Title
Essays in Public Economics

Author
Saucedo Hernandez, Monica

Publication Date
2023

Peer reviewed|Thesis/dissertation

Essays in Public Economics

by

Monica Saucedo Hernandez

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Emmanuel Saez, Chair
Professor Hilary Hoynes
Professor Jesse Rothstein
Associate Professor Danny Yagan

Spring 2023

Essays in Public Economics

Abstract

Essays in Public Economics

by

Monica Saucedo Hernandez

Doctor of Philosophy in Economics

University of California, Berkeley

Professor Emmanuel Saez, Chair

This dissertation uses econometric models to reevaluate key questions in public finance economics and estimate causal effects of funding changes on safety net programs. In particular, this dissertation explores various funding to federal programs and their impact on the social welfare of vulnerable Americans. The main thread connecting these chapters is their relevance to policy and programs that affect disadvantaged communities.

The first chapter studies the effects of government funding on private funding to charitable organizations. I exploit a recent release of tax microdata from electronic filers spanning an eight-year period and analyze the financial responses of food banks and similar charitable organizations. Using a U.S. federal program that allocates funding to states on a formula-basis, I implement an instrumental variables design to analyze the aggregate state-by-year change in private funding to emergency food providers as a result of an increase in exogenous government funding. Contrary to predictions from the standard crowd-out theory, I find that private funding increases by 1% to 1.6% as a result of a 1% increase in government funding. This increase is driven by growth on the intensive margin, or growth among existing organizations rather than an increase in number of organizations, and is robust to various specifications. I also find crowd-in of fundraising expenditures, which suggests the increase in private funding is driven by increased fundraising efforts. My results highlight the importance of accounting for heterogeneity in financial responses across types of charitable organizations and how government funding may help food providers increase their scale to help solve social needs.

The second chapter evaluates the effects of low-income housing on crime rates in affluent neighborhoods, which are frequently excluded in similar analyses. I exploit a change in policy on how the Low Income Housing Tax Credit (LIHTC) is awarded in Texas, which incentivizes the construction of subsidized housing by private developers. The policy change created a rule to award more generous tax credits to developments located in affluent communities. Using the quasiexperimental variation on project location generated by this rule, I find that

eligible areas see an increase of 8% in their flow of units over comparable areas that do not qualify for the tax credit boost. Additional LIHTC units do not appear to affect property or violent crime rates in their low-poverty neighborhood. I also do not find evidence of an effect on drug related offenses.

The third chapter builds on the second chapter's empirical setting and the first chapter's theoretical framework to estimate the effects of the change in LIHTC construction in affluent neighborhoods and consequent effects on the housing market. I use a fuzzy difference-in-difference design to evaluate the crowd out effects of subsidized housing on privately owned housing. I also analyze the effects on other market outcomes. I find that subsidized housing does not crowd out market rate housing in affluent areas. I also find that the rental vacancy rate increases as a result of new subsidized housing, which may suggest that the change in housing is driven by increased rental housing rather than homeowner-occupied housing.

In loving memory of my dear friend and mentor, Roy Robbins, Ph.D.

Acknowledgments

I am deeply thankful to my committee chair, Emmanuel Saez, who patiently listened to my ideas before they were even complete thoughts and helped me shape a research agenda I am proud of. I am constantly in awe of Emmanuel and his way of digesting information and providing the most insightful feedback. I am also thankful to Hilary Hoynes, who more than an advisor was a mentor in navigating the ins and outs of research and graduate school. Hilary was tremendously generous with her time, meeting with me often weekly, and went above and beyond in offering me support every step of the way. Her willingness to challenge me when necessary and support me in hashing out details in my research really helped shape my job market paper, and for that I'm indebted to her. I am also thankful to Jesse Rothstein and Danny Yagan, who were always willing to step in to offer feedback and push me to dig deeper or analyze new angles. Jesse was also an incredible supervisor in research and I learned a great deal working alongside him at the California Policy Lab. I am thankful for the many Berkeley faculty who have given support and feedback at various points during my time in graduate school: Alan Auerbach, David Card, Barry Eichengreen, Cecile Gaubert, Chris Walters, and Gabriel Zucman.

I am also thankful to the many people I met while at Berkeley. The department staff, in particular Patrick Allen, kept me sane and answered my many questions as a first-year student. My cohort, who offered so much incredible feedback at various points in my research. Particularly Tatiana Reyes, who kept me on track as I finished my job market paper meeting with me weekly and working through multiple rounds of proofing while also being a friend. I am also thankful to my St. Mary's University family who supported me and always believed in me. Having Steve Nivin, Belinda Roman, Irma Herrera, Margaret Robbins, and the late Roy Robbins in my corner kept me going even in the toughest moments of graduate school.

I am thankful to my partner, Jose Alvarado. Thank you for being a sounding board, an editor, and an audience. But mostly, thank you for always loving me, supporting me and cheering me on. You, along with Bruno, Nala, and Pepper keep me sane and fill my heart with so much joy.

Finally, I am so grateful to my parents, who instilled in me a love for education and always fostered my curiosity for learning. They left their country without financial means or formal education to lean on, just so I could have a better future. Graduating with a Ph.D. from such an incredible university is proof that they achieved this goal. To this day, they don't fully understand what I do, but have always offered me unconditional support and love. Gracias mami y papi, esto es para y por ustedes.

Contents

List of Figures

List of Tables

Chapter 1

Effects of Government Funding to Charitable Organizations: Evidence from Food Banks

1.1 Introduction

The recent health crisis resulting from the COVID-19 pandemic highlighted the large role that charitable organizations play in the U.S. social safety net. While some countries have public systems in place that helped mitigate the issues that arose, including healthcare needs and food insecurity, the U.S. heavily depended on nonprofit organizations like hospitals and food banks to fill in gaps that the government was not set up to address directly. Given the prominent role of charitable organizations in the U.S. economy, the interaction between private and public funds given to these organizations has been an important subject in the study of public finance and crowd-out theory since the 1980s (B. A. Abrams & Schmitz, 1984; Bergstrom, Blume, & Varian, 1986; Andreoni, 1989).

The widely accepted theory from Androeni's (1989; 1990) work on impure altruism suggests that government funds inevitable crowd out private funds. However, subsequent empirical studies have produced inconsistent results (see De Wit & Bekkers (2017) for a meta-analysis). These studies are often limited by data access and the validity of their results across different types of charitable organizations is inconclusive. Another drawback in this empirical literature is the difficulty in finding exogenous variation in government funding. Convincing causal evidence is still needed to evaluate the effects of government funding on charitable organizations.

This paper exploits a relatively new and rich tax data set on nonprofit organizations and uses an instrumental variable empirical design to obtain causal estimates of the effect of government funding on private funding. The granularity of this data set allows me to focus the analysis on a specific sector within the nonprofit umbrella. I restrict the analysis to emergency food providers. By focusing on emergency food providers, I am able to exploit a federal funding program that allocates money to states on a formula-basis for my estimation strategy. The allocation formula uses lagged values of poverty and unemployment, which allows me to control for current economic conditions and retain identification power. Studying this type of charitable organization sets up the framework to study similar charitable organizations that play an important role in the social safety net.

Contrary to standard theoretical predictions, my estimates show that government funding crowds in private funding. The results indicate a 1 to 1.6 percent increase in private funding as a result of a one percent increase in government funding. These results are driven by intensive margin growth, rather than an increase number of service providers. I conduct the analysis at both the state and county levels spanning a period of eight years. I also explore potential mechanisms that result in crowding in and find that fundraising expenditures also increase while revenue from mission-detached programming decreases.

I focus the analysis on emergency food providers because of their important function in the U.S. economy and their relation to other charitable organizations. The role of emergency food providers is to supply food to people who may be experiencing food insecurity, whether or not they qualify for the established and recurring federal programs, with temporary food assistance in order to address this gap in the safety net. An added benefit of studying emergency food providers is the ease with which we can understand their mission and role and compare it to other charitable organizations. Primarily, they differ from organizations like museums, schools, and hospitals whose donors are also customers, and instead serve people who are not in a position to be donors. This difference is important when it comes to the way we have interpreted giving behavior and crowd-out theory in the past.

In addition to the tax data used to study financial elements of organizations, I also use data from USDA congressional reports and supplement those with ACS and BLS data to develop the estimation strategy. The tax microdata encompasses nonprofit organizations who filed their tax return with the IRS electronically, and covers such organization back to 2011. The data synthesizes all information from the tax return for each organization, something that had not been available in a machine readable format since the early 2000s. The availability of these data allows me to use an exhaustive sample of organizations. I aggregate financial elements from the sample of organizations to various geographic levels, that correspond to the level of variation in government funding, and combine with financial and demographic data from the USDA, ACS, and BLS to carry out the empirical analysis.

The instrument for the IV estimation strategy is based on a large federal funding program that the USDA uses to allocate money to states on a formula basis. This program, called The Emergency Food Assistance Program (TEFAP), is the only federal program to fund food relief to any American in need through their food distribution networks. The allocation formula for this funding relies on lagged measures of poverty and unemployment levels relative to national totals. The total funding is specified in the U.S. Farm Bill, up to five years in advance. The exogeneity of these determining factors and the important role the program plays in funding emergency food providers result in a strong first stage and make this type of government funding an ideal instrument.

The results of the analysis show that government funding crowds in private funding at both the state and county levels. Though the results are statistically significant, the number of observations in the state-level sample is limited by the years of data available. I take advantage of the fact that sixteen states replicate the federal formula to disburse TEFAP funding to their counties and validate the model and results by applying the same empirical strategy at the county level. The county-level analysis provides a source of comparison and a more comprehensive data set.

Although these results are at odds with the canonical theoretical model (Andreoni, 1989), recent work from M. Kotchen & Wagner (2019) (KW) helps to rationalize these findings. The Andreoni model assumes that government transfers to charitable organizations are funded through lump sum taxes, and thus net substitutes for private funding from a donor's perspective because when donors are taxed, they reduce their private donations by an equal or smaller amount. KW's work highlights that by relaxing this assumption, crowd-in may be possible. In the context of emergency food providers, government transfers, including TEFAP funding, are not funded through lump-sum taxes, but rather set at the federal level as part of budget negotiations. From the donor's perspective, this funding will not necessarily be a substitute for their private donation because they do not know which organizations their tax dollars are going to and how much those payments will be. We can also think of cases where a donor may see their private donation and government funding as complements rather than substitutes. I present various scenarios that illustrate how differently motivated donors may perceive this relationship.

In order to verify my results and draw comparisons to existing empirical work, I test instruments that have been previously used and conduct a placebo test of my own instrument on a different sector of nonprofit organizations. Borrowing from the Andreoni & Payne papers (2003; 2011), I construct instruments based on areas represented in the federal budget appropriations committee and NIH grants to local research universities. While the instruments are relatively weak compared to TEFAP funding, they also suggest crowd-in of similar magnitude as my main specification. The placebo tests recreate the main specification but applies it using arts and humanities organizations as well as other organizations discarded from the sample. I find no first stage or significant results, which provides evidence that my results are not capturing some other trends.

I also present some findings to rationalize what may be driving the crowding in. I show that an increase in government funding is associated with an increase in total fundraising expenditures suggesting that organizations are able to invest more in securing additional funds. I also see a decrease in activities unrelated to the food provider's mission. These activities are outside of the scope of the providers' mission and are a means to obtaining additional revenue. With additional government support, it makes sense that organizations are able to move away from these activities to focus on their mission. These results provide evidence to support the narrative that there is organizational growth of mission.

This paper contributes to the existing literature on crowd-out and nonprofit organizations. The paper builds on previous empirical work that uses an IV framework to estimate the relationship between government funding and other financial outcomes, but takes advantage of a federal program to construct a valid and strong instrument. The crowd-in results and specific setting help cement the work by KW that expands the classic Andreoni theory. The results also speak to the differences in function of organizations within the nonprofit sector. In particular, I show that for emergency food providers, fundraising does not decrease in response to exogenous government funding as is the case with some other types of organizations (Andreoni & Payne, 2011).

The rest of the paper is organized as follows: Section 1.2 summarizes the theoretical background and existing empirical evidence on crowding effects. Section 1.3 describes the empirical setting of emergency food providers and the relevant U.S. federal program. Section 1.4 breaks down the data sources and the construction of the sample. Section 1.5 defines the estimation strategy along with the construction and valitity of the instrument. Sections 1.6 and 1.7 provide results and the state and county levels respectively. Section 1.8 relates the findings back to the background described in section 1.2 and supplements the results with placebo tests for robustness. Section 1.9 analyzes various other relationships of government funding to explore the mechanisms driving the crowd-in results. Finally, section 1.10 concludes.

1.2 Background

Theory

Early work by Andreoni (1989; 1990) develops a theoretical model of charitable giving. A core finding of that work is that private funds through charitable giving are inevitably crowded out in response to government funds. However, recent work by M. Kotchen & Wagner (2019) generalizes the central assumptions of the original model and shows that under the relaxed assumptions, crowd-in may be possible.

There is an underlying tension between the public and private sector in terms of funding streams for public goods. The response of private funding to public funding is defined in the framework of pure altruism. The simplified version of the Bergstrom, Blume, & Varian (1986) framework of pure altruism posits a donor i's utility maximizing problem relative to private consumption x and total supply of the public good G:

$$\max_{x_i, g_i} U_i(x_i, G) \text{ s.t. } G = G_{-i} + g_i \text{ and } w_i = g_i + x_i \tag{1.1}$$

Where g_i is donor i's private contribution and G_{-i} is the total exogenous funding, in this case private donations from others, to the public good. The starting point of this framework is that all funding to the public good is through voluntary giving.

The implication of this model is that there is an optimal level G^* which is determined by either private giving, exogenous giving, or a combination of both. If the government then contributes fund to the public good, which it collects through lump-sum taxes on donors, donors will reduce their private contributions by an amount equal to the tax. It is easy to see why this model predicts total crowd out, given that when optimal G^* is supplied, whether through taxes or direct giving, there is no added utility to the donor from additional private giving.

Andreoni (1989, and more concretely 1990) expands on this model to make space for impure altruism where a donor also obtains utility from the act of giving, referred to as warm glow. This model extends the utility problem to include g_i in the utility function:

$$\max_{x_i, g_i} U_i(x_i, G, g_i) \text{ s.t. } G = G_{-i} + g_i \text{ and } w_i = g_i + x_i \tag{1.2}$$

This model also predicts crowding out, though it is incomplete. In the case of pure altruism, donations by person i and funding through other donors or lump-sum taxes are seen as perfect substitutes for each other. In the case with impure altruism, however, a donor is not as affected by the contributions from other donors, as they continue to receive utility from warm glow. But if lump-sum tax to fund the public good is levied on the donor, the donor will still decrease their own donation, though by less than the amount of the tax. This is also because their utility increases with the warm glow of a voluntary gift, and thus they will give some level g_i. While crowd-out is expected, it is incomplete.

Recent work by M. Kotchen & Wagner (2019) (KW) extends the Andreoni (1990) theory to evaluate how predictions for the crowding effect change when the assumption that private and public funding are substitutes for one another is challenged. The addition of warm glow to the utility function means g_i enters the utility function in the impure altruism model through two terms. This implies a joint production of the private and public good. In contrast, KW, following Cornes & Sandler (1994), reframes the problem in familiar income and Hicksian compensated price responses, where demand for the private and public good are obtained separately and are based on virtual prices. They present this as a thought exercise to explore the relationship between the elements of the utility function and how they respond to changes in one another. Of particular interest is the crowding effect $\frac{\partial g_i^*}{\partial G_{-i}}$, which is defined by the the change in private funding in response to exogenous funding. Within this framework, KW also evaluate the claim that donors are not affected by exogenous giving, or funding by others. They show results for an unfunded system where public provision is not funded through a lump-sum tax, but is provided either voluntarily by others or via another form of transfer not directly impacting the donor.

KW find that $\frac{\partial g_i^*}{\partial G_{-i}}$ is only negative (crowd-out) when private and public provision are substitutes, but may be positive when they are complements. They also find that in both the funded and unfunded systems, the sign of $\frac{\partial g_i^*}{\partial G_{-i}}$ is indeterminate when private and public provision are complements. The only case where crowding out is guaranteed is under a funded system where public and private funding are net substitutes. Key insights of the KW paper are summarized in Table 1.1. We learn from this analysis that crowd-in is admissible under any other setting.

Empirical Evidence

Previous empirical work on the effects of government funding on private funding within charitable organizations has produced inconsistent results often due to data limitations and the inherently endogenous relationship between the two funding streams. For example, inherent characteristics of organizations, such as age, size, and influence may affect both private and pubic funding. Identifying exogenous variation in public funding to estimate causal effects has been a problem in this literature.

There have been several empirical papers that estimate a government crowd-out and crowd-in effect among charitable organizations. A recent meta analysis by De Wit & Bekkers (2017) provides a review of some of the key findings of the related empirical literature. They review a sample of 70 papers that contain a mixture of experimental and non-experimental designs. Figure 1.1 plots the results from these studies to show the effect of a $1 increase of government funding on private funding.

Their evaluation finds that one-third of the studies found a positive correlation between government funding and private funding. As seen in the summary figure, most of the crowding-in results are from non-experimental studies. Importantly, experimental studies, or lab experiments, often model their design following a funded lump-sum tax system (Eckel et al., 2005), which may speak to some of the theoretical assumptions discussed in the previous section.

Another interesting finding from the meta analysis was the breakdown of type of charitable organizations studied. Out of 33 studies with 48 results focusing on non-experimental designs within the U.S. context, almost a third focused on organizations within the Arts and Humanities category. The full break down is in Figure 1.2. Nine results make up the "Other" category and represent types of organizations each with one relevant result. The nine types are Crime, Animal Welfare, Food, Employment, Housing, Religious, Philanthropy, Environment, and Community Improvement.

Aside from a few studies (e.g.Hungerman (2005),Borgonovi (2006),M. Kotchen & Wagner (2019)), who obtain sector specific proprietary data, most empirical studies rely on a small sample of nonprofit tax forms from late 1990s and early 2000s. Given the limited availability of data, many studies rely on small samples of organizations, particularly when studying specific sectors. For example Smith (2007) has a sample of under 500 organizations in each category for five out of the six categories studied. Many other studies ((B. Abrams & Schmitz, 1978),(B. A. Abrams & Schmitz, 1984),(O'Regan & Oster, 2002) rely on aggregating all nonprofits, or several categories. While this strategy may provide more power for estimation, it ignores the key differences across organizations and how they may respond to government funding. By this same token, even some studies ((Andreoni & Payne, 2011), (Tinkelman & Neely, 2011))that focus on specific types of organizations, do not distinguish between organizations within that type, such as pre-schools and alumni associations, which both fall under the education category, but may have very different funding strategies and priorities.

Aside from data limitations, a key limitation to studying this type of crow-out effects is finding exogenous variation to estimate causal effects. A number of empirical studies have identified correlation rather than causation using an OLS or GLS framework (Borgonovi, 2006; Smith, 2007). Though causal estimates are not presented, both of these papers find crowd-in results. The studies that do find causal effects typically use an instrumental variables framework and instruments based on political variables to predict government funding (Andreoni & Payne, 2011; M. Kotchen & Wagner, 2019). The criticism with these types of instruments, however, is that donors will vote for representatives that likely match their giving preferences (Andreoni & Payne, 2013). There is also the question of influence actual congress members may have over levels of funding allocated to their districts.

Other related results attempt to understand the effect of government funding on fundraising expenditures as a means to disentangle the mechanisms driving crowd-out or in of private funding. Thornton (2014) uses political variables to instrument for various types of federal grants. The paper finds that program-based grants and formula-block grants crowd-out fundraising expenditures, but matching grants, those based on organization-specific performance, crowd-in fundraising expenditures. However these results were only significant for certain sectors. In fact, for food and nutrition organizations, formula-block grants increased fundraising expenditures, though the result is not statistically significant. The variation in results across categories of organizations points to the heterogeneity among organizations and their financial decisions.

1.3 Empirical Setting

Prior empirical work often analyzes together the entire nonprofit sector. Instead, here, I focus on one sector – emergency food providers – that is important, understudied, and offers the opportunity for causal analysis using formula-funded government program. Analysis of this sector sets up the framework to study other types of similar charitable organizations, which are designed to fill gaps in the social safety net.

Emergency Food Providers

The sector of emergency food providers includes food banks, food pantries, and soup kitchens. This sector models a type of organization that can be easily classified as a quintessential charitable organization that fills a role in the social safety net. It also allows us to better understand a key player in the food insecurity policy discussion.

During the Covid-19 health crisis, the issue of food insecurity was brought to the forefront of many discussions. Given the rising unemployment rates and delay in accessing benefits such as SNAP, news stories of food banks with mile-long lines were circulating (Martin, 2020) and highlighted the roles emergency food providers serve in times of crisis. Given this increased need to get food to people, especially vulnerable populations like children who relied on free school lunches, emergency food providers were there to serve their communities when the bureaucratic red tape of SNAP and other benefits programs delayed the provision of more sustainable services or proved insufficient.

In response to the changing needs during the pandemic, the Census Household Pulse Survey introduced questions on food insecurity and food insufficiency. Research by Bitler, Hoynes, & Schanzenbach (2020) (BHS) compared pre-pandemic rates to new data and found that there was a big surge. In 2018, pre-pandemic times, 11% of the population reported not having enough to eat or not knowing where their next meal would come from. In April 2020, that number jumped to 23%. BHS also present estimates based on the Census Household Pulse Survey on household food bank usage, which showed an increase of almost one percentage point between 2018 compared to May 2020. While there are some questions on the comparability to pre- pandemic measures, it serves as a starting point for talking about the challenges in feeding everybody.

Though the pandemic was a global crisis that took the world by storm, many people rely on emergency food providers during more frequent shocks, such as natural disasters, and even on the day-to-day. Proprietary research from Feeding America, which is the largest network of food banks in the United States, estimates that one-third of people who face food insecurity are not eligible for SNAP or other federal food programs, like WIC. While SNAP has requirements for citizenship status, income thresholds and other eligibility rules, even people who meet the stringent requirements still have to deal with the bureaucratic process and delay in getting their benefits. SNAP is not designed to address immediate food needs especially when people face volatile situations. Also, SNAP does not address situations where people are in food deserts and do not have access to nutritious meals.

This is where emergency food providers come in. They are often embedded in the communities they serve and can provide immediate support to families in need. Another important distinction in the role of food banks is that the scope of who they serve is often broader than federal programs when it comes to citizenship and income requirements. Even though food providers play an important role in addressing food needs, they also heavily rely on donations and volunteers for support and may not always have the capacity to help everyone in need. Estimates from BHS based on the CPS Food Security Supplement indicate that in 2018 about two percent of households received food from an emergency food provider in the last year. Comparing that with food insecurity figures from Feeding America, which estimated about 3.5% of the population faced food insecurity but were above the income thresholds to qualify for SNAP and similar programs, suggests that 5 million in need people did not have access to aid. These numbers were exacerbated in recent years. Given the limited resources emergency food providers receive and their potential to help close food insecurity gaps in the short run, it is important to understand how government funding may affect their overall impact.

The Emergency Food Assistance Program

The Emergency Food Assistance Program (TEFAP) provides the basis for the instrument used in my estimation strategy. The TEFAP program is the largest federal program to fund emergency food providers and supplies a sizable percentage of their food commodities and total government funding. Funding from the program is set by a formula, which provides an exogenous stream of government funding to test causal effects.

TEFAP was established in the 1980s as part of an effort to redistribute surplus commodities from American farms to communities across the country. What was once meant to be a temporary program became a flagship program under the U.S. Department of Agriculture (USDA) that continues to receive annual federal funding through the Farm Bill. In practice, there is a total annual budget allocated to the program at the national level predetermined every year, referred to as entitlement funds. This program allocates funding to states using a weighted measure of their relative poverty and unemployment levels. The formula is:

$$A_{st} = TEFAP_t \times \left(0.6 \frac{poverty_{st-2}}{\sum_r poverty_{rt-2}} + 0.4 \frac{unemp_{st-1}}{\sum_r unemp_{rt-1}} \right) \tag{1.3}$$

A is the amount state s receives in fiscal year t. $TEFAP$ is the total amount allocated to entitlement funds in the fiscal year at the national level. Inside the parentheses we have weighted measure of poverty and unemployment levels. Where *poverty* is the number of persons living in poverty in state i, divided by the total number of persons living in poverty across the country, including state s. Due to the census data release schedules, this measure is taken from the ACS 1-year estimates of the calendar year in t-2. Similarly, *unemp* is the number of persons who are unemployed in state s. This term, similarly to poverty, forms a ratio to gauge state unemployment relative to the number of persons unemployed in the whole country. This measure is also delayed since it takes the average of the monthly unemployment levels of the 10-month period of October through July preceding the fiscal year. This means that high unemployment, high poverty states receive more funding, but funding from this year may not reflect current conditions in the state. The per capita entitlement funds states received in 2018 are shown in Figure 1.3.

Within the program framework, the USDA functions as a grocery store to states and TEFAP funds as a food voucher. The USDA works with food suppliers to price available food inventory, which states then order directly from the USDA. Most states work with a state agency or large food bank to figure out the commodities that will meet food demand in their communities. Methods of distribution vary by state, with some letting counties place their own orders and others ordering for the state and using a couple big subcontractors to distribute food across the state. A flowchart depicting the flow of TEFAP funding through the agencies can be seen in Figure 1.4. Regardless of distribution method, for which states also receive funding in the form of administrative-specific dollars, the food purchased through TEFAP is then distributed to low-income households at food banks, soup kitchens, food pantries, and other similar organizations, what is referred to as an emergency food provider.

Commodities from TEFAP make up a significant portion of the food emergency food providers distribute. Figures A5 and A6 show extracts from annual reports of two organizations. In these examples, TEFAP accounts for twenty to forty percent of their food. A brief review of other emergency food providers of different sizes and in different states showed a similar range. While this level of precision in data is not widely available for all organizations that receive TEFAP, Feeding America estimates that a third of the meals provided by emergency food providers in their network are funded through TEFAP (Feeding America).

In terms of total government funding, TEFAP is very important to emergency food providers. Table A7 in Appendix A shows a detailed budget that compares TEFAP funding to other government funding for a food bank in central California. In this case, TEFAP accounts for nearly 75% of total government funding. A national survey of states from 2015 indicated that only 13 of 46 states used state funds to supplement TEFAP funding and support emergency food providers. Four out of the thirteen only provide limited administrative support statewide.

Throughout the decades the program has continued to receive support. While funding often increases and provides additional support to communities in the form of Bonus Commodities in times of need (e.g.the 2008 Financial Crisis, the COVID-19 pandemic, etc.), the guaranteed piece of entitlement commodities has been virtually stable and a source of funding local emergency food providers can rely on. This is evident from Figure 1.5, which breaks down the total funding over the years. A key distinction of the entitlement funds is that they include administrative funds to help in the transportation, storage and distribution of commodities. Administrative funds are not proportional to bonus commodities, disaster funds, or trade mitigation commodities. For the purpose of this paper, only entitlement funds, which are allocated on a formula-basis, are considered in order to separate the exogenous funding from confounding economic phenomena.

1.4 Data

I use nonprofit tax data to calculate levels of government and private funding to emergency food providers. These data contain all financial information reported to the IRS by each organization. These data are aggregated and combined with institutional and demographic data from the U.S. Department of Agriculture, Bureau of Labor Statistics, and Census Bureau, which are used to construct the instrument. All data are combined into a balanced panel at the state-year level used to implement the research design across states and years.

Organization Data

The primary data to identify the sample of charitable organizations in this analysis comes from a recent release of electronic tax filers. I combine data from the IRS and the National Center for Charitable Statistics (NCCS) to identify the sample of emergency food providers to extract variables needed to calculate government and private funding.

4.1.1 IRS Data

In 2016, the IRS made all nonprofit tax forms, form 990, filed electronically since 2011 available to the public and has continued releasing new filings. Prior to this release, data from accessible 990 forms was limited to high-level extracts and a small sample of full 990 forms from the late 1990's and early 2000's. Though the data set is restricted to electronic filers, the sample is large and representative of the sector. This highly detailed data set allows emergency food providers to be clearly identified along with their sources of funding. Due to data availability, I restrict the analysis to organizations who filed their 990 form between 2011 and 2018.

Tax-exempt organizations must register with the IRS, with the exception of churches and very small organizations. Since 2010, organizations with more than \$50,000 in gross receipts have been required to file a 990 short form. And organizations with over \$200,000 in gross receipts must file the 990 long form, which contains much more detailed information on their financial status. The long form requires organizations to breakout different types of contributions (see A1 for full breakdown), which allows me to differentiate between government funding and private funding. Other important financial elements in the 990 long form are sources of revenue and expenses. My sample is restricted to those larger organizations who file the 990 long form.

While the data set only includes electronic filers, emergency food providers are well represented. Take-up for electronic filing was said to represent over 60% of all filings in 2016 IRS (2016) and likely increased over the years. However, emergency food providers in the sample for this analysis have had better representation since the inception of this platform. Figure 1.6 shows the ratio of e-filers to registered nonprofits required to file a 990 tax form across the categories and years relevant to this analysis. We see that e-filers classified under the Food and Nutrition umbrella include up to 90% of all organizations within that category required to file in that year.

The sample of emergency food providers is deduced from a larger sample of organizations classified under Food & Nutrition, Housing, and Religious categories. For housing organizations, I only keep organizations that are temporary housing or homeless shelters in an attempt to identify soup kitchens and similar organizations. The religious organizations are included to make sure religious food pantries are present in the sample. Then, I extrapolate the mission statements from the remaining housing and religious organizations and keep those that contain key terms related to food. Figure 1.7 contains the list of key terms and some examples of mission statements, whice come directly from each organization's 990 form. I further exclude organizations whose primary purpose is related to research, fundraising, or technical assistance. Finally, I exclude Feeding America, which is estimated to be the second largest charitable organization in the U.S., in order to not inflate numbers from Chicago, where it is headquartered. The goal is to retain only organizations that would participate in food distribution. The final sample contains almost 7,500 organizations. Table 1.3 contains summary statistics of all the organizations in the sample.

Another important feature of the IRS data is that it contains information about each organization's fiscal year. I use this to standardize organizations' fiscal year to match up with the fiscal year that TEFAP operates on in order to construct a panel where each fiscal year is clearly defined. Organizations may set their own fiscal year and are required to file within five months after the end of the 12-month period. Tax forms are categorized by the year in which each organization's fiscal year begins. This means an organization with a fiscal year between January 2018 through December 2018 would file the same 2018 990 form as an organization with a fiscal year from November 2018 through October 2019. These periods are very different, but are often both lumped into the 2018 tax year. Standardizing these periods is important when considering the effects of government government funding, which follows its own fiscal year, October through September.

4.1.2 NCCS Data

The National Center for Charitable Statistics houses limited financial tax data on all registered and active charitable organizations. I use these data and their classification system to identify emergency food providers in the electronic filers IRS data set.

Another key source of data is the National Center of Charitable Statistics (NCCS) hosted through the Urban Institute. Previous related research on charitable organizations almost always uses data from the NCCS. However, for most years of data within the NCCS data sets, specific sources of revenue and expenses are excluded and only aggregates are reported. The data that include all the variables in the 990 forms are limited to the period of 1998 through 2003. Few other sources have digitized small samples of organizations' 990 forms, but these are too limited for the purpose of this analysis.

The NCCS has developed a classification system called the National Taxonomy Exempt Entities (NTEE) Codes using the NAICS codes that the IRS assigns each registered organization based on the primary purpose they indicate at the time of registration. The NTEE system is widely used in this literature as it provides a dependable and easy way to categorize organizations. I use NCCS data on registered nonprofits to map the NTEE codes and extract Food and Nutrition, Housing, and Religious organizations whose primary role is as emergency food providers.

NCCS data also provides a bank of all organizations that have filed a 990 both electronically and on paper. Though some key financial variables, like government funding, are missing from this data set, they report other organization characteristics This helps identify the richness of the e-filer sample by comparing it to organizations that are left out. Table 1.4 presents the results from this analysis. There are no significant differences between organizations who filed tax forms electronically or in paper for the categories in the sample.

Supplemental Data

The instrument and empirical strategy are developed with data from the USDA, BLS, and Census. I extract data from the USDA's Food and Nutrition Services congressional budget reports to obtain total TEFAP allocation per fiscal year. The totals reported in these reports accurately reflect the totals spent rather than the initial allocation. They are also adjusted for inflation from their initial congressional allocation in the Farm Bill. I also obtain both poverty rates and levels from the 1-year ACS for the state analysis and from the Small Area

Income and Poverty Estimates (SAIPE) for the county analysis. Unemployment rates and levels come from The Local Area Unemployment Statistics (LAUS) under the BLS.

1.5 Estimation Strategy

A common threat to identification in this literature is the confounding relationship between government funding and private funding. Events such as natural disasters or financial crises could impact both how much government gives as well as how much donors give, in either direction. There may be other instances where government funding serves as a signal to private funders when they evaluate how much and to which organizations they give. The opposite may be true if certain government grants depend on how much a organization receives from the private sector. I use an instrumental variable approach exploiting exogenously assigned government funding from the TEFAP program to estimate causal effects and address issues of endogeneity.

IV Specification (2SLS)

I estimate the effect of government funding on private funding of emergency food providers based on the following framework:

$$\log Y_{st} = \beta_0 + \beta_1 \log G_{st} + \pi P_{st} + \nu U_{st} + \delta_s + \gamma_t + \epsilon_{st} \tag{1.4}$$

Here G is the total level of government contributions indicated in the 990 forms for a state in fiscal year t. The outcome variable of interest, Y for the main specification is the private funding. In the 990 form, this is the aggregate of all contributions except government funding (see Figure A1 in Appendix A for an example of the 990 form). The remaining types of contributions come from both grants and donations. Importantly, P is the poverty rate of state s in fiscal year t and similarity U is the unemployment rate. Finalle, δ and γ are state and year fixed effects, respectively.

I include poverty rate and unemployment rate as control variables for a couple of reasons. These variables counteract the argument that the TEFAP formula may be driven by the poverty and unemployment situations that will encourage or discourage private donations. This is revisited in more detail while assessing the instrument's validity in the following section. On their own, these are still important controls because they paint a picture of the current economic situation of an area aside from state fixed effects. The values are in logs for easy comparison of results because there is a variation in how much funding a state receives as shown in figure 1.3.

The first-stage regression, which identifies government funding G is

$$\log G_{st} = \alpha_0 + \alpha_1 \log A_{st} + \mu P_{st} + \phi U_{st} + \delta_s + \gamma_t + \rho_{st} \tag{1.5}$$

A is the instrument, which is described in more detailed in the following section, based on TEFAP funding.

Instrument Construction

The instrument replicates formula 1.3, which is used by the USDA to assign TEFAP funding to each state. The total funding is specified in the U.S. Farm Bill, up to five years in advance, and the formula distribution relies on lagged levels of poverty and unemployment levels. The exogeneity of these determining factors make this type of government funding an ideal instrument.

Figure 1.8 compares the actual state funding amounts that states received in 2019 to those I predict using the formula. There is slight variation because states are able to roll unused funding from the previous year. Also, any unemployment or poverty levels that are revised may differ from the figures used at the time. And finally, I was not able to find reliable data for monthly unemployment and annual poverty levels for Guam, the US Virgin Islands, and the Northern Mariana Islands, thus the total national levels in the denominators for the variables *unemp* and *poverty* are smaller than what is used in practice. Given the small populations of these territories, these changes do not affect the predicted amounts significantly.

The first consideration for the validity of $TEFAP$ as an instrument is its direct effect on government funding. Given the evidence presented in the empirical context and data sections, the function of TEFAP is clearly defined as a direct source of government in the context of emergency food providers. Panels B and C of Table 1.3 further compares TEFAP funding to reported government funding in the sample data. On average, TEFAP entitlement funds account for 23% of total government grants in a given year and state for all organizations in sample, and 48% for food related organizations. Differences in these measures can be attributed to the inclusion of some religious and housing organizations that may be eligible for additional government funding, such as Housing and Urban Development grants and community block grants, that do not serve their food programs.

The more contentious assumption is the exclusion restriction. Given the allocation formula, which is based on state poverty and unemployment levels, we may be inclined to think that these would also affect private funding. In times where poverty and unemployment is rampant in a community, philanthropists may be more inclined to donate or even resistant due to their own possible hardship. However, the TEFAP formula uses lagged poverty and unemployment shares, which may not accurately describe the unemployment and poverty situation of a state in present times. They are also values that are relative to the country's situation. Thus a small state may have a large unemployment rate, but receive little funding if they don't have a large share of persons unemployed relative to other large states. Regardless, I include current poverty and unemployment rates as control variables in order to appease any remaining concerns about the correlation of the instrument with other determinants of the outcome variable.

1.6 State-Level Analysis

I aggregate emergency food provider data to the state level by fiscal year to estimate the two-stage least-squares framework described in the previous section. I present results of various subsets of the organization data for completeness. My results show government funding crowds-in private funding at the state level by about one percent at the state level.

I first estimate the OLS version of the model, with private funding as the dependent variable and reported government funding as the independent variable. The results are in Table 1.5. I break out the results using the full sample of emergency food providers described in the data section (column 1), which results in 27,899 observations, only organizations from the sample classified under the Food NTEE category (column 2), and organizations from the sample that are in the data set for at least six out of the eight years (column 3). The purpose of the last breakout, what I refer to as the panel organizations in the results, is to capture extensive versus intensive results. By eliminating new organizations in and out of the sample we can evaluate whether effects are due to organizational growth or changes in the composition of the sample. The OLS results all show a positive correlation between government funding and private funding.

First-Stage

I estimate the two-stage least-squares framework for the same three partitions of emergency food providers data. The first-stage results in Table B3 show a strong relationship between TEFAP funding and total government funding reported in the tax data in all specifications. For the full sample, we see that a one percent increase in TEFAP funding is correlated with an 1.5% increase in reported government funding. This may be due to the correlation of TEFAP entitlement funds with TEFAP bonus funds or other similar programs, like the Commodity Supplemental Food Program, or even grants from state governments that may use similar criteria as TEFAP to distribute funding.

Effect on Private Funding

The second stage results in the bottom panel of Table B3 indicates that government funding crowds-in private funding at the state level. All partitions of the data suggest that a one percent increase in government funding increases private funding by about one percent. The result for the panel organization sample indicates that this increase is at the intensive margin, meaning the crowding-in is driven by existing organizations rather than an increase in filings by additional emergency food providers. In Table 1.9, I compare the IV results to OLS results for the full sample of organizations, and separately estimate effects on the two types of private funding, donations and grants. This holds true for both grants and donations coming from private supporters. Donations include contributions raised during fundraising events, membership dues, and an other category. Grants include contributions from related organizations and federated campaigns. Results for donations and grants are to be interpreted with the caveat that the subcategories are assumed to be understood by the tax filers, though it is common for contributions to be erroneously added to the "Other" category.

Taking Stock

The number of observations in the state-level sample is limited by the years of data available. Though the results are statistically significant, it's important to consider whether they are robust. In the following section, I replicate the analysis at the county-level in order to compare the state-level results.

1.7 County-Level Analysis

Given the limited number of observations in the state-level analysis, I validate the model and results by applying the same empirical strategy at the county level, where the instrument maintains exogeneity. The county-level analysis provides a source of comparison and a more comprehensive dataset. Results from this analysis support findings from the state-level analysis.

Background

Sixteen states replicate the federal formula to distribute TEFAP funding to their counties. I replicate the state-wide analysis at the county level for these sixteen states in order to exploit a more granular and comprehensive level of data. Figure 1.9 maps out the different methods of allocation each states uses to disburse TEFAP funding and commodities.

States who replicate the federal formula, known as the fair-share formula, encompass a wide geographical and demographic variety. Other methods of allocation include a focus on SNAP participation or poverty rates. Because these methods are not clearly exogenous, as in the case with the fair-share formula, these are excluded from the county analysis. I observe a total or 1,280 counties, which account for just over 50% of the total U.S. population, over the eight year analysis period. The instrument is constructed as in the same manner described in section 5.2, though I use poverty estimates from the SAIPE, rather than ACS 1-year estimates, to account for counties with small populations not included in the ACS data. As in the state-level case, the poverty measures are lagged by two years and thus are different than contemporaneous market conditions.

Results

The county-level analysis also shows that government funding crowds in private funding, which is consistent with state-level results. Table 1.8 breaks down the first and second stage results, which use the full sample of organizations. We see a strong first stage indicating a positive correlation between TEFAP and reported government funding. The second stage shows that a one percent increase in government funding crowds in private funding by 1.6%. As with the previous section, I also provide a comparison between the sources of private funding, grants and donations. These are compared to OLS results of the same specification in Table **??**. Crowd-in results seem to be driven by donations over grants, though I reiterate the caveat that these results may be noisy due to misinterpretation of categories in the tax forms, especially at the county level.

1.8 Reconciliation with theory and empirical work

Given my results contradict the findings of the canonical theoretical model, which predicts crowding out, I check my results using other instruments, adapted from previous empirical work, and test my model on the arts and humanities nonprofit organization as a placebo test for the TEFAP instrument.

Theory

The canonical model hinges on the assumptions that government and private funding are substitutes and that government funding to charitable organizations is funded through lump-sum taxation. Using the framework introduced by KW, we can rationalize that while the canonical model assumptions may be true in some settings, including with emergency food providers, the government and private funding may actually be complements and government funding is exogenous from the donor's perspective.

Referring back to Table 1.1 where KW breakdown the possible outcomes in terms of crowding, we can separate sources of variation by analyzing first whether government funding and private funding are net substitutes. This would be the case when a donor does not have a preference over whether funding to charitable organizations comes from government funding or their own donations. In this scenario, there is a level of total funding to the public good provided by an organization that maximizes the donor's utility function and the source of the funding is not relevant. This is a corner solution, where there is pure altruism, though we can imagine the scenario extending to the interior solutions, where the donors receive an added warm glow benefit from donating, but in net terms, private and public funding are still substitutes. We can think of scenarios where an organization receiving funding brings utility to a donor because they obtain services from that organization, such as a museum or a hospital.

However, when it comes to emergency food providers, and similar charitable organizations, people receiving services are rarely the ones donating. Given the progressive income tax model in the United States, they are also the ones paying least, in absolute terms, toward a government grant via their taxes. Therefore, the utility a donor obtains from donations is mostly a warm glow, whether that comes from feeling good about helping people in need or even improving their social standing. When utility from warm glow supersedes altruistic motives, as is possibly the case here, private and government funding may be complements. With EFPs, a donor with the purpose of helping a community will be incentivized to continue donating, unaffected by government funding, because the act alone brings utility. Another way to look at it is that additional exogenous funding, may mean organizational growth and visibility, increasing utility for the donor concerned with societal standing.

The other piece of the KW setup and allowing crowd-in relies on evaluating whether the system is funded or unfunded. A funded system, and what is described in the canonical theory, is one where government funding to charitable organizations is financed by a lump-sum taxation to taxpayers. In our case, there is exogenous funding applied to nonprofits that is not obtained through lump-sum taxation. Though the federal budget depends on taxpayer dollars, these are not in the form of a lump-sum tax. These grants may be partially funded by income taxes, but taxpayers do not designate where their tax dollars are spent in the U.S. From the perspective of the taxpayer, the system is unfunded.

Test of other instruments

I retest my model using three instruments that have been inspired by previous empirical work. My results are consistent when using two instruments related to congress members in the federal budget appropriation committee. I found no significant results using an instrument related to university research funding.

In order to relate the instrument used in this analysis to previous empirical work, I use instruments inspired by two Andreoni and Payne papers (2003, 2011) on estimating crowding out. Two of the instruments are based on congressional membership in the Committees on Appropriations, which enact bills that determine how federal dollars are spent. I aggregate organizational data to the county level and define the first instrument as an indicator of whether a member of congress representing an area, either a senator or house representative, was part of the committee in a given year. The hypothesis is that as areas with representation in the appropriations committee would be more likely to receive federal funding, that would trickle down to charitable organizations. I also use the number of years served by the congress member as an instrument, taking the maximum tenure when there is overlap. The third instrument is the research universities receive from the NIH in a given year. Funding is tallied at the state level. The hypothesis for this instrument is that organizations in states with higher research funding would likely also receive higher funding for charitable organizations as it would indicate public support.

Results from these specifications are in Table 1.10. The first stage for all three of the instruments is not particularly strong, the F-statistic is similar to what was found in the AP papers. The results from the second stage are similar in magnitude to the results from my main specification. It is also worth noting that these instruments performed better in their original papers when analyzing Arts & Humanities organizations versus Human Services organizations. These tests highlight the strength of TEFAP as an instrument for the emergency food providers organizations.

Placebo on Instrument

I conduct another validity test using the TEFAP instrument on the sector of Arts Humanities organizations as a placebo. Due to data availability, the sample of organizations includes over 40,000 total unique organizations who filed electronically with the IRS between 2015 and 2019. The first and second stages of the regression analysis are in Table 1.11. There is no relationship between TEFAP funding and government funding to these types of organizations, as expected. I also compare these results to the original emergency food providers sample while restricting that to 2015-2019. The first stage results retain a strong relationship between TEFAP funding and government funding to emergency food providers. The final placebo test uses the organizations from the housing and religious organizations that we discarded from the original sample because they did not contain relevant keywords in their mission statements. Again, there is no relationship in the first stage. These results support the importance of TEFAP and its validity as an instrument specifically for emergency food providers.

1.9 Mechanisms for Crowd-In

In order to understand what drives the crowd-in results, I conduct an analysis using the same IV approach as above, but taking other financial outcomes as dependent variables. Table 1.12 presents findings for outcomes related to organization expenses. In particular, I find that an increase in government funding is associated with an increase in total fundraising expenditures. The intuition, in terms of crowd-in, is that organizations may dedicate more money to fundraising efforts if they obtain exogenous government funding. Another interesting finding is the increase in program expenses. If we assume that these are correlated with clients served, we can establish that government funding can increase the impact of emergency food providers.

I also present findings related to organization revenue in Table 1.13. Revenue, in this sense, is money obtained from programs, or the sale of services, as well as investment activities. This is split up into related and unrelated revenue to distinguish between activities that coincide with an organization's mission with those that do not. To illustrate this difference, we can think of an organization that hosts a farmers market where they sell food at discounted prices and also rents their warehouse as meeting space. Revenue from the farmers market would be related and revenue from the rents would be unrelated. Revenue excludes all money classified as contributions, which are composed of private funding (donations and grants) and government funding. Note that contributions include funds and commodities that pass through large food banks to smaller EFPs in the form of grants.

I show that government funding decreases unrelated revenue-producing activities. Here, the intuition is that government funding may allow organizations to shift their priorities and decrease their investment in non-mission related activities to focus on their mission of distributing food. While these may not be necessarily caused by the influx of private funding, they provide a basis for a discussion on the mechanisms for crowding in.

1.10 Conclusion

Given the recent Covid-19 health crisis, the role charitable organizations play in the social safety net has become even more important to policy discussion. This paper furthers the understanding we have of the relationship between public and private funding that these organizations, specifically emergency food providers, receive. The context of emergency food providers is valuable since it speaks directly to the organizations that fill gaps in the safety net and how government funding may help this type of organization grow and expand their reach.

In this paper I take advantage of underused tax microdata to identify the specific sources of funding that organizations in the emergency food providers sector receive. The data affords me the opportunity to delve deeper into a category of organizations that has not been thoroughly studied but has important implications for policy. I am also able to use a large federal program, TEFAP, to instrument for government funding in an instrumental variables framework and estimate causal effects. The TEFAP funding allocates annual funding to states based on lagged levels of poverty and unemployment relative to national values. Because these measures are lagged by one year in the unemployment case and two years in the poverty level case, I can control for contemporaneous poverty and unemployment rates at the state level to disentangle market conditions that drive both private and public funding from direct effects of government funding on private funding.

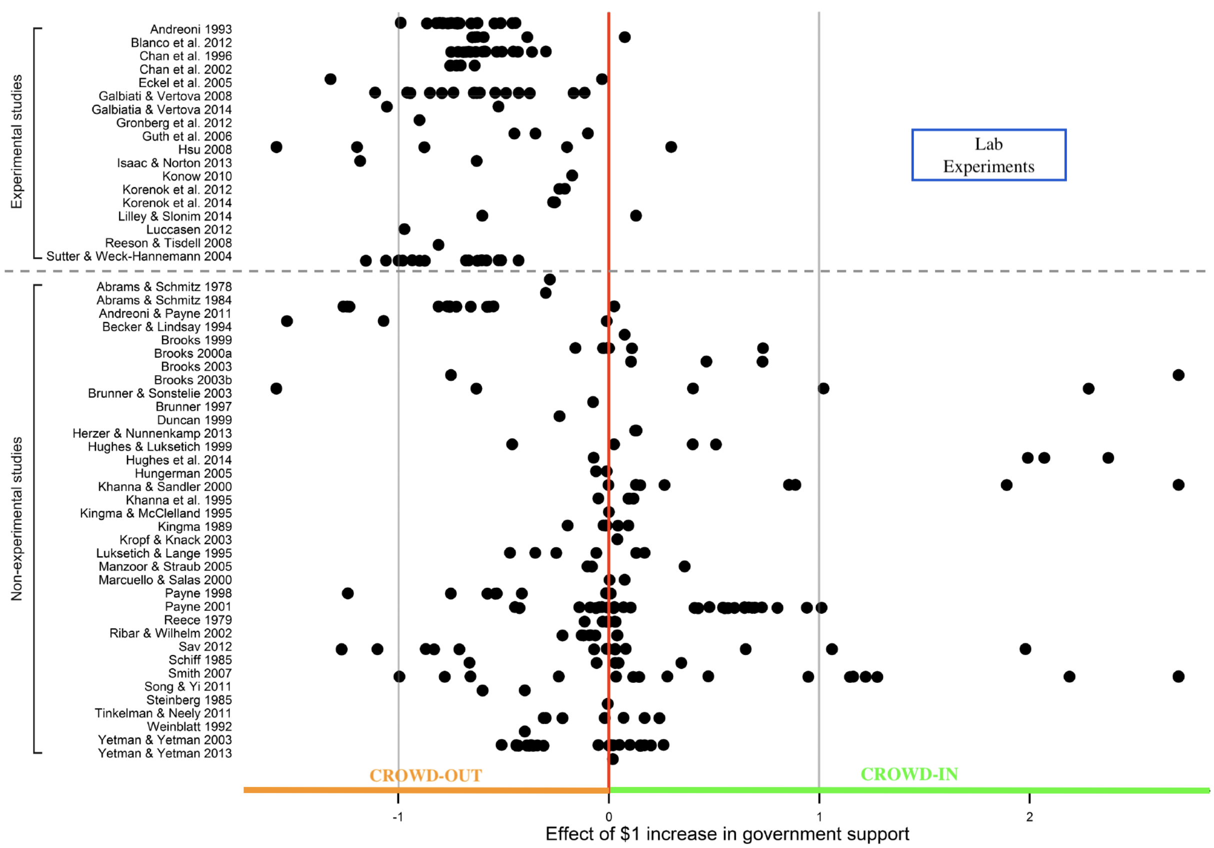
I find that a one percent increase in government funding crowds-in private funding by 1 to 1.6 percent. While crowd-in effects are contradictory to the canonical crowd-out theory, these results are robust to various specifications. The intuition for these results follows from exogenous government funding spurring fundraising efforts and a drive to shift priorities to grow areas of the organization that further their missions. I test and confirm this intuition by estimating the effects of government on fundraising expenses and streams of revenue from programming. I find that fundraising expenses have an elasticity of 0.33 and revenue from unrelated programming, that is revenue from activities not directly related to an organization's mission, have an inverse elasticity of 0.48.

A program in 2019 increased funding distributed through TEFAP channels by over 100%. This program was part of a trade mitigation program that was designed to distribute leftover commodities and compensate American farmers negatively affected by tax tariffs. This large increase in the funding brings into question the capacity of constraints of organizations and whether organizations would experience diminishing returns to additional government funding. Given how recently the program was implemented and the policy response to COVID-19, which also allocated additional funding to TEFAP, it is difficult to study the effects of this change at the time of this study. After some years have passed and 990 tax forms have been filed for the years in question, it would be interesting to explore how results may hold up.

Further research could also analyze the general equilibrium effects and explore whether an increase in private funding to emergency food providers is a result of an increase in philanthropic giving as a whole or if donors redistribute across categories of charitable organizations. Exploring this topic would help us understand whether increasing government funding can help other charitable organizations increase their scale and efficiency to solve social needs at a larger scale or whether the effects are restricted to emergency food providers and the food insecurity setting.

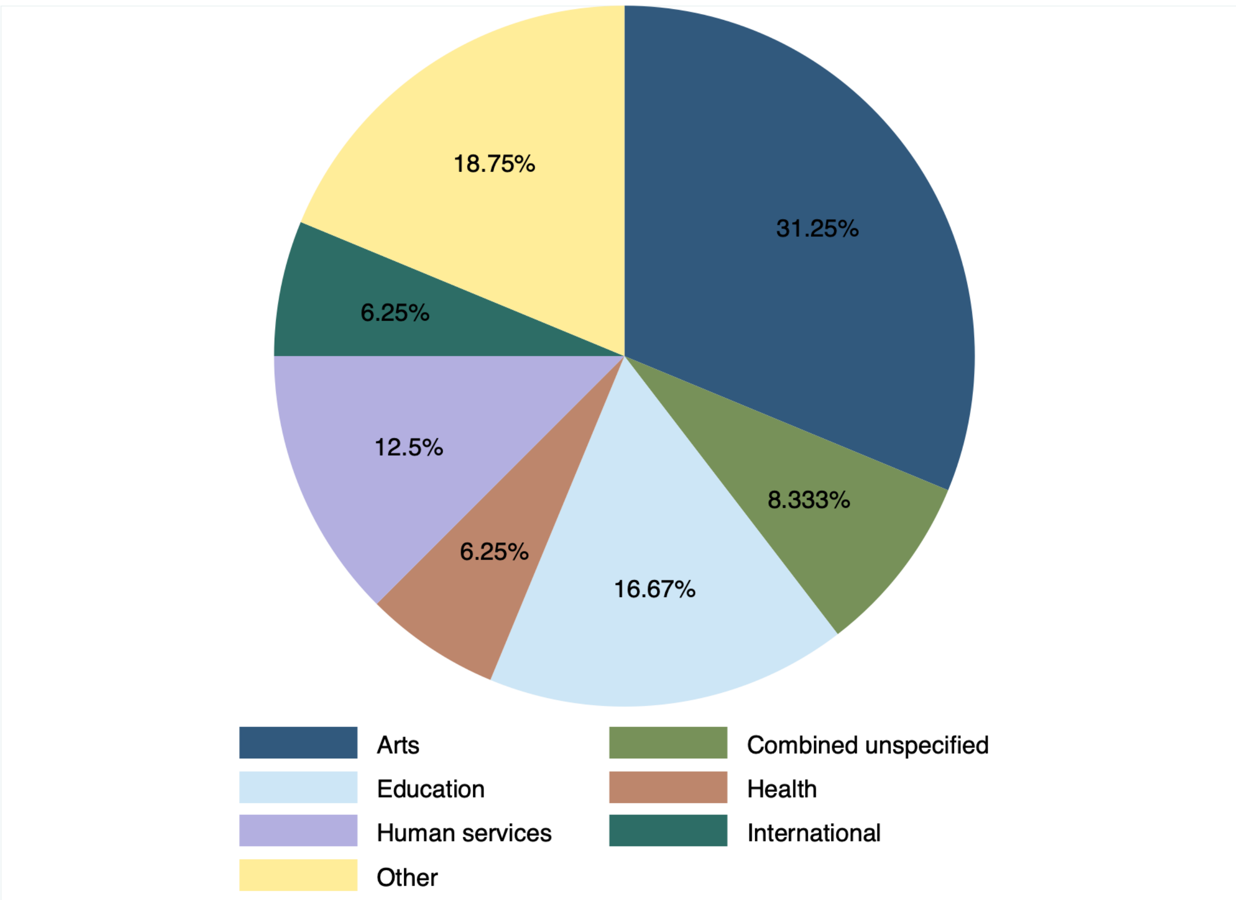
Figures

Figure 1.1: Empirical Estimates from Meta Analysis



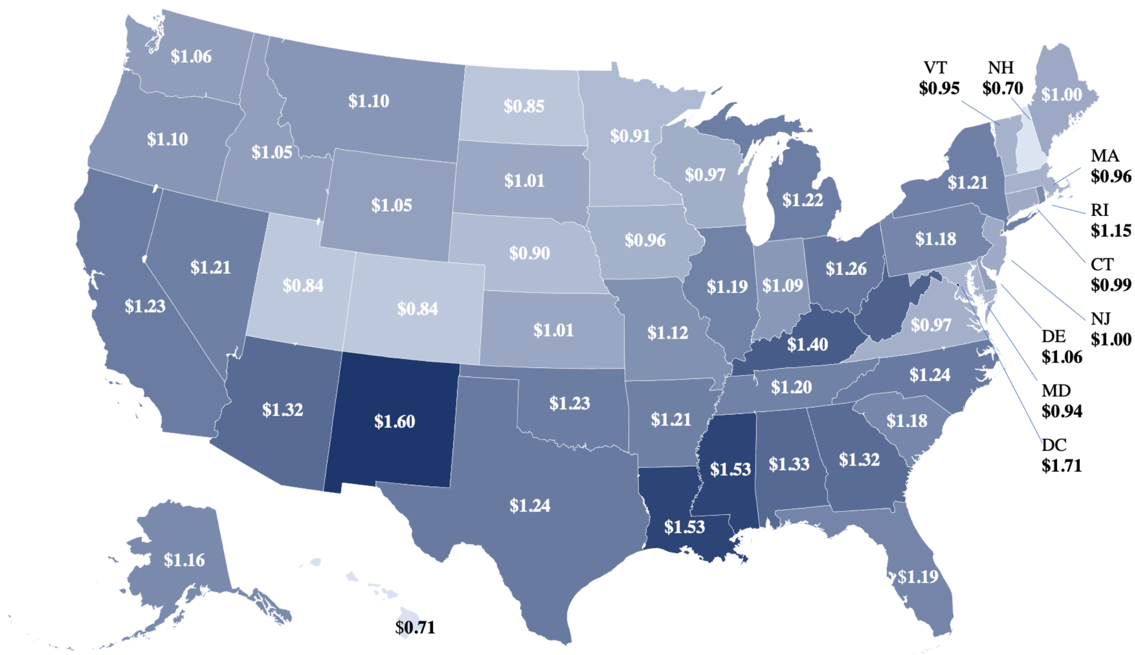
Notes: This figure is directly from De Wit & Bekkers (2017) which is a meta analysis of 70 empirical studies related to crowding theory and nonprofits. This chart shows the estimated correlation between a $1 increase in government funding and private funding. Assuming causality, results < 0 a imply crowding-out and > 0 imply crowding-in.

Figure 1.2: Organization Types in Empirical Work



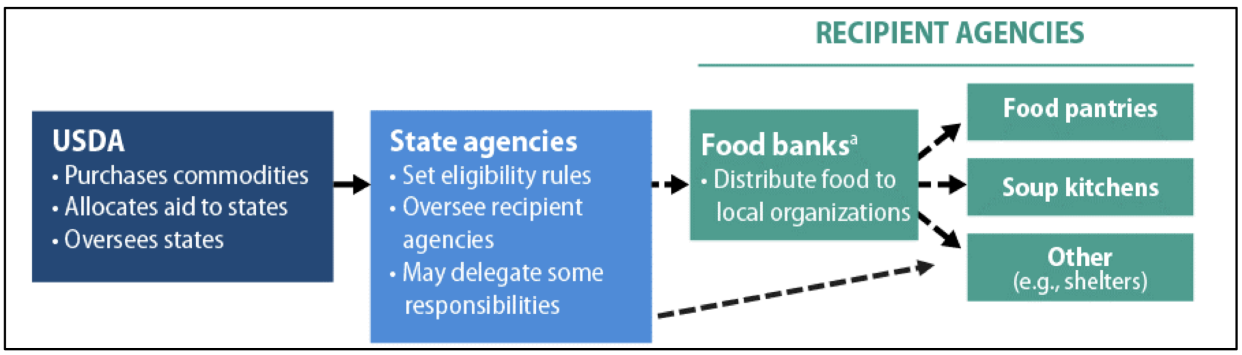
Notes: This chart compares the various categories of nonprofit organizations studied in previous crowding empirical papers (from De Wit & Bekkers (2017)). The total number of results included here is 48. The "Other" category combines nonprofit categories with only one result. The categories included are Crime, Animal Welfare, Food, Employment, Housing, Religious, Philanthropy, Environment, and Community Improvement.

Figure 1.3: Per Capita TEFAP Funding (2018 $)



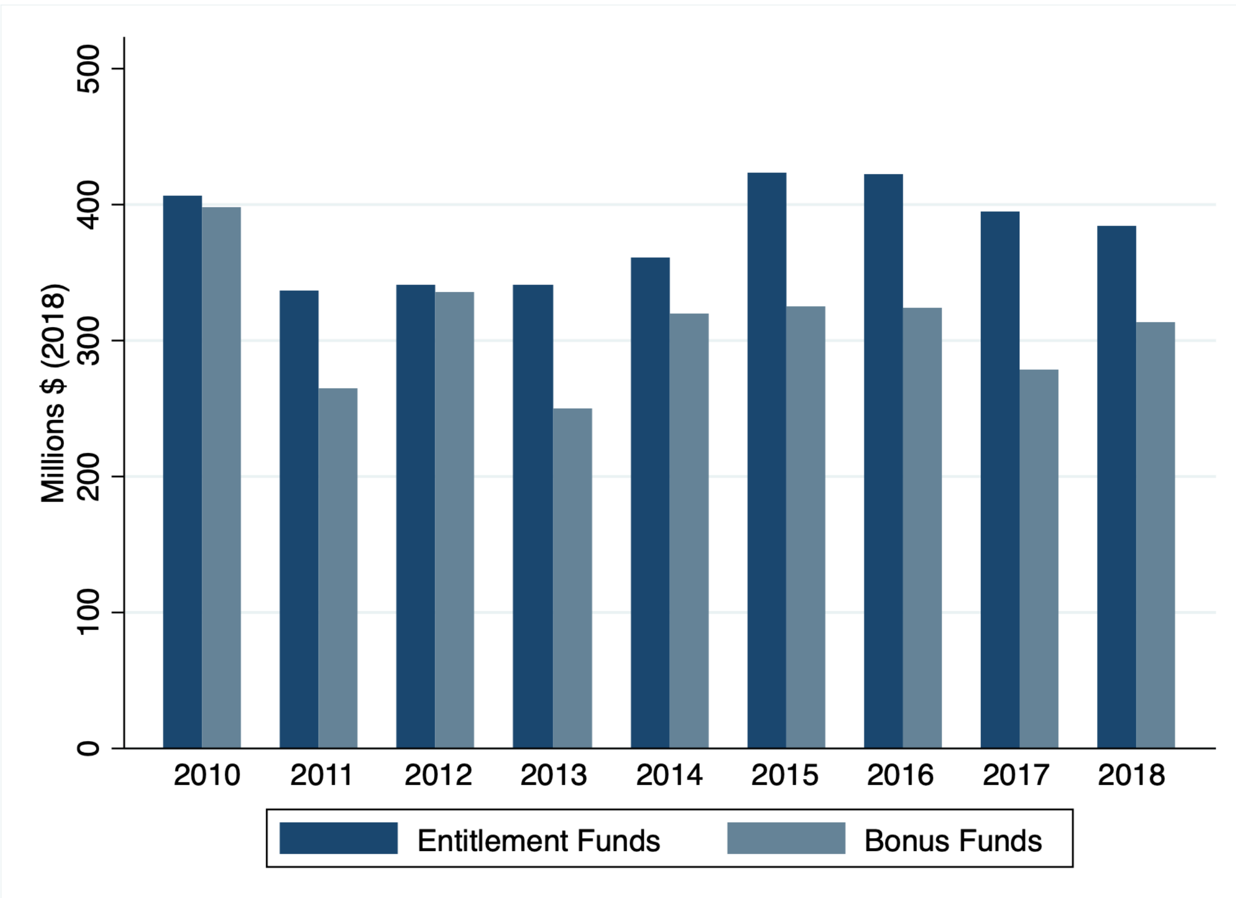
Notes: This map shows the per capita TEFAP funding each state received in 2018. These figures only include entitlement funding.

Figure 1.4: Flow of TEFAP Funding



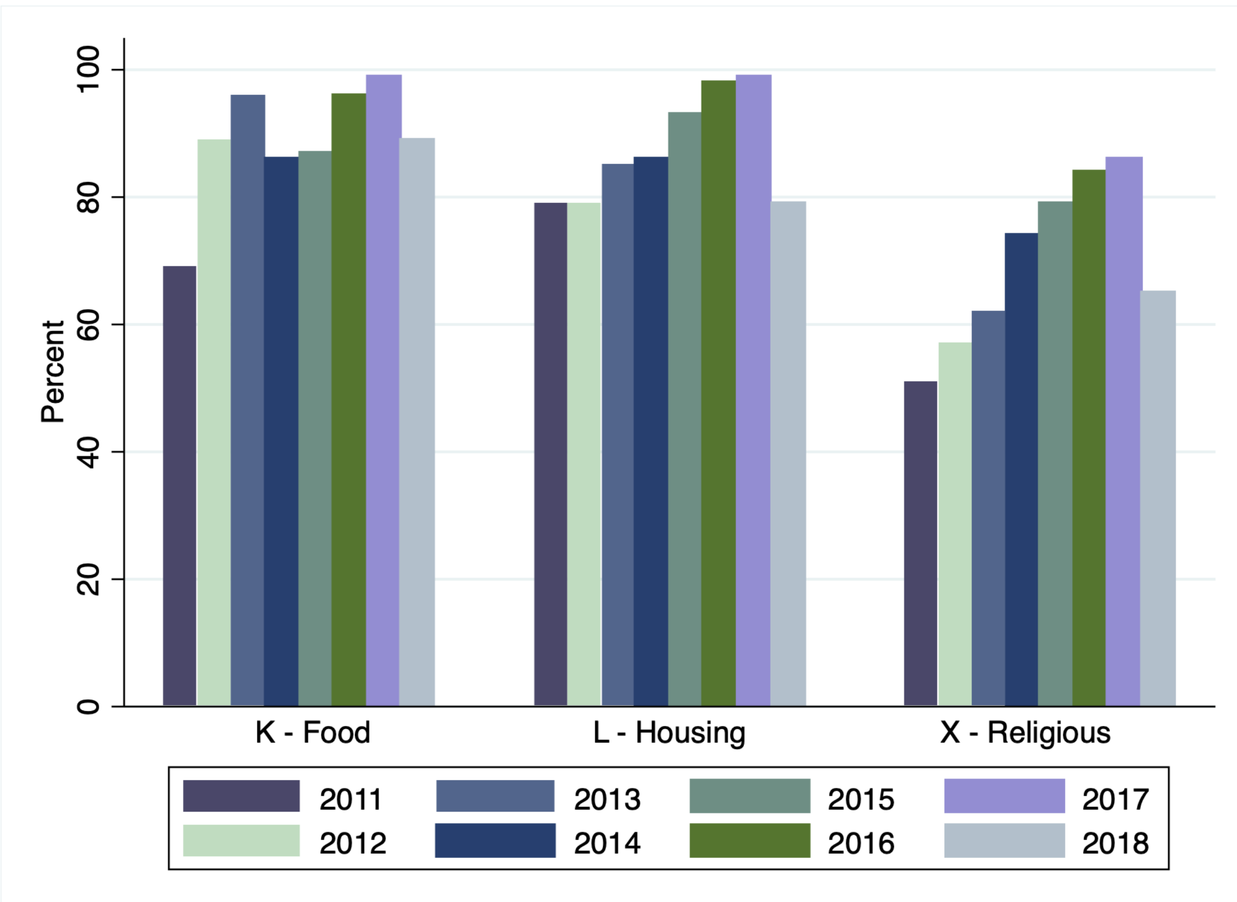
Notes: This flow chart comes from the 2022 Congressional Research Service Report on TEFAP and details how TEFAP funding moves from the USDA to emergency food providers.

Figure 1.5: Total TEFAP Funding



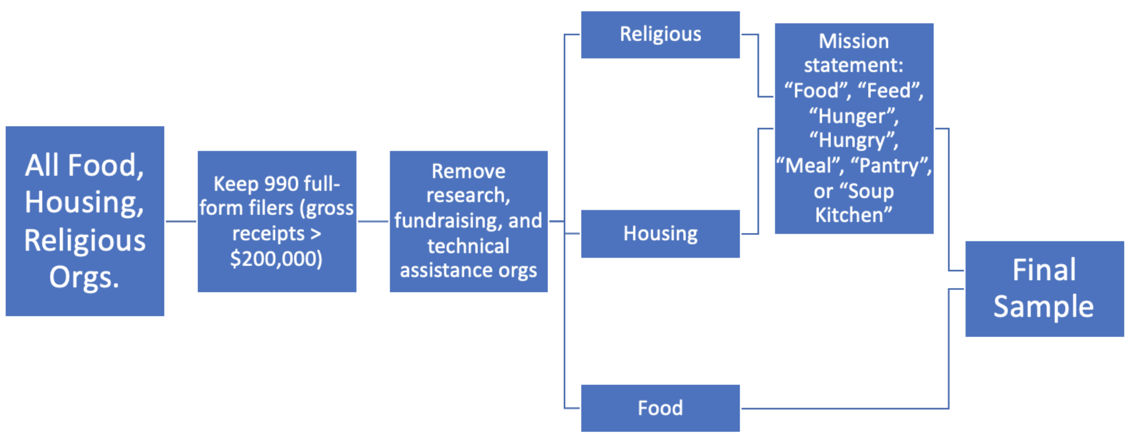
Notes: This figure plots total TEFAP funding for the country in each fiscal year included in the analysis. Entitlement funds, which are what this analysis uses, include the administrative funding and entitlement commodities that are designated by the U.S. Farm Bill. Bonus funds funds, which are excluded from the analysis, are discretionary funds that vary with agricultural surpluses and other economic phenomena.

Figure 1.6: Electronic Filers Representation



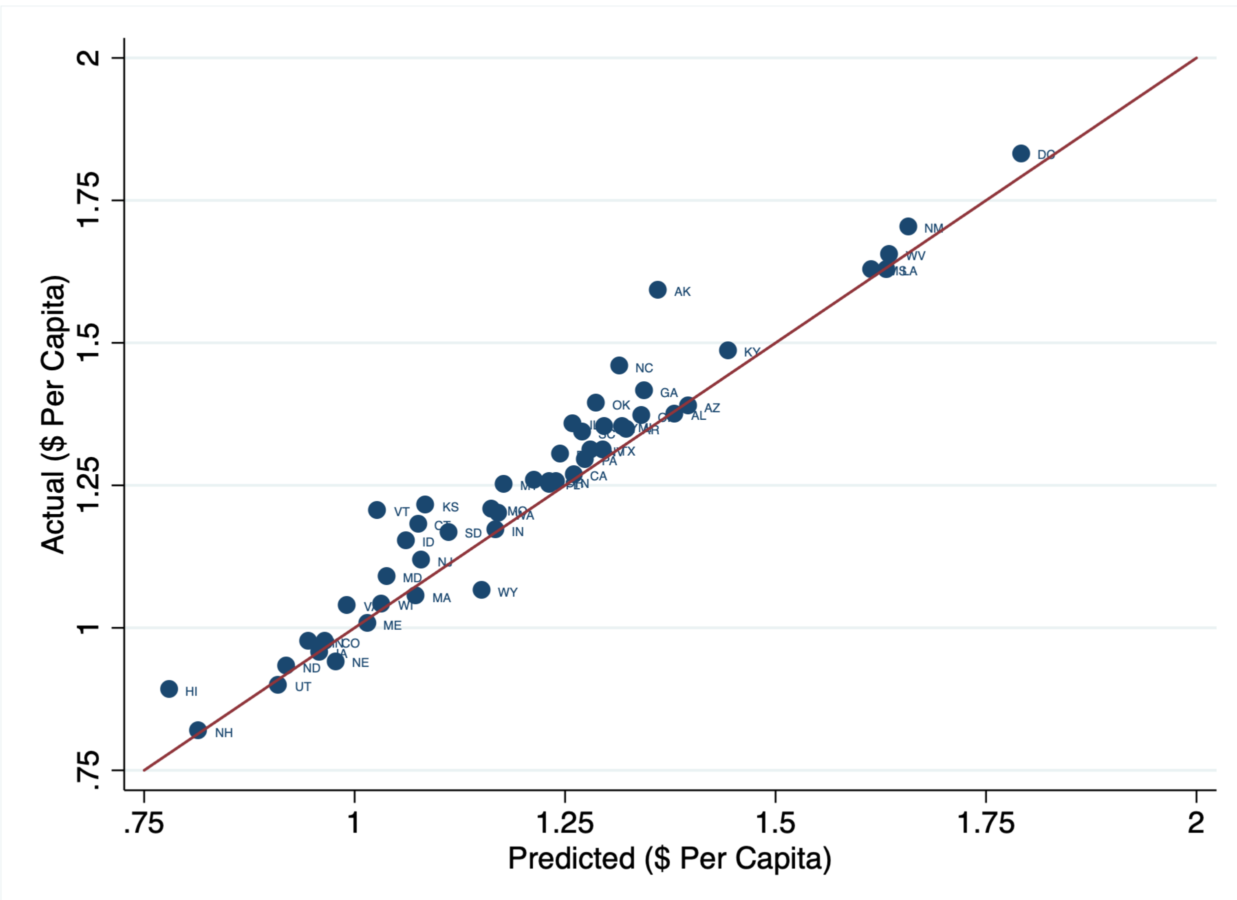
Notes: This chart plots the ratio of electronic filers to active organizations required to file in a given year. The data for the total organizations was obtained from the NCCS Business Master Files. For this analysis, I only have detailed microdata for the electronic filers.

Figure 1.7: Sample Construction



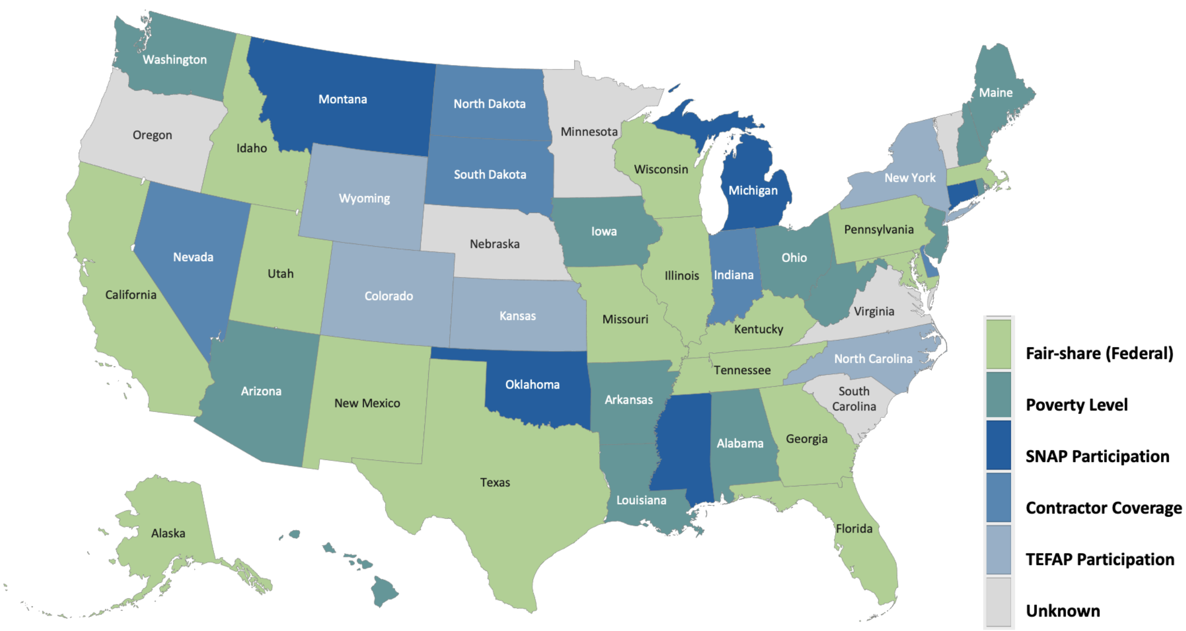
Notes: This flow chart summarizes the process for choosing the emergency food providers. The last step was to only retain organizations that mention one of the key words in the mission statement, which is stated in the electronic filers' tax forms.

Figure 1.8: Per Capita TEFAP State Allocation (2019 $)



Notes: The figure plots actual per capita TEFAP funding dollars states received in 2019 against the funding predicted by the allocation formula 1.3. The reference line is the 45 degree diagonal. The formula is a strong predictor of actual funding states receive.

Figure 1.9: County TEFAP Allocation Method



Notes: The map shows the different methods states use to disburse TEFAP funding to their populations. The Fair-share is the name of the federal formula used as the instrument in this analysis. The states in light green replicate this federal formula to allocate TEFAP funding at the county level. Sixteen total states use the Fair-share allocation method. Wisconsin makes a slight deviation by weighing poverty and unemployment levels equally rather than 60-40 percent. Some states use measures of "Poverty Level" and past "SNAP Participation" and "TEFAP Participation" relative to the state a whole to distribute TEFAP to their counties. The "Contractor Coverage" method identifies states who base TEFAP allocation on the contractors (or distributing organizations) that are available and the locations the serve; these tend to be states with few contractors.

Tables

Table 1.1: Crowding Predictions from M. Kotchen & Wagner (2019)

		Substitutes	Complements
Unfunded	$\frac{\partial g_i^*}{\partial G_{-i}}$	> -1	?
Funded	$\frac{\partial g_i^*}{\partial G_{-i}}\Big\|_{dG_{-i}=-dw_i}$	< 0	?

Notes: This table is a condensed version of Table 1 in M. Kotchen & Wagner (2019). The columns compare predictions of crowding when person i's personal contribution to a public good and public provision to a public good are substitutes versus complements. The rows compare the two systems of provision for G. The funded system refers to one dependent on lump-sum transfers, the unfunded system allows funding to be exogenous. A result under zero, as in the Funded-Substitutes square, means crowding-out is predicted by the model. The standard model assumes a Funded-Substitutes state. A positive result, which is an option given any other combination, would predict crowding-in. As discussed in the paper, in the context of EFPs, the system is unfunded and private and public provision may either be substitutes or complements.

Table 1.2: Mission Statement Examples

Name	Category	Mission
Seed Sowers Christians in Action	Housing	"Aid to homeless and low income individuals through a soup kitchen, food boxes, and a 12 bed male shelter..."
Our Brother's Keeper Shelter	Housing	"To provide shelter and a meal to those who are homeless in the Big Rapids Area"
Miami Rescue Mission	Housing	"We provide food, shelter, substance abuse treatment, educations, computer literacy, job placement, healthcare..."
Northwest Harvest	Religious	"...to provide nutritious food to hungry people in a manner that respects their dignity, while fighting to eliminate hunger"
Harvest Outreach Center	Religious	"Services to homeless individuals including temporary housing, food, medical care, job seeking assistance..."
Faith Outreach Ministry	Religious	"To distribute donated food, clothing, toys, etc to needy families"

Notes: This table shows some examples of mission statements from the Housing and Religious categories of organizations. The mission excerpts show how the systematic sample selection framework identifies emergency food providers not classified under the Food category.

Table 1.3: Summary Statistics (millions 2018 $)

Panel A: Organization-Level			
	N=Org-Year	Mean	SD
Gross Receipts	27,899	4.118	13.300
Total Contributions	27,899	2.814	11.100
Government Funding	27,899	0.527	2.397
Private Funding	27,899	2.287	9.984
Total Revenue	27,899	3.827	12.100
Total Expenses	27,899	3.693	11.900

Panel B: All Organizations State-Level			
	N=State-Year	Mean	Median
TEFAP/Government Funding	408	22.7%	15.6%

Panel C: Only Food Organizations State-Level			
	N=State-Year	Mean	Median
TEFAP/Government Funding	408	48.8%	26.2%

Notes: This table shows the averages of various financial variables across all organizations in the sample in panel A. Panel B and C collapse the data to the state-year level and compare the total TEFAP funding by state to government funding in that state within a fiscal year. The mean and median of those ratios are shown. Panel C uses only the food organizations in the sample.

Table 1.4: Comparison of Tax Filers

	E-Filers		Paper Filers			
	Mean	SD	Mean	SD	Diff.	p-value
Gross Receipts	1.80	0.03	1.89	0.08	-0.09	0.21
Total Contributions	0.85	0.02	0.91	0.04	-0.06	0.09
Total Revenue	1.51	0.02	1.53	0.04	-0.02	0.69
Total Expenses	1.37	0.02	1.39	0.04	-0.02	0.64
Observations	282,215		79,828			

Notes: This table shows the results of a t-test comparing means of electronic filers and paper filers from the NCCS data in millions of 2018 dollars. The sample is restricted to organizations in the fourth step of the sample selection process (see Figure 1.7). Due to data limitations, I cannot restrict paper filers to emergency food providers by their mission statement. A p-value greater than the critical value $\alpha = 0.05$, indicates we fail to reject the null hypothesis $H_0 : \text{Diff} = 0$. This suggests there are no significant differences between these groups in terms of means.

Table 1.5: OLS Results

	(1) Full Sample	(2) Food Orgs Only	(3) Panel Orgs Only
Govt. Funding	0.668***	0.776***	0.804***
	(0.0405)	(0.0506)	(0.0401)
Observations	408	408	408
Organizations x Year	27,899	13,331	11,909
R-squared	0.916	0.880	0.936
State FE	YES	YES	YES
Year FE	YES	YES	YES

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

Notes: Table shows results of estimating equation 1.4: $\log Y_{st} = \beta_0 + \beta_1 \log G_{st} + \pi P_{st} + \nu U_{st} + \delta_s + \gamma_t + \epsilon_{st}$. OLS results in column (1) use the full sample of food, housing, and religious organizations that serve as emergency food providers. Results in column (2) only uses food organizations and results in column (3) uses any organization that is in the sample at least 6 out the 8 years. Organizations x Year refers to the total number of observations that are collapsed to the state-year level for analysis. Errors are clustered at the state level. Unemployment rate and poverty rate at the state-year level are included as covariates.

Table 1.6: 2SLS Results: State-Level

	(1) Full Sample	(2) Food Orgs Only	(3) Panel Orgs Only
First Stage	1.515***	1.764***	1.260***
	(0.281)	(0.283)	(0.286)
F-test	29.01	38.80	19.46
Second Stage	1.123***	1.199***	1.002***
	(0.171)	(0.176)	(0.182)
Observations	408	408	408
Organizations x Year	27,899	13,331	11,909
State FE	YES	YES	YES
Year FE	YES	YES	YES

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

Notes: Table shows results for the first and second stages with private funding as the dependent variable and TEFAP funding as the instrument. Results in column (1) represent the full sample of food, housing, and religious organizations that serve as emergency food providers. Results in column (2) only uses food organizations and results in column (3) uses any organization that is in the sample at least 6 out the 8 years. Organizations x Year refers to the total number of observations that are collapsed to the state-year level for analysis. Errors are clustered at the state level. Unemployment rate and poverty rate at the state-year level are included as covariates. The results show a strong first stage. The second stage shows that a 1% increase in government funding crowds in private funding by over 1%.

Table 1.7: Private Funding Decomposition Results (State-Level)

	OLS			IV		
	(1) Total	(2) Donations	(3) Grants	(4) Total	(5) Donations	(6) Grants
Govt. Funding	0.668***	1.172***	0.325***	1.123***	2.843***	0.584***
	(0.0405)	(0.109)	(0.0483)	(0.171)	(0.512)	(0.182)
Observations	408	408	408	408	408	408
R-squared	0.916	0.716	0.662			
State FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

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

Notes: Table shows comparison of OLS and IV results. Total refers to total private funding, which is the sum of donations and grants. Donations are contributions directly from private people or corporations. Grants typically have restrictions or qualifications that are only available to particular organizations. Errors are clustered at the state level. Unemployment rate and poverty rate at the state-year level are included as covariates.

Table 1.8: 2SLS Results: County-Level

	State-Level	County-Level
First Stage	1.515***	1.585***
	(0.281)	(0.370)
F-test	29.01	18.34
Second Stage	1.123***	1.564***
	(0.171)	(0.378)
Observations	408	10,240
County FE	YES	YES
Year FE	YES	YES

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

Notes: Table shows results for the first and second stages with private funding as the dependent variable and TEFAP funding as the instrument for government funding. Errors are clustered at the county level. Unemployment rate and poverty rate at the county-year level are included as covariates.

Table 1.9: Private Funding Decomposition Results (County-Level)

	OLS			IV		
	(1) Total	(2) Donations	(3) Grants	(4) Total	(5) Donations	(6) Grants
Govt. Funding	0.686***	0.949***	0.0917***	1.564***	2.709***	0.210*
	(0.0144)	(0.0225)	(0.00504)	(0.378)	(0.645)	(0.115)
Observations	10,240	10,240	10,240	10,240	10,240	10,240
R-squared	0.915	0.725	0.523	-	-	-
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

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

Notes: Table shows comparison of OLS and IV results. Total refers to total private funding, which is the sum of donations and grants. Donations are contributions directly from private people or corporations. Grants typically have restrictions or qualifications that are only available to particular organizations. Errors are clustered at the county level. Unemployment rate and poverty rate at the county-year level are included as covariates.

Table 1.10: 2SLS Results of Additional Instruments

	(1) TEFAP	(2) Dem	(3) Rep	(4) Any Party	(5) Tenure	(6) NIH
First Stage	1.585***	-0.00777	0.272**	0.189*	0.0174	0.152
	(0.370)	(0.229)	(0.130)	(0.114)	(0.0220)	(0.182)
F-test	18.34	0.00115	4.388	2.725	0.628	0.694
Second Stage	1.564***	-26.33	1.121**	1.373*	2.963	0.757
	(0.378)	(791.6)	(0.503)	(0.740)	(3.300)	(1.068)
Observations	10,240	25,144	25,144	25,144	25,144	25,144
County FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

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

Notes: Table shows results for the first and second stages with private funding as the dependent variable and a different instrument per column. Column (1) is the baseline using TEFAP as the instrument. Columns (2) through (4) use indicator variables for the whether a representative from a district where a provider is located was in the U.S. committee of appropriations and evaluates the different parties separately. Column (5) uses the number of years a representative has sat on the committee. Column (6) uses total dollars at the state level of NIH funding that research universities received. All results are at the county-state year. Errors are clustered at the county level.

Table 1.11: 2SLS Results of Placebo Sectors

	(1) EFPs	(2) Arts & Humanities	(3) Housing & Religious
First Stage	1.585***	0.290	0.0382
	(0.370)	(0.522)	(0.0379)
F-test	18.34	0.309	1.02
Second Stage	1.564***	-3.410	2.269
	(0.378)	(6.974)	(2.306)
Observations	10,240	6,400	10,240
County FE	YES	YES	YES
Year FE	YES	YES	YES

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

Notes: Table shows results for the first and second stages with private funding as the dependent variable and TEFAP funding as the instrument. Errors are clustered at the county level. Unemployment rate and poverty rate at the county-year level are included as covariates.

Table 1.12: 2SLS Results with Organization Expenses

	(1) Total Expenses	(2) Fundraising Expenses	(3) Program Expenses
Govt. Funding	1.900***	0.331***	2.205***
	(0.420)	(0.0917)	(0.460)
Observations	10,240	10,240	10,240
County FE	YES	YES	YES
Year FE	YES	YES	YES

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

Notes: Total Expenses include those listed as well as management and general expenses. Errors are clustered at the county level. Unemployment rate and poverty rate at the county-year level are included as covariates.

Table 1.13: 2SLS Results with Organization Revenue

	(1) Total Revenue	(2) Related Revenue	(3) Unrelated Revenue
Govt. Funding	0.575*	0.736**	-0.480**
	(0.301)	(0.304)	(0.226)
Observations	10,240	10,240	10,240
County FE	YES	YES	YES
Year FE	YES	YES	YES

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

Notes: Organization revenue is money received through organization programming or other activities and does not include contributions from private funding. Total Revenue includes revenue directly related and unrelated to an organization's mission. Unrelated revenue is taxed, except under very specific circumstances (Tax Code Sections 512-514). Errors are clustered at the county level. Unemployment rate and poverty rate at the county-year level are included as covariates.

Chapter 2

Low-Income Housing in High-Opportunity Neighborhoods: Effects on Crime

2.1 Introduction

Recent policy debate has brought to the forefront the geographic income segregation that persists throughout the United States. Concerning this matter, research has focused on topics around redlining, subsidized housing, and others, highlighting that housing policy in the United States has contributed to keeping poor people in poor places and diminishing their opportunity for economic mobility (e.g. Rothstein, 2017; Carter et al., 1998). A study by the Harvard Joint Center for Housing Studies found that over 30% of American households are cost-burdened when it comes to housing, which means that a household spends more than 30% of their income on housing. When so many people struggle to pay for housing, the options for where they can live are heavily limited.

In line with this debate, in the last decade, there have been a series of lawsuits (e.g *Texas Department of Housing and Community Affairs v. Inclusive Communities Project, Inc.*, 2015) condemning how state and local governments have violated provisions of the Fair Housing Act by disproportionately funding affordable housing in urban cores over suburbs, which perpetuated housing segregation. While the government, at all levels, has institutions in place to provide affordable housing to low-income people, both politicians and non-governmental organizations frequently question their effectiveness. One crucial criticism when it comes to affordable housing is that these place-based type policies help poor places, not poor people. In response to these criticisms, some states have reevaluated the way they provide subsidized housing to low-income folks in order to promote economic mobility.

One of the shifts came in the form of incentivizing construction of affordable housing in designated high opportunity areas through more generous tax credits for developers (*Spotlight on Underserved Markets: Opportunity Incentives in LIHTC Qualified Allocation Plans*, 2019). High opportunity areas, though broadly defined, are generally neighborhoods that have low poverty rates, relatively high median incomes, good schools, and other high-quality amenities. However, this shift in policy has not come without opposition. Some residents of these so-called high opportunity areas have fought back against having affordable housing in their neighborhoods, adopting the framework of the ”Not In My Backyard” (NIMBY) movement (e.g. Reaves, 2019; Shay, 2016). Concerns range from spikes in crime rates to changes in property values. The extent to which their concerns are valid has not been thoroughly studied. The purpose of this project is to further that understanding.

I explore how affordable housing in high opportunity areas affects local crime rates, which is heavily cited as a reason to oppose subsidized housing developments (Dougherty, 2020). I focus on housing developments funded through the Low Income Housing Tax Credit (LIHTC) program, which funds over 90% of all subsidized housing in the United States. In order to estimate causal effects, I exploit a change in the way Texas awarded LIHTC starting in 2013, which incentivized construction in low-poverty areas and use a fuzzy regression discontinuity design. The policy change creates a quasi-experimental setting to study the effects of affordable housing on high opportunity areas. I find that this added incentive, which in the long run typically covers over 70% of construction costs, incentivizes construction of an additional 8% low-income housing units relative to non-qualifying areas. This equates to approximately two additional housing units. I use these first-stage estimates in order to estimate the effect of these additional units on property and aggravated crime rates. Contrary to concerns raised by NIMBY supporters regarding this approach to siting affordable housing, I find that there are no statistically significant effects on crime stemming from additional LIHTC housing.

Previous research that has studied neighborhood effects of affordable housing has mostly focused on high poverty areas and has provided evidence regarding patterns of gentrification and resident turnover at one point in time (e.g. Baum-Snow & Marion, 2009; Dawkins, 2013; Freedman & McGavock, 2015). A smaller collection of papers (e.g. Freedman & Owens, 2011; Diamond & McQuade, 2019) has provided evidence for effects on neighborhood crime. However, the findings are limited in scope. There is also a shortage of evidence regarding how very low-poverty places, specifically, react to affordable housing. With many states moving in the direction of funding more and more low-income housing in wealthier places, it is paramount to understand the dynamics.

In the next section, I review the related literature as well as recent findings. Section 3.3 gives an overview of the LIHTC setting and policy changes. Section 3.4 details the data used in this project. Section 3.5 describes the empirical strategy. Sections 2.6 and 2.7 explain the results of both the first and second stages of this analysis. Section 3.7 concludes.

2.2 Related Literature

Policies regarding subsidized housing for low-income individuals has long been a research topic of interest both for policymakers and academics. In particular, housing location and its effects on neighborhoods and tenants has garnered attention in recent years. With the recent shift to subsidize housing in affluent areas, the external validity of previous research on subsidized housing, which has traditionally been located in high-poverty areas, comes into question.

Low Income Housing

There are a series of papers that evaluate the effect of LIHTC developments on various outcomes. Baum-Snow & Marion (2009) find that for certain low-income census tracts, the tax credit incentivizes the construction of six additional low-income units. This paper exploits a discontinuity in the funding formula to study causality. The authors also find that these additional units cause homeowner turnover to increase and property values to decrease in declining neighborhoods. However, they do observe that some neighborhoods are more prone to gentrification. Similarly to Baum-Snow & Marion, Freedman & Owens (2011) use another funding rule to test whether low-income housing units impact crime rates. Again, they focus on high-poverty neighborhoods receiving developments. They find that the revitalization stemming from the tax credit reduces violent crime but has no effects on property crime.

Diamond & McQuade (2019) study the spillovers of LIHTC properties on heterogenous census tracts. They find that when developments are located in high-income areas, property values decrease, and low-income families move in. They also show some results for the impact on crime rates, however, these are limited to one point in time and do not differentiate between immediate changes and long-run effects. In this paper, the construction of LIHTC is taken as randomly assigned rather than using a notch in funding formulas like previous papers have done. The underlying assumption is that developers are not strategic about where they locate and when they build due to the competitive nature of securing tax credits. However, this is not always the case and there is a lot of variation across states. For example, in Texas, developers can submit a pre-application where they can find out who the competition will be for that application cycle. They may then strategically choose whether to apply or not. Regardless their findings are suggestive of how these LIHTC developments can impact affluent communities.

Neighborhood Economic Integration

Another important strand of literature that this research speaks to, which has many policy implications, is the sorting of people into neighborhoods. The Tiebout model explains that people sort into neighborhoods based on their preference for amenities (Tiebout, 1956). By this logic, an affordable housing development in an affluent neighborhood may be an unwelcome amenity for some of the existing residents. Therefore, there may be some re-sorting that occurs, causing resident turnover to increase. While one of the goals of the high opportunity area policy is to increase economic mobility through the economic integration of neighborhoods, it may come with the side effect of wealthier people leaving these communities. This effect was a common phenomenon during the school integration era and became known as white flight (Clotfelter, 1975). In the long run, these affluent communities could experience a decrease in the quality of amenities currently available if the current consumers of those amenities decide to leave.

Ellen, O'Regan, & Voicu (2009) explain that, historically, LIHTC has not only been situated in high poverty areas but also attracts impoverished individuals to those areas. Hence, it is plausible that there would be an increase of poverty within an affluent neighborhood, given this chain reaction of the events. On the other hand, Glaeser, Kahn, & Rappaport (2008) emphasize that low-income people live in urban, more impoverished, cities because they have better access to amenities that suit their needs, like public transportation. If this is the case, communities would only be directly impacted by the LIHTC development without added negative externalities, like violence and drugs, as detailed by B. Katz & Turner (2007), that are historically typical of subsidized housing.

Neighborhood Effects and Crime

While the effects LIHTC properties on their tenants have not been studied as much as other topics, recent research by Chetty, Hendren, & Katz (2016) suggests that living in low poverty neighborhoods improves long term outcomes for children who move from high poverty communities. Policymakers have drawn from this logic in order to amend affordable housing policy and designate high opportunity areas. However, the Chetty et al. research has primarily used the Moving to Opportunity experiment, which offered vouchers for families to relocate to other, more affluent communities. While this evidence is suggestive, it may not be directly applicable to the current context because the magnitude of the change to the overall neighborhood is unclear.

Drawing from this literature, Chyn (2018) studies the effects of moving out of impoverished areas on crime outcomes. He finds that when children move out of public housing, they face fewer violent crime arrests in adulthood. It is not clear whether this reduction in crime outcomes is due to leaving public housing or due to moving to a better neighborhood. If the latter, this may suggest that as LIHTC projects are built in more affluent neighborhoods, children may also see reductions in projected crime outcomes. Therefore, the area should not experience any effects on crime. However, if it is the former, these children would technically still live in subsidized housing, which may in itself create a community that fosters adverse outcomes. Studying causal effects on crime due to an increase in subsidized housing is essential. Research on localized crime outcomes (e.g. Diamond & McQuade (2019), Freedman & Owens (2011)) focus primarily on property and violent crime given they are likely correlated with where people live due to ease of access. These types of crime, along with drug-related offenses, are of particular concern for NIMBY proponents.

2.3 LIHTC Program

The Low Income Housing Tax Credit (LIHTC) program was established in the late 1980s and is the primary source of government funding for affordable rental housing in the United States, covering about 90% of subsidized housing. The purpose of this program is to incentivize the construction and rehabilitation of affordable housing by private developers via a non-refundable tax credit. In practice, private or nonprofit developers apply for these credits through their state agencies and if their application is chosen they then sell these credits, which are generally paid out over ten years, to investors to finance their projects. The program is administered by the U.S. Department of the Treasury, unlike most other affordable housing programs, which are managed by the Department of Housing and Urban Development (HUD). While HUD houses much of the data on LIHTC properties and regulations, this distinction makes LIHTC unique as it gives states discretion to determine how they allocate tax credits through their annual Qualified Allocation Plans (QAPs).

With a set congressional yearly budget —$3.1 million total in 2018, the Treasury grants each state a set dollar amount in tax credits to be allocated that year. Each state's housing agency then awards these credits through a competitive and non-competitive program to developers who meet the respective criteria. This paper focuses only on the competitive tax credit program, also known as the 9% tax credit program, which has an acceptance rate of about 50%. Developers apply annually to the 9% program and are rated based on criteria from the current QAP. Developments proposing new construction or significant rehabilitation qualify for this type of tax credit, which, on average, provides $200,000 per housing unit. The specific annual credit amounts to 9% of the total project cost multiplied by the eligible basis of the project, which is the fraction of units in a development allocated to low-income families. The program requires that projects allocate either at least 20% of units to tenants with income below 50% of the area's median income (AMI) or 40% of units to tenants with income below 60% of AMI. Rent for the low-income units is typically capped at 30% of 60% of AMI, which is determined by HUD based on census or ACS data.

Texas Setting

Though the LIHTC program allows states to write their own QAP to set their selection criteria, until 2009, it required that they offer a 30% basis boost to developments located in Qualified Census Tracts (QCT) and Difficult to Develop Areas (DDA). Both QCTs and DDAs are areas designated by HUD as underserved either because they are severely impoverished or the construction costs are relatively high. The 30% basis boost allowed developers to claim an additional 30% for their eligible basis, increasing their credit rate by up to 2.7%. Starting with the 2009 QAP, after the enactment of the Housing and Economic Recovery Act of 2008, states were allowed to modify the criteria for developments to receive the basis boost. At the same time, the Texas Department of Housing (TDHCA), which is responsible for the allocation and distribution of LIHTC funds, was being sued for violating the Fair Housing Act by disproportionately funding affordable housing in "predominantly black inner-city areas and too few in predominantly white suburban neighborhoods"(*Texas Department of Housing and Community Affairs v. Inclusive Communities Project, Inc.*, 2015).

Given this criticism, which was ultimately supported by the Supreme Court's ruling on the case, and the new flexibility states obtained in allocating credits, Texas began to prioritize qualifying developments proposed in low-poverty areas. In Texas, the 2009 QAP stipulated that projects located in census tracts with poverty rates at or below 10% were eligible for the basis boost. In 2012, the qualifying threshold increased to 15%. In 2013, the QAP cemented the focus on low-poverty, what were called high-opportunity areas, by awarding additional points when scoring applications that proposed developments in these areas. This meant that there was a greater likelihood that a project would be funded, plus the project would receive a credit boosts. TDHCA releases the list of tracts that eligible for a funding boost and application points well in advance.

As a high-level overview, I examine developments approved between 2008 and 2016, breaking them up pre- and post- 2013, and the poverty rate at the time of the awards of the tracts in which they are located. Figure 2.1 shows that over 68% of developments awarded between 2013 and 2016 were proposed in census tracts with poverty rates at or below 15%. This is in comparison to those properties selected between 2008 and 2013, in which only 46% were located in tracts with such low poverty. I exclude 2012 because, while the tax boost threshold of 15% poverty rate was established that year, the changes made in 2013 were significant and were stable through 2016. In figure 2.2, I plot trends in LIHTC developments. Using the sample of approved projects in Texas, I categorize projects approved between 2003 and 2016 into bins depending on their poverty rates. This time-series data shows the trends of the binned projects as a share of total projects approved in a given year. Noticeably, the share of projects in census tracts with poverty rates under 15% begin to increase after 2009, while projects with poverty rates above 25% decrease. Those with poverty rates between 15% and 25% are stable throughout the period.

One unique feature of Texas' LIHTC allocation process is that the TDHCA encourages developers to submit a pre-application, which is a short version of the full application. The pre-applications are scored and data is published online before the official applications are due. The pre-application process is designed to give developers a chance to scope out their competition for the funding year and make a strategic decision over whether to complete the full application or wait. Applicants also receive additional points in the final scoring if they submit a pre-application. This process invalidates the assumption that funding to developments is randomly assigned by nature of a competitive application process, which is central to some related research (Diamond & McQuade, 2019). Rather, the pre-application process allows developers to be strategic and apply when they have the best chances of getting funding.

Funding Formula

Given the size of these credits, LIHTC is a popular program. The amount of the annual credit is calculated using the following formula:

$$T = .09C \times B$$

Where T is the dollar amount of the credit, C is the development costs, excluding land acquisition, and B is the eligible basis.

As a concrete example, suppose a developer, whose project will cost $1,000,000, applies for a tax credit. This specific project has 100 housing units, 80 of which are allocated to low-income families ($B = 0.8$). If selected, this developer will receive a $72,000 tax credit every year for ten years. With the 30% basis boost ($B' = 1.1$), the annual credit increases to $99,000. These credits are adjusted for inflation. In practice, developers typically receive tax credits to cover 70% of their entire construction costs, and even more when they site developments in qualifying areas, in this case 99%.

2.4 Data

The primary data used in this project is from the Texas Department of Housing and Community Affairs (TDHCA). This agency is responsible for writing the Texas QAP and awarding LIHTC. I use their annual Site Demographics Characteristics Report, annual report on developments awarded, and property inventory list. From the report, I draw the poverty rate used to determine tract eligibility as well as tract funding eligibility status, which is necessary due to restrictions of awarding tracts with an already large concentration of LIHTC units. The poverty rate published in these reports is particularly important because it does not necessarily coincide with current ACS or Census data. For example, for the properties awarded in 2012, TDHCA used the 2005-2009 5-year ACS estimates. Previous to that, they relied on the 2000 census. The annual report on sites awarded provides details on the sites that received LIHTC, including the award amount and eligible LIHTC units out of the total units. The property inventory is more high level and is used for historical data. It also serves to eliminate developments that may have received an award, but were never put into service or whose award was revoked.

I supplement these data using the 2000 and 2010 censuses as well as the 5-year ACS estimates from 2009 through 2016. From these, I draw all of the controls used in the empirical analysis. I assign tract demographic characteristics from their respective ACS estimates.. Also, I use geography relationship files to normalize all census tracts and their characteristics to their 2000 geographic boundaries. I do this with the help of Brown University's Longitudinal Tract Data Base program. The reasoning is that for the historical analysis and placebo, the census tracts on the THDCA property inventory use 2010 census tracts and need to be converter to their 2000 equivalent to get property rates and demographic characteristics. I also rely on the U.S. Department of Agriculture (USDA) categorization of census tracts into Rural-Urban Commuting Areas (RUCA). The RUCA categories allow me to approximate which census tracts obtain rural classification for LIHTC purposes and drop them from the sample because rural areas have a set-aside budget under the LIHTC program, which may impact how often developments in those areas are chosen.

The final data sources are crime data from the Houston, Austin, Dallas, and Fort Worth Police Departments' inventory of reported crimes. These data show all reported crimes and the city block where they occurred, categorized following the Uniform Crime Report (UCR) Program standards set by the FBI. I focus on residential burglaries, vehicular burglaries, robberies, assaults, and drug related offenses that were reported between 2013 and 2019. All cities, except for Houston, provide exact latitutde and longitude coordinates the location of the incident, which makes it easy to match to census tracts. The Houston Police Department provide a block range and street name of the location of the incident, which makes matching the exact census tract challenging. I geocode the approximate location of the crimes using the Census Geocoder to match them to census tracts. About 20% of crime observations from Houston are discarded due to unreliable location information.

2.5 Empirical Strategy

We cannot directly estimate the effects of low-income housing on crime in low-poverty neighborhoods because there are likely unobservable factors that make some neighborhoods more favorable than other, which may impact crime rates. Because developers are encouraged to be strategic when submitting their LIHTC applications in Texas through the pre-application process, this is an even bigger indicator that there would be selection bias if we were to estimate the relationship directly. In order to tackle this issue, I exploit the rule-based policy change in Texas that boosts the tax credit and increases the points an application scores for developments located in census tracts with poverty rates under 15%. This rule lends itself to a regression discontinuity design (RDD). However, because the rule does not guarantee that a development will be built in every qualifying tract, this is a fuzzy RDD, rather than sharp, which will provide intention-to-treat (ITT) effects rather than treatment-on-treated (TOT) effect.

RDD Setup

An added benefit of the RDD strategy is that I can restrict the analysis to the years where the policy was consistent (2013-2016). Because there were several policy changes between 2009 and 2013 related to LIHTC in Texas, by restricting the sample to projects approved between 2013 and 2016 and comparing to those approved between 2008 and 2011, I can disentangle the effects of the policy rule from effects due to the scoring structure of the program.

One of the conditions of a valid RDD is that the running qualifying variable, in this case poverty rate, is continuous. Figure 2.3 plots a histogram of the frequencies of census tracts by their poverty rate along with a kernel density function. The data is restricted to tracts with poverty rates under 30%, which is the focus of the analysis. We see that at the 15% threshold, there are no apparent signs of bunching or jumps. It is not expected that tracts can manipulate their poverty rate. In fact, the poverty rate used for the selection rule of the LIHTC program is determined by poverty rates in previous years due to ACS data release schedules.

Another important consideration for the RDD setup is that observations below and above the threshold are comparable groups. I restrict the bandwidth of the analysis to tracts with poverty rates between 5% and 25%. I obtain a sample of 12,094 tract-year observations, with 7,063 below the threshold. A comparison between the treated and control group across demographics and LIHTC related variables can be found in table 3.1. The demographic characteristics for both groups are virtually the same. The differences in the LIHTC units and LIHTC awards are more prominent. These measures are higher for tracts with poverty rates at or below 15%, which is in line with the increased credit boost policy change.

Estimation

The estimating equation for this design is

$$Y_i = \alpha_0 + \alpha_1 1\{r_i \leq c\} + \beta_1 f(r_i) + X_i + \nu_i \tag{2.1}$$

where Y_i is the crime outcome of interest for tract i. The second term is an indicator function for whether the poverty rate r of tract i is less than or equal to the cutoff point c of 15%. $f(r_i)$ is a k-th order polynomial used to fit the model which is defined as

$$f(r_i) = \sum_{k=1}^{K} [d_{1k}(r_i - c)^k + d_{2k} 1\{r_i < c\}(r_i - c)^k] \tag{2.2}$$

For the analysis that follows, I estimate both a parametric and a non-parametric model. I show results for local linear and quadratic functions for of $f(r_i)$. The non-parametric model uses a triangular kernel function to assign weights to observations based on their distance from the cutoff, which is useful given the limited share of tracts that have nonzero LIHTC units (Cattaneo et al., 2019). The local linear non-parametric model is used for the outcomes analysis and RDD plots.

2.6 First Stage

The first step is estimating the relationship between the LIHTC policy rule and LIHTC units. This first stage is estimated as follows:

$$LIHTC_i = \beta_0 + \beta_1\{r_i < c\} + f(r_i) + \gamma X_i + \epsilon_i \tag{2.3}$$

The outcome $LIHTC_i$ is a measure of LIHTC units. Figure 2.4 shows the regression discontinuity plot for the 2013 policy change. Here, I use the nonparametric model to fit a local linear polynomial for the function $f(r_i)$. At the cutoff line, tracts immediately above the 15% poverty rate receive less LIHTC unit construction in a given year than tracts under the cutoff. In order to validate this graph, I also plot the LIHTC flow against poverty rates for the years before 2011. The results are shown in Figure 2.5. The absence of a clear discontinuity in figure 2.5 suggests that the effect depicted in figure 2.4 is due to the change in policy that grants a tax credit boost.

The RD estimates are shown in table 2.2. Columns (1) and (2) display the estimates for the log transformation of LIHTC units. On average, tracts that qualify for the credit boost receive an additional 5% LIHTC units under the parametric model and over 8% using the non-parametric model. Columns (3) and (4) show the estimates for the flow of units. These results suggest that qualifying census tracts are awarded approximately one to two additional LIHTC units relative to comparable tracts with poverty rates about 15%. The results are similar in magnitude, though less significant, when using a quadratic polynomial to approximate $f(r_i)$. The non-parametric model weighs observations near the cutoff point higher, which indicates that the difference is more prominent right around the 15% mark and not as pronounced in tracts farther away. This is also evident visually.

2.7 Effects on Crime

To estimate the effects of LIHTC developments on crime, I use the fuzzy regression discontinuity model and approximate the intent to treat effect. Because a poverty rate under 15% does not guarantee that a census tract will contain LIHTC units, I cannot directly estimate the average treatment effect. In this setup, I compare two measures of crime, the crime rate and the log transformation of the number of crimes, in census tracts within ten 10% poverty of the cutoff to receive the additional 30% boost. I analyze crime statistics for one and two years after the credits are allocated. Though HUD provides dates for when projects were placed in service, their data are notoriously inconsistent. For example, between 2003 and 2016, the HUD LIHTC database shows that approximately 300 projects were approved, compared to 1,100 in the Texas data. Also, between 2013 and 2016, the data for when projects were placed in service is only plausible fewer than five observations. Of the 300 projects with reliable service dates between 2003 and 2012, the average period between when a credit is allocated and when a development's units are ready to be leased is 1.6 years. Also, as a rule, developments must be placed in service no later than two years after the credit is awarded. Therefore, any immediate effects on crime would be evident one to two years after credit allocation. I also evaluate effects on crime reported three years after the tax credits are allocated to capture any lagged effects resulting from developments that may have taken longer than average to be put into service. Those results can be found in Appendix B.

Based on the fuzzy regression discontinuity model used to estimate LIHTC unit construction after the 2013 policy change, I estimate the reduced form equation to capture the ITT effect on crime:

$$Y_{i(t+n)} = \alpha_0 + \alpha_1 1\{r_i < c\} + \beta_1 f(r_i) + X_{it} + \delta_t + \nu_{it} \tag{2.4}$$

Here, Y_{it+n} is either log number of crimes in a given tract on a given year or the crime rate, specifically the number of crimes reported per 1000 persons in a tract. $t + n$ indicates the years after the LIHTC is allocated in year t and where $n = \{0, 1, 2, 3\}$ indicates up to three years post award. I evaluate effects on three different types of crimes that are often identified as reasons for opposition to LIHTC housing by NIMBY advocates (McNee & Pojani, 2022). The three types are (1) burglaries, which include residential and vehicular burglaries, (2) violent offenses, which include robberies and assaults, and (3) drug offenses, which include any drug and narcotic related crimes. For each of these types of crime, I find no evidence that the new construction of LIHTC developments have any statistically significant effects on either burglary or robbery rates in qualifying census tracts.

Burglaries (Property Crime)

I first estimate the effects on burglaries, both residential and vehicular, which may serve as a representation of changes to property crime. Figure 2.6 plots the regression discontinuity style graph using the 15% poverty rate cut off. We see that there is no variation in treatment below and above the cutoff, suggesting that there is no difference in ITT effect for tract on either side of the cutoff rule. More concretely table 2.3 presents the reduced forms estimates for the log transformation of reported burglaries and the tract burglary rate per 1000 persons. Neither specification shows any significant results. In fact, the coefficients are below zero for some year specifications.

Violent Offenses

I conduct the same type of analysis for violent offenses, which include robberies and assaults. Robberies are differentiated from burglaries in that they are considered to be crimes against a person, rather than a building or vehicle, and entail force or violence. Figure 2.8 plots this relationship for the one-year post LIHTC crime data. Visually, there is no jump in the line that indicates a difference among the tracts near the cutoff point. In fact, the plot follows a similar trend to the effect on log violent offenses (figure 2.7), which also shows no visual jump at the poverty rate cutoff. Similarly to the results for effects on burglaries, the results, which are presented in table 2.4, show no statistical difference between census tracts above and below the 15% cut off. The only estimates with statistical significance is the ITT effect on the violent offense rates one and 2 years after the LIHTC award year. These results indicate that tract with poverty under 15% reported two additional violent offenses per 1000 tract residents than tracts above the cutoff.

The magnitude of these coefficients and the predominately insignificant results indicate that we cannot claim that LIHTC construction resulting from a higher tax credit in a census tract impacts either robberies or burglaries at a different level than other census tracts.

Appendix B contains additional results and an additional figure showing the analysis for more years post-LIHTC.

Drug Offenses

The final type of crime included in the analysis is drug and narcotic related incidents. While this has not typically been included in related LIHTC papers, increased drug possession is often cited as a concern by NIMBY advocates. Figure 2.9 shows there is a large jump in between census tracts under the cutoff and above. This jump indicates that tracts with poverty rates above the cutoff reported higher drug related crimes. The reduced form results for effects on drug related offenses can be found in table 2.5. Similarly to the estimates for burglaries and violent offenses, there appear to be no statistically significant differences between census tract on either side of the 15% poverty rate cut off in any year specification. While the reduced form results are not significant, the jump in the figure supports the story for property and violent crime that additional LIHTC units are likely indicative of increasing crime rates.

2.8 Conclusion

This paper furthers the understanding of low-income housing developments on their neighborhoods. The recent shift in policy across states that incentivizes the construction of affordable housing in high-opportunity neighborhoods has brought with it plenty of opposition from current residents of affluent communities. A often cited concern is increasing crime rates and drug use. This paper shows that there is no significant link between crime and LIHTC construction.

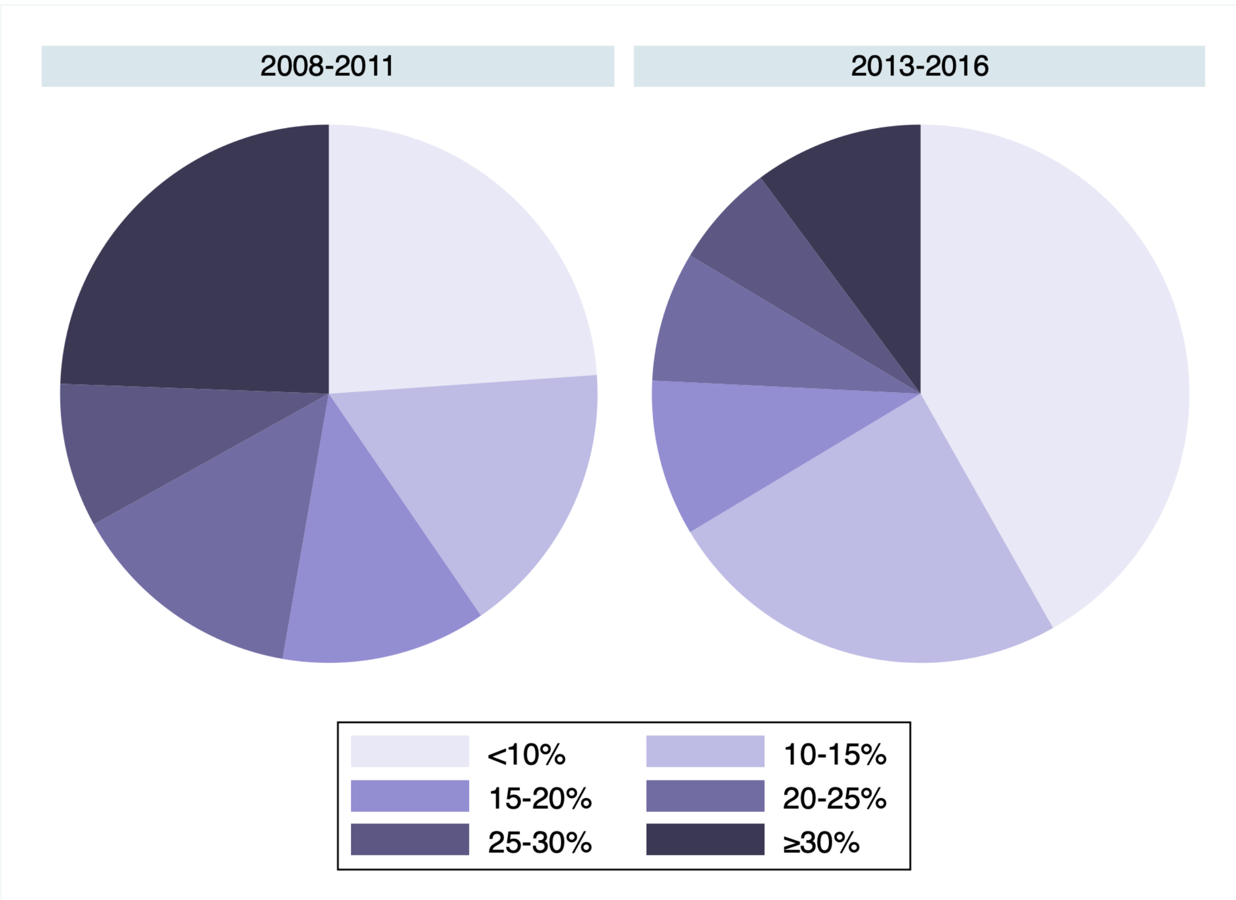
In this project, I take advantage of a clearly defined rule in Texas that awards housing developers a tax credit boost for building in census tracts with poverty rates at of below 15%. I use an fuzzy RDD design to estimate the ITT effects on the construction of LIHTC units and crime rates.

I find that qualifying low-poverty neighborhoods obtain 8% additional units of low-income housing in a given year. This is an approximate increase of two units on a base of the one unit each tract receives on average. While the first stage shows a strong relationship between poverty rate near the qualifying threshold and LIHTC construction, the same relationships is not present for property or drug related crimes. I find that tracts below the cutoff poverty rate experience crime that is not statistically different than those immediately above. This is the case to 1, 2, and 3 years after the tax credit is awarded, giving ample time to account for construction of the units and for new residents to occupy the units. I find a slight difference in the violent offenses crime rate for tract eligible to the tax credit boost. The model estimates that eligible tract report 2 additional crimes per 1000 persons over the control group. However, the difference is very small and not visually distinguishable. It also contradicts the findings using the log transformation outcome of the same variable. This may point to measurement error, but further analysis is needed to validate the results. Barring limitations to data availability, this analysis could be replicated in other cities throughout Texas. This would be particularly interesting for less dense geographic areas, as different places may respond differently.

This paper opens up discussion to focus more on affluent neighborhoods when it comes to affordable housing construction. Given the recency in policy changes, long-run effects are cannot yet be studied but would be of interest to compare to findings related to LIHTC in high-poverty areas. This paired with analysis on tenants who occupy these developments would be useful in understanding policy designed to further economic mobility.

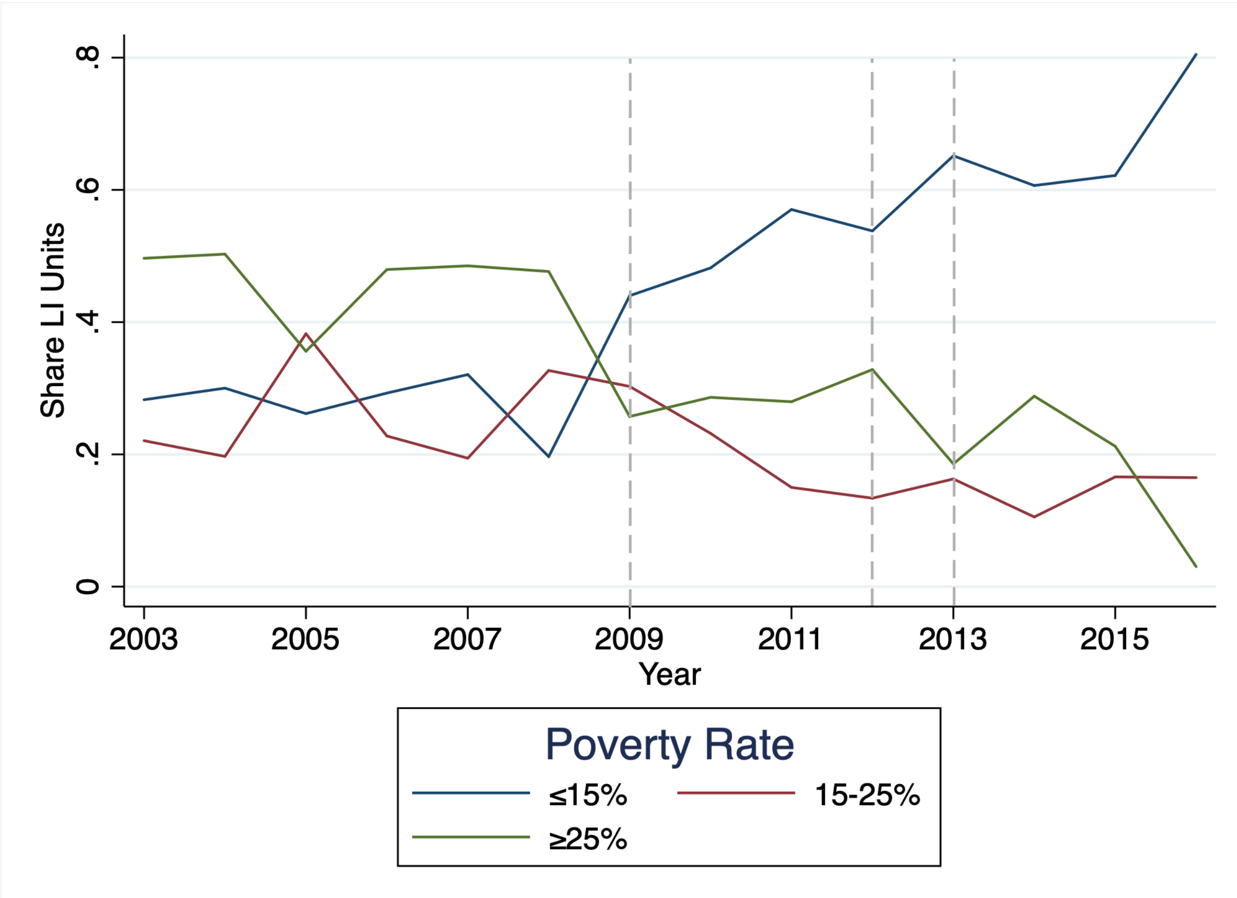
Figures

Figure 2.1: Developments by Tract Poverty Rate



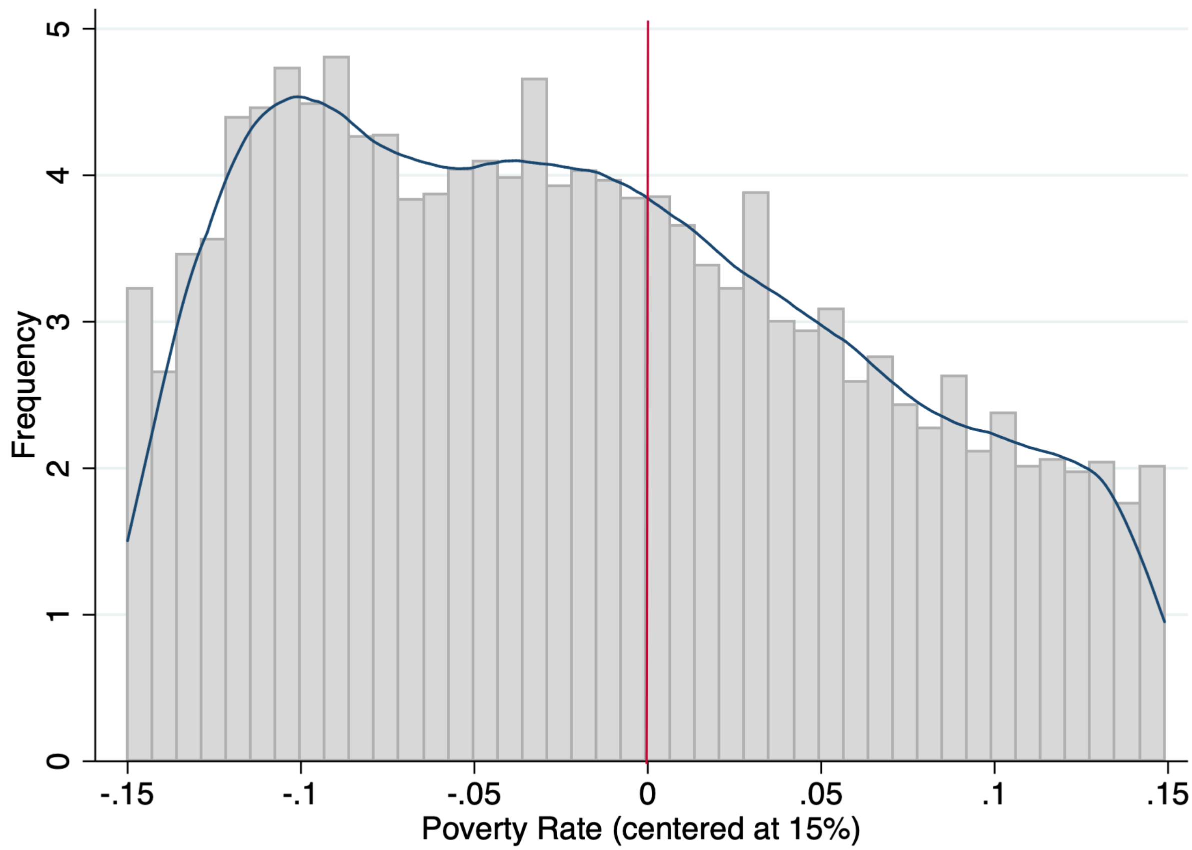
Notes: This chart shows the developments that were awarded LIHTC broken out by poverty rates of their census tract location. Between 2013 and 2016, when developments located in census tracts with a poverty rate under 15% received a credit boost, shows that over two thirds of developments were located in these low-poverty areas. This share is a large increase from the 46% of developments awarded between 2008 and 2011 that were located in tracts with poverty rates under 15%.

Figure 2.2: Share of Low Income (LIHTC) Units by Tract Poverty Rate



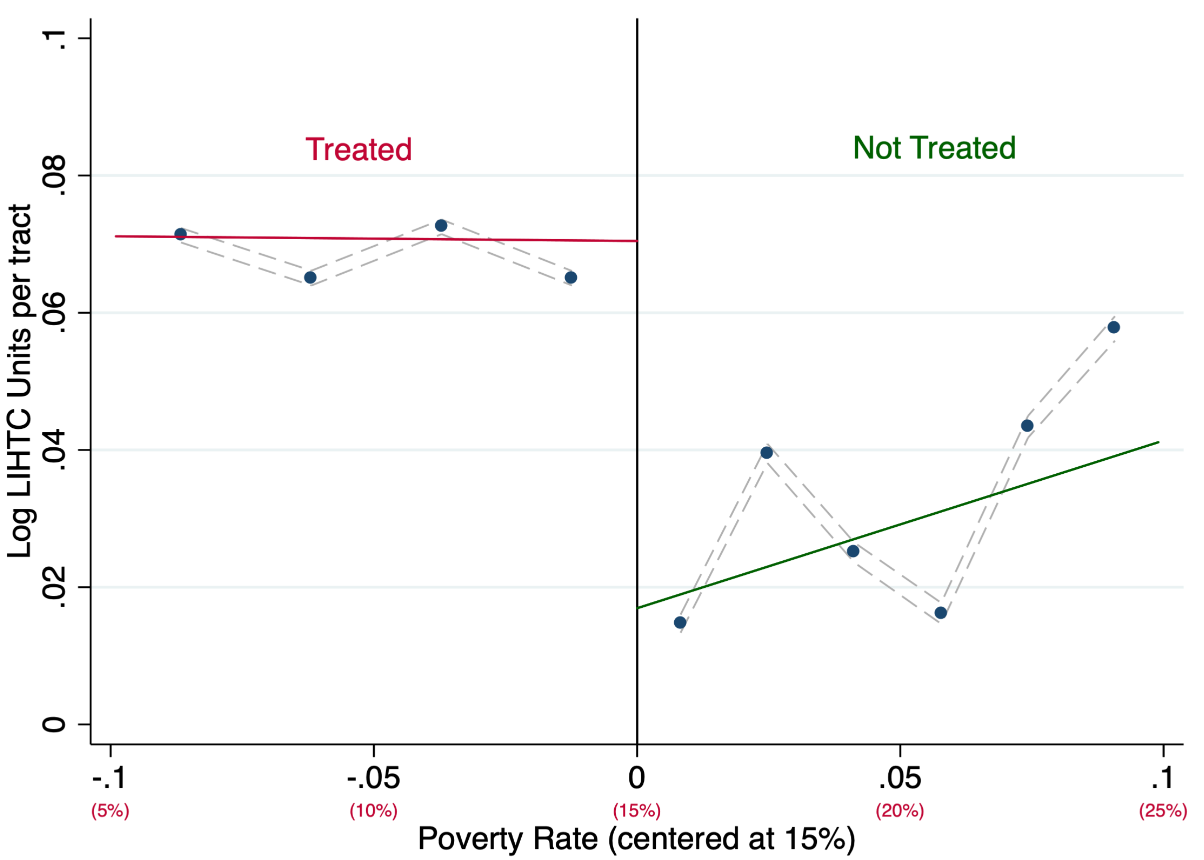
Notes: This figure shows the evolution of LIHTC projects approved in Texas in a given year by the poverty rate of the census tract where they are located. The y-axis shows number of projects as a fraction of total projects approved that year. Policy changes in 2009 and 2013 incentivized construction of developments in census tracts with poverty rates of 10% and 15%, respectively, through a tax credit boost. The bin of 15-25% poverty rate is not inclusive of its end points.

Figure 2.3: Poverty Rate Distribution (2013-2016)



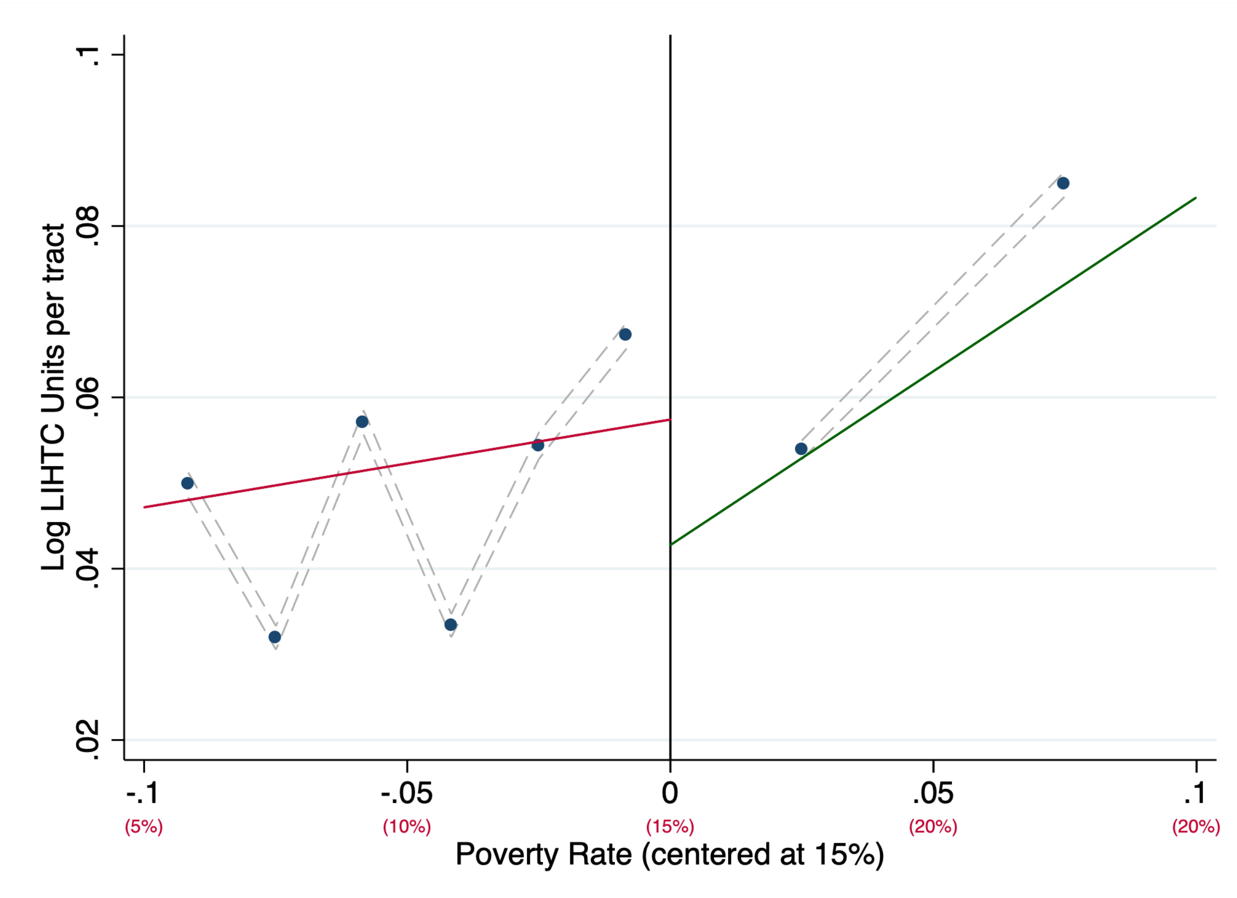
Notes: This figure shows the histogram and accompanying kernel function plotting the frequency and density of poverty rates at the census tract level for the years between 2013 and 2016. Poverty rates from 0% to 30% are shown, with a reference line at the 15% poverty rate. There is no bunching or jumps before or after the cutoff of interest.

Figure 2.4: First Stage RDD Plot (2013-2016)



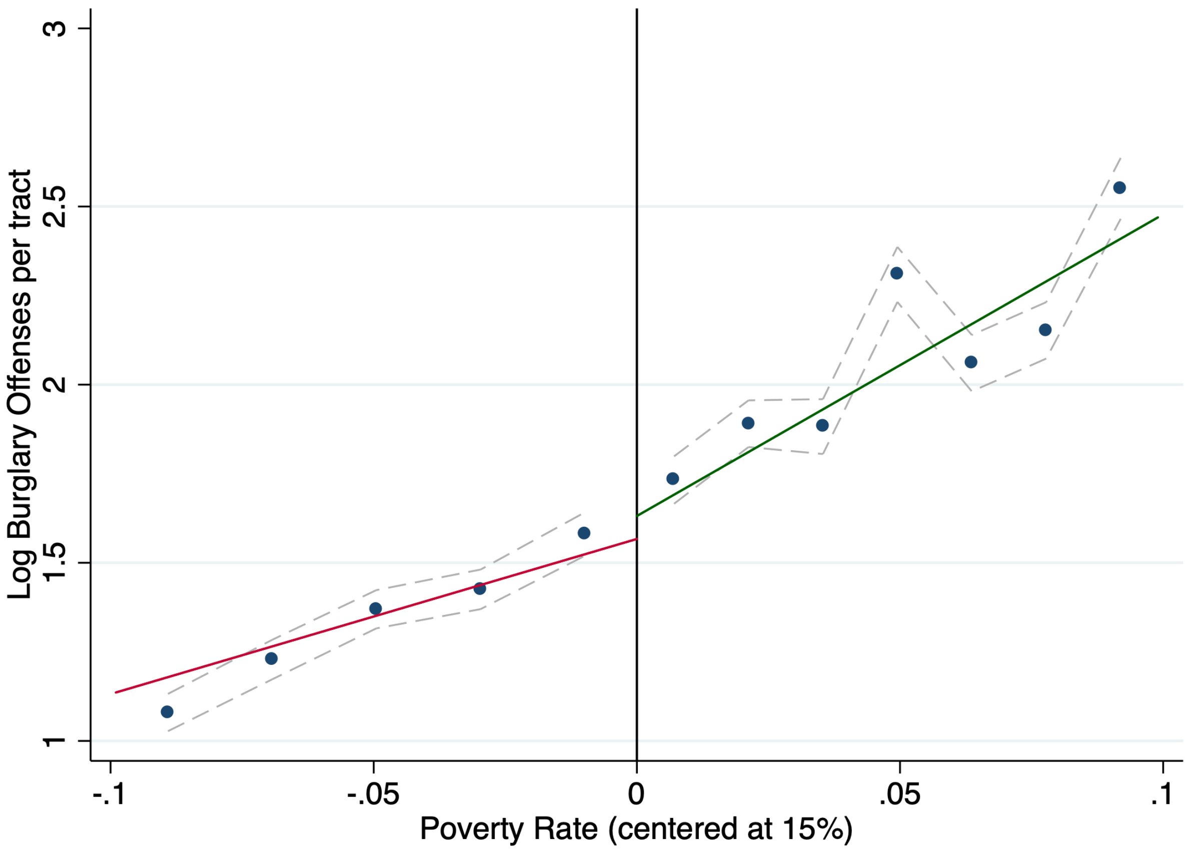
Notes: This plot shows the first stage results of the fuzzy regression discontinuity analysis in the post policy years (2013-2016). On the y-axis, the log transformation of LIHTC units per tract in census tract below and above the 15% poverty rate cutoff. There is a clear decline in the percent of LIHTC units constructed in census tracts with higher poverty rates that do not receive the 30% boost.

Figure 2.5: First Stage Placebo (2008-2011)



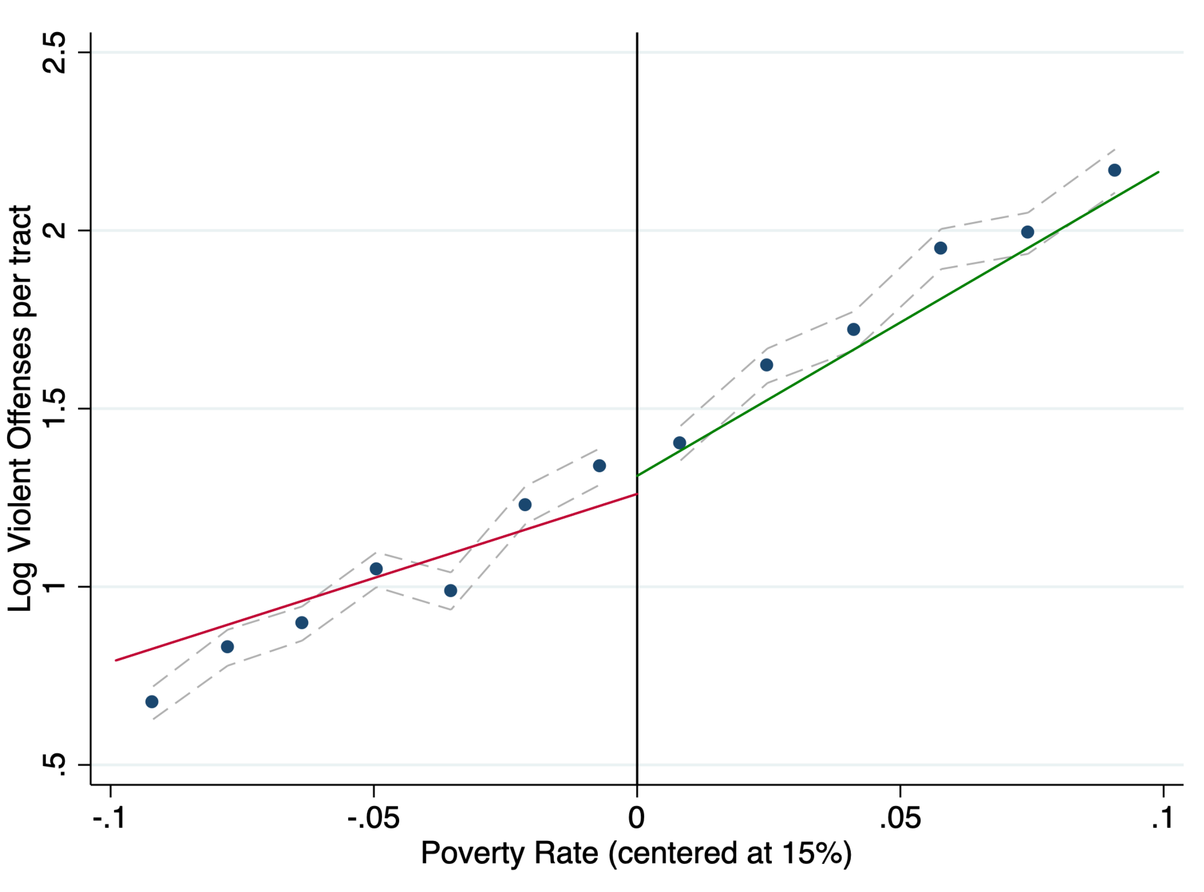
Notes: This plot mirrors the analysis shown in Figure 2.4 using data from 2008-2011. This plot serves as a placebo to verify the validity that the policy change in 2013 drives the decreased in units constructed in high-poverty areas. In this plot, the drop after the cutoff is not as evident and rather appears to show an increase in LIHTC units at higher poverty rates.

Figure 2.6: Burglaries (1 year post LIHTC)



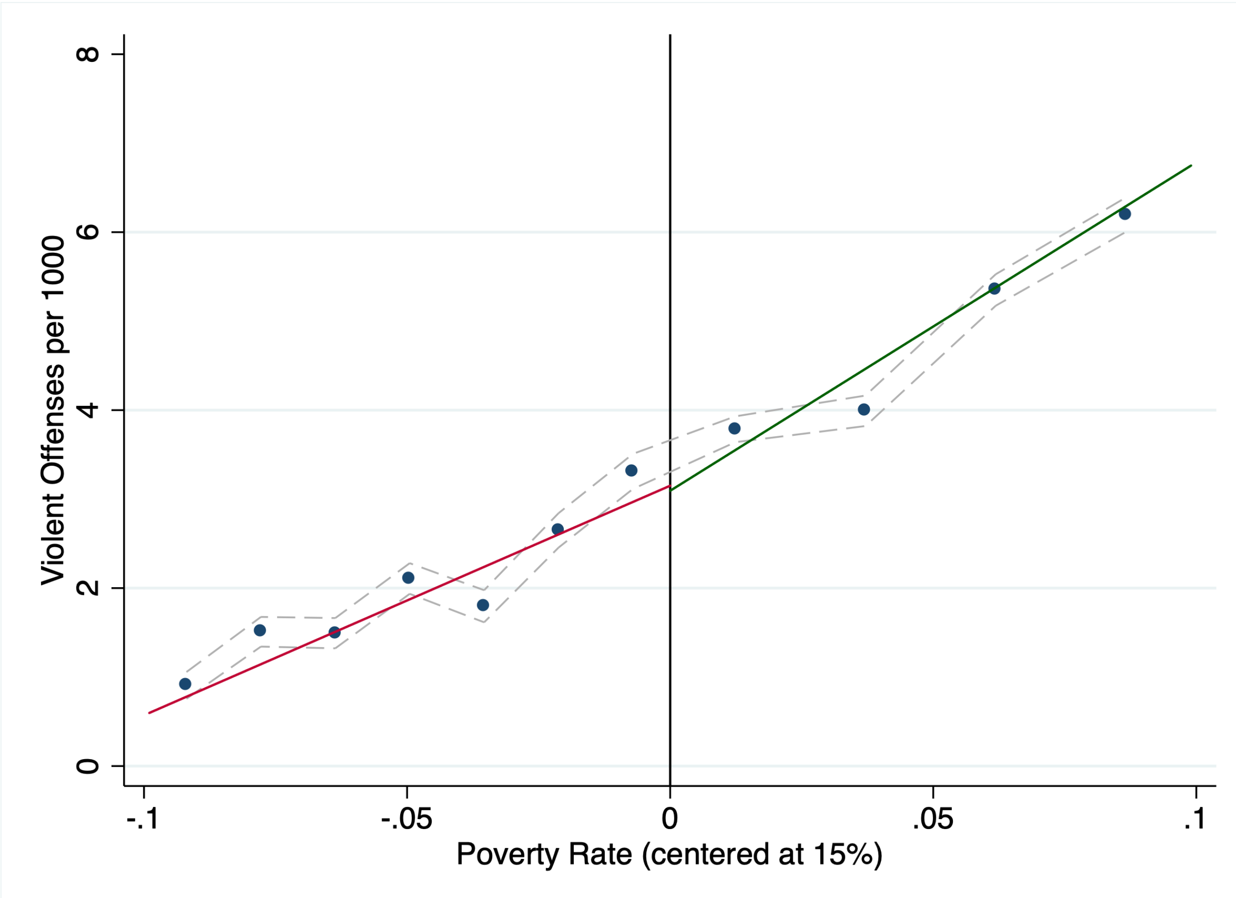
Notes: The figure plots burglary offenses (in logs) one year after LIHTC awards were announced by poverty rates centered at 15%. Burglary offenses include residential and vehicular burglary, which are proxies for overall property crime. The tracts eligible to receive a LIHTC boost are on the left side of the chart. The model includes population controls and year fixed effects from each year in 2013-2016. Tracts are restricted to served by police departments in Dallas, Fort Worth, Austin, and Houston. There appears to be no discontinuity at the cutoff poverty rate (15%).

Figure 2.7: Violent Offenses (1 year post LIHTC)



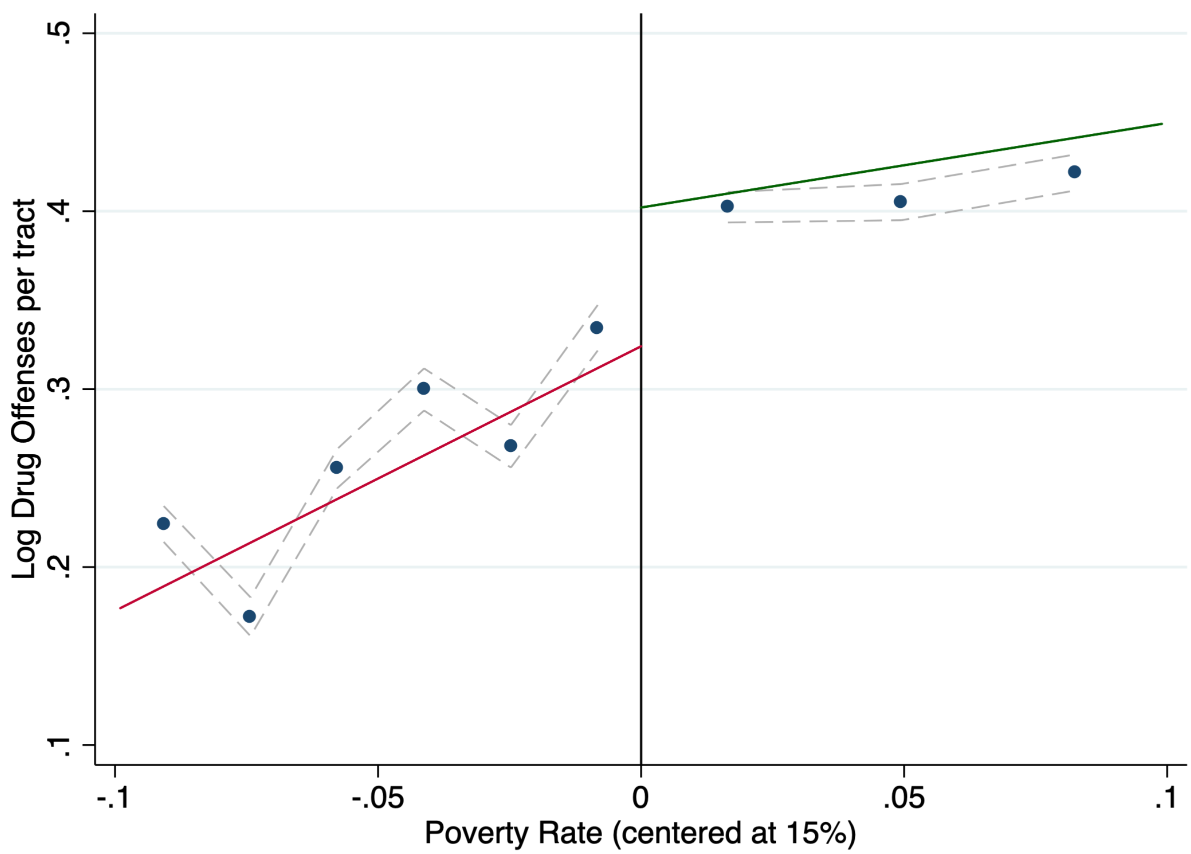
Notes: The figure plots violent offenses (in logs) one year after LIHTC awards were announced by poverty rates centered at 15%. Violent offenses include robberies and assault related offenses. The tracts eligible to receive a LIHTC boost are on the left side of the chart. The model includes population controls and year fixed effects from each year in 2013-2016. Tracts are restricted to served by police departments in Dallas, Fort Worth, Austin, and Houston. There appears to be no discontinuity at the cutoff poverty rate (15%).

Figure 2.8: Violent Offense Rate (1 year post LIHTC)



Notes: The figure plots the rate of violent offenses per 1000 persons one year after LIHTC awards were announced by poverty rates centered at 15%. Violent offenses include robberies and assault related offenses. The tracts eligible to receive a LIHTC boost are on the left side of the chart. The model includes population controls and year fixed effects from each year in 2013-2016. Tracts are restricted to served by police departments in Dallas, Fort Worth, Austin, and Houston. There appears to be no discontinuity at the cutoff poverty rate (15%).

Figure 2.9: Drug Offenses (1 year post LIHTC)



Notes: The figure plots drug offenses (in logs) one year after LIHTC awards were announced by poverty rates centered at 15%. Drug offenses are any offenses related to drug and narcotics. The tracts eligible to receive a LIHTC boost are on the left side of the chart. The model includes population controls and year fixed effects from each year in 2013-2016. Tracts are restricted to served by police departments in Dallas, Fort Worth, Austin, and Houston. There appears to be small discontinuity pointing to drug offenses being higher in census tracts with poverty rates over 15%.

Tables

Table 2.1: Summary Statistics

	Control			Treated		
	Mean	Min	Max	Mean	Min	Max
Demog. Characteristics						
Share Children (< 18)	0.13	0	0.48	0.13	0	0.48
	(0.06)			(0.06)		
Share Elderly (> 65)	0.38	0	0.63	0.39	0	0.64
	(0.05)			(0.04)		
Share Female	0.50	0	0.66	0.50	0	0.69
	(0.05)			(0.04)		
Share Hispanic	0.59	0	1.00	0.73	0	1.00
	(0.26)			(0.20)		
Share Black	0.01	0	0.14	0.02	0	0.12
	(0.02)			(0.02)		
LIHTC Characteristics						
Total Units	0.70	0	373	1.50	0	220
	(10.12)			(13.15)		
Low Income Units	0.61	0	3073	1.28	0	185
	(8.63)			(11.05)		
Amount Awarded	7,235	0	4,460,096	15,353	0	1,612,000
	(108,036)			(129,830)		
N		5,031			7,063	

Notes: This table compares the treated tract, those with poverty levels between 5% and 15%, with the control group of tracts, with poverty rates of above 15% up to 25%. There are no significant differences in the demographic characteristics of the two groups. However, on average, the treated group has a higher level of LIHTC units and received more LIHTC funds. This supports the hypothesis that the additional tax credit boost incentivizes construction in low-poverty tracts. Standard deviations are in parentheses.

Table 2.2: First Stage RD Results

	Log LIHTC Units		Flow LIHTC Units	
	(1)	(2)	(3)	(4)
Panel A: Parametric Estimation				
RD Estimate (linear)	0.0458***	0.0479***	0.771**	0.834**
	(0.0159)	(0.0159)	(0.387)	(0.387)
RD Estimate (quadratic)	0.0480***	0.0500***	0.813**	0.868**
	(0.0154)	(0.0154)	(0.386)	(0.387)
Panel B: Non-parametric Estimation				
RD Estimate (linear)	0.0820**	0.0823**	1.875**	1.867**
	(0.0387)	(0.0390)	(0.933)	(0.931)
RD Estimate (quadratic)	0.0827*	0.0835*	1.805	1.831*
	(0.0486)	(0.0478)	(1.116)	(1.085)
Observations	12,094	12,094	12,094	12,094
Controls	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows results for the first stage fuzzy RD estimation for the log transformation of LIHTC units at the census tract level and the number of units. The model is estimated both parametrically and non-parametrically, which uses a triangular kernel function to assign observation weights relative to their proximity to the cutoff. The model uses a local linear and quadratic polynomial for the to approximate $f(r_i)$. Results in columns (2) and (4) include tract by year level demographic controls as well as controls for the respective year. For the non-parametric model, which is the preferred specification, the results show an 8% increase in LIHTC units for tracts below the 15% poverty rate cutoff, which corresponds to approximately two additional units. Errors are clustered at the county level.

Table 2.3: Reduced Form Results for Burglaries

	(1) $Year_t$	(2) $Year_t$	(3) $Year_{t+1}$	(4) $Year_{t+1}$	(5) $Year_{t+2}$	(6) $Year_{t+2}$
Panel A: Log Offenses						
Linear Estimate	-0.00153	0.0284	-0.0321	0.0590	-0.124	-0.0667
	(0.256)	(0.236)	(0.256)	(0.225)	(0.281)	(0.251)
Panel B: Offenses per 1000 persons						
Linear Estimate	0.423	0.932	-0.417	0.311	-0.793	-0.204
	(1.044)	(1.017)	(1.448)	(1.310)	(1.798)	(1.588)
Observations	5,046	5,046	5,046	5,046	5,046	5,046
Controls	No	Yes	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows reduced form estimates for the effects of the 2013 policy change on the log transformation of burglaries in Dallas, Fort Worth, Austin, and Houston. The results are broken out by years relative to the LIHTC award year. Developments are required to be placed in service by the end of the second year following the award. Results for the third year post award, which can be found in Appendix B, are consistent with years 0-2. Errors are clustered at the county level.

Table 2.4: Reduced Form Results for Violent Offenses

	(1)	(2)	(3)	(4)	(5)	(6)
	$Year_t$	$Year_t$	$Year_{t+1}$	$Year_{t+1}$	$Year_{t+2}$	$Year_{t+2}$
Panel A: Log Offenses						
Linear Estimate	0.0687	0.0697	0.0170	0.0669	0.160	0.230
	(0.187)	(0.173)	(0.187)	(0.193)	(0.197)	(0.216)
Panel B: Offenses per 1000 persons						
Linear Estimate	1.430	1.457	1.964	2.340*	2.101	2.471*
	(1.240)	(1.158)	(1.417)	(1.391)	(1.432)	(1.417)
Observations	5,046	5,046	5,046	5,046	5,046	5,046
Controls	No	Yes	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows reduced form estimates for the effects of the 2013 policy change on the log transformation of violent offenses in Dallas, Fort Worth, Austin, and Houston. The results are broken out by years relative to the LIHTC award year. Developments are required to be placed in service by the end of the second year following the award. Results for the third year post award, which can be found in Appendix B, are consistent with years 0-2. Errors are clustered at the county level.

Table 2.5: Reduced Form Results for Drug Offenses

	(1) $Year_t$	(2) $Year_t$	(3) $Year_{t+1}$	(4) $Year_{t+1}$	(5) $Year_{t+2}$	(6) $Year_{t+2}$
Panel A: Log Offenses						
Linear Estimate	-0.0419	-0.0177	-0.0381	-0.0137	-0.0591	-0.0577
	(0.120)	(0.121)	(0.128)	(0.128)	(0.134)	(0.133)
Panel B: Offenses per 1000 persons						
Linear Estimate	-0.317	-0.235	-0.174	-0.102	-0.247	-0.2086
	(0.402)	(0.378)	(0.389)	(0.385)	(0.376)	(0.372)
Observations	5,046	5,046	5,046	5,046	5,046	5,046
Controls	No	Yes	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows reduced form estimates for the effects of the 2013 policy change on the log transformation of drug related in Dallas, Fort Worth, Austin, and Houston. The results are broken out by years relative to the LIHTC award year. Developments are required to be placed in service by the end of the second year following the award. Results for the third year post award, which can be found in Appendix B, are consistent with years 0-2. Errors are clustered at the county level.

Chapter 3

Crowd Out Effects of Low-Income Housing in High-Opportunity Neighborhoods

3.1 Introduction

Housing supply is an issue across the United States. Housing shortages have made affordable housing even more scarce and as the number of cost-burdened households increases this has become an even more pressing issue (Betancourt et al., 2022). The federal government's most important tool for addressing affordable housing supply is the Low Income Housing Tax Credit (LIHTC) program, which awards developers tax credits to help offset the cost of multifamily housing construction, given that the housing is made available to low-income individuals (Keightley, 2023). A natural question in regard to this program for economists is whether this government-funded housing program crowds out privately-funded housing construction.

Studies that have tackled this question have traditionally relied on pre-LIHTC government housing or 2000 census data that evaluated the effects of LIHTC in its beginning stages (Murray, 1983, 1999; Malpezzi & Vandell, 2002; Eriksen, 2009; Baum-Snow & Marion, 2009; Eriksen & Rosenthal, 2010). However, the program has not only grown in size, but has also undergone significant changes in the way it awards credits in recent years. In several states, the LIHTC program has incentivized the construction of housing in affluent neighborhoods, deviating from the standard practice of incentivizing construction in low-income areas. Given this shift, researchers have called into question whether the results found in previous empirical studies are consistent with the affordable housing landscape today (Eriksen & Rosenthal, 2010). An updated evaluation of crowd out effects of government-funded housing is necessary to address these concerns.

In this paper, I estimate the effects of LIHTC construction on the stock and flow of housing units overall as well as other housing outcomes, such as vacancy rates for rental and non-rental units. I use a policy change in the state of Texas that explicitly gave preference and awarded additional tax credits to developers constructing LIHTC housing in low-poverty neighborhoods, thereby speaking to the literature gap left by the changing dynamic of the LIHTC program. Using the rule introduced by the policy change, I construct two comparable groups of census tracts with poverty rates above and below the threshold to estimate a difference-in-difference model.

Unlike previous studies, I do not find that LIHTC units crowd out construction of new units. This suggest that in affluent areas LIHTC housing may not be a substitute for market-priced housing. I also find that in affluent areas, neighborhoods eligible for a LIHTC tax credit boost experienced increased vacancy rates for rental units and lower percentages of owner-occupied units. The higher vacancy rate for the rental market may point to new additional rental units that have not yet been occupied. This may suggest that LIHTC construction does not crowd out private rental unit construction. There appears to be no impact on either median rents or home prices.

This way of awarding tax credits was a big shift away from prior rules where areas with high-poverty rates were prioritized. The policy introduced a rule where census tracts with poverty rates below 15% qualified for a 30% tax credit boost. Using this poverty rate threshold, I construct two comparable groups of census tracts with poverty rates above and below the threshold to estimate a difference-in-difference model for five years before the 2012 policy change and five years after.

I use microdata from the Texas Department of Housing and Community Affairs (TDHCA) on the universe of LIHTC properties constructed between 2007 and 2016. These data provide detailed information on location, tax credit amounts, and the scoring outcomes. I pair these with ACS 5-year estimates on housing unit characteristics at the census tract level. Given the nature of these estimates, the outcomes are presented for the entirety of the five year period to address the bias that would occur with ACS estimates from overlapping time periods.

This study helps fill in the gaps in understanding of crowd out effects of the LIHTC program given the relatively recent changes in policy. I verify the findings of previous empirical research in the context of affluent neighborhoods. However, given the limited data on the housing market, additional research is needed to fully understand the mechanisms driving these effects.

In the following section, the paper explores the related research. Section 3.3 provides more background on the LIHTC program and the specific Texas policy change. Section 3.4 explains the data used in the study. Section 3.5 details the estimation strategy used and section 3.6 presents the findings. Section 3.7 concludes.

3.2 Background

The Low Income Housing Tax Credit (LIHTC) program was established in the late 1980s and is the primary source of government funding for affordable rental housing in the United States, covering about 90% of subsidized housing. The program is administered at the state level and award private developers with non-refundable tax credits to fund the construction or rehabilitation of affordable multifamily housing. The program requires that the housing developments allocate either at least 20% of units to tenants with income below 50% of the area's median income (AMI) or 40% of units to tenants with income below 60% of AMI. Rent for the low-income units is typically capped at 30% of 60% of AMI, which is determined by HUD based on census or ACS data.

Notably, rents for units in LIHTC properties do not necessarily cater to families with the lowest incomes as other government-subsidized housing programs like the Section 8 Housing Voucher Program. A study from the early stage of the program found that LIHTC unit families had, on average, income levels that amounted to 45% of AMI where are Section 8 households had average income of 22% of the AMI (McClure, 2006). Given that LIHTC housing caters to families with moderate income, researchers have question whether it has become a substitute for privately-funded rental housing.

The economic literature in this space is limited for recent years, and has primarily focused on the LIHTC program as it was in the 1990s, when the program was significantly young, or subsidized housing that preceded this program. A series of papers (Murray, 1983, 1999) estimate the crowd out effect of public housing, before LIHTC developments. They find small crowd out effects, which suggests that privately funded rental units are not substitutes for housing that catered primarily to very low-income families. Since the LIHTC program was introduced, it has shifted the way government subsidized housing is allocated. One of the key distinctions between this and traditional public housing is that the income limit to qualify for this type of housing is much higher. The rents for these developments are also capped at a higher level than what was typical of public housing. In turn, researchers have revisited the crowd out effect, given that these units are likely better substitutes for private housing than public housing ever was. Baum-Snow & Marion (2009) estimate a 20% crowd out of rental units as a result of LIHTC units. Eriksen & Rosenthal (2010) present much higher estimates of 100% crowd out. In other words, nearly all LIHTC development is offset by a reduction in the number of newly built unsubsidized rental units. They also find that LIHTC development has very small effects on owner-occupied housing, which are not statistically significant.

The theory that drives this crowd out result is that demand for housing is inelastic, while supply is elastic. The figure 3.1, replicated from Eriksen & Rosenthal (2010), shows that elastic supply of housing implies that high levels of crowd out will occur otherwise rents will be forced to decrease. The implication of this finding is that government funded housing does not add to the overall housing stock and fills a place in the market that would have been captured by the private market regardless. However, the context of these analyses is outdated as even Eriksen & Rosenthal point out that placement of LIHTC housing has shifted from high-poverty areas to low-poverty areas. In this context, the argument that LIHTC housing is a perfect substitute for market-rate housing may not be as obvious because, although LIHTC rates may not cater to the very high-poverty populations, it may still not be comparable to the market rate units in affluent areas. This paper evaluates that distinction.

3.3 Texas Setting

Due to ongoing litigation, which culminated in a Supreme Court ruling, Texas was forced to reevaluate how it allocated LIHTC awards. The ruling stated that Texas violated the Fair Housing Act by disproportionately funding affordable housing in "predominantly black inner-city areas and too few in predominantly white suburban neighborhoods"(*Texas Department of Housing and Community Affairs v. Inclusive Communities Project, Inc.*, 2015). In 2012, the Texas Department of Housing (TDHCA), which is responsible for the allocation and distribution of LIHTC funds, the included a provision in the annual QAP to provide an addtional tax credit to projects located in census tracts with poverty rates at or below 15%. In 2013, the QAP cemented the focus on low-poverty, what were called high-opportunity areas, by awarding additional points when scoring applications that proposed developments in these areas. This meant that there was a greater likelihood that a project would be funded, plus the project would receive a credit boosts. TDHCA releases the list of tracts that eligible for a funding boost and application points well in advance.

One unique feature of Texas' LIHTC allocation process is that the TDHCA encourages developers to submit a pre-application, which is a short version of the full application. The pre-applications are scored and data is published online before the official applications are due. The pre-application process is designed to give developers a chance to scope out their competition for the funding year and make a strategic decision over whether to complete the full application or wait. Applicants also receive additional points in the final scoring if they submit a pre-application. This process invalidates the assumption that funding to developments is randomly assigned by nature of a competitive application process, which is central to some related research (Diamond & McQuade, 2019). Rather, the pre-application process allows developers to be strategic and apply when they have the best chances of getting funding.

Funding Formula

Given the size of these credits, LIHTC is a popular program. The amount of the annual credit is calculated using the following formula:

$$T = .09C \times B$$

Where T is the dollar amount of the credit, C is the development costs, excluding land acquisition, and B is the eligible basis.

As a concrete example, suppose a developer, whose project will cost $1,000,000, applies for a tax credit. This specific project has 100 housing units, 80 of which are allocated to low-income families ($B = 0.8$). If selected, this developer will receive a $72,000 tax credit every year for ten years. With the 30% basis boost ($B' = 1.1$), the annual credit increases to $99,000. These credits are adjusted for inflation. In practice, developers typically receive tax credits to cover 70% of their entire construction costs, and even more when they site developments in qualifying areas, in this case 99%.

3.4 Data

The primary data used in this project is from the Texas Department of Housing and Community Affairs (TDHCA). This agency is responsible for writing the Texas QAP and awarding LIHTC. I use their annual Site Demographics Characteristics Report, annual report on developments awarded, and property inventory list. From the report, I draw the poverty rate used to determine tract eligibility as well as tract funding eligibility status, which is necessary due to restrictions of awarding tracts with an already large concentration of LIHTC units. The poverty rate published in these reports is particularly important because it does not necessarily coincide with current ACS or Census data. For example, for the properties awarded in 2012, TDHCA used the 2005-2009 5-year ACS estimates. Previous to that, they relied on the 2000 census. The annual report on sites awarded provides details on the sites that received LIHTC, including the award amount and eligible LIHTC units out of the total units. The property inventory is more high level and is used for historical data. It also serves to eliminate developments that may have received an award, but were never put into service or whose award was revoked.

The data on LIHTC construction comes from the Texas Department on Housing and Community Affairs. I limit the sample to developments awarded between 2007 and 2016 which reflect five years before and after the policy intervention. The other benefit of this time period is that award rules were consistent for this period.

I also use ACS 5-year estimates for the 2007-2011 period and the 2012-2016 period. I rely on these estimates for population controls as well as housing estimates. The data from the ACS is comparable to housing estimates collected as part of the 2000 and previous decennial census, which are used in related literature. Questions related to housing units were not included in the 2010 and 2020 census, thus the ACS estimates are the next best estimates. Due to the nature of how estimates for small geographic areas, like tracts, are collected I cannot evaluate the year by year change of the different outcomes. Instead, I aggregate LIHTC construction variables to their respective 5 year periods and look at the total change by each period.

Also, I use geography relationship files to normalize all census tracts and their characteristics to their 2000 geographic boundaries. I do this with the help of Brown University's Longitudinal Tract Data Base program. The reasoning is that for the historical analysis and placebo, the census tracts on the THDCA property inventory use 2010 census tracts and need to be converter to their 2000 equivalent to get property rates and demographic characteristics. I also rely on the U.S. Department of Agriculture (USDA) categorization of census tracts into Rural-Urban Commuting Areas (RUCA). The RUCA categories allow me to approximate which census tracts obtain rural classification for LIHTC purposes and drop them from the sample because rural areas have a set-aside budget under the LIHTC program, which may impact how often developments in those areas are chosen.

3.5 Empirical Strategy

I cannot estimate the effect of LIHTC developments on private development directly because there may be unobserved variables that influence developers' location decisions, such as potential fruitful rental markets or low land costs. Instead, I use the LIHTC program rule in Texas that awards additional tax credits to developments in low-poverty areas. I constructed a treatment and control group based on comparable census tracts that were above and below the threshold for qualifying for the additional tax credit boost. The treatment group is made up of census tracts that had a poverty rate below 15% every year and would have qualified for the tax credit boost every year. The control group is made up of tracts that had a poverty rate of above 15% every year in the analysis time period. I restrict the bandwidth of poverty rate to 10% above and below the threshold. As a first step, I use an event study model to estimate the effect of the LIHTC units between the two groups before and after the policy change. Figure 3.2 shows this change. Prior to the policy intervention, the difference in the effect was not statistically different from zero for most years. There is an evident change after the policy was instituted that indicates that census tracts that qualified for the additional tax credits, obtained more LIHTC units over the control group.

Due to the availability of housing data from the ACS 5 year estimates, I cannot estimate the year by year effects. Instead, I separate the groups into a pre and post period where the pre-period is from 2007-2011 and the post-period is from 2012-2016. I then estimate the difference-in-difference effect for both the LIHTC units and various outcomes related to the overall housing market. Table 3.1 shows the comparison of means between the treatment and control groups as well and lists the various outcomes available for the analysis.

I only use urban tracts in this analysis. I also exclude Texas service regions 11 and 13, which encompass the entire border region. I exclude these regions because they have different eligibility rules to qualify for LIHTC. Besides that, I only use housing projects designated as general housing, as opposed to supportive housing or other non-qualifying categories. Tables 1 and 2 show the summary statistics for the 2009 and the 2012 samples, respectively.

DID Setup

The research design compares housing outcomes for census tracts above and below the 15% poverty rate threshold before and after the policy change. I use the following general model to estimate these effects:

$$\text{log}Y_i = \alpha + \beta_1(\text{time} \times \text{treat}) + \beta_2\text{treat} + \beta_3\text{time} + \gamma X_i + \epsilon_i \tag{3.1}$$

The outcome variable Y_i is the census tract-level outcome, such as total housing units. The β coefficients estimate the effect of the treatment, the treatment group specific effect, and the time trend. X_i is a vector of tract specific population controls. One caveat to this analysis is that because poverty rate below 15% only increases the likelihood of LIHTC construction in a census tract, β_3 is a "fuzzy" treatment effect that captures the intent-to-treat (ITT) effect rather than the local average treatment effect (LATE) (De Chaisemartin & d'Haultfoeuille, 2018).

First Stage

The first piece of the analysis is understanding how the policy rule affected LIHTC construction. Following the event study, there is reason to believe that census tracts with poverty rate below 15% experienced an increase in LIHTC unit construction. I estimate the following model for LIHTC units that mirrors equation 3.1:

$$\log LIHTC_i = a + b_1(\text{time} \times \text{treat}) + b_2\text{treat} + b_3\text{time} + \delta X_i + \upsilon_i \tag{3.2}$$

The outcome variable refers to log transformation of LIHTC units in the sample used to for the main estimating equation. However, because the model and data only allow for in pre-period and one post-period, I aggregate the LIHTC units for all developments constructed within the five year years before and after the intervention. The unit of observation is still the census tract i. Here b_i is the coefficient for the interaction term between a dummy variable for a tract receiving treatment and a dummy for pre and post intervention.

Table 3.2 shows the results of this first stage estimation of DID on the treatment. Columns (1) and (2) show results for the sample of census tracts before they are aggregated to two periods as a comparison of the event study framework. Columns (3) and (4) are the estimates that follow from equation 3.2. Note that first estimates are roughly one-fifth of the last two, which is in line with the aggregation procedure. I find that tract with poverty rates below 15% received 30% more LIHTC units than census tracts with poverty rates above the threshold, or approximately 8.5 more units.

3.6 Results

To evaluate crowd out effects I compare the results from the first stage to the ITT estimate for new construction. Results for the DID of the relevant outcome variables are presented in table 3.3. The second column represents units constructed within the ACS time period and the first column captures the stock of housing units. From this table, census tracts with poverty rates below 15% obtained 8.6 additional units over tracts in the control group. Relative the DID estimate on the treatment, treated tracts, on average, received 10 additional units non-LIHTC units, which suggest that LIHTC construction does not necessarily crowd out private construction. However, the total new construction difference may be made up of non-rental housing, which is not theoretically subject to crowding out. Due to data limitations, I cannot disaggregate effect for rental housing and owner-occupied housing, but I estimate the effects on additional related outcome to provide some context. The results also show that the stock of housing is marginally less for the treatment group. Which shows that there isn't significant growth of these tracts, but perhaps more rehabilitation of older housing or replacement.

I estimate the DID model for various outcomes separated by the rental market and the homeowner market in table 3.4. There appear to be no difference on either the median rents or home values between the treatment and control groups. I see statistically significant effects for the rental vacancy rate, with census tract with poverty rates experiencing rental vacancy of 2% higher. The results also show that eligible tracts have 14% fewer owner-occupied units than non-eligible tracts. The higher rental vacancy rate may suggest there is additional rental housing that is not yet occupied and lower owner-occupied housing could suggest that non-rental housing is not the main component of new construction. However, more analysis is needed to understand the underlying mechanisms and is left for future research.

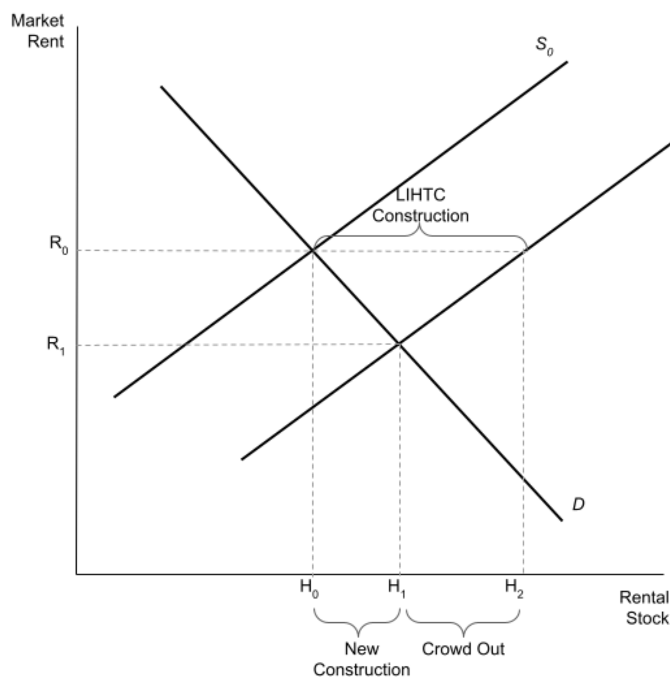
3.7 Conclusion

Government subsidized housing has undergone significant changes in the last several decades. While at one point, the most notable type of government funded housing was what was referred to as section 8 public housing, today, most government is funded through the LIHTC program. This program accounts for 90% of all public housing. While previous public housing served a population with very low incomes, LIHTC has higher income limits and higher rent ceilings. A natural question that arises from this shift in the public economics literature is whether government public housing crowds out privately funded housing. The literature on the topic is compelling, but limited. This area of research has become even more limited in recent years as the older research tells the story of LIHTC in the 1990s. Since then, the program has continued to evolve and in particular has changed in a big way by moving from funding housing in high-poverty areas to funding housing in affluent areas. It's important to revisit the question of crowd out effect, because of this change.

This paper attempts to address this gap in the literature while exploring a policy change in Texas that incentivized construction of LIHTC housing in low-poverty neighborhoods. The results indicate the LIHTC construction is not completely offset by crowd out of privately-funded construction. While this result is not line with previous studies, it is reasonable to expect that in affluent areas market-priced rental housing is not a perfect substitute for LIHTC housing, despite the its high rent ceilings. Its suggests that LIHTC housing may still be a useful tool in making housing affordable to families with lower incomes that may not be able to access market-priced housing. Additional analysis is needed to understand the mechanisms driving the growth on private-funded housing units effect and is left for future research.

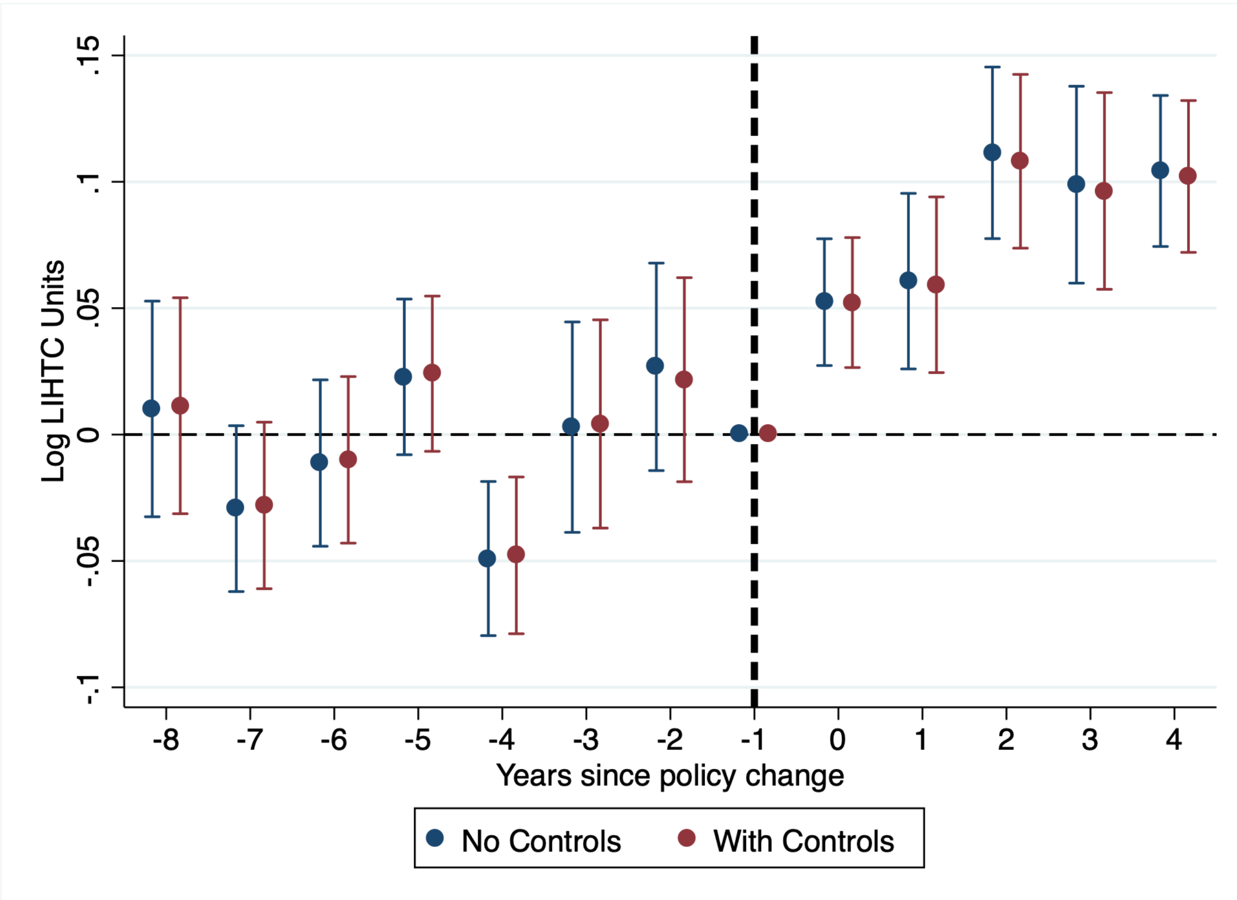
Figures

Figure 3.1: Theoretical Crowd Out Model



Notes: This figure is a replication of figure 2 from Eriksen & Rosenthal (2010). This chart shows what happens when supply increases from S_0 to S_1 due to LIHTC construction. In this framework, LIHTC construction decreases the market rent and increases the rental stock. However, the increase in rental stock is less in magnitude than LIHTC construction. As demand becomes more inelastic, the crowd out effect will increase.

Figure 3.2: Event Study of Policy Change



Notes: The chart shows an event study plot that estimates the difference in effects between the treatment and control groups. After the policy change, census tracts that qualified for additional LIHTC tax credits experienced a steady increase in log LIHTC units over the control group.

Tables

Table 3.1: Summary Statistics

	Control			Treated		
	Mean	Min	Max	Mean	Min	Max
Demographic Characteristics						
Log Med. Income	10.64	10	12.20	11.15	10	12.29
	(0.32)			(0.28)		
Log Population	8.34	5	9.77	8.50	7	10.51
	(0.44)			(0.51)		
Share Elderly (> 65)	0.11	0	0.40	0.12	0	0.45
	(0.05)			(0.06)		
Share Male	0.50	0	0.99	0.49	0	0.86
	(0.04)			(0.03)		
Share Black	0.15	0	0.98	0.10	0	0.94
	(0.19)			(0.11)		
Housing Characteristics						
LIHTC Units	4.32	0	448	5.07	0	190
	(26.19)			(23.87)		
Total Housing Units	1926.73	4	5782	2337.92	304	14422
	(814.29)			(1214.24)		
Total New Units	14.66	0	540	59.76	0	1944
	(39.74)			(128.67)		
N	1,670			1,380		

Notes: This table compares the treated tracts, those with poverty levels between 5% and 15%, with the control group of tracts, with poverty rates of above 15% up to 25%. There are no significant differences in the demographic characteristics of the two groups. However, on average, the treated group has a higher level of LIHTC units and housing in general. This supports the hypothesis that the additional tax credit boost incentivizes construction in low-poverty tracts. Standard deviations are in parentheses.

Table 3.2: First Stage DID on Treatment

	(1) LI Units	(2) Log LI Units	(3) LI Units	(4) Log LI Units
DID	1.615***	0.0664***	8.569***	0.301***
	(0.327)	(0.0125)	(1.662)	(0.0445)
Constant	-5.740	-0.334	-0.232	-0.500
	(5.088)	(0.250)	(24.22)	(1.140)
Observations	13,597	13,597	3,022	3,022
R-squared	0.011	0.017	0.042	0.057
Controls	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table shows results for the first stage DID estimation for the log transformation of LIHTC units at the census tract level and the number of units. Results in columns (1) and (2) show the results at the tract-level that correspond with Figure 3.2. The results in column (3) and (4) are from the model in Equation 3.2 and use the aggregate number of LIHTC units over the 5 year pre- and post- period. The results show a 30% increase in LIHTC units for tracts below the 15% poverty rate cutoff, which corresponds to approximately 8.6 additional units. All estimates include population controls. Errors are clustered at the county level.

Table 3.3: DID on Housing Outcomes

	(1) Log Total Units	(2) Total Units	(3) Log New Const.	(4) New Const.
DID	-0.0355***	-35.404	0.286**	19.157**
	(0.0112)	(19.539)	(0.111)	(5.567)
Observations	3,022	3,022	3,022	3,022
R-squared	0.957	0.802	0.307	0.206
Controls	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Table shows results for the estimation equation 3.1. Results in columns (1) and (2) estimate the DID of the stock of total housing across the treatment and control groups at the tract level. Results in columns (3) and (4) focus on new construction, which refers to housing units built during the respective 5-year estimation time period before and after the policy change. Both variables include rental and non-rental residential housing units. All estimates include population controls. Errors are clustered at the county level. The results show that while LIHTC specific construction adds about 8.5 more units in census tracts with poverty rates below 15%, total new construction is of about 19 more units to this group of tracts over the control group. This does not suggest crowd out.

Table 3.4: DID on Outcomes by Rental and Homeowner Markets

	Vacancy Rate (1)	Log Occ Units (2)	Log Med. Value (3)
Panel A: Rental Market			
DID	1.987**	-0.0189	0.0403
	(0.833)	(0.0266)	(0.156)
Panel B: Homeowner Market			
DID	0.227	-0.141**	0.0421
	(0.257)	(0.0609)	(0.0281)
Observations	3,022	3,022	3,022
Controls	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: The table shows results for the estimation equation 3.1. Results in Panel A estimates the DID of outcomes for the rental market and Panel B presents the homeowner market results. This table shows that while new construction in census tracts that qualify for the LIHTC tax boost is higher by 28.6%, the percentage of owner occupied units is lower by 14%. This suggests that the growth in number of units in the treatment group is likely not due to owner-occupied units. The higher vacancy rate for the rental market may also point to new additional rental units that have not yet been occupied. This may suggest that LIHTC construction does not crowd out private rental unit construction.

Bibliography

Abrams, B., & Schmitz, M. (1978). The crowding out effect on government transfers on.

Abrams, B. A., & Schmitz, M. D. (1984). The crowding-out effect of governmental transfers on private charitable contributions: cross-section evidence. *National Tax Journal*, *37*(4), 563–568.

Andreoni, J. (1989). Giving with impure altruism: Applications to charity and ricardian equivalence. *Journal of political Economy*, *97*(6), 1447–1458.

Andreoni, J. (1990). Impure altruism and donations to public goods: A theory of warm-glow giving. *The economic journal*, *100*(401), 464–477.

Andreoni, J. (1993). An experimental test of the public-goods crowding-out hypothesis. *The American economic review*, 1317–1327.

Andreoni, J., Payne, A., & Smith, S. (2014). Do grants to charities crowd out other income? evidence from the uk. *Journal of Public Economics*, *114*, 75–86.

Andreoni, J., & Payne, A. A. (2003). Do government grants to private charities crowd out giving or fund-raising? *American Economic Review*, *93*(3), 792–812.

Andreoni, J., & Payne, A. A. (2011). Is crowding out due entirely to fundraising? evidence from a panel of charities. *Journal of public Economics*, *95*(5-6), 334–343.

Andreoni, J., & Payne, A. A. (2013). Charitable giving. In *Handbook of public economics* (Vol. 5, pp. 1–50). Elsevier.

Baum-Snow, N., & Marion, J. (2009). The effects of low income housing tax credit developments on neighborhoods. *Journal of Public Economics*, *93*(5-6), 654–666.

Bergstrom, T., Blume, L., & Varian, H. (1986). On the private provision of public goods. *Journal of public economics*, *29*(1), 25–49.

Betancourt, K., Gardner, S., & Palim, M. (2022). *The u.s. housing shortage from a local perspective.* Fannie Mae.

Billings, K. (2020). *The emergency food assistance program (tefap): Background and funding* (Tech. Rep.). Congressional Research Service.

Bitler, M. P., Hoynes, H. W., & Schanzenbach, D. W. (2020). The social safety net in the wake of covid-19. *Brookings Papers on Economic Activity*, *2020*(Special Edition), 119–158.

Bjerk, D. (2010). Thieves, thugs, and neighborhood poverty. *Journal of Urban Economics*, *68*(3), 231–246.

Borgonovi, F. (2006). Do public grants to american theatres crowd-out private donations? *Public choice*, *126*(3), 429–451.

Cabili, C., Eslami, E., Briefel, R., et al. (2013). *White paper on the emergency food assistance program (tefap)* (Tech. Rep.). Mathematica Policy Research.

Carter, W. H., Schill, M. H., & Wachter, S. M. (1998). Polarisation, public housing and racial minorities in us cities. *Urban Studies*, *35*(10), 1889–1911.

Cattaneo, M. D., Idrobo, N., & Titiunik, R. (2019). *A practical introduction to regression discontinuity designs: Foundations*. Cambridge University Press.

Chetty, R., & Hendren, N. (2015). The effects of neighborhoods on children's long-term outcomes: Quasi-experimental estimates for the united states. *Unpublished Working Paper*.

Chetty, R., Hendren, N., & Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, *106*(4), 855–902.

Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review*, *108*(10), 3028–56.

Clotfelter, C. T. (1975). The effect of school desegregation on housing prices. *The Review of Economics and Statistics*, 446–451.

Cornes, R., & Sandler, T. (1994). The comparative static properties of the impure public good model. *Journal of public economics*, *54*(3), 403–421.

Dawkins, C. (2013). The spatial pattern of low income housing tax credit properties: Implications for fair housing and poverty deconcentration policies. *Journal of the American Planning Association*, *79*(3), 222–234.

De Chaisemartin, C., & d'Haultfoeuille, X. (2018). Fuzzy differences-in-differences. *The Review of Economic Studies*, *85*(2), 999–1028.

De Wit, A., & Bekkers, R. (2017). Government support and charitable donations: A meta-analysis of the crowding-out hypothesis. *Journal of Public Administration Research and Theory*, *27*(2), 301–319.

Diamond, R., & McQuade, T. (2019). Who wants affordable housing in their backyard? an equilibrium analysis of low-income property development. *Journal of Political Economy*, *127*(3), 1063–1117.

Dougherty, C. (2020). *Golden gates: Fighting for housing in america*. Penguin.

Duflo, E. (2001). Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment. *American economic review*, *91*(4), 795–813.

Eckel, C. C., Grossman, P. J., & Johnston, R. M. (2005). An experimental test of the crowding out hypothesis. *Journal of Public Economics*, *89*(8), 1543–1560.

Edin, K., & Lein, L. (1998). The private safety net: The role of charitable organizations in the lives of the poor. *Housing Policy Debate*, *9*(3), 541–573.

Ellen, I. G., Horn, K., Kuai, Y., Pazuniak, R., & Williams, M. D. (2015). Effect of qap incentives on the location of lihtc properties.

Ellen, I. G., O'Regan, K., & Voicu, I. (2009). Siting, spillovers, and segregation: A reexamination of the low income housing tax credit program. *Housing markets and the economy: Risk, regulation, policy; essays in honor of Karl Case*, 233–67.

Ellen, I. G., & Turner, M. A. (1997). Does neighborhood matter? assessing recent evidence. *Housing policy debate*, *8*(4), 833–866.

The emergency food assistance program (tefap): Distribution national survey (Tech. Rep.). (2015). Washington State Department of Agriculture.

Eriksen, M. D. (2009). The market price of low-income housing tax credits. *Journal of Urban Economics*, *66*(2), 141–149.

Eriksen, M. D., & Rosenthal, S. S. (2010). Crowd out effects of place-based subsidized rental housing: New evidence from the lihtc program. *Journal of Public Economics*, *94*(11-12), 953–966.

Fabricant, S. (1970). Philanthropy in the american economy: An introduction. In *The changing position of philanthropy in the american economy* (pp. 3–30). NBER.

Feeding America. (2022). *Our lawmakers must invest in tefap.* Retrieved 2022-08-14, from `https://www.feedingamerica.org/take-action/advocate/federal-hunger-relief-programs/the-emergency-food-assistance-program`

Freedman, M., & McGavock, T. (2015). Low-income housing development, poverty concentration, and neighborhood inequality. *Journal of Policy Analysis and Management*, *34*(4), 805–834.

Freedman, M., & Owens, E. G. (2011). Low-income housing development and crime. *Journal of Urban Economics*, *70*(2-3), 115–131.

Glaeser, E., Kahn, M. E., & Rappaport, J. (2008). Why do the poor live in cities? the role of public transportation. *Journal of Urban Economics*.

Glazer, A., & Konrad, K. A. (1996). A signaling explanation for charity. *The American Economic Review*, *86*(4), 1019–1028.

Gould Ellen, I., & Voicu, I. (2006). Nonprofit housing and neighborhood spillovers. *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, *25*(1), 31–52.

Harcourt, B. E., & Ludwig, J. (2006). Broken windows: New evidence from new york city and a five-city social experiment. *The University of Chicago Law Review*, 271–320.

Hungerman, D. M. (2005). Are church and state substitutes? evidence from the 1996 welfare reform. *Journal of public economics*, *89*(11-12), 2245–2267.

IRS. (2016). *Irs makes electronically filed form 990 data available in new format.* Retrieved 2021-10-17, from `https://www.irs.gov/newsroom/irs-makes-electronically-filed-form-990-data-available-in-new-format`

Jacob, B. A. (2004). Public housing, housing vouchers, and student achievement: Evidence from public housing demolitions in chicago. *American Economic Review*, *94*(1), 233–258.

Johnson, R. C. (2011). *Long-run impacts of school desegregation & school quality on adult attainments* (Tech. Rep.). National Bureau of Economic Research.

Katz, B., & Turner, M. A. (2007). Rethinking us rental housing policy. *Joint Center for Housing Studies of Harvard University, March*.

Katz, L. F., Kling, J. R., & Liebman, J. B. (2001). Moving to opportunity in boston: Early results of a randomized mobility experiment. *The Quarterly Journal of Economics*, *116*(2), 607–654.

Keightley, M. P. (2023). *An introduction to the low-income housing tax credit.* Library of Congress, Congressional Research Service Washington, DC.

Kim, M., & Van Ryzin, G. G. (2014). Impact of government funding on donations to arts organizations: A survey experiment. *Nonprofit and Voluntary Sector Quarterly*, *43*(5), 910–925.

Kline, P., & Moretti, E. (2014). Local economic development, agglomeration economies, and the big push: 100 years of evidence from the tennessee valley authority. *The Quarterly Journal of Economics*, *129*(1), 275–331.

Kling, J. R., Liebman, J. B., & Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, *75*(1), 83–119.

Kling, J. R., Ludwig, J., & Katz, L. F. (2005). Neighborhood effects on crime for female and male youth: Evidence from a randomized housing voucher experiment. *The Quarterly Journal of Economics*, *120*(1), 87–130.

Kotchen, M., & Wagner, K. R. (2019). *Crowding in with impure altruism: Theory and evidence from volunteerism in national parks* (Tech. Rep.). National Bureau of Economic Research.

Kotchen, M. J. (2006). Green markets and private provision of public goods. *Journal of Political Economy*, *114*(4), 816–834.

Lecy, J., & Thornton, J. (2016). What big data can tell us about government awards to the nonprofit sector: Using the faads. *Nonprofit and Voluntary Sector Quarterly*, *45*(5), 1052–1069.

Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of economic literature*, *48*(2), 281–355.

Lens, M. C. (2014). The impact of housing vouchers on crime in us cities and suburbs. *Urban Studies*, *51*(6), 1274–1289.

Lilley, A., & Slonim, R. (2014). The price of warm glow. *Journal of Public Economics*, *114*, 58–74.

Malpezzi, S., & Vandell, K. (2002). Does the low-income housing tax credit increase the supply of housing? *Journal of Housing Economics*, *11*(4), 360–380.

Martin, R. (2020). *Lines at food banks grow longer.* Retrieved 2022-08-17, from `https://www.npr.org/transcripts/836719896`

Marwell, N. P. (2004). Privatizing the welfare state: Nonprofit community-based organizations as political actors. *American sociological review*, *69*(2), 265–291.

McClure, K. (2006). The low-income housing tax credit program goes mainstream and moves to the suburbs. *Housing Policy Debate*, *17*(3), 419-446. Retrieved from `https://doi.org/10.1080/10511482.2006.9521576`

McNee, G., & Pojani, D. (2022). Nimbyism as a barrier to housing and social mix in san francisco. *Journal of housing and the built environment*, *37*(1), 553–573.

Murray, M. P. (1983). Subsidized and unsubsidized housing starts: 1961-1977. *The Review of Economics and Statistics*, 590–597.

Murray, M. P. (1999). Subsidized and unsubsidized housing stocks 1935 to 1987: crowding out and cointegration. *The Journal of Real Estate Finance and Economics*, *18*, 107–124.

O'Regan, K., & Oster, S. (2002). Does government funding alter nonprofit governance? evidence from new york city nonprofit contractors. *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, *21*(3), 359–379.

Ottoni-Wilhelm, M., Vesterlund, L., & Xie, H. (2017). Why do people give? testing pure and impure altruism. *American Economic Review*, *107*(11), 3617–33.

Payne, A. A. (2001). Measuring the effect of federal research funding on private donations at research universities: is federal research funding more than a substitute for private donations? *International Tax and Public Finance*, *8*(5), 731–751.

Payne, A. A. (2009). Does government funding change behavior? an empirical analysis of crowd-out. *Tax policy and the economy*, *23*(1), 159–184.

Powell, W. W., & Bromley, P. (2020). *The nonprofit sector: A research handbook*. Stanford University Press.

Reaves, G. (2019, Jul). *The house always wins.* Retrieved from `https://www.texasobserver.org/affordable-housing-house-wins/`

Reid, C. K. (2019). Rethinking "opportunity" in the siting of affordable housing in california: Resident perspectives on the low-income housing tax credit. *Housing Policy Debate*, *29*(4), 645–669.

Rosen, H. S. (1983). *Housing subsidies: Effects on housing decisions, efficiency, and equity.* National Bureau of Economic Research Cambridge, Mass., USA.

Rosenthal, S. (2006). Where poor renters live in our cities. *Harvard Joint Center for Housing Studies working paper RR07-2*.

Rothstein, R. (2017). *The color of law: A forgotten history of how our government segregated america*. Liveright Publishing.

Schelling, T. C. (1971). Dynamic models of segregation. *Journal of mathematical sociology*, *1*(2), 143–186.

Shay, M. (2016, Jan). *Briargrove residents voice concern over plan to build low-income apartment complex.* Retrieved from `https://abc13.com/realestate/plan-to-build-low-income-apartments-in-briargrove-draws-concerns/1170275/`

Singh, D. (2019). Do property tax incentives for new construction spur gentrification? evidence from new york city.

Smith, T. M. (2007). The impact of government funding on private contributions to nonprofit performing arts organizations. *Annals of Public and cooperative Economics*, *78*(1), 137–160.

Spotlight on underserved markets: Opportunity incentives in lihtc qualified allocation plans. (2019). Freddie Mac Multifamily.

Texas department of housing and community affairs v. inclusive communities project, inc. (Vol. 135) (No. No. 13-1371). (2015). Supreme Court.

Thornton, J. P. (2014). Flypaper nonprofits: The impact of federal grant structure on nonprofit expenditure decisions. *Public Finance Review*, *42*(2), 176–198.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of political economy*, *64*(5), 416–424.

Tinkelman, D., & Neely, D. G. (2011). Some econometric issues in studying nonprofit revenue interactions using nccs data. *Nonprofit and Voluntary Sector Quarterly*, *40*(4), 751–761.

Twombly, E. C. (2003). What factors affect the entry and exit of nonprofit human service organizations in metropolitan areas? *Nonprofit and voluntary sector quarterly*, *32*(2), 211–235.

Voith, R., Liu, J., Zielenbach, S., Jakabovics, A., An, B., Rodnyansky, S., ... Bostic, R. W. (2022a). Effects of concentrated lihtc development on surrounding house prices. *Journal of Housing Economics*, *56*, 101838. Retrieved from `https://www.sciencedirect.com/science/article/pii/S1051137722000134` doi: https://doi.org/10.1016/j.jhe.2022.101838

Voith, R., Liu, J., Zielenbach, S., Jakabovics, A., An, B., Rodnyansky, S., ... Bostic, R. W. (2022b). Effects of concentrated lihtc development on surrounding house prices. *Journal of Housing Economics*, *56*, 101838.

Appendix A

Appendix to Chapter 1

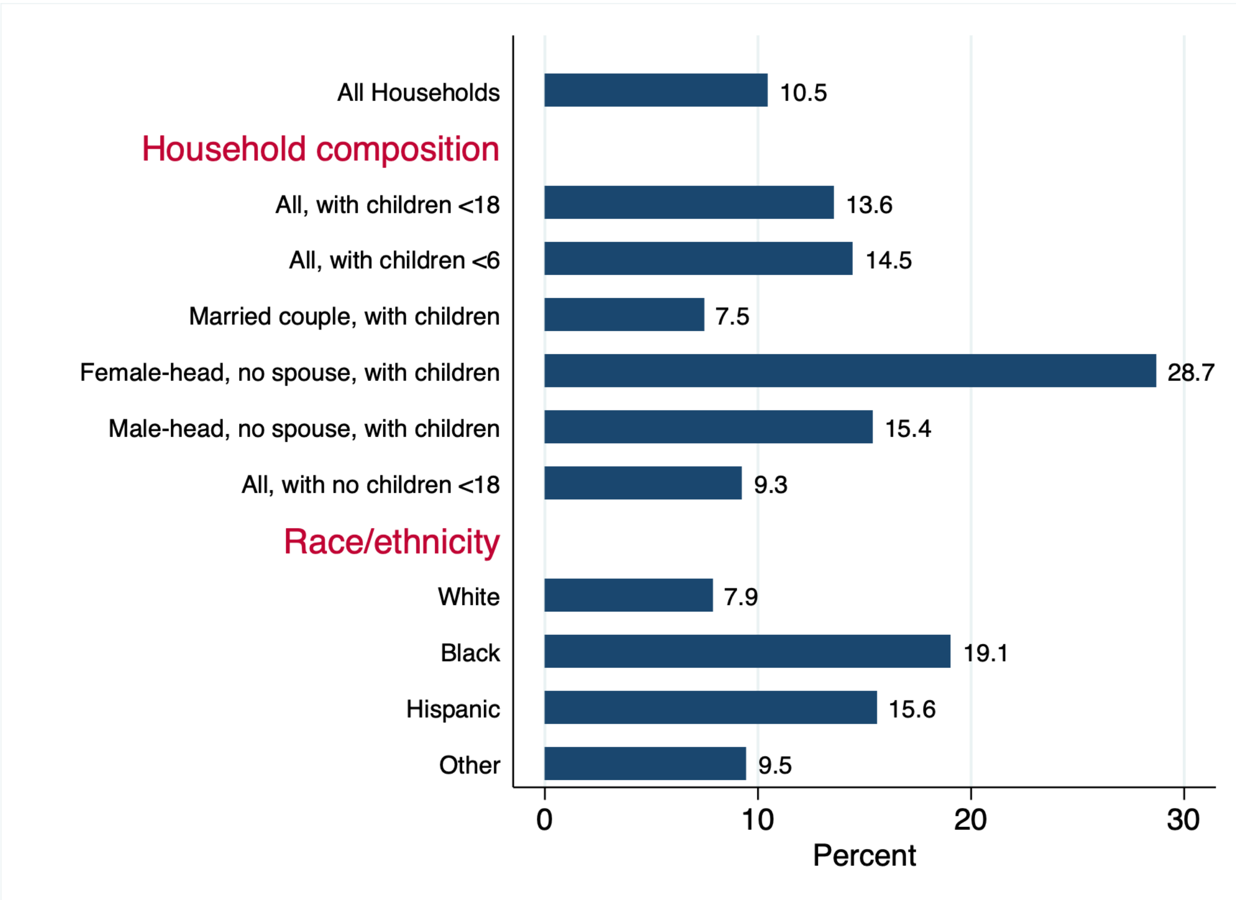
A.1 Additional Figures

Figure A1: Form 990 Extract (2018)

Contributions, Gifts, Grants and Other Similar Amounts	1a	Federated campaigns . . .	1a	
	b	Membership dues	1b	
	c	Fundraising events	1c	
	d	Related organizations . . .	1d	
	e	Government grants (contributions)	1e	
	f	All other contributions, gifts, grants, and similar amounts not included above	1f	
	g	Noncash contributions included in lines 1a–1f: $		
	h	**Total.** Add lines 1a–1f . . . . ▶		

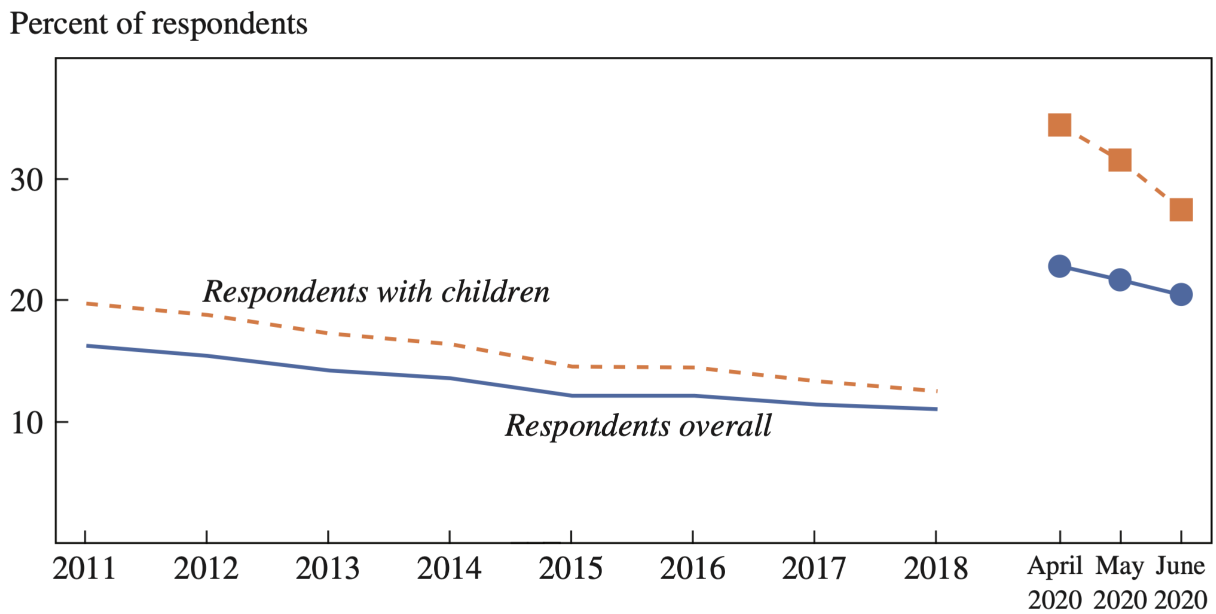
Notes: The figure shows how organizations report funding from various sources in their tax form. “Government grants (contributions)” make up government funding in my analysis. Contributions from “Federated campaigns” and “Related organizations” make up the Grants category. While “Membership dues”, “Fundraising events”, and “All other contributions...” make up the Donations category. Together, grants and donations total private funding.

Figure A2: Food Insecurity Rates (2019)



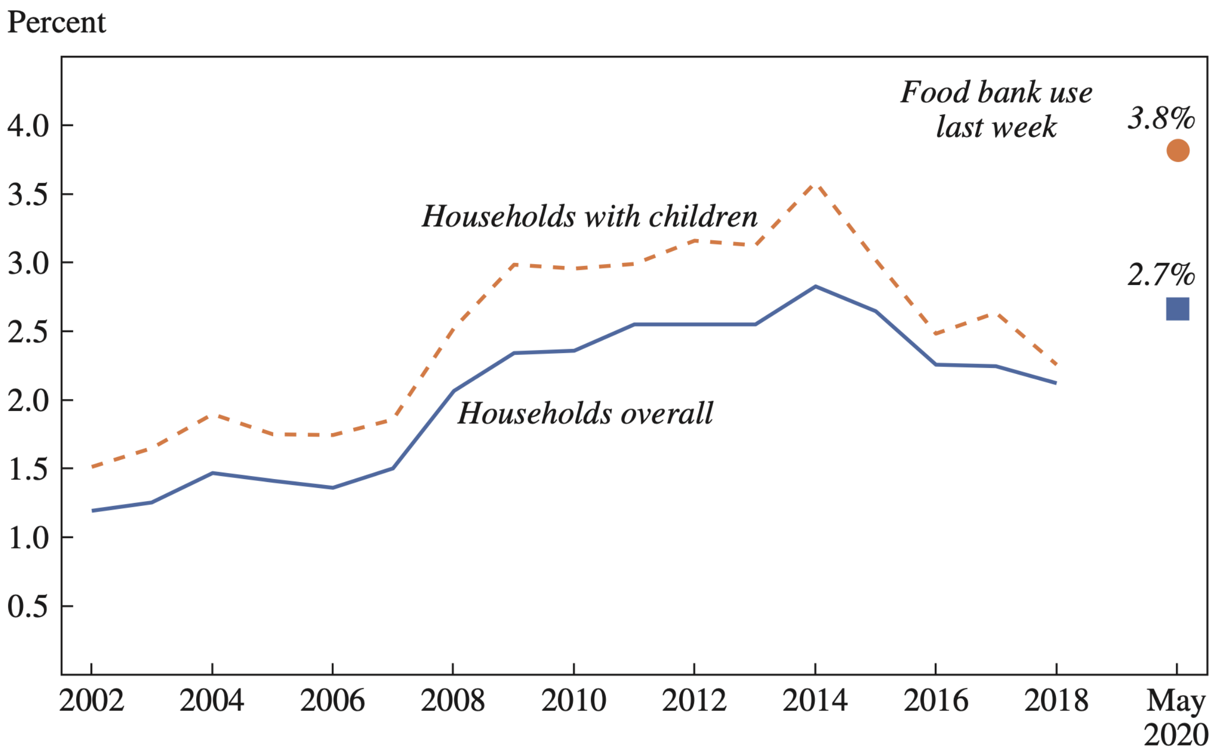
Notes: Author's tabulations based on the USDA's Economic Research Service 2019 report on food security `https://www.ers.usda.gov/publications/pub-details/?pubid=99281`

Figure A3: Food Insecurity Rates Over Time



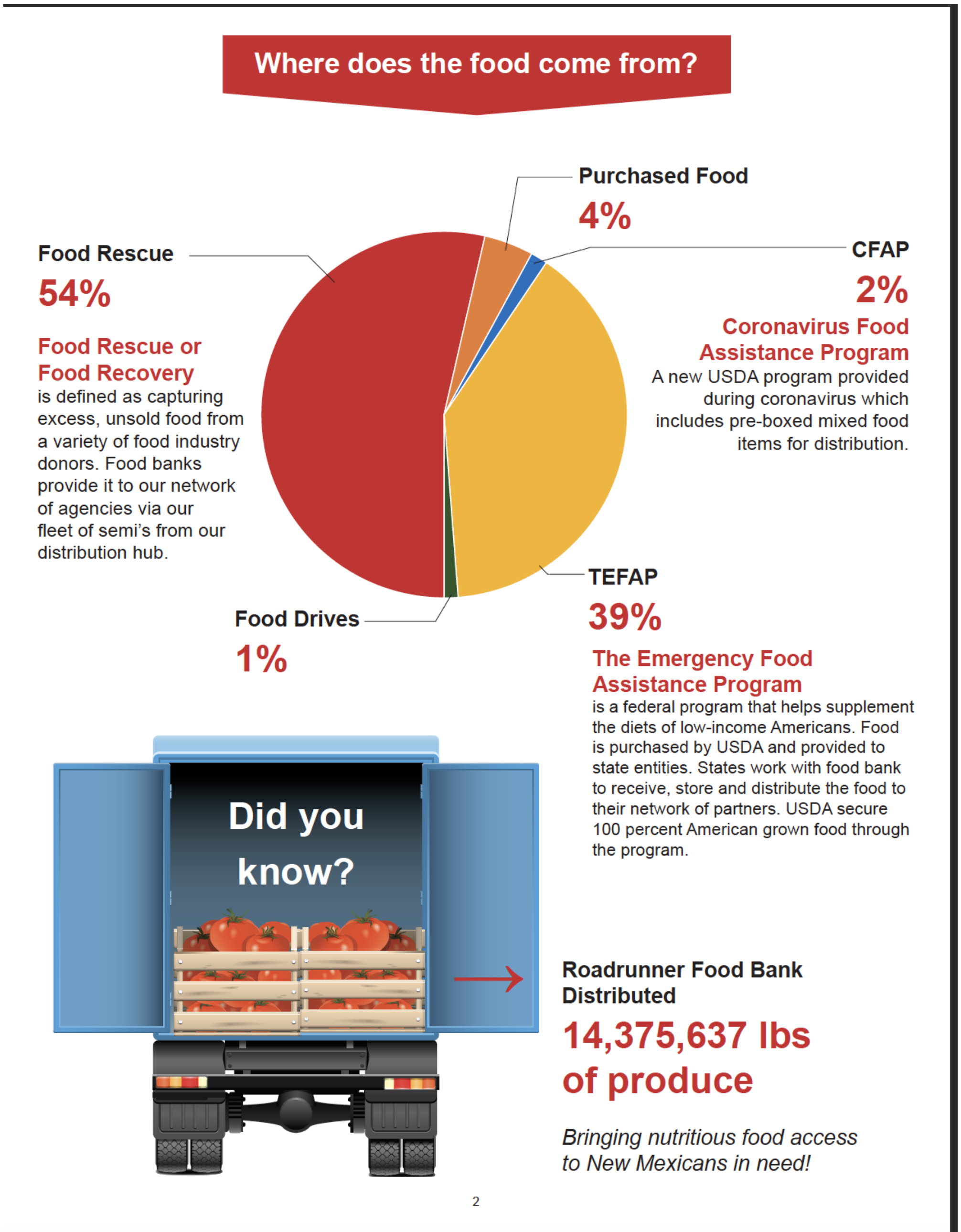
Notes: Figure originally from Bitler et al. (2020).

Figure A4: Food Bank Usage Over Time



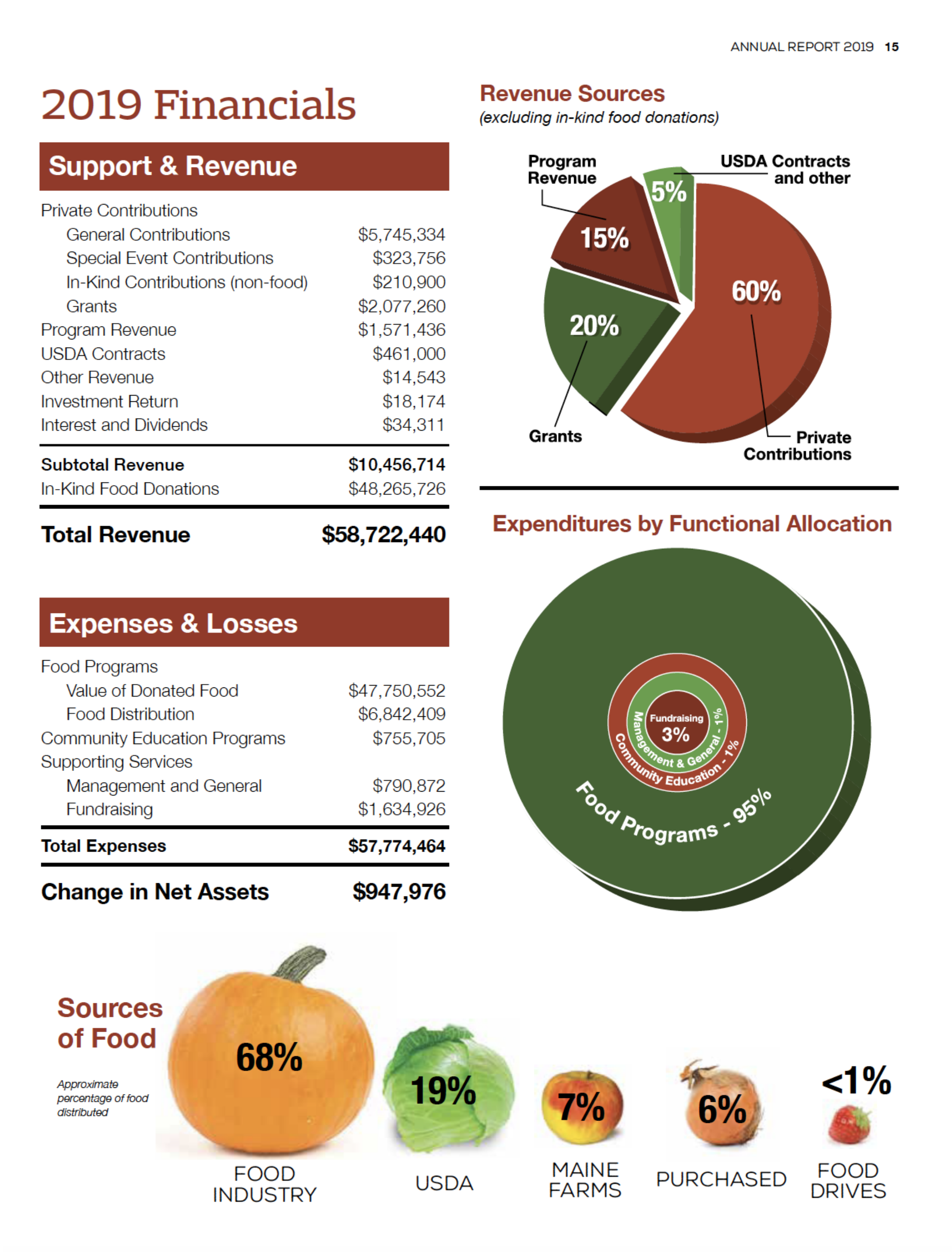
Notes: Figure originally from Bitler et al. (2020)

Figure A5: Road Runner Food Bank Financial Report (2020)



Notes: The pie chart in this extract from the report compares the size of TEFAP (yellow) to private sources of food (red, green, and orange).

Figure A6: Good Shepherd Food Bank Financial Report (2018)



2019 Financials

Support & Revenue

Private Contributions	
General Contributions	$5,745,334
Special Event Contributions	$323,756
In-Kind Contributions (non-food)	$210,900
Grants	$2,077,260
Program Revenue	$1,571,436
USDA Contracts	$461,000
Other Revenue	$14,543
Investment Return	$18,174
Interest and Dividends	$34,311
Subtotal Revenue	**$10,456,714**
In-Kind Food Donations	$48,265,726
Total Revenue	**$58,722,440**

Expenses & Losses

Food Programs	
Value of Donated Food	$47,750,552
Food Distribution	$6,842,409
Community Education Programs	$755,705
Supporting Services	
Management and General	$790,872
Fundraising	$1,634,926
Total Expenses	**$57,774,464**
Change in Net Assets	**$947,976**

Notes: TEFAP funding is represented by the USDA food, which accounts for 19% of all food received.

Figure A7: Central California Food Bank Financial Report (2018)

Central California Food Bank and Subsidiary
Schedule of Expenditures of Federal Awards
Year Ended June 30, 2018

Federal Grantor/Pass-through Grantor/Program Title	Federal CFDA #	Pass-through Entity Identifying #	Federal Expenditures
Food Distribution Cluster			
U.S. Department of Agriculture			
Passed through California State Department of Social Services:			
Emergency Food Assistance Program – administrative costs	10.568	15-MOU-00114	$ 234,023
Emergency Food Assistance Program – commodities	10.569	15-MOU-00114	2,657,554
Total Food Distribution Cluster			2,891,577
SNAP Cluster			
U.S. Department of Agriculture			
Passed through California Association of Food Banks:			
State Administrative Matching Grants for the Supplemental Nutrition Assistance Program	10.561	16-SUB-00965	154,560
Total U.S. Department of Agriculture			3,046,137
Other Programs			
U.S. Department of Homeland Security			
Direct Award:			
Emergency Food and Shelter National Board Program	97.024	Phase 34 & 35	43,374
Total U.S. Department of Homeland Security			43,374
Total Expenditures of Federal Awards			$ 3,089,511

NOTE 10 – GRANT REVENUE

Grant revenue is recognized when expenditures are incurred in accordance with the applicable grant agreements. The Organization also receives commodities from the USDA and CDSS. Revenues for these contributions are recognized when the commodities are received. Grant revenue for the year ended June 30, 2018, consisted of the following:

Agency or Organization	
Federal grants	
USDA – commodities, distributed	$ 2,657,554
USDA – beginning inventory	(734,637)
USDA – ending inventory	516,512
USDA – commodities, received	2,436,796
USDA – cost reimbursements	234,023
Department of Homeland Security	154,560
California Nutrition Network	43,374
	2,868,753
Nonfederal grants	
CDSS	361,921
Administrative cost reimbursements	308,840
California Nutrition Network	50,340
	721,101
	$ 3,589,854

Notes: This audit report shows TEFAP relative to all types of government funding, including state grants. In the top panel, Emergency Food Assistance Program administrative costs and commodities add up to the TEFAP entitlement funding used in this analysis. TEFAP makes up 94% of all government funding for this food bank.

A.2 Additional Table

Table A1: Populations Served by Federal Food Assistance Program

	Program	Adults	Children	Seniors	Non-citizens
SNAP	Supplemental Nutrition Assistance	✓	✓		
CACFP	Child and Adult Care Food		✓	✓	✓
NLSP	National School Lunch		✓		✓
SBP	School Breakfast		✓		✓
SFSP	Summer Food Service		✓		✓
WIC	Women, Infants, and Children		✓		✓
CSFP	Commodity Supplemental Food			✓	✓
TEFAP	The Emergency Food Assistance	✓	✓	✓	✓

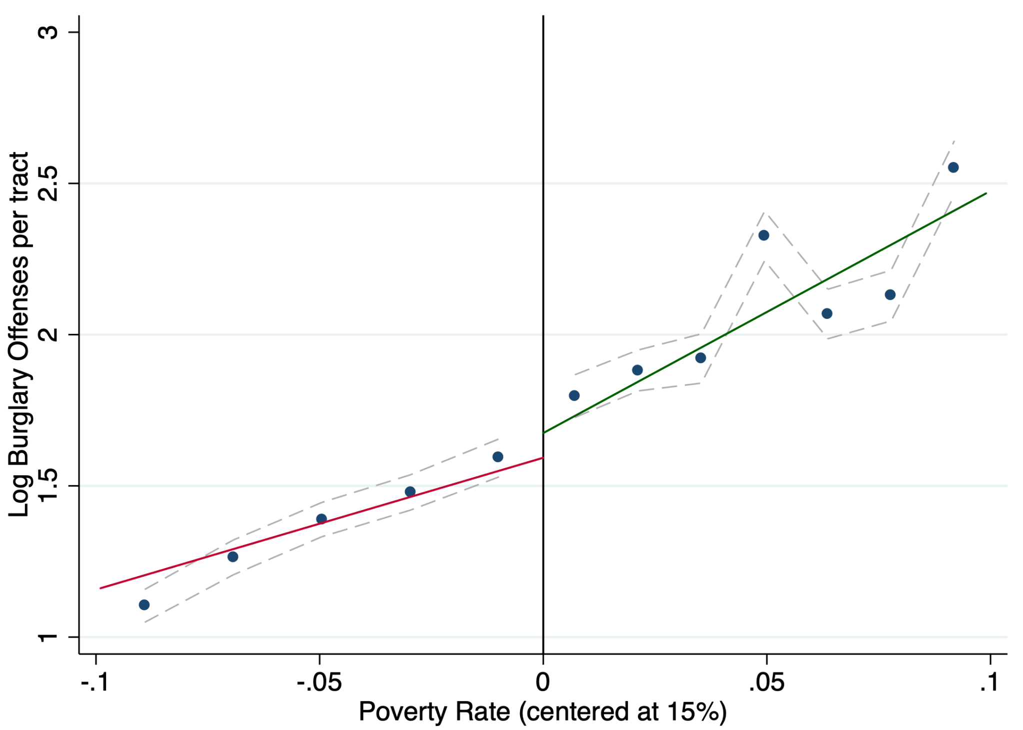
Notes: This table compares eligibility for the various federal food assistance programs. There are often other restrictions, particularly for adults seeking out SNAP. TEFAP, which is the basis for the instrument in this paper, is the most inclusive program.

Appendix B

Appendix to Chapter 2

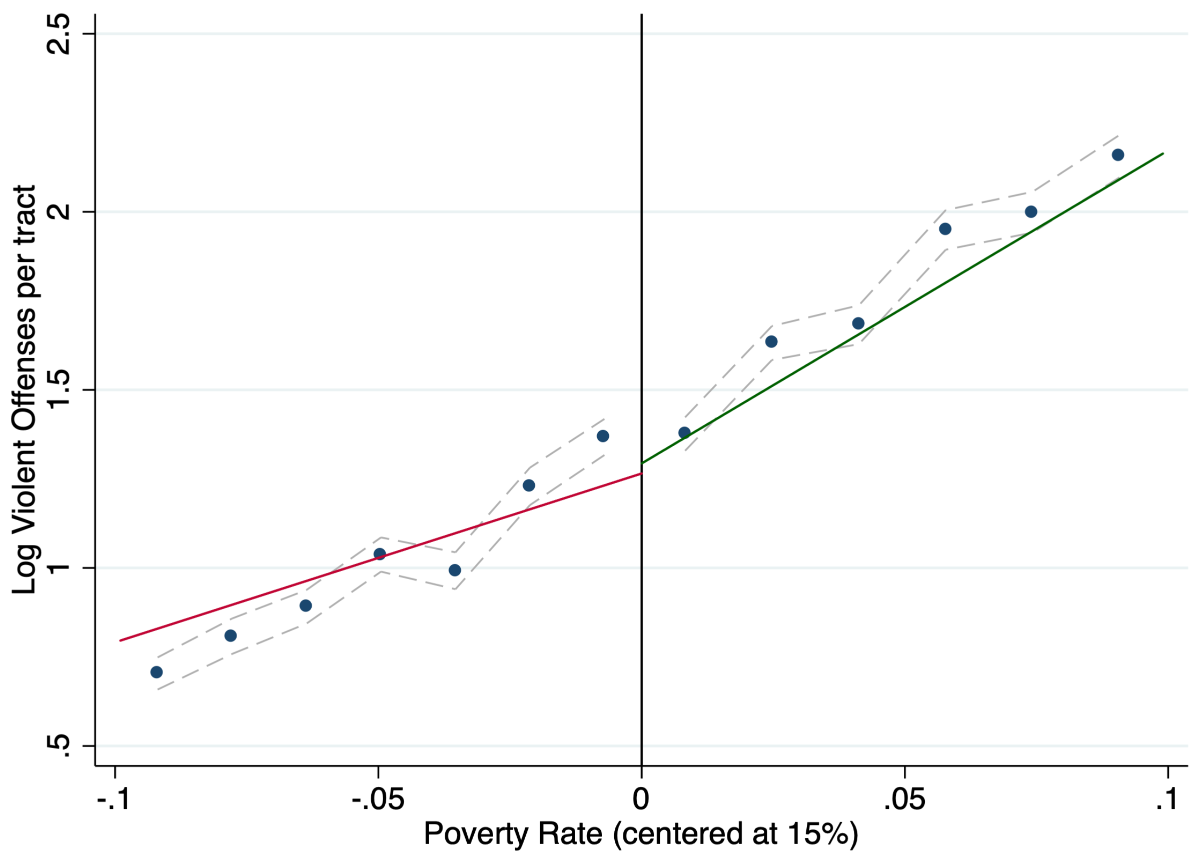
B.1 Additional Figures

Figure B1: Burglaries (2 years post LIHTC)



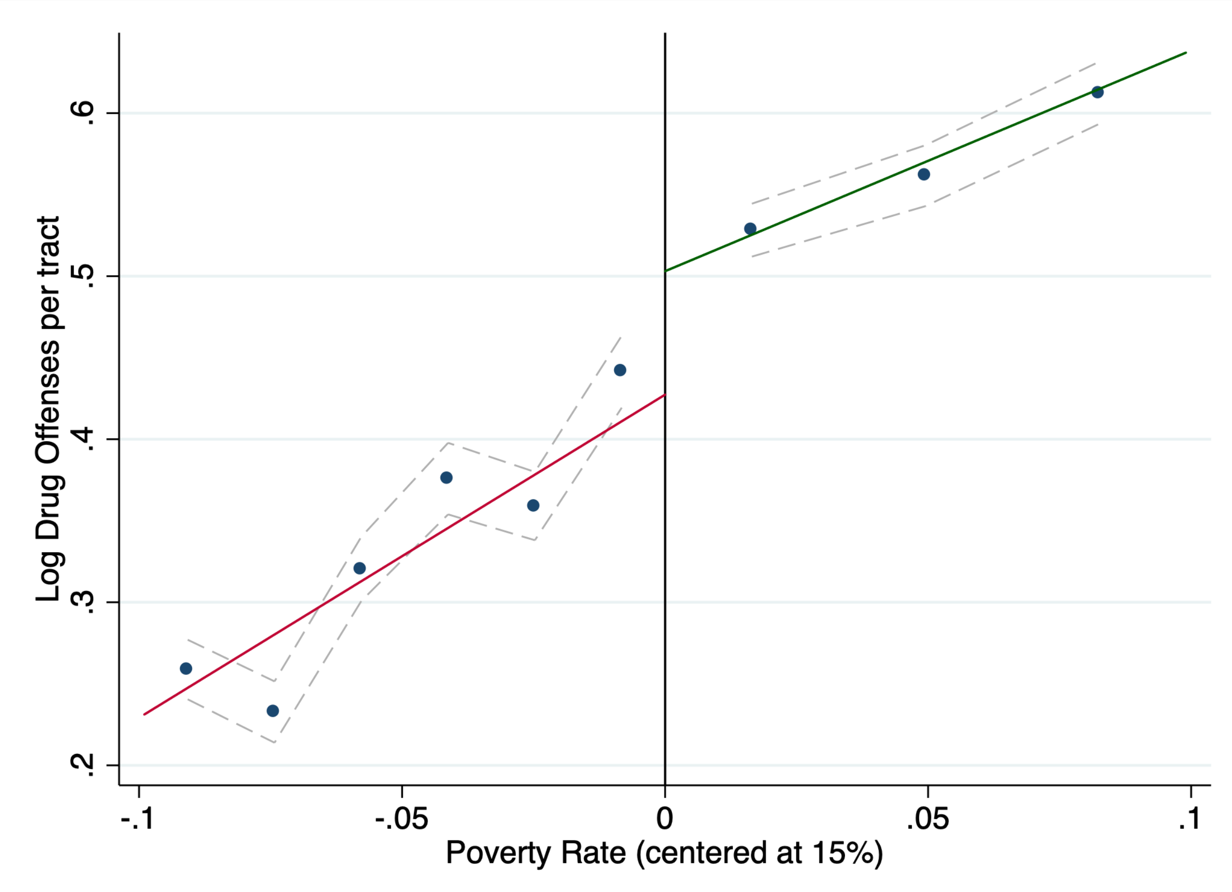
Notes: The figure plots burglary offenses (in logs) two year after LIHTC awards were announced by poverty rates centered at 15%. The plot coincides with effects on crime one year post LIHTC award, which shows no significant jumps.

Figure B2: Violent Offenses (2 years post LIHTC)



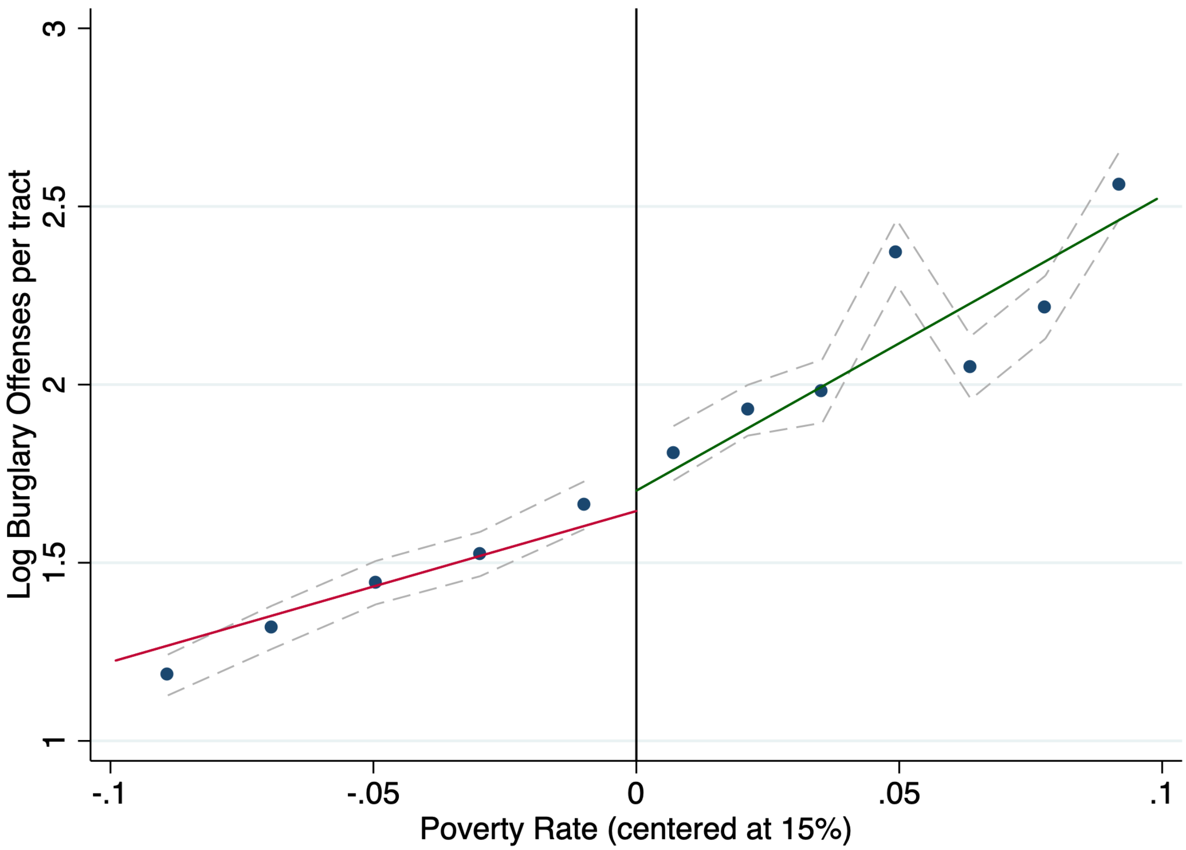
Notes: The figure plots violent offenses (in logs) two years after LIHTC awards were announced by poverty rates centered at 15%. The plot coincides with effects on crime one year post LIHTC award, which shows no significant jumps.

Figure B3: Drug Offenses (2 years post LIHTC)



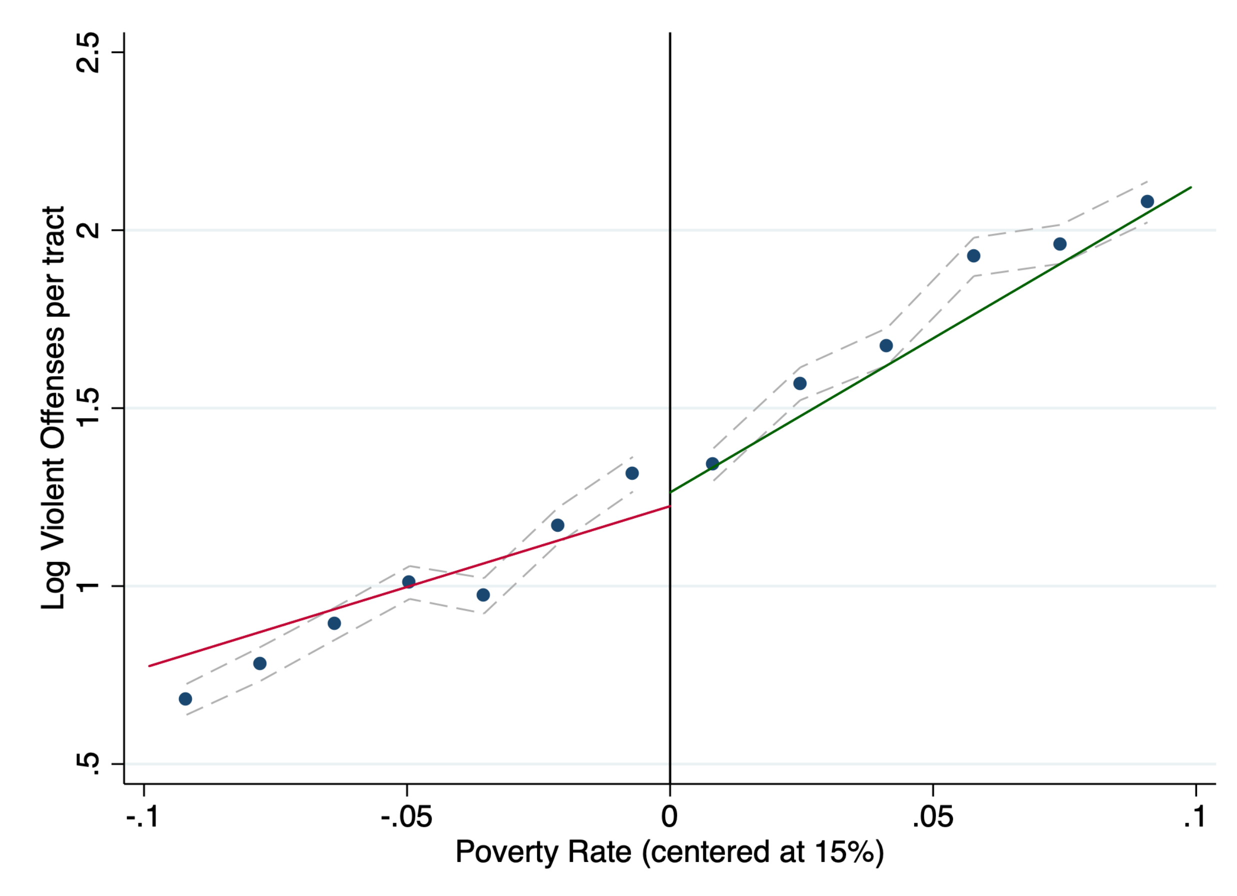
Notes: Notes: The figure plots drug offenses (in logs) two years after LIHTC awards were announced by poverty rates centered at 15%. The plot coincides with effects on crime one year post LIHTC award, which shows no significant jumps.

Figure B4: Burglaries (3 years post LIHTC)



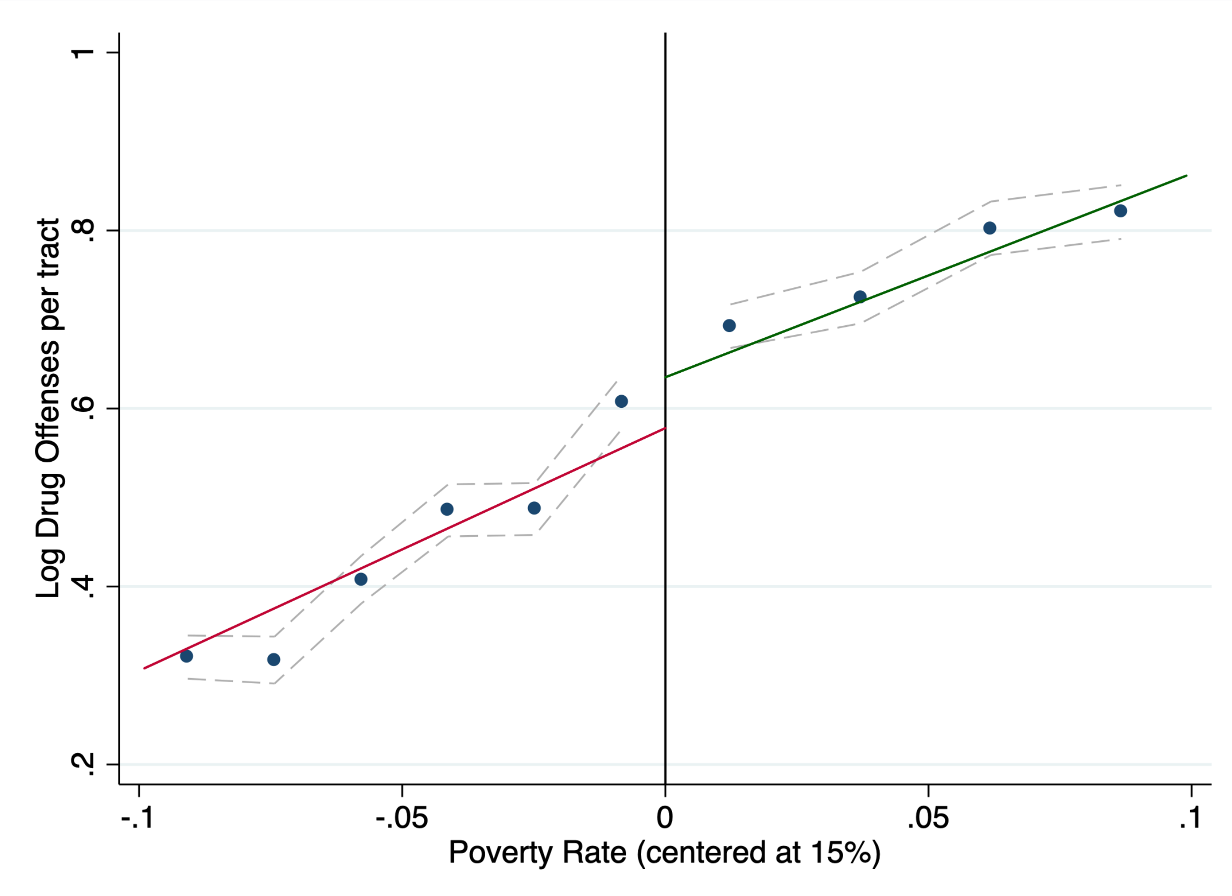
Notes: The figure plots burglary offenses (in logs) three year after LIHTC awards were announced by poverty rates centered at 15%. The plot coincides with effects on crime one year post LIHTC award, which shows no significant jumps.

Figure B5: Violent Offenses (3 years post LIHTC)



Notes: The figure plots violent offenses (in logs) three years after LIHTC awards were announced by poverty rates centered at 15%. The plot coincides with effects on crime one year post LIHTC award, which shows no significant jumps.

Figure B6: Drug Offenses (3 years post LIHTC)



Notes: Notes: The figure plots drug offenses (in logs) three years after LIHTC awards were announced by poverty rates centered at 15%. The plot coincides with effects on crime one year post LIHTC award, which shows no significant jumps.

B.2 Additional Tables

Table B1: Reduced Form Results for Burglaries (3 years post)

	(1) $Year_{t+1}$	(2) $Year_{t+1}$	(3) $Year_{t+2}$	(4) $Year_{t+2}$	(5) $Year_{t+3}$	(6) $Year_{t+3}$
Panel A: Log Offenses						
Linear Estimate	-0.0321	0.0590	-0.124	-0.0667	-0.00178	0.0287
	(0.256)	(0.225)	(0.281)	(0.251)	(0.290)	(0.265)
Panel B: Offenses per 1000 persons						
Linear Estimate	-0.417	0.311	-0.793	-0.204	-2.312	-2.188
	(1.448)	(1.310)	(1.798)	(1.588)	(2.192)	(1.903)
Observations	5,046	5,046	5,046	5,046	5,046	5,046
Controls	No	Yes	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table replicates the treatment effect results on burglaries with the addition of effects three years after LIHTC is awarded.

Table B2: Reduced Form Results for Violent Offenses (3 years post)

	(1) $Year_{t+1}$	(2) $Year_{t+1}$	(3) $Year_{t+2}$	(4) $Year_{t+2}$	(5) $Year_{t+3}$	(6) $Year_{t+3}$
Panel A: Log Offenses						
Linear Estimate	0.0170	0.0669	0.160	0.230	0.113	0.154
	(0.187)	(0.193)	(0.197)	(0.216)	(0.180)	(0.176)
Panel B: Offenses per 1000 persons						
Linear Estimate	1.964	2.340*	2.101	2.471*	1.244	1.383
	(1.417)	(1.391)	(1.432)	(1.417)	(1.100)	(1.045)
Observations	5,046	5,046	5,046	5,046	5,046	5,046
Controls	No	Yes	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table replicates the treatment effect results on violent offenses with the addition of effects three years after LIHTC is awarded.

Table B3: Reduced Form Results for Drug Offenses (3 years post)

	(1) $Year_{t+1}$	(2) $Year_{t+1}$	(3) $Year_{t+2}$	(4) $Year_{t+2}$	(5) $Year_{t+3}$	(6) $Year_{t+3}$
Panel A: Log Offenses						
Linear Estimate	-0.0381	-0.0137	-0.0591	-0.0577	0.0141	-0.00720
	(0.128)	(0.128)	(0.134)	(0.133)	(0.132)	(0.128)
Panel B: Offenses per 1000 persons						
Linear Estimate	-0.174	-0.102	-0.247	-0.208	-0.440	-0.426
	(0.389)	(0.385)	(0.376)	(0.372)	(0.477)	(0.444)
Observations	5,046	5,046	5,046	5,046	5,046	5,046
Controls	No	Yes	No	Yes	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table replicates the treatment effect results on drug offenses with the addition of effects three years after LIHTC is awarded.