The Low-Income Housing Tax Credit and Economic Opportunity[*]

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Abstract

We study the effects of the Low-Income Housing Tax Credit (LIHTC)—the nation's largest federal housing subsidy—on tenants' residential mobility and earnings. Using proprietary application records from a major private developer linked to administrative data, we show that successful applicants move to lower-quality neighborhoods and experience wage declines relative to comparable unsuccessful applicants. These outcomes appear concentrated in high-poverty areas where federal rules grant bonus tax credits to developers, suggesting that the LIHTC's current incentive structure may often steer households toward lower opportunity neighborhoods than they otherwise would have chosen.

JEL Classification: H20, H31, I38, R23

1 Introduction

The Moving to Opportunity (MTO) housing mobility demonstration catalyzed a wave of research showing that neighborhood environments profoundly shape outcomes for low-income families (Chyn and Katz, 2021). Now more than three decades later, policies modeled on MTO—vouchers paired with supports or incentives to move to higher-quality neighborhoods—remain fragmented and largely comprised of local pilots and small-scale regional demonstrations (Galvez and Oppenheimer, 2020; Mumphery et al., 2023). Yet in the absence of these targeted supports, voucher holders seldom leave high-poverty neighborhoods (Sard et al., 2018; Galvez and Oppenheimer, 2020; Collinson and Ganong, 2018). The persistence of post-MTO residential segregation by income and race (Reardon et al., 2018) further underscores the difficulty of achieving large-scale opportunity moves through demand-side programs alone.

In this paper, we shift attention to an often-overlooked dimension of the housing mobility discourse: the Low-Income Housing Tax Credit (LIHTC). The LIHTC operates on the supply side of the affordable housing landscape, providing over $10 billion annually in tax incentives to developers who set aside units for low-income renters at below-market rents. It is the nation's largest and fastest-growing federal affordable housing program (Figure 1), financing more than 20% of all multi-family housing development over the past two decades (Diamond and McQuade, 2019) and supporting as many 90% of newly built affordable rentals (Axel-Lute, 2023).

We estimate how the LIHTC affects renters' neighborhood choices and labor market outcomes. The LIHTC mechanically differs from vouchers in two key ways with important implications for geographic and economic mobility. First, vouchers subsidize rental costs wherever families to choose reside, whereas the LIHTC subsidizes a fixed set of affordable units and the program's mobility benefits depend largely on where developers choose to build. Because federal and state governments hold significant authority over LIHTC siting decisions (Ellen and Horn, 2017), they can—in principle—shape access to high-opportunity neighborhoods at scale by incentivizing development in those places. Second, whereas voucher benefits phase out as earnings increase, the LIHTC institutes a system of flat rents that theoretically does not generate labor supply disincentives related to substitution effects. The LIHTC therefore provides an opportunity to understand how housing subsidies impact labor market outcomes for low-income renters through other margins,

including the role of income effects, neighborhood effects, and housing stability.

Most existing causal studies on the LIHTC focus on the program's effects on neighborhoods, including impacts on local housing supply (Baum-Snow and Marion, 2009; Eriksen and Rosenthal, 2010), crime (Freedman and Owens, 2011), neighborhood composition (Diamond and McQuade, 2019; Davis et al., 2024; McGuire and Seegert, 2023), and aggregate housing markets (Soltas, 2023; Cook et al., 2023). We know far less about the program's impacts on individual renters, in large part due to limitations of existing data. The LIHTC is highly decentralized, and while the U.S. Department of Housing and Urban Development (HUD) monitors site-by-site compliance with federal affordability rules, detailed records on applicants, waitlists, and tenants are held as proprietary data by the thousands of developers and property management companies across the country that lease out LIHTC-subsidized units.

We address this limitation by obtaining access to applicant and tenant data from one of the nation's largest LIHTC developers, which we subsequently link to consumer reference data and tax return records to estimate individual-level impacts of living in LIHTC-subsidized housing. Our data include all information captured in an individual's rental application, as well as the ultimate outcome of their application. In event study designs, we use the outcomes of unsuccessful applicants to infer counterfactual outcomes trajectories for successful applicants, which allows us to credibly identify the causal impact of moving to a LIHTC-subsidized unit.

We first estimate impacts on applicants' neighborhood quality, measured by poverty rates, social and economic outcomes for children, demographic composition, and labor market vitality. To do so, we link personal identifiers from applicant records to individual-level migration histories from Infutor Data Solutions, a consumer reference dataset containing over 375 million individual records connected to nearly 1 billion address and name histories.[1] We use these data to capture high-frequency changes in applicants' place of residence before and after applying to LIHTC housing.

We find that placement into LIHTC-subsidized units affects some, but not all, of applicants' neighborhood characteristics. Within two years of application, placed applicants live in Census tracts with higher poverty rates (+1 percentage point; +5.5% vs. baseline), higher predicted adult incarceration (+0.175 percentage points; +7% vs. baseline), and lower intergenerational income

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[1]This dataset has been employed in numerous peer-reviewed economic studies, including Bernstein et al. (2022); Diamond et al. (2019); Pennington (2021); Asquith et al. (2023); Mast (2023); Phillips (2020), and Collinson et al. (2022).

mobility (-0.35 percentage point; -4% vs. baseline) relative to similar but unsuccessful applicants to the same property. These differences emerge within 6-12 months of application and grow over time. These patterns indicate that placement in LIHTC units may "lock-in" residents to low opportunity neighborhoods, whereas unsuccessful applicants gradually transition over time into tracts with stronger opportunity profiles. We do not detect statistically significant effects on other tract attributes, including measures of labor market strength, school quality, and demographic composition.

We show that these average treatment effects mask underlying heterogeneity. Applicants placed in LIHTC-subsidized units are more likely to end up in both higher- *and* lower-opportunity tracts. However, although some individuals move to LIHTC units in neighborhoods with marginally higher opportunity levels, these positive effects are more than offset by moves to neighborhoods with significantly lower opportunity levels. The geography of development helps explain this pattern. Approximately 40% of LIHTC properties are located in Qualified Census Tracts (QCTs), where LIHTC developers receive bonus tax credit allocations for building in economically disadvantaged areas. When we re-estimate our event studies for applicants who apply to properties in QCT versus non-QCT tracts, we find that successful applicants to properties located in QCTs moved to significantly worse neighborhoods than the comparison group. By contrast, successful and unsuccessful applicants to properties in non-QCT locations experienced similar post-application trajectories. These results strongly suggest that QCTs play a central role in driving LIHTC residents to lower opportunity tracts.

We also examine applicant heterogeneity to understand who bears the cost of these siting patterns and who might benefit most from policy reforms. We find that the negative effects are most pronounced among smaller households seeking 1 bedroom units, as well as applicants aged 45-64. We speculate that fierce competition for smaller affordable units—which are relatively scarce in major U.S. housing markets—may cause individuals seeking such units to trade off neighborhood quality for lower rents. Additionally, since neighborhood quality tends to most strongly impact young children (Chetty et al., 2016), households without young children may be more willing to compromise on neighborhood quality in exchange for more affordable rents.

We next turn to labor market outcomes, which we measure using earnings from federal tax returns. We utilize two measures of earnings: 1) any wages reported on an individual's W-2, and

2) total "HUD income", which includes additional income sources including safety net benefits and non-wage earnings from sources such as businesses and property. Unlike the Infutor data, which is updated monthly, tax return data can only provide annual snapshots.

Prior to applying, matched treated and comparison applicants exhibit nearly identical earnings profiles. However, their trajectories diverge shortly after. Two years post-application, LIHTC residents earn almost $3,000 less in annual wages than comparable non-residents (-18% relative to baseline mean). We find similar effect sizes when estimating effects on total income from all sources. Previous work on housing subsidies, including public housing (Susin, 2005) and vouchers (Jacob and Ludwig, 2012), find similar decreases in earnings. Our findings suggest that housing subsidies impact earnings even in the case where the value of the subsidy is not directly tied to the tenant's own income – a distinction which sets LIHTC subsidies apart from others.

To contextualize these earnings results within the broader framework of net financial impacts, we conduct a back-of-the-envelope calculation comparing our estimated effects on earnings to cost savings associated with moving to LIHTC units. We estimate that for successful applicants, average housing costs are $1,300 less per year in their new neighborhoods. We then use results from Cook et al. (2023) to estimate the annualized "discount" that LIHTC residents of those neighborhoods receive relative to market rents. In our sample, we estimate these within-neighborhood subsidies range from $1,500 to $5,000. Taking the between- and within-neighborhood effects together, our estimates imply total cost savings ranging from $2,800 to $6,300. By comparison, estimated wage losses total $2,900, or approximately 45-100% of savings. Despite the wide range covered by these estimates, this exercise suggests that most LIHTC renters likely break-even – or even come out ahead – financially despite working relatively less.

This paper provides the first causal evidence on how LIHTC placement shapes residents' neighborhood environments and economic outcomes, using a novel dataset that overcomes long-standing data constraints. In doing so, we shed new light on how supply-side housing interventions shape residential mobility and access to opportunity. Our findings indicate that, in its current form, the LIHTC does not consistently enable moves to higher-opportunity neighborhoods. Some households benefit, but others experience sharp declines in neighborhood quality. This variation reflects the LIHTC's supply-oriented design, in which families must accept housing where it is built rather than choose among locations in a more unconstrained manner. Our analysis highlights both the

promise and the limitations of a system in which federal and state incentive structures direct where development occurs—and thereby whether low-income families are guided towards higher- or lower-opportunity neighborhoods.

2 The Low Income Housing Tax Credit (LIHTC)

2.1 How do the subsidies work?

The LIHTC was established as part of the Tax Reform Act of 1986 to expand the supply of affordable housing via developer subsidies. Since its passage, the tax incentive has become a bedrock of the U.S. housing landscape, with over 3.5 million affordable units placed in service between 1987-2021. Today, the LIHTC costs an estimated \$13.5 billion annually (Keightley, 2023). Figure 1 shows that the LIHTC is both the largest and fastest-growing source of affordable housing units operated by HUD. These tax credits have helped to finance more than 20% of all multi-family housing development over the past two decades (Diamond and McQuade, 2019).

The LIHTC differs from other federal housing initiatives in that it provides developers with incentives (in the form of tax credits) to build rent-restricted housing that is set aside for low-income tenants.[2] There are two distinct types of LIHTC subsidy: the 70% credit—which is awarded by state housing credit agencies through a competitive process—and the 30% credit, which is not competitive.[3] Here, "70%" and "30%" refer to the present value of the tax credits a successful LIHTC project can receive; i.e., projects financed with the 70% credit receive tax credits with a present value of 70% of the "qualified basis" of the building (i.e., the share of the building devoted to affordable housing). Upon application for LIHTC financing, prospective developers choose from a menu of options for rent-restricted units set aside for low-income residents. This menu includes: 1) $\geq 20\%$ of units occupied by tenants earning below 50% of the Area Median Income (AMI), or 2) $\geq 40\%$ of units occupied by tenants earning below 60% of AMI.[4] Rents for these units cannot

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[2]Developers sell tax credits to investors, who in turn receive a dollar-for-dollar credit against their federal tax liability for a period of 10 years, if the property remains in compliance with the LIHTC rules.

[3]The 30 percent subsidy covers either the acquisition cost of existing buildings or the construction of new buildings when paired with other subsidies. The 70 percent subsidy supports only new construction and does not allow additional federal subsidies to be used.

[4]There is a third criterion, added in 2018, which allows developers to set aside 40% of units such that the average AMI threshold among these units does not exceed 60%. The units contributing to this average must range between 20% and 80% AMI.

exceed 30% of the corresponding AMI threshold. Unlike other federal affordable housing initiatives, the rents for LIHTC-financed units are not tied to the income of the tenant, but rather linked to set shares of the AMI. Tenants' incomes are assessed upon initial move-in, to ensure that the overall property meets the minimum low-income occupancy requirements. Incomes are re-assessed annually to ensure the property remains in compliance in subsequent years.[5] The low-income requirements bind for 15 years, and properties can gradually phase out of compliance over the course of the subsequent 15 years.

2.2 How does the LIHTC affect mobility to high-opportunity places?

Currently, housing vouchers remain the primary policy lever for mobility. The Moving to Opportunity (MTO) experiment showed that vouchers tied to moving to low-poverty neighborhoods improved long-run outcomes for young children (Chetty et al., 2016). However, voucher-based housing mobility programs still have yet to become commonplace (Galvez and Oppenheimer, 2020; Mumphery et al., 2023). The federal Housing Choice Voucher (HCV) program also faces significant constraints. Only about one in four eligible households receives any federal rental assistance, and families that ultimately obtain vouchers remain on waitlists for an average of 2.5 years (and as long as eight years in some areas) before ultimately receiving vouchers (Acosta and Gartland, 2021). Despite local reforms, landlord refusal of vouchers remains common where state or local source-of-income protections are absent, limiting neighborhood choice for low-income renters in these places (Sard et al., 2018). Unrestricted HCVs typically yield modest neighborhood upgrading: relative to other low-income renters, voucher recipients move to slightly safer and sometimes lower-poverty areas, but still well below their metro's average opportunity levels (Lens et al., 2011; Collinson and Ganong, 2018).

In theory, the LIHTC could complement vouchers by expanding the supply of units in high-opportunity areas, aligning private development incentives with affordability needs. In many states, LIHTC properties cannot refuse voucher holders, creating potential complementarities between the programs (Emmanuel and Aurand, 2024). States also ultimately decide which construction proposals receive tax credits through scoring systems defined in their Qualified Allocation Plans

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[5]In subsequent years, individual tenants' incomes may rise past the applicable income limit without causing the property to forfeit its LIHTC financing.

(QAPs), and they can steer development toward higher-opportunity neighborhoods by awarding points, set-asides, or tiebreakers to proposals located in low-poverty, high-amenity tracts. Many states have already incorporated opportunity-based criteria into their QAPs, including access to education, economic vitality, health care access, and access to transportation (Freddie Mac, 2023). Empirical work shows that strengthening these QAP incentives measurably shifts funded developments into better-opportunity areas (Ellen et al., 2015; Ellen and Horn, 2017).

In practice, developers face cost-containment pressures (e.g., land prices, construction costs, zoning) which often steer affordable development preferences towards low-amenity neighborhoods. In addition, federal rules provide a 30% tax credit boost to projects built within qualified low-income census tracts ("QCTs")—areas that are disproportionately high-poverty by definition.[6] These forces can pull in opposite directions: QAP opportunity points push toward high-amenity neighborhoods, while QCT boosts and feasibility considerations pull toward lower-cost, lower-amenity tracts. Given these misaligned incentives, it is ambiguous *ex ante* whether the LIHTC promotes or hinders moves to opportunity on average, and how these impacts vary across policy settings.

3   Data

Due to the highly decentralized nature of the LIHTC, no national dataset (including major surveys such as the ACS and CPS) identifies individual residents of LIHTC-funded housing(Collinson et al., 2016). Recent studies link known locations of LIHTC properties to administrative records (Derby, 2021; Cook et al., 2023), but such approaches still cannot identify who *applied* to LIHTC properties, which forms the basis for our empirical approach. Since government agencies do not collect applicant data, such information can only be obtained from proprietary developer or property management records.[7] In what follows, we describe how we link application, waitlist, and tenant data from one of the nation's largest LIHTC developers to administrative tax and consumer records, creating the first dataset that identifies both successful and unsuccessful applicants and their outcomes before

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[6]A tract is designated as a QCT if either 1) at least half of resident households have incomes below 60% of Area Median Gross Income, or 2) the tract has a poverty rate of at least 25%.

[7]HUD only began collecting project-level demographic and economic information on tenants in LIHTC units in 2008. The annual "tenant reports" produced from these aggregated data are limited by incomplete coverage and the lack of uniform reporting requirements. Most of the data collection is carried out by the respective state housing finance agencies (HFAs), and per HUD documentation: "although income and rent information was collected across HFAs using fairly uniform standards and definitions, the demographic information was not standardized and, for some HFAs, not collected at all."

and after applying.

3.1 Applicant and Tenant Data

We draw on applicant and tenant data from a large, private developer of affordable housing. With more than 30,000 residents and 300 LIHTC properties in over a dozen states, the developer ranks among the nation's largest developers of new LIHTC units (HUD, 2023). Crucially, the developer is fully vertically integrated: the development, design, construction, and management of each property is all conducted in-house. Applicant and tenant data for all properties are subject to uniform standards, a key advantage in our setting. Figure 2 shows that the developer has a presence in the Midwest, Appalachia, and the Southeast. The portfolio includes developments in several large cities, but also includes large concentrations in suburban and exurban counties—a notable difference from much of the existing affordable housing research.

Since 2012, the developer has tracked all interactions with prospects, applicants, and tenants at each property using Yardi, an internal client management database. Figure 3 depicts the end-to-end flow for the application and placement processes. In general, applications are reviewed on a first come, first served basis. Property staff primarily rely on unit preferences and income to match applicants to available units. Units not only vary by bedroom size, but also have different income restrictions to remain compliant with affordability rules. If no suitable unit is available, the applicant is placed on a waitlist based on bedroom preference. Otherwise, the applicant undergoes an eligibility screening, which involves criminal background and credit checks as well as third-party income verification. If the applicant is determined to be eligible, they can accept the offered unit and begin planning their move-in. At any point after an application is submitted (but before a unit is accepted), an applicant may cancel their application.

For all applicants, we observe the applicant's full name and date of birth, Social Security number, the address they applied from, information about the property they are applying to, unit preferences, the application date, and the outcome of their application. For placed tenants, we further observe race,[8] verified income, unit characteristics, and move date.

We also obtained property-level characteristics, including each property's name, date placed-

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[8]Properties are not permitted to record the race of non-residents.

in-service,[9] funding sources, tax credit details, and affordability rules. Since some properties are "scattered sites" with units spread across multiple locations, we further received all unit-level addresses associated with each property, allowing us to identify the exact location to which placed residents move. Finally, we observe unit-level characteristics such unit size (sq ft), number of bedrooms, rent and utility allowance, and whether the unit is designated as subsidized or market rate. For all subsidized units, we observe the income restriction rule associated with the unit.

3.2 Consumer Reference Data

To assess the impacts of moving to LIHTC-subsidized housing on neighborhood-level outcomes, we link applicant and tenant records from the Yardi data to migration histories available from Infutor Data Solutions' consumer reference database. Infutor aggregates information from numerous private and public record sources, including USPS change of address forms, county assessor records, magazine subscriptions, and phone books. Infutor contains over 375 million individual-level records linked with nearly 1 billion address and name histories. Each address record in Infutor is associated with an "effective date", interpretable as a move-in date, which allows us to sequence an individual's move history over time. Because Infutor provides name histories, we can further link records for individuals associated with multiple aliases and for those whose surname changes over time. Previous work finds that Infutor is highly representative of the U.S. adult population, especially since the 1990s.[10] Phillips (2020) shows that Infutor move dates can detect highly-localized migration shocks, including out-migration from Hurricane Katrina and closures of public housing projects in Chicago. Infutor data has been used in previous work to study immigration (Bernstein et al., 2022), rent control (Diamond et al., 2019), housing markets (Pennington, 2021; Asquith et al., 2023; Mast, 2023), and housing stability (Phillips, 2020; Collinson et al., 2022).

In Section 5.1, we describe how we construct a panel of neighborhood-level outcomes by linking

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[9]The LIHTC "placed-in-service" date corresponds to the date that a property is certified as ready for occupancy. For new construction, this date generally corresponds to the inspector's certification. LIHTC-funded properties can begin earning tax credits for investors only after the units are placed-in-service and occupied.

[10]Due to the nature of the source data, Infutor is generally unable to detect individuals who do not have a "paper trail", such as minors, undocumented immigrants, or homeless populations. Given that our population consists of adults who are able to afford LIHTC-subsidized housing, we believe that Infutor is well-suited to measure migration-related outcomes in our context. Using 2017 ACS 5-year estimates, Asquith et al. (2023) finds a median of 0.88 Infutor observations per Census individual aged 25+ across all Census tracts. Bernstein et al. (2022) find that variation in county populations estimated from Infutor can account for 99% of the variation observed in county populations in the 2000 Census.

applicants to the Infutor database using personally-identifiable information available from the Yardi data.

3.3 Administrative Tax Data

To measure the impact of LIHTC subsidies on renters' labor market outcomes, we draw on tax data from applicants' individual income tax returns (Form 1040), as well as select information returns which provide third-party reports of individuals' wages (Form W-2), unemployment compensation (Form 1099-G), and interest income (Form 1099-INT). Linking to Forms 1040, W-2, 1099-G, and 1099-INT allows us to measure income according to several different definitions. HUD uses a specific definition of income to determine LIHTC eligibility,[11] which does not correspond precisely to the definition of income used to determine income tax liability.[12] However, the Form 1040 includes a variety of line items which can be used to approximate HUD's definition of income. We construct our approximation of HUD income by adding back the non-taxable portions of interest income, dividends, IRA distributions, pensions, annuities, and Social Security benefits to total income.[13] When an applicant does not file a Form 1040 in a particular year, we impute the approximate HUD income as the sum of that individual's wages from Form W-2, unemployment compensation from Form 1099-G, and interest income from Form 1099-INT. By aggregating information from the Form W-2, we can construct an alternative definition of income: the total wages earned by the applicant and their spouse, if any.

For Form 1040 filers, we also observe applicants' marital status and the number of dependents present in the tax unit. This may differ from what is reported in the developer data for several reasons. First, a tax unit includes only the filer and their spouse and dependents, if they have any. By contrast, a household as defined on a housing application may include individuals from multiple tax units. For example, if a LIHTC applicant resides with their parent, the household as listed on a LIHTC application could include both individuals but each individual would constitute a separate tax unit. Second, the tax data provides a snapshot of the tax unit at the end of each tax year,

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[11]https://www.hud.gov/sites/documents/calculatingattachment.pdf

[12]For example, HUD's definition of income includes TANF and SNAP benefits, which are not taxable and so do not appear on the Form 1040.

[13]Total income appears on Form 1040, line 9. The other line items appear on Form 1040, lines 1 through 6. On each line l, the full value of the line item appears on line la, whereas the taxable portion appears on line lb. This taxable portion is included in total income. To add in the non-taxable portion, we subtract lb from la and add that amount to total income.

whereas the developer's application data may be observed anytime during the year. As such, any changes to household composition (e.g., marriages, separations, births, deaths) that occur after the application date may lead to discrepancies between the two sources.

In Section 6.1, we describe how we construct a panel of labor market outcomes by linking applicants to their tax records using Social Security Numbers.

3.4 Publicly Available Data

HUD LIHTC Property Database: HUD maintains a publicly available database of all LIHTC-funded projects placed in service since the tax credit's founding in 1986. These data include developer identifiers, property names and addresses, placed-in-service and credit allocation dates, whether the property targets specific populations (e.g., families, elderly, disabled, homeless), unit counts (total and subsidized), funding sources, and several variables related to the siting of properties. We use these data to compare the properties in our sample to other LIHTC properties. We also fuzzy match the sample of properties to the HUD database to supplement the set of property-level variables available from the developer.[14]

Opportunity Atlas: These data provide Census tract-level variables capturing observed and predicted socioeconomic mobility indicators Chetty et al. (2014), which we use for our neighborhood quality outcomes. We focus on two opportunity measures: (1) the predicted probability of reaching the top income quintile, and (2) the predicted probability of adult incarceration, each conditional on being born to parents in the bottom quartile of income nationally. We caveat that these mobility measures represent neighborhood opportunity lagged by several decades, since they are constructed based on the childhood locations of current adults. We therefore supplement these measures with contemporaneous neighborhood attributes, including poverty rate, district-level 3rd grade math test scores, single parent share, share of non-white residents, job counts within five miles, and average annualized job growth rate.

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[14]One important shortcoming of these data are that they only include one address for each property, which is problematic for multi-site properties. These addresses also commonly reference property management offices instead of actual units.

3.5 Summary Statistics and Sample Representativeness

More than 2% of all U.S. households live in LIHTC-funded units (Soltas, 2023). LIHTC features prominently in most housing markets across the U.S. and so reflects national heterogeneity in neighborhood and housing conditions. We consequently recognize that there are limitations to what can be learned by analyzing data from a single developer. As a starting point, we compare features of properties in our sample to a national sample of all LIHTC-funded properties.

Table 1 provides summary statistics at the property level. Panel A (Property Characteristics) shows that over 90% of units in both property samples are income-restricted, well above HUD compliance minimums and consistent with a competitive credit allocation process that forces developers into greater low-income commitments than is minimally required. Vacancy rates are very low: only 4.2% of all subsidized units were vacant in 2023, consistent with strong demand and the LIHTC's requirement that all subsidized units be occupied within one year of the placed-in-service data, absent extenuating circumstances.[15]

Panel B (Tract Characteristics) merges geocoded property addresses to Census tract characteristics. Consistent with our provider's development footprint, properties in our sample tend to be located in areas with lower population density, higher White shares of the population, and higher homeownership rates relative to the typical LIHTC property. However, in terms of opportunity-related neighborhood measures— share with a bachelor's degree or higher, poverty rate, and single parent share—our properties resemble other LIHTC properties. LIHTC properties overall tend to be located in disadvantaged neighborhoods, with the average LIHTC tract falling near the 75th percentile nationally in terms of poverty rate and single parent share.[16]

In Panel C (HUD characteristics), we used fuzzy matching to successfully link 270/342 properties (79%) to HUD's LIHTC Placed-in-Service Database. We highlight the proportion of properties that are built in Qualified Census Tracts (QCTs), which are tracts where (1) >50% of households have incomes below 60% of the Area Median Gross Income, or (2) a poverty rate of 25% of more. Since 1990, federal statutes have provided properties in QCTs with a 30% tax credit boost. While this federal incentive has been shown to stimulate LIHTC construction (Baum-Snow and Marion,

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[15]The vacancy rate for all LIHTC two-bedroom units from 2010-2020 was approximately 4%.

[16]This is consistent with Ellen et al. (2018), who find that LIHTC units are located in neighborhoods with higher poverty rates, lower quality schools, and more polluted environments relative to other rental units.

2009), it also anchors development in the most disadvantaged neighborhoods (Ellen et al., 2015). We find that 15% of properties in our sample are located in QCTs, compared to 19% of all LIHTC properties.

Taken together, the properties in our data are located in areas that are more White and less dense compared to national LIHTC averages, but appear otherwise representative in terms of key measures of economic opportunity. Both groups exhibit high shares of subsidized units and excess demand for units. These similarities suggest that our findings could credibly generalize to other settings, especially to those where LIHTC development is concentrated outside of central cities.

4   Empirical Strategy

In this section, we begin by outlining a conceptual framework for modeling search and placement into LIHTC-subsidized units, followed by our empirical strategy for estimating the causal effects of LIHTC moves on measures of neighborhood quality and labor market outcomes.

4.1   Conceptual Framework

We first assume that low-income households are characterized by heterogeneous human capital (H_i) and preferences over housing attributes (ϕ_i). Households initially reside in market rate housing and are induced to apply to at least one LIHTC-subsidized unit due to an increase in current rents or some other budgetary shock. At the same time that they search for a LIHTC unit, households may apply to other market rate units. The probability of a vacancy opening for a LIHTC-subsidized unit or other market rate unit in any period is given by p_i, where i indexes households. Since the demand for and supply of subsidized and market rate units varies according to applicant and unit characteristics, p_i is a function of H_i and ϕ_i.[17]

In each period, households consider only the first option j that becomes available. When a vacant LIHTC-subsidized or other market rate unit becomes available, they choose according to the decision rule:

$$\max \left(\frac{\tilde{u}_{it}(j)}{1 - \delta}, V_{it} \right)$$

$\tilde{u}_{it}(j)$ represents the per-period flow utility of a prospective new unit j, which is also a function

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[17]We assume that the probability of a vacancy opening for a given individual i is time-invariant.

of H_i and ϕ_i because applicants with different underlying characteristics and preferences are likely to face a different set of outside options. δ is the discount factor, so the first term represents the present value of lifetime utility gained from the new unit. For parsimony we assume that once a household moves to a new unit (LIHTC or otherwise) they remain there indefinitely. The second term represents the value of continuation: rejecting the open unit, remaining at the application address, and searching again next period. We define the present value of continuation as:

$$V_{it} = (u_i - c_i) + \delta\mathbb{E}\left[(1 - p_i)(u_i - c_i) + p_i \cdot \max\left(\frac{\tilde{u}_{i,t+1}}{1 - \delta}, V_{i,t+1}\right)\right]$$

where u_i is per period flow utility from remaining at the application address, and c_i is the per period search cost. Both are time-invariant, known to the household, and are functions of H_i and ϕ_i. When a household opts to continue searching, they receive $(u_i - c_i)$ in the current period – i.e., the utility of remaining at the application address, net of search costs. In the next period, the household would again receive $(u_i - c_i)$ if no vacancy opens, which occurs with probability $1 - p_i$. If a vacancy opens (with probability p_i), the household's choice would depend on their specific draw of $\tilde{u}_{i,t+1}(j)$ from the distribution of $\tilde{u}_{i,t+1}$. Importantly, the household does not know *ex ante* which draw of $\tilde{u}_{i,t+1}(j)$ they will get next period, so the continuation value is a function of the expected value of $\tilde{u}_{i,t+1}$, which is also a function of H_i and ϕ_i.

This basic model illustrates several key features of our setting. First, the value of continuation is increasing in u_i and decreasing in c_i, which implies that households with better initial housing situations and lower search costs will tend to remain in place longer and will be less likely to accept a LIHTC unit for any time horizon. Second, the value of continuation is increasing in the expected value of a new unit ($\mathbb{E}(\tilde{u}_{i,t+1})$) but decreasing in the realized value of a vacant new unit ($\tilde{u}_{it}(j)$). In other words, households who expect to have many desirable options in the future will be more likely to pass on vacancies in general, but a good draw will make any household more likely to accept a given vacant unit. If we further accept the assumption that subsidized units tend to be more desirable than unsubsidized units, it follows that individuals who have the opportunity to rent a vacant LIHTC unit will be more likely to accept the new unit and will remain at the application address for a shorter duration, all else equal.

Before conditioning on H_i and ϕ_i, heterogeneous households are unlikely to have comparable

probabilities of placing into LIHTC-subsidized units. For example, households with high human capital may have earnings that price them out of some income-restricted LIHTC units, which could impact the value of continuation both by changing the expected value of new units ($\mathbb{E}(\tilde{u}_{i,t+1})$) and by influencing the probability of encountering a vacancy (p_i). Households may also have strong preferences for certain neighborhoods, which would alter the distribution of new units to which they would consider applying. Ultimately, any heterogeneity in H_i and ϕ_i that generates heterogeneity in p_i, u_i, c_i, and $\mathbb{E}(\tilde{u}_{i,t+1})$ is a potential source of selection into treatment. Since many of these factors are also likely to be correlated with our outcomes of interest, they will tend to confound causal estimates of the effect of moving to LIHTC.

Conditional on H_i and ϕ_i, the underlying (residualized) distribution of $\tilde{u}_{it}$ does not vary across applicants. Importantly, however, variation in treatment status across applicants can still result in practice from variation in the realized draw of $\tilde{u}_{it}(j)$. For example, consider two comparable applicants who apply for the same subsidized unit, just days apart. Since applications are typically reviewed in the order in which they are received, the applicant who was first to submit may have the first decision on the vacant unit. If they choose to accept it, the other applicant will be forced to continue searching. In the meantime, they would remain at their application address, and are strictly more likely to accept a market rate unit in some future period and cancel their LIHTC application. In this way, idiosyncratic factors related to the specific timing of application and churn at a given property can lead to good-as-random variation in treatment status, conditional on H_i and ϕ_i. In the next section, we discuss how our empirical design seeks to isolate variation in treatment that is orthogonal to H_i and ϕ_i.

4.2 Identification

To fix ideas, consider a simple setup with 2 time periods (t=1 and t=2) and 2 groups: a treatment group (D=1) and an untreated group (D=0). Treatment is defined as being placed into a subsidized LIHTC unit and occurs in period 2. In a potential outcomes framework, the target parameter is the average treatment effect on the treated, which is given by the estimand:

$$ATT = \mathbb{E}[Y_{i,t=2}(2)|D = 1] - \mathbb{E}[Y_{i,t=2}(\infty)|D = 1]$$

where $Y_{i,t}$ is a neighborhood level measure of opportunity that corresponds to household i's residence at time t. The first term, $\mathbb{E}[Y_{i,t=2}(2)|D=1]$, is interpretable as the mean outcome for households in the treated group after treatment, and is directly estimable from the data. In our context, this refers to the average neighborhood quality that individuals experience after moving to LIHTC-funded units. The second term, $\mathbb{E}[Y_{i,t=2}(\infty)|D=1]$, however, is not directly estimable, since we do not observe the counterfactual outcomes for treated individuals in the absence of treatment.

A key feature of our design is that we are are able to construct a panel of outcomes for both placed and non-placed applicants by linking households from the Yardi sample to their own records in the Infutor and IRS databases, respectively. As we discuss in Section 4.1, unconditional comparisons of applicants who do versus do not place into LIHTC units are likely to contaminated by various forms of selection bias, including important differences in the quality and quantity of outside options and the reservation utility of remaining in place. However, the panel structure of our data allows us to defend a weaker identification assumption; namely, we do not require that outcomes for successful and unsuccessful applicants are comparable in level terms, but only that the relative difference in mean outcomes across treatment groups would have remained constant in the absence of treatment ("parallel trends"). Formally, returning to a 2 group $\times$ 2 period setup:

$$\mathbb{E}[Y_{i,t=2}(\infty)|D=1] - \mathbb{E}[Y_{i,t=1}(\infty)|D=1] = \mathbb{E}[Y_{i,t=2}(\infty)|D=0] - \mathbb{E}[Y_{i,t=1}(\infty)|D=0]$$

Assuming that there is no anticipation of treatment by treated individuals, this can be rewritten as:

$$\mathbb{E}[Y_{i,t=2}(\infty)|D=1] - \mathbb{E}[Y_{i,t=1}(2)|D=1] = \mathbb{E}[Y_{i,t=2}(\infty)|D=0] - \mathbb{E}[Y_{i,t=1}(\infty)|D=0]$$

Rearranging terms:

$$\mathbb{E}[Y_{i,t=2}(\infty)|D=1] = \mathbb{E}[Y_{i,t=1}(2)|D=1] + \mathbb{E}[Y_{i,t=2}(\infty)|D=0] - \mathbb{E}[Y_{i,t=1}(\infty)|D=0]$$

The term on the left-hand side is the counterfactual component that we cannot directly estimate from the data. However, it is now a function of terms that are all directly estimable. Plugging

back into the expression for the ATT:

$$ATT = \mathbb{E}[Y_{i,t=2}(2)|D=1] - (\mathbb{E}[Y_{i,t=1}(2)|D=1] + \mathbb{E}[Y_{i,t=2}(\infty)|D=0] - \mathbb{E}[Y_{i,t=1}(\infty)|D=0])$$

The "parallel trends" assumption is more likely to hold when identifying variation comes from comparisons of applicants who are observably similar; in other words, when we hold human capital (H_i) and preferences over housing attributes (ϕ_i) constant, such that variation in treatment status is only attributable to good-as-random draws from the residualized distribution of $\tilde{u}_{it}$. We use two variations on an event study design – one for each set of outcomes, respectively – to isolate plausibly exogenous variation in treatment status among applicants.

When estimating the effect of moving to LIHTC on neighborhood-level outcomes, we estimate the following regression:

$$Y_{it} = \alpha + \mu_i + \delta_t + \theta_{it(p)} + \sum_{t=-24}^{-2} u_t D_i + \sum_{t=0}^{24} u_t D_i + \mathbf{X}_{it}'\Pi + v_{it} \tag{1}$$

where i indexes individual applicants and t indexes months in event time; i.e., relative to the month of application (t=0). In our preferred specifications, Y_{it} is a continuous variable. In supplementary results, we re-estimate Equation 1 using a binarized transformation of Y_{it} that compares the outcome for individual i at event time t to that individual's observed outcome at time of application. The variables μ_i and δ_t represent applicant and event time fixed effects, respectively, so the u_t coefficients are identified by variation in outcomes within an applicant's address history over time. We also include $\theta_{it(p)}$, a fixed effect for the property p that individual i applied to interacted with event time, which further restricts identifying variation to comparisons of applicants within the same LIHTC property. The vector $\mathbf{X}_{it}'$ contains additional controls that are meant to improve precision and strengthen the case for the (conditional) exogeneity of treatment. Specifically, to account for possibly endogenous factors related to application timing, we include fixed effects for combinations of the calendar month and year of application interacted with event time. We also include a baseline measure of the tract-level outcome (corresponding to measurement in the application tract) interacted with event time fixed effects. This control allows us to restrict attention to comparisons between applicants from the same sending neighborhood, who are more

17

likely to be similar in terms of both H_i and ϕ_i. Finally, motivated by the differences between treatment and control individuals that we observe in Table 2, we explicitly control for observable characteristics that proxy for ϕ_i including property fixed effects, bedroom preference, and age at time of application, all interacted with event time.[18] We cluster standard errors at the individual level, which reflects the unit of randomization into treatment (Abadie et al., 2023).

To estimate the effects of moving to LIHTC on labor market outcomes, we use a two-step estimation process that incorporates a nearest neighbor matching approach with our event study design. For each applicant who places into a LIHTC-subsidized unit, we aim to identify a comparable applicant who does not place into a LIHTC-subsidized unit to serve as a valid counterfactual. We form pairs between treated and control units using a logit model that predicts the probability of treatment using baseline characteristics:

$$Treat_i = f(\beta_x X_i) \tag{2}$$

where $f(\cdot)$ represents the logistic function. All covariates included in X_i are measured as of the year prior to the application year. These covariates include the applicant's wages, approximate HUD income, the count of unique W-2 forms received per filer in the tax unit, indicators for a set of age bins,[19] the year and month of application, stated preference for number of bedrooms in the LIHTC unit, and a property-level fixed effect.[20] We depict predicted propensities for both treatment and control groups in Figure 4.

With these treatment probabilities in hand, we proceed to construct matches between treatment and control units. We require that each matched pair apply to a LIHTC unit from the same sending zip code, which we argue eliminates unobservable differences between treated and control units that are a function of H_i and ϕ_i and may be correlated with treatment status. To ensure that the matched units are sufficiently similar, we further require that the absolute value of the difference in predicted treatment probability between treatment and control units not exceed the 90th percentile observed in the data.[21] We note that while each member of the treatment group is assigned to

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[18]Unit preference and age variables occasionally contain missing values. When data is missing, we impute the variable to $= 0$ and include dummy variables in the regression that indicate the presence of missing data.

[19]There are four bins: [19,25), [25,35), [35,55), [56,84].

[20]Note that the predicted probability of treatment is undefined when some combination of covariates perfectly predicts treatment status. This removes approximately half of applicants from our sample.

[21]This value is equal to 0.37.

exactly one control unit, it is possible for a control unit to be assigned to multiple treatment units. As a result, the effective sample size is larger than the number of unique applicants due to repeated observations of certain control units.

We then estimate the treatment effect of LIHTC residency on income and wages using the following variant of (1):

$$Y_{it} = \alpha + \mu_i + \delta_t + \lambda_{it(g)} + \sum_{t=-4}^{-2} u_t D_i + \sum_{t=0}^{2} u_t D_i + v_{it} \tag{3}$$

In addition to the variables defined above, $\lambda_{i(g)}$ represents a fixed effect for the pair group g to which applicant i belongs. As such, identifying variation in this specification comes only from within-pair variation. We cluster standard errors at the applicant level.

5 Effects on Mobility

5.1 Sample Construction

We construct our mobility analysis sample from individual-level application records covering all applicants since 2012. We drop observations with missing identifiers (full name, date of birth, or application at address), missing application date, or with multiple applications (whether at the same property or across properties).[22]

We link the remaining Yardi observations to Infutor using name, month and year of birth, and the zipcode associated with the application address.[23] Overall, we successfully match about 45% of our applicant sample to Infutor address records, consistent with other studies using Infutor to link to external, individual-level data (Collinson et al., 2024). For matched records, we construct a panel of address histories by sequencing Infutor addresses according to the "effective date", which corresponds to the date at which Infutor first observes an individual as residing at a given address. We interpret effective dates as proxies for address start dates, following Phillips (2020).[24] We

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[22]The last of these sample restrictions helps us in three ways. First, it prevents contamination by individuals who are in the control group for one property but in the treatment group for another. Second, multiple applications may signal unobservable differences that are possibly correlated with treatment. Finally, in some cases, LIHTC residents can re-apply to move to a different unit within the same property. Moves that result from such re-applications do not provide useful variation for our study.

[23]We allow for fuzzy matches on name, but require exact matches on month and year of birth and on zipcode.

[24]As Phillips (2020) shows in validation exercises from Hurricane Katrina migration and public housing closures

geocode each address and link the corresponding Census tracts attributes.[25]

We apply several final sample restrictions on the linked Yardi-Infutor mobility sample. First, we require that each applicant's Yardi address matches to at least one address in their Infutor record.[26] We also exclude applicants with multiple addresses in their Infutor history that share the same effective date. Our understanding, based on conversations with Infutor data specialists, is that this issue is related to the manner in which Infutor ingests data from its various sources, and it is not possible to disambiguate these cases using other available data.[27] Finally, to ensure that we have a balanced panel, we restrict to applicants for whom we observe address histories for at least two years before and after the application date.

Our final analysis sample contains 4,556 applicants who are associated with 351 unique properties. Of the 4,556 applicants, 41.2% (1,878) are identified as residents; i.e., successful applicants who moved in to a unit in a LIHTC-funded property. The remaining group of non-resident applicants have a variety of application statuses: 29% ultimately canceled their application, 15.7% were denied due to ineligibility, and 13.9% remain on a waitlist. In the analyses that follow, we classify residents in the treatment group and non-residents of any application status in the untreated group.[28]

We present summary statistics for the main sample of applicants in Table 2. Columns (1) and (2) present unconditional means for non-residents and residents, and column (3) reports the raw difference in means. We also test for differences at time of application using the following regression

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in Chicago, Infutor's effective date appears to be a reasonable proxy for the timing of moves. For any non-terminal address in an individual's history, we can use the effective date as the "begin date" for any address and the effective date of the subsequent address as the "end date." However, for terminal addresses (i.e., an individual's most recent address) we cannot directly define an "end date." In such cases, we assume indefinite continuity at the most recent address, unless Infutor explicitly observes that the individual is deceased.

[25]We use 2010 Census tract boundaries. Tract-level data are from the TIGER shape files provided by the U.S. Census Bureau. We employ *geoinpoly*, a user-written Stata command (Picard, 2015), to execute the spatial join of latitude and longitude to shape files containing tract-level information.

[26]This restriction serves two purposes. First, such individuals may have applied with an address that does not correspond to their actual place of residence (e.g., a relative or partner's address). A non-validated address could also be evidence of measurement error in Infutor; i.e., a "missed move." In either case, we cannot confidently infer the sequencing or timing of moves within an individual's Infutor address history.

[27]The effective date corresponds to the date that Infutor first observes an individual at an address. However, if Infutor ingests more than one novel address for an individual from the same source at the same time, they attribute the same effective date for each address. In some but not all cases, records with identical effective dates may be resolved over time as additional data is ingested and linked to an individual's history.

[28]We are continuing to find ways to increase our sample size by reducing attrition from matching and address validation, which in turn will allow us to be more selective about the composition of the untreated group.

equation, estimated by OLS:

$$X_i = \beta_0 + \beta_1 Resident_i + \theta_p + \varepsilon_i \tag{4}$$

where the dependent variable X_i is an observable characteristic at baseline, $Resident_i$ is a treatment indicator equal to 1 if the applicant is a placed resident, and θ_p is a property-level fixed effect. Column (4) reports the β_1 coefficient for each baseline characteristic X_i, which is interpretable as the "regression-adjusted" difference in means from columns (1) and (2). $\beta_1 > 0$ implies that the resident group mean is greater than the non-resident group mean when using only within-property variation.

Panel A summarizes applicant characteristics. Residents tend to be slightly older than non-residents, although much of this difference dissipates after controlling for property fixed effects, which suggests that age differences are mostly driven by selection at the property level. Even within properties, however, residents experience significantly higher quoted rent than non-residents. Since bedroom preferences are similar, we interpret this difference as indicative of increased competition for units with deeper subsidies within the same property.[29] We also characterize applicants based on the the geographic proximity of their application address and the LIHTC address. Nearly half of successful applicants apply from the same zipcode where the LIHTC-funded property is located, and more than a quarter apply from the same tract. This is consistent with existing evidence that most LIHTC movers are local (Derby, 2021).[30] By comparison, non-residents are less likely to apply from the local area, which may partly explain lower placement rates if they are less willing to wait or relocate. These imbalances may reflect unobservable differences between treated and untreated applicants, which we address in Section 4.

Panel B compares characteristics of applicants' "origin" tracts. Successful and unsuccessful applicants generally come from similar neighborhoods, with regression-adjusted differences that tend to be statistically insignificant or small in magnitude. Although our empirical strategy

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[29]LIHTC rents are not based on a tenant's income, but on affordability rules stipulated by HUD. Property owners have some flexibility to set rents at the unit level, as long as the overall composition of rents remains compliant with affordability rules at the property level.

[30]Using administrative data from tax records, Derby (2021) finds that people who move to LIHTC-funded units often move from nearby addresses. As we discuss in Section 4, however, we cannot infer from this fact that moves to LIHTC are unlikely to impact neighborhood quality on average. Such a conclusion could only be drawn by making comparisons to a relevant counterfactual, as we do in the main analysis that follows.

relies on the comparability of outcome trajectories instead of levels, this result is encouraging because it suggests that the outside option of remaining in place offers similar opportunity for both groups. Although we do not directly observe applicant race in our data, we find that unsuccessful applicants tend to come from significantly less white tracts, though this imbalance disappears with property fixed effects. To the extent that tract-level racial composition proxies for applicants' race, this suggests that non-white applicants may be disproportionately applying to more competitive properties.

Panel C reports property-level characteristics. Most differences are small or statistically insignificant. A notable exception is that, compared to residents, non-residents are much more likely to have applied to a property that specifically targets families. Such properties make up about one-third of our the properties in our sample. These properties may face greater demand, resulting in longer wait times and higher cancellation rates.

5.2 Descriptive Results

Before turning to main outcomes, we first analyze move activity to assess the reliability of the Infutor data for tracking month-level move timing and to understand how migration outcomes evolve before and after treatment for residents and non-residents. Figure 5 plots monthly move rates (i.e., moves per 100 applicants) separately for residents (solid line) and for non-residents (dashed line), with $t = $ -1 denoting the month immediately before application. Move activity is virtually identical across groups prior to the month of application, suggesting that non-residents provide a compelling counterfactual for residents in terms of baseline migration propensity in the absence of treatment. At $t = 0$, migration activity sharply increases among residents and further increases in $t = 1$. This result suggests that despite known measurement issues in the Infutor data, it nonetheless appears that Infutor's effective dates serve as reasonable proxies for move timing at monthly frequencies. Move activity quickly decays but remains elevated in the treatment group for up to 9 months after the month of application, potentially reflecting residual waitlist activity. Non-residents also experience an uptick in move activity around the time of application, though less than half the resident rate and with slower subsequent decay. In short, conditional on failing to secure placement into a unit, non-residents are more likely to remain at their application address, though many of these individuals eventually do move from their application address, and in general

22

experience less housing stability than placed applicants in the post period.

Turning to neighborhood outcomes, we plot mean outcomes for each group by event time in Figure 6. Pre-application, levels differ modestly between residents and non-residents. Although some measures suggest that residents tend to live in lower opportunity neighborhoods (higher poverty rates, lower predicted income mobility, lower job growth rates), others suggest that they live in higher opportunity neighborhoods (lower single parent share, lower predicted incarceration, higher math scores). In general, pre-application levels appear visually parallel across most neighborhood attributes, with the exception of job growth.

Post-application, neighborhood poverty rates clearly diverge, driven by non-resident moves to lower-poverty tracts. We note that without this counterfactual group, mobility estimates based off of changes in outcomes for LIHTC residents alone would be significantly attenuated. Predicted income mobility, predicted incarceration, and math score measures all similarly indicate opportunity-decreasing moves for LIHTC residents, though the evidence is less stark. The job concentration (jobs within 5 miles) measure slightly complicates this narrative, suggesting that placed applicants end up in tracts with greater job density.

5.3 Event Study Results

We separately estimate Equation 1 for each outcome and report the results in Figure 7. In each case, point estimates (solid lines) represent the regression-adjusted mean difference in treatment group outcomes (residents minus non-residents) at event time t for the specified outcome. Dashed lines represent the upper and lower bounds of the 95% confidence interval.

We find that within about one year of application, individuals who move to LIHTC-funded units reside in tracts with higher poverty rates (panel a), higher predicted incarceration rates (panel c), and lower income mobility (panel f) relative to comparable non-residents. There is no evidence of pre-trends for these outcomes, and treatment effects tend to increase over time. Two years after application, residents live in tracts with 1 pp higher poverty rates (+5.5% over base period mean), 0.175 pp higher predicted incarceration rates (+7%) and 0.35 pp lower income mobility (−4%). In each case, we observe a clear trend break in outcomes coinciding with the month of application. Though not statistically significant, we also find suggestive evidence that individuals who move to LIHTC-funded units experience relative declines in neighborhood 3rd grade math scores (panel d).

23

Back-of-the-envelope calculations suggest that these neighborhood effects correspond to substantial impacts on long-run outcomes for both adult tenants of LIHTC-subsidized units and their children. Using data from the MTO experiment, Ludwig et al. (2012) find that a 13 pp decrease in neighborhood poverty increases subjective well-being by an amount equal to the gap in subjective well-being between people whose annual incomes differ by $13,000 (in 2009 USD). Although this estimate is unlikely to scale linearly across treatment doses, it nonetheless suggests that a 1 pp *increase* in neighborhood poverty may have measurable negative effects on adults' well-being. Moreover, Chetty et al. (2016) find that children who move to lower poverty neighborhoods before age 13 earn $3,477 more, on average, in annual income as adults (aged in mid-20s). Although the difference in neighborhood poverty rates was much more pronounced for movers in that study than in our context, we expect that there would likely be detectable negative impacts on adult earnings for children who move to LIHTC-funded properties in higher poverty neighborhoods, though these effects may only be on the order of a few hundred dollars per year.

The effects on poverty rates, predicted incarceration rates, and income mobility suggest that moving to LIHTC-funded units tends to decrease neighborhood opportunity on average. However, opportunity is multifaceted, and for many neighborhood outcomes we do not find statistically significant effects—with the caveat that these null effects are often not estimated precisely enough to rule out economically meaningful treatment effects in either direction. For example, for the local job concentration measure (jobs within 5 miles), we cannot rule out positive or negative effects up to 20% of the base period mean two years after application.

We unpack the variation underlying these treatment effects by first analyzing differences in outcomes between an individual's application (origin) tract and the LIHTC property's (destination) tract, for all residents in our main sample. We plot the full distribution of individual-level changes separately for each outcome in Figure 8.[31] The x-axis corresponds to the origin tract, and the y-axis corresponds to the destination tract. Intuitively, the further away a data point is from the 45 degree line, the greater is the implied change in the neighborhood outcome that results from moving to a LIHTC unit. For most measures, we observe considerable heterogeneity in individual-level outcomes, as represented by dispersion about the 45 degree line. In some cases

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[31]We use Stata's *binscatter* program to non-parametrically group individual-level data into bins of equal observations.

(e.g., single parent rate) there is a wide range in observed changes from sending to receiving tract, but this variation is masked in the aggregate because the prevalence of opportunity-improving moves is roughly symmetrical to the prevalence of opportunity-reducing moves. This underlying heterogeneity suggests that moving to LIHTC creates a mix of winners and losers with respect to neighborhood opportunity, even in cases where there is little to no average change. In addition, we find evidence that the relative increases in the poverty rate from our event study analysis is driven, at least in part, by moves by treated individuals to LIHTC units in poorer tracts: in panel (a), the mass of data points lie above the 45 degree line, which indicates that placed individuals tend to end up in higher poverty tracts than they applied from. The results for the income mobility measure (panel f) are more mixed; most residents end up in tracts that are fairly similar to their application tracts, but there is a subset of individuals who start off in very high mobility tracts and do much worse after moving to LIHTC.

With the insights from this distributional analysis, we return to our event study design but now re-specify the outcomes of interest to capture effects on the *extensive* margin. Specifically, we construct dummy indicators for whether an applicant's current tract at event time t is a higher (or lower) opportunity tract than their tract at the time of application. We present results in Figure 9, focusing on the three opportunity measures for which we detect statistically significant treatment effects on the intensive margin. For each measure, we estimate separate regressions using indicators for (1) an increase relative to the application tract and (2) a decrease relative to the application tract.

Consistent with our results from Figure 8, we find evidence that average effects mask considerable heterogeneity. In all cases, treated individuals are more likely to end up in both higher opportunity neighborhoods *and* lower opportunity neighborhoods compared to non-residents after application. Part of this simply reflects their greater initial propensity to move (see Figure 5). However, the time path of treatment effects points to more complicated dynamics. Focusing on the poverty rate outcome (panels a and b), effects on both extensive margins (relative increases and relative decreases) are typically comparable in magnitude for a few months immediately after application. Given the increasing effect size on the intensive margin over this time period (in Figure 7), and that moves by residents drive most of the variation in this timeframe, this result suggests that some placed residents move to much poorer neighborhoods while a roughly equal

25

share move to just slightly less poor neighborhoods. This interpretation is broadly consistent with the heterogeneity we observe in Figure 8.

However, over time, moves by non-residents account for an increasing share of the variation and we begin to see treatment effects for opportunity-improving moves converge back toward zero. During this period, non-residents increasingly make opportunity-improving moves while residents tend to remain in place, an interpretation supported by Figure 6. We observe similar dynamics for the predicted incarceration rate and income mobility measures.

5.4   Heterogeneity Analyses

We close the analysis by testing for heterogeneous effects based on property and applicant characteristics, focusing on the three outcomes for which we detect statistically significant effects in the aggregate analysis. First, for most of our measurement period, projects built in Qualified Census Tracts (QCTs), were awarded a 30% increase in tax credits. Previous work shows that these extra incentives had measurable impacts on the spatial distribution of LIHTC-funded housing: QCTs received twice as many subsidized units on average (Baum-Snow and Marion, 2009). Because QCTs are, by definition, the most economically disadvantaged neighborhoods in each metro area, critics have long argued that the incentive risks "locking in" LIHTC tenants in low-opportunity areas.

We re-estimate event studies separately for applicants to properties in QCT and non-QCT tracts and present the results in Figure 10. The results strongly suggest that QCTs play a central role in driving applicants to low-opportunity neighborhoods. For all three measures, we find that virtually all of the difference in aggregate outcomes between residents and non-residents is driven by properties located in QCT tracts. By contrast, we estimate relatively precise null effects when comparing applicants to properties in non-QCT tracts. Overall, we view these results as clear evidence that the design of developer incentives mediates the impacts of moving to LIHTC on neighborhood opportunity.

We also use our rich individual-level data to explore whether effects are heterogeneous by applicant characteristics. Although we do not directly observe the number of individuals in each applicant's household, we use applicants' unit size preferences – specifically, the preferred number of bedrooms – as a proxy for family size. We re-estimate effects on the intensive margin separately for individuals seeking 1, 2 and 3+ bedroom units and present the results in Figure 11. In each

26

case, we find that effects are concentrated among applicants seeking 1 bedroom units, with smaller or null effects for applicants seeking larger units. One potential interpretation is that applicants for smaller affordable units face more competition given the relative scarcity of such units,[32] and so are more willing to trade off neighborhood quality for subsidized rents.

Given that previous work shows that the effects of neighborhood quality are highly heterogeneous by age (Chetty et al., 2016), we also run event studies by age group and present the results in Figure 12. The key pattern that emerges from this analyses is that effects tend to be driven by individuals aged 45-64. Taken together with the unit size analysis in Figure 11, this result suggests that individuals who are most likely to be on the margin for making opportunity-reducing moves to subsidized LIHTC units are couples or single adults who are either childless or whose adult children have left the household, but who have not yet reached retirement age. It is possible that such households are more willing to trade off neighborhood quality for affordable rents than younger families, who may be more concerned about local factors that influence the development of children. This distinction matters for interpreting downstream impacts: if the households most likely to move into lower-opportunity neighborhoods are those for whom neighborhood conditions have weaker long-run consequences, the negative implications for intergenerational mobility may be less severe.

6 Effects on Labor Market Outcomes

6.1 Sample Construction

Given the differences in coverage, frequency, and richness between the Infutor data and tax data, we briefly describe differences in sample construction. To construct the sample for earnings outcomes, we begin by matching individual-level records from the Yardi database to IRS tax records available through the U.S. Department of the Treasury. We match individuals to their tax records by exact matching on Social Security Number.[33] As in the mobility analysis sample, we drop observations that are missing an application date and any individuals associated with more than one application.

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[32]There is some empirical evidence that shortages of affordable units in major U.S. housing markets tend to be most severe for smaller units (i.e., studios and 1 bedroom units) (Scott, 2014). Anecdotally, property managers interviewed for this study also noted that smaller units tend to be easier to lease up than larger units.

[33]71.7% of individuals in the Yardi database have valid SSNs (must be non-missing and in an acceptable SSN format).

And, as before, we limit the sample to only include individuals who applied to a LIHTC-funded property in 2012 or later.

As tax data are available at the annual level instead of monthly, we have fewer observations per applicant in the Yardi-tax data sample than in the Yardi-Infutor sample. To make the most of our sample, we focus on applicants to LIHTC properties in the years when the volume of those applicants was greatest: between 2012 and 2021. Due to data availability at time of writing, this limits our post-period to two years following the year of application. We include a longer pre-period, which begins 4 years prior to the application year, to observe a longer range of pre-treatment outcome trajectories.

Finally, to construct a balanced panel of outcomes, we limit our sample to those individuals for whom we observe either a Form 1040 individual tax return or a Form W-2 employee wage statement in each year, from four years prior to application to two years after application. There is likely to be selection into the resulting analysis sample, as this restriction excludes any individuals who ever lack labor market earnings (as observed on 1040s and W-2s). However, since we cannot reliably infer outcomes for individuals without at least one of the two forms in a given year,[34] this restriction allows us to make internally valid comparisons that are not confounded by changes in sample composition over time that may result from selection into measurement.[35]

The final analysis sample includes only those treatment and control individuals who were successfully paired as described in Section 4. There are 6,246 unique applicants in the final sample, though we note that since it is possible for a single control unit to be paired with multiple treatment units, our "effective" sample size exceeds the number of unique applicants.[36]

We present summary statistics for the final analysis sample of applicants in Table 3. Columns (1) and (2) present unconditional means for non-residents and residents, respectively, and column (3) reports the raw difference in means. We also test for differences at time of application using

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[34]When we observe an individual's W-2(s) but not 1040, we impute income to equal the sum of W-2 wages, unemployment compensation from Form 1099-G, and interest income from Form 1099-INT.

[35]While we observe W-2s for any individual who is listed on at least one employer's payroll, we only observe 1040s for individuals who file taxes in a given year, or who are claimed as a dependent of or co-file with a tax filer in the same household. While filing rates are generally high among those required to file ($> 90\%$), not all Americans are required to file. The most important exception in this context is the minimal income filing threshold, which exempts very low-income households from the tax filing requirement. In 2024, the threshold for household heads without children under age 65 was \$21,900. Previous work has shown that filing rates for households under the minimal income threshold can be substantially lower (Hauck and Wallossek, 2024).

[36]66% of unique applicants in the final sample are treated.

28

Equation 4 with property-level fixed effects, as in Section 5.1, and report the regression-adjusted differences in means in column (4). We measure each characteristic in the year prior to application. We observe statistically significant and large differences between residents in non-residents along a set of application characteristics including bedroom preference, age, and number of dependents. These results suggest that there is considerable selection into LIHTC tenancy, an issue that motivates our nearest-neighbor matching strategy. However, once we include property-level fixed effects, there is virtually no difference in the rate of filing or filing status across treatment and control individuals, which suggests that our sample restrictions are effective in mitigating possible bias from selection into measurement.

6.2 Descriptive Results

We plot unconditional mean outcomes for residents (treatment) and non-residents (control) by event time in Figure 13.[37] For both HUD income (panel (a)) and W-2 wages (panel (b)), pre-period outcomes are very similar across the two treatment groups, both in levels and in changes. Therefore, although residents and non-residents may differ along some observable dimensions, they appear to be comparable in terms of labor market outcomes in the years leading up to application.

Although mean income and wages increase for both groups starting in the year of application ($t = 0$), non-residents' experience much greater growth in outcomes relative to LIHTC residents in the post-application period. Specifically, average wages are nearly identical between the two groups in the year prior to application, but non-residents earn about \$2,500 more on average during the year of application and about \$3,200 more on average two years after application.

Why do income and wages increase after application in both groups? We speculate that individuals who apply to LIHTC-subsidized housing likely experience a shock—for example, an increase in current housing rents—in the year of application that simultaneously trigger searches for cheaper housing and higher labor market earnings. A joint housing and job search of this nature could result in higher earnings for successful applicants, especially for those who receive a relatively modest LIHTC subsidy. In future work, we hope to shed more light on the specific factors that drive income and wage growth in the post-application period.

[37]Both 1040 income and W-2 wages are measured in 2019 USD.

6.3 Event Study Results

The descriptive results in the previous section suggest that applicants who place into LIHTC-subsidized units experience reductions in income and wages relative to similar but unsuccessful applicants. In this section, we formally test for differences in outcomes using the matching strategy and event study model described in Section 4.

We present the results for income and wages in panels (a) and (b), respectively, of Figure 14. The event study figures depict point estimates and 95% confidence intervals across event time, with the base year (the year prior to application) omitted. We do not find evidence of differential pre-application trends across treatment groups for either outcome. Starting in the year of application ($t = 0$), however, we detect negative and statistically significant effects of moving to LIHTC on both income and wages. During the application year, residents' wages decline by \$2,000 relative to comparable non-residents, about a 12% decline from the base period mean. Two years after application, the difference in wages grows to nearly \$3,000, about an 18% decline from the base period mean. The time path and magnitude of treatment effects are very similar for both income and wages; since HUD income is comprised of both wages and income from various other sources, the loss of labor market earnings by LIHTC tenants does not appear to be compensated by gains from other sources.

The magnitudes of the treatment effects are comparable to those found in other studies on the effects of housing subsidies on income/wages. For example, Susin (2005) finds that moving to public housing is associated with a 19% decline in earnings, while Jacob and Ludwig (2012) estimate that voucher receipt reduces quarterly earnings by 10%. However, unlike public housing subsidies and housing vouchers, LIHTC subsidies do not affect net wage rates by reducing benefits as labor market earnings increase. In the absence of such substitution effects, our expectation was that the effects of LIHTC subsidies on earnings would be less than those associated with voucher and public housing subsidies. On the one hand, we cannot rule out that our larger estimates are an artifact of our selected sample of individuals, who either file tax returns and/or have labor market earnings for seven consecutive years around the time of application. Such individuals are likely to be more attached to the labor market and may thus have relatively high levels of (and variation in) earnings compared to individuals who are out of the labor force for multiple years.

On the other hand, it is possible that our estimates do not necessarily represent pure income effects due to possible complementarities between LIHTC and housing voucher programs. Since LIHTC properties are prohibited from discriminating against prospective tenants on the basis of voucher status, it is possible that at least some of the placed tenants in our sample are voucher holders. For these individuals, the value of the subsidy tied to the voucher would be reduced as earnings increase, which may still generate substitution effects even though the LIHTC subsidy is, under most circumstances, not a function of a tenant's individual earnings after initial move-in.

To further contextualize the impacts on labor market outcomes, we conduct a "back-of-the-envelope" exercise to compare the estimated effects on income and wages to estimates of the housing cost savings associated with moving to LIHTC units. There are two primary factors that contribute to reduced housing costs for LIHTC movers: (1) changes in average market rents between the sending and receiving neighborhoods and (2) the difference between LIHTC rents and market rents in a given neighborhood. The former gives the average change in housing costs that an individual would expect if they moved from a market rate unit in their application neighborhood to a market rate unit in the neighborhood of the LIHTC property to which they applied; the latter relates to the within-neighborhood value of the LIHTC subsidy.

We first estimate the change in market rents between sending and receiving neighborhoods using tract-level data from the 2019 American Community Survey (5-year estimates). We find that the neighborhoods that successful LIHTC applicants move to are cheaper than where they moved from. Specifically, average annual housing costs are about $1,300 less in the neighborhoods in which successful applicants reside two years after application relative to their neighborhood in the year prior to application. We then draw on existing work by Cook et al. (2023) to indirectly estimate the likely distribution of LIHTC subsidies in our sample.[38] Cook et al. (2023) estimates the range of "implicit" LIHTC subsidies across neighborhood types in the Chicago metro area, where the implicit subsidy is equal to the difference between predicted market rents and documented LIHTC rents. Since rents in Chicago tend to be much higher than rents in the markets in our sample, we adjust Cook et al. (2023)'s estimates downward to match the composition of our sample.[39] Based

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[38]In future work, we hope to directly estimate this distribution using the developer's internal data.

[39]An implicit assumption with this approach is that LIHTC subsidies scale linearly with average market rents. This is a strong assumption, but is generally consistent with Cook et al. (2023)'s finding that subsidies tend to be largest in the highest opportunity neighborhoods and smallest in the lowest opportunity neighborhoods.

on this approach, we estimate that the annual value of LIHTC subsidies for the properties in our sample ranges from approximately $1,500 to $5,000. Therefore, the average housing cost reduction associated with moving to LIHTC – including both the between-neighborhood change in average market rents and the within-neighborhood value of implicit subsidies – is about $2,800 (= $1,500 + $1,300) to $6,300 (= $5,000 + $1,300).

Finally, we compare our treatment effect estimates to the above estimates of housing cost savings. Two years after application, annual wages for LIHTC residents are about $2,900 less than those of non-residents (see panel (b) of Figure 14), which implies that roughly 46-103% of housing cost savings are offset by reductions in earned wages. We interpret these specific figures with caution, but are reassured by the general finding that the estimated negative impacts on wages and income likely do not exceed the housing cost savings associated with moving to LIHTC-subsidized units. In our view, therefore, the magnitudes are indeed large but not implausible. This exercise also suggests that most LIHTC renters likely break-even – or even come out ahead – financially despite working relatively less, which would tend to increase overall tenant welfare.

7 Conclusion
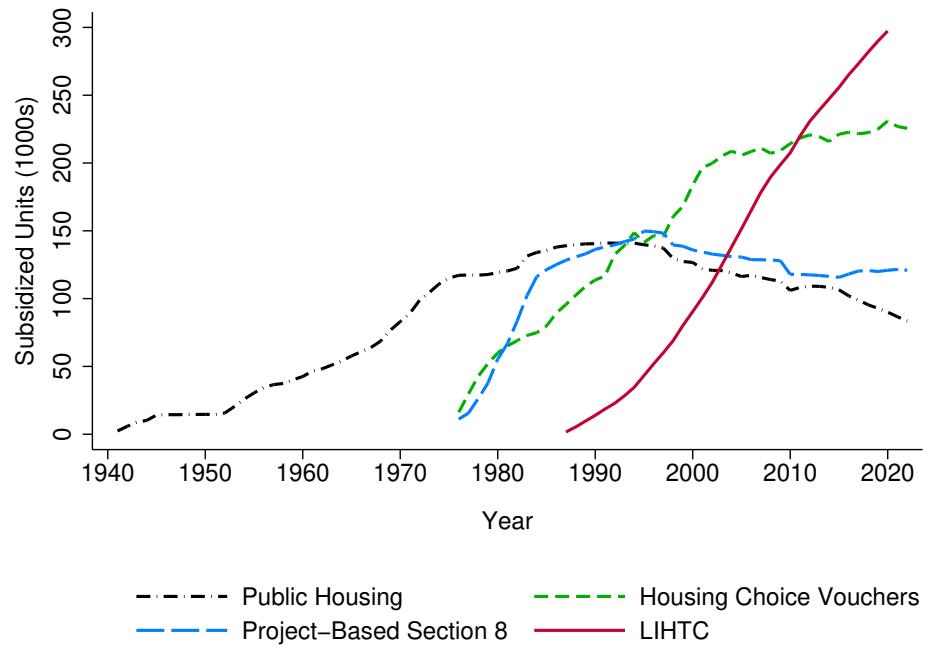
This study provides the first causal evidence on how the Low-Income Housing Tax Credit—the nation's largest source of affordable housing units—affects individual renters' neighborhood environments and earnings. Using novel data on applicants to LIHTC properties linked to administrative address histories and tax records, we find that placement into LIHTC units often leads to residence in lower-opportunity neighborhoods and, within two years, to declines in labor market earnings. These patterns are concentrated in developments sited in economically disadvantaged Qualified Census Tracts where LIHTC developers receive boosts to their tax credits, underscoring the importance of siting incentives in shaping outcomes. An important implication of these results is that although the LIHTC in its current form may not reliably expand access to high-opportunity neighborhoods, specific federal and state incentive structures will likely continue to play an outsized role in determining whether families are incentivized toward or away from disadvantaged areas. Some states have already experimented with incorporating opportunity-based criteria in their Qualified Allocation Plans, which detail how prospective LIHTC development applications are

scored. As such, the impacts of the LIHTC on economic opportunity are not pre-determined, but can in fact be shaped by the decisions and priorities of policymakers.

Our findings also point to broader implications for housing policy. While demand-side subsidies like vouchers directly enable families to choose where to live, they remain constrained by frictions such as information and landlord resistance (Bergman et al., 2024). Supply-side incentives like the LIHTC can, in principle, complement vouchers by creating a stock of affordable units in high-opportunity neighborhoods. Our results highlight both the promise and the limitations of supply-side subsidies as mobility tools. Understanding how these two approaches interact, and how siting rules can be refined to maximize opportunity gains, remains a pressing area for future research. In its current form, ensuring that the LIHTC expands access to opportunity will require careful attention to how its incentives guide development, and to the tradeoffs low-income renters face when affordability comes at the cost of neighborhood quality.
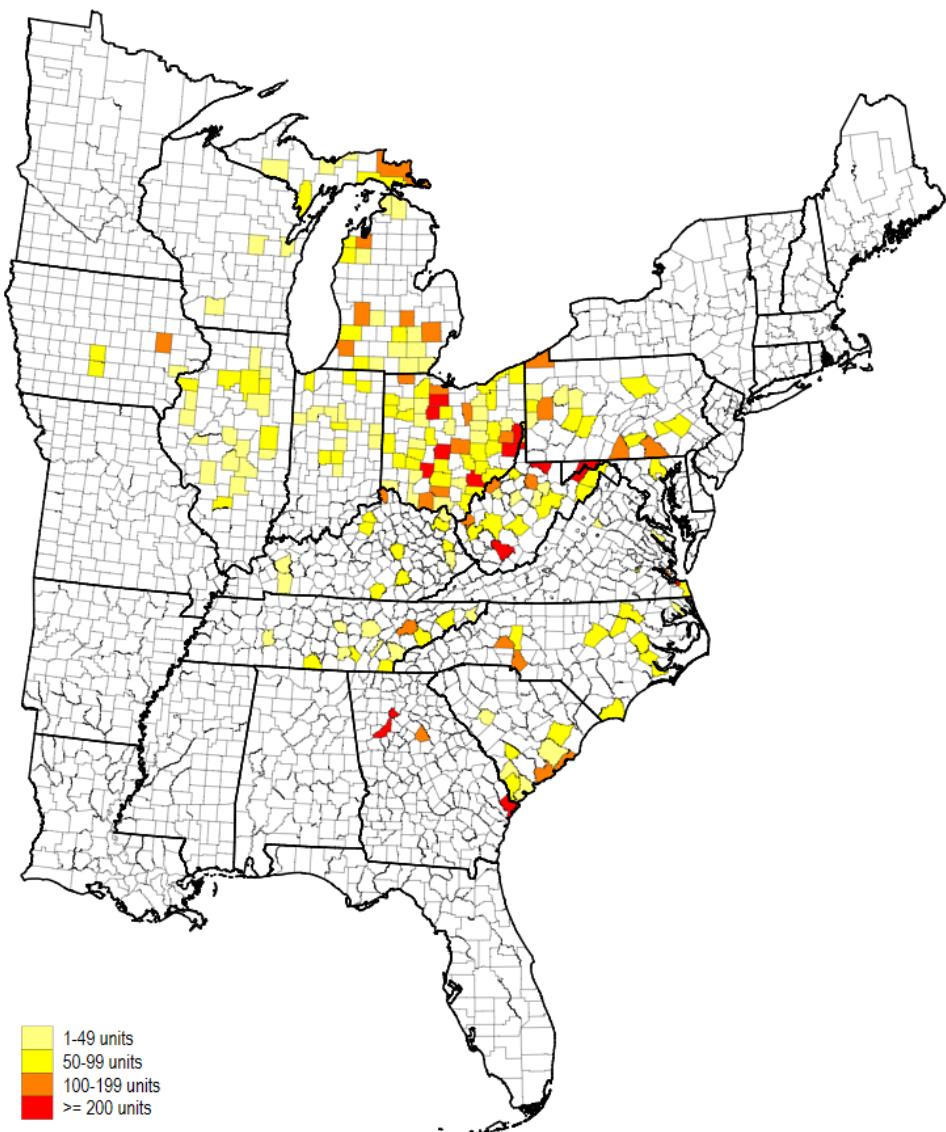
Finally, our results paint a complicated picture of the impacts of LIHTC subsidies on overall tenant welfare. On the one hand, the LIHTC appears to provide access to units that are more affordable and higher quality compared to market-rate units that would be available to low-income renters. As a result, LIHTC-subsidized renters can likely achieve a "bundle" of consumption that would have been otherwise unattainable at lower levels of labor supply. On the other hand, to the extent that moving to lower-opportunity neighborhoods is associated with disamenities – for example, exposure to crime or weaker labor markets – individuals who move to LIHTC may be worse off in other dimensions, or in the long-term. In future work, we hope to bring these countervailing features of the LIHTC into clearer focus, as well as incorporate other elements of tenant well-being.

Figure 1. Subsidized Units per Year, by HUD Program
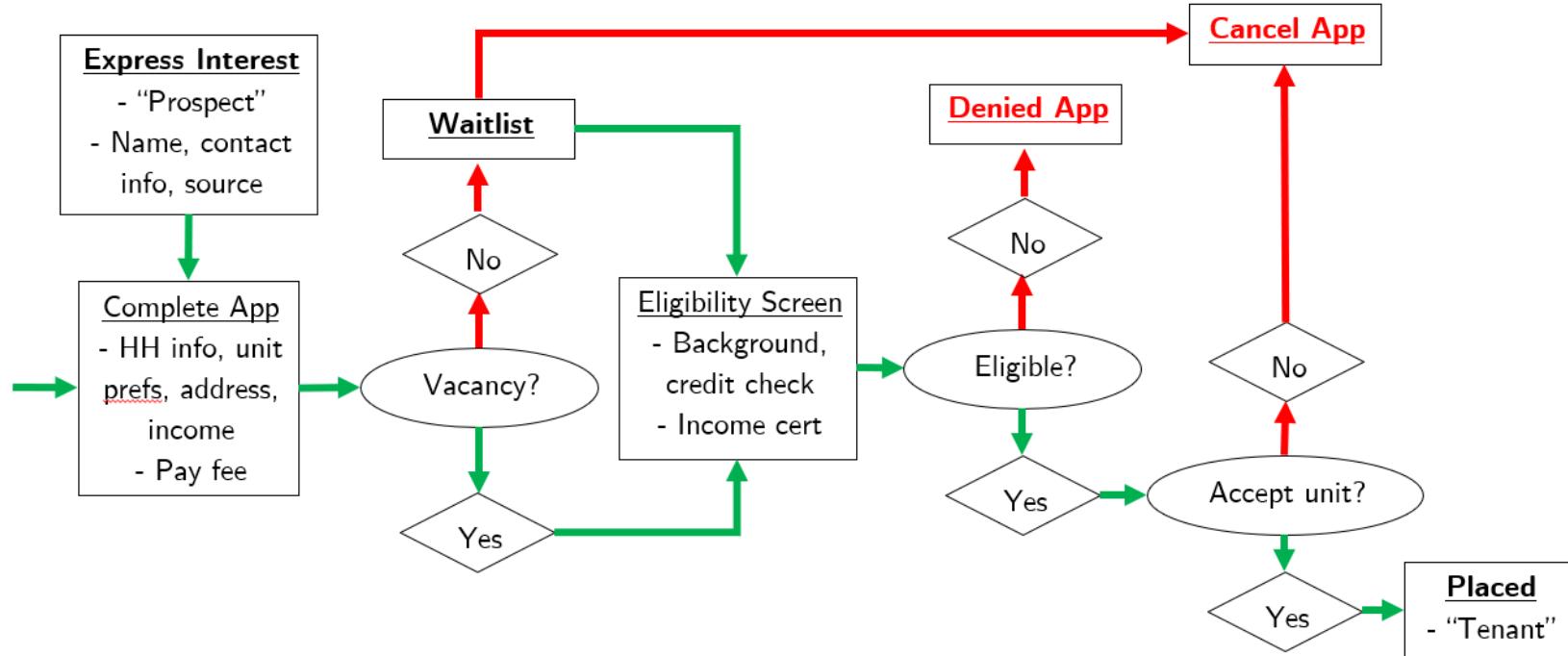
Figure 2. LIHTC-Funded Properties Managed by Developer

Notes: The sample includes all LIHTC-funded properties managed by the developer who provided data for our analysis, regardless of target population or year placed-in-service. Unit counts reflect both LIHTC-subsidized units and market rate units.

Figure 3. Applicant Process Flow Diagram
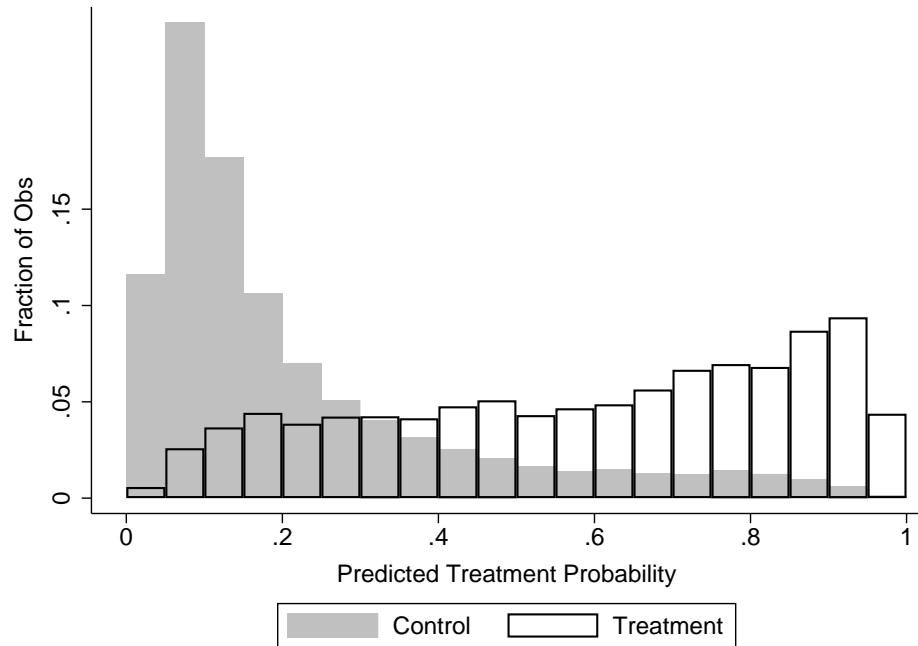
*Notes:* This chart describes the LIHTC application process for our sample. Prospective applicants ("prospects") initiate the process by contacting the property manager directly or through an online listing platforms (e.g., Zillow, Apartments.com). At this stage, prospects submit basic contact information and wait for property staff to follow up with details on how to apply.[40] The application collects additional information including family size, unit preferences (e.g., # bedrooms, special needs), current address, and self-reported income. Once the application is submitted and the application fee is paid,[41] the application is forwarded for processing.

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[40]Alternatively, an interested individual can simply submit a full application without first expressing interest.
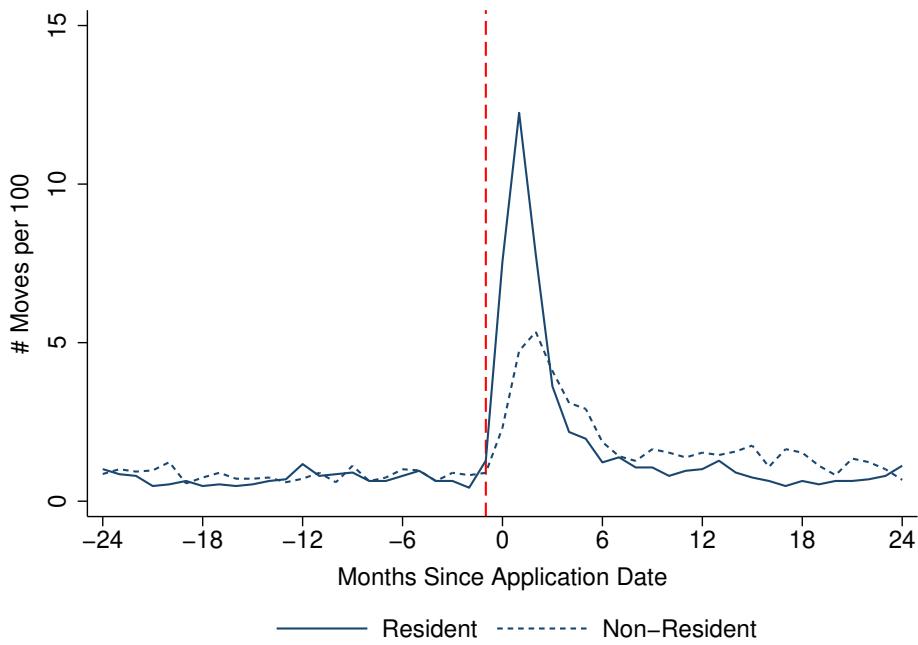
[41]Application fees at these properties are typically low (less than $25). They are used to cover administrative costs as well as costs associated with background checks used for eligiblity screening.

Figure 4. Tax Data Sample:
Predicted Treatment Distribution by Group
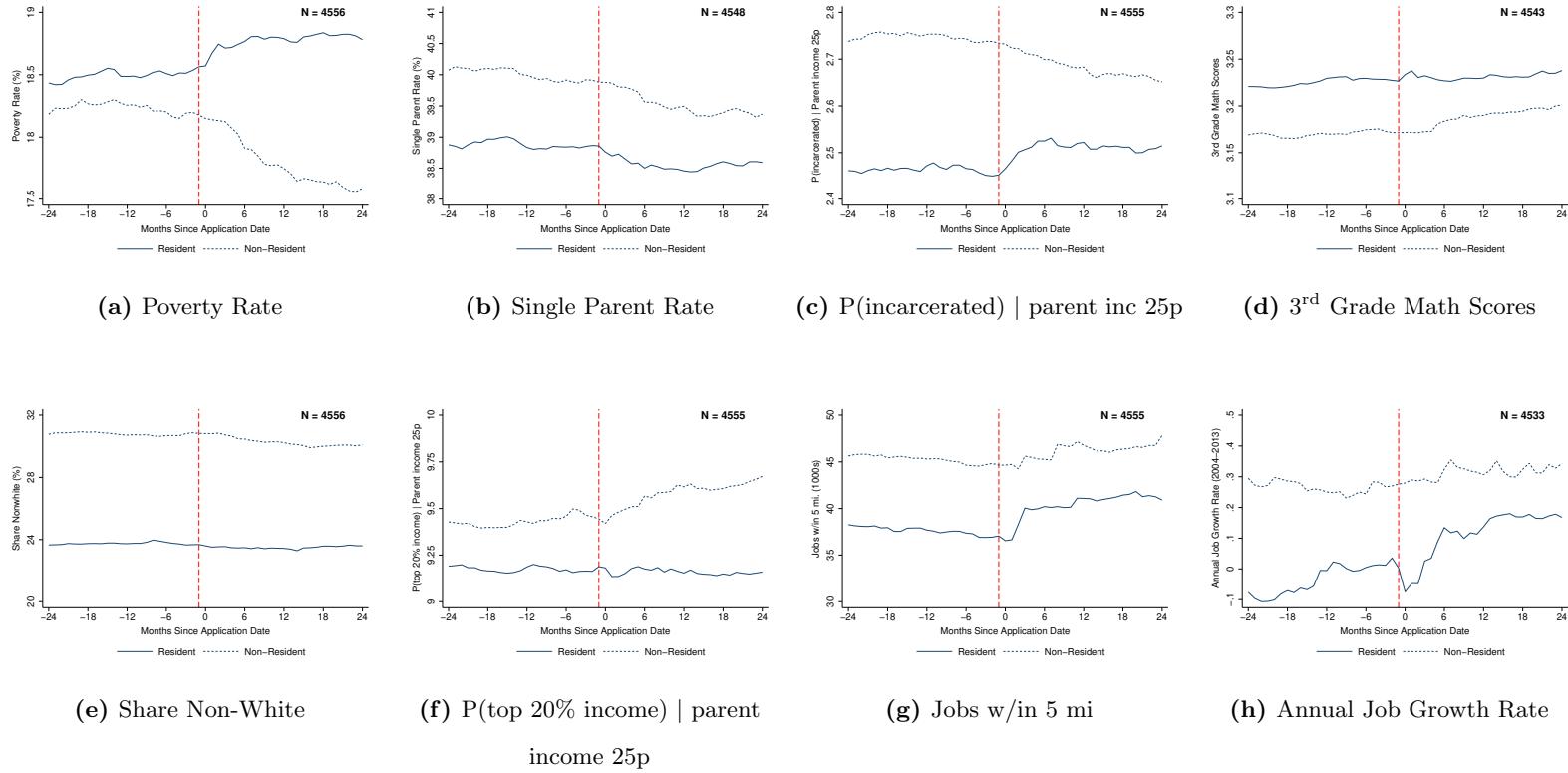
Notes: The sample contains applicants in the main analysis sample as described in Section 6.1. The "Control" group includes applicants with the following statuses: Applied, Approved, Canceled, and Denied. The "Treatment" group refers to applicants who become LIHTC residents. We predict treatment status following Equation 2.

Figure 5. Mobility Sample:
Move Activity in Infutor
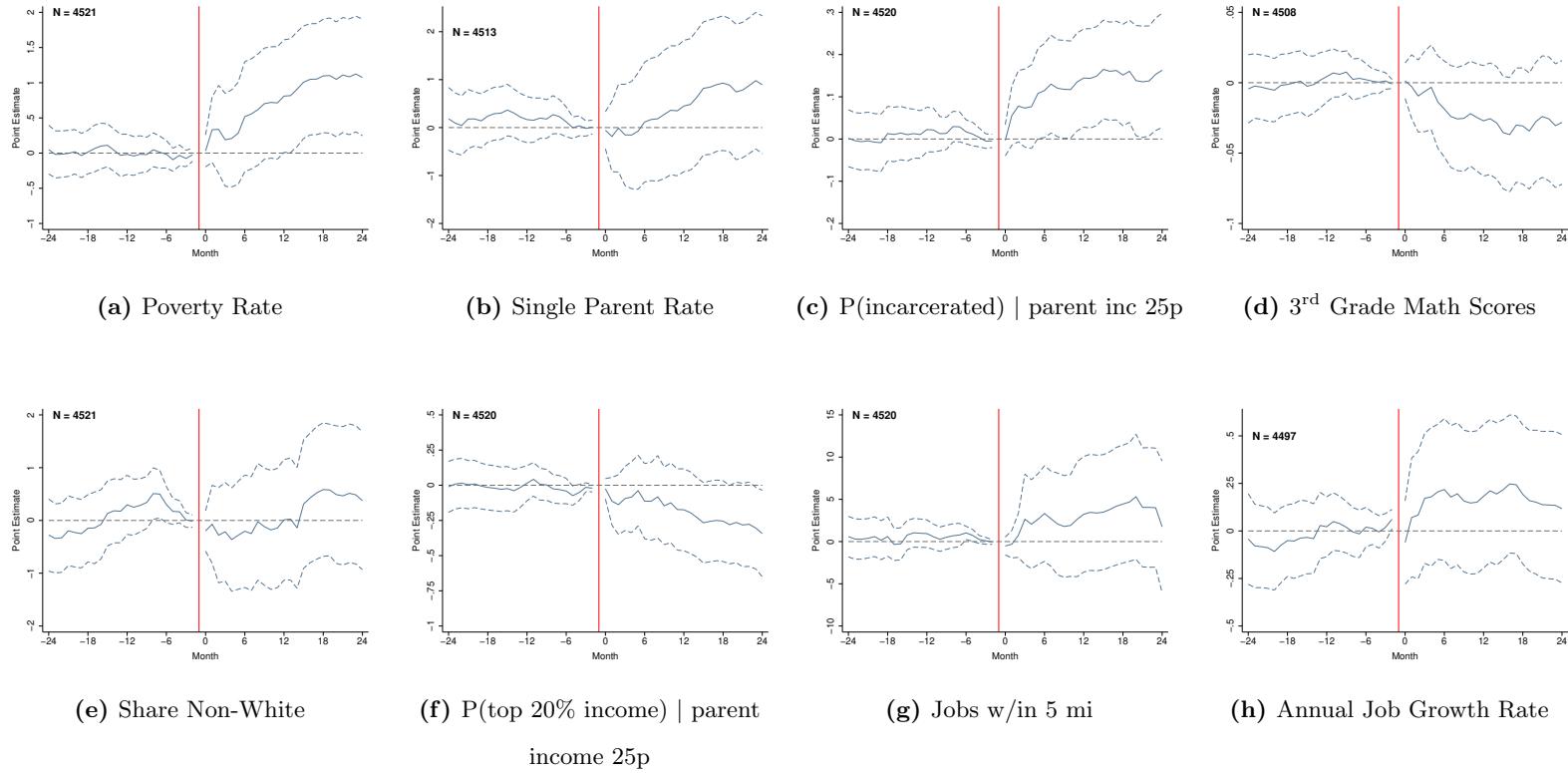
Figure 6. Neighborhood-Level Outcomes:
Unconditional Means by Event Time and Treatment Group
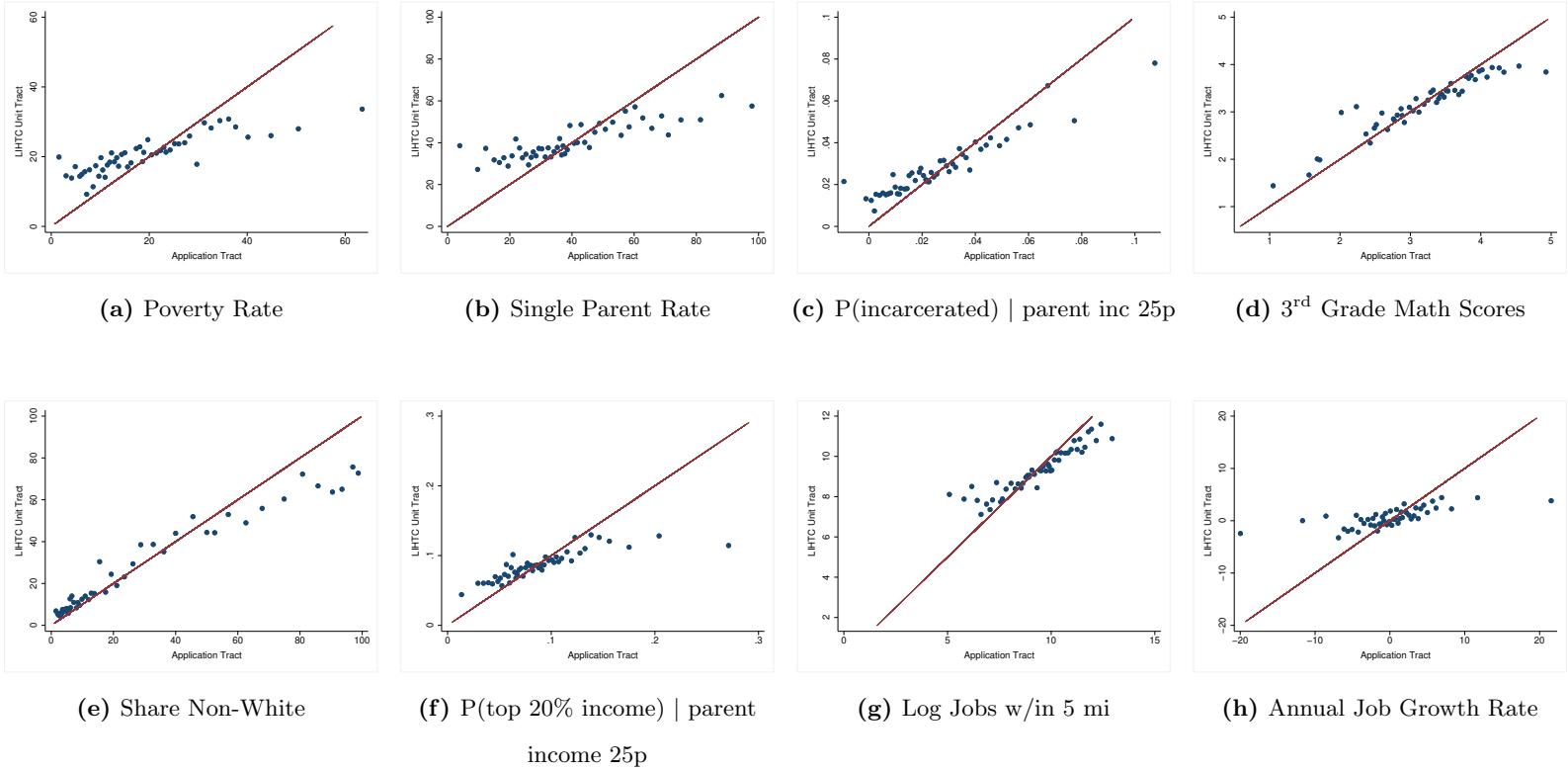
(a) Poverty Rate

(b) Single Parent Rate

(c) P(incarcerated) | parent inc 25p

(d) 3rd Grade Math Scores

(e) Share Non-White

(f) P(top 20% income) | parent
income 25p

(g) Jobs w/in 5 mi

(h) Annual Job Growth Rate

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. For each specified outcome, we further restrict to a balanced panel of applicants with non-missing outcome data 2 years pre/post application.

Figure 7. Neighborhood-Level Outcomes:
Event Study Results, Continuous
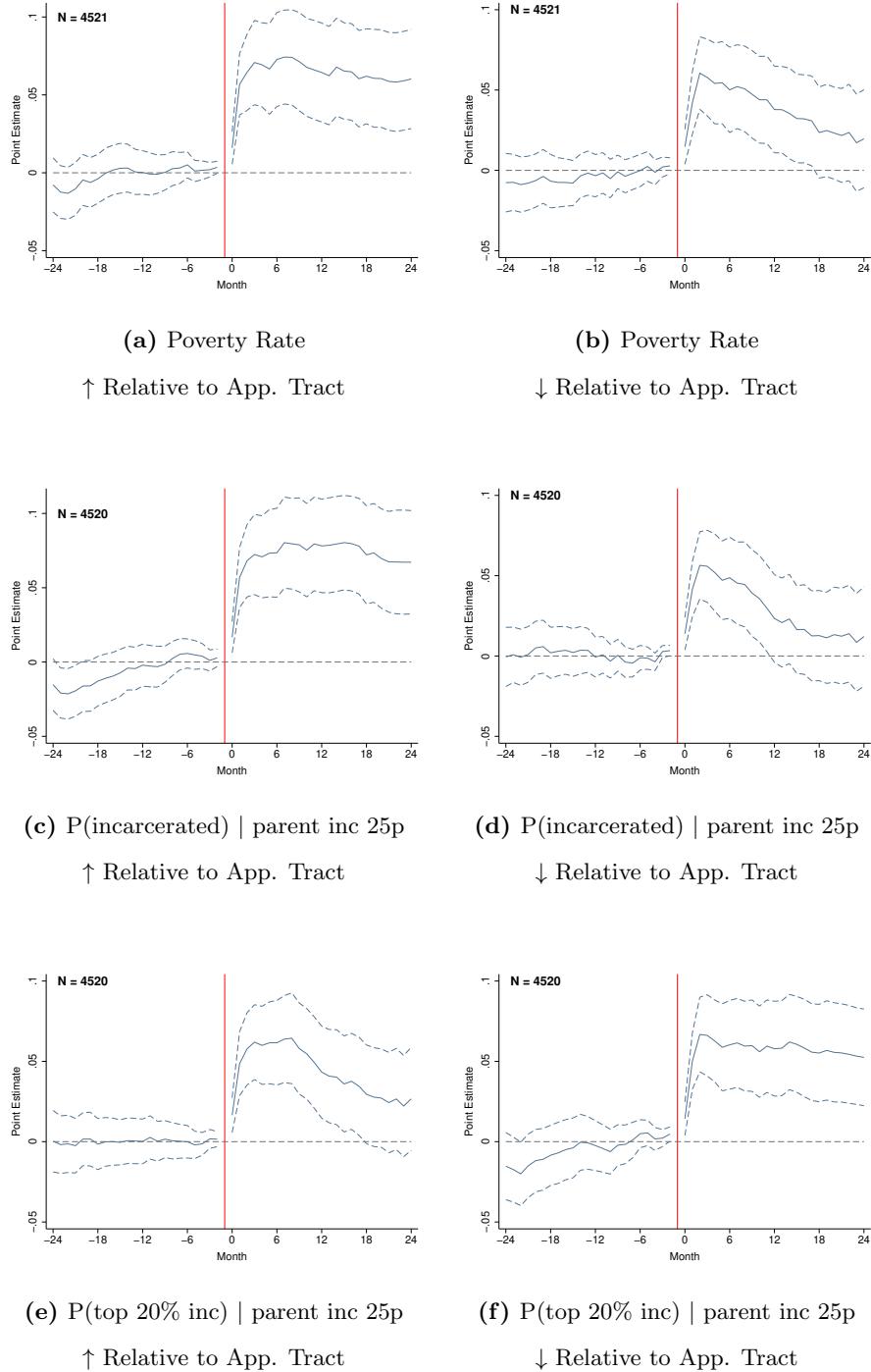
(a) Poverty Rate

(b) Single Parent Rate

(c) P(incarcerated) | parent inc 25p

(d) 3$^{\text{rd}}$ Grade Math Scores

(e) Share Non-White

(f) P(top 20% income) | parent income 25p

(g) Jobs w/in 5 mi

(h) Annual Job Growth Rate

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. For each specified outcome, we further restrict to a balanced panel of applicants with non-missing outcome data 2 years pre/post application. Each figure plots point estimates and 95% confidence intervals from separate regressions of the outcome on event month treatment indicators and the baseline set of controls, following the continuous specification of Equation 1. We cluster standard errors at the applicant level.

Figure 8. Neighborhood-Level Outcomes:
Characterizing Individual Moves to LIHTC
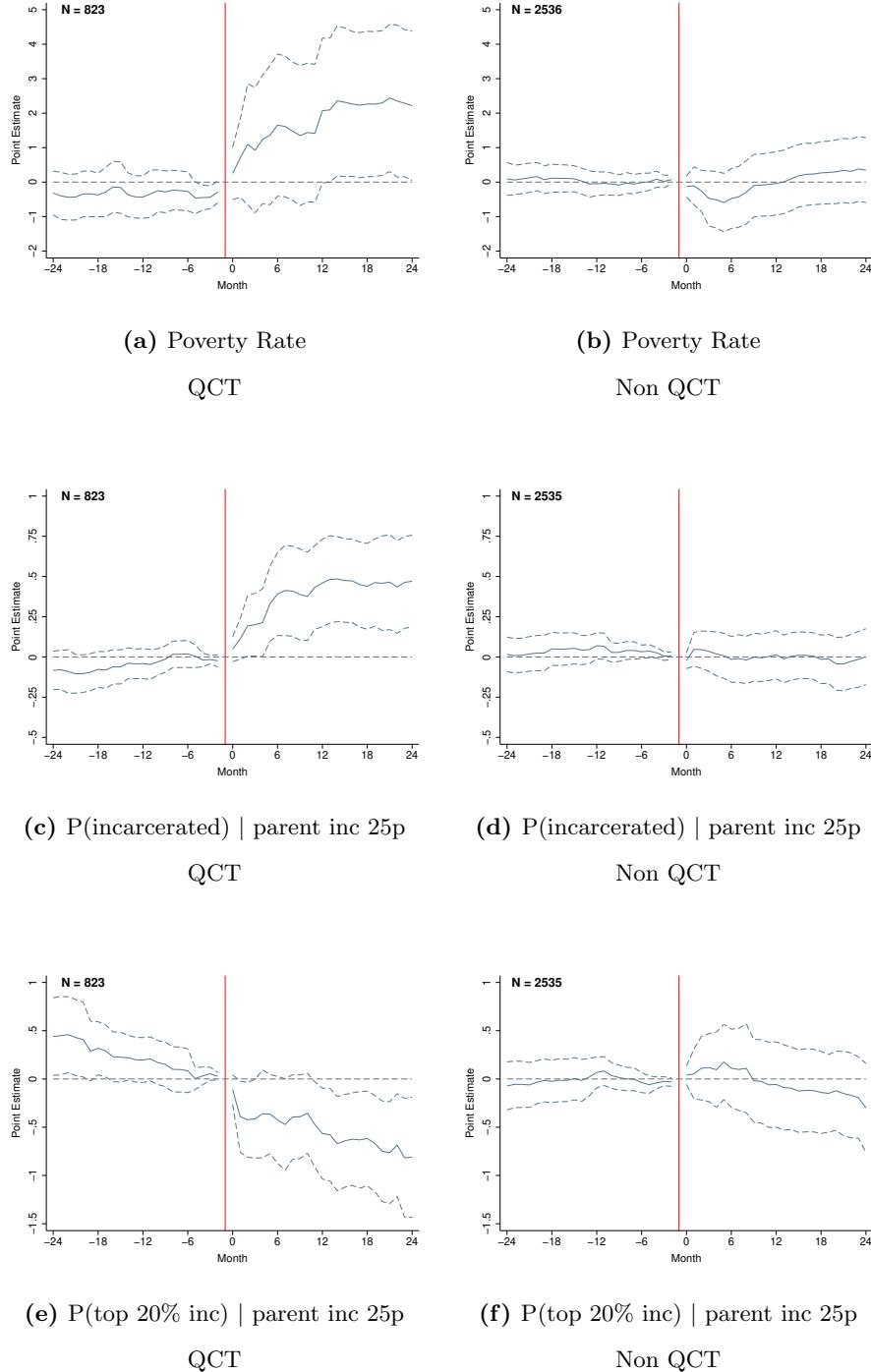
(a) Poverty Rate

(b) Single Parent Rate

(c) P(incarcerated) | parent inc 25p

(d) 3$^{\text{rd}}$ Grade Math Scores

(e) Share Non-White

(f) P(top 20% income) | parent

income 25p

(g) Log Jobs w/in 5 mi

(h) Annual Job Growth Rate

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. We use Stata's *binscatter* program to collapse into bins that each contain 50 applicants.

Figure 9. Neighborhood-Level Outcomes:
Event Study Results, Binarized
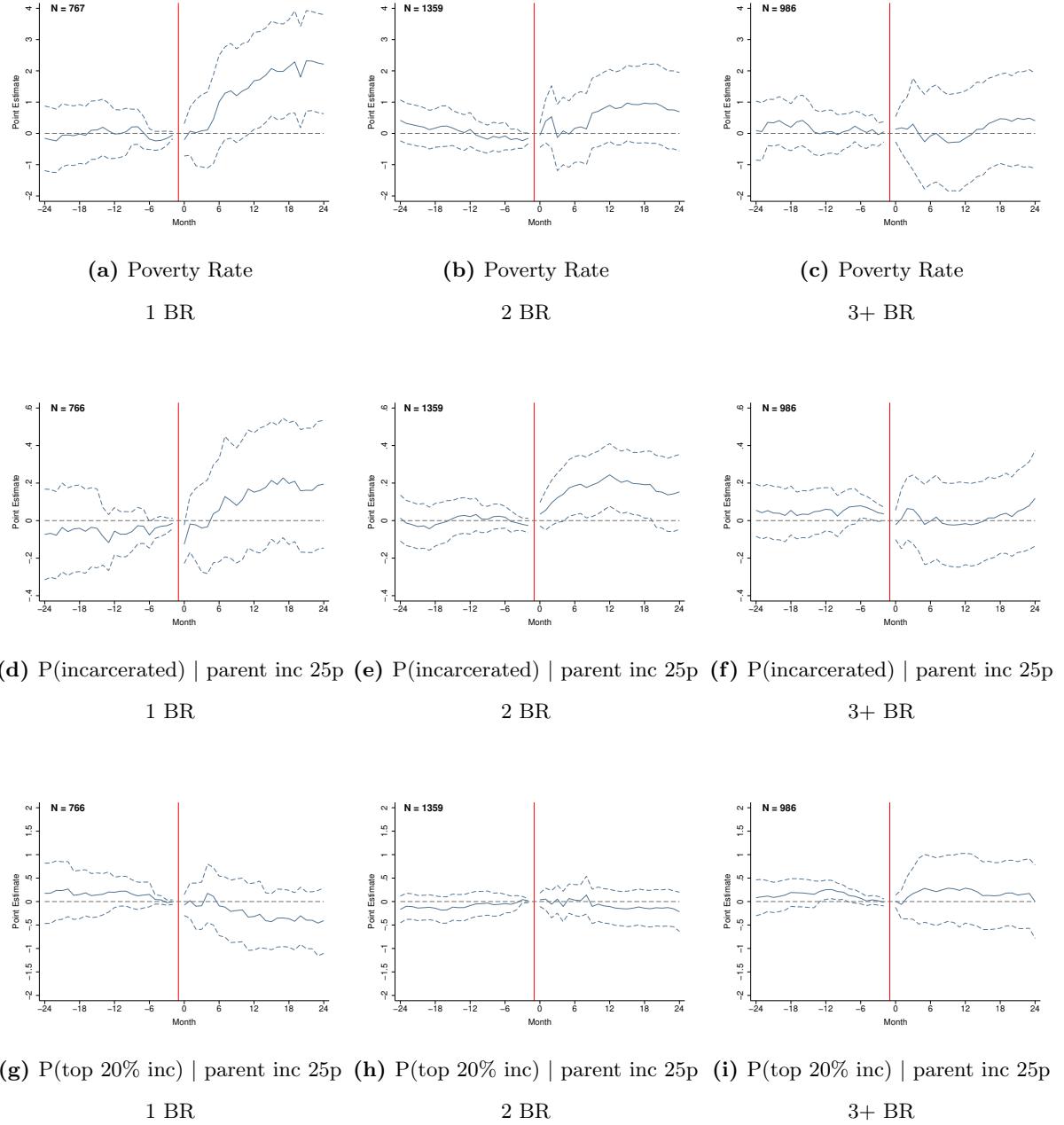
(a) Poverty Rate

↑ Relative to App. Tract

(b) Poverty Rate

↓ Relative to App. Tract

(c) P(incarcerated) | parent inc 25p

↑ Relative to App. Tract

(d) P(incarcerated) | parent inc 25p

↓ Relative to App. Tract

(e) P(top 20% inc) | parent inc 25p

↑ Relative to App. Tract

(f) P(top 20% inc) | parent inc 25p

↓ Relative to App. Tract

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. For each specified outcome, we further restrict to a balanced panel of applicants with non-missing outcome data 2 years pre/post application. Each figure plots point estimates and 95% confidence intervals from separate regressions of the outcome on event month treatment indicators and the baseline set of controls, following the binarized specification of Equation 1. We cluster standard errors at the applicant level.

42

Figure 10. Neighborhood-Level Outcomes:
Event Study Results, Heterogeneity by QCT Status
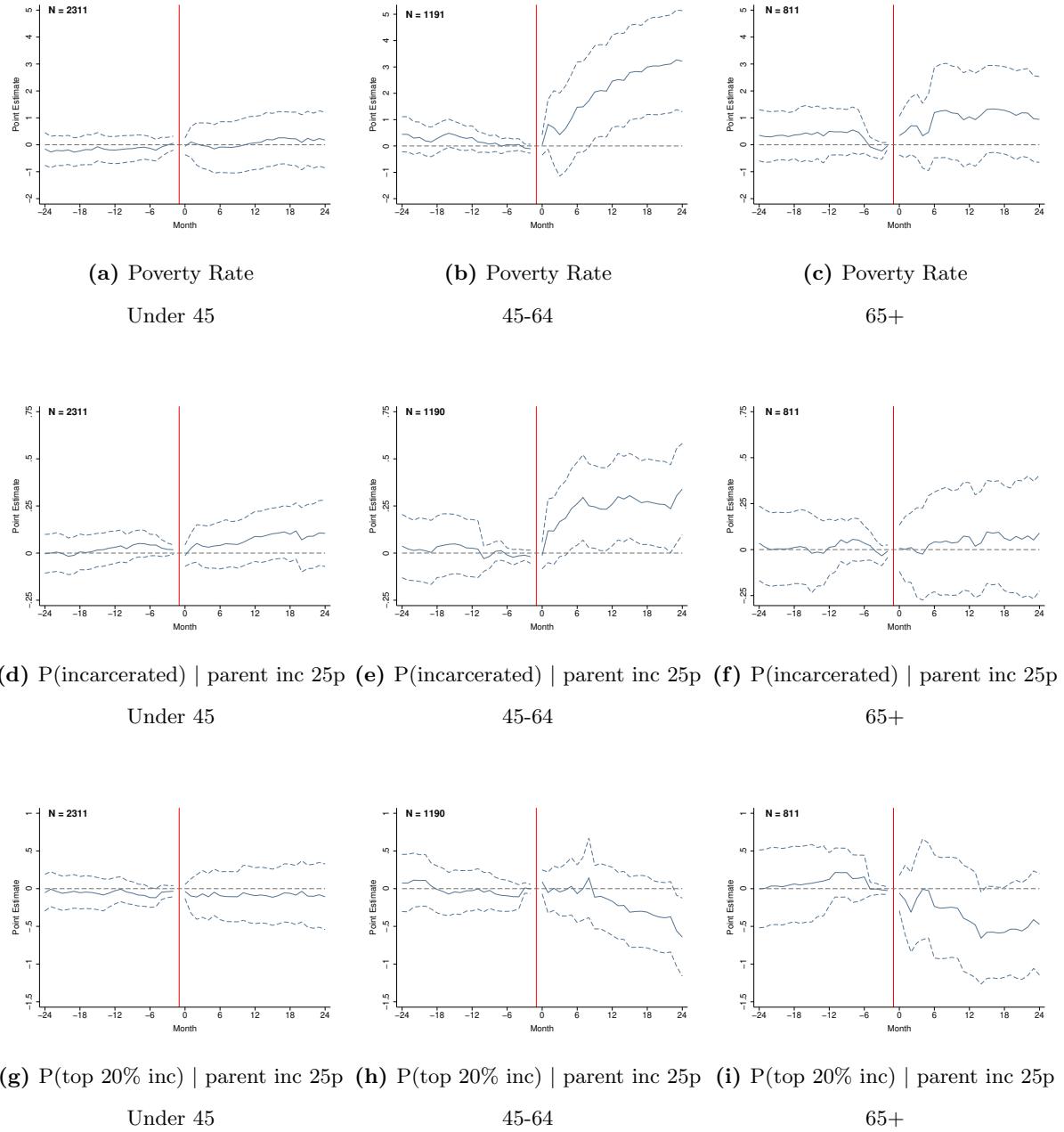
(a) Poverty Rate

QCT

(b) Poverty Rate

Non QCT

(c) P(incarcerated) | parent inc 25p

QCT

(d) P(incarcerated) | parent inc 25p

Non QCT

(e) P(top 20% inc) | parent inc 25p

QCT

(f) P(top 20% inc) | parent inc 25p

Non QCT

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. For each specified outcome, we further restrict to a balanced panel of applicants with non-missing outcome data 2 years pre/post application. Each figure plots point estimates and 95% confidence intervals from separate regressions of the outcome on event month treatment indicators and the baseline set of controls, following the continuous specification of Equation 1. We cluster standard errors at the applicant level.

Figure 11. Neighborhood-Level Outcomes:
Event Study Results, Heterogeneity by Unit Preference
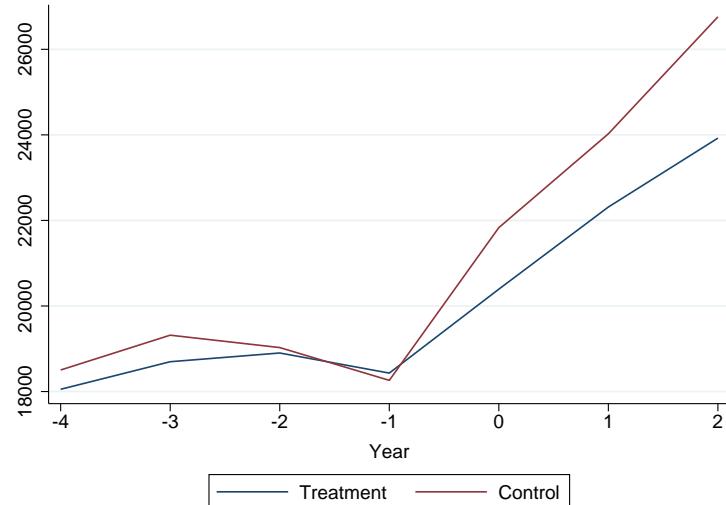
(a) Poverty Rate

1 BR

(b) Poverty Rate

2 BR

(c) Poverty Rate

3+ BR

(d) P(incarcerated) | parent inc 25p

1 BR

(e) P(incarcerated) | parent inc 25p

2 BR

(f) P(incarcerated) | parent inc 25p

3+ BR

(g) P(top 20% inc) | parent inc 25p

1 BR

(h) P(top 20% inc) | parent inc 25p

2 BR

**(i)** P(top 20% inc) | parent inc 25p

3+ BR

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. For each specified outcome, we further restrict to a balanced panel of applicants with non-missing outcome data 2 years pre/post application. Each figure plots point estimates and 95% confidence intervals from separate regressions of the outcome on event month treatment indicators and the baseline set of controls, following the continuous specification of Equation 1. We cluster standard errors at the applicant level.

Figure 12. Neighborhood-Level Outcomes:
Event Study Results, Heterogeneity by Age Group
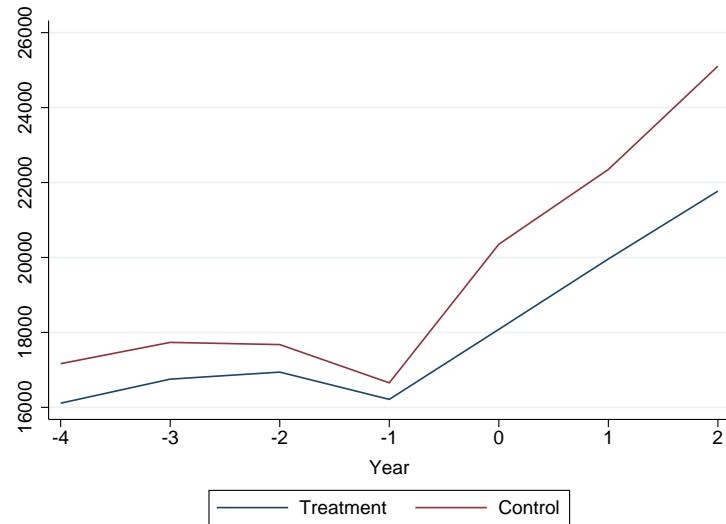
(a) Poverty Rate

Under 45

(b) Poverty Rate

45-64

(c) Poverty Rate

65+

(d) P(incarcerated) | parent inc 25p

Under 45

(e) P(incarcerated) | parent inc 25p

45-64

(f) P(incarcerated) | parent inc 25p

65+

(g) P(top 20% inc) | parent inc 25p

Under 45

(h) P(top 20% inc) | parent inc 25p

45-64

**(i)** P(top 20% inc) | parent inc 25p

65+

Notes: The sample contains applicants in the main analysis sample as described in Section 5.1. For each specified outcome, we further restrict to a balanced panel of applicants with non-missing outcome data 2 years pre/post application. Each figure plots point estimates and 95% confidence intervals from separate regressions of the outcome on event month treatment indicators and the baseline set of controls, following the continuous specification of Equation 1. We cluster standard errors at the applicant level.

Figure 13. Labor Market Outcomes:
Unconditional Means by Event Time and Treatment Group
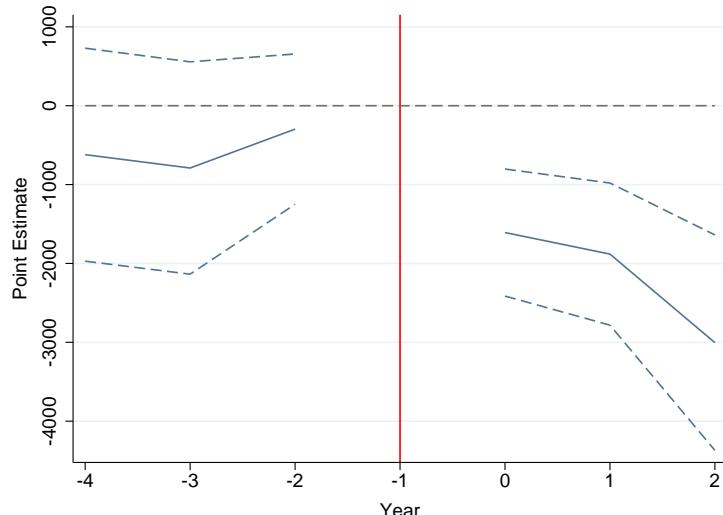
(a) 1040 HUD Income
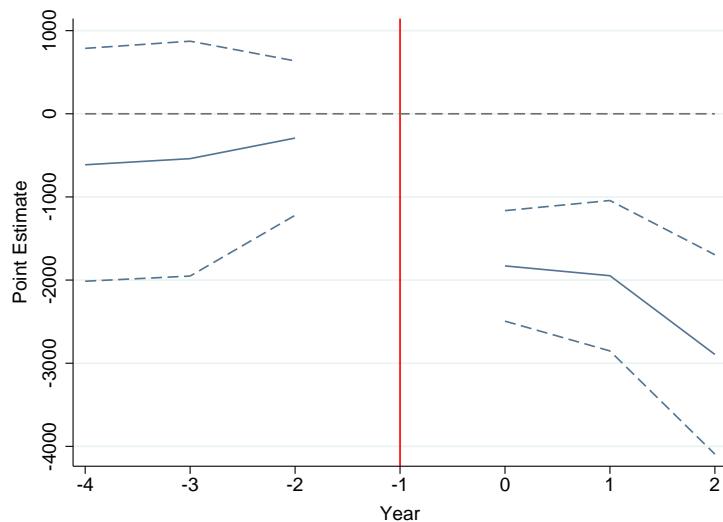
(b) W-2 Wages

Notes: The sample includes all applicants in the main analysis sample as described in Section 6.1. The "Control" group includes applicants with the following statuses: Applied, Approved, Canceled, and Denied. The "Treatment" group refers to applicants who become LIHTC residents. Income and wages are measured in 2019 USD.

**Figure 14.** Labor Market Outcomes:
Event Study Results, Continuous

(a) 1040 HUD Income

(b) W-2 Wages

Notes: The sample includes all applicants in the main analysis sample as described in Section 6.1. Event study estimation follows Equation 3. Income and wages are measured in 2019 USD.

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Table 1. Summary Statistics: LIHTC-Funded Properties

	Data Provider	Other LIHTC
Property Characteristics		
# Units	45.4	71.2
# Low Income Units	42.5	64.7
Vacancy Rate - Low Income Units	0.042	.
Applicants per Units	2.67	.
Multiple Buildings	0.49	.
HOME funding	0.40	.
Rural Development funding	0.31	.
Local funding	0.032	.
2010 Census Tract Characteristics		
Pop per sq mi	1094.0	7080.8
% Black	0.11	0.25
% Hispanic	0.038	0.17
% owner occupied units	0.56	0.44
% college plus	0.18	0.21
% in poverty	0.19	0.23
% single parent HHs	0.38	0.46
N	342	48103
HUD Characteristics		
New construction	0.63	0.55
QCT	0.15	0.19
Metro, outside city center	0.89	0.81
Metro, in city center	0.037	0.12
Non-metro	0.059	0.048
Target pop = families	0.35	0.32
N	270	48227

Notes: For HUD characteristics, the sample is restricted to properties that could be fuzzy matched to HUD's LIHTC Placed-in-Service Database.

Table 2. Mobility Sample:
Applicant Summary Statistics

	(1) Non-Residents	(2) Residents	(3) Raw Diff	(4) Adj. Diff
Panel A. Application Characteristics				
BR preference	2.16	2.11	-0.045	0.059**
Quoted rent	590.3	626.4	36.1***	53.8***
Age at application	45.1	50.2	5.12***	1.55***
Applied from same zip	0.29	0.47	0.18***	0.14***
Applied from same tract	0.093	0.29	0.20***	0.18***
Panel B. Application Tract Characteristics				
Poverty rate	0.18	0.19	0.0039	0.014***
Annual job growth rate	0.0027	0.000	-0.0029	-0.0015
Pop per sq mi	2198.5	1986.8	-211.7	31.9
% Non-White	0.31	0.24	-0.071***	-0.0042
P(incarcerated) | parent inc 25p	0.027	0.025	-0.0028***	0.0005
P(top 20% income) | parent inc 25p	0.094	0.092	-0.0026*	-0.0045***
Panel C. Property Characteristics				
# Low Income Units	49.6	49.5	-0.084	.
Year placed in service	2011.9	2011.5	-0.40**	.
Target pop. = families	0.63	0.54	-0.094***	.
QCT	0.25	0.25	0.000	.
Income Limit = 50% AMGI	0.10	0.099	-0.0054	.
Income Limit = 60% AMGI	0.88	0.87	-0.0059	.
Metro: central city	0.045	0.050	0.0055	.
Non metro	0.034	0.050	0.016**	.
HOME funding	0.44	0.48	0.031*	.
N	2677	1878	4555	4524

Notes: The sample includes all applicants in the main analysis sample as described in Section 5.1. In the final column, we estimate regression adjusted differences with property fixed effects and cluster standard errors at the applicant level.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

Table 3. Tax Data Sample:
Applicant Summary Statistics

	Non-Residents	Residents	Raw Diff	Adj. Diff
Bedroom Preference	1.675	1.572	-0.104*	0.306*
Age	38.053	34.097	-3.955***	-1.550***
Form 1040 Filer	0.945	0.953	0.008***	0.003
Married Filing Jointly	0.222	0.184	-0.038***	0.001
Number of Dependents	1.131	0.987	-0.144***	-0.172***
N	2117	4129	6246	6246

Notes: The sample includes all applicants in the main analysis sample as defined in Section 6.1. In the final column, we estimate regression adjusted differences with property fixed effects and cluster standard errors at the applicant level.
* – significant at 10%, ** – significant at 5%, *** – significant at 1%.

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