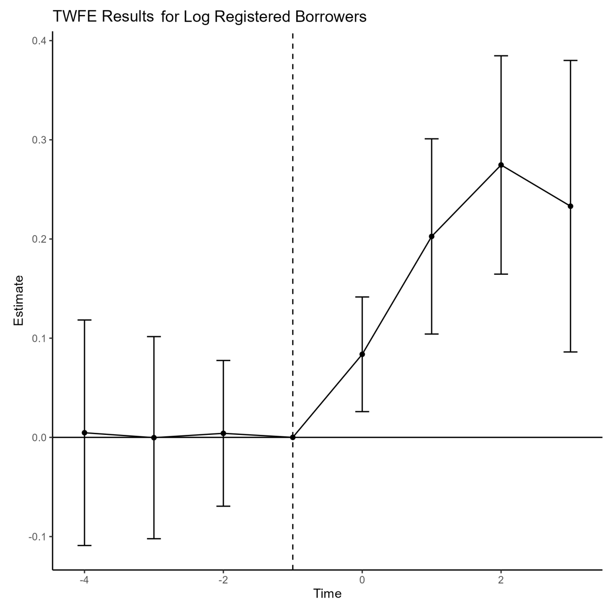
2.A.1 Robustness to Excluding Chicago

The following charts present the results of the TWFE estimation with the exclusion of the Chicago Public Library. The Chicago Public Library is a significant outlier in the Illinois library system, both in size and in scope of operations. As one of the largest public library systems in the country, it differs substantially from other libraries in terms of budget, resources, and user base. Given these differences, we might expect the Chicago Public Library to respond differently to the removal of overdue fines compared to smaller, more typical libraries. For this reason, excluding the Chicago Public Library allows for a more accurate estimate of the effects on the remaining libraries, which are more representative of the overall population. Despite its exclusion, my results remain basically unchanged. The sign and magnitude of all estimates are nearly identical to the estimation including Chicago, indicating that the presence of the Chicago Public Library has minimal influence on the general findings.

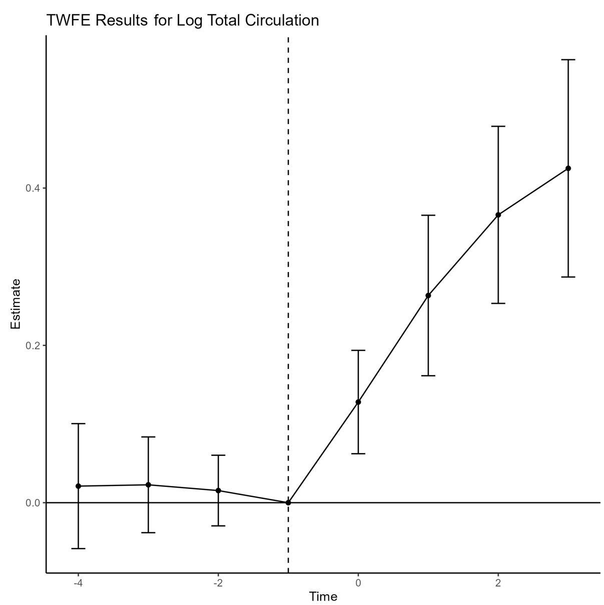
Figure A.2.1: Results for Library Usage: TWFE Excluding The Chicago Public Library



(a) Log Total Visits



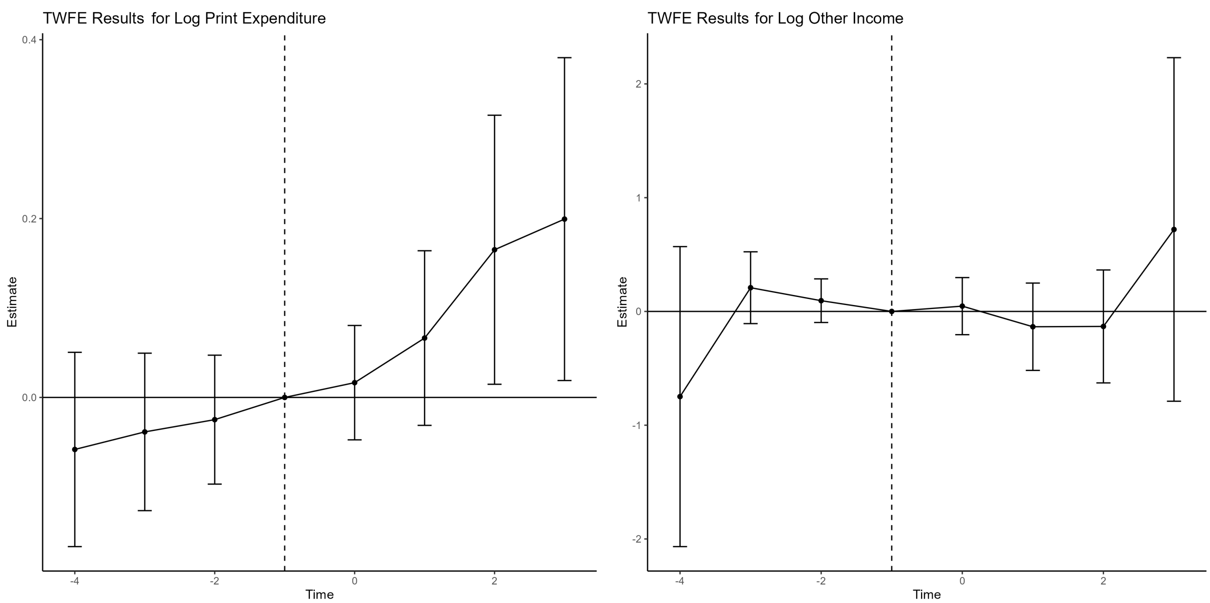
(b) Log Registered Borrowers



(c) Log Total Circulation

Notes: Estimated using Equation 2.1. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure A.2.2: Results for Library Finances: TWFE Excluding The Chicago Public Library



(a) Log Print Expenses

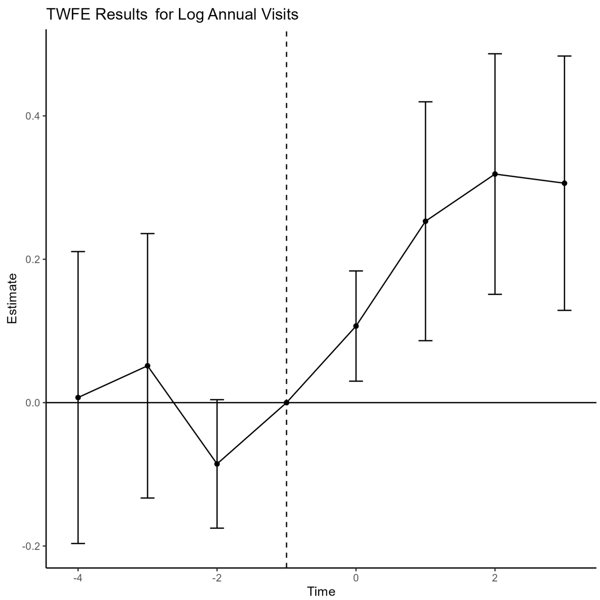
(b) Log Other Income

Notes: Estimated using Equation 2.1. Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

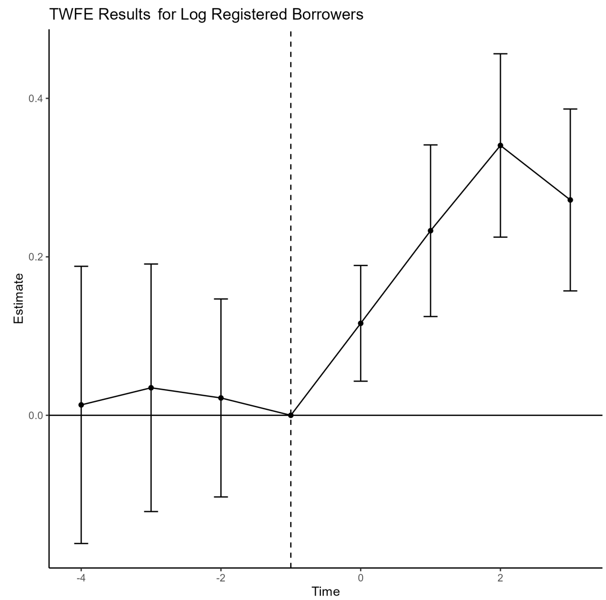
2.A.2 Robustness to Excluding Late Adopters

The following charts demonstrate the robustness of the TWFE and Callaway and Sant'Anna estimators when excluding "late adopters" and some later "early adopters". The primary distinction from the main results is the enforcement of a balanced panel, which excludes libraries that were treated after 2016 due to the insufficient number of post-treatment periods for those libraries. Despite this exclusion, the results remain robust, with the sign and magnitude of all estimates remaining consistent with the original findings.

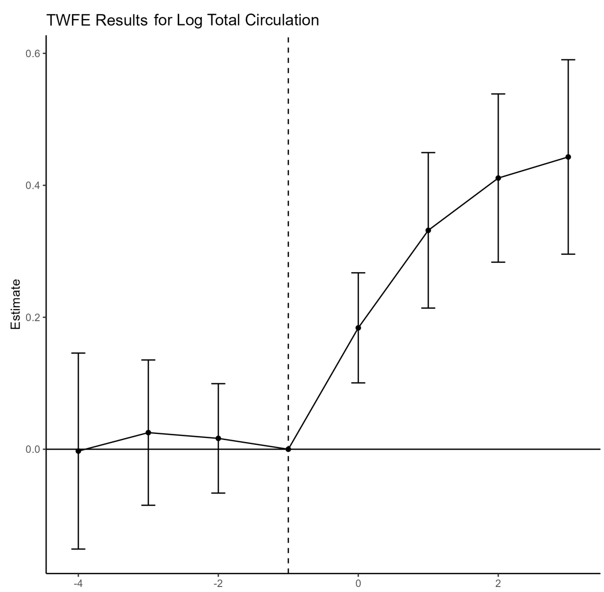
Figure A.2.3: Results for Library Usage: TWFE, Balanced



(a) Log Total Visits



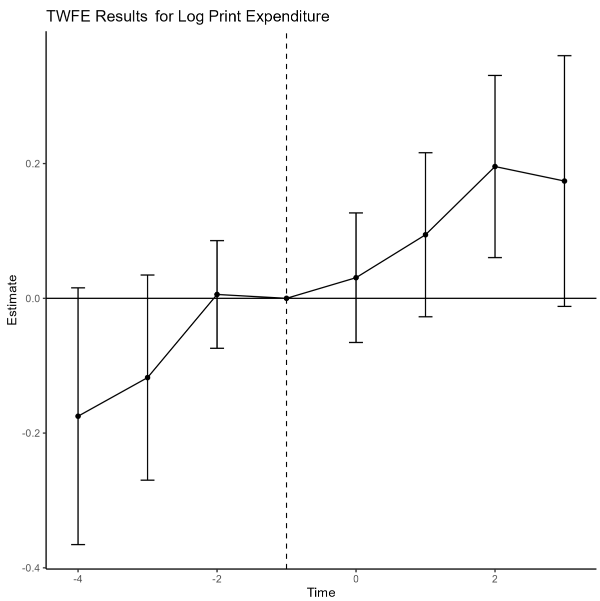
(b) Log Registered Borrowers



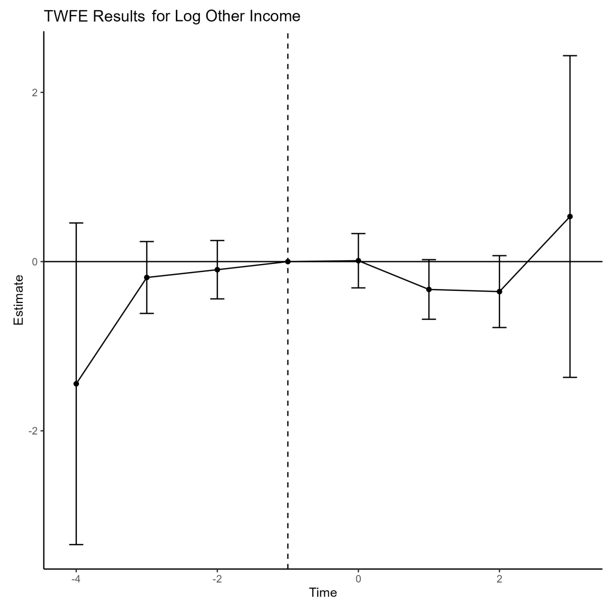
(c) Log Total Circulation

Notes: Estimated using Equation 2.1. Balanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure A.2.4: Results for Library Finances: TWFE, Balanced



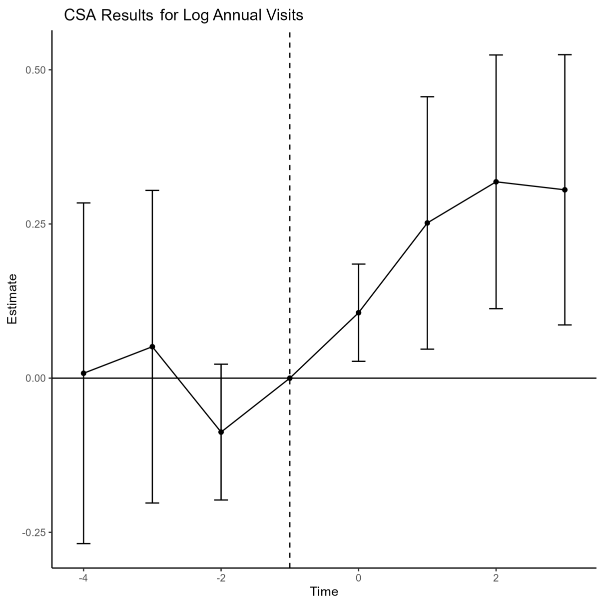
(a) Log Print Expenses



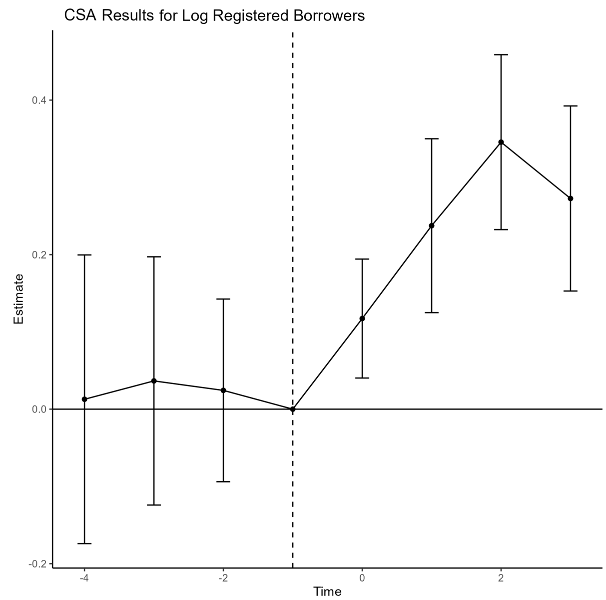
(b) Log Other Income

Notes: Estimated using Equation 2.1. Balanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

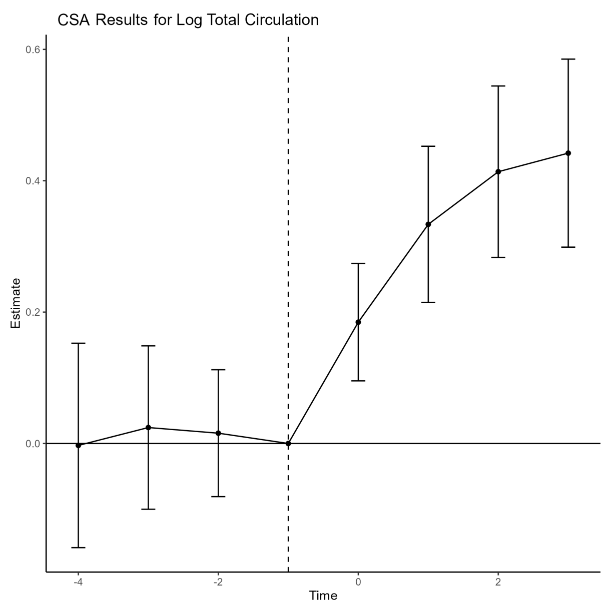
Figure A.2.5: Results for Library Usage: Callaway and Sant'Anna, NYT, Balanced



(a) Log Total Visits



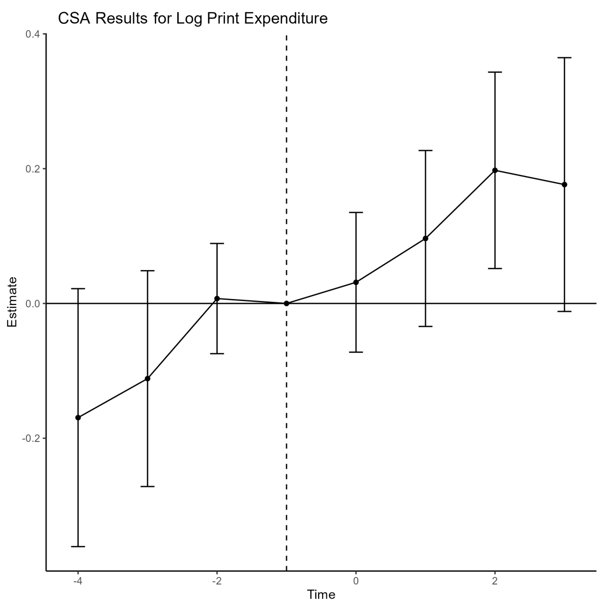
(b) Log Registered Borrowers



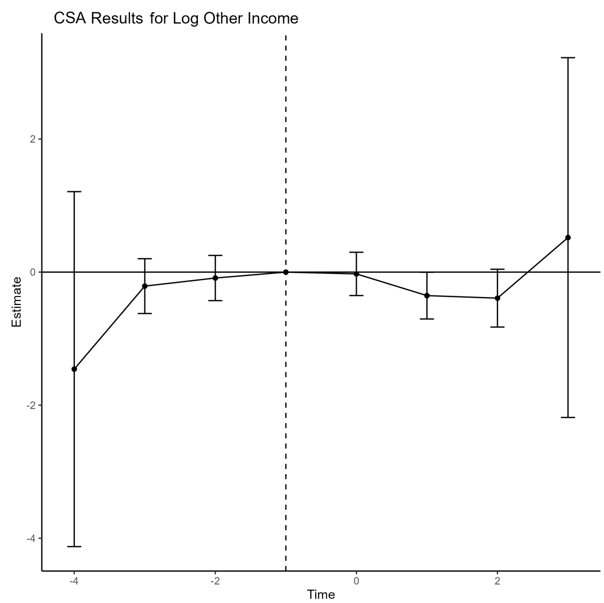
(c) Log Total Circulation

Notes: Estimated using Equations 2.3 and 2.4. Balanced panel used. 95% confidence intervals shown. Not yet treated used as control group.

Figure A.2.6: Results for Library Finances: Callaway and Sant'Anna, Not Yet Treated, Balanced



(a) Log Print Expenses



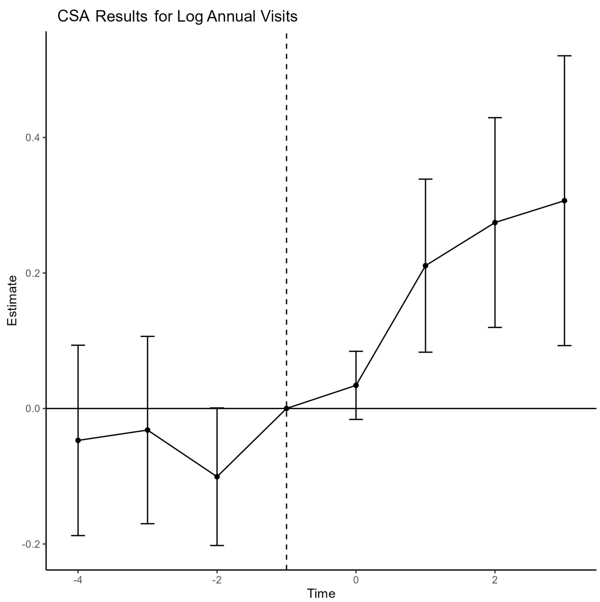
(b) Log Other Income

Notes: Estimated using Equations 2.3 and 2.4. Balanced panel used. 95% confidence intervals shown. Not yet treated used as control group.

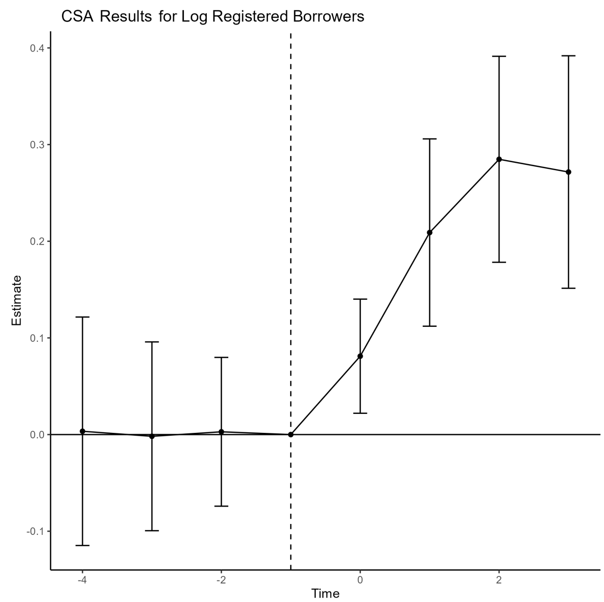
2.A.3 Robustness to Using Never Treated as Controls

The following charts show the robustness of the results to the definition of the control group. In the primary results, I define the control group as libraries that have not yet adopted the fine-free policy at the time of analysis. Here, I define the control group as libraries that are "never treated," meaning those that did not adopt the fine-free policy at any point in my estimation period. It is important to note that most libraries adopted fine-free policies during the Covid-19 pandemic; however, because my panel stops at 2019, these Covid-adopting libraries are considered "never treated" for the purposes of this analysis. Despite this change in control group definition, the results remain robust, with the sign and magnitude of all estimates largely unchanged.

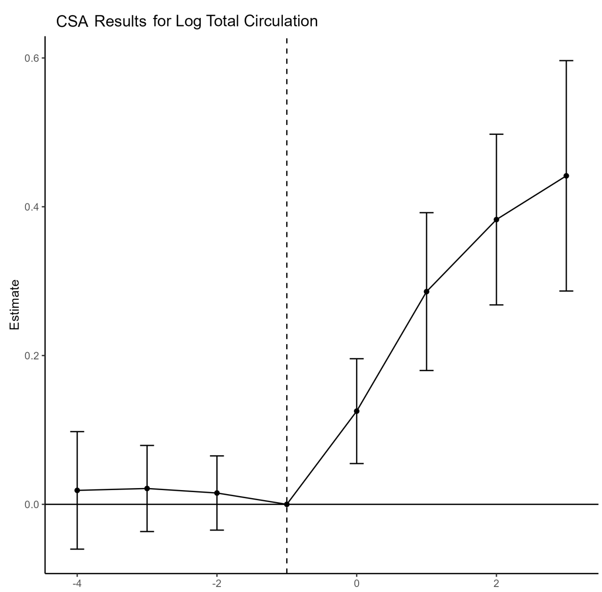
Figure A.2.7: Results for Library Usage: Callaway and Sant'Anna, Never treated



(a) Log Total Visits



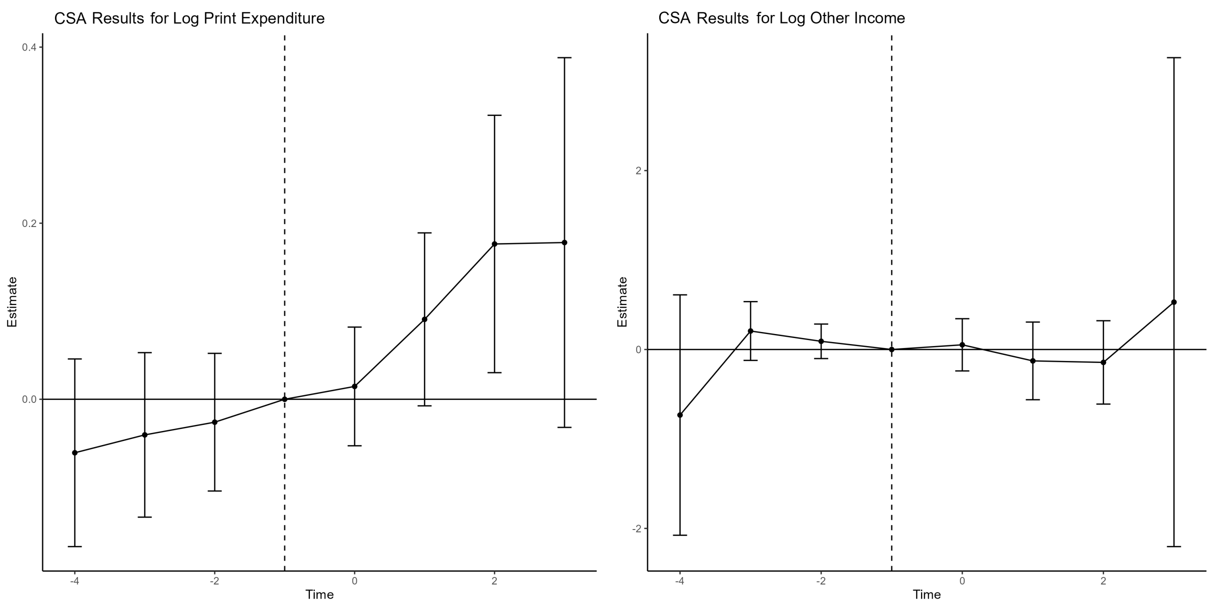
(b) Log Registered Borrowers



(c) Log Total Circulation

Notes: Estimated using Equations 2.3 and 2.4. Unbalanced panel used. 95% confidence intervals shown. Never treated used as control group.

Figure A.2.8: Results for Library Finances: Callaway and Sant'Anna, Never treated



(a) Log Print Expenses

(b) Log Other Income

Notes: Estimated using Equations 2.3 and 2.4. Unbalanced panel used. 95% confidence intervals shown. Never treated used as control group.

2.A.4 Robustness to Using Other Estimators

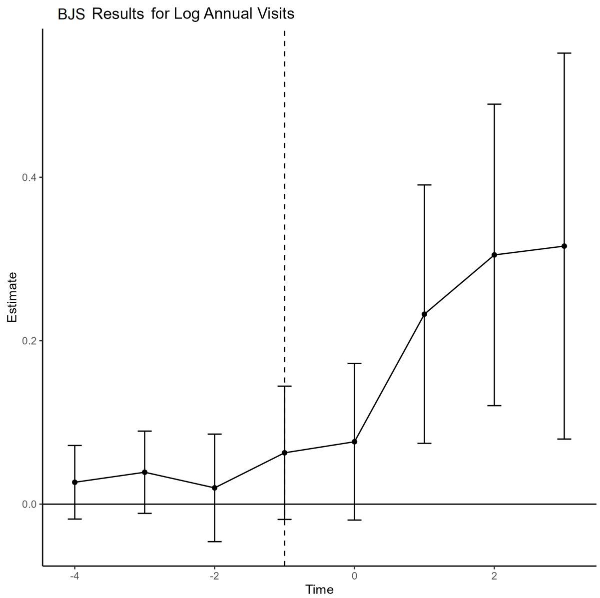
I report the TWFE results as my main results, as it is the standard in the literature. However, given recent methodological advances, I also explore alternative estimators. In the main body of the paper, I report on the Callaway and Sant'Anna estimator, as the assumptions required are more reasonable for my setting. Here I also test two other alternative estimators including the Borusyak et al. (2024) estimator and the Synthetic Difference-in-Differences (SDID) estimator (Arkhangelsky et al., 2021).

The Borusyak et al. (2024) estimator incorporates pre-trends to estimate counterfactual outcomes, but it does not explicitly model cohort treatment effect heterogeneity. The SDID method constructs an aggregate treatment group and then uses time and unit weights to match the pre-treatment periods of the aggregate treated unit with the untreated units. SDID is useful if there are systematic differences between treated and control units that may violate the parallel trends assumption. However, SDID shares some of the same challenges as TWFE, as it allows for "unclean" comparisons between treated units.

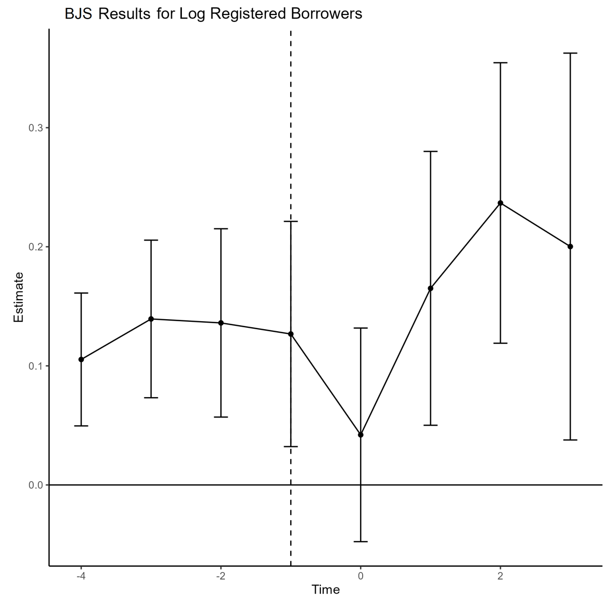
For both methods, the overall patterns remain consistent with the TWFE results, but the estimates tend to be noisier. For the Borusyak et al. (2024) estimator, the patterns are similar, but there are statistically significant estimates in the pre-treatment periods for registered borrowers and total circulation. In the case of SDID, most effects are if the same sign but statistically insignificant, except for the 3-year period after treatment for registered borrowers and the 2–3 years out for total circulation.

Borusyak, Jaravel, and Spiess 2024

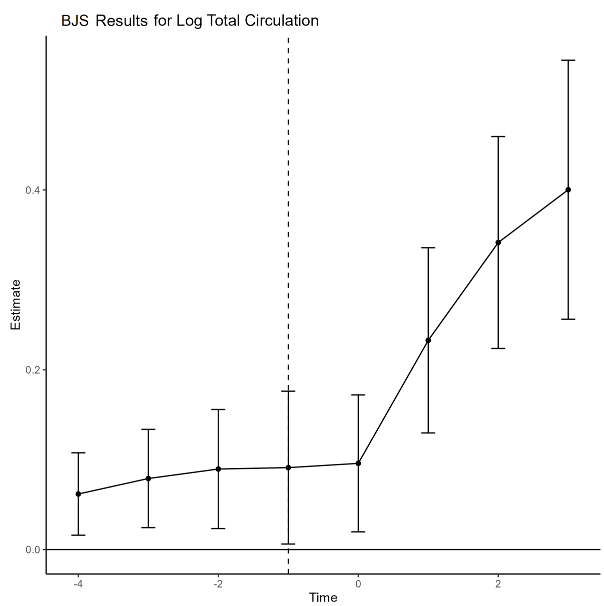
Figure A.2.9: Results for Library Usage: Borusyak, Jaravel, and Spiess 2024



(a) Log Total Visits



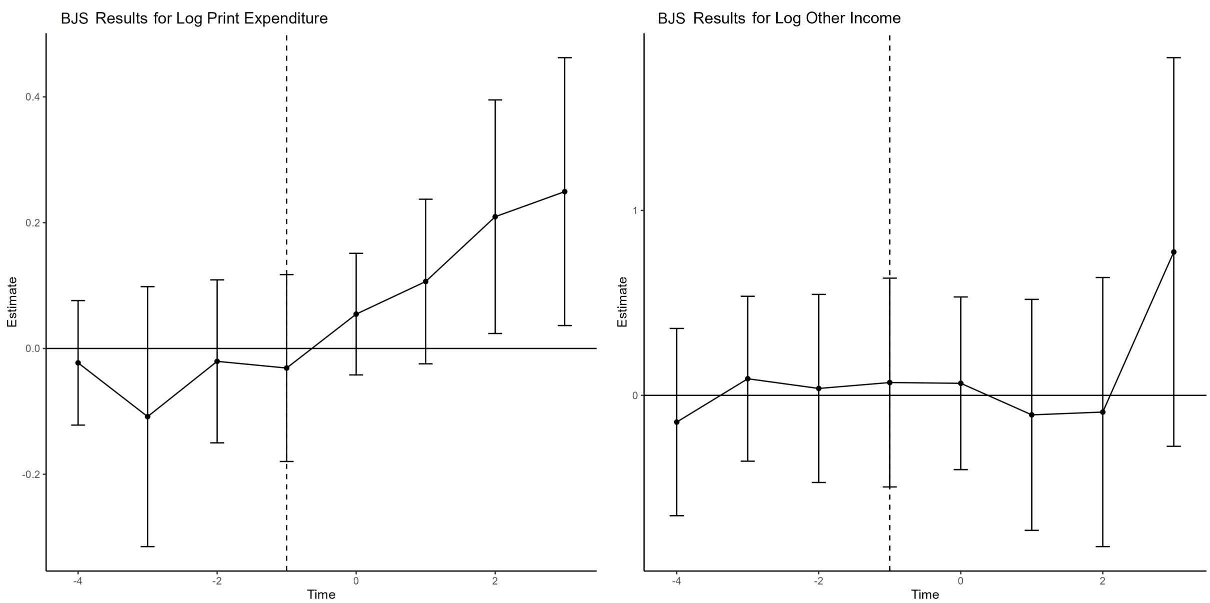
(b) Log Registered Borrowers



(c) Log Total Circulation

Notes: Estimated using Borusyak et al. (2024). Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure A.2.10: Results for Library Finances: Borusyak, Jaravel, and Spiess 2024

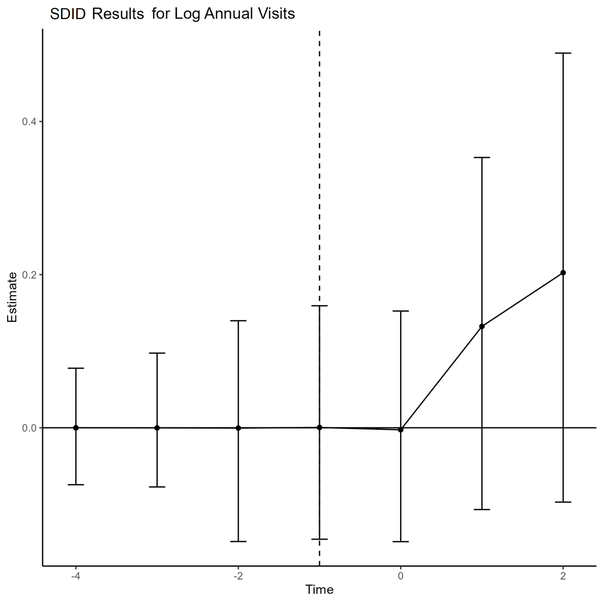


(a) Log Print Expenses (b) Log Other Income

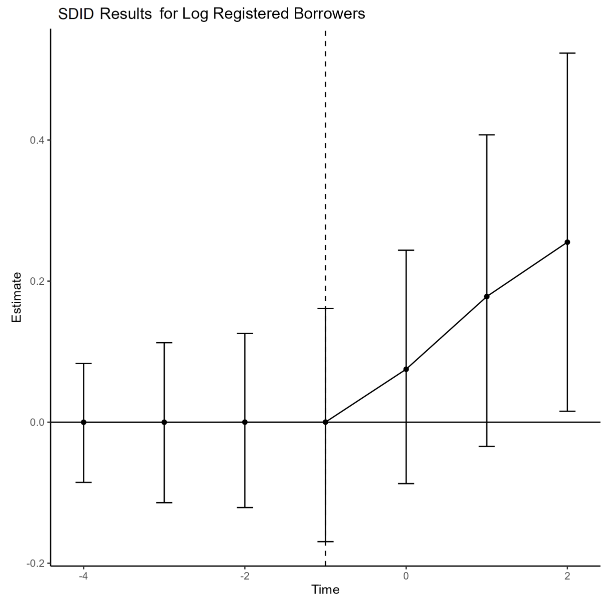
Notes: Estimated using Borusyak et al. (2024). Unbalanced panel used. 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Synthetic Difference-in-Differences (Arkhangelsky et al. 2021)

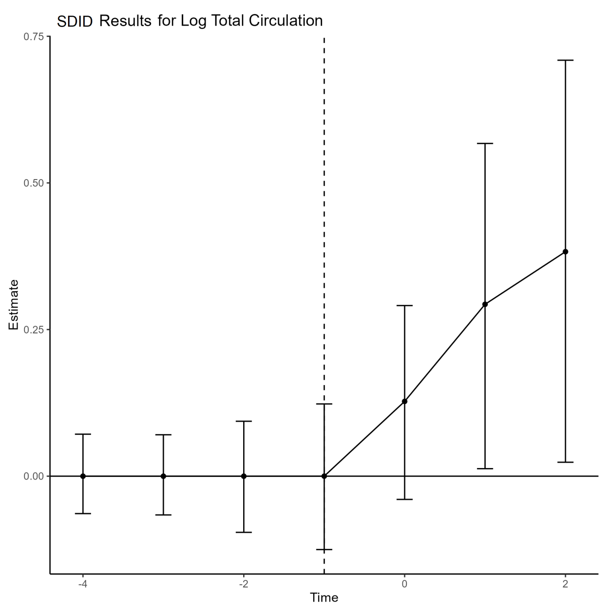
Figure A.2.11: Results for Library Usage: Synthetic DID



(a) Log Total Visits



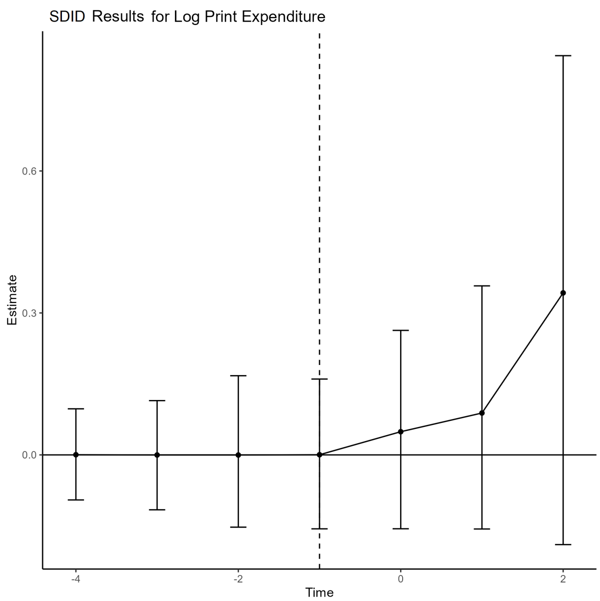
(b) Log Registered Borrowers



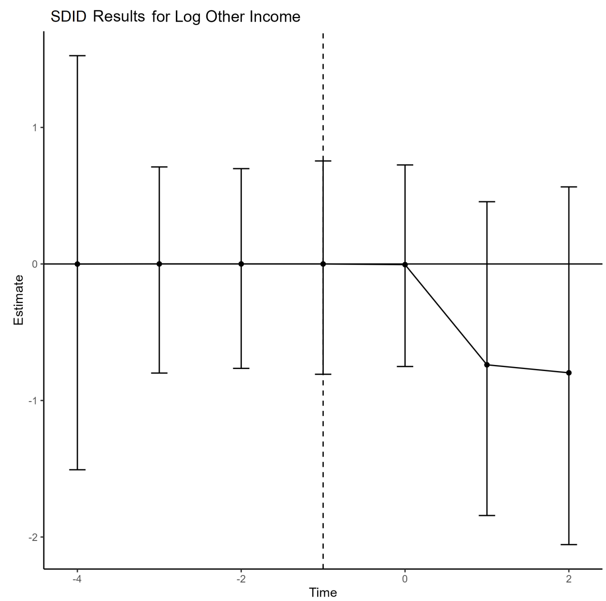
(c) Log Total Circulation

Notes: Estimated using Arkhangelsky et al. (2021). 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure A.2.12: Results for Library Finances: Synthetic DID



(a) Log Print Expenses



(b) Log Other Income

Notes: Estimated using Arkhangelsky et al. (2021). 95% confidence intervals shown. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Chapter 3: Private Philanthropy in Public Institutions: Evaluating Gates Foundation Library Grants

3.1 Introduction

There is a significant "digital divide" in technology access and usage between lower-income and higher-income individuals, a gap that was especially pronounced during the early days of the internet. The rapid but uneven growth of home broadband and internet usage during the late 1990s and early 2000s created these disparities, with low-income and rural communities particularly lagging in adoption. As the internet became increasingly crucial for accessing job opportunities, educational resources, and other services, this divide became even more consequential for disadvantaged populations.

Early efforts to improve access often prioritized communal over individual connectivity, both to reduce costs and to maximize the potential reach of under-served groups. Public libraries were a natural venue of communal internet access, as they are ubiquitous across the US and already serve as community hubs. Many libraries were also well positioned to provide internet access to the community; by 2000, nearly 96% of public libraries were connected to the internet (Bertot & McClure, 2000). However, internet connectivity alone did not guarantee public access. In some libraries, internet access was limited to staff use, and in others, there were too few public-use computers to meet demand. Libraries in rural and low-income areas in particular continued to face significant infrastructure deficits, especially in terms of hardware. These gaps, coupled with the increasing need for internet access in a digital world, prompted philanthropic initiatives aimed at bridging the digital divide. One of the most significant efforts was the Bill and Melinda Gates Foundation Library (BMGF) Grants, which in the late 1990s and early 2000s funded libraries in high-poverty areas across the United States to support the purchase of public-use computers, software, and employee training in the new technology. These grants went to a mix of libraries, some that lacked internet access altogether and others that had internet, but insufficient hardware to serve their communities effectively.

This paper examines the impact of the first BMGF library grant program on public libraries, focusing first on how grant receipt influenced capital spending and public computer availability as direct outcomes of the intervention. In addition, I assess whether the program affected broader aspects of library function, such as usage, spending, and service provision, to capture potential spillover effects beyond technology. I construct a panel dataset of library districts from 1992 to 2007 by merging historical grant data from the BMGF with outcome data from the Public Library Survey (PLS). To estimate the program's effects, I use a staggered difference-in-differences framework, comparing grant-recipient districts to those that did not receive funding. Because grant receipt is not randomly assigned, treated and control libraries differ on observable pre-treatment characteristics. To address this, I estimate a specification that incorporates inverse probability weights (IPW) based on the predicted probability of treatment, thereby improving covariate balance across groups (Abadie, 2005). Given that treatment timing is also non-random, I additionally implement the estimator proposed by Callaway and Sant'Anna (2021), which allows for treatment effect heterogeneity across cohorts and time. In this specification, I use never-treated libraries as the control group and again apply IPWs, reporting average treatment effects aggregated across cohorts.

I begin by examining the effects on capital spending and public use computers, the two outcomes most directly impacted by the program, as purchasing computers qualifies as a capital expense. The results suggest that there was an immediate spike in capital spending following receipt of a BMGF grant, with increases of 40 to 50 percent in the year of grant receipt. This surge is likely associated with the initial purchase of computers, peripheral equipment, and possibly the infrastructure needed to provide a sufficient connection to the internet. Capital spending tends to be lumpy, with many libraries reporting zero capital expenditures in a given year. The decline in capital spending in subsequent years following the grant may reflect a shift in the timing of investments rather than a permanent increase in capital outlays. It is also possible that the grants accelerated planned computer adoption among libraries that would have adopted eventually. Consistent with the program's objectives, I also find evidence of an increase in the number of public-use computers, though the interpretation of this effect is complicated by problematic pre-trends, and all other effects should be viewed cautiously given differential pre-trends in a number of outcomes.

After examining these most direct outcomes, I turn to broader measures of library usage and services to assess whether the grants affected other aspects of library function. There is some evidence of a short-run increase in library visits, but these effects are not robust across specifications and tend to dissipate over time. Similarly, the effects for total circulation are not robust across specifications and often have a statistically significant differential pre-trend that limits the interpretation. For library expenses, I find a negative but inconsistently significant effect on total collection expenditures but consistent declines in operating expenses. The reduction in operating expenses may be related to a decline in hours open, as I find a robust negative effect on library hours following grant receipt.

Overall, the results suggest that while the BMGF grants may have expanded technological access in the short run, they did not lead to broad or sustained improvements in library usage or services. The lack of robustness across specifications and the presence of differential pre-trends in several outcomes underscore the need for caution in interpreting these results and highlight the broader challenges in rigorously evaluating the effects of this program. While I do not conduct a formal welfare analysis, the results do not provide a clear picture of whether the program improved welfare. Computer users at the library may be better off, especially if they use the computers to access educational resources or search for employment; however, if there was a reduction in hours and in collection spending, then users of traditional library services could be made worse off. These findings raise questions about whether the benefits of improved computer access were enough to justify the program's costs and potential trade-offs in other library services.

The remainder of the paper is organized as follows. Section 3.2 provides an overview of the relevant institutional details including the digital divide, the Bill and Melinda Gates Foundation Library Grants, and the relevant existing literature. Section 3.3 describes the analysis inducing the data used in this study and the empirical approaches. In Section 3.4, I present the results of the analysis. Finally in Section 3.5 I conclude.

3.2 Institutional Details

3.2.1 Internet Access in the Late 1990's and Early 2000's

In the late 1990s and early 2000s, the internet became an increasingly vital tool for education, work, and communication, but its availability was not equally distributed across different populations. There were significant disparities in how access to the internet expanded along racial and ethnic, income, and geographic boundaries. This led to a digital divide, where lower-income, rural, and minority communities faced substantial barriers to internet access. In both the 1995 and the 1999 National Telecommunications and Information Administration (NTIA) reports on the digital divide, lower income households were the least likely to have access to or use the internet, with only 12% of individuals making $5,000 to $9,999 annually using the internet compared to 58.9% of individuals making $75,000 or more (McConnaughey et al., 1995, 1999).

Additionally, the Pew Research Center found that by 2000, internet usage was expanding rapidly, but adoption rates among individuals with lower incomes and less education lagged well behind those of their wealthier, more educated counterparts (Sidoti & Dawson, 2024). Similarly, Goldfarb and Prince (2008) found evidence of a digital divide, with high-income, higher educated people more likely to use the internet by the end of 2001. At the same time, the value of internet access was growing rapidly. It was becoming a resource for finding jobs, learning about government programs, accessing educational resources, and staying connected in an increasingly digital world. This led to growing concerns that low-income and rural communities would be left further behind, deepening existing inequalities.

By the late 1990s, public libraries were already emerging as natural hubs for internet access in under-served communities. Most libraries had internet connections, 83.6% by 1998, making them a promising vehicle for expanding community access (Bertot & McClure, 2000). While the connection infrastructure was largely in place, the availability of computers for public use lagged behind. In 1998, just 73.3% of library outlets offered public access to the internet, with much lower shares in low-income and rural communities. Additionally, most libraries did not have a sufficient number of public computers to meet the growing demand. This gap between connectivity and meaningful public access underscored a key remaining challenge: libraries had network access, but many lacked the equipment, staff capacity, and training needed to serve as true digital access points. The BMGF recognized the potential of libraries and started the Library Grants program with a goal of expanding digital access in under-served areas by funding the purchase of computers, networking equipment, and technical support for public libraries.

3.2.2 The Bill and Melinda Gates Foundation

The BMGF's first major library grant program ran from 1997 through 2003 and aimed to supply libraries in low-income communities with the necessary hardware, software, and training to support internet access. Grants were awarded in four rounds and involved a total funding commitment of $400 million, with half provided by the Gates Foundation and the other half in the form of software donations from Microsoft. In this paper I specifically focus on the grants awarded to individual public libraries.[1] Libraries were assigned to a treatment round based on their state's 1990 Census poverty rate. Generally, worse off states were assigned to earlier rounds, as illustrated in Figure 3.1.

[1]In the first grant program, there were other grants given out to state libraries as well, but those cannot be tied directly to library outcomes so they are not included in my study.

During their assigned treatment round, individual public libraries qualified for the grant if they were in areas with a poverty rate of at least 10 percent, also based on 1990 Census data. Libraries meeting this criteria could apply for funding through state-administered partnerships, with states responsible for identifying and communicating with eligible libraries and then distributing grants within their jurisdiction. The Gates Foundation specified that grant funds had to be used within a year from receipt, as outlined in the grant agreement. Libraries were also subject to reporting requirements, which included submitting annual updates on project progress, financial expenditures, and overall outcomes. These stipulations were intended to ensure that funds were used appropriately and effectively to expand public technology access (BMGF, 2022).

Although there were only four rounds of state eligibility, the actual awarding and disbursement of the grants rolled out over longer periods of time. Grant receipt took as long as four years following the initial year of state eligibility, as shown in Figure 3.2. Grant sizes also often deviated from the program's original guidelines, which suggested that awards should scale with library size and need, ranging from a minimum of \$4,000 to a maximum of \$30,000. As shown in Figure 3.3, most grants fell within this range with an average of roughly \$21,000 and a median of \$12,000; however, a substantial number of grants exceeded the initial cap.[2] This reflects programmatic flexibility as the Foundation adjusted its approach to meet the needs of larger or more complex library systems. Importantly, these grants were not trivial relative to library budgets: the average grant equaled 23 percent of a library's total annual income, with a median of 10 percent. Figure 3.4 illustrates the distribution of grant size as a share of annual income, which is similarly right skewed, with most grants falling between 0 and 25 percent.

Following the initial 1997–2003 program, the BMGF continued to support public libraries

[2]Grant sizes reported in \$1992 dollars.

through a series of subsequent initiatives, including Staying Connected (2004), Opportunity Online (2007–2009), and targeted broadband expansion efforts (2008–2009). Unlike the original grants, these later programs typically required local matching funds, intended to encourage long-term sustainability of technology investments. While these initiatives are also of interest, this paper focuses exclusively on the first grant program, which featured more consistent eligibility criteria and treatment assignment. It also reached a larger number of libraries, providing greater statistical power. As detailed in the data section, I exclude libraries that received support from these later efforts during the sample period to isolate the effect of the original program.

In this paper, I evaluate both the outcomes most closely related to the program's goals, capital spending and public use computers, as well as a range of outcomes focused on library use, operations, and spending. Looking at this broader set of outcomes allows me to assess whether libraries and users are responding to the grant on a variety of margins.

3.2.3 Prior Literature

Although public libraries are a long-standing institution in the US, relatively few economics papers have credibly estimated the causal effects of library investments or services. Much of the existing research on public libraries comes from the library sciences literature, which offers descriptive insights and case studies but typically does not employ causal identification strategies. A growing number of recent studies in economics have begun to fill this gap by leveraging policy variation and investment shocks for identification.

This paper is most closely related to the literature on library access and investment. For example, Gilpin et al. (2024), estimated the causal effects of large capital investments in public libraries on usage and reading test scores. They found that capital spending increased visits, children's circulation, and attendance at children's programs by 5–15 percent in the years following investment. They also found modest positive effects on children's reading scores. Bhatt (2010) instrumented for library use using distance to the nearest library and found that library usage increased children's reading time and homework completion while reducing television watching. Bekkerman and Gilpin (2013) found that increased residential access to high-speed internet in a library's service area boosted demand for library services, suggesting that internet access is a complement to rather than a substitute for libraries. Related work by Rodríguez-Lesmes et al. (2014) used a difference-in-differences approach to examine how new library construction in Bogotá impacts test scores and found no effects. Other papers linked library investments to reductions in crime (Neto, 2019; Porter, 2015) or to changes in local labor market outcomes (Ferreira Neto, 2023). And in Chapter 2, I examined the effects of removing overdue fines on library usage and finances and found that fine-free policies increased usage without compromising revenue.

Several other studies have used historical expansions of library access to estimate the long-run effects of public library investment. A notable example is the early 20th-century Carnegie library program, which built 1,689 libraries in the US alone between 1883 and 1929 and which has served as a natural experiment in several papers. For example, Karger (2021) found that access to a Carnegie library increased long-run educational attainment and earnings. Berkes and Nencka (2024) found that Carnegie libraries increased local patenting rates, while Kevane and Sundstrom (2016) reported no effects of Carnegie libraries on political participation. These studies generally relied on the fact that Carnegie libraries were often placed in communities that lacked existing access to library services, and by comparing them to similar communities that did not build libraries are able to identify the effect of library access. My paper relates to this literature by examining a more recent and distinct form of library investment—private grants for service improvements—assessing their short-run effects on library services, utilization, and spending.

More broadly, this paper also contributes to the literature on public goods provision and nonprofit financing. Public libraries are often framed as local public goods, and the case for government or philanthropic support stems from their expected positive externalities. Early theoretical work outlined these features (Tiebout & Willis, 1965), while later empirical work evaluated the efficiency of library operations or funding (DeBoer, 1992; Feldstein, 1977; Getz, 1980; Vitaliano, 1997, 1998; Worthington, 1999). This paper also relates to a broader literature on philanthropic giving and nonprofit finance, including studies of how private contributions respond to public funding and how funders affect the provision of public goods through nonprofits (Andreoni, 2006; Andreoni & Payne, 2011; Karlan & List, 2007).

Although BMGF internal analyses of the grant program exist, none are publicly available.[3] To my knowledge, this paper is the first to causally evaluate the effects of large-scale, targeted library funding from a private foundation in the modern era. The BMGF library initiative represented one of the largest philanthropic efforts in the US library sector, distributing more than $400 million in grants, software, and training during the late 1990s and early 2000s.

[3] In the course of writing this paper, I contacted the Gates Foundation to request access to these internal reports but was denied.

3.3 Analysis of BMGF Grants

3.3.1 Data

In this section, I will begin by describing the data sources used in this analysis. I then outline the steps taken to construct the final analytic sample and compare the characteristics of districts that were removed versus retained, as well as those that received grants versus those that did not.

3.3.1.1 Data on Public Library Outcomes

The data for public libraries are from the Public Library Survey (PLS). The PLS is a voluntary annual survey of all public libraries in the US with data available starting from 1992. Observations are annual library district-level measurements, with variables including revenue sourced, expenditures by category, collection and operation details, and utilization of library services (IMLS, 2021).[4] I use this survey to create my main dataset, a panel of public libraries in the US between 1992 and 2007.[5] A key advantage of this dataset is its longitudinal structure, which enables tracking the same library districts over time. By analyzing changes within the same libraries over multiple years, I can account for unobserved, time-invariant factors that might otherwise bias the estimation. My main outcome variables are from the PLS and include capital spending, the number of public use computers, total visits, total circulation, collection expenditure, operational expenditure, hours open, and reference transactions.

[4] Despite being a voluntary survey, the PLS has high response rates. The average unit response rate between 1992 and 2007 was 97.7 percent, with a minimum response rate of 96.4

[5] All years refer to fiscal rather than calendar years.

3.3.1.2 BMGF Grant Data

The Gates Foundation maintains a public database of all grants awarded across its various programs (BMGF, 2024). This dataset includes information on the type of grant program, recipient organization, date of disbursement, and award amount. I restrict the data to grants related to the Foundation's libraries initiatives, specifically those awarded to institutions in the United States. The resulting dataset is merged with PLS data using library name and state while city name is used to confirm and assess the quality of each match.

3.3.1.3 1990 Census data

I use data from the 1990 Census at the ZIP code-level to assess baseline socioeconomic characteristics of library districts prior to the introduction of the Gates Foundation grants (Manson et al., 2024). Specifically, I match each library in my panel to its corresponding ZIP code in the 1990 Decennial Census to obtain estimates of the local poverty rate, racial and ethnic composition, household and per-capita income, education levels, and public assistance receipt. I use these data to perform balance checks, assessing whether observable pre-treatment characteristics are similar across libraries receiving grants at different times. Importantly, I use the 1990 poverty rate to examine whether baseline economic conditions are correlated with the timing of grant receipt.[6]

[6] I was unable to match a small subset of libraries to the 1990 Census data because of inaccurate ZIP codes for libraries and chose not to use the 1990 Census for calculating IPWs rather than drop the districts from the analysis.

3.3.1.4 Sample Creation and Summary Statistics

I define my sample period as 1992 through 2007, ending prior to the Great Recession to avoid the confounding effects of the economic downturn. The panel data from 1992 to 2007 form my estimation sample; however, I also use data from 1992 to 1996 to construct pre-treatment characteristics for calculating IPWs, as the first round of grants began to be disbursed in 1997. I aim to assess the effect of grants on traditional public libraries. To that end, I exclude libraries that are only bookmobiles, law libraries, and those associated with schools, museums, or tribes, as these types have distinct funding structures, governance, and service models that make them less comparable to traditional public libraries. This exclusion removes 151 libraries (54 treated, 97 control). After refining the population to traditional public libraries, I exclude libraries missing three or more consecutive years of data, as well as those with fewer than five observations during the sample period, to ensure data quality.[7] These exclusions primarily affect control libraries: 203 non-grant libraries are removed compared to 43 libraries that received grants.

As mentioned previously, I focus on the first BMGF grant program, which distributed funds between 1997 and 2003. Beginning in 2004, the BMGF launched subsequent grant programs with different eligibility criteria and intended uses. To ensure clean identification of the original program's effects, I exclude libraries that received multiple grants before 2004 or received a grant between 2004 and 2007, my post-treatment period. Excluding these libraries helps isolate the impact of the initial grants without contamination from other interventions. Specifically, I drop 55 libraries that received multiple pre-2004 grants, 93 libraries treated between 2004 and 2007, and 35 libraries that meet both criteria. I retain libraries that received grants after 2007, as

[7] For one year or two consecutive years of missing data, I impute values using the average of the surrounding years to maximize the number of libraries retained in the sample.

these later treatments fall outside my sample period and do not influence the outcomes I measure.

After applying these exclusions, my final analytical sample consists of 8,635 libraries, with 4,608 treated and 4,027 control units. Table 3.1 shows the frequency of sample exclusions by treatment status. Most dropped libraries are control units, with missing or insufficient data as the primary reason for exclusion. Among treated libraries, the most common reason for exclusion is receiving multiple grants before 2004 and/or receiving a grant between 2004 and 2007.

I begin by examining characteristics of my sample, both overall and by grant receipt status, as shown in Table 3.2. As before, the top panel displays average characteristics from the PLS between 1992 and 1996, while the bottom panel presents 1990 Census characteristics of the ZIP code associated with the library district. Overall, treated (grant-receiving) and control (non-grant-receiving) districts are similar in many respects but exhibit some notable differences. Treated libraries tend to be larger in some regards, serving an average population of 23,049 compared to 16,150 for control libraries. They also have more outlets (1.78 vs 1.29) and longer annual operating hours (3,502 vs 2,585). However, there are no statistically significant differences in total employees or librarians. Collection sizes are also similar, with no significant differences in books, audio, or video items.

In terms of funding, treated libraries receive significantly more federal revenue ($4,931 vs $2,937). However, total revenue and operating expenses do not differ significantly between the two groups. Treated libraries are in more racially diverse and economically disadvantaged areas. On average, they serve ZIP codes with a lower share of white residents (90.2% vs 94.8%) and a higher share of Hispanic residents (4.4% vs 2.7%). These areas also have significantly higher poverty rates (16.1% vs 8.2%), a larger share of households receiving public assistance (8.3% vs 4.9%), and lower educational attainment, with 28.8% of adults having less than a high school diploma compared to 20.5% in areas with non-grant libraries. Median household income is lower ($23,632 vs $35,440), as is per capita income ($11,057 vs $15,631). The lack of balance between the treated and control districts is not surprising, given that only libraries in disadvantaged areas were eligible for the grants. However, this weakens support for the parallel trends assumption: the less similar the two groups are, the less likely it is that they would have trended together in the absence of the grant. As discussed further in Section 3.3.2, I supplement my baseline estimates with an analysis based on a re-weighted sample that better reflects the treated population.

3.3.2 Empirical Approach

I exploit variation in BMGF grant receipt across public library districts and across time to estimate the causal effect of receiving a grant on library outcomes. Ideally, I would be able to measure how different subgroups of patrons react to the grant and subsequent increase in public use computers. More specifically, it would be interesting to know if the response varied by patron income or education. However, the PLS data are only available for library-level outcomes, so I am unable to look at anything more granular. I focus on four broad categories of outcomes. I begin by looking at the outcomes most directly impacted by the policy: public use computers and capital expenditure. If the grants are successfully used to purchase computers, one would expect to see an increase in capital spending and an associated increase in computers available. While a measure of internet or computer users would provide a more direct indicator of how patrons responded to the expansion in technology access, such data are not available in the PLS until 2006, after all grants had already been awarded. Even data on the number of public use computers is only available beginning in 1998, which means this outcome can only be observed for libraries treated in the later years of the grant program. As a result, estimates of treatment effects on public use computers are identified using variation in treatment timing among this later group of recipients.

I also assess more secondary effects of grant receipt on other aspects of library operations. I examine measures of library utilization, including annual visits and total circulation, to assess whether the grants influenced engagement with library resources, both traditional services like checking out books or new digital resources. In addition, I analyze library budgets, including collection and operational expenditure, to determine how the grants impacted library spending patterns. Although earmarked for computer purchases, the grants function as an increase to total income and as such may lead libraries to reallocate their non-earmarked income. Finally, I investigate changes in indicators of other library services including hours open and reference transactions. Hours open could adjust in response to demand, while reference transactions could be either a substitute for internet use or a compliment if patrons require assistance with their internet use. To account for the skewed distribution of most of the outcome variables, I log-transform them prior to estimation and interpret results as percent changes.

3.3.2.1 Two-Way Fixed Effects Estimation

Libraries received grants at different times between 1997 and 2003. I begin by estimating an event study two-way fixed effects (TWFE) regression to account for staggered treatment timing. The baseline specification is:

$$Y_{i,t} = \sum_{l \neq -1} \beta_l T^l_{i,t} + \mu_i + \phi_t + \epsilon_{i,t} \tag{3.1}$$

where $Y_{i,t}$ is the log transformed outcome, l is the number of periods since treatment, $T_{i,t}^{l}$ is a dummy variable that takes a value of 1 if library district i in period t was treated l years ago, μ_i are the library district fixed effects and ϕ_t are the year fixed effects. The causal interpretation of the coefficients β_l is the average effect of receiving a grant on the outcome of interest l years since treatment. I report standard errors clustered at the library district level, adjusted for multiple inference using the Benjamini and Hochberg (1995) adjustment method.

For this approach to be informative about the casual effect of BMGF grants, it requires several identification assumptions. The first is that of no anticipation, that is, libraries did not adjust their behavior in the periods preceding grant receipt. This assumption is reasonable given the available documentation and timing of the program. Most libraries were informed of their eligibility shortly before receiving the grant, leaving little opportunity to alter their behavior in advance. Moreover, because I measure outcomes at an annual level, libraries would have had to anticipate the grant well in advance of formal notification to affect measured pre-treatment outcomes. The second identifying assumption is the parallel trends assumption—that in the absence of treatment, treated and untreated libraries would have experienced similar trends in outcomes. I assess the plausibility of this assumption by looking at pre-treatment values of β_l. While not a conclusive test, statistically insignificant values of β_l would suggest that treated and untreated libraries followed similar pre-treatment trends, that those trends would have continued in the absence of treatment.

3.3.2.2 Re-Weighted TWFE Estimation

While the TWFE regression accounts for unobserved time-invariant heterogeneity, observable differences in pre-treatment characteristics between treated and untreated libraries may indicate that the control group is not a suitable counterfactual. In particular, if treatment assignment is correlated with covariates that also influence the outcome, the parallel trends assumption is violated, leading to biased estimates. To address this concern, I re-weight the sample to improve balance between the treatment and control groups and improve the plausibility of the parallel trends assumption. Specifically, I implement an event study version of the semiparametric difference-in-differences estimator proposed by Abadie (2005), whereby untreated units are weighted by inverse probability weights of their probability of being treated.

This approach involves estimating the probability of treatment as a function of pre-treatment characteristics using a logistic regression model. I construct these covariates as averages over the 1992–1997 period.[8] The inverse of these estimated probabilities (defined as $P(\text{Treated}_i = 1|X_i)/(1 - P(\text{Treated}_i = 1|X_i)))$ is then used as weights for the untreated units in equation 3.1, again I report standard errors clustered at the library district level, adjusted for multiple inference using the Benjamini and Hochberg (1995) adjustment method. To assess the validity of the re-weighting procedure, I examine covariate balance between the treated and control groups after weighting; results are reported in Appendix 3.A.2.

This method requires one additional assumption and a refinement of a standard TWFE assumption. First, it assumes overlap in the distribution of propensity scores, specifically, that

[8]Covariates used to estimate the propensity score include: total visits, legal service area population, number of outlets, total circulation, number of books, audio units, video units, reference transactions, attendance at children's programs, hours open, number of librarians, total staff, total income, income from federal, state, local, and other sources, capital expenditures, collection expenditures, and total operating expenditures.

the support of the propensity score for treated units lies within the support of the control group. This ensures the existence of comparable control units for each treated unit. Second, the parallel trends assumption must hold conditional on covariates; that is, in the absence of treatment, the weighted average outcomes of treated and control libraries would have followed similar trends, conditional on the observed characteristics. In this context, it is reasonable to assume that I observe some, though likely not all, of the characteristics that influence treatment assignment. As a result, while re-weighting should mitigate bias arising from observable differences, it may not fully account for unobserved factors that jointly affect both treatment status and outcomes.

3.3.2.3 Recent Developments in Staggered DID

While the re-weighting procedure described above improves pre-treatment covariate balance between treated and untreated libraries, it is not fully satisfactory in the context of staggered treatment adoption. The method involves estimating a single set of propensity scores based on pre-treatment characteristics and applying these weights uniformly over time. However, both the treated and control populations evolve over time as additional libraries become treated. The weighting approach in a TWFE setting cannot accommodate these dynamic changes in the composition of the comparison group.

Another concern is the potential endogeneity of treatment timing. As noted earlier, state-level eligibility for the grant program was determined in four distinct waves, based on poverty rates from the 1990 Census. If more disadvantaged library districts are concentrated in states that became eligible earlier and if treatment effects vary with baseline disadvantage, then the timing of treatment is endogenous to unobserved characteristics that are correlated with the outcome.

In this case, the earliest-treated libraries would systematically differ from those treated later or not at all, and the β_l coefficients would reflect comparisons between groups that differ along dimensions related to both treatment and potential outcomes. Appendix 3.A.3 provides evidence that libraries serving higher-poverty areas were disproportionately treated in the earlier waves.

To address these limitations, I also run the estimator proposed by Callaway and Sant'Anna (2021), which is designed for settings with staggered treatment adoption and treatment effect heterogeneity. This method estimates average treatment effects for each cohort defined by treatment year and then aggregates these to compute an overall average treatment effect. I incorporate the inverse probability weights from Abadie (2005) when estimating the cohort-specific effects, allowing me to account for differences in observed characteristics across cohorts and address the evolving composition of the treated and control groups over time. Importantly, this method also avoids contamination from comparisons between units treated at different times, a source of bias in traditional TWFE estimators. Given these features, this approach is particularly well suited to my setting, where treatment timing is not plausibly exogenous, treated units differ systematically from untreated units, and the composition of treated and control units changes over time. Specifically, I estimate cohort-by-time average treatment effects using equation 3.2, and then construct average effects by time since treatment using equation 3.3:

$$\widehat{ATT}(g,t) = \frac{1}{N_g} \sum_{i:G_i=g} [Y_{i,t} - Y_{i,g-1}] - \frac{1}{\sum\limits_{i:G_i \in \mathbb{G}_{comp}} \pi_i} \sum_{i:G_i \in \mathbb{G}_{comp}} (\pi_i \times [Y_{i,t} - Y_{i,g-1}]) \tag{3.2}$$

$$ATT_l = \sum_g w_g ATT(g, g+l) \tag{3.3}$$

where g indexes treatment cohorts defined by treatment year, t indexes the year, while l indexes time since treatment. π_i is the inverse probability weight, defined as $P(\text{Treated}_i = 1|X_i)/(1 - P(\text{Treated}_i = 1|X_i))$. The X_i used include the same pre-treatment characteristics used for the re-weighted TWFE. I use never-treated library districts as the comparison group to ensure a stable and uncontaminated counterfactual across treatment cohorts.[9] I report bootstrapped standard errors and confidence bands that are robust to multiple hypothesis testing.

This method relies on three key identification assumptions. First, treatment must be an absorbing state, a condition that holds by construction, as libraries were only eligible to receive a single grant under this program.[10] Second, the method assumes no anticipation of treatment, consistent with the assumption required for the TWFE estimator. Third, it requires parallel trends in the absence of treatment. As with TWFE, I examine the estimated pre-treatment coefficients to assess the plausibility of this assumption.

3.4 Results

3.4.1 TWFE Analysis

I begin by presenting results from the TWFE regression without weights as a baseline. However, as discussed previously, this specification is the most likely to violate the parallel trends

[9]As a robustness check, I re-estimate the effects using not-yet-treated units as the comparison group; results are presented in Appendix 3.A.4.

[10]The BMGF dataset identifies grants only as "library" grants without indicating the specific funding initiative. As a result, a small number of grants included in the analysis may not correspond to the program of interest. To partially mitigate the effect of this, I exclude libraries that appear to receive multiple grants or grants outside the designated eligibility period.

assumption, particularly due to systematic differences between treated and untreated libraries and endogenous treatment timing. Figure 3.5 displays the results for capital spending and the number of public use computers, while Table 3.3 displays the numeric results. Given the program's goal of increasing publicly available computers—and the fact that expenditure on computers falls under capital expenditure—these are the two outcomes where we might most expect to see an effect. For capital spending, the event study reveals an increase of 48 percent in the year of treatment, followed by a decline over years one through five. For public use computers, the event study shows a large and stable increase of around 25 percent after treatment. However, both outcomes have statistically significant pre-trends, which complicate interpretation and suggest that the identifying assumptions required for causal inference may not fully hold. In addition, as mentioned previously public use computers as an outcome are only available beginning in 1998, limiting comparability.

I then turn to potential effects beyond the direct acquisition of computers. The BMGF grants may have influenced broader aspects of library services, for instance, by prompting libraries to change their digital presence or reallocate spending based on ongoing internet and technology costs. Patron behavior could also shift, with individuals using a different mix of services or changes in the composition of users themselves, as some groups start or stop using the library. However, because my data are limited to the library level, I can only observe aggregate patterns of usage rather than individual behavioral changes.

To explore how the Gates Foundation grants affected library utilization, I begin by analyzing total annual visits and total circulation. Annual visits measure how many times people come into the library, which would include changes from both the internet-using patrons and patrons who only consume other library services. Total circulation, on the other hand, captures whether patrons are adjusting their consumption of borrowing books and other materials, a more traditional library service than internet use. As shown in Figure 3.6 and Table 3.3, total visits increase in the year of adoption and remain elevated for two subsequent years before returning to baseline levels, with statistically significant effects of roughly 3 percent during the peak years. In contrast, total circulation exhibits statistically significant differential pre-trends, suggesting a violation of the parallel trends assumption. The presence of differential pre-trends in some of my outcomes warrants a cautious interpretation of the findings across all outcomes. Taken together, these patterns potentially suggest that the grants may have increased general library attendance in the short run, but the evidence on longer-run effects or changes in traditional service usage is wholly inconclusive.

To assess the margin of how libraries may have adjusted their spending in response to the grant, I examine effects on collection and operating expenses. BMGF grants could increase library interest in technology through the exposure or training, leading to a shift in spending that may affect collection or other service spending. In the baseline TWFE, collection expenditure declined by between 1.8 and 6.2 percent in the post-treatment period, with mixed statistical significance. This trend suggests a potential reallocation away from collection expenditure, though the effect is not strong or consistent enough for drawing firm conclusions. For operating expenses, I find a modest increase of about 2.2 percent in the year of treatment, followed by a declining trend in subsequent years. This pattern, along with the capital spending results, is consistent with libraries front-loading their investment, spending initially on both capital and operating costs to implement the new technology, but not sustaining these expenditures over time. This may help explain why future rounds of BMGF library grants typically required matching funds as evidence of ability to generate and sustain funding.

Finally, I consider library service adjustments by evaluating hours open and reference transactions. Hours open serves as a measure of service intensity, with more hours indicating greater availability of library resources and staff. Meanwhile, reference transactions capture patron engagement with librarians and information services. These interactions may increase with greater library use or to learn more about the new computers but could also decline over time if patrons begin substituting librarian assistance with internet searches. For hours open, I find a steady decline following adoption, reaching a 5 percent reduction by five years post-treatment, but with a strong differential pre-trend that limits my ability to attribute the change to the grant receipt. For reference transactions, the event study reveals positive and statistically significant differential pre-trends that reverse post-treatment. Again, the presence of the differential pre-trend indicates a violation of the parallel trends assumption, limiting interpretation.

3.4.1.1 Re-Weighted TWFE

The statistically significant differential pre-treatment effects for several outcomes—particularly total circulation, capital spending, public use computers, and reference transactions—suggest that there may be a violation of the parallel trends assumption, because the full population of untreated libraries are poor controls for the treated libraries. To address this concern, I re-estimate the TWFE regression using IPW weights of treatment to reduce the differences in baseline characteristics between treated and control libraries. Re-weighting on propensity to be treated improves the balance between treatment and control groups on pre-treatment characteristics, improving the plausibility of the parallel trends assumption. The event study results for the weighted TWFE are shown in Figures 3.7 and 3.8, with numeric results presented in Table 3.4.

For capital expenditure the results remain largely unchanged compared with the baseline TWFE. There are still statistically significant differential pre-trends, a large increase in the year of adoption (about 40 percent), followed by a similar magnitude decline for years two through five. Public use computers again show large positive post-treatment coefficients, but the consistent presence of statistically significant differential pre-trends complicates causal interpretation. In the re-weighted TWFE, the increase in total visits post-treatment remains, with statistically significant increases persisting for the first four years, peaking at approximately 2.7 percent, roughly the same as the baseline TWFE. For total circulation, the differential pre-trend is slightly weaker, but still problematic for interpretation. This pattern suggests that while it is possible that grant receipt increased visits to the library, the evidence does not strongly support that grants led to an increase in borrowing activity, although all results are tentative given the presence of differential pre-trends.

The results for collection expenditure are still generally statistically insignificant, although a couple of post-treatment coefficients are significant and negative. Operating expenses continue to show a modest 2.5 percent increase in the year of treatment, followed by a steady decline from years one through five. This suggests the sign of the increase in the year of treatment and subsequent decline may be a robust finding, though no strong conclusions about the exact magnitude of these effects should be drawn. The pre-trends for hours open are improved in the re-weighted TWFE, and there is still evidence of a post-treatment decline with effects ranging from -1.9 to -3.6 percent. The differential pre-trend for reference transactions remains problematic, so I do not attempt to make a claim about the effect of the grant. The results for these outcomes suggest that re-weighting was able to improve the plausibility of the parallel trends assumption at least for some outcomes, though I still do not take any results as conclusive, given the remaining concerns with pre-treatment trends.

3.4.1.2 Conclusions from the TWFE Results

Overall, the TWFE estimates suggest that the Gates Foundation grants were likely able to increase the number of public use computers, through an initial capital spending event. All results are difficult to interpret exactly given the presence of differential pre-trends. If anything, the results suggest the grants may have led to short-term increases in library attendance, though there is no evidence of increased use of core services such as borrowing materials. The results on spending suggest the possibility of some reallocation of funds, with potential declines in collection spending and declines in operations spending. There is also evidence of a potential associated decline in hours open.

For some outcomes, re-weighting appears to improve the fit between the treated and control groups, though it does not completely resolve issues with differential pre-treatment trends, and as such, no results should be viewed as conclusive. Overall, the TWFE evidence points to the potential short-run increases in library visits, but no evidence of increased use of traditional services such as circulating materials. There is some limited evidence of longer-run decreases in operations spending and hours open. This suggests that libraries and patrons could be adjusting their service offerings and usage in response to the grant, but differential pre-trends make it difficult to draw clear conclusions about the magnitude or persistence of these changes.

3.4.2 CSA Analysis

While re-weighting improves balance on observed pre-treatment characteristics, concerns remain as the implemented re-weighting cannot address changing sample characteristics over time and potential endogeneity in treatment timing. Specifically, calculating weights once on the full untreated and treated populations ignores the fact that different library districts are treated over time, shifting from untreated to treated. Later post-treatment estimates in the TWFE event studies rely increasingly on comparisons to a selected group of untreated libraries—those never treated or treated much later—which may systematically differ from earlier-treated units.

To address these related concerns, I implement the estimator proposed by Callaway and Sant'Anna (2021) (CSA), using only never-treated libraries as the comparison group.[11] Here, I estimate treatment year cohort-specific effects and average them to obtain time-since-treatment ATT's. Within each treatment cohort, I continue to apply IPW weighting to improve covariate balance, as never-treated libraries differ from treated ones on key characteristics. This approach allows for more credible comparisons at the cohort level, helping to mitigate potential bias from endogenous treatment timing.

Figures 3.9 and 3.10 present the dynamic treatment effects from the CSA method. As expected, the estimates in general exhibit wider confidence intervals than the TWFE specifications, reflecting the smaller and more selective comparison group. The spike in capital spending remains robust to the change in specification, with an estimated increase of about 41 percent in the year of grant receipt, followed by a steady decline over the next five years of between 50 and 70 percent. This pattern suggests that libraries may have concentrated their spending upfront, possi-

[11] In Appendix 3.A.4 I also report the results using not-yet-treated.

bly making one-time investments in equipment or infrastructure, before reducing capital expenditure in subsequent years. For public use computers, the estimates continue to show statistically significant coefficients in the pre-treatment period, suggesting the parallel trends assumption is still violated. However, the large and stable positive post-treatment coefficients remain, still potentially suggesting an increase in the number of public use computers following grant receipt. While the presence of a differential pre-trend complicates interpretation of the precise effect size, the direction and persistence of the post-treatment effects across all methods point to a likely positive impact on the number of computers available to the public following receipt of a BMGF grant.

For total visits, the post-treatment effect has reversed from a positive post-treatment effect to a declining trend starting in the year of grant receipt and reaching -20 percent by five years post-grant receipt. In contrast, total circulation now has statistically significant pre-treatment coefficients, but continues to exhibit a downward trend after adoption, reaching a statistically significant decline of -8.7 percent five years post-treatment. If we view this specification as the most credible—accounting for both baseline differences and treatment timing heterogeneity—then the results may suggest the program at least did not increase library utilization.

The CSA estimates for collection expenditure are similar to the baseline and re-weighted TWFE results, with some statistically significant negative post-treatment coefficients. At most, this suggests libraries may have reduced spending on the collection in response to the grant. In contrast, the spike in operating expenses in the year of treatment is gone; however, the sustained decline in years one through five remains. This pattern reinforces the interpretation that libraries did not maintain elevated spending levels over time. This pattern may be in some way related to the reduced operating hours, as hours open continue to show a negative post-treatment effect of up to 10.3 percent. Finally, the estimated effect on reference transactions is consistently negative, but generally not statistically significant. This may reflect the noisy trends observed in the TWFE estimates and limits the ability to draw firm conclusions about this and other outcomes.

These estimates suggest that while some effects—such as capital spending, operating expenses, and hours open—remain relatively robust across specifications, others, particularly visits and reference transactions, are more sensitive to the choice of comparison group and methodology. Taking the CSA estimates as the most credible, as they address both selection into treatment and treatment timing endogeneity under certain assumptions, this analysis suggests that the BMGF grants were potentially able to increase public use computers, consistent with the program's goals. However, there is little evidence of broader or persistent benefits for libraries. Instead, I observe potential reductions in spending on core library services and potential declines in the use of those services. However, the presence of consistent pre-treatment trends across several outcomes, even after re-weighting and alternative specifications, underscores the difficulty of making confident causal claims, and suggests that these estimates should be interpreted with considerable caution.

3.5 Conclusion

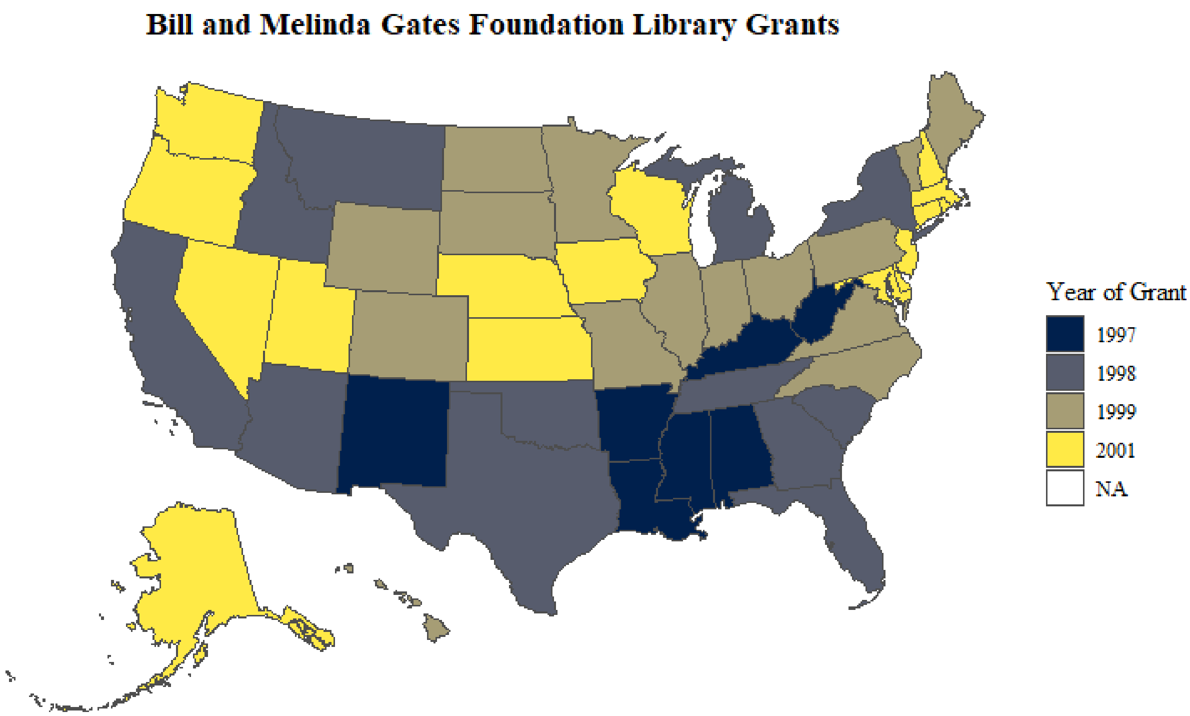
In the late 1990s and early 2000s, the Bill and Melinda Gates Foundation launched a major philanthropic initiative aimed at reducing the digital divide by increasing computer and internet access in public libraries. The program targeted high-poverty areas and sought to modernize library infrastructure by funding the purchase of computer hardware, software, and training. Given the increasing importance of digital access during this period and the central role libraries play in low-income communities, these grants had the potential to reshape internet access, library services and usage in meaningful ways. In this paper, I use a panel of public libraries and a staggered difference-in-differences framework to evaluate the effects of receiving a BMGF Library Grant on library outcomes including computer availability, library use, spending, and service capacity. To address concerns about differences between treated and untreated libraries and the timing of treatment, I incorporate re-weighting methods following Abadie (2005) and implement the Callaway and Sant'Anna (2021) estimator using never-treated libraries as a comparison group.

The results do not present a clear picture of strong or sustained benefits for libraries or their patrons, and interpretation of all estimated effects is complicated by the presence of persistent differential pre-treatment trends. While estimates from the TWFE estimations suggest increases in visits and reference transactions, these findings are sensitive to the choice of control group and the estimation method. There is a consistent pattern of an increase in capital spending of between 40 and 50 percent in the year of grant receipt, suggesting that libraries are spending their grants on capital investment, which includes computers. This is followed by a decline in capital spending in the following years. Other relatively consistent findings include a decline in operating expenses, with the post-treatment effect reaching roughly 8.7 percent in the CSA estimation. Hours open also shows a consistently negative post-treatment effect across specifications. There may also be a negative effect on collection spending, although the effect is not consistently statistically significant. There appears to be a negative effect on total circulation and a sustained increase in public use computers following grant receipt that appears robust; however, the persistent differential pre-trends make the interpretation or magnitude of these and all outcomes challenging to determine.

In short, if we take the CSA estimates as the most credible, the program appears to have had narrow success. The grants may have increased the number of public access computers, but there is little evidence of broader improvements in library services or utilization. If anything, the grants may have contributed to a reduction in operating hours and an associated with operating expenses. These findings suggest that although the evidence is not conclusive across many dimensions, at best they point to limited and non-persistent positive impacts of the BMGF Library Grants on libraries. To the extent that this is the case, the results imply that these types of philanthropic investments may address infrastructure deficits in the short run, but they have more limited effects on long-term public service usage. In particular, this may be a result of a lack of complementary efforts, especially funding, to sustain and adapt services over time. As public institutions continue to evolve in a digital age, understanding how infrastructure investments translate into lasting engagement remains a critical and open question. More broadly, this research underscores not only the importance of evaluating such programs, but also the practical challenges of doing so, even with robust methods.

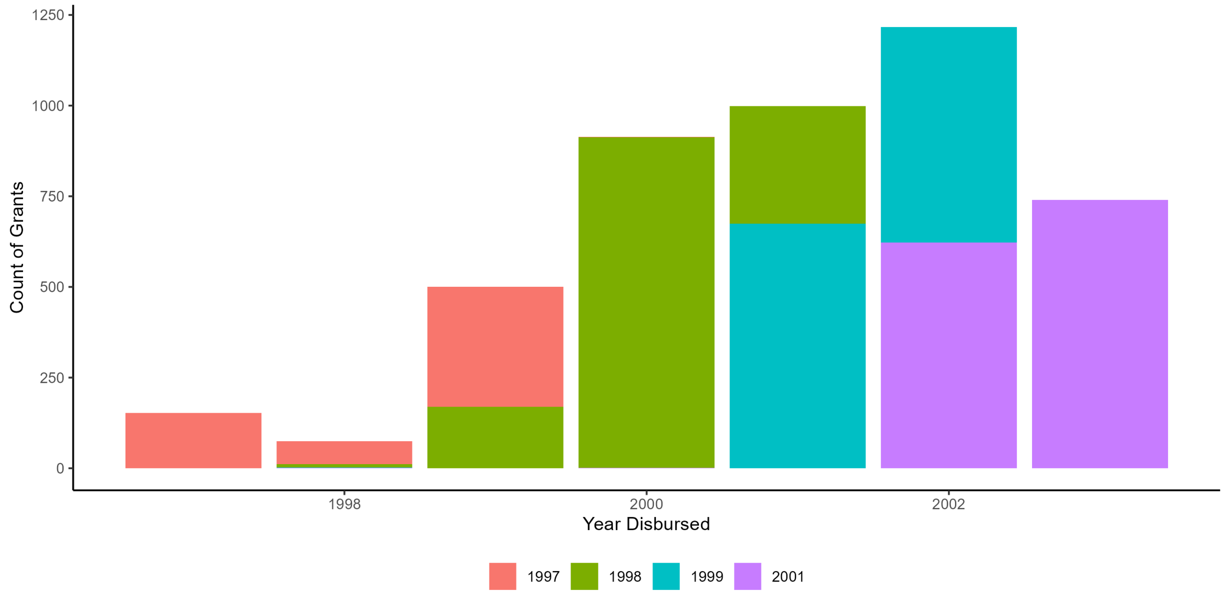
3.6 Tables and Figures

Figure 3.1: Grant Eligibility by State and Year



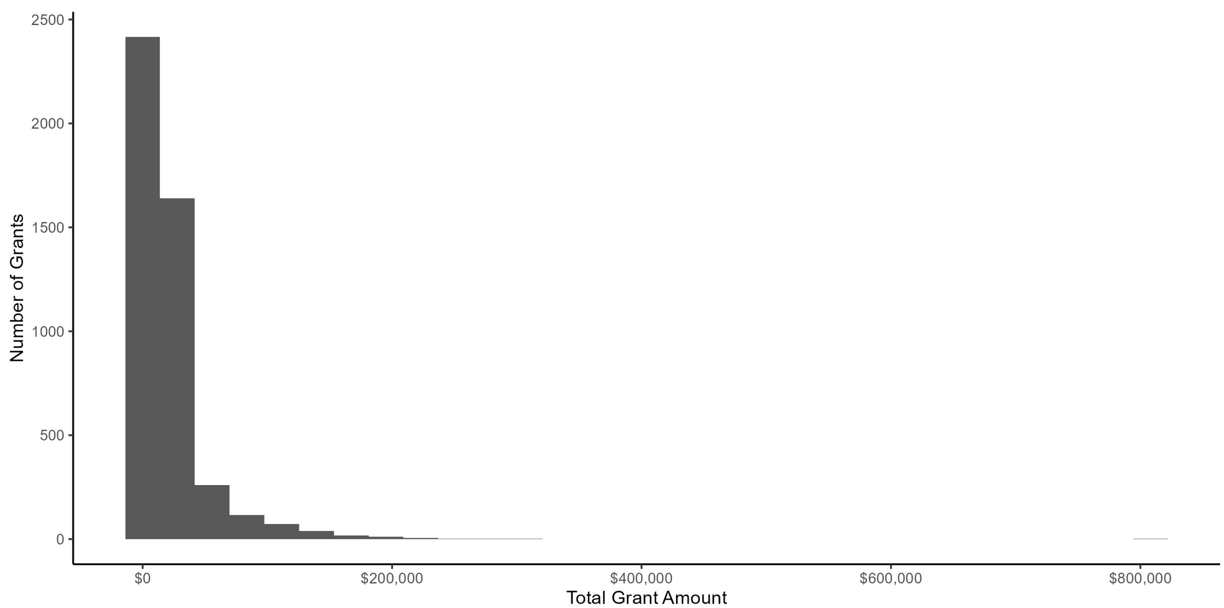
Notes: Author produced using data on grant eligibility from the Bill and Melinda Gates Foundation Library Grants documents.

Figure 3.2: Grant Year by Adoption Group



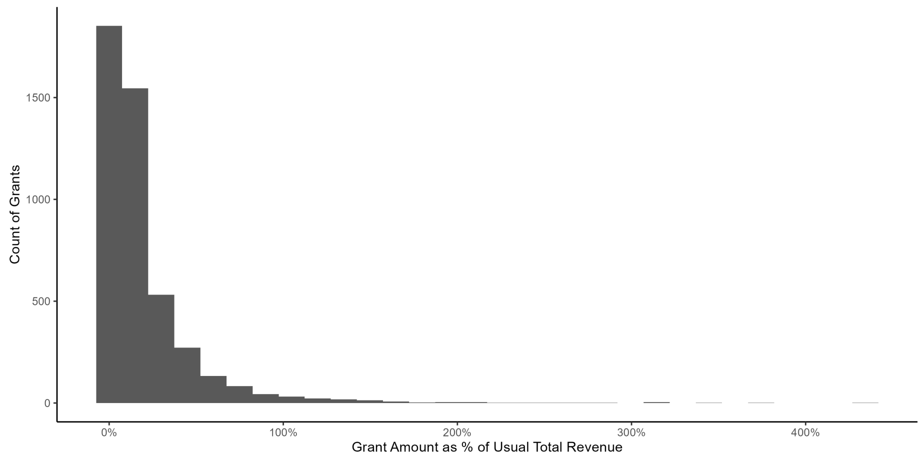
Notes: Author produced using data on grants from the Bill and Melinda Gates Foundation grant award data.

Figure 3.3: Grant Amount Histogram

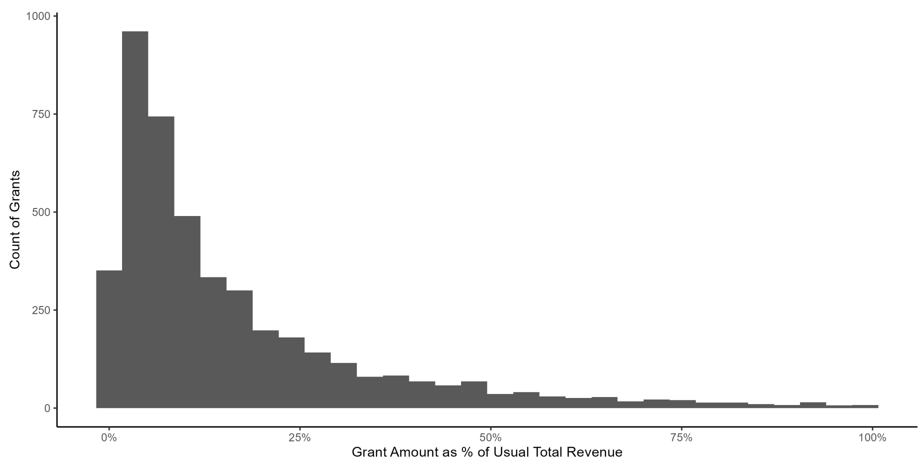


Notes: Author produced using data on grants from the Bill and Melinda Gates Foundation grant award data.

Figure 3.4: Grant Amount as Share of Total Income



(a) Full Distribution



(b) Distribution capped at 100%

Notes: Author produced using data on grants from the Bill and Melinda Gates Foundation grant award data.

Table 3.1: Reasons for Library Districts Being Dropped

	All	Treated	Control
Starting Total Libraries	9215	4885	4330
Dropped: Law, School, Museum, and Bookmobile	151	54	97
Total Libraries After Sample Cleaning	9,064	4,831	4,233
Dropped: <5 obs	172	15	157
Dropped: 3+ consecutive missing years	74	28	46
Total Libraries After Data Quality Exclusions	8,818	4,788	4,030
Dropped: Multiple pre-2004 grants	55	55	0
Dropped: Any 2004–2007 grants	93	90	3
Dropped: Both multiple pre-04 and 04–07 grants	35	35	0
Total Libraries in Final Sample	8,635	4,608	4,027

Notes: Author-produced counts following data cleaning process described in section 3.3.1.4.

Table 3.2: Characteristics of Library Districts that Received vs Did Not Receive Grants

Variable	All Mean	All Std. Dev.	Received Grant Mean	Received Grant Std. Dev.	Did Not Receive Grant Mean	Did Not Receive Grant Std. Dev.	(Rec. vs Not Rec.) Adj P Value
1992–1997 Averages: PLS Data							
Service Area Population	19,812	(64,620)	23,049	(68,904)	16,150	(59,197)	0
Total Outlets	1.5492	(2.1955)	1.7812	(2.3300)	1.2866	(2.0007)	0
Total Hours Open	3,072.2	(4,852.1)	3,502.4	(4,934.2)	2,585.4	(4,710.9)	0
Total Employees	9.4662	(38.358)	9.5909	(24.396)	9.3251	(49.631)	0.8138
Librarians	3.2622	(10.823)	3.3409	(8.0352)	3.1733	(13.291)	0.6324
Books	57,301	(229,020)	60,058	(131,421)	54,180	(303,732)	0.4045
Audio Books	1,880.9	(16,270)	1,670.4	(4,452.8)	2,119.0	(23,277)	0.3761
Video Items	947.7	(2,823.3)	911.48	(2,231.6)	988.68	(3,369.6)	0.3712
Total Visits	82,445	(242,269)	83,468	(205,598)	81,287	(278,013)	0.7715
Reference Transactions	18,288	(145,967)	19,062	(158,695)	17,413	(130,086)	0.7321
Total Circulation	134,247	(406,633)	135,580	(308,642)	132,738	(494,674)	0.8138
Total Revenue	404,089	(1,930,437)	378,494	(1,042,538)	433,053	(2,590,951)	0.3712
Federal Gvt Revenue	3,995.5	(31,650)	4,931.4	(24,487)	2,936.6	(38,143)	0.0111
State Gvt Revenue	51,038	(551,030)	49,504	(231,631)	52,772	(765,887)	0.8327
Local Gvt Revenue	310,016	(1,192,767)	293,278	(890,280)	328,956	(1,461,296)	0.332
Other Income	39,040	(545,183)	30,780	(87,180)	48,388	(790,489)	0.3099
Capital Expenditures	48,812	(247,637)	43,220	(173,159)	55,140	(311,012)	0.0695
Total Operating Exp	374,772	(1,856,858)	354,993	(1,039,816)	397,155	(2,475,076)	0.4811
Total Collection Exp	57,777	(262,461)	53,191	(144,248)	62,966	(351,081)	0.2115
Grant Size			17,213	(24,709)			
N: Library Districts	8,635		4,584		4,051		
1990 Census Characteristics							
Share Pop White	0.9235	(0.1304)	0.9019	(0.1533)	0.9483	(0.0919)	0
Share Pop Hispanic	0.0363	(0.0973)	0.0445	(0.1163)	0.0268	(0.0682)	0
Share Pop Urban	0.4033	(0.4058)	0.3705	(0.3837)	0.4408	(0.4268)	0
Share HH With Public Assistance	0.067	(0.0454)	0.0826	(0.0483)	0.0492	(0.034)	0
Share Pop 25+ Less HS	0.2494	(0.1041)	0.2884	(0.0981)	0.2047	(0.0921)	0
Poverty Rate	0.1238	(0.0833)	0.1607	(0.0801)	0.0815	(0.0649)	0
Median Household Income	29,138	(11,676)	23,632	(6,079.1)	35,440	(13,254)	0
Per Capita Income	13,190	(5,398.8)	11,057	(2,950.5)	15,631	(6,433.1)	0
N: Library Districts	8,454		4,512		3,942		

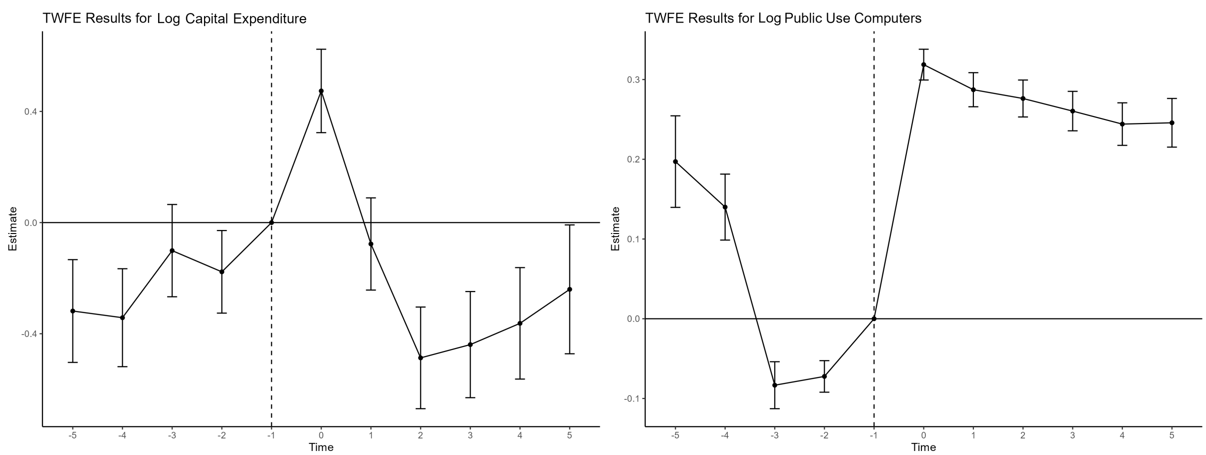
Notes: The number of library districts for both the PLS data and the 1990 Census data vary because some libraries are associated with ZIP codes created after the 1990 census and as such do not appear in the 1990 Census data. P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 1992 dollars.

Table 3.3: TWFE Results

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.3183***	0.1971***	-0.0128	0.037***	0.0128	0.0109	0.0256***	0.1205***
	(0.0943)	(0.0293)	(0.011)	(0.0085)	(0.0161)	(0.007)	(0.0089)	(0.0239)
-4	-0.3423***	0.14***	0.0007	0.0288***	0.0238*	0.0013	0.0148**	0.0936***
	(0.0899)	(0.0211)	(0.0094)	(0.0071)	(0.0143)	(0.0068)	(0.0063)	(0.0205)
-3	-0.1009	-0.0833***	0.0053	0.021***	0.0132	0.004	0.0134***	0.0714***
	(0.0847)	(0.015)	(0.0076)	(0.0064)	(0.0122)	(0.0046)	(0.0038)	(0.0149)
-2	-0.1772**	-0.0723***	-0.0018	0.0143***	0.0029	-0.0107***	0.0108***	0.0293***
	(0.0758)	(0.0101)	(0.0059)	(0.0041)	(0.0104)	(0.0035)	(0.0029)	(0.0105)
0	0.4737***	0.3187***	0.0199***	-0.0034	0.0008	0.0224***	-0.0007	-0.0016
	(0.0766)	(0.0098)	(0.0053)	(0.0043)	(0.0102)	(0.0039)	(0.0024)	(0.0102)
1	-0.077	0.2873***	0.0288***	-0.0161***	-0.0281**	-0.0316***	-0.0037	0.0187
	(0.0846)	(0.0109)	(0.0069)	(0.0055)	(0.0124)	(0.0043)	(0.0031)	(0.0135)
2	-0.4868***	0.2762***	0.0243***	-0.0348***	-0.0207	-0.0453***	-0.0202***	0.0306*
	(0.0933)	(0.0118)	(0.0084)	(0.007)	(0.0136)	(0.0049)	(0.004)	(0.0162)
3	-0.4391***	0.2604***	0.0184*	-0.0457***	-0.0182	-0.0514***	-0.024***	0.0452**
	(0.0974)	(0.0126)	(0.0097)	(0.008)	(0.0149)	(0.0057)	(0.0045)	(0.0188)
4	-0.3626***	0.2441***	0.0043	-0.0603***	-0.0619***	-0.0672***	-0.0313***	0.0537**
	(0.1023)	(0.0136)	(0.0109)	(0.0093)	(0.016)	(0.0065)	(0.005)	(0.0209)
5	-0.2402**	0.2458***	0.0022	-0.0732***	-0.0416**	-0.0725***	-0.0514***	0.0554**
	(0.1183)	(0.0156)	(0.0132)	(0.0113)	(0.0187)	(0.008)	(0.0064)	(0.0253)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

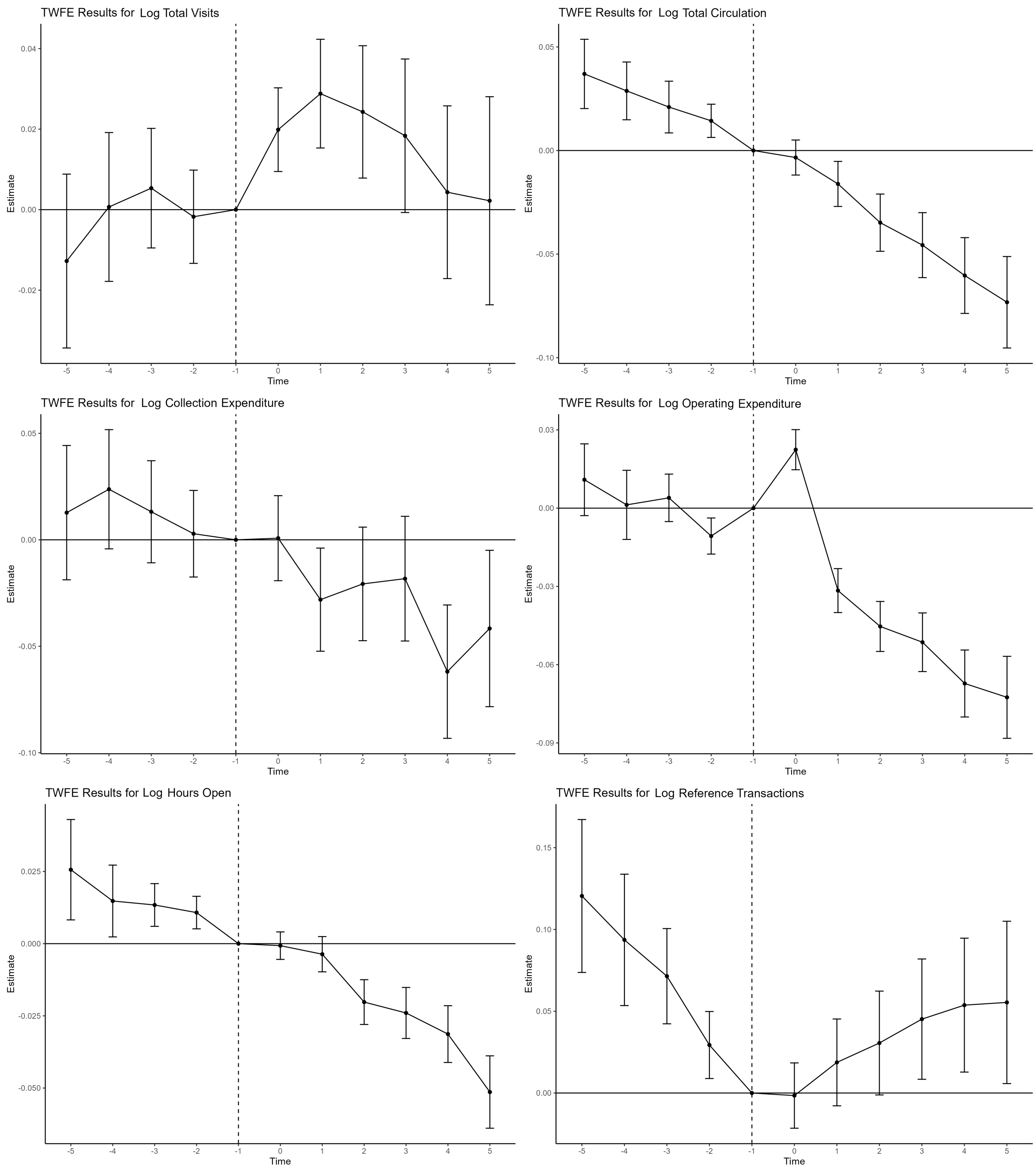
Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equation 3.1. Unbalanced panel used. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level and adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Figure 3.5: Capital and Computers Results: TWFE no weights



Notes: Estimated using Equation 3.1. Unbalanced panel used. 95% confidence intervals shown. Numeric results available in Table 3.3. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure 3.6: Usage and Finance Results: TWFE no weights



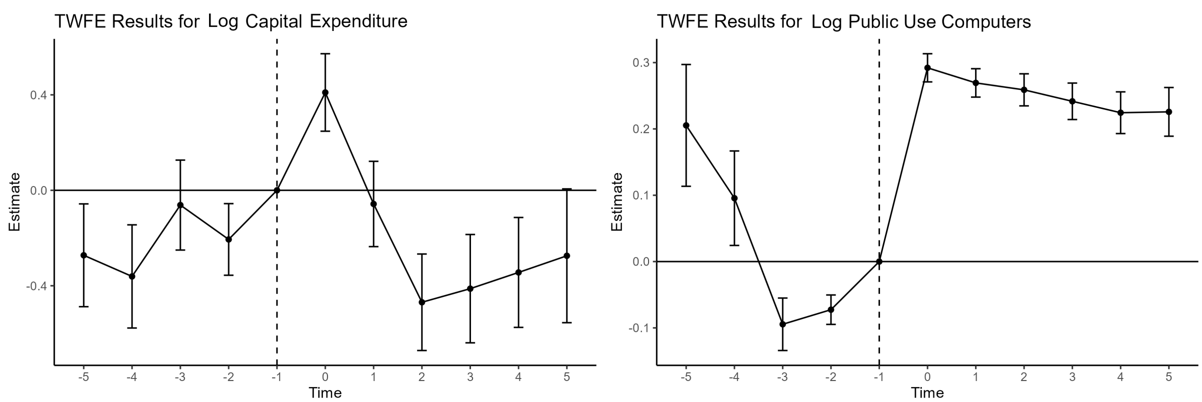
Notes: Estimated using Equation 3.1. Unbalanced panel used. 95% confidence intervals shown. Numeric results available in Table 3.3. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Table 3.4: TWFE Results: Weighted Sample

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.2723**	0.2053***	-0.0284**	0.0273***	0.0139	0.0063	0.0015	0.1174***
	(0.1099)	(0.0468)	(0.0138)	(0.0093)	(0.0168)	(0.0076)	(0.0105)	(0.0301)
-4	-0.3609***	0.0955***	-0.0037	0.0222***	0.0271*	-0.0014	0.0023	0.1007***
	(0.1103)	(0.0363)	(0.0109)	(0.0077)	(0.0148)	(0.0073)	(0.007)	(0.0235)
-3	-0.0619	-0.0945***	0.0013	0.0166**	0.0145	0.0017	0.0079*	0.0678***
	(0.0962)	(0.0202)	(0.0082)	(0.0065)	(0.0128)	(0.005)	(0.0043)	(0.0168)
-2	-0.2059***	-0.0726***	-0.0022	0.0116***	0.001	-0.012***	0.0094***	0.0265**
	(0.0765)	(0.0113)	(0.0064)	(0.0042)	(0.0107)	(0.0038)	(0.0034)	(0.011)
0	0.41***	0.2921***	0.0195***	-0.0006	0.0023	0.0246***	0.0011	-0.0051
	(0.0828)	(0.0108)	(0.0054)	(0.0045)	(0.0104)	(0.0041)	(0.0026)	(0.0105)
1	-0.0573	0.2694***	0.0269***	-0.0107*	-0.0305**	-0.0288***	0.0003	0.0156
	(0.0912)	(0.0109)	(0.0074)	(0.0061)	(0.0126)	(0.0046)	(0.0039)	(0.0145)
2	-0.4691***	0.2589***	0.0252***	-0.0244***	-0.0177	-0.0397***	-0.0187***	0.0367**
	(0.1032)	(0.0123)	(0.0093)	(0.0091)	(0.014)	(0.0055)	(0.0066)	(0.0181)
3	-0.4121***	0.2416***	0.0195*	-0.0315***	-0.0143	-0.043***	-0.0149**	0.057***
	(0.1158)	(0.014)	(0.0112)	(0.0117)	(0.0155)	(0.0066)	(0.0068)	(0.0216)
4	-0.3442***	0.2244***	0.0081	-0.0408***	-0.0544***	-0.0562***	-0.0174**	0.0729***
	(0.1175)	(0.016)	(0.0134)	(0.0144)	(0.0168)	(0.0077)	(0.0077)	(0.0249)
5	-0.2744*	0.2257***	0.0064	-0.0484***	-0.033*	-0.0592***	-0.036***	0.0757**
	(0.143)	(0.0187)	(0.0157)	(0.0169)	(0.0194)	(0.0093)	(0.0091)	(0.03)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

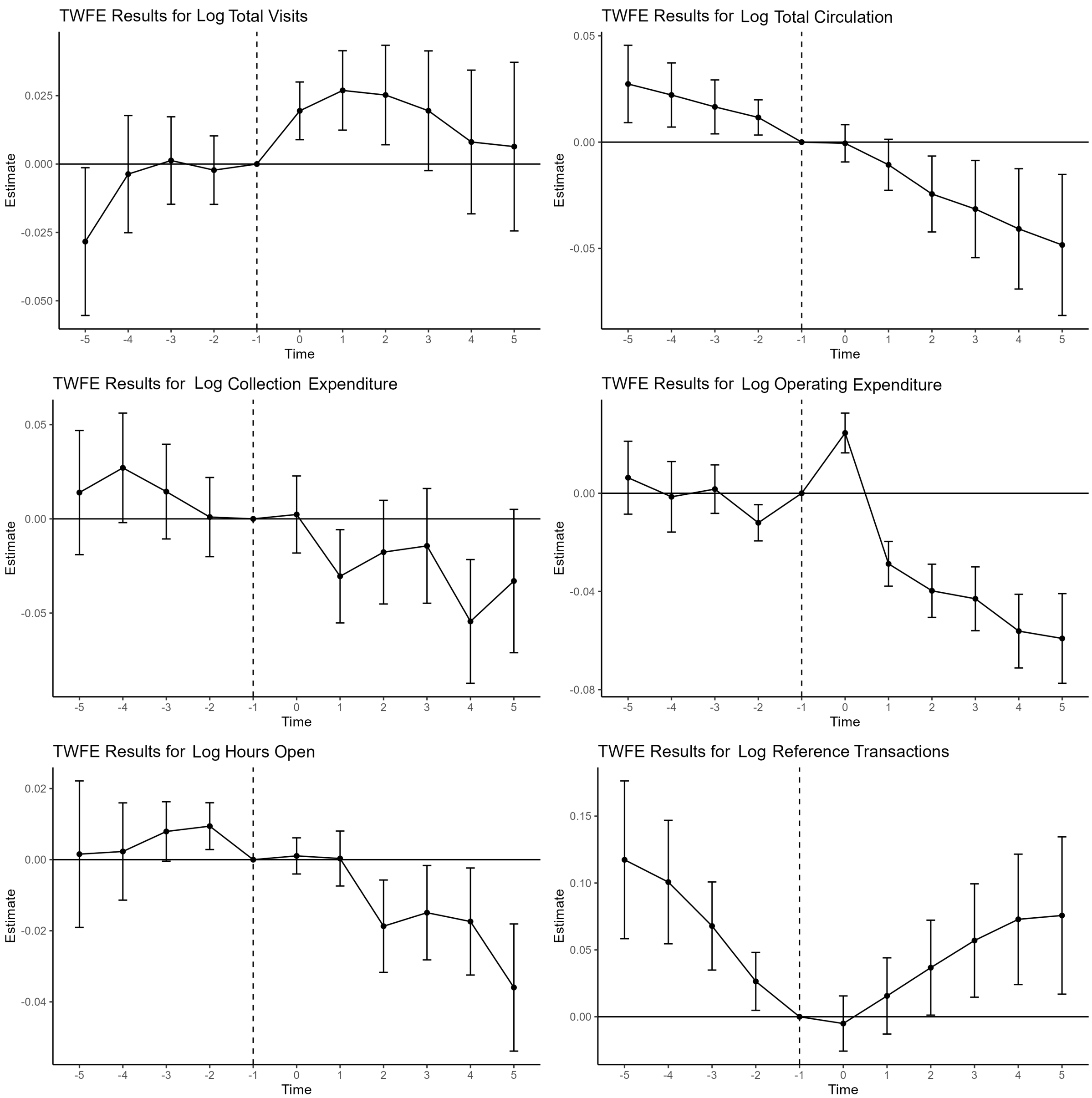
Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equation 3.1, with untreated units re-weighted based on inverse probability weights of being treated following Abadie (2005). Unbalanced panel used. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level and adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Figure 3.7: Capital and Computers Results: Weighted TWFE



Notes: Estimated using Equation 3.1, with untreated units re-weighted based on inverse probability weights of being treated following Abadie (2005). Unbalanced panel used. 95% confidence intervals shown. Numeric results available in Table 3.4. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Figure 3.8: Usage and Finances Results: Weighted TWFE



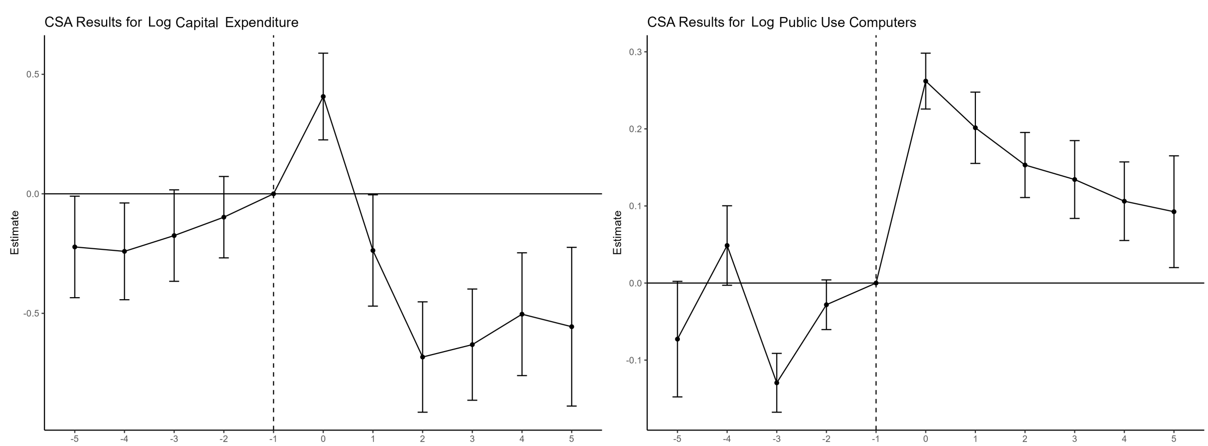
Notes: Estimated using Equation 3.1, with untreated units re-weighted based on inverse probability weights of being treated following Abadie (2005). Unbalanced panel used. 95% confidence intervals shown. Numeric results available in Table 3.4. Adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995).

Table 3.5: Callaway and Sant'Anna Results

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.2223**	-0.0728*	-0.0144	0.0453***	0.0082	-0.0011	0.006	0.0097
	(0.1083)	(0.0382)	(0.0224)	(0.0156)	(0.0242)	(0.0136)	(0.0202)	(0.0362)
-4	-0.2407**	0.0487*	-0.0057	0.0347**	0.0098	-0.0127	-0.0063	0.0266
	(0.1033)	(0.0263)	(0.0205)	(0.014)	(0.019)	(0.013)	(0.0162)	(0.0314)
-3	-0.1747*	-0.1294***	-0.0008	0.0265**	0.001	-0.005	0.0039	0.0143
	(0.0976)	(0.0195)	(0.0152)	(0.0104)	(0.0182)	(0.0093)	(0.0134)	(0.0245)
-2	-0.0977	-0.0282*	-0.0085	0.0206***	-0.0062	-0.0169**	0.0011	0.0118
	(0.0869)	(0.0164)	(0.0101)	(0.0066)	(0.0133)	(0.0073)	(0.007)	(0.017)
0	0.407***	0.262***	-0.0066	-0.0151*	-0.0142	0.012*	-0.0003	-0.0241
	(0.0925)	(0.0185)	(0.0147)	(0.0081)	(0.0145)	(0.0068)	(0.0109)	(0.0207)
1	-0.237**	0.2014***	-0.0989*	-0.0376***	-0.0283*	-0.0399***	-0.0579**	-0.0874**
	(0.1188)	(0.0236)	(0.0521)	(0.0102)	(0.0164)	(0.0086)	(0.0294)	(0.0444)
2	-0.6829***	0.1531***	-0.1089*	-0.0625***	-0.0344*	-0.0597***	-0.0642**	-0.0653
	(0.1178)	(0.0215)	(0.0593)	(0.0117)	(0.0199)	(0.0093)	(0.0292)	(0.0437)
3	-0.631***	0.1343***	-0.1332**	-0.0631***	-0.0137	-0.0602***	-0.0782**	-0.0772
	(0.1187)	(0.0258)	(0.063)	(0.0152)	(0.0241)	(0.013)	(0.0341)	(0.0492)
4	-0.5037***	0.1062***	-0.1464**	-0.075***	-0.0761***	-0.0773***	-0.0791**	-0.0704
	(0.1311)	(0.026)	(0.0619)	(0.0177)	(0.0231)	(0.0142)	(0.0338)	(0.0534)
5	-0.5559***	0.0926**	-0.2038**	-0.0874***	-0.0083	-0.0511***	-0.1033**	-0.1074
	(0.1694)	(0.037)	(0.0882)	(0.0242)	(0.0292)	(0.018)	(0.0506)	(0.0798)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

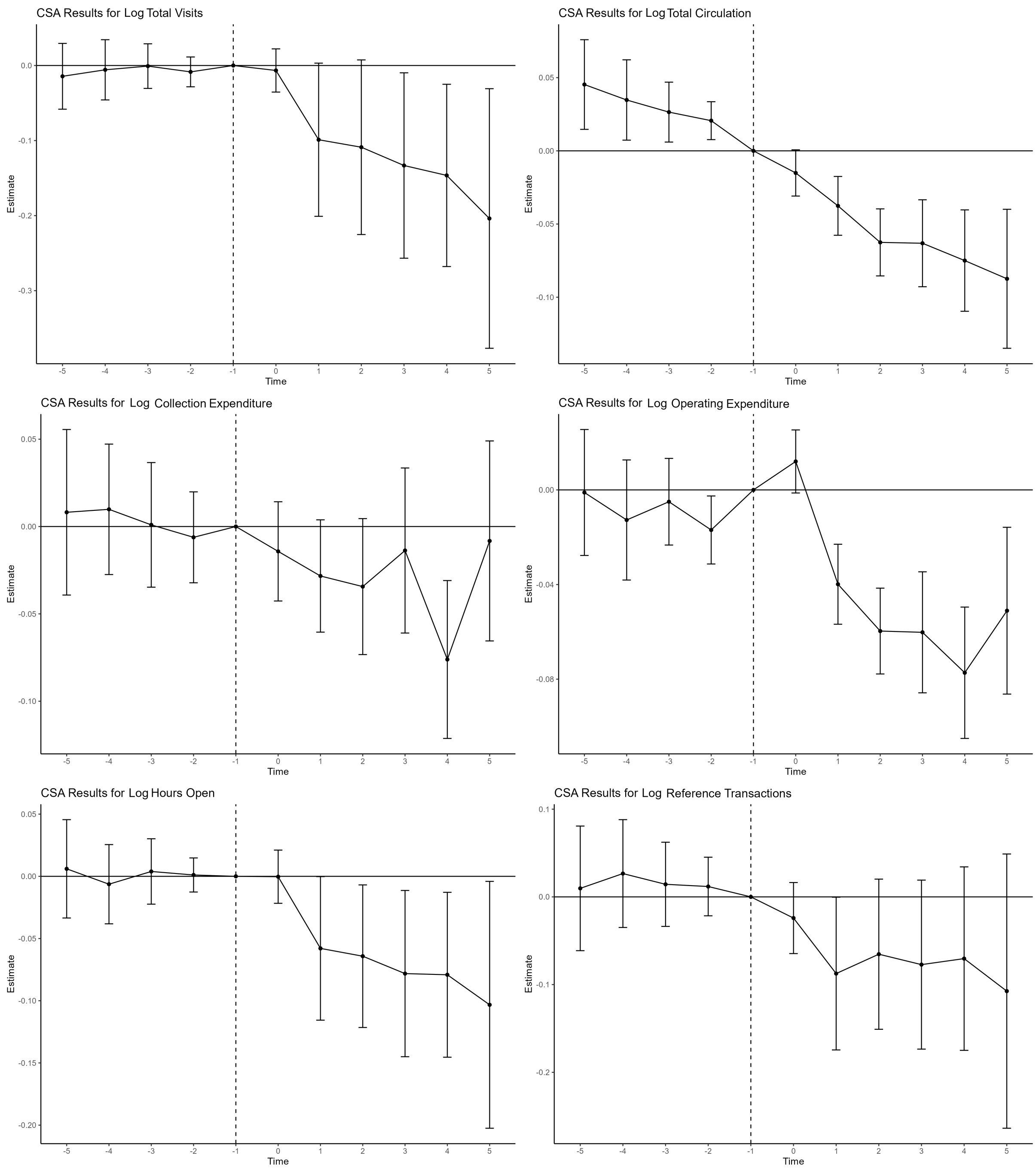
Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equations 3.2 and 3.3. Unbalanced panel used. Never treated used as control group. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are bootstrapped and are robust to multiple inference. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Figure 3.9: Capital and Computers Results: Callaway and Sant'Anna



Notes: Estimated using Equations 3.2 and 3.3. Unbalanced panel used. 95% confidence intervals shown. Never treated used as control group. Numeric results available in Table 3.5.

Figure 3.10: Usage and Finances Results: Callaway and Sant'Anna



Notes: Estimated using Equations 3.2 and 3.3. Unbalanced panel used. 95% confidence intervals shown. Never treated used as control group. Numeric results available in Table 3.5.

3.A Appendix

3.A.1 Assessing the Dropped Vs Remaining Library Districts

3.A.1.1 Summary Statistics

I begin by assessing the balance between the dropped and remaining libraries, excluding the non-traditional libraries. Table A.3.1 summarizes the differences between the library districts that were removed and those that remain in my estimation sample. The top panel displays average characteristics from the PLS between 1992 and 1996, while the bottom panel presents 1990 Census characteristics of the ZIP code associated with each library district. Libraries removed from the sample differ significantly from those that remain. Removed libraries are larger, serving a bigger population (148,582 vs 19,812), having more outlets (5.08 vs 1.55), longer operating hours (11,241 vs 3,072), and more employees (60.03 vs 9.47). Their collections and usage are also significantly greater, with nearly six times more books (343,828 vs 57,301) and over six times more total circulation (814,553 vs 134,247). They receive more revenue, with total income averaging $2.98 million, more than seven times higher than the $404,089 received by remaining libraries.

The removed libraries also tend to be in more racially diverse and lower-income areas. On average, they serve ZIP codes with a higher share of Hispanic residents (6.7% vs 3.6%), higher poverty rates (18.6% vs 12.4%), and more households receiving public assistance (9.3% vs 6.7%). While removing libraries with missing data or problematic treatment timing is necessary for the analysis, it is important to note that the dropped libraries differ in meaningful ways from those that remain. This introduces potential bias. As a result, my estimation sample may differ in important ways from the overall population of U.S. libraries. To address this, I calculate inverse probability weights to re-weight my sample to better reflect the full population.

Table A.3.1: Characteristics of Library Districts that were Removed vs Remain in Sample

	All		Removed from sample		Retained in Sample		(Rem. vs Not Rem.)
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Adj P Value
1992–1997 Averages: PLS Data							
Service Area Population	28,284	(109,568)	198,810	(373,335)	19,812	(64,620)	0
Total Outlets	1.7825	(3.3556)	6.4796	(10.862)	1.5492	(2.1955)	0
Total Hours Open	3,613.70	(7,715.4)	14,513	(25,702)	3,072.20	(4,852.1)	0
Total Employees	12.818	(53.625)	80.274	(162.55)	9.4662	(38.358)	0
Librarians	4.264	(15.755)	24.428	(49.651)	3.2622	(10.823)	0
Books	76,223	(318,076)	457,100	(965,216)	57,301	(229,020)	0
Audio Books	2,783.60	(23,383)	20,954	(76,748)	1,880.90	(16,270)	0
Video Items	1,181.70	(4,344.8)	5,891.70	(14,683)	947.70	(2,823.3)	0
Total Visits	109,522	(392,082)	654,529	(1,326,155)	82,445	(242,269)	0
Reference Transactions	30,151	(222,057)	268,925	(744,540)	18,288	(145,967)	0
Total Circulation	179,437	(643,312)	1,089,033	(2,134,776)	134,247	(406,633)	0
Total Revenue	574,429	(2,783,658)	4,003,056	(8,748,553)	404,089	(1,930,437)	0
Federal Gvt Revenue	6,019.30	(46,218)	46,753	(152,573)	3,995.50	(31,650)	0
State Gvt Revenue	71,165	(681,744)	476,304	(1,882,512)	51,038	(551,030)	0
Local Gvt Revenue	446,910	(2,063,769)	3,202,339	(7,314,386)	310,016	(1,192,767)	0
Other Income	50,334	(558,694)	277,660	(747,908)	39,040	(545,183)	0
Capital Expenditures	68,662	(510,406)	468,209	(2,027,660)	48,812	(247,637)	0
Total Operating Exp	534,421	(2,596,543)	3,747,870	(7,895,884)	374,772	(1,856,858)	0
Total Collection Exp	80,840	(370,108)	545,051	(1,133,236)	57,777	(262,461)	0
N: Library Districts	9,064		429		8,635		
1990 Census Characteristics							
Share Pop White	0.9172	(0.1420)	0.7866	(0.2594)	0.9235	(0.1304)	0
Share Pop Hispanic	0.038	(0.1010)	0.0723	(0.1554)	0.0363	(0.0973)	0
Share Pop Urban	0.4096	(0.4085)	0.5404	(0.4410)	0.4033	(0.4058)	0
Share HH With Public Assistance	0.0684	(0.0480)	0.0974	(0.0805)	0.067	(0.0454)	0
Share Pop 25+ Less HS	0.2509	(0.1054)	0.2815	(0.1251)	0.2494	(0.1041)	0
Poverty Rate	0.1271	(0.0879)	0.1955	(0.1382)	0.1238	(0.0833)	0
Median Household Income	28,905	(11,659)	24,093	(10,184)	29,138	(11,676)	0
Per Capita Income	13,139	(5,381.4)	12,094	(4,900.2)	13,190	(5,398.8)	0
N: Library Districts	8,863		409		8,454		

Notes: The number of library districts for both the PLS data and the 1990 Census data vary because some libraries are associated with ZIP codes created after the 1990 census and as such do not appear in the 1990 Census data P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 1992 dollars.

3.A.1.2 Calculating Sample Weights

Because the excluded libraries differ systematically from those retained as shown in Table A.3.2, the resulting sample may suffer from selection bias. To address potential bias, I test the effects of applying an inverse probability weighting (IPW) approach based on the likelihood of inclusion in the sample. The IPW model is estimated using the full set of traditional public libraries, but I exclude the 151 non-traditional libraries from both the estimation and the weighting procedure, as I do not want to reweight the sample to resemble those institutions.

To implement inverse probability weighting (IPW), I estimate propensity scores using a logistic regression and covariates that are likely to be related to the probability of being included in the sample. These covariates include the 1992–1997 average values of: total visits, legal service area population, number of outlets, total circulation, number of books, audio units, video units, reference transactions, attendance at children's programs, hours open, number of librarians, total staff, total income, income from federal, state, local, and other sources, capital expenditures, collection expenditures, and total operating expenditures. I use the estimated propensities to construct inverse probability weights.

To reduce the influence of extreme weights that can arise from propensity scores near 0 or 1, I stabilize the weights and trim the sample to exclude observations with estimated propensity scores outside the common support region. This trimming step improves the reliability of the weighted estimates by ensuring better overlap in covariate distributions between treated and control units. These stabilized IPW weights are applied to the data to adjust for differences in the probability of treatment assignment. To verify that the weighting procedure improves covariate balance, I report the distribution of pre-treatment characteristics in the reweighted sample in Table A.3.2. Compared to Table A.3.1 above, the balance is improved and most of the statistically significant differences between the groups are now insignificant.

Table A.3.2: Characteristics of Library Districts that were Removed vs Retained, after IPW Weighting

Variable	All Weighted Mean	Std. Dev.	Removed from sample Weighted Mean	Std. Dev.	Retained in Sample Mean	Std. Dev.	(Rem. vs Not Rem.) Adj P Value
1992–1996 Averages: PLS Data							
Service Area Population	43,672	240,873	32,822	114,243	44,191	245,287	0.4716
Total Outlets	2.1105	6.3466	1.6833	3.3599	2.1310	6.4542	0.3982
Total Hours Open	4,162.7	12,780	3,552.9	8,177.3	4,191.9	12,959	0.4527
Total Employees	23.050	171.89	13.793	49.056	23.493	175.61	0.4361
Librarians	7.1087	46.536	4.2473	14.751	7.2456	47.522	0.4478
Books	141,578	1,103,135	80,669	287,426	144,492	1,127,385	0.4629
Audio Books	7,358.3	85,055	3,205.6	21,661	7,557.0	86,933	0.4793
Video Items	1,633.6	8,079.2	1,328.7	4,644.7	1,648.2	8,207.3	0.5325
Total Visits	146,467	775,886	127,458	413,403	147,376	789,056	0.6734
Reference Transactions	69,921	645,520	33,784	202,827	71,650	659,243	0.4446
Total Circulation	224,547	1,025,265	217,727	714,996	224,874	1,037,788	0.9387
Total Revenue	1,315,285	10,672,545	752,701	3,139,712	1,342,205	10,902,566	0.4379
Federal Gvt Revenue	20,387	186,499	8,558.1	50,233	20,953	190,574	0.4293
State Gvt Revenue	194,958	1,961,113	77,473	674,159	200,579	2,001,887	0.4404
Local Gvt Revenue	826,868	5,680,912	602,430	2,605,319	837,608	5,787,029	0.5132
Other Income	273,072	3,678,467	64,240	277,458	283,065	3,764,667	0.4185
Capital Expenditures	127,215	991,114	87,196	689,482	129,130	1,003,236	0.5329
Total Operating Exp	1,271,360	10,496,632	713,260	2,884,918	1,298,066	10,725,554	0.4462
Total Collection Exp	180,751	1,387,857	105,743	431,675	184,340	1,417,432	0.4568
N: Library Districts	9064		429		8635		
1990 Census Characteristics							
Share Pop White	0.9173	0.1375	0.8692	0.2043	0.9195	0.1331	0.0000
Share Pop Hispanic	0.0373	0.0986	0.0574	0.1277	0.0364	0.0970	0.0001
Share Pop Urban	0.4166	0.4057	0.4345	0.4031	0.4157	0.4058	0.4211
Share HH With Public Assistance	0.0682	0.0468	0.0779	0.0606	0.0677	0.0460	0.0001
Share Pop 25+ Less HS	0.2506	0.1049	0.2573	0.1104	0.2503	0.1047	0.2421
Poverty Rate	0.1273	0.0859	0.1527	0.1045	0.1261	0.0847	0.0000
Median Household Income	28,894	11,680	26,196	9,189.0	29,020	11,769	0.0000
Per Capita Income	13,372	7,133.1	12,120	3,635.4	13,431	7,250.9	0.0006
N: Library Districts	8863		409		8454		

Notes: P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 1992 dollars.

3.A.1.3 Results using IPW Weighting

I then apply the weights to my three methods to see whether the results are affected. Overall, my results are robust to IPW weighting. There are some changes in magnitude and statistical significance changes; however, the general patterns remain the same.

Table A.3.3: TWFE Results

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.3204***	0.2232***	0.0122	0.0386***	0.0143	0.0116	0.0737**	0.1163***
	(0.0957)	(0.0449)	(0.0236)	(0.0095)	(0.0165)	(0.0078)	(0.0321)	(0.024)
-4	-0.342***	0.1472***	-0.009	0.0265***	0.0278*	0.0023	0.0157**	0.0935***
	(0.0902)	(0.0265)	(0.0125)	(0.0074)	(0.0147)	(0.0073)	(0.0075)	(0.0206)
-3	-0.1009	-0.0816***	0.0024	0.0201***	0.0159	0.005	0.0143***	0.0725***
	(0.0855)	(0.0184)	(0.0086)	(0.0065)	(0.0125)	(0.0049)	(0.0046)	(0.015)
-2	-0.1736**	-0.1042***	-0.0029	0.0127***	0.0052	-0.0101***	0.0011	0.0293***
	(0.0768)	(0.0338)	(0.0065)	(0.0042)	(0.0107)	(0.0036)	(0.0073)	(0.0106)
0	0.4827***	0.3132***	0.0297***	-0.0026	0.0024	0.0219***	0.003	-0.0005
	(0.0761)	(0.0115)	(0.0091)	(0.0043)	(0.0103)	(0.004)	(0.004)	(0.0102)
1	-0.0937	0.2811***	0.0504***	-0.0137**	-0.0267**	-0.0323***	0.0107	0.0203
	(0.0852)	(0.0123)	(0.0176)	(0.0057)	(0.0126)	(0.0045)	(0.0114)	(0.0136)
2	-0.5619***	0.2718***	0.0418***	-0.0323***	-0.0173	-0.0456***	-0.011	0.0305*
	(0.1157)	(0.0137)	(0.0158)	(0.0072)	(0.0143)	(0.0051)	(0.0098)	(0.0164)
3	-0.511***	0.2564***	0.041**	-0.041***	-0.0189	-0.052***	-0.0105	0.0469**
	(0.1277)	(0.015)	(0.0195)	(0.0084)	(0.0154)	(0.006)	(0.0133)	(0.019)
4	-0.3733***	0.2407***	0.0253	-0.0557***	-0.0647***	-0.0657***	-0.0173	0.0557***
	(0.1026)	(0.0165)	(0.0202)	(0.0099)	(0.0164)	(0.0072)	(0.0138)	(0.0212)
5	-0.3602**	0.2425***	0.0191	-0.068***	-0.0432**	-0.0719***	-0.0405***	0.0563**
	(0.162)	(0.02)	(0.0192)	(0.0119)	(0.0191)	(0.0086)	(0.0137)	(0.0257)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equation 3.1, but with all observations weighted by their inverse probability weight of being included in the sample. Unbalanced panel used. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level. Observations are weighted by the inverse probability of being included in the sample. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Table A.3.4: TWFE Results: Re-Weighted Sample

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.2813**	0.1928***	0.0017	0.0296***	0.0132	0.0066	0.06	0.1039***
	(0.1161)	(0.0662)	(0.0337)	(0.0109)	(0.0174)	(0.0083)	(0.0477)	(0.033)
-4	-0.3563***	0.106***	-0.0175	0.0187**	0.0333**	0.0007	-0.0028	0.1031***
	(0.1105)	(0.0401)	(0.0167)	(0.0081)	(0.0155)	(0.0079)	(0.0095)	(0.0249)
-3	-0.0507	-0.0922***	-0.0029	0.0144**	0.0178	0.0031	0.0039	0.0698***
	(0.0958)	(0.0227)	(0.0102)	(0.0067)	(0.0133)	(0.0054)	(0.0059)	(0.0176)
-2	-0.1887**	-0.089***	-0.0048	0.009*	0.0037	-0.011***	-0.0064	0.029**
	(0.0778)	(0.0213)	(0.0083)	(0.0047)	(0.0111)	(0.0039)	(0.0129)	(0.0117)
0	0.412***	0.2885***	0.0353***	0.001	0.003	0.0244***	0.0078	-0.0064
	(0.0828)	(0.0114)	(0.0136)	(0.0046)	(0.0106)	(0.0042)	(0.0063)	(0.0108)
1	-0.0687	0.265***	0.0573**	-0.0067	-0.0315**	-0.0293***	0.0203	0.0126
	(0.0919)	(0.0117)	(0.0257)	(0.0067)	(0.0129)	(0.0049)	(0.0168)	(0.0152)
2	-0.5124***	0.2564***	0.0503**	-0.0195*	-0.0168	-0.0395***	-0.0038	0.036*
	(0.1116)	(0.0134)	(0.0223)	(0.01)	(0.0147)	(0.006)	(0.015)	(0.0192)
3	-0.4293***	0.239***	0.0519*	-0.0238*	-0.016	-0.0424***	0.0068	0.058**
	(0.1326)	(0.0155)	(0.0282)	(0.0131)	(0.0163)	(0.0073)	(0.0198)	(0.0233)
4	-0.3483***	0.2218***	0.0397	-0.0322**	-0.0578***	-0.0539***	0.0052	0.0746***
	(0.1202)	(0.0179)	(0.0292)	(0.0162)	(0.0177)	(0.0085)	(0.0205)	(0.0269)
5	-0.3446**	0.2246***	0.0323	-0.0388**	-0.0336*	-0.0564***	-0.0169	0.0805**
	(0.159)	(0.0213)	(0.0266)	(0.0188)	(0.0203)	(0.0102)	(0.0198)	(0.0322)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equation 3.1, but with all observations weighted by their inverse probability weight of being included in the sample multiplied by their inverse probability weights of being treated following Abadie (2005). Unbalanced panel used. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level. Observations are weighted by the inverse probability of being included in the sample and the inverse probability of being treated. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Table A.3.5: Callaway and Sant'Anna Results

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.1987*	-0.0652*	-0.0025	0.0435**	0.0058	-0.0032	0.0415*	0.0278
	(0.1056)	(0.0356)	(0.0236)	(0.018)	(0.0228)	(0.0145)	(0.0229)	(0.0363)
-4	-0.2323**	0.0402*	0.0093	0.0284**	0.0025	-0.0175	0.0167	0.0449
	(0.1025)	(0.0224)	(0.0219)	(0.0135)	(0.0179)	(0.0111)	(0.0169)	(0.0285)
-3	-0.169*	-0.1333***	0.0007	0.0229**	-0.002	-0.0073	0.0123	0.0273
	(0.0937)	(0.017)	(0.0154)	(0.0098)	(0.0164)	(0.0088)	(0.0123)	(0.0227)
-2	-0.0964	-0.0493	-0.0088	0.0191***	-0.009	-0.0186***	0.0042	0.0179
	(0.0903)	(0.0349)	(0.0109)	(0.0064)	(0.0132)	(0.0068)	(0.0082)	(0.0172)
0	0.4068***	0.2558***	-0.0203	-0.0185**	-0.0144	0.0083	-0.0194**	-0.0443**
	(0.0973)	(0.0171)	(0.0132)	(0.0086)	(0.0134)	(0.0076)	(0.0097)	(0.0205)
1	-0.2222*	0.2101***	-0.0556***	-0.0383***	-0.0336**	-0.0464***	-0.0546***	-0.0658***
	(0.1134)	(0.0222)	(0.019)	(0.0102)	(0.0164)	(0.0088)	(0.0122)	(0.0249)
2	-0.7301***	0.1626***	-0.0645***	-0.0643***	-0.0409**	-0.0673***	-0.0674***	-0.0488*
	(0.1354)	(0.0208)	(0.0196)	(0.0117)	(0.0194)	(0.01)	(0.0129)	(0.0277)
3	-0.6797***	0.1429***	-0.0826***	-0.066***	-0.0264	-0.07***	-0.0788***	-0.0511*
	(0.1567)	(0.021)	(0.0213)	(0.0138)	(0.0211)	(0.0103)	(0.0136)	(0.0286)
4	-0.4983***	0.1185***	-0.0982***	-0.0803***	-0.0885***	-0.0866***	-0.0811***	-0.0464
	(0.1212)	(0.0215)	(0.0217)	(0.0153)	(0.0223)	(0.0133)	(0.0138)	(0.0348)
5	-0.6954***	0.1133***	-0.1292***	-0.0949***	-0.0209	-0.0667***	-0.1043***	-0.0752
	(0.1917)	(0.0297)	(0.0332)	(0.0229)	(0.0261)	(0.0195)	(0.0234)	(0.0479)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equations 3.2 and 3.3, with the inverse probability weight of being included in the sample passed as a weight. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level. Observations are weighted by the inverse probability of being included in the sample. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

3.A.2 Additional Detail on Re-Weighted TWFE

In my re-weighted TWFE specification, I use inverse probability weighting (IPW) to adjust for differences in observed pre-treatment characteristics between treated and control libraries. This approach assigns a weight to each observation based on the inverse of its estimated probability of receiving treatment, effectively re-weighting the control group to resemble the treated group in terms of covariate distributions.

The propensity scores used to construct these weights are estimated using a logistic regression model, where treatment status is regressed on a set of pre-treatment characteristics that are plausibly related to both treatment assignment and outcomes. These characteristics include total visits, legal service area population, number of outlets, total circulation, number of books, audio units, video units, reference transactions, attendance at children's programs, hours open, number of librarians, total staff, total income, income from federal, state, local, and other sources, capital expenditures, collection expenditures, and total operating expenditures. All covariates are averaged over the 1992–1997 period to smooth over year-to-year variation and better capture baseline library characteristics prior to treatment.

The aim of this re-weighting procedure is to approximate a setting in which treated and control libraries are comparable on observable characteristics, thereby strengthening the plausibility of a conditional parallel trends assumption. To assess the effectiveness of this re-weighting, Appendix Table A.3.6 presents the balance of covariates before and after weighting.

Table A.3.6: Characteristics of Library Districts that were Received vs Did Not Receive Grants, after Re-Weighting

Variable	All Mean	All Std. Dev.	Control Districts Mean	Control Districts Std. Dev.	Treated Districts Mean	Treated Districts Std. Dev.	(Control vs Treated) Adj P Value
1992—1996 Averages: PLS Data							
Service Area Population	23,585	(62,292)	24,946	(65,872)	22,379	(58,914)	0.1177
Total Outlets	1.6755	(2.2507)	1.7543	(2.5651)	1.6058	(1.9274)	0.0092
Total Hours Open	3,316.1	(4,947.0)	3,418.2	(5,557.2)	3,225.6	(4,333.6)	0.1452
Total Employees	10.729	(32.318)	10.751	(38.919)	10.71	(25.06)	0.9809
Librarians	3.6774	(9.3515)	3.6827	(10.727)	3.6727	(7.9369)	0.9809
Books	63,093	(184,064)	64,618	(234,639)	61,743	(122,984)	0.66
Audio Books	2,028.0	(12,100)	2,007.0	(16,725)	2,046.6	(5,326.4)	0.9445
Video Items	1,064.0	(2,616.9)	1,081.5	(2,832.6)	1,048.4	(2,409.6)	0.762
Total Visits	96,279	(240,514)	97,528	(260,082)	95,173	(221,745)	0.8266
Reference Transactions	22,237	(121,283)	22,525	(113,230)	21,983	(127,990)	0.9323
Total Circulation	159,038	(412,091)	162,847	(470,794)	155,664	(351,998)	0.6136
Total Revenue	578,728	(2,009,018)	572,509	(2,438,968)	584,234	(1,530,601)	0.9125
Federal Gvt Revenue	6,219.9	(34,229)	7,463.3	(43,068)	5,118.9	(23,749)	0.0067
State Gvt Revenue	78,729	(608,136)	73,603	(788,246)	83,268	(383,526)	0.66
Local Gvt Revenue	444,378	(1,395,059)	439,533	(1,482,250)	448,668	(1,313,016)	0.8976
Other Income	49,401	(475,596)	51,909	(682,044)	47,180	(120,682)	0.8266
Capital Expenditures	66,932	(308,718)	60,713	(299,221)	72,439	(316,788)	0.1553
Total Operating Exp	538,861	(1,923,392)	530,935	(2,321,945)	545,879	(1,483,629)	0.8757
Total Collection Exp	84,161	(282,769)	83,075	(333,904)	85,123	(228,111)	0.8832
Grant Size	10,083	(21,002)			19,011	(25,728)	
Share Treated	0.5304	(0.4991)			1		
N: Library Districts	8,635		4,051		4,584		

Notes: P-values adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). All dollar values in 1992 dollars.

3.A.3 Additional Detail on Treatment Timing Endogeneity

To assess whether treatment timing is related to eligibility through poverty rates, I estimate a regression of treatment year on the 1990 ZIP code-level poverty rate on the population of treated libraries. If treatment year was assigned at random, we would expect to see no relationship between the treatment timing and poverty rates. The estimated equation is as follows:

$$\text{TreatmentYear}_i = \alpha + \beta \ \text{PovertyRate}_{i,1990} + \varepsilon_i \tag{3.4}$$

Table A.3.7 summarizes the results of this regression. The results show a strong and statistically significant negative relationship between treatment year and baseline poverty rate: library districts in higher-poverty areas were on average treated earlier. This suggests that later-treated libraries have systematically lower poverty rates than those treated in earlier rounds. Consequently, estimates based on traditional TWFE methods may be biased, as later post-treatment periods rely on increasingly selective, lower-poverty comparison groups.

Table A.3.7: Regression of Treatment Year on 1990 ZIP Code-Level Poverty Rate

	Estimate	Std. Error	t value	$\Pr(>\|t\|)$
(Intercept)	2002.13	0.046	43377.1	$<$2e-16
Poverty Rate (1990)	-7.07	0.257	-27.48	$<$2e-16

Notes: Estimated using Equation 3.4. Poverty rate from 1990 Census.

3.A.4 Sensitivity Analysis: CSA Using Not-Yet-Treated as the Comparison Group

As a sensitivity check, I re-estimate the main effects using the Callaway and Sant'Anna (2021) difference-in-differences estimator, but instead of using never-treated libraries as the comparison group, I use not-yet-treated libraries. This approach restricts comparisons in each period to treated units and those that have not yet received treatment by that time, which could provide a more credible counterfactual if never-treated units differ systematically from those on a path to eventual treatment.

This re-specification allows me to assess the robustness of my results to alternative control group choices. While the sample size decreases slightly in later periods since more libraries receive treatment over time, the overall pattern of results is consistent with the main specification. Full estimates from this alternative comparison group are reported in Table A.3.8.

Table A.3.8: Callaway and Sant'Anna Results: Not Yet Treated as Control Group

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.273***	-0.0731*	0.0047	0.0429***	-0.0052	-0.0069	0.0192	0.033
	(0.0984)	(0.0409)	(0.0144)	(0.0124)	(0.0196)	(0.0095)	(0.0117)	(0.0236)
-4	-0.286***	0.0405*	0.0147	0.0348***	0.0022	-0.0128	0.0152	0.06***
	(0.0978)	(0.0217)	(0.0108)	(0.0107)	(0.0178)	(0.009)	(0.0111)	(0.0221)
-3	-0.1693*	-0.1508***	0.0228**	0.0302***	-0.0037	-0.0041	0.0234**	0.0327*
	(0.09)	(0.0152)	(0.0102)	(0.0089)	(0.0162)	(0.0078)	(0.0098)	(0.0171)
-2	-0.1718*	-0.035***	0.0031	0.0223***	-0.0058	-0.0153**	0.0098**	0.0207
	(0.09)	(0.0124)	(0.0075)	(0.0057)	(0.0123)	(0.0061)	(0.004)	(0.0132)
0	0.3896***	0.2823***	0	-0.012	-0.018	0.0095	-0.0089*	-0.0158
	(0.0844)	(0.0111)	(0.0087)	(0.0075)	(0.0139)	(0.0065)	(0.005)	(0.014)
1	-0.2112**	0.2348***	-0.0428*	-0.0298***	-0.0359**	-0.0425***	-0.0339***	-0.0237
	(0.0996)	(0.0133)	(0.0257)	(0.0087)	(0.0153)	(0.0079)	(0.0099)	(0.0208)
2	-0.6625***	0.1793***	-0.072**	-0.0546***	-0.0407**	-0.0607***	-0.0523***	-0.0341
	(0.1144)	(0.0161)	(0.0323)	(0.0112)	(0.0199)	(0.0091)	(0.0138)	(0.0294)
3	-0.6237***	0.1498***	-0.148**	-0.0672***	-0.0282	-0.0645***	-0.0812***	-0.0688
	(0.1206)	(0.022)	(0.0723)	(0.0142)	(0.0215)	(0.0122)	(0.0311)	(0.0492)
4	-0.515***	0.1061***	-0.1484**	-0.0785***	-0.0821***	-0.0819***	-0.0796**	-0.0708
	(0.129)	(0.0257)	(0.0639)	(0.0175)	(0.0233)	(0.014)	(0.0361)	(0.0522)
5	-0.5445***	0.0926**	-0.2144**	-0.0893***	-0.0109	-0.0533***	-0.1096**	-0.1084
	(0.1646)	(0.0386)	(0.0919)	(0.0244)	(0.0295)	(0.0177)	(0.0506)	(0.076)
N	135,811	84,728	135,811	135,811	135,811	135,811	135,811	135,811
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equations 3.2 and 3.3. Unbalanced panel used. Not yet treated used as control group. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level and adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels represent: * 10% level, ** 5% level, *** 1% level.

3.A.5 Sensitivity Analysis: Removing 1997

3.A.5.1 Removing All Libraries Treated in 1997

I re-ran the analysis excluding any library that was treated in 1997, given that the program guidelines changed between 1997 and 1998. In this version, I allow untreated libraries in states that were eligible in 1997 to remain in the sample, as they may serve as better controls for other treated libraries. I report results using the weighted TWFE and Callaway and Sant'Anna's estimator, as these approaches appear to be the least susceptible to bias.

Table A.3.9: TWFE Results: Re-Weighted, Removing 1997 Treated Units

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.3104***	0.2003***	-0.0219	0.028***	0.0222	0.0088	0.0091	0.1097***
	(0.1132)	(0.0471)	(0.0142)	(0.0091)	(0.0175)	(0.0078)	(0.01)	(0.031)
-4	-0.3793***	0.0978***	-0.0035	0.0199**	0.0318**	0.0037	0.0049	0.0973***
	(0.1133)	(0.0363)	(0.0112)	(0.0079)	(0.0148)	(0.0066)	(0.0062)	(0.0242)
-3	-0.0799	-0.0939***	0.0019	0.0169**	0.016	0.0014	0.0036	0.0629***
	(0.0982)	(0.0201)	(0.0084)	(0.0066)	(0.0128)	(0.0052)	(0.0044)	(0.0166)
-2	-0.2147***	-0.0714***	0.0006	0.0128***	-0.0008	-0.0131***	0.0047	0.033***
	(0.0781)	(0.0113)	(0.0062)	(0.0043)	(0.0108)	(0.0039)	(0.0035)	(0.011)
0	0.4327***	0.2931***	0.0168***	0	0.0031	0.0249***	0.0019	-0.0137
	(0.0843)	(0.0108)	(0.0055)	(0.0045)	(0.0108)	(0.0042)	(0.0026)	(0.0107)
1	-0.0606	0.284***	0.0255***	-0.0109*	-0.0295**	-0.029***	0.0015	0.0056
	(0.0933)	(0.0107)	(0.0077)	(0.0063)	(0.0131)	(0.0048)	(0.0041)	(0.0142)
2	-0.5288***	0.2546***	0.0228**	-0.0264***	-0.0226	-0.0434***	-0.0193***	0.0215
	(0.1059)	(0.0122)	(0.0097)	(0.0095)	(0.0142)	(0.0057)	(0.007)	(0.0181)
3	-0.4353***	0.2433***	0.0186	-0.0351***	-0.0134	-0.0451***	-0.0138*	0.0405*
	(0.1198)	(0.014)	(0.0116)	(0.0122)	(0.0161)	(0.0068)	(0.0071)	(0.0216)
4	-0.339***	0.2226***	0.0072	-0.049***	-0.043**	-0.0573***	-0.016**	0.0551**
	(0.1224)	(0.0161)	(0.014)	(0.0152)	(0.0173)	(0.0078)	(0.008)	(0.0248)
5	-0.3047**	0.2235***	0.005	-0.0587***	-0.0153	-0.0598***	-0.0343***	0.0542*
	(0.1493)	(0.0189)	(0.0163)	(0.0179)	(0.02)	(0.0095)	(0.0095)	(0.03)
N	133,388	83,208	133,388	133,388	133,388	133,388	133,388	133,388
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,432	4,432	4,432	4,432	4,432	4,432	4,432	4,432

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equation 3.1, with untreated units re-weighted based on inverse probability weights of being treated following Abadie (2005). Unbalanced panel used. Units treated in 1997 dropped from estimation. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level and adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Table A.3.10: Callaway and Sant'Anna Results: Removing 1997 Treated Units

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.2342**	-0.0728**	-0.0011	0.0563***	0.0182	0.0049	0.0143	0.0167
	(0.1117)	(0.0367)	(0.0232)	(0.0152)	(0.0253)	(0.0144)	(0.0202)	(0.0326)
-4	-0.2358**	0.0487**	0.0014	0.0449***	0.0163	-0.0042	-0.0038	0.0399
	(0.1052)	(0.024)	(0.0183)	(0.0147)	(0.0196)	(0.0118)	(0.0178)	(0.0319)
-3	-0.1803*	-0.1294***	0.002	0.0321***	0.0025	-0.0037	0.0023	0.0143
	(0.0944)	(0.0202)	(0.016)	(0.0117)	(0.0175)	(0.0098)	(0.0126)	(0.0226)
-2	-0.0986	-0.0282	-0.0052	0.0222***	-0.0081	-0.0163**	-0.0053	0.0159
	(0.0865)	(0.0178)	(0.0107)	(0.0071)	(0.0135)	(0.0067)	(0.0078)	(0.0159)
0	0.4387***	0.262***	-0.0113	-0.0149*	-0.0182	0.0102	0.0026	-0.0265
	(0.0909)	(0.0181)	(0.0153)	(0.0082)	(0.0151)	(0.0062)	(0.012)	(0.0197)
1	-0.2139	0.2014***	-0.1049*	-0.0362***	-0.0293*	-0.0408***	-0.0565*	-0.0933**
	(0.1324)	(0.0235)	(0.0548)	(0.0104)	(0.0167)	(0.0091)	(0.0305)	(0.0419)
2	-0.7262***	0.1531***	-0.121**	-0.0641***	-0.0439**	-0.0646***	-0.0734**	-0.0882*
	(0.1236)	(0.0243)	(0.0613)	(0.0121)	(0.0202)	(0.0097)	(0.031)	(0.0499)
3	-0.6254***	0.1343***	-0.1303**	-0.0731***	-0.0402*	-0.0714***	-0.0736**	-0.0728
	(0.1257)	(0.0258)	(0.0587)	(0.0157)	(0.0215)	(0.0117)	(0.0328)	(0.0494)
4	-0.4745***	0.1062***	-0.1423**	-0.0841***	-0.0848***	-0.0873***	-0.0736**	-0.0648
	(0.1328)	(0.0266)	(0.0599)	(0.0192)	(0.0239)	(0.0144)	(0.0332)	(0.0543)
5	-0.5635***	0.0926**	-0.2001**	-0.098***	-0.0136	-0.0631***	-0.094*	-0.0972
	(0.1655)	(0.0388)	(0.0899)	(0.0262)	(0.0295)	(0.0198)	(0.051)	(0.075)
N	133,388	83,208	133,388	133,388	133,388	133,388	133,388	133,388
Untreated Districts	4,051	4,051	4,051	4,051	4,051	4,051	4,051	4,051
Treated Districts	4,432	4,432	4,432	4,432	4,432	4,432	4,432	4,432

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equations 3.2 and 3.3. Unbalanced panel used. Never treated used as control group. Units treated in 1997 dropped from estimation. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are bootstrapped and are robust to multiple inference. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

3.A.5.2 Removing All Libraries Treated in 1997 and Eligible States

I also re-ran the analysis excluding both libraries that were treated in 1997 and libraries located in states that were eligible in 1997. This specification builds on the prior analysis by additionally removing untreated libraries in eligible states. This helps address potential selection bias: because treatment was not randomly assigned, removing only the treated libraries could alter the composition of the sample in a way that biases comparisons between treated and control groups. I again report results for the weighted TWFE and Callaway and Sant'Anna.

Table A.3.11: TWFE Results: Re-Weighted, Removing 1997 Eligible and Treated Units

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.4074***	0.2041***	-0.0184	0.0332***	0.0266	0.0171**	0.0044	0.1037***
	(0.1198)	(0.0446)	(0.0135)	(0.0084)	(0.0182)	(0.0078)	(0.0106)	(0.0313)
-4	-0.4007***	0.1117***	0.0009	0.0259***	0.038**	0.0096	-0.0007	0.102***
	(0.1191)	(0.0355)	(0.0102)	(0.007)	(0.0156)	(0.0066)	(0.0059)	(0.024)
-3	-0.1384	-0.0877***	0.0083	0.0226***	0.0148	0.0012	0.005	0.0611***
	(0.1037)	(0.0203)	(0.0078)	(0.0055)	(0.0137)	(0.0052)	(0.0044)	(0.0163)
-2	-0.2763***	-0.061***	0.0044	0.0137***	-0.0061	-0.0123***	0.006*	0.0164
	(0.0826)	(0.0108)	(0.006)	(0.0038)	(0.0114)	(0.004)	(0.0036)	(0.0108)
0	0.3981***	0.2775***	0.0091	-0.0042	0.0012	0.0232***	-0.0027	-0.0263**
	(0.0874)	(0.0104)	(0.0057)	(0.0041)	(0.0113)	(0.0044)	(0.0033)	(0.0115)
1	-0.1455	0.2866***	0.0211***	-0.016***	-0.034**	-0.0306***	-0.0011	-0.0014
	(0.0973)	(0.0104)	(0.0079)	(0.0061)	(0.0141)	(0.0051)	(0.0044)	(0.015)
2	-0.6011***	0.2626***	0.0232**	-0.0285***	-0.0218	-0.0459***	-0.0206***	0.0149
	(0.1119)	(0.0123)	(0.0101)	(0.0101)	(0.0152)	(0.006)	(0.0074)	(0.0191)
3	-0.5793***	0.2589***	0.0248**	-0.0298**	-0.0099	-0.0462***	-0.0144*	0.0385*
	(0.1261)	(0.0141)	(0.012)	(0.0134)	(0.0173)	(0.0072)	(0.0075)	(0.0229)
4	-0.527***	0.2456***	0.0176	-0.0385**	-0.038**	-0.0592***	-0.0166**	0.0515**
	(0.1266)	(0.0164)	(0.0147)	(0.0169)	(0.0186)	(0.0081)	(0.0084)	(0.0262)
5	-0.5943***	0.2553***	0.0219	-0.0432**	-0.0129	-0.0661***	-0.037***	0.0436
	(0.1574)	(0.0193)	(0.0173)	(0.02)	(0.0213)	(0.0099)	(0.0099)	(0.032)
N	125,943	78,560	125,943	125,943	125,943	125,943	125,943	125,943
Untreated Districts	3,976	3,976	3,976	3,976	3,976	3,976	3,976	3,976
Treated Districts	4,038	4,038	4,038	4,038	4,038	4,038	4,038	4,038

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equation 3.1, with untreated units re-weighted based on inverse probability weights of being treated following Abadie (2005). Unbalanced panel used. Units eligible for treatment or treated in 1997 dropped from estimation. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are clustered at the library district level and adjusted for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Table A.3.12: Callaway and Sant'Anna Results: Removing 1997 Eligible and Treated Units

Time Since Treatment	Capital Expenditure	Public Use Computers	Total Visits	Total Circulation	Collection Expenditure	Operating Expenditure	Hours Open	Reference Transactions
-5	-0.2582**	-0.0728*	0.0028	0.0577***	0.0107	0.0105	0.0056	0.0084
	(0.126)	(0.0394)	(0.0256)	(0.0154)	(0.0267)	(0.0147)	(0.0221)	(0.0384)
-4	-0.217*	0.0541**	-0.0052	0.0381***	0.0192	-0.0015	-0.0219	0.0366
	(0.1118)	(0.0272)	(0.0216)	(0.0143)	(0.0208)	(0.0132)	(0.0185)	(0.0357)
-3	-0.2076**	-0.1266***	0.008	0.0264**	0.002	-0.0058	-0.0102	0.0052
	(0.1016)	(0.0208)	(0.016)	(0.0109)	(0.0189)	(0.0106)	(0.0129)	(0.0248)
-2	-0.1777*	-0.0275	-0.0022	0.0172**	-0.0111	-0.0164**	-0.0058	0.0092
	(0.1005)	(0.0176)	(0.0117)	(0.008)	(0.0143)	(0.0081)	(0.0088)	(0.0185)
0	0.3627***	0.2492***	-0.0532**	-0.0242***	-0.027*	0.0051	-0.0379**	-0.099***
	(0.0994)	(0.0183)	(0.0229)	(0.0088)	(0.0157)	(0.0077)	(0.0186)	(0.0337)
1	-0.3171**	0.2386***	-0.0504**	-0.0413***	-0.05***	-0.0502***	-0.0478***	-0.0767**
	(0.1405)	(0.0185)	(0.0237)	(0.0104)	(0.0175)	(0.0088)	(0.0177)	(0.0349)
2	-0.7874***	0.1947***	-0.0568**	-0.0654***	-0.0607***	-0.0739***	-0.0646***	-0.0689*
	(0.1162)	(0.0203)	(0.0262)	(0.0117)	(0.0215)	(0.0107)	(0.0189)	(0.0361)
3	-0.7361***	0.1804***	-0.0606**	-0.068***	-0.0513**	-0.0781***	-0.0625***	-0.0435
	(0.1287)	(0.0215)	(0.0266)	(0.0152)	(0.0232)	(0.0114)	(0.0192)	(0.0401)
4	-0.5195***	0.16***	-0.0723**	-0.0771***	-0.0906***	-0.0968***	-0.0639***	-0.0391
	(0.1392)	(0.0239)	(0.0293)	(0.02)	(0.025)	(0.0149)	(0.0187)	(0.0409)
5	-0.8322***	0.1783***	-0.0928**	-0.0862***	-0.0362	-0.0812***	-0.085***	-0.0664
	(0.1693)	(0.0327)	(0.0432)	(0.0282)	(0.0283)	(0.02)	(0.0291)	(0.0625)
N	125,943	78,560	125,943	125,943	125,943	125,943	125,943	125,943
Untreated Districts	3,976	3,976	3,976	3,976	3,976	3,976	3,976	3,976
Treated Districts	4,038	4,038	4,038	4,038	4,038	4,038	4,038	4,038

Notes: All outcomes are log transforms of the listed variables. All dollar values are reported in 1992 dollars. Estimated using Equations 3.2 and 3.3. Unbalanced panel used. Never treated used as control group. Units eligible for treatment or treated in 1997 dropped from estimation. N refers to the total number of observations used in the estimation; it is lower for some outcomes because the data are unavailable in earlier years. The excluded reference year is -1. The standard errors reported in parentheses below the estimates are bootstrapped and are robust to multiple inference. Significance levels represent: * 10% level, ** 5% level, *** 1% level.

Chapter 4: Tax Incentives and Neighborhood Change: Estimating the Impact of the New Market Tax Credit

4.1 Introduction

Persistent variation in economic outcomes across regions in the US is well documented, including disparities in unemployment, productivity, wages, and poverty rates (Austin et al., 2018; Autor, 2019; Kline & Moretti, 2013, 2014b; Moretti, 2011; Neumark & Simpson, 2015). In response, policymakers at the federal, state, and local levels have increasingly turned to place-based policies. Although there are theoretical foundations for such policies, the empirical literature offers mixed results. Even studies of the same program often arrive at different conclusions, leaving no clear consensus on their effectiveness or mechanisms. One such policy that remains relatively understudied is the New Markets Tax Credit (NMTC)

The NMTC is a federal place-based policy designed to increase investment in low-income communities by offering large federal tax credits to investors in approved projects. The program supports a wide range of activities, including the rehabilitation, construction, and acquisition of commercial real estate, as well as low-cost business financing. A small but growing literature has examined the effects of the NMTC on local economic outcomes, including poverty rates, employment, and wages in targeted census tracts (Freedman, 2012, 2015). These studies find evidence that the NMTC can reduce poverty and increase access to higher-paying jobs. However, they also document longer commute times for local workers, raising questions about whether the benefits of the policy accrue to original residents. To date, empirical research on the NMTC remains limited, with no studies examining outcomes beyond 2009. My research updates and extends this literature by incorporating more recent data and analyzing a broader set of economic indicators, providing an updated assessment of the program's impacts.

There are several potential ways the NMTC could affect local economic outcomes. By subsidizing private investment in low-income communities, the NMTC can lower the cost of capital for businesses, which can stimulate job creation and local economic activity (Bartik, 2020a, 2020b). These investments may lead to higher employment and wages, particularly in firms that benefit directly from the investment. At the same time, increased investment can improve local amenities and infrastructure, which may enhance the quality of life for residents or improve efficiency of local businesses. However, the effects of place-based investment policies like the NMTC are complex and can depend on how mobile residents and businesses are. If new jobs are filled by in-movers rather than existing residents, or if rising rents and property taxes displace low-income households, the benefits may not accrue to the original target population (Kline & Moretti, 2013; Neumark & Simpson, 2015). Moreover, the direct benefits to local residents may be limited if businesses receiving NMTC subsidies rely on commuting workers or capital-intensive production that generates relatively few jobs. Understanding the extent to which these channels operate, and for whom, remains a key empirical question.

In this paper, I study the effects of NMTC investment on local economic and demographic outcomes to assess whether the program achieves its stated goals of promoting job creation and improving conditions in low-income communities. In particular, I examine changes in population composition, labor market indicators, and housing dynamics to identify shifts in neighborhood conditions and potential migration and labor responses resulting from NMTC investment. To do so, I use administrative data on NMTC investments linked to census tract characteristics over time. I exploit a policy rule that makes census tracts eligible for investment if their median family income (MFI) falls below 80% of the greater area's MFI, defined as the higher of the state or metropolitan statistical area (MSA) median. I implement a fuzzy regression discontinuity design (RDD), using this MFI ratio threshold as an instrument for NMTC investment. This strategy follows Freedman (2012; 2015), who uses the approach to examine the causal impacts of NMTC investment on poverty and employment. Because investment depends on developer behavior and project approval, not eligibility alone, the fuzzy RDD isolates plausibly exogenous variation in investment intensity near the cutoff. My contribution extends this research by leveraging more recent data and examining a broader set of outcomes.

I construct a panel dataset of census tracts by combining data from multiple sources. NMTC investment data come from the Community Development Financial Institutions (CDFI) Fund, which provides annual information on location and size of approved NMTC projects. I use the National Cancer Institute's (NCI) SEER*Stat database to obtain annual tract-level estimates of total population by race and age group, which serve as demographic outcomes. I then incorporate Census data from the American Community Survey (ACS) to identify eligible tracts using the 2006–2010 five-year estimates, control for baseline covariates using the 2008–2012 five-year estimates, and measure post-treatment outcomes, such as labor force participation, employment, housing characteristics, and household structure, using the 2013–2017, 2014–2018, and 2015–2019 five-year files.

I examine a broad set of demographic, labor market, and housing outcomes to capture the channels through which NMTC investment may influence local communities. For demographic changes, I study total population, racial and ethnic composition, age structure, household types, and population shares by marital status and educational attainment. Overall, the results do not provide statistically significant evidence that NMTC investment affects these outcomes. For labor market outcomes, I examine employment-to-population ratios, labor force participation, unemployment, median earnings, usual hours worked, and indicators of public assistance or poverty. Although NMTC investment is designed to spur economic development, I find little evidence of improved labor market outcomes. If anything, the estimates suggest statistically imprecise declines in employment. These findings are inconsistent with the program's goals and with Freedman (2012) and reflect the broader pattern in the place-based policy literature of the same program yielding different results across time and context. Variation in implementation, local conditions, or project types may be contributing to treatment effects that are not stable or easily generalized.

Finally, I examine housing market indicators, including housing costs, home values, household sizes, and the vintage of housing units and resident tenure. For owner-occupied units, NMTC investment is associated with an initial decline in median monthly housing costs and home values, followed by an increase by 2019. The median year residents moved-in and the median year units were constructed also rose initially but reversed by 2019. For renter-occupied units, NMTC investment leads to sizable and statistically significant increases in gross rent by 2019. Overall, while the results point to changes in neighborhood housing dynamics, they do not provide a clear picture of the program's effects on affordability or housing access for existing residents in the short or medium term.

To explore whether the effects of NMTC investment differ based on local community characteristics, I conduct heterogeneity analyses by housing vacancy rates and per capita income. These factors serve as proxies for housing supply flexibility and local economic conditions, respectively. Overall, I find little consistent evidence of meaningful impacts across these subgroups. There is some suggestive evidence of housing market effects in higher-vacancy areas and possible labor market responses among women and housing market changes in lower-per-capita income tracts. However, these findings are generally imprecise and should be interpreted cautiously.

In the Appendix, I present a variety of tests to assess the robustness of my results. First, I conduct placebo tests using arbitrary eligibility thresholds. Reassuringly, these placebo cutoffs yield no statistically significant effects in either the first or second stage, suggesting that the observed results are not spurious. Second, I further test the sensitivity of my findings to model specification by varying the set of covariates, the kernel type, the order of the control function, and the bandwidth. The estimates remain largely stable across these alternative specifications, indicating that the main results are not driven by particular functional form assumptions or covariate choices.

To my knowledge, this is the first paper to estimate the effects of NMTC investment beyond 2009, offering new evidence on the program's impact in the aftermath of the Great Recession and following a program update that shifted investment focus away from real estate towards business investment. By applying the same empirical strategy as Freedman (2012, 2015), I am able to replicate his findings in earlier years, but my updated analysis yields different labor market results—highlighting the challenges of drawing consistent conclusions about place-based policies. Overall, the findings offer little evidence of broad economic improvement for existing residents. As the scale of the NMTC program continues to grow, these results underscore the need for continued evaluation and a better understanding of the conditions under which place-based investments succeed.

4.2 Institutional Details

4.2.1 Overview of the New Markets Tax Credit

The NMTC is a place-based policy created by the Community Renewal Tax Relief Act of 2000 as part of a broader reform effort to improve the economic conditions of low-income communities (LICs). The program operates as follows: Community Development Entities (CDEs)—legal entities serving LICs—develop project proposals and apply for tax credit allocations from the Community Development Financial Institutions Fund (CDFI Fund), an agency of the Treasury. The CDFI Fund competitively selects CDE projects and allocates tax credits, up to the maximum annual amount approved by Congress. Selected CDEs then raise funding for their projects by offering the tax credits to individual and corporate investors who make qualified equity investments. Investors claim the tax credit, equal to 39% of their investment, over seven years, while the CDE implements the project in the LIC.[1] Figure 4.1 illustrates the flow of funding and tax credits across the different entities.

Similar to Enterprise Zones, another place-based policy, NMTC eligibility is based on economic distress. Census tracts are eligible to be LICs if they meet one of two primary criteria: (1) a poverty rate of at least 20%, or (2) a median family income (MFI) that does not exceed 80% of the area MFI.[2] Following a 2012 eligibility update, 30,021 census tracts (41% of all tracts)

[1]Investors can claim 5% of their investment amount as federal tax credits in the first three years and 6% in the remaining four years, totaling 39%.

[2]For tracts located in Metropolitan Statistical Areas (MSAs), the area MFI is the greater of the MSA MFI or the state MFI. Additional eligibility criteria include: (3) MFI not exceeding 85% of the state MFI in high-migration rural counties; (4) tracts with fewer than 2,000 residents located in a Federally designated Empowerment Zone and contiguous to at least one other LIC; and (5) targeted populations of low-income individuals or Gulf Opportunity Zone populations in otherwise non-LIC tracts. See CDFI (2022a) for more details.

were LIC-eligible. Similar to Opportunity Zones, another place-based policy, a wide range of projects are eligible for NMTC funding, including rehabilitation, construction, and acquisition of commercial real estate, as well as low-cost business financing. The core requirement of the program is that CDEs must invest substantially all of their raised equity into low-income businesses located within LICs.[3] Between 2013 and 2019, $17.53 billion in tax credits were allocated to support business investments, while an additional $6.46 billion financed commercial real estate developments, including community facilities (CDFI, 2022a, 2022b).[4]

The involvement of CDEs in the allocation process distinguishes the NMTC from other place-based policies. CDEs can be a variety of entities, but to be certified as a CDE, they must primarily serve LICs and in principle remain accountable to the residents of the communities they serve. Within these broad requirements, CDEs can take many forms, including CDFI Fund designations, mission-driven organizations, for-profit financial and non-financial institutions, governmental and quasi-governmental entities, and nonprofit organizations.[5] Between 2001 and 2019, CDFI designations and mission entities received nearly 60% of total allocation authority. For-profit financial and non-financial institutions accounted for roughly a quarter, while the remaining 15% was split 2:1 between governmental or quasi-governmental and nonprofit non-financial institutions (Theodos et al., 2021b). Compared to local or state governments selecting programs, in principle CDEs may better target investments to community needs, as they are locally focused and intended to be accountable to the communities they serve. Between 2013 and 2019, an average of 243 CDEs applied for NMTC allocations annually, with 34% receiving allocations,

[3]Eligible investments include: (1) capital or equity investments or loans to qualified active low-income businesses; (2) purchasing loans from other CDEs; (3) equity investments or loans in other CDEs; and (4) financial counseling or services to LIC businesses or residents.

[4]See Figure A.4.1 in Appendix 4.A.1 for further details on NMTC disbursements by project type.

[5]Both for-profit and nonprofit CDEs can receive tax credit allocations from the CDFI Fund; however, nonprofit CDEs must create for-profit subsidiaries to allocate tax credits and receive funding from NMTC investors.

averaging $48 million per project.

To contextualize the NMTC within the broader landscape of place-based policies and highlight its distinguishing features, Table 4.1 summarizes key similarities and differences between the NMTC and other major tax-based place-based programs. The NMTC offers tax benefits and low-cost loans to businesses, similar to the Empowerment Zones, Enterprise Communities and Renewal Communities programs, while providing tax benefits to corporate and individual investors, like the Opportunity Zones program. Additionally, the NMTC program is on a scale similar to, if not larger, other place-based policies and it is growing. Figure 4.2 shows that the program growth over time, with nominal annual funding ranging from $1 billion to $5.6 billion, averaging $3.6 billion. By the end of the 2020 fiscal year, the NMTC program had awarded over $61 billion tax credits. With the Consolidated Appropriations Act of 2020 extending the program through 2025, $5 billion in tax credits will be available annually (Moore, 2021). Its substantial scale, continued federal support, and the fact that it shares features with multiple place-based policy models while remaining institutionally distinct make the NMTC an interesting subject for policy analysis and empirical evaluation.

4.2.2 Prior Literature

The broader literature on place-based policies offers several theoretical justifications for their use, particularly in the presence of market frictions, imperfections, or externalities (Kline & Moretti, 2013).[6] In the US, persistent spatial variation in unemployment, productivity, wages, and poverty rates suggests the potential for efficiency gains through place-based interventions

[6]Other theoretical justifications, as outlined in Neumark and Simpson (2015), include agglomeration economies, knowledge spillovers, industry localization, spatial mismatch, network effects, and equity motivations.

(Austin et al., 2018; Autor, 2019; Freedman & Neumark, 2024; Kline & Moretti, 2014b; Moretti, 2011; Neumark & Simpson, 2015). However, distributional efficiency is a concern if the newly created jobs are taken by non-residents, if rising rents or property taxes displace original low-income residents, or if other mechanisms prevent program benefits from reaching the targeted population (Bartik, 2020a).

Measuring program benefits is further complicated by the heterogeneity of interventions and their context-specific effects. Consequently, the empirical literature produced mixed results. Some studies identified short-term employment and growth benefits from place-based policies (Busso et al., 2013; Freedman, 2013; Kolko & Neumark, 2010), while others found no significant employment effects (Neumark & Kolko, 2010; Neumark & Young, 2021). Long-run evidence was similarly inconclusive: Neumark and Young (2021) reported no lasting benefits from state Enterprise Zones, whereas Kline and Moretti (2014a) documented sustained increases in manufacturing employment resulting from the Tennessee Valley Authority program.

A generally underexplored aspect of place-based policies is their potential to induce displacement and gentrification. Recent theoretical work highlighted these concerns, emphasizing that while such policies may increase employment, they also risk raising property values and shifting benefits toward higher-income residents (Bartik, 2020a, 2020b). However, empirical evidence on these outcomes remains limited. Kennedy and Wheeler (2021) found that Opportunity Zones may have disproportionately benefited high-income investors in less disadvantaged areas, while Busso et al. (2013) found no evidence of gentrification effects from Enterprise Zones, likely because employment credits were only available to resident workers.

The NMTC also remains relatively under-studied. There is some descriptive evidence of NMTC, for example the CDFI Fund claims that each $1 of NMTC tax credits generates an additional \$8 of private investment (CDFI, 2022a); however, Gurley-Calvez et al. (2009) argues that only a small portion of NMTC funds represents genuinely new investment, with much of it reflecting reallocated capital. Other descriptive evidence suggests that NMTC investments often flow to census tracts already undergoing economic improvement (Corinth et al., 2025; Layser, 2021; Theodos et al., 2021a).

Empirical evidence on the impact of the NMTC is limited but offers some insights. Freedman (2012) found that NMTC investments improve neighborhood conditions, including higher home values, lower poverty and unemployment rates, and increased private employment—but also that it increased housing turnover. Freedman also found a decline in low-paying jobs and an increase in medium- and high-paying jobs in treated census tracts. In a follow-up study, Freedman (2015) showed that NMTC investments shifted employment composition, reducing service-sector jobs while increasing goods-producing employment. Freedman also found that NMTC investments increased the share of workers of the tract commuting 20+ miles, though there was no significant change in commuting patterns for residents of the treated tracts, suggesting that local jobs may be going to commuters rather than residents.[7]

These two papers provide evidence of positive employment effects from NMTC investment but suggest that the benefits may accrue to commuters rather than local residents. In this paper, I expand upon Freedman's papers in three key ways: (1) I examine more recent outcomes using five-year ACS estimates from 2013 to 2019, whereas Freedman's analysis covers results through 2009; (2) I expand the set of outcomes to include measures of geographic mobility; and (3) I examine heterogeneous treatment effects across community characteristics to assess whether the

[7]Freedman showed that the communities sending 20+ mile commuters to treated tracts are on average more affluent than the treated tracts themselves.

policy exhibits differential effectiveness depending on community context.

4.3 Conceptual Framework

The conceptual framework presented here is a simplified version of the spatial equilibrium model outlined in Busso et al. (2013), which builds on the classic spatial equilibrium models by Rosen (1979) and Roback (1982). This framework incorporates heterogeneity in the valuation of local amenities, commuting cost preferences, and individual wealth levels. The primary modifications from Busso et al. (2013) include defining neighborhoods by NMTC eligibility and receipt rather than Enterprise Zone eligibility, removing sectoral employment choices, and introducing exogenous wealth levels.

4.3.1 Worker Utility and Location Choice

Assume there is a finite population of workers who choose where to live and work. Assume also that there is also a finite set of neighborhoods, denoted by $N = \{N_0, N_1, N_2\}$, where: N_0 neighborhoods are not eligible for NMTC, N_1 neighborhoods are eligible for and receive NMTC, and N_2 neighborhoods are eligible but do not receive NMTC.

Neighborhoods have a set of amenities valued by both workers and firms, and commuting between neighborhoods incurs a cost. Workers inelastically demand one unit of housing at market rental rates. The utility function of worker i, living in neighborhood j, and working in neighborhood k, is given by:

$$u_{ijk} = I_i + w_k - r_j - c_{jk} + A_j + \epsilon_{ijk} \tag{4.1}$$

$$u_{ijk} = I_i + v_{jk} + \epsilon_{ijk} \tag{4.2}$$

where: I_i is the worker's exogenously assigned wealth level, drawn from a continuous distribution; w_k is the wage received when working in neighborhood k; r_j is the rental price in neighborhood j; c_{jk} is the commuting cost from neighborhood j to k; A_j represents the amenity level in neighborhood j; and ϵ_{ijk} captures idiosyncratic preferences for amenities and commuting costs, assumed to be i.i.d. across individuals and drawn from a continuous multivariate distribution independent of I_i and v_{jk}.

The heterogeneity in ϵ_{ijk} allows some workers to be inframarginal, enabling the potential for economic rents. I then define a set of indicators $\{D_{ijk}\}$ equal to one if $\max_{j'k'}\{u_{ijk}\} = u_{ijk}$, where $j', k' \in N$. The number of workers for each residential/work location pair is then given by:

$$N_{jk} = P(D_{ijk} = 1 \,|\, \{v_{j'k'}\}) \tag{4.3}$$

4.3.2 Firms and Production

As in Busso et al. (2013), assume there is a finite number of firms producing a single final output x, sold at a fixed price of 1. Goods are produced using a constant returns to scale production function:

$$F(K_k, B_k L_k) = B_k L_k f(\chi_k) \tag{4.4}$$

where: K_k and L_k denote capital and labor inputs, respectively; B_k is the productivity level of neighborhood k, which depends on amenity levels A_k; and $\chi_k = \frac{K_k}{B_k L_k}$ is the capital-to-effective-labor ratio.

All workers are perfect substitutes in production, and capital is supplied inelastically at price ρ. The firm's first-order conditions are:

$$w_k = B_k[f(\chi_k) - \chi_k f'(\chi_k)] \tag{4.5}$$

$$\rho = f'(\chi_k) \tag{4.6}$$

Inverting the second equation and plugging back into the wage equation yields the following:

$$\chi_k = h(\rho), \quad \text{where } h'(\cdot) \leq 0 \tag{4.7}$$

$$w_k = B_k[f(h(\rho)) - h(\rho)\rho] \tag{4.8}$$

where $f(h(\rho)) - h(\rho)\rho$ is the marginal product of a "raw" unit of labor. Differences in firm productivity across neighborhoods lead to variations in input demand, which, combined with heterogeneous worker preferences, generate wage differences across neighborhoods.

4.3.3 Housing Supply

As in Busso et al. (2013), for all neighborhoods, I assume upward-sloping housing supply curves and the presence of risk-neutral landowners who decide whether to develop housing on their plots. If a landowner develops their land, they receive r_j minus the cost of development. Assume that the marginal landowner in each neighborhood breaks even and let H_j denote the number of rented units in neighborhood j. Finally, assume that housing markets clear.

4.3.4 Predicted Effects of NMTC Investment

I now use this conceptual framework to outline predicted effects of NMTC investment. The NMTC program supports a range of projects that may influence communities either by increasing neighborhood amenities—for instance, through the development of community centers or healthcare facilities—or by lowering operational costs for firms via low-interest loans or business subsidies. Within this framework, all else equal, neighborhoods with higher amenity levels and wages should experience greater demand, leading to increases in rental prices. Given exogenous wealth and heterogeneous preferences, this shift in demand is expected to induce sorting across neighborhoods.

A positive amenity shock, such as NMTC investment, increases neighborhood productivity B_k, which raises both wages. Rental prices should also rise with the magnitude depending on the level of the demand shift and the housing supply elasticity. Similarly, a shock that reduces firm costs increases employment through an income effect, making the neighborhood more attractive. The conceptual framework predicts that with low housing supply elasticity, rental prices increase substantially following an amenity shock. In contrast, if housing supply elasticity is high, the primary response is a large population increase with little to no change in rental prices. Finally, if worker preferences are highly heterogeneous, with few workers at the margin of moving, the effects on both population and rental prices should be minimal. This gives me a baseline set of expectations to use in my analysis of the NMTC.

4.4 Analysis of NMTC Investments

In this paper, I expand upon previous work investigating the causal effects of the NMTC, following the methodology of Freedman (2012, 2015). Like Freedman, I exploit the discontinuity in census tract eligibility based on the median family income (MFI) ratio using a fuzzy regression discontinuity (RDD) framework. However, I extend this approach by incorporating the updated eligibility thresholds adopted in 2012 and focus on outcomes from 2013 to 2019.

4.4.1 Regression Discontinuity Design

To estimate the effect of NMTC investment on an outcome y, the naive baseline equation of interest is:

$$y_i = \beta_0 + \beta_1 N_i + X_i\gamma + \text{MSA}_i + \epsilon_i, \tag{4.9}$$

where: y_i is the outcome variable in census tract i; N_i is a dummy variable equal to 1 if the census tract received any NMTC investment between 2013 and the outcome year; X_i is a set of pre-treatment baseline characteristics of the census tract measured in 2012; and MSA_i is an MSA fixed effect.[8] The coefficient of interest, β_1, is intended to capture the effect of NMTC investment on the relevant outcome.

However, even with a robust set of controls in X_i, unobserved characteristics may still bias the estimates. For example, census tracts with more proactive community and business leaders may be more likely to attract NMTC projects, while also being more prone to positive economic growth, leading to upward-biased estimates of β_1. To address this endogeneity, I take advantage

[8]For census tracts not located in an MSA, the MSA fixed effect is replaced with a state fixed effect.

of the discontinuity in census tract eligibility for NMTC investment, as NMTC projects can only be initiated in Low-Income Community (LIC) designated census tracts. The two primary criteria for LIC status are: the census tract MFI does not exceed 80% of the local area MFI (the MFI ratio rule); and the census tract poverty rate is 20% or higher.[9]

Figure 4.3 illustrates the sharp drop-off in LIC eligibility at the 80% MFI ratio threshold and the corresponding change in the average number of census tracts receiving NMTC projects across this threshold. Using this discontinuity in eligibility and a fuzzy RDD framework, I focus on a window of census tracts around the MFI ratio cutoff, where LIC eligibility is effectively randomly assigned. This allows MFI ratio to be used as an instrument for NMTC investment. The first- and second-stage equations are:

$$N_i = \alpha_0 + \alpha_1 D_i + f(\text{MFIR}_i) + X_i\phi + \text{MSA}_i + u_i \tag{4.10}$$

$$y_i = \eta_0 + \eta_1 \hat{N}_i + f(\text{MFIR}_i) + X_i\mu + \text{MSA}_i + v_i \tag{4.11}$$

where: D_i is an indicator variable equal to 1 if census tract i's MFI ratio is less than or equal to 80%; and $f(\text{MFIR}_i)$ is a control function of the running variable MFIR_i that allows for a slope change at the cutoff.[10]

The coefficient of interest, η_1, provides the IV estimate of β_1, which can be interpreted as the local average treatment effect (LATE). In this context, it identifies the causal effect of NMTC

[9]The local area MFI is defined as the higher of the state MFI and the MSA MFI. For census tracts not located in MSAs, the state MFI is used.

[10]The control function takes the form:

$$f(\text{MFIR}_i) = \sum_{k=1}^{l} [\tau_{1k}(\text{MFIR}_i - 0.8)^k + \tau_{2k} D_i (\text{MFIR}_i - 0.8)^k],$$

where l is the polynomial order. Following Gelman and Imbens (2019), I will restrict the control function to linear and quadratic forms to avoid overfitting.

investment on the outcome y for the subset of compliers: census tracts whose treatment status is influenced by their marginal eligibility based on the MFI ratio threshold. These are the tracts that receive NMTC investment because they narrowly meet the eligibility criteria, but would not have received it otherwise.[11] The regressions in this paper adhere to best practices in regression discontinuity (RD) design, incorporating methodological innovations from recent literature. The primary specifications employ a 10% bandwidth around the MFI ratio threshold, with robustness checks using alternative bandwidths selected via the optimal procedure proposed by Calonico et al. (2020). Standard errors are clustered at the MSA level or at the state level when a census tract is not in a MSA. To address concerns about multiple hypothesis testing, all standard errors are adjusted using the Benjamini and Hochberg (1995) method.

Although manipulation of the running variable is theoretically possible, it is highly unlikely in this context because census tract-level MFIs are constructed as aggregate measures based on data from hundreds to thousands of families that are then averaged over a five-year period. These features make precise manipulation infeasible. Nevertheless, to formally assess the validity of the RDD design, I implement the McCrary density test to detect any discontinuities in the distribution of the running variable around the eligibility threshold (McCrary, 2008).[12] Figure 4.4 presents the results of the McCrary test for the MFI ratio threshold. The test yields a p-value of 0.395, indicating no statistically significant discontinuity in the density at the cutoff and providing no evidence of manipulation or sorting around the threshold.

[11] Given the multiple criteria for LIC eligibility, it would also be feasible and potentially informative to estimate a two-instrument version of the regression, using both the MFI ratio and the poverty rate as instruments for NMTC investment.

[12] The measurement of the running variable occurred prior to the policy announcement, meaning census tracts had no opportunity to manipulate their MFI values in anticipation of eligibility. Even if tracts had foreknowledge of both the measurement period and the eligibility criteria, it would be nearly impossible to shift tract- or MSA-level median family income with sufficient precision to affect eligibility. Any such manipulation, if it occurred, would likely be partial and, as noted by McCrary (2008), does not necessarily undermine identification.

4.4.2 Data

My analysis uses NMTC project-level data from the CDFI Fund and census tract characteristics from the five-year American Community Survey (ACS) and the National Cancer Institute (NCI) to construct a panel of census tract-level eligibility, NMTC investment information, outcomes, and covariates.

4.4.2.1 CDFI Fund Data

The CDFI Fund's 2020 NMTC public data release provides detailed information on NMTC treatment, including the total value of the project and tax credits, and the type of investment by project (CDFI, 2021). I use these data to construct census tract measures of NMTC investment including cumulative NMTC investment, cumulative project counts, and indicator variables for whether a census tract ever received NMTC investment during three periods: 2000–2009, 2009–2013, and 2013 onward through the relevant outcome year.[13] I also use the CDFI Fund's list of tracts eligible under the Gulf Opportunity Zone and high-migration rural tract provisions to exclude those tracts from the analysis.[14]

[13] I separate prior NMTC investments into those made prior to the eligibility update (2000–2009) vs more recent investments (2009–2013) to allow for potentially different effects. Cumulative NMTC amounts and project counts are measured through the end of the relevant five-year ACS measurement period. This may attenuate the estimates toward zero, as the averaging period includes up to four years in which a given census tract may not have received treatment. Thus, the absolute values of the estimates presented here should be interpreted as lower bounds on the true effects.

[14] While Go Zone and high-migration rural tracts represent interesting populations in their own right, they are excluded from this analysis as they qualify under different eligibility criteria.

4.4.2.2 Five-Year American Community Survey Data

To construct outcome variables, I use the 2013–2017, 2014–2018, and 2015–2019 five-year ACS estimates (Manson et al., 2024). The outcomes I study fall into three broad categories. First, population and household characteristics include changes in household type, presence of children, marital status, educational attainment, and the share of families receiving public assistance or living below the poverty line. Second, labor market outcomes capture the employment-to-population ratio, labor force participation and unemployment rates, median earnings, average usual hours worked, and the share of workers with a commute of less than 30 minutes. Third, housing characteristics encompass median monthly housing costs, gross rent, the median year residents moved in, the median year housing units were built, and average household size.[15] I use the 2006–2010 ACS to recreate census tract NMTC eligibility indicators and I generate covariates using the 2008–2012 ACS, including average characteristics of individuals (marital status, highest education level, median age, and race/ethnicity), households (household type), and housing units (number of bedrooms, number of units in the structure, and median rooms by tenure).

4.4.2.3 National Cancer Institute

I use census tract-level population data from the National Cancer Institute's (NCI) Surveillance, Epidemiology, and End Results (SEER) Program (NCI, 2024). These data include annual estimates of total population, as well as population counts disaggregated by race and by age group. The SEER population estimates are widely used for small-area demographic analysis be-

[15] I use five-year ACS estimates because one-year estimates are not released at the census tract level. Additionally, the five-year estimates offer larger sample sizes and more reliable measures. However, the main drawback of using five-year estimates is that direct year-to-year comparisons are not possible.

cause they provide consistent, annually updated tract-level data that align with Decennial Census population estimates.

4.4.2.4 Sample Creation and Summary Statistics

To construct the analytic sample, I merge NMTC investment data from the CDFI Fund, five-year census tract estimates from the ACS for both outcomes and controls, and population estimates from the NCI. This process yields one dataset for each ACS five-year period used in the analysis. I restrict the final sample to census tracts with non-missing information on treatment status, eligibility based on the median family income ratio, relevant covariates, and outcome variables. I also exclude census tracts that are a part of the high migration rural tracts list and those in GO zones, as their eligibility criteria for NMTC investment is different.

Table 4.2 presents summary statistics for census tracts near the 80 percent MFI eligibility threshold used in the RDD design (tracts with MFI ratios between 70 and 90 percent). This subset includes 16,284 tracts, of which 8,805 are eligible for NMTC investment and 7,479 are ineligible. On average, eligible tracts are more disadvantaged than ineligible ones: they have higher poverty and unemployment rates, lower MFI ratios, and lower levels of educational attainment. The housing stock in eligible tracts also tends to consist of smaller units and more multi-unit buildings, consistent with a greater presence of apartment housing. Among eligible tracts, 359 received NMTC investment, while 8,446 did not. Tracts receiving investment are somewhat more disadvantaged than those that did not, though the differences are modest. They exhibit higher poverty and unemployment rates and tend to have a higher share of minority residents.

4.4.2.5 Balance Checks

The regression discontinuity framework relies on the assumption that observations around the threshold are comparable and that those just barely ineligible make up a reasonable control group for those just barely eligible. To test this assumption, I run balance tests on several covariates to ensure that there are no jumps in other variables at the eligibility threshold. Here I estimate Equation 4.11 but with the pre-treatment covariate as the outcome. Figure 4.5 reports the results of the balance test on all covariates included in the main specification, standardized to mean 0 and standard deviation 1. There are no indications of differences in the covariates between those with an MFI ratio just above or below 80%, suggesting that the identifying assumption of the RDD design is likely to hold in this context.

4.5 Results

In this section, I present the results of the baseline RDD estimates of the impact of receiving NMTC investment. All regressions include controls for prior NMTC amounts received before the 2012 eligibility change, census tract baseline characteristics from 2012, and MSA fixed effects. For census tracts not located in MSAs, state fixed effects are used. Standard errors are clustered at the MSA level (or at the state level for tracts outside MSAs), and I report robust p-values as described in Calonico et al. (2014), adjusted for multiple hypothesis testing following Benjamini and Hochberg (1995). Prior to conducting my analysis, I verified, but do not report here, that I could replicate the main ACS-based findings from Freedman (2012, 2015) using his sample period and methods.

In Section 4.5.5 I explore treatment effect heterogeneity by census tract characteristics, including vacancy rates and per capita income. Additionally, I assess the robustness of my results by conducting several sensitivity analyses, reported in the Appendix. In Appendix 4.A.2 I report the results of using alternative measurements of NMTC investment. Appendix 4.A.3 presents alternative specifications of the RD, including variations in bandwidth, control functions, kernel, and covariates included. In general, the results remain consistent across these robustness checks, confirming that the main findings are not driven by modeling choices.

4.5.1 First Stage and Population Outcomes

I begin by presenting the first-stage results, which estimate the effect of crossing the NMTC eligibility threshold on the probability of receiving any NMTC investment.[16] The first rows of each panel in Tables 4.3 and 4.4 show that, for both the NCI and ACS samples, eligibility by crossing the MFI ratio threshold is associated with a 1.4 to 2 percentage point increase in the likelihood of receiving any NMTC investment between 2013 and the outcome year. These estimates are statistically significant at the 5% or 1% level, depending on the year.

The strength of the first stage varies by sample and year. In the NCI sample, the Cragg-Donald Wald F statistics are 9.21 (2017), 16.73 (2018), and 15.33 (2019), while in the ACS sample, the corresponding values are 6.19, 13.22, and 11.25. These values can be compared to the Stock-Yogo critical values for weak instruments in the case of one endogenous regressor and one instrument: 8.96 for a 15% maximal relative bias and 16.38 for a 10% maximal bias. Based on these benchmarks, the 2017 estimates in both samples may be subject to weak instrument concerns, while the 2018 and 2019 estimates in the NCI sample, and the 2018 estimate in the

[16] In Appendix 4.A.2, I report results using alternative treatment definitions, including the cumulative dollar amount (in 2019 dollars) of NMTC investment and the cumulative number of NMTC projects, rather than a binary indicator for any investment.

ACS sample, exceed the 15% threshold but not always the stricter 10% threshold. Given that some of the first-stage F statistics fall below the conventional Stock-Yogo critical values for weak instruments, especially in 2017, the corresponding second-stage estimates should be interpreted with caution. Kleibergen-Paap rk Wald F statistics, reported in the tables, are slightly higher in each case and suggest a similar pattern.

Turning to the second stage, I begin by examining changes in total population, a natural starting point for capturing the net effects of in- and out-migration, including both voluntary relocation and displacement. As discussed in Section 4.3, NMTC investment may influence these dynamics by improving local amenities or lowering business costs, thereby affecting residential and workplace location decisions. Table 4.3 reports estimates of the impact of NMTC investment on total population and a range of demographic characteristics. The first panel draws on population estimates from the NCI, which reflect annual population counts. The second panel uses data from the ACS, which provides average population characteristics over the five-year period ending in the outcome year.

The results show a statistically imprecise association between NMTC investment and total population, with point estimates ranging from an increase of 1,465 to 1,751 individuals across the three outcome years. Given the large standard errors, these estimates are not statistically distinguishable from zero. NMTC investment is also associated with statistically imprecise increases in the share of the non-white and Hispanic population and a decrease in the working-age population share (6.4 to 8.9 percentage points). The latter is statistically significant at the 10% level, though the limited precision cautions against drawing firm conclusions.

The ACS-based estimates suggest a possible increase in the share of married-couple households and households with children, though these effects are generally not statistically significant.

The largest estimated effect on households with children (14.6 percentage points) is statistically significant at the 1% level in 2017, but this pattern is not consistent across other years. In 2017, there is a statistically significant decline in the share of the population with only a high school degree (-16.8 percentage points), and in 2019, an increase in the share with a bachelor's degree (4.6 to 9.1 percentage points) significant at the 10% level. However, the inconsistency of results across years limits the strength of the interpretation. Other reported indicators of population characteristics are not statistically distinguishable from zero. Overall, the estimates are imprecise and offer little evidence of systematic changes in population composition associated with NMTC investment.

4.5.2 Labor Market Outcomes

Table 4.4 presents estimates of the effect of NMTC investment on labor and housing market outcomes. As outlined in the conceptual framework, subsidized capital from NMTC funding may lower business costs, potentially stimulating local employment or wage growth. Alternatively, if NMTC investment primarily enhances local amenities, labor market changes could reflect shifts in who lives and works in the area, rather than direct job creation. Because I do not track individuals over time, it is not possible to distinguish between changes in the behavior of original residents and changes driven by population turnover. To explore the potential for displacement or residential turnover, I also examine indicators of housing costs and market activity.

The labor market results suggest negative, but statistically insignificant, effects of NMTC investment. Across all post-investment years, point estimates for the employed-to-population ratio and labor force participation rate decline by sizable margins, ranging from 8.1 to 17.6 percentage points and 6.6 to 13.3 percentage points, respectively, while the unemployment rate increases by 4.2 to 16.9 percentage points. None of these estimates are statistically significant, so while the direction of the effects points toward worsening labor market conditions, one cannot conclude that NMTC investment is harmful to employment. These patterns do, however, contrast with those reported by Freedman (2012), who found positive effects of NMTC investment on employment. Differences in findings may reflect variation in program implementation, geographic or temporal focus, or sample composition.

Results for earnings and hours worked similarly yield an inconclusive picture. Male median earnings decline sharply in 2017 by $11,342 (in 2019 dollars), a statistically significant drop at the 10% level, but show no consistent pattern in other years. For women, average usual hours worked rise by 2 to 6.7 hours per week in 2017 and 2018, with statistically significant gains in those years, before the effect fades in 2019. Other labor market indicators are statistically indistinguishable from zero. Overall, the evidence provides limited support for meaningful labor market impacts from NMTC investment and raises questions about the program's effectiveness in improving employment outcomes, particularly in more recent years.

4.5.3 Housing Market Outcomes

The most consistent evidence of impact appears in the housing market, particularly by 2019, when NMTC investment is associated with a statistically significant increase in housing costs. For owner-occupied units, median monthly housing costs decline slightly in the first two years following investment (–$65 in 2017, significant at the 10% level; –$18 in 2018, significant at the 5% level), but rise by $122 in 2019 (10% level). A similar, though statistically insignificant, pattern emerges for median home values, which fall initially and then increase by 2019. Other indicators, such as the median year a household moved in and the median year the unit was built, show positive and statistically significant shifts in 2017 and 2018, but not in 2019. For renter-occupied units, NMTC investment is associated with increases in gross rent of \$263.9 to \$466.7 across the three post-investment years, statistically significant at the 10% level in 2018 and the 1% level in 2019. While these results do not reveal a uniformly consistent trajectory, they suggest that NMTC investment may have contributed to rising housing costs by 2019 for both owners and renters.

4.5.4 Interpretation and Implications

Overall, I find little evidence that NMTC investment leads to population or demographic changes in treated census tracts. Unlike Freedman (2012, 2015), I find no evidence of positive labor market effects. While some housing market outcomes are statistically significant, they do not reveal a consistent or clearly interpretable pattern. If anything, there may be suggestive, though inconclusive, evidence of increased housing costs. These findings underscore the challenges of estimating the effects of place-based policies and suggest that the impacts of NMTC investment may vary depending on time period, location, and project type.

4.5.5 Heterogeneous Effects

To investigate whether the effects of NMTC investment vary by community characteristics, I re-estimate the main specification on subgroups defined by two key measures from the 2006–2012 ACS: per capita income and housing vacancy rate. Per capita income serves as a proxy for local labor market quality and economic distress, as NMTC investment may have limited impact in lower-income areas facing entrenched challenges such as crime or weak labor markets. Housing vacancy rates indicate housing supply elasticity, with higher vacancy potentially allowing new residents to move in without displacing existing ones, whereas low vacancy rates may increase displacement pressure.

4.5.5.1 Housing Vacancy Rate

Columns 1 through 6 in Tables 4.5 and 4.6 present heterogeneity analyses based on housing vacancy rates, comparing tracts with below- and above-average vacancy (mean of 0.09). In low-vacancy areas, NMTC investment is generally not associated with statistically significant changes in population or demographic characteristics, except for fluctuations in the share of residents with at most a high school education, which vary in direction and significance across years. A statistically significant increase in owner-occupied household size appears in 2017 but is followed by insignificant effects in subsequent years. Gross rent rises substantially and significantly by approximately $838 in 2019.

In contrast, tracts with above-average vacancy rates show some increases in households with children in 2018 and 2019, as well as a rise in the share of married residents in 2017, followed by positive but statistically imprecise estimates in 2018 and 2019. Additionally, there are increases in owner housing costs and the median years that owner-occupied units were built and occupied in 2018 and 2019, alongside increases in the median year renter-occupied units were built in 2017 and 2018.

Given the limited statistical significance of both the first and second stages, these results should be interpreted cautiously. However, they tentatively suggest that higher vacancy areas may experience a more noticeable response in housing market outcomes, consistent with the idea that greater vacancy offers more flexibility for housing adjustments following NMTC investment.

4.5.5.2 Per Capita Income

Columns 7 through 12 in Tables 4.5 and 4.6 present heterogeneity analyses by per capita income, comparing census tracts with below- and above-average income levels (mean $27,588). In lower-income areas, NMTC investment is associated with some changes in demographic characteristics in 2017, including an increase in the share of married-couple families, a decrease in the share of residents with a high school diploma or less, and an increase in those holding a bachelor's degree. However, the direction and statistical significance of these effects vary across years. There is a negative and statistically significant reduction in the share of families receiving public assistance in 2017 and 2018, which remains negative but becomes statistically insignificant in 2019. Similarly, the share of families below the poverty line declines in 2019, but with positive and insignificant effects in earlier years.

Regarding labor market outcomes, positive and statistically significant increases in mean usual hours worked by women are observed consistently across all years, while other labor market indicators show no significant changes. In the housing market, monthly owner housing costs decline in 2017, but increase in 2018 and 2019. Median year of owner-occupied household move-in increases in all years, though it loses significance in 2019. Median year that owner-occupied housing units were built rises in 2017 and 2018 but falls in 2019. Gross rent increases significantly in 2017 and 2018, but not in 2019. Average renter household size grows in all years, achieving statistical significance only in 2019, alongside positive effects on the median year rental units were built, significant only in 2018.

In census tracts with above-average per capita income, fewer statistically significant effects are detected. Notable exceptions include increases in the share of households with children in 2017, the married share of the population in 2019, and a decline in the share with at most a high school degree in 2017. There are also positive effects on average owner-occupied household size in 2017 and on average gross rent in 2019. However, most other outcomes are statistically indistinguishable from zero, which aligns with weaker first-stage effects in these higher-income areas during 2017 and 2018.

Taken together, these patterns suggest that NMTC investment may have more observable effects in lower-income tracts, particularly in labor market engagement among women and certain housing market dynamics. This may reflect greater room for change or economic distress in these areas, although the evidence is not definitive. Nonetheless, given the variability in estimate precision and the differing strength of the first stage across income groups, these findings should be interpreted cautiously.

4.5.6 Robustness Checks

To assess the validity of the fuzzy regression discontinuity design, I conduct placebo tests by setting the eligibility threshold at the median MFI ratio below 80%, where no discontinuity in NMTC investment is expected. As anticipated, Appendix Tables A.4.3 and A.4.4 show that the first-stage coefficients at these placebo thresholds are all statistically insignificant, and the second-stage estimates generally lack significance as well. These results support the validity of the main RDD identification strategy.

Additionally, I perform sensitivity analyses to examine the robustness of the findings to alternative functional form choices and model specifications. Appendix Tables A.4.5 and A.4.6 report estimates varying the kernel type, polynomial order, and bandwidth size. The results remain broadly consistent, although some effect magnitudes show sensitivity to these choices. Further, Appendix Tables A.4.7 and A.4.8 explore variation in included covariates, such as census tract controls and MSA fixed effects. While certain estimates fluctuate depending on covariate inclusion, most are not sensitive to covariate inclusion and provide further confirmation that there is no strong evidence of effects of NMTC investment on the outcomes I study.

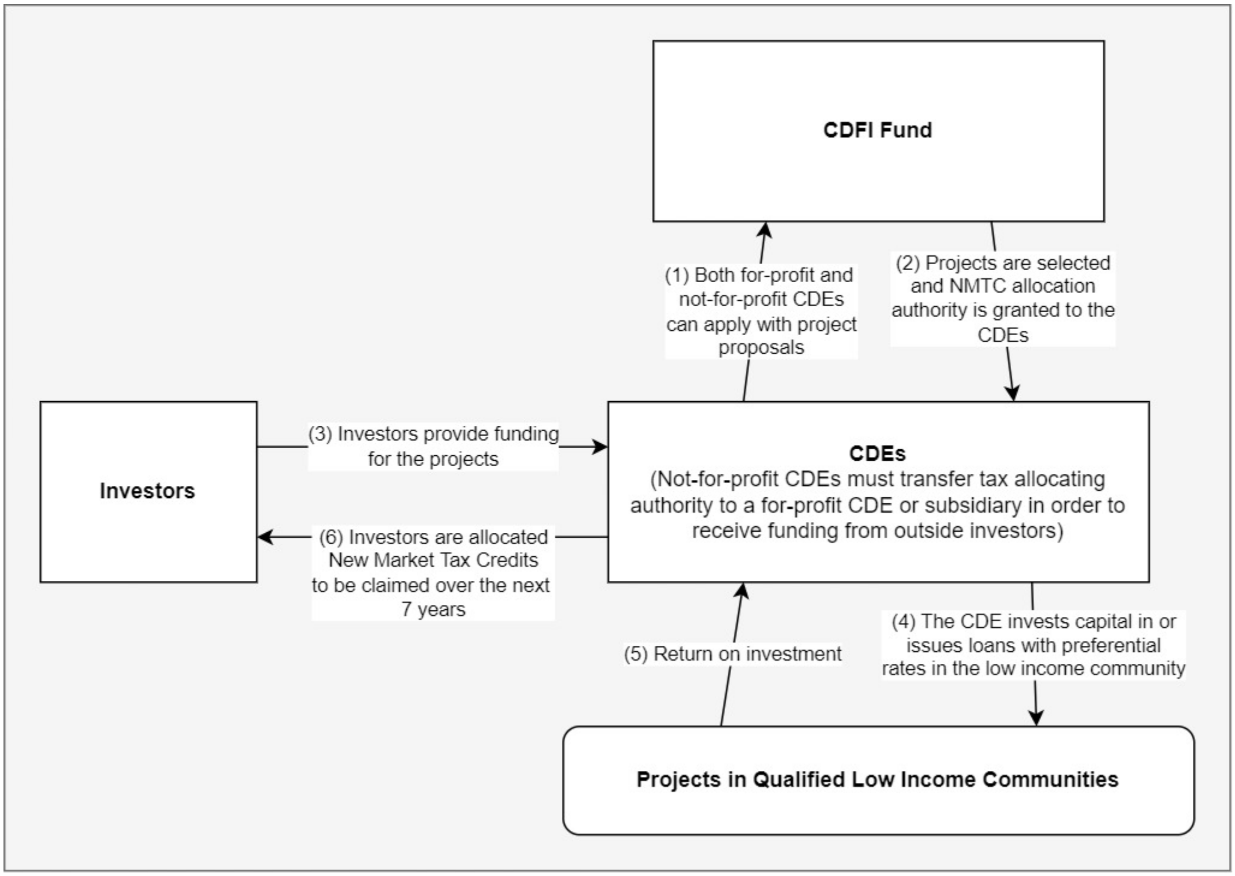
4.6 Conclusion

This paper provides updated evidence on the effects of the New Markets Tax Credit, a place-based policy that shares key features with more prominent programs such as Enterprise Zones and Opportunity Zones. Building on the work of Freedman (2012, 2015), I extend the analysis to cover the 2013–2019 period and broaden the range of outcomes to include detailed housing market indicators. This expanded scope allows for a closer examination of an often-overlooked aspect of place-based investment: the potential for gentrification and resident turnover, particularly as reflected in housing conditions and population shifts.

Overall, I find limited and mixed evidence on the effects of NMTC investment in treated census tracts. I observe little consistent impact on population size or demographic composition, and contrary to prior research, no clear positive effects on labor market outcomes. While some housing market indicators show statistically significant changes, such as increased rent, most patterns are not uniform across years or easily interpretable. Heterogeneity analyses suggest that impacts may be more pronounced in higher-vacancy and lower-income areas; however, variability in statistical significance and the strength of the first stage across groups, these findings should be viewed cautiously. Taken together, the results underscore the challenges of measuring the local effects of place-based investments like the NMTC, both because their influence may depend heavily on local context and conditions, and because such investments may, in some cases, have limited or no measurable impact.

4.7 Figures and Tables

Figure 4.1: NMTC Flow Chart



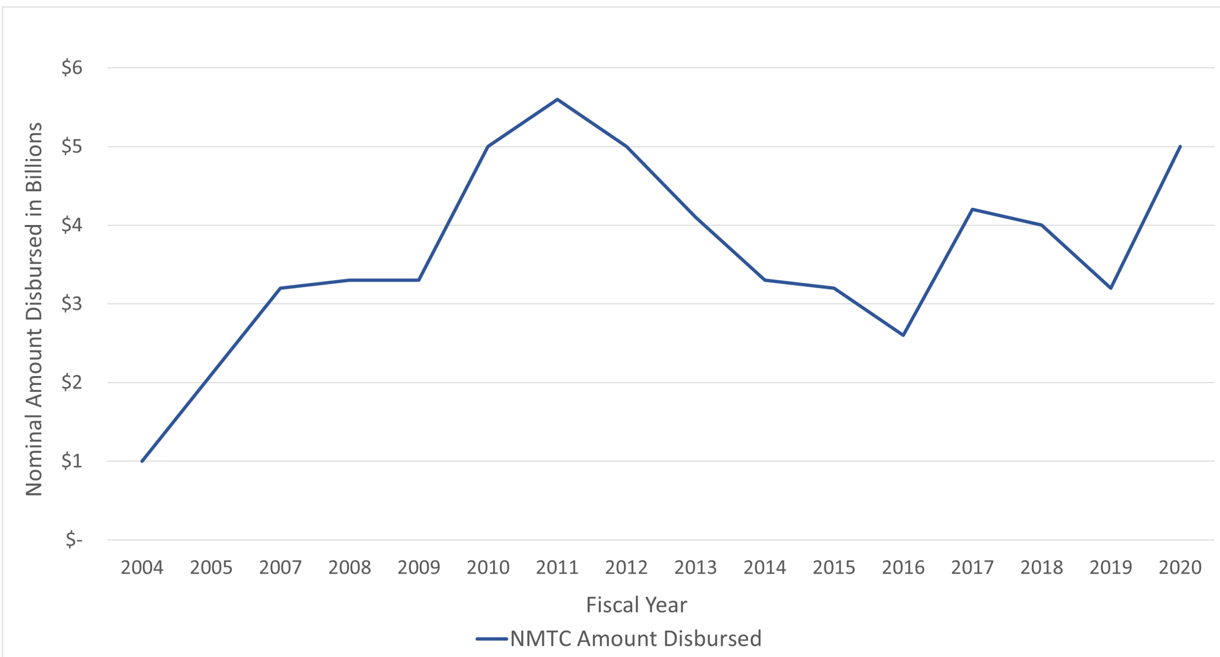
Notes: Author produced using data from CDFI documents.

Table 4.1: Details of US Tax-Based Place-Based Policies

Program	**Started**	**Recipient**	**Program Scale**	**Details**
Tax Increment Financing (State/Local Program)	1952	Local governments	Varies by location and year	A portion of property taxes of designated zones are diverted for a set period of time to finance a public infrastructure project.
Community Development Block Grant (Federal program)	1974	State and Local Governments	$3.47 Bil in FY22	Funding levels are based on community size and level of need. Funds are used for acquiring or rehabilitating real estate, constructing public facilities, providing public services, and business assistance.
Empowerment Zones, Enterprise Communities, & Renewal Communities (Federal program, with similar state programs)	1993, 1993, 2000	Businesses and Local Governments	$1.9 Bil in grants between 1993 and 2011	Targets high poverty and high emigration communities. Benefits include grants, employment credits for hiring community residents, low-cost loans, and tax benefits.
New Market Tax Credit (Federal program)	2000	Corporate and Individual Investors	$5 Bil in tax credits in FY22	Targets communities with high poverty, high emigration, or low family income. Tax credits are given for investment in selected projects. Projects include loans to businesses, commercial real estate investment, provision of public facilities, and financial counseling.
Opportunity Zones (Federal program)	2018	Corporate and Individual Investors	Estimated to cost $8.2 Bil between FY20 and FY24	census tracts were nominated by governors and certified by the Treasury. Provides capital gains tax benefits on Opportunity Funds that vary by time held. Investment project eligibility is broad and includes commercial and residential real estate, infrastructure, and business financing.

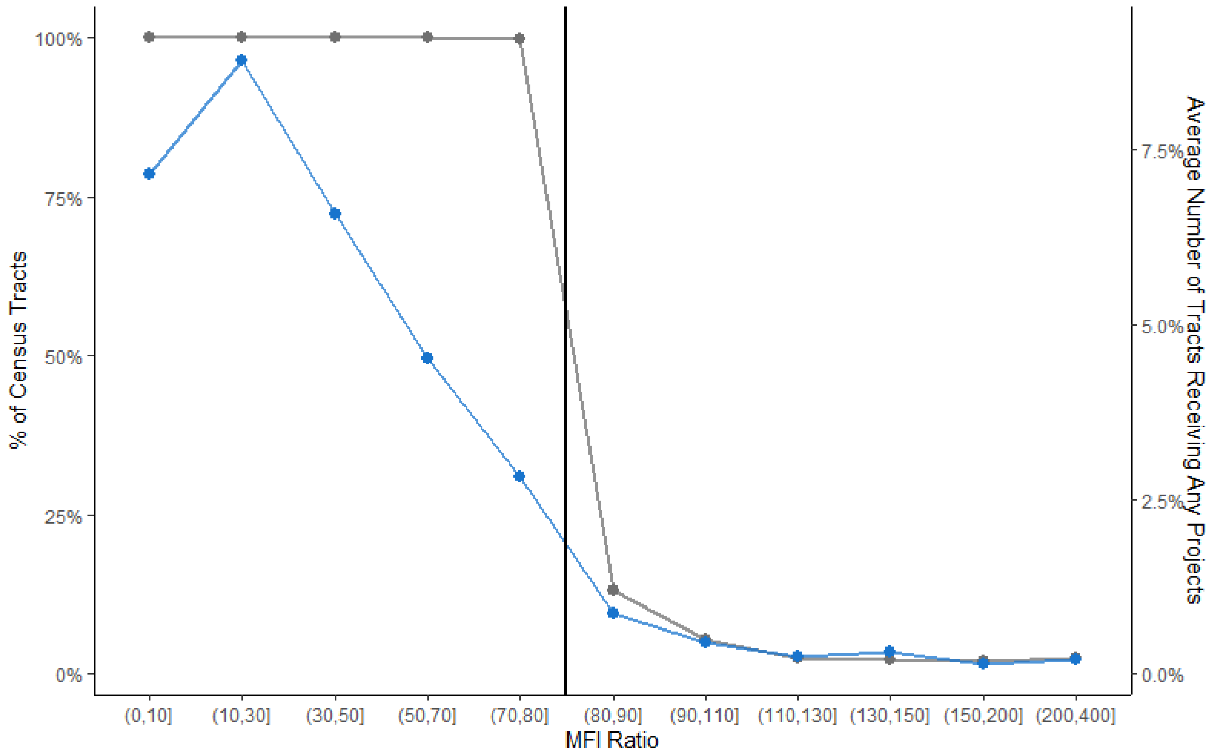
Sources: (CRS, 2011; Dye & Merriman, 2006; HUD, 2022; Minott et al., 2023; Theodos et al., 2023; TPC, 2020, 2021; Treasury, 2022)

Figure 4.2: Total Amount of NMTC Disbursed to QALICBs by FY



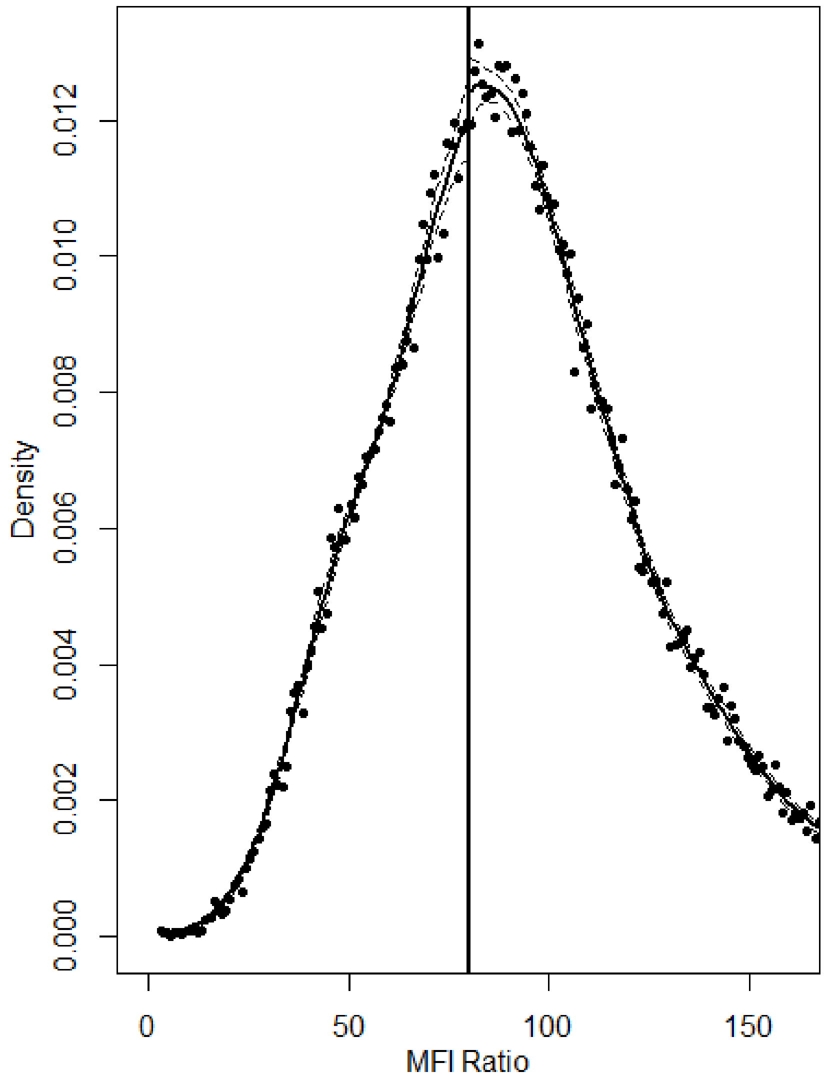
Notes: Author produced using data from the CDFI fund. Data unavailable for 2006.

Figure 4.3: Eligibility and % Ever Received NMTC Allocations - MFI Ratio



Notes: Author produced using data from the CDFI fund and the ACS. Includes census tracts that are not in GO Zones and not high migration rural counties. Ever received NMTC allocations is a flag that takes a value of 1 if a census tract receives any disbursements between 2013 and 2019.

Figure 4.4: McCrary Density Test: MFI Ratio



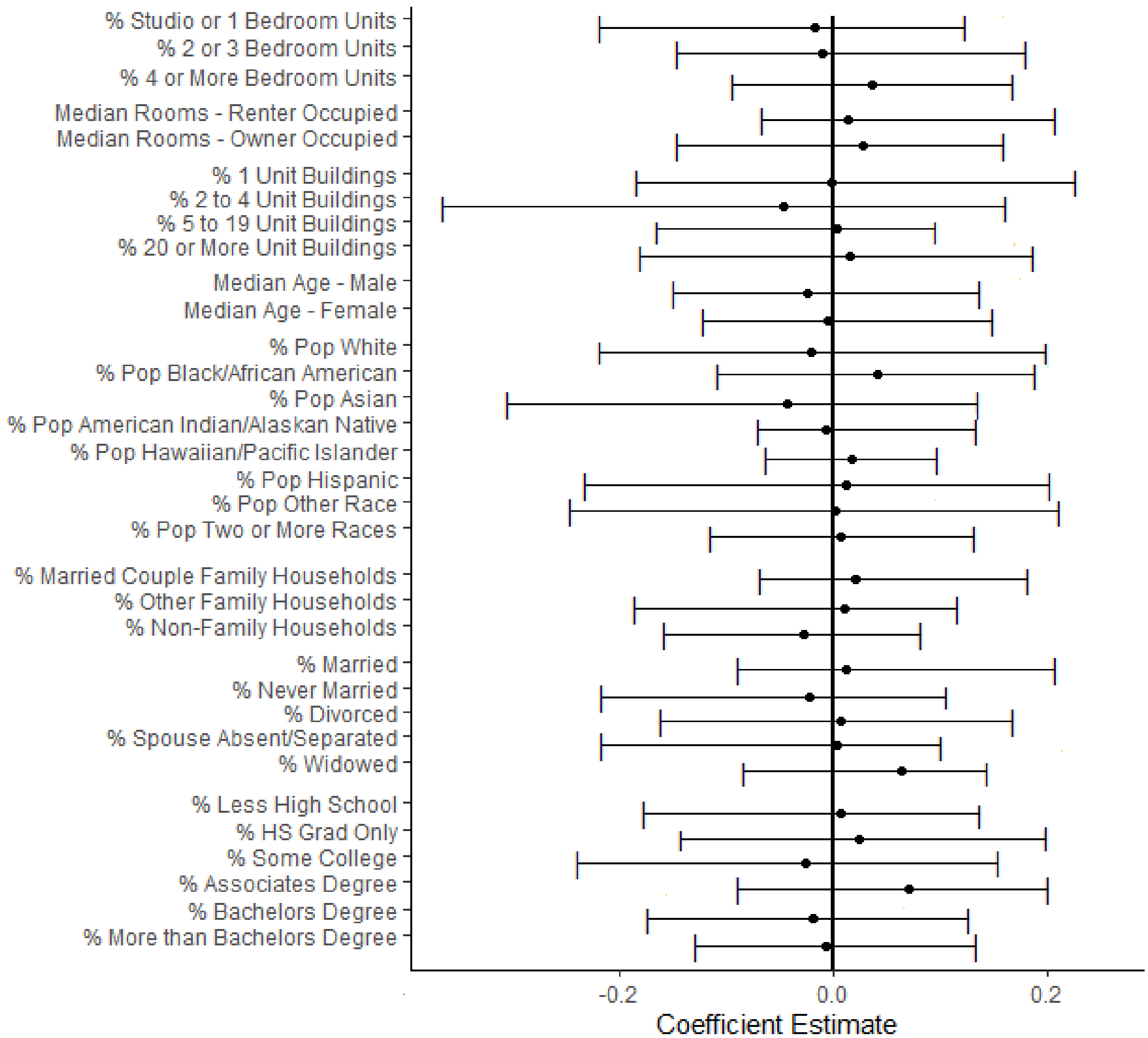
Notes: Author produced using data from the ACS. Cutoff line a 80%. Bin size = 1. Bandwidth = 10.

Table 4.2: Summary Statistics: MFI Ratio between 70% and 90%

	Full Population		NMTC Elig		NMTC Inelig		Elig-Received		Elic-Not Received	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Poverty Rate	14.66	(6.849)	17.38	(7.380)	11.47	(4.369)	20.84	(8.546)	17.23	(7.291)
MFI Ratio	80.33	(5.754)	76.27	(4.259)	85.11	(2.905)	76.65	(4.951)	76.25	(4.227)
Unemployment Rate	8.536	(4.064)	9.153	(4.279)	7.810	(3.666)	10.03	(4.449)	9.116	(4.268)
House Unit: Studio or 1-Bedroom	0.124	0.112	0.136	0.120	0.111	0.100	0.161	0.139	0.135	0.119)
House Unit: 2- or 3-Bedroom	0.722	0.111	0.722	0.115	0.722	0.107	0.705	0.127	0.723	0.115)
House Unit: 4 or more Bedroom	0.154	0.084	0.142	0.079	0.168	0.088	0.134	0.072	0.142	0.079)
Median Rooms - Renter Occupied	4.558	(0.703)	4.512	(0.667)	4.613	(0.739)	4.350	(0.616)	4.519	(0.668)
Median Rooms - Owner Occupied	6.006	(0.625)	5.940	(0.639)	6.085	(0.600)	5.935	(0.694)	5.940	(0.636)
House Unit: 1 Unit in Structure	0.678	(0.203)	0.652	(0.209)	0.708	(0.191)	0.619	(0.211)	0.653	(0.209)
House Unit: 2–4 Units in Structure	0.086	(0.112)	0.095	(0.120)	0.075	(0.100)	0.116	(0.120)	0.094	(0.120)
House Unit: 5–19 Units in Structure	0.078	(0.102)	0.085	(0.108)	0.070	(0.094)	0.092	(0.095)	0.084	(0.108)
House Unit: 20 or More Units in Structure	0.059	(0.116)	0.065	(0.125)	0.053	(0.105)	0.078	(0.143)	0.064	(0.124)
Median Age: Female	40.21	(7.274)	39.49	(7.489)	41.06	(6.916)	38.26	(6.860)	39.55	(7.511)
Median Age: Male	37.56	(7.108)	36.78	(7.250)	38.47	(6.824)	35.46	(6.306)	36.84	(7.283)
Pop Race: White	0.769	(0.229)	0.742	(0.239)	0.799	(0.213)	0.722	(0.247)	0.743	(0.239)
Pop Race: Black/African American	0.117	(0.191)	0.137	(0.207)	0.094	(0.168)	0.168	(0.229)	0.136	(0.205)
Pop Race: Asian	0.034	(0.078)	0.033	(0.076)	0.035	(0.080)	0.019	(0.040)	0.033	(0.077)
Pop Race: American Indian/Alaskan Native	0.010	0.044	0.012	0.054	0.008	0.027	0.024	0.099	0.011	0.052)
Pop Race: Native Hawaiian/Pacific Islander	0.001	0.010	0.001	0.009	0.002	0.011	0.001	0.008	0.001	0.009)
Pop Race: Hispanic	0.145	(0.198)	0.160	(0.214)	0.127	(0.176)	0.125	(0.185)	0.162	(0.215)
Pop Race: Other Race	0.043	0.076	0.049	0.083	0.036	0.066	0.039	0.075	0.049	0.084)
Pop Race: Two or More Races	0.026	0.028	0.026	0.027	0.025	0.029	0.026	0.026	0.026	0.027)
Household: Married Couple Family	0.470	(0.118)	0.447	(0.121)	0.497	(0.108)	0.406	(0.123)	0.449	(0.120)
Household: Other Family	0.185	(0.076)	0.197	(0.079)	0.171	(0.069)	0.200	(0.082)	0.197	(0.079)
Household: Non-Family	0.344	(0.116)	0.355	(0.123)	0.332	(0.105)	0.394	(0.143)	0.354	(0.122)
Marital Status: Married	0.456	(0.109)	0.433	(0.111)	0.482	(0.099)	0.394	(0.117)	0.435	(0.111)
Marital Status: Never Married	0.306	(0.101)	0.322	(0.109)	0.287	(0.086)	0.356	(0.126)	0.321	(0.108)
Marital Status: Divorced	0.120	(0.040)	0.123	(0.042)	0.118	(0.037)	0.129	(0.042)	0.122	(0.042)
Marital Status: Spouse Absent or Separated	0.048	0.029	0.052	0.030	0.044	0.026	0.052	0.027	0.052	0.030)
Marital Status: Widowed	0.069	0.031	0.070	0.032	0.069	0.030	0.069	0.029	0.070	0.032)
Education: Less HS	0.164	(0.085)	0.181	(0.090)	0.144	(0.074)	0.181	(0.091)	0.181	(0.090)
Education: HS Only	0.338	(0.092)	0.338	(0.093)	0.338	(0.091)	0.325	(0.087)	0.338	(0.093)
Education: Some College	0.221	0.058	0.218	0.059	0.226	0.057	0.218	0.054	0.218	0.059)
Education: Associates Degree	0.079	0.032	0.075	0.031	0.083	0.031	0.073	0.033	0.075	0.031)
Education: Bachelors Degree	0.131	(0.067)	0.124	(0.067)	0.139	(0.066)	0.130	(0.072)	0.124	(0.067)
Education: More than Bachelors	0.067	(0.052)	0.064	(0.054)	0.070	(0.050)	0.073	(0.061)	0.064	(0.053)
N	16,284		8,805		7,479		359		8,446	

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones and not high migration rural counties.

Figure 4.5: Balance Test: MFI Ratio



Notes: Estimates are calculated using a linear control function, a uniform kernel, BW = 10. Variables are standardized prior to estimation.

Table 4.3: Main Results: Census Tract Population Outcomes

	2017	2018	2019
NCI Data			
First Stage: MFI Eligibility			
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.014***	0.018***	0.019***
	(0.005)	(0.006)	(0.007)
Second Stage: NMTC Flag			
Total Population	1,751	1,465	1,586
	(1,469)	(972.4)	(1,265)
% Non-White (Including Hispanic)	0.184	0.145	0.144
	(0.173)	(0.149)	(0.122)
% White Non-Hispanic	-0.184	-0.145	-0.144
	(0.139)	(0.146)	(0.101)
% Aged 20–64	-0.089*	-0.068*	-0.064*
	(0.048)	(0.036)	(0.034)
N	16,866	16,866	16,866
Clusters	461	461	461
Cragg-Donald Wald F statistic	9.21	16.73	15.33
Kleibergen-Paap rk Wald F statistic	9.66	14.33	13.81
ACS Data			
First Stage: MFI Eligibility			
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.015**	0.02***	0.02***
	(0.006)	(0.008)	(0.007)
Second Stage: NMTC Flag			
% Households: Married Couple Family	0.166	0.021	0.092
	(0.131)	(0.016)	(0.081)
% Households: Other Family	-0.035	0.06	-0.036
	(0.024)	(0.057)	(0.039)
% Households: With Children	0.146***	0.055	0.047
	(0.05)	(0.047)	(0.04)
% Pop 15+: Married	0.17	0.071	0.069
	(0.185)	(0.055)	(0.055)
% Pop 25+: Highest Education: Less than a HS Degree	0.051	-0.006	-0.088
	(0.037)	(0.004)	(0.093)
% Pop 25+: Highest Education: HS Degree	-0.168***	-0.021	0.143
	(0.056)	(0.017)	(0.091)
% Pop 25+: Highest Education: Bachelors Degree	0.091	0.046	0.057*
	(0.104)	(0.034)	(0.033)
% Families: With Public Assistance	-0.039	0.057	-0.075
	(0.029)	(0.063)	(0.086)
% Families: Below the Poverty Line	-0.0637	0.0013	-0.0789
	(0.029)	(0.063)	(0.086)
N	13,811	12,830	12,621
Clusters	455	454	457
Cragg-Donald Wald F statistic	6.199	13.22	11.25
Kleibergen-Paap rk Wald F statistic	6.364	13.48	11.90
Stock-Yogo 10% Critical Value	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS/NCI Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table 4.4: Main Results: Census Tract Labor Market And Housing Outcomes

	2017	2018	2019
First Stage: MFI Eligibility			
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.015**	0.02***	0.02***
	(0.006)	(0.008)	(0.007)
Second Stage: NMTC Flag			
Pop 16+: Employment to Population Ratio	-0.176	-0.081	-0.134
	(0.16)	(0.067)	(0.138)
Pop 16+: Labor Force Participation Rate	-0.109	-0.066	-0.133
	(0.119)	(0.051)	(0.082)
Pop 16+: Unemployment Rate	0.169	0.065	0.042
	(0.161)	(0.068)	(0.034)
Workers 16+: Median Earnings: Male	-11,342*	4,591	6,711
	(5,967)	(3,120)	(6,586)
Workers 16+: Median Earnings: Female	-9,668	-8,863	-3,974
	(10,572)	(7,366)	(3,416)
Workers 16–64: Mean Usual Hours: Male	-0.766	0.05	0.743
	(0.515)	(0.053)	(0.507)
Workers 16–64: Mean Usual Hours: Female	6.71*	3.036**	1.949
	(3.939)	(1.215)	(1.974)
% Workers: Commute Less than 30 Minutes	-0.347	-0.302	-0.316
	(0.281)	(0.247)	(0.256)
Median Monthly Housing Costs: Owner Occupied	-65.31**	-18.45*	121.7**
	(27.55)	(10.39)	(61.59)
Median Unit Value: Owner Occupied	-67,376	-25,122	33,169
	(42,343)	(24,439)	(35,745)
Average Household Size: Owner Occupied	0.381***	0.358*	0.603
	(0.139)	(0.208)	(0.698)
Median Year Household Moved In: Owner Occupied	8.599**	10.57***	8.121
	(4.276)	(3.709)	(8.354)
Median Year Housing Unit Built: Owner Occupied	25.36**	3.686**	-37.53**
	(12.78)	(1.67)	(14.76)
Median Gross Rent: Renter Occupied	263.9	289.9*	466.6***
	(263.9)	(149.2)	(161.7)
Average Household Size: Renter Occupied	2.679*	1.624	0.784
	(1.613)	(1.196)	(0.622)
Median Year Household Moved In: Renter Occupied	0.27	-2.758	-1.855
	(0.31)	(2.833)	(1.659)
Median Year Housing Unit Built: Renter Occupied	-2.056	29.23**	-3.208
	(1.669)	(11.53)	(2.127)
N	13,811	12,830	12,621
Clusters	455	454	457
Cragg-Donald Wald F statistic	6.199	13.22	11.25
Kleibergen-Paap rk Wald F statistic	6.364	13.48	11.90
Stock-Yogo 10% Critical Value	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table 4.5: Heterogeneous Effects: Census Tract Population Outcomes

	Vacancy Rate < Mean			Vacancy Rate > Mean			Per Cap Income < Mean			Per Cap Income > Mean		
	2017	2018	2019	2017	2018	2019	2017	2018	2019	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
NCI Data												
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.021**	0.021*	0.024**	0.008	0.015***	0.014**	0.015**	0.02***	0.021***	0.015*	0.018**	0.014*
	(0.009)	(0.012)	(0.012)	(0.008)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)
Second Stage: NMTC Flag												
Total Population	-56.74	88.19	230.7	8,069	6,569.2	8,498.1	3,780.3	3,912.3	3,656.2	3,574.9	2,033.7	2,365
	(62.48)	(57.70)	(171.1)	(5,318)	(4,449)	(7,947)	(3,613)	(4,204)	(3,424)	(2,863)	(2,226)	(2,693)
% Non-White (Including Hispanic)	0.13	0.1089	0.102	0.185	0.141	0.18	0.071	0.0572	0.042	0.374	0.216	0.242
	(0.135)	(0.072)	(0.101)	(0.121)	(0.142)	(0.124)	(0.081)	(0.057)	(0.034)	(0.439)	(0.146)	(0.277)
% White Non-Hispanic	-0.13	-0.109	-0.102	-0.185	-0.141	-0.18	-0.071	-0.057	-0.042	-0.374	-0.216	-0.242
	(0.118)	(0.108)	(0.115)	(0.124)	(0.092)	(0.143)	(0.052)	(0.036)	(0.027)	(0.358)	(0.169)	(0.223)
% Aged 20–64	0	0	0	-0.107	-0.085	-0.101	0.044	0.046	0.046	-0.389	-0.207	-0.223
	(0)	(0)	(0)	(0.095)	(0.094)	(0.113)	(0.044)	(0.053)	(0.035)	(0.379)	(0.22)	(0.26)
N	6,878	6,878	6,878	9,988	9,988	9,988	11,725	11,725	11,725	5,141	5,141	5,141
Clusters	408	408	408	408	408	408	429	429	429	347	347	347
Cragg-Donald Wald F statistic	5.44	3.06	4.01	1.02	6.25	4.01	6.25	8.16	9.03	2.78	5.06	3.06
Kleibergen-Paap rk Wald F statistic	5.94	3.56	4.50	1.50	6.75	4.50	6.76	8.66	9.51	3.28	5.56	
ACS Data												
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.021	0.021*	0.013	0.014	0.023***	0.023***	0.01	0.017**	0.015**	0.036	0.024	0.028**
	(0.018)	(0.012)	(0.01)	(0.013)	(0.009)	(0.008)	(0.012)	(0.008)	(0.007)	(0.023)	(0.025)	(0.014)
Second Stage: NMTC Flag												
% Households: Married Couple Family	0.034	-0.027	0.161	-0.644	0.071	0.015	0.177**	-0.121	0.198	0.118	0.054	0.079
	(0.025)	(0.02)	(0.158)	(0.431)	(0.065)	(0.015)	(0.09)	(0.081)	(0.131)	(0.129)	(0.037)	(0.066)
% Households: Other Family	-0.039	0.066	-0.175	-0.075	0.064	0.066	-0.055	0.27	0.168	-0.046	-0.01	-0.082
	(0.046)	(0.058)	(0.137)	(0.082)	(0.073)	(0.071)	(0.039)	(0.312)	(0.182)	(0.038)	(0.007)	(0.05)
% Households: With Children	-0.169	-0.057	-0.134	-0.483	0.201**	0.157*	0.083	0.11	0.337	0.144*	0.049	-0.009
	(0.136)	(0.048)	(0.158)	(0.505)	(0.094)	(0.091)	(0.063)	(0.086)	(0.396)	(0.081)	(0.044)	(0.01)
% Pop 15+: Married	0.11	0.065	0.071	0.256**	0.076	0.051	0.129	-0.01	0.06	0.137	0.089	0.079*
	(0.103)	(0.066)	(0.062)	(0.119)	(0.057)	(0.034)	(0.095)	(0.007)	(0.068)	(0.148)	(0.074)	(0.047)
% Pop 25+: Highest Education: Less than a HS Degree	0.015	-0.022	-0.09	-0.606	-0.017	-0.124*	0.006	-0.019	0.011	0.065	-0.005	-0.107
	(0.017)	(0.015)	(0.061)	(0.587)	(0.014)	(0.063)	(0.006)	(0.015)	(0.008)	(0.054)	(0.004)	(0.124)
% Pop 25+: Highest Education: HS Degree	-0.187**	0.019*	0.185	0.352	-0.056	0.118	-0.146**	0.044	0.227	-0.166*	-0.039	0.127
	(0.073)	(0.011)	(0.168)	(0.387)	(0.059)	(0.089)	(0.073)	(0.046)	(0.18)	(0.1)	(0.036)	(0.135)
% Pop 25+: Highest Education: Bachelors Degree	0.009	0.016	0.057	-0.274	0.076	0.061	0.077*	-0.095	-0.013	0.081	0.067	0.057
	(0.008)	(0.014)	(0.039)	(0.232)	(0.055)	(0.051)	(0.046)	(0.08)	(0.015)	(0.071)	(0.057)	(0.052)
% Families: With Public Assistance	-0.215	0.062	-0.048	-1.015	0.048	-0.056	-0.086*	-0.261**	-0.449	-0.036	0.164	0.015
	(0.188)	(0.049)	(0.054)	(0.687)	(0.03)	(0.042)	(0.046)	(0.114)	(0.372)	(0.029)	(0.178)	(0.016)
% Families: Below the Poverty Line	-0.1693	-0.0806	-0.1357	2.0984	0.0993	-0.0197	0.0144	0.0048	-0.2299**	-0.1274	0.0168	-0.0306
	(0.177)	(0.05)	(0.091)	(1.685)	(0.074)	(0.016)	(0.013)	(0.003)	(0.108)	(0.086)	(0.012)	(0.032)
N	8,070	7,527	7,378	5,741	5,303	5,243	4,068	3,664	3,626	9,743	9,166	8,995
Clusters	411	401	406	393	395	393	317	328	327	421	414	413
Cragg-Donald Wald F statistic	1.36	3.06	1.69	1.16	6.53	8.27	0.69	4.52	4.59	2.45	0.92	4.01
Kleibergen-Paap rk Wald F statistic	1.39	3.12	1.72	1.18	6.66	8.43	0.71	4.61	4.68	2.50	0.94	4.08
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS/NCI Data. For each column, the regression is only run on the subset of census tracts with a vacancy rate $<$ or $>$ the mean value 0.09; or with a per capita income $<$ or $>$ the mean value 27,588. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table 4.6: Heterogeneous Effects: Census Tract Labor Market and Housing Outcomes

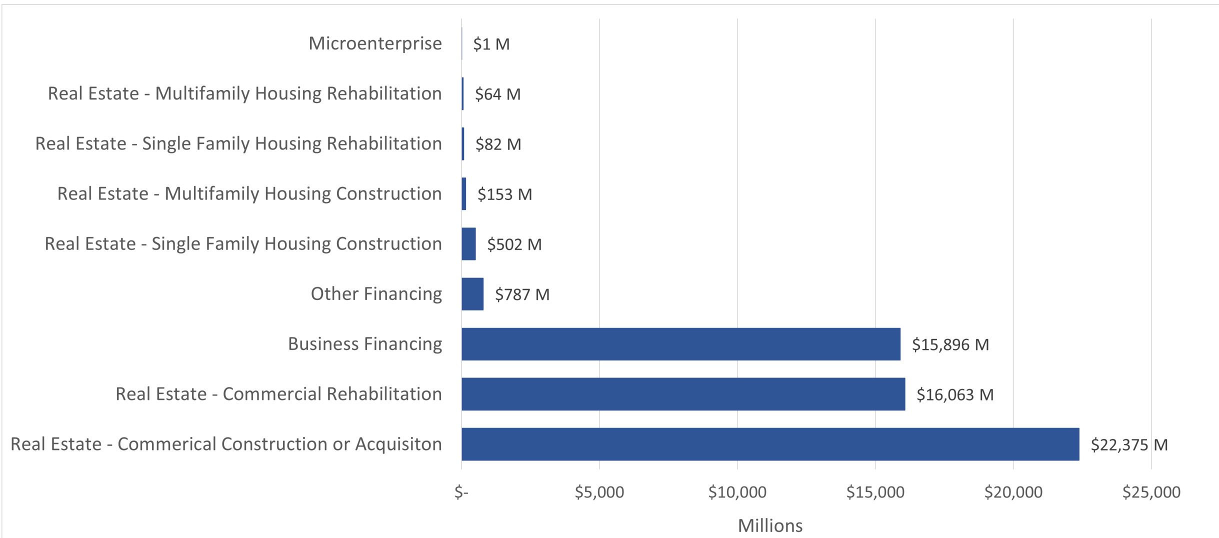
	Vacancy Rate < Mean			Vacancy Rate > Mean			Per Cap Income < Mean			Per Cap Income > Mean		
	2017	2018	2019	2017	2018	2019	2017	2018	2019	2017	2018	2019
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.021	0.021*	0.013	0.014	0.023***	0.023***	0.01	0.017**	0.015**	0.036	0.024	0.028**
	(0.018)	(0.012)	(0.01)	(0.013)	(0.009)	(0.008)	(0.012)	(0.008)	(0.007)	(0.023)	(0.025)	(0.014)
Second Stage: NMTC Flag												
Pop 16+: Employment to Population Ratio	-0.587	-0.161	-0.235	-1.283	0.003	-0.073	-0.129	-0.286	-0.48	-0.13	0.011	-0.03
	(0.433)	(0.13)	(0.21)	(1.39)	(0.003)	(0.078)	(0.088)	(0.185)	(0.392)	(0.146)	(0.01)	(0.032)
Pop 16+: Labor Force Participation Rate	-0.602	-0.223	-0.252	1.344	0.107*	-0.041	-0.092	-0.221	-0.48	-0.076	0.014	-0.028
	(0.533)	(0.176)	(0.173)	(1.053)	(0.062)	(0.042)	(0.062)	(0.154)	(0.537)	(0.057)	(0.012)	(0.027)
Pop 16+: Unemployment Rate	0.182	0.001	0.036	1.488	0.155	0.062	0.095	0.183	0.162	0.167	0.025	0.008
	(0.203)	(0.001)	(0.023)	(1.35)	(0.183)	(0.067)	(0.087)	(0.217)	(0.114)	(0.121)	(0.025)	(0.006)
Workers 16+: Median Earnings: Male	-7,183	8,488	2,077	402,012	2,749	10,679	2,097	21,136	8,679	-4,681	2,250	8,191
	(8,428)	(8,914)	(1,991)	(446,414)	(1,727)	(11,154)	(2,240)	(24,517)	(9,958)	(3,438)	(2,335)	(6,493)
Workers 16+: Median Earnings: Female	-18,361	-13,269	-9,004	-25,735	-5,316	1,639	-8,175	4,545	-7,085	1,080	-6,740	-1,676
	(12,514)	(13,599)	(10,555)	(16,959)	(5,471)	(1,569)	(9,538)	(3,341)	(6,208)	(825.9)	(5,633)	(1,197)
Workers 16–64: Mean Usual Hours: Male	11.27	3.585	7.976	76.84	-4.498	-4.564	2.781	0.611	1.584	0.295	-0.583	0.765
	(9.27)	(4.099)	(6.954)	(71.02)	(2.945)	(5.417)	(2.056)	(0.421)	(1.326)	(0.238)	(0.432)	(0.529)
Workers 16–64: Mean Usual Hours: Female	11.67	5.81**	2.088	-59.47	0.773	1.398	15.34***	13.28**	14.03**	3.8	0.558	-0.583
	(10.58)	(2.318)	(1.289)	(48.61)	(0.874)	(1.099)	(5.842)	(5.589)	(5.577)	(2.862)	(0.457)	(0.578)
% Workers: Commute Less than 30 Minutes	-0.863	-0.49	-0.411	-2.362	-0.084	-0.293	0.06	-0.207	-0.63	-0.381	-0.207	-0.111
	(0.654)	(0.497)	(0.295)	(1.803)	(0.078)	(0.317)	(0.04)	(0.16)	(0.661)	(0.336)	(0.185)	(0.085)
Median Monthly Housing Costs: Owner Occupied	-487.6	-352.9	-89.08	4,520	370.8*	275.6*	-38.93***	278.9**	448.7***	11.07	-54.29	63.04
	(444.4)	(392.1)	(86.73)	(2,844)	(222.7)	(141.8)	(13.93)	(124.1)	(162.5)	(13.03)	(62.46)	(70.51)
Median Unit Value: Owner Occupied	-200,387	-210,612	-169,843	96,872	119,546	120,138	-189,467	-86,391	110,284	39,172	37,459	58,736
	(147,224)	(186,892)	(125,588)	(88,305)	(77,155)	(85,352)	(127,054)	(76,826)	(107,539)	(32,602)	(32,064)	(63,697)
Average Household Size: Owner Occupied	0.427***	0.153	0.507	7.179	0.571	0.349	-0.586	0.211	1.082	0.491**	0.197	0.28
	(0.146)	(0.11)	(0.384)	(4.57)	(0.525)	(0.388)	(0.375)	(0.175)	(1.213)	(0.225)	(0.145)	(0.293)
Median Year Household Moved In: Owner Occupied	-4.51	0.041	0.3	78.27	20.66***	14.47*	19.69***	11.67**	15.47	1.144	8.754	3.209
	(5.302)	(0.028)	(0.194)	(75.56)	(7.43)	(7.799)	(7.612)	(5.521)	(18.11)	(1.259)	(9.616)	(2.956)
Median Year Housing Unit Built: Owner Occupied	-36.95	-6.87	-96.62	275.1	9.185**	3.459***	59.96***	12.49**	-153.9**	4.221	6.972	-2.97
	(22.71)	(5.732)	(60.85)	(292.6)	(4.32)	(1.173)	(20.52)	(6.273)	(73.54)	(2.695)	(5.399)	(3.335)
Median Gross Rent: Renter Occupied	831.6	433.1	838.1**	3,226	247.2	207.6	402.2**	1,031*	703.8	198.9	19.86	379.7**
	(528.8)	(340.9)	(391.8)	(2,883)	(151.0)	(151.4)	(188.2)	(607.1)	(494.2)	(158.8)	(13.23)	(154.3)
Average Household Size: Renter Occupied	5.066	1.736	1.426	3.029	1.668	0.214	1.601	2.56	1.993*	2.695	0.945	0.337
	(4.803)	(1.783)	(1.463)	(3.296)	(1.087)	(0.213)	(1.139)	(2.824)	(1.155)	(2.262)	(1.072)	(0.233)
Median Year Household Moved In: Renter Occupied	-0.222	-2.026	0.778	-6.33	-2.561	-2.979	-3.297	-1.695	-8.535	2.585	-2.564	-0.008
	(0.221)	(1.236)	(0.711)	(5.27)	(2.538)	(2.268)	(2.774)	(1.104)	(8.541)	(2.574)	(1.897)	(0.01)
Median Year Housing Unit Built: Renter Occupied	-67.55	9.208	64.34	78.34**	45.03**	-51.46	34.37	76.93**	132.1	-22.01	10.54	-76.71
	(44.65)	(10.32)	(49.47)	(32.20)	(19.68)	(33.01)	(25.99)	(36.40)	(146.4)	(13.93)	(8.477)	(79.20)
N	8,070	7,527	7,378	5,741	5,303	5,243	4,068	3,664	3,626	9,743	9,166	8,995
Clusters	411	401	406	393	395	393	317	328	327	421	414	413
Cragg-Donald Wald F statistic	1.36	3.06	1.69	1.16	6.53	8.27	0.69	4.52	4.59	2.45	0.92	4.01
Kleibergen-Paap rk Wald F statistic	1.39	3.12	1.72	1.18	6.66	8.43	0.71	4.61	4.68	2.50	0.94	4.08
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. For each column, the regression is only run on the subset of census tracts with a vacancy rate < or > the mean value 0.09; or with a per capita income < or > the mean value 27,588. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

4.A Appendix

4.A.1 Additional NMTC Details

Figure A.4.1: Cumulative Amount of NMTC Disbursed to QALICBs by Project Type (FY 2004–2019)



Notes: Author produced using data from the CDFI Fund. Data unavailable for 2006.

4.A.2 Alternative Measures of NMTC Treatment

Tables A.4.1 and A.4.2 report results when using the cumulative dollar amount (in 2019 dollars) of NMTC investment and the cumulative number of NMTC projects instead of the NMTC project flag as the treatment variable. Coefficients can be interpreted as the effect of a $1 Million increase in NMTC on the outcome or the effect of an additional NMTC project on the outcome respectively. In general, the results are of the same sign but with smaller magnitudes and less statistical precision, as expected.

Table A.4.1: Alternative Measures: Census Tract Population Outcomes

	NMTC Amount (In Millions)			Number of Projects		
	2017	2018	2019	2017	2018	2019
				NCI Data		
First Stage: MFI Eligibility						
NMTC Amount or Projects	0.132	0.227	0.241	0.018*	0.024**	0.025**
	(0.152)	(0.2)	(0.283)	(0.009)	(0.011)	(0.01)
Second Stage: NMTC Amount or Projects						
Total Population	177.1	113.1	117.3	1,371	1,083	1,177
	(176.1)	(96.31)	(92.08)	(1,231)	(932.2)	(927.5)
% Non-White (Including Hispanic)	0.020	0.0117	0.011	0.146	0.1087	0.109
	(0.019)	(0.012)	(0.012)	(0.148)	(0.097)	(0.128)
% White Non-Hispanic	-0.02	-0.012	-0.011	-0.146	-0.109	-0.109
	(0.017)	(0.011)	(0.011)	(0.110)	(0.084)	(0.077)
% Aged 20–64	-0.010*	-0.006*	-0.005	-0.070*	-0.050*	-0.048*
	(0.006)	(0.003)	(0.006)	(0.037)	(0.030)	(0.026)
N	16,866	16,866	16,866	16,866	16,866	16,866
Clusters	461	461	461	461	461	461
Cragg-Donald Wald F statistic	0.75	1.29	0.73	4.02	4.76	6.25
Kleibergen-Paap rk Wald F statistic	0.77	1.31	0.74	4.08	4.86	6.38
				ACS Data		
First Stage: MFI Eligibility						
NMTC Amount or Projects	0.161	0.288	0.253	0.020*	0.028**	0.026**
	(0.109)	(0.332)	(0.182)	(0.011)	(0.014)	(0.012)
Second Stage: NMTC Amount or Projects						
% Households: Married Couple Family	0.016	0.002	0.007	0.124	0.017	0.07
	(0.018)	(0.002)	(0.008)	(0.132)	(0.012)	(0.08)
% Households: Other Family	-0.003	0.004	-0.003	-0.025	0.044	-0.027
	(0.003)	(0.004)	(0.003)	(0.026)	(0.044)	(0.021)
% Households: With Children	0.014**	0.004	0.004	0.108***	0.039	0.034
	(0.007)	(0.004)	(0.004)	(0.041)	(0.044)	(0.036)
% Pop 15 and Over: Married	0.016	0.005	0.006	0.128	0.053	0.053
	(0.016)	(0.003)	(0.007)	(0.128)	(0.046)	(0.058)
% Pop 25+: Highest Education: Less than a HS Degree	0.005	0	-0.007	0.039	-0.004	-0.066
	(0.005)	(0)	(0.007)	(0.045)	(0.003)	(0.051)
% Pop 25+: Highest Education: HS Degree	-0.015**	-0.001	0.011	-0.127***	-0.016	0.106
	(0.007)	(0.001)	(0.01)	(0.045)	(0.014)	(0.073)
% Pop 25+: Highest Education: Bachelors Degree	0.008	0.003	0.004	0.067	0.034	0.043*
	(0.005)	(0.003)	(0.003)	(0.053)	(0.025)	(0.023)
% Families: With Public Assistance	-0.004	0.004	-0.006	-0.027	0.041	-0.056
	(0.012)	(0.007)	(0.008)	(0.093)	(0.071)	(0.078)
% Families: Below the Poverty Line	-0.006	0.0001	-0.0061	-0.0473	0.0006	-0.0599
	(0.01)	(0.006)	(0.007)	(0.082)	(0.061)	(0.062)
N	13,811	12,830	12,621	13,811	12,830	12,621
Clusters	455	454	457	455	454	457
Cragg-Donald Wald F statistic	2.18	0.75	1.93	3.31	4.02	4.69
Kleibergen-Paap rk Wald F statistic	2.23	0.77	1.97	3.37	4.08	4.79
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS/NCI Data. Models estimated using local linear regression. Prior NMTC investment controls include either the cumulative amount of NMTC investment in millions or the number of NMTC projects between 2000 and 2009 and between 2009 and 2012 separately. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table A.4.2: Alternative Measures: Census Tract Labor Market and Housing Outcomes

	NMTC Amount (In Millions)			Number of Projects		
	2017	2018	2019	2017	2018	2019
First Stage: MFI Eligibility						
NMTC Amount or Projects	0.161	0.288	0.253	0.02*	0.028**	0.026**
	(0.109)	(0.332)	(0.182)	(0.011)	(0.014)	(0.012)
Second Stage: NMTC Amount or Projects						
Pop 16+: Employment to Population Ratio	-0.016	-0.006	-0.011	-0.131	-0.059	-0.1
	(0.012)	(0.006)	(0.008)	(0.089)	(0.066)	(0.072)
Pop 16+: Labor Force Participation Rate	-0.01	-0.005	-0.01	-0.081	-0.048	-0.098
	(0.011)	(0.006)	(0.008)	(0.086)	(0.066)	(0.07)
Pop 16+: Unemployment Rate	0.015	0.005	0.003	0.127	0.048	0.032
	(0.009)	(0.004)	(0.004)	(0.061)	(0.038)	(0.039)
Workers 16+: Median Earnings: Male	-1,021	351.7	519.4	-8,420*	3,306	4,908
	(1,312)	(646.8)	(813.1)	(10,136)	(6,558)	(7,699)
Workers 16+: Median Earnings: Female	-898.5	-615.1	-308.2	-7,133	-6,649	-3,088
	(1,146)	(620.7)	(713.1)	(8,865)	(6,268)	(6,881)
Workers 16–64: Mean Usual Hours: Male	-0.063	0.01	0.065	-0.575	0.011	0.53
	(0.391)	(0.222)	(0.281)	(3.138)	(2.301)	(2.713)
Workers 16–64: Mean Usual Hours: Female	0.621	0.213**	0.143	5.086*	2.221**	1.467
	(0.418)	(0.207)	(0.227)	(2.967)	(2.069)	(2.171)
% Workers: Commute Less than 30 Minutes	-0.032	-0.021	-0.025	-0.265	-0.224	-0.24
	(0.026)	(0.015)	(0.018)	(0.197)	(0.153)	(0.169)
Median Monthly Housing Costs: Owner Occupied	-5.349**	-0.955*	9.429**	-50.87*	-14.34*	89.74**
	(25.99)	(15.07)	(19.35)	(209.1)	(155.9)	(183.5)
Median Unit Value: Owner Occupied	-6157.4	-1594.3	2779.4	-49878	-19713	24851
	(9,009)	(5,808)	(7,525)	(70,121)	(60,107)	(71,612)
Average Household Size: Owner Occupied	0.036***	0.025	0.047	0.283***	0.264*	0.452
	(0.057)	(0.033)	(0.037)	(0.451)	(0.344)	(0.353)
Median Year Household Moved In: Owner Occupied	0.785	0.745**	0.615	6.417**	7.74***	6.031
	(0.705)	(0.438)	(0.459)	(5.248)	(4.143)	(4.215)
Median Year Housing Unit Built: Owner Occupied	2.278**	0.195**	-2.982**	19.41**	3.029**	-27.75**
	(2.041)	(1.532)	(3.987)	(16.28)	(15.92)	(36.63)
Median Gross Rent: Renter Occupied	25.41	20.88	36.33**	196.5	212.8*	348.2**
	(31.91)	(19.74)	(25.84)	(244.9)	(193.9)	(228.1)
Average Household Size: Renter Occupied	0.248	0.116	0.062	1.999*	1.188	0.582
	(0.139)	(0.061)	(0.057)	(0.824)	(0.535)	(0.516)
Median Year Household Moved In: Renter Occupied	0.027	-0.195	-0.145	0.165	-2.056	-1.366
	(0.241)	(0.152)	(0.211)	(1.935)	(1.537)	(1.985)
Median Year Housing Unit Built: Renter Occupied	-0.212	2.036*	-0.29	-1.316	21.59**	-2.327
	(1.932)	(1.259)	(7.056)	(15.50)	(12.82)	(67.61)
N	13,811	12,830	12,621	13,811	12,830	12,621
Clusters	455	454	457	455	454	457
Cragg-Donald Wald F statistic	2.18	0.75	1.93	3.31	4.02	4.69
Kleibergen-Paap rk Wald F statistic	2.23	0.77	1.97	3.37	4.08	4.79
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. Models estimated using local linear regression. Prior NMTC investment controls include either the cumulative amount of NMTC investment in millions or the number of NMTC projects between 2000 and 2009 and between 2009 and 2012 separately. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

4.A.3 Robustness Checks

4.A.3.1 Placebo Tests

Table A.4.3: Placebo Tests: Census Tract Population Outcomes

	2017 (1)	2018 (2)	2019 (3)
		NCI Data	
First Stage: MFI Eligibility			
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.006	0.005	0.005
	(0.007)	(0.004)	(0.006)
Second Stage: NMTC Flag			
Total Population	-22,405	-26,141	-28,814
	(26,566)	(17,021)	(19,553)
% Non-White (Including Hispanic)	-0.672	-0.8448	-1.024
	(0.758)	(0.783)	(0.632)
% White Non-Hispanic	0.672	0.845	1.024
	(0.618)	(0.861)	(0.88)
% Aged 20–64	0.219	0.249	0.276
	(0.136)	(0.177)	(0.183)
N	12,060	12,060	12,060
Clusters	432	432	432
Threshold	62.2	62.2	62.2
Cragg-Donald Wald F statistic	0.73	1.56	0.69
Kleibergen-Paap rk Wald F statistic	0.78	1.62	0.71
		ACS Data	
First Stage: MFI Eligibility			
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.002	-0.005	-0.007
	(0.002)	(0.004)	(0.005)
Second Stage: NMTC Flag			
% Households: Married Couple Family	-0.314	0.174*	0.219*
	(0.232)	(0.098)	(0.114)
% Households: Other Family	-0.268	0.015	-0.018
	(0.172)	(0.017)	(0.021)
% Households: With Children	1.977	-0.689	-0.502
	(1.756)	(0.766)	(0.426)
% Pop 15 and Over: Married	-0.583	0.355	0.13
	(0.503)	(0.393)	(0.084)
% Pop 25+: Highest Education: Less than a HS Degree	0.61	-0.37	-0.06
	(0.488)	(0.422)	(0.069)
% Pop 25+: Highest Education: HS Degree	0.334	-0.183	-0.451
	(0.305)	(0.119)	(0.429)
% Pop 25+: Highest Education: Bachelors Degree	0.088	-0.15	0.026
	(0.094)	(0.094)	(0.03)
% Families: With Public Assistance	1.175	0.031	-0.243
	(1.211)	(0.028)	(0.238)
% Families: Below the Poverty Line	2.0448	-0.4728	-0.3241
	(2.38)	(0.522)	(0.283)
N	9,258	7,958	7,788
Clusters	417	403	410
Threshold	63.1	62.8	62.8
Cragg-Donald Wald F statistic	0.97	1.58	1.96
Kleibergen-Paap rk Wald F statistic	0.98	1.66	2.01
Stock-Yogo 10% Critical Value	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between +/- 10% of the placebo threshold that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table A.4.4: Placebo Tests: Census Tract Labor Market and Housing Outcomes

	2017 (1)	2018 (2)	2019 (3)
First Stage: MFI Eligibility			
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.002	-0.005	-0.007
	(0.002)	(0.004)	(0.005)
Second Stage: NMTC Flag			
Pop 16+: Employment to Population Ratio	0.51	0.62	0.049
	(0.316)	(0.384)	(0.055)
Pop 16+: Labor Force Participation Rate	0.142	0.608	0.099
	(0.126)	(0.675)	(0.096)
Pop 16+: Unemployment Rate	-0.154	-0.292	-0.149
	(0.149)	(0.306)	(0.136)
Workers 16+: Median Earnings: Male	130,129	-4,705	-18,747
	(115,011)	(3,164)	(20,650)
Workers 16+: Median Earnings: Female	-3,359	19,907	71,015
	(3,730)	(20,130)	(59,004)
Workers 16–64: Mean Usual Hours: Male	71.04	-0.153	2.167
	(57.37)	(0.162)	(1.602)
Workers 16–64: Mean Usual Hours: Female	-42.79	10.37	-6.673
	(38.22)	(10.05)	(4.314)
% Workers: Commute Less than 30 Minutes	1.426	-1.415	-0.536
	(1.441)	(0.934)	(0.588)
Median Monthly Housing Costs: Owner Occupied	-592.1	522.8	528.4
	(443.9)	(514.7)	(519.2)
Median Unit Value: Owner Occupied	-190,101	761,020	450,011
	(215,379)	(574,930)	(377,454)
Average Household Size: Owner Occupied	2.714	0.136	-2.252
	(2.019)	(0.104)	(1.774)
Median Year Household Moved In: Owner Occupied	97.09	10.83	13.40
	(63.75)	(8.06)	(14.82)
Median Year Housing Unit Built: Owner Occupied	28.09	-19.29***	20.98
	(32.94)	(7.038)	(23.91)
Median Gross Rent: Renter Occupied	-2,498	805.2	538.3
	(1,851)	(631.2)	(629.5)
Average Household Size: Renter Occupied	7.191	-1.792	-2.658
	(4.848)	(1.416)	(1.746)
Median Year Household Moved In: Renter Occupied	22.67	-20.70	-20.02
	(23.44)	(14.84)	(12.57)
Median Year Housing Unit Built: Renter Occupied	-96.09	-276.5	-22.84
	(112.1)	(193.6)	(23.90)
N	9,258	7,958	7,788
Clusters	417	403	410
Threshold	63.1	62.8	62.8
Cragg-Donald Wald F statistic	0.97	1.58	1.96
Kleibergen-Paap rk Wald F statistic	0.98	1.66	2.01
Stock-Yogo 10% Critical Value	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960

Notes: Includes census tracts with MFI ratios between +/- 10% of the placebo threshold that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

4.A.3.2 Sensitivity Tests

Table A.4.5: Sensitivity to Functional Form: Census Tract Population Outcomes

	2017				2018				2019			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						NCI Data						
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.013**	0.013	0.014*	0.012***	0.019***	0.02*	0.02**	0.016***	0.02*	0.019***	0.019**	0.016***
	(0.006)	(0.012)	(0.007)	(0.004)	(0.007)	(0.012)	(0.009)	(0.006)	(0.011)	(0.007)	(0.009)	(0.005)
Second Stage: NMTC Flag												
Total Population	2,360	1,967	883.8	3,172	1,757	1,404	692.6	2,514	1,858	1,436	770.8	2,589
	(1,728)	(1,830)	(773.4)	(2,774)	(1,213)	(1,298)	(608.8)	(2,910)	(2,114)	(1,494)	(708.2)	(2,313)
% Non-White (Including Hispanic)	0.188	0.208	0.185	0.175	0.1389	0.1532	0.1367	0.146	0.143	0.162	0.15	0.155
	(0.222)	(0.208)	(0.176)	(0.136)	(0.133)	(0.149)	(0.103)	(0.094)	(0.115)	(0.107)	(0.169)	(0.142)
% White Non-Hispanic	-0.188	-0.208	-0.185	-0.175	-0.139	-0.153	-0.137	-0.146	-0.143	-0.162	-0.15	-0.155
	(0.165)	(0.176)	(0.192)	(0.136)	(0.12)	(0.129)	(0.134)	(0.138)	(0.109)	(0.124)	(0.108)	(0.102)
% Aged 20–64	-0.157**	-0.229**	-0.171*	-0.074	-0.11**	-0.153**	-0.117*	-0.057	-0.107**	-0.152**	-0.118*	-0.058
	(0.07)	(0.11)	(0.1)	(0.08)	(0.044)	(0.069)	(0.065)	(0.055)	(0.045)	(0.067)	(0.063)	(0.05)
N	16,866	16,866	12,713	24,556	16,866	16,866	12,713	24,556	16,866	16,866	12,713	24,556
Clusters	461	461	457	465	461	461	457	465	461	461	457	465
Cragg-Donald Wald F statistic	4.69	1.17	4	9	7.37	2.78	4.94	7.11	3.31	7.37	4.46	10.24
Kleibergen-Paap rk Wald F statistic	4.76	1.19	4.06	9.16	7.45	2.83	5.03	7.24	3.35	7.49	4.54	10.44
						ACS Data						
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.013*	0.012	0.014	0.011***	0.02***	0.021*	0.022**	0.018***	0.02***	0.021*	0.02**	0.016***
	(0.008)	(0.01)	(0.015)	(0.004)	(0.007)	(0.011)	(0.011)	(0.006)	(0.007)	(0.013)	(0.008)	(0.006)
Second Stage: NMTC Flag												
% Households: Married Couple Family	0.119	0.056	0.148	0.201	-0.003	-0.042	0.001	0.058	0.008	-0.124	0.037	0.078
	(0.112)	(0.056)	(0.136)	(0.224)	(0.002)	(0.03)	(0.001)	(0.038)	(0.007)	(0.095)	(0.027)	(0.069)
% Households: Other Family	0.061	0.229	0.102	-0.029	0.08	0.111	0.077	0.029	0.023	0.131	0.052	-0.028
	(0.07)	(0.271)	(0.114)	(0.025)	(0.055)	(0.096)	(0.074)	(0.025)	(0.024)	(0.092)	(0.061)	(0.021)
% Households: With Children	0.303**	0.587	0.306**	0.096	0.087	0.128	0.074	0.055	0.063	0.058	0.077	0.032
	(0.14)	(0.554)	(0.152)	(0.094)	(0.066)	(0.104)	(0.048)	(0.06)	(0.063)	(0.047)	(0.061)	(0.031)
% Pop 15 and Over: Married	0.132	0.031	0.173	0.241	0.042	-0.009	0.048	0.095	-0.013	-0.148*	-0.002	0.054
	(0.144)	(0.024)	(0.184)	(0.177)	(0.049)	(0.008)	(0.033)	(0.11)	(0.014)	(0.077)	(0.002)	(0.056)
% Pop 25+: Highest Education: Less than a HS Degree	0.089	0.105	0.087	0.028	0.003	0.019	-0.011	0.018	-0.096	-0.091	-0.099	-0.05
	(0.074)	(0.065)	(0.075)	(0.018)	(0.003)	(0.018)	(0.013)	(0.016)	(0.077)	(0.095)	(0.074)	(0.038)
% Pop 25+: Highest Education: HS Degree	-0.315**	-0.523	-0.316**	-0.118	-0.084	-0.152	-0.071	-0.021	0.058	-0.033	0.064	0.133
	(0.155)	(0.415)	(0.128)	(0.132)	(0.066)	(0.13)	(0.049)	(0.014)	(0.044)	(0.025)	(0.042)	(0.143)
	(0.043)	(0.046)	(0.042)	(0.096)	(0.003)	(0.018)	(0.021)	(0.013)	(0.015)	(0.021)	(0.007)	(0.018)
% Pop 25+: Highest Education: Bachelors Degree	0.152**	0.208*	0.135**	0.069	0.085	0.099	0.074	0.052	0.115*	0.158	0.098*	0.011
	(0.073)	(0.11)	(0.065)	(0.081)	(0.067)	(0.1)	(0.071)	(0.038)	(0.069)	(0.167)	(0.057)	(0.011)
% Families: With Public Assistance	-0.055	0.03	-0.143	-0.218	-0.004	-0.017	-0.087	-0.041	-0.079	-0.029	-0.131	-0.036
	(0.058)	(0.021)	(0.104)	(0.221)	(0.003)	(0.02)	(0.08)	(0.044)	(0.055)	(0.022)	(0.089)	(0.024)
% Families: Below the Poverty Line	-0.0134	0.1473	-0.0911	-0.2305	-0.0005	0.0403	-0.0487	-0.0866	-0.1163	-0.1431	-0.1509	-0.0764
	(0.008)	(0.157)	(0.099)	(0.174)	(0)	(0.04)	(0.037)	(0.095)	(0.136)	(0.17)	(0.118)	(0.049)
N	13,811	13,811	10,434	20,011	12,830	12,830	9,701	18,649	12,621	12,621	9,553	18,305
Clusters	455	455	450	460	454	454	446	461	457	457	451	463
Cragg-Donald Wald F statistic	2.64	1.44	0.87	7.56	8.16	3.64	4	9	8.16	2.61	6.25	7.11
Kleibergen-Paap rk Wald F statistic	2.68	1.46	0.88	7.67	8.25	3.71	4.07	9.11	8.29	2.64	6.34	7.25
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960			
Control Function Order	1	2	1	1	1	2	1	1	1	2	1	1
Kernel	Tri	Uni	Uni	Uni	Tri	Uni	Uni	Uni	Tri	Uni	Uni	Uni
Bandwidth	10	10	7.5	15	10	10	7.5	15	10	10	7.5	15

Notes: Includes census tracts that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS/NCI Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table A.4.6: Sensitivity to Functional Form: Census Tract Labor Market and Housing Outcomes

	2017				2018				2019			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.013*	0.012	0.014	0.011***	0.02***	0.021*	0.022**	0.018***	0.02***	0.021*	0.02**	0.016***
	(0.008)	(0.01)	(0.015)	(0.004)	(0.007)	(0.011)	(0.011)	(0.006)	(0.007)	(0.013)	(0.008)	(0.006)
Second Stage: NMTC Flag												
Pop 16+: Employment to Population Ratio	-0.083	0.046	-0.068	-0.134	-0.009	0.049	-0.009	-0.05	-0.049	0.04	-0.051	-0.114
	(0.053)	(0.031)	(0.069)	(0.092)	(0.009)	(0.051)	(0.009)	(0.053)	(0.046)	(0.027)	(0.058)	(0.12)
Pop 16+: Labor Force Participation Rate	0.006	0.162	-0.006	-0.093	0.007	0.089	-0.005	-0.064	-0.043	0.075	-0.057	-0.133
	(0.006)	(0.191)	(0.005)	(0.066)	(0.008)	(0.065)	(0.005)	(0.049)	(0.028)	(0.052)	(0.058)	(0.115)
Pop 16+: Unemployment Rate	0.169	0.154	0.119*	0.13	0.05	0.049	0.028	0.02	0.011	0.001	-0.002	0.022
	(0.193)	(0.176)	(0.064)	(0.113)	(0.045)	(0.046)	(0.03)	(0.021)	(0.012)	(0.001)	(0.002)	(0.015)
Workers 16+: Median Earnings: Male	-19,647**	-40,556**	-10,149**	1,379	-1,669	-11,591*	2,498*	6,857	-1,052	-14,585*	4,541	7,037
	(7,850)	(20,551)	(4,381)	(1,491)	(1,344)	(6,566)	(1,348)	(7,519)	(944.7)	(7,818)	(5,023)	(5,358)
Workers 16+: Median Earnings: Female	-5,858	-3,138	-2,226	-7,127	-10,074	-8,298*	-4,973	-8,826	253.5	5,265	4,596	-12,292
	(6,278)	(2,795)	(1,653)	(5,771)	(9,471)	(4,748)	(5,121)	(8,712)	(293.6)	(5,700)	(3,262)	(13,143)
Workers 16–64: Mean Usual Hours: Male	-0.885	-6.057	-1.151	1.792	1.797	2.835	1.742	-0.184	1.061	0.283	1.739	0.827
	(0.766)	(7.162)	(0.934)	(1.125)	(1.36)	(2.085)	(1.172)	(0.2)	(0.846)	(0.246)	(1.36)	(0.595)
Workers 16–64: Mean Usual Hours: Female	10.80**	13.29	9.867*	4.098	6.842***	10.71**	5.997***	1.861	4.782	7.003	5.941	1.429
	(4.315)	(14.97)	(5.501)	(3.206)	(2.349)	(5.071)	(2.077)	(1.374)	(3.643)	(4.502)	(6.645)	(1.622)
% Workers: Commute Less than 30 Minutes	-0.046*	0.57*	-0.005	-0.322	-0.041	0.387*	0.007	-0.23	-0.032*	0.449	-0.024	-0.306
	(0.027)	(0.313)	(0.005)	(0.279)	(0.047)	(0.201)	(0.006)	(0.218)	(0.019)	(0.352)	(0.028)	(0.344)
Median Monthly Housing Costs: Owner Occupied	319.7**	983.4	390.1*	-276.6	255.3**	566.5	273.9**	-81.34	462.8***	863.7*	476.6**	-29.99
	(137.9)	(946.1)	(225.8)	(311.7)	(117.8)	(391.1)	(123.7)	(73.99)	(159.2)	(443.4)	(217.3)	(32.04)
Median Unit Value: Owner Occupied	-53,643	-37,172	-91,136	-240,130	31,965	82,554	5,536	-140,081	90,587	151,032	70,659	-110,020
	(54,360)	(31,782)	(66,218)	(166,315)	(28,954)	(84,027)	(3,932)	(131,999)	(88,485)	(129,284)	(52,862)	(76,324)
Average Household Size: Owner Occupied	1.672***	3.762	1.866***	0.045*	0.728*	1.142*	0.742**	0.129	0.762	0.884	0.843	0.534
	(0.632)	(3.022)	(0.604)	(0.025)	(0.433)	(0.656)	(0.292)	(0.106)	(0.873)	(0.746)	(0.938)	(0.545)
Median Year Household Moved In: Owner Occupied	15.42*	23.79	14.68	3.804	15.35***	20.61*	13.64***	6.204**	9.156	9.329	7.306	5.255
	(8.538)	(24.08)	(15.76)	(4.266)	(5.424)	(11.79)	(4.507)	(2.607)	(6.803)	(7.01)	(4.843)	(4.552)
Median Year Housing Unit Built: Owner Occupied	54.87**	105.1	49.58**	5.397*	36.60*	78.26	26.48**	-8.694	30.25*	122.1	27.72*	5.449
	(23.74)	(104.2)	(24.26)	(3.105)	(18.82)	(92.08)	(13.41)	(6.944)	(17.71)	(125.2)	(14.39)	(4.639)
Median Gross Rent: Renter Occupied	399.1*	634.1*	447.1	125.9	483.7**	675.7**	426.4**	211.9	741.8***	1,118*	782.7**	479.4
	(234.1)	(326.2)	(501.6)	(116.8)	(206.6)	(276.1)	(191.4)	(164.6)	(259.5)	(614.5)	(322.7)	(308.5)
Average Household Size: Renter Occupied	2.678**	2.884**	1.935**	1.763**	1.614**	1.584**	1.088**	1.062*	1.005	1.425	0.754*	0.491
	(1.095)	(1.302)	(0.823)	(0.877)	(0.718)	(0.697)	(0.425)	(0.573)	(1.147)	(1.494)	(0.428)	(0.5)
Median Year Household Moved In: Renter Occupied	-0.073	0.433	-0.012	2.437	-3.045	-2.884	-2.637	0.179*	-1.624	-1.753	-2.674	-0.46
	(0.054)	(0.35)	(0.012)	(2.124)	(3.264)	(3.251)	(2.193)	(0.1)	(1.498)	(1.182)	(2.134)	(0.456)
Median Year Housing Unit Built: Renter Occupied	10.08	31.74	10.41	-1.915	41.85**	53.96**	35.09***	31.79	-5.453	-33.14	-21.85	-22.73
	(8.317)	(22.92)	(6.503)	(2.1)	(17.77)	(21.44)	(12.78)	(33.29)	(5.874)	(35.02)	(24.95)	(23.39)
N	13,811	13,811	10,434	20,011	12,830	12,830	9,701	18,649	12,621	12,621	9,553	18,305
Clusters	455	455	450	460	454	454	446	461	457	457	451	463
Cragg-Donald Wald F statistic	2.64	1.44	0.87	7.56	8.16	3.64	4	9	8.16	2.61	6.25	7.11
Kleibergen-Paap rk Wald F statistic	2.68	1.46	0.88	7.67	8.25	3.71	4.07	9.11	8.29	2.64	6.34	7.25
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960			
Control Function Order	1	2	1	1	1	2	1	1	1	2	1	1
Kernel	Tri	Uni	Uni	Uni	Tri	Uni	Uni	Uni	Tri	Uni	Uni	Uni
Bandwidth	10	10	7.5	15	10	10	7.5	15	10	10	7.5	15

Notes: Includes census tracts that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table A.4.7: Sensitivity to Specification: Census Tract Population Outcomes

	2017				2018				2019			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						NCI Data						
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.013**	0.013**	0.013**	0.014***	0.017***	0.018***	0.018***	0.018***	0.018***	0.018***	0.018***	0.019***
	(0.006)	(0.007)	(0.005)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)
Second Stage: NMTC Flag												
Total Population	-1,955	-1,930	2,162	1,751	-1,404	-1,393	1,743	1,465	-1,267	-1,257	1,841	1,586
	(2,233)	(1,729)	(2,219)	(1,520)	(1,523)	(1,461)	(1,331)	(1,244)	(834.7)	(1,151)	(1,356)	(1,090)
% Non-White (Including Hispanic)	-0.163	-0.154	0.205	0.184	-0.113	-0.1067	0.1608	0.1449	-0.108	-0.102	0.154	0.144
	(0.173)	(0.155)	(0.164)	(0.19)	(0.109)	(0.098)	(0.184)	(0.156)	(0.077)	(0.074)	(0.106)	(0.127)
% White Non-Hispanic	0.163	0.154	-0.205	-0.184	0.113	0.107	-0.161	-0.145	0.108	0.102	-0.154	-0.144
	(0.175)	(0.12)	(0.162)	(0.169)	(0.075)	(0.077)	(0.161)	(0.091)	(0.077)	(0.073)	(0.115)	(0.17)
% Aged 20–64	0.045	0.051	-0.094**	-0.089*	0.032	0.037	-0.07*	-0.068*	0.031	0.035	-0.066*	-0.064*
	(0.04)	(0.036)	(0.04)	(0.049)	(0.025)	(0.042)	(0.04)	(0.037)	(0.032)	(0.03)	(0.038)	(0.037)
N	16,866	16,866	16,866	16,866	16,866	16,866	16,866	16,866	16,866	16,866	16,866	16,866
Clusters	461	461	461	461	461	461	461	461	461	461	461	461
Cragg-Donald Wald F statistic	4.69	3.45	6.76	7.84	8.03	6.61	6.61	6.61	9	6.61	9	10.03
Kleibergen-Paap rk Wald F statistic	4.74	3.51	6.85	7.98	8.15	6.72	6.71	6.69	9.13	6.7	9.15	10.16
						ACS Data						
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.014*	0.015*	0.013*	0.015**	0.02***	0.02***	0.02**	0.02***	0.02***	0.02***	0.02***	0.02***
	(0.008)	(0.008)	(0.007)	(0.006)	(0.007)	(0.008)	(0.01)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Second Stage: NMTC Flag												
% Households: Married Couple Family	0.108	0.096	0.13	0.166	-0.038	-0.035	0.038	0.021	0.086	0.088	0.099	0.092
	(0.094)	(0.108)	(0.129)	(0.182)	(0.034)	(0.032)	(0.032)	(0.013)	(0.078)	(0.062)	(0.07)	(0.093)
% Households: Other Family	-0.164	-0.159	0.005	-0.035	0.027	0.028	0.055	0.06	-0.005	-0.004	-0.046	-0.036
	(0.121)	(0.162)	(0.004)	(0.032)	(0.018)	(0.025)	(0.059)	(0.044)	(0.006)	(0.005)	(0.036)	(0.025)
% Households: With Children	-0.054	-0.055	0.266*	0.146***	-0.044	-0.043	0.091	0.055	0.042	0.044	0.047	0.047
	(0.039)	(0.04)	(0.142)	(0.052)	(0.027)	(0.05)	(0.107)	(0.056)	(0.046)	(0.031)	(0.029)	(0.054)
% Pop 15 and Over: Married	0.14	0.128	0.14	0.17	0.01	0.012	0.059	0.071	0.036	0.035	0.07	0.069
	(0.151)	(0.112)	(0.144)	(0.202)	(0.007)	(0.012)	(0.069)	(0.078)	(0.022)	(0.036)	(0.045)	(0.064)
% Pop 25+: Highest Education: Less than a HS Degree	-0.005	-0.001	0.008	0.051	-0.011	-0.01	-0.038	-0.006	0.005	0.008	-0.103	-0.088
	(0.005)	(0.001)	(0.007)	(0.047)	(0.012)	(0.006)	(0.03)	(0.006)	(0.005)	(0.009)	(0.101)	(0.096)
% Pop 25+: Highest Education: HS Degree	-0.065	-0.071	-0.217	-0.168***	0.161	0.163	-0.022	-0.021	0.255	0.254	0.148	0.143
	(0.057)	(0.064)	(0.207)	(0.056)	(0.11)	(0.18)	(0.018)	(0.017)	(0.275)	(0.161)	(0.169)	(0.112)
% Pop 25+: Highest Education: Bachelors Degree	0.148	0.148	0.079	0.091	0.017	0.016	0.069	0.046	0.003	0.003	0.05	0.057*
	(0.15)	(0.145)	(0.075)	(0.106)	(0.019)	(0.019)	(0.074)	(0.035)	(0.003)	(0.002)	(0.049)	(0.034)
% Families: With Public Assistance	-0.118	-0.112	-0.004	-0.039	0.044	0.043	0.025	0.057	-0.077	-0.078	-0.109	-0.075
	(0.114)	(0.126)	(0.005)	(0.041)	(0.037)	(0.041)	(0.026)	(0.048)	(0.056)	(0.074)	(0.080)	(0.046)
% Families: Below the Poverty Line	-0.128	-0.121	-0.025	-0.064	-0.018	-0.019	-0.022	0.001	-0.064	-0.064	-0.106	-0.079
	(0.091)	(0.092)	(0.022)	(0.046)	(0.012)	(0.017)	(0.026)	(0.001)	(0.074)	(0.062)	(0.101)	(0.093)
N	13,811	13,811	13,811	13,811	12,830	12,830	12,830	12,830	12,621	12,621	12,621	12,621
Clusters	455	455	455	455	454	454	454	454	457	457	457	457
Cragg-Donald Wald F statistic	3.06	3.52	3.45	6.25	8.16	6.25	4	8.16	8.16	8.16	8.16	6.25
Kleibergen-Paap rk Wald F statistic	3.1	3.59	3.49	6.33	8.29	6.32	4.07	8.29	8.3	8.25	8.28	6.33
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960			
Prior NMTC Investment		x	x	x		x	x	x		x	x	x
Census Tract Covars			x	x			x	x			x	x
MSA Fixed Effects				x				x				x

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS/NCI Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

Table A.4.8: Sensitivity to Specification: Census Tract Labor Market and Housing Outcomes

	2017				2018				2019			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
First Stage: MFI Eligibility												
NMTC Flag (Cumulative NMTC Amount After 2013 >0)	0.014*	0.015*	0.013*	0.015**	0.02***	0.02***	0.02**	0.02***	0.02***	0.02***	0.02***	0.02***
(0.008)	(0.008)	(0.007)	(0.006)	(0.007)	(0.008)	(0.01)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	
Second Stage: NMTC Flag												
Pop 16+: Employment to Population Ratio	-0.103	-0.103	-0.092	-0.176	-0.003	-0.003	0.027	-0.081	-0.076	-0.077	-0.060	-0.134
	(0.083)	(0.098)	(0.103)	(0.120)	(0.002)	(0.003)	(0.026)	(0.059)	(0.053)	(0.047)	(0.057)	(0.140)
Pop 16+: Labor Force Participation Rate	-0.069	-0.068	-0.017	-0.109	-0.010	-0.010	0.041	-0.066	-0.083	-0.084	-0.066	-0.133
	(0.072)	(0.058)	(0.019)	(0.087)	(0.007)	(0.010)	(0.043)	(0.062)	(0.073)	(0.052)	(0.074)	(0.120)
Pop 16+: Unemployment Rate	0.141	0.141	0.167	0.169	0.049	0.050	0.053	0.065	0.035	0.036	0.026	0.042
	(0.145)	(0.146)	(0.182)	(0.109)	(0.052)	(0.033)	(0.054)	(0.043)	(0.031)	(0.042)	(0.029)	(0.035)
Workers 16+: Median Earnings: Male	-14,575	-14,670	-22,280	-11,342*	2,316	2,293	3,187	4,591	5,620	5,546	10,547	6,711
	(14,492)	(11,774)	(25,802)	(6,461)	(2,642)	(1,491)	(2,505)	(4,458)	(4,429)	(5,798)	(6,545)	(7,354)
Workers 16+: Median Earnings: Female	-3,189	-2,876	-5,739	-9,668	-4,948	-5,114	1,073	-8,863	355.1	280.6	3,282	-3,973
	(2,495)	(2,214)	(4,802)	(10,369)	(4,548)	(3,134)	(696.2)	(7,893)	(350.1)	(172.5)	(3,036)	(4,524)
Workers 16–64: Mean Usual Hours: Male	-3.773	-3.842	-1.237	-0.766	-2.226	-2.281	1.916	0.050	-1.013	-1.179	0.749	0.743
	(3.118)	(3.782)	(1.431)	(0.859)	(2.632)	(1.524)	(2.165)	(0.054)	(1.067)	(1.248)	(0.812)	(0.539)
Workers 16–64: Mean Usual Hours: Female	4.172	4.083	9.161*	6.710*	2.113	2.106	6.820**	3.036**	1.144	1.156	2.199	1.949
	(2.701)	(3.698)	(4.990)	(3.746)	(1.832)	(2.481)	(3.395)	(1.406)	(0.936)	(1.222)	(2.390)	(1.251)
% Workers: Commute Less than 30 Minutes	-0.276	-0.274	0.1	-0.347	-0.171	-0.177	-0.021	-0.302	-0.3	-0.306	-0.22	-0.316
	(0.288)	(0.3)	(0.086)	(0.403)	(0.124)	(0.184)	(0.019)	(0.265)	(0.293)	(0.195)	(0.234)	(0.208)
Median Monthly Housing Costs: Owner Occupied	5.556	13.17	-27.74	-65.31**	-25.22	-22.21	273.18	-18.45*	423.1	436.9	295.3	121.6**
	(4.063)	(10.22)	(24.05)	(29.05)	(20.23)	(17.93)	(228.7)	(9.713)	(316.3)	(380.2)	(309.3)	(47.50)
Median Unit Value: Owner Occupied	110,134	128,987	-152,003	-67,376	29,815	29,869	12,963	-25,122	251,112	260,304	74,053	33,169
	(72,655)	(115,446)	(118,957)	(79,731)	(19,901)	(28,124)	(10,640)	(24,910)	(207,610)	(228,179)	(67,971)	(27,447)
Average Household Size: Owner Occupied	-0.075	-0.076	1.474**	0.381***	0.173	0.189	0.819**	0.358*	0.953	0.981	0.708	0.603
	(0.07)	(0.063)	(0.577)	(0.132)	(0.187)	(0.211)	(0.354)	(0.214)	(0.968)	(0.675)	(0.745)	(0.714)
Median Year Household Moved In: Owner Occupied	6.239	6.259	16.16	8.599**	3.094	3.085	13.94**	10.57***	1.895	1.984	6.767	8.121
	(5.77)	(4.05)	(18.45)	(3.945)	(1.904)	(2.251)	(5.901)	(3.435)	(1.963)	(2.198)	(6.071)	(8.351)
Median Year Housing Unit Built: Owner Occupied	-13.72	-13.61	66.70	25.36**	-36.85	-37.04	38.68	3.686**	-69.68	-69.93	-37.03**	-37.53**
	(9.09)	(12.02)	(66.85)	(10.63)	(31.52)	(31.87)	(24.3)	(1.535)	(43.21)	(78.67)	(16.77)	(14.92)
Median Gross Rent: Renter Occupied	392.7	387.1	167.0	263.9	198.8	204.8	486.2	289.9*	784.7	798.3	629.1*	466.6***
	(301.4)	(236.3)	(196.7)	(280.6)	(145.1)	(194.2)	(527.4)	(171.4)	(673.3)	(740.7)	(358.1)	(170.6)
Average Household Size: Renter Occupied	1.891	1.85	2.37**	2.679*	0.705	0.724	1.226	1.624	0.701	0.715	0.359	0.784
	(2.092)	(1.844)	(1.119)	(1.564)	(0.525)	(0.558)	(1.186)	(1.439)	(0.701)	(0.468)	(0.299)	(0.684)
Median Year Household Moved In: Renter Occupied	-0.028	-0.051	1.555	0.27	-3.552	-3.607	-3.01	-2.758	-3.474	-3.487	-1.541	-1.855
	(0.029)	(0.06)	(1.456)	(0.227)	(3.987)	(2.267)	(2.704)	(3.224)	(3.177)	(3.388)	(1.088)	(1.157)
Median Year Housing Unit Built: Renter Occupied	-36.16	-35.79	29.14	-2.056	-6.262	-6.309	43.16*	29.22**	-66.05	-66.29	-42.49	-3.208
	(40.03)	(30.86)	(19.93)	(1.49)	(7.044)	(4.398)	(25.57)	(13.36)	(68.95)	(67.78)	(43.93)	(3.007)
N	13,811	13,811	13,811	13,811	12,830	12,830	12,830	12,830	12,621	12,621	12,621	12,621
Clusters	455	455	455	455	454	454	454	454	457	457	457	457
Cragg-Donald Wald F statistic	3.06	3.52	3.45	6.25	8.16	6.25	4	8.16	8.16	8.16	8.16	6.25
Kleibergen-Paap rk Wald F statistic	3.1	3.59	3.49	6.33	8.29	6.32	4.07	8.29	8.3	8.25	8.28	6.33
Stock-Yogo 10% Critical Value	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Stock-Yogo 15% Critical Value	8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960 8.960	8.960	8.960			
Prior NMTC Investment		x	x	x		x	x			x	x	x
Census Tract Covars			x	x			x	x			x	x
MSA Fixed Effects				x				x				x

Notes: Includes census tracts with MFI ratios between 70% and 90% that are not in GO Zones, not high migration rural counties, and those will all relevant outcomes and covariates available in the ACS Data. Models estimated using local linear regression. Prior NMTC investment controls include a flag for any NMTC projects between 2000 and 2009 and a flag for any NMTC projects between 2009 and 2012. Census tract covariates include all variables listed in Table 4.2. For census tracts not in a MSA, a state fixed effect is used. Robust standard errors clustered at the MSA level (or state level for tracts not in a MSA) are used, with adjustments for multiple hypothesis testing using Benjamini and Hochberg (1995). Significance levels based on robust p values represent: * 10% level, ** 5% level, *** 1% level.

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