Dynamic effects of LIHTC developments on neighborhood gentrification in Texas

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Introduction

Lack of affordable and stable housing is a growing policy concern among cities across the world. In the United States, housing policy is determined at the state and federal levels. According to the Center on Budget and Policy Priorities, public housing has declined in the US and priorities have shifted towards market-based measures to stimulate housing supply. One such policy is the Low-Income Housing Tax Credit (LIHTC), which was developed in 1986. This supply-side policy reduces the tax liability of private developers and investors in exchange for providing financing to develop housing that is specifically targeted to low-income individuals. Ideally, appropriately targeted LIHTC policy should bring in more lower-income residents in neighborhoods and should reduce housing costs over time. However, past research has suggested that LIHTC developments bring negative spillover effects in terms of raising housing costs and crowding out alternative housing supply (Voith et al., 2022; Baum-Snow & Marion, 2009). These unintended consequences are closely related to processes of gentrification, whereby neighborhood changes lead to the displacement of lower-income residents by higher-income populations.

To better understand whether LIHTC developments contribute to gentrification, this paper estimates dynamic effects of neighborhood change, while considering differential treatment timing. I use LIHTC property data from the U.S. Department of Housing and Urban Development (HUD), the Longitudinal Employer-Household Dynamics (LEHD) Origin-Destination Employment Statistics (LODES) from the U.S. Census Bureau, and house price index data from the Federal Housing Finance Agency (FHFA). I use the difference-in-differences (DID) method proposed by Callaway and Sant'Anna (2021) to estimate the long-run effects of LIHTC developments on several key gentrification indicators. I show that LIHTC developments eventually increase the share of low-income and non-white individuals, and lead to a decline in housing prices.

Background and Literature Review

During the Great Depression, sweeping reforms enacted by the New Deal reforms saw the creation of the Public Works Administration (PWA). Under this, several public housing programs were created for the working class and were expected to be supported by rent revenue. However, after World War 2, the American middle class largely moved to suburban living, brought about by a shift towards more market-based approaches to affordable housing and the widespread availability of automobiles. This led to a lot of deterioration of public housing constructed in cities and eventually units were demolished. The need for reforms in affordable housing was apparent at the time. Eventually, the American shift towards neoliberalism led to the Tax Reform Act of 1986 (Freeman & Lei, n.d.).

The Low-Income Housing Tax Credit (LIHTC) was developed in the Tax Reform Act of 1986 and is to date the largest federal program for affordable housing in the United States. Since its inception, it has subsidized over 3 million units in the country (Stamm, 2024). Under LIHTC, developers can get either the 9 % credit or the 4% credit. The 9% credit corresponds to a subsidy of up to 70% and is targeted to new constructions without any other source of federal funding, while the 4% credit corresponds to a subsidy of up to 30% and is aimed at developments that are already financed through federally-exempt bonds (Keightley, 2014). In order to qualify for the LIHTC, developers must ensure that at least 40% of units are occupied by tenants with less than 60% of Area Median Income (AMI), and that no unit is occupied by anyone with more than 80% of AMI.

Prior research of the effects of LIHTC units on gentrification have yielded different results, due to the fact that many variables can be used to measure gentrification. LIHTC developments have been found to crowd out new private construction especially in gentrifying and in less populous areas (Baum-Snow & Marion, 2009; Sinai & Waldfogel, 2005). Additionally, Voith et al. (2022) use an interrupted time series (ITS) to compare pre-development and post-development real estate prices and a DID to identify price changes through time and spatial variation. This allows them to study changes in house prices for neighborhoods near ones that have a LIHTC development. Subsequently, they find that LIHTC developments increase house prices for nearby neighborhoods, and particularly in low-income areas. Similarly, An et al. (2023) also finds that LIHTC developments have positive spillover house price effects for nearby tracts in the city of Los Angeles. Although, my strategy does not track housing prices for nearby neighborhoods, I attempt to devise how LIHTC developments affect house prices for the particular census tract where it is present. Similar to these studies, I also decipher dynamic effects by applying a DID method but I instead also consider that different census tracts could receive LIHTC at different times.

The compositional change of low-income and minority individuals is an other important measurement of gentrification. Park et al. (2021) find that LIHTC increases the share of low-income households while Ellen et al. (2016) show that LIHTC investments do not have any affect on the concentration of low-income individuals. Horn and O'Regan (2011) use neighborhood compostion data to show that LIHTC units reduce segregation in Metropolitan Statistical Areas (MSAs) that have more construction of LIHTC units. My results are consistent with Park et al. (2021) and Horn and O'Regan (2011) as I show that LIHTC developments increase the share of low-income and minority individuals.

Cavalleri et al. (2021) apply a gravity model to understand inter-regional migration in 14 OECD countries and conclude that in the United States, inter-regional mobility is particularly responsive to local labor market conditions, whereas mobility negatively responds to higher housing prices. The baseline gravity model that they approximate is:

$$
\begin{aligned}
MIG_{ij,t} = \beta_0 + \beta_1 DIS_{ij} + \beta_2 POP_{i,t-1} + \beta_3 POP_{j,t-1} + \sum_{s=1}^{3}(\gamma_s X^s_{i,t-1} + \delta_s X^s_{j,t-1}) \\
+ \sum_{s=0}^{n}(\theta_s Z^s_{i,t-1} + \tau_s Z^s_{j,t-1}) + \alpha_i + \alpha_j + \alpha_t + \epsilon_{ij,t} \qquad \forall i \neq j
\end{aligned}
$$

Here, the authors specifically model the movement of individuals from origin (i) to destination (j). For United States, the authors use variables like employment share of manufacturing and labor force with elementary education as some of the Z variables in the model. Although I do not explicity model migration flows, I study dynamic effects on variables such as the share of employment among low-income individuals, which I think should proxy for inter-regional migration of individuals. Additionally, Park et al. (2021) also suggest that the gravity model is what drives their results. I hypothesize then that local labor market conditions are what ultimately drive the effects of LIHTC developments on gentrification.

Data

The data used to answer this question comes from multiple sources. The LIHTC property data from the US Department of Housing and Urban Development (HUD) has information on all LIHTC properties in the United States from 1991 to 2022. The dataset was filtered for Texas properties. LIHTC data includes a 11-digit Geocode that uniquely identifies a census tract. For the LODES data, the focus is specifically on the Residence Area Characteristics (RAC) data, which tracks number of jobs held by residents in a census block. The data includes jobs held by individuals of different races, income levels, age, gender, and even number of jobs by industry. Since the LODES-RAC focuses on a census block (15 digit code) and the LIHTC data focuses on a census tract (11 digit code), the data for LODES-RAC was aggregated to the census tract level. This was done in order to merge the LIHTC data with the LODES-RAC data on year and corresponding census tracts. The original dataset spans the years 2002 to 2022. However, a substantial portion of the early data, in particular, job counts separated by race and education, contained zeros that likely reflected missing values rather than true zeros. To avoid potential bias, these entries were excluded, resulting in a final dataset that covers the years 2009 to 2022. An important limitation of the LODES dataset is that it counts jobs held by individuals and hence does not include the unemployed or vulnerable population such as the elderly, disabled, and students. The LIHTC dataset contains information on the number of units that are targeted to low-income individuals and the total number of units. However, amongst tracts that get LIHTC, it seems that about 95% of them have at least 74% low-income units. So, treated tracts have a substantial concentration of LIHTC units, and hence I ignore the concentration of low-income units.

The data for house price indices comes from the Federal Housing Finance Agency (FHFA). An attempt to merge the LIHTC-LODES merged dataset with the house price index data resulted in a substantial number of unmatched observations, reflecting tracts present in the LIHTC-LODES data but missing from the house price index data. This led to a lot of imbalance in the data and led to many observations being dropped, which will affect results substantially. I analyze house prices separately and do not consider it it in the set of covariates.

The key variables for the analysis involve ratios of jobs held by individuals living in a specific census tract. Ratios are computed from the total job counts in order to analyze how the share of individuals in a census tract is changing over time. The analysis focuses on several key variables: the shares of low- and high-income individuals, the shares of white and non-white individuals, the shares of high-skill and low-skill jobs, and house prices in order to get an expansive measure of gentrification. Additionally, I include a set of covariates to control for potential pre-treatment differences across census tracts. These covariates include the shares of individuals by age group, education level, Hispanic ethnicity, as well as indicators for whether a tract is classified as urban, a Qualified Census Tract (QCT), or a Difficult Development Area (DDA). Additionally, I also included variables that I used for estimation. For example, while looking at the effect on the share of low-income individuals, I included the share of high-skill jobs as a covariate.

QCTs and DDAs are particularly important criteria as they determine a large part of where LIHTC developments happen. A QCT is defined as a census tract that has at least 50 percent of housholds at or below 60% of area median gross income or where the poverty rate is 25% or more, whereas a DDA is an area with higher land, construction ad utility costs relative to area median income.

Treatment tracts are those that are in both the LIHTC and LODES datasets. Control tracts are those that are only in the LODES dataset. For simplicity, rows that are only in the LIHTC data but not the LODES are ignored as the change in the share of individuals over time cannot be analyzed. For treated tracts, the first year of match was considered as the year of treatment. This means that for census tracts that are matched at multiple years, treatment happens at the first year where match happens. A simplifying assumption made here is that once a census tract gets a LIHTC development, it continues to be treated till 2022 (the final year in the data). This is not necessarily true, as Texas specifically has faced a lot of issues in maintaining the status of LIHTC properties, but it is nonetheless an important assumption for the empirical strategy.

Table 1 below shows the distribution of treatment years in the dataset. As mentioned previously and as seen below, different tracts receive treatment at different times. This suggests that a difference-in-differences (DID) framework is appropriate for the empirical strategy of this paper, as the objective is to estimate how the share of individuals in a census tract evolves over time following the entry of LIHTC developments, relative to census tracts that do not experience any LIHTC activity. However, due to the differential timing of treatment, a simple DID framework will not suffice and instead the focus should be on an extension that models dynamic effects for staggered adoption of treatment.

Table 1: Distribution of Treatment Years

Treatment Year	Frequency	Percent	Cumulative
2009	168	3.39%	3.39%
2010	406	8.19%	11.58%
2011	504	10.17%	21.75%
2012	546	11.02%	32.77%
2013	308	6.21%	38.98%
2014	168	3.39%	42.37%
2015	224	4.52%	46.89%
2016	420	8.47%	55.37%
2017	308	6.21%	61.58%
2018	210	4.24%	65.82%
2019	168	3.39%	69.21%
2020	546	11.02%	80.23%
2021	406	8.19%	88.42%
2022	574	11.58%	100.00%

Methods

This paper develops an event study DID with staggered adoption of treatment in order to consider dynamic heterogeneous treatment effects of LIHTC developments in Texas from 2009-2022. It is important to note that the heterogenity here refers to the different effects based on treatment timing and not the different effects amongst census tracts. While that would be interesting, it would be unfeasible as there are 6,896 distinct census tracts in the dataset.

The use of such a empirical strategy is particularly important as naive comparisons between tracts that get LIHTC developments and tracts that do not, will not yield causal effects of LIHTC developments. This is because tracts that do have any LIHTC units probably systematically differ in other ways that are unrelated to gentrification. Baum-Snow and Marion (2009) suggest that developers might choose to build LIHTC units in tracts that are likely to yield the maximum present discounted value in rent. However, the unconditional parallel trends assumption underlying a standard DID design may also not be plausible in this context, as it is unlikely that census tracts receiving LIHTC developments and those that do not would have exhibited similar trends in gentrification-related outcomes over time in the absence of treatment. To circumvent this issue, I consider an empirical strategy that only assumes parallel trends conditional on a set of covariates. The rest of this section is devoted to explaining the method and its assumptions.

The empirical strategy is adopted from Callaway and Sant'Anna (2021). In order to completely understand this method, we first consider a generalized dynamic two-way fixed effects (TWFE) event study framework presented in Sun and Abraham (2021). In particular, the specification is given by:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{l<-K} \beta_l D^l_{i,t} + \sum_{l=-K,l\neq -1}^{L} \mu_l D^l_{i,t} + \sum_{l>L} \gamma_l D^l_{i,t}$$

Sun and Abraham (2021) argue that the above specification would result in a misleading interpretation for all coefficients. This is because researchers are interested in the average treatment effects for some l periods relative to treatment. The problem is that with heterogeneous treatment effects, the coefficients do not capture dynamic treatment effects, as they are now "contaminated" by the effect of units that were previously treated.

Callaway and Sant'Anna (2021) propose a robust methodology for estimation and inference within DID frameworks that accounts for multiple time periods, variation in treatment timing (staggered adoption), and requires only conditional parallel trends. The data does not have units that are never treated and hence I focus only on comparisons with not-yet treated units. The conditional parallel trends assumption for not-yet treated units as controls is as follows:

For each (s,t) $\in \{2, ...\mathcal{T}\} \times \{2, ...\mathcal{T}\}$, and each $g \in \mathcal{G}$ such that $t \geq g, s \geq t$:

$$E[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = E[Y_t(0) - Y_{t-1}(0)|X, D_s = 1, G_g = 0]$$

This assumption states that, conditional on a set of covariates X and in the absence of treatment, the difference in mean outcomes between $t-1$ and t for units first treated at time g is the same as for the units not yet treated at time g, but will be at some point later.

The specific model(s) that I estimate in this paper are given by:

$$\begin{aligned} Share\_Individuals_{i,t} &= Geocode_i + Year_t + X'_{i,t}\delta \\ &+ \sum_{l<-K} \beta_l LIHTC^l_{i,t} + \sum_{\substack{-K \leq l \leq L \\ l \neq -1}} \mu_l LIHTC^l_{i,t} + \sum_{l>L} \gamma_l LIHTC^l_{i,t} + \varepsilon_{i,t} \end{aligned}$$

The event study estimates that highlight treatment effects with respect to event time $e = t - g$ are given by:

$$\theta_{es}(e) = \sum_{g \in \mathcal{G}} 1\{g + e \leq \mathcal{T}\} P(G = g | G + e \leq \mathcal{T}) ATT(g, g + e)$$

This is essentially a probability weighted average of the event time ATT's, where probabilities represent the relative frequency of observations for that particular event time in the sample. These are the estimates that I will present in the Results section.

The ATT estimated here is through a specific method called Doubly-Robust Estimator. The ATT for comparisons to not-yet-treated group is given by:

$$ATT^{ny}_{dr}(g, t; \delta) = E\left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{p_{g,t+\delta}(X)(1 - D_{t+\delta})(1 - G_g)}{1 - p_{g,t+\delta}(X)}}{E\left[\frac{p_{g,t+\delta}(X)(1 - D_{t+\delta})(1 - G_g)}{1 - p_{g,t+\delta}(X)}\right]}\right)\left(Y_t - Y_{g-\delta-1} - m^{ny}_{g,t,\delta}(X)\right)\right]$$

where, $m^{ny}_{g,t,\delta}(X) = E[Y_t - Y_{g-\delta-1}|X, D_{t+\delta} = 0, G_g = 0]$. The second part of this equation is easier to understand as it just represents a regular DID estimate but now with consideration of the not-yet-treated group as a control and conditional on covariates X. The first part of this equation is essentially just a method to assign weights. For tracts first treated at g, $G_g = 1$, and we get a postive weighted term $\frac{1}{E[G_g]}$, but for $G_g = 0$, we have that units are not-yet-treated at time g, which is our control here. The weights would be the huge negative term that weighs specific observations by $\frac{p(X)}{1-p(X)}$. The idea here is to use the set of covariates X to ensure that the control group of not-yet treated group is probabilistically similar to the treated group. This could lead to issues as I cannot be sure that X is sufficient to meaningfully estimate this probability.

Results

Table 2 below shows the event study estimates for the variables of interest.

Table 2: Event Study Estimates: Impact of LIHTC

	Share of LI Residents	Share of HI Residents	Share of White Individuals	Share of Non-White Individuals	Share of HS Jobs	Share of LS Jobs	House Prices
Pre-Avg	0.0007 (0.0021)	0.0039 (0.0035)	-0.0037 (0.0024)	0.0037 (0.0024)	0.0049** (0.0025)	-0.0049** (0.0025)	0.0387 (0.0352)
Post-Avg	0.0043** (0.0020)	-0.0069* (0.0040)	-0.0049 (0.0043)	0.0049 (0.0043)	-0.0015 (0.0028)	0.0015 (0.0028)	-0.0499 (0.0365)
Tm12	0.0048 (0.0060)	-0.0009 (0.0138)	-0.0096 (0.0088)	0.0096 (0.0088)	-0.0002 (0.0101)	0.0002 (0.0101)	-0.0327 (0.1141)
Tm11	0.0014 (0.0044)	0.0004 (0.0083)	-0.0056 (0.0063)	0.0056 (0.0063)	0.0070 (0.0060)	-0.0070 (0.0060)	0.0042 (0.0902)
Tm10	-0.0002 (0.0045)	0.0069 (0.0071)	0.0007 (0.0054)	-0.0007 (0.0054)	0.0065 (0.0048)	-0.0065 (0.0048)	-0.0752 (0.0614)
Tm9	0.0009 (0.0039)	0.0055 (0.0062)	-0.0064 (0.0047)	0.0064 (0.0047)	0.0063* (0.0038)	-0.0063* (0.0038)	0.0359 (0.0604)
Tm8	0.0039 (0.0031)	-0.0001 (0.0046)	-0.0044 (0.0041)	0.0044 (0.0041)	0.0080** (0.0038)	-0.0080** (0.0038)	-0.0144 (0.0608)
Tm7	-0.0016 (0.0025)	0.0072** (0.0036)	-0.0060** (0.0030)	0.0060** (0.0030)	0.0048 (0.0031)	-0.0048 (0.0031)	0.0517 (0.0535)
Tm6	-0.0017 (0.0023)	0.0069** (0.0032)	-0.0039 (0.0024)	0.0039 (0.0024)	0.0022 (0.0026)	-0.0022 (0.0026)	0.0741 (0.0718)
Tm5	-0.0003 (0.0022)	0.0072** (0.0029)	-0.0019 (0.0025)	0.0019 (0.0025)	0.0063** (0.0025)	-0.0063** (0.0025)	0.1417** (0.0642)
Tm4	0.0017 (0.0023)	0.0028 (0.0027)	-0.0027 (0.0034)	0.0027 (0.0034)	0.0073*** (0.0022)	-0.0073*** (0.0022)	0.0660 (0.0585)
Tm3	-0.0017 (0.0018)	0.0057*** (0.0021)	0.0006 (0.0026)	-0.0006 (0.0026)	0.0037* (0.0021)	-0.0037* (0.0021)	0.1125*** (0.0389)
Tm2	0.0004 (0.0015)	0.0012 (0.0018)	-0.0018 (0.0021)	0.0018 (0.0021)	0.0025 (0.0018)	-0.0025 (0.0018)	0.0622* (0.0348)
Tp0	0.0014 (0.0016)	0.0011 (0.0018)	-0.0004 (0.0014)	0.0004 (0.0014)	-0.0010 (0.0017)	0.0010 (0.0017)	0.0378 (0.0323)
Tp1	0.0012 (0.0020)	0.0015 (0.0023)	0.0006 (0.0021)	-0.0006 (0.0021)	-0.0027 (0.0025)	0.0027 (0.0025)	-0.0172 (0.0380)
Tp2	0.0022 (0.0020)	-0.0019 (0.0028)	-0.0006 (0.0023)	0.0006 (0.0023)	0.0024 (0.0024)	-0.0024 (0.0024)	-0.0307 (0.0558)
Tp3	0.0017 (0.0021)	-0.0024 (0.0029)	-0.0032 (0.0027)	0.0032 (0.0027)	0.0026 (0.0023)	-0.0026 (0.0023)	-0.0576 (0.0664)
Tp4	0.0037 (0.0021)	-0.0039 (0.0032)	-0.0056* (0.0031)	0.0056* (0.0031)	0.0023 (0.0027)	-0.0023 (0.0027)	-0.1113 (0.0810)
Tp5	0.0055** (0.0023)	-0.0095** (0.0039)	-0.0018 (0.0038)	0.0018 (0.0038)	0.0038 (0.0032)	-0.0038 (0.0032)	-0.1173* (0.0644)
Tp6	0.0058** (0.0028)	-0.0091** (0.0045)	-0.0052 (0.0046)	0.0052 (0.0046)	-0.0005 (0.0037)	0.0005 (0.0037)	-0.1196* (0.0627)
Tp7	0.0033 (0.0035)	-0.0117** (0.0056)	-0.0095 (0.0058)	0.0095 (0.0058)	-0.0032 (0.0050)	0.0032 (0.0050)	-0.1889* (0.1052)
Tp8	0.0060 (0.0038)	-0.0123* (0.0070)	-0.0117** (0.0057)	0.0117** (0.0057)	-0.0034 (0.0046)	0.0034 (0.0046)	-0.1241 (0.0900)
Tp9	0.0094 (0.0056)	-0.0099 (0.0090)	-0.0147** (0.0069)	0.0147** (0.0069)	-0.0027 (0.0050)	0.0027 (0.0050)	0.0830 (0.1125)
Tp10	0.0052 (0.0052)	0.0003 (0.0132)	-0.0087 (0.0129)	0.0087 (0.0129)	-0.0016 (0.0097)	0.0016 (0.0097)	-0.0668 (0.0960)
Tp11	0.0064 (0.0081)	-0.0253 (0.0192)	0.0025 (0.0216)	-0.0025 (0.0216)	-0.0138 (0.0113)	0.0138 (0.0113)	0.1139 (0.1062)
Observations	4,434	4,434	4,434	4,434	4,434	4,434	1,789

Notes: Standard errors clustered at the tract level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.
Tm# represents # number of years pre-treatment and Tp# represents # number of years post-treatment.

The results above indicate that there are significant post-treatment average effects on the share of Low-Income Individuals. In particular, LIHTC developments increase the share of Low-Income Individuals by 0.43 percentage points across all years in the sample. Before treatment, there are no significant effects, which means that the parallel trends assumption conditional on covariates holds. The post-treatment effects suggest that relative to just before treatment, LIHTC increases the share of low-income residents by 0.55 percentage points 5 years after treatment and by 0.58 percentage points 6 years after treatment. The results here are in line with Park et al. (2021), and I additionally demonstrate some dynamic effects as well. However, for the share of high-income residents, no causal interpretation can be made. This is because it seems likely that even the conditional parallel trends assumption is not satisfied, as we see significant persistent effects from 7 years before treatment to 5 years before treatment. This means that even with covariates, the trend of high-income residents over time in treated tracts was not similar to untreated tracts before treatment, so we cannot be sure that LIHTC is driving the change.

When we consider the change in the share of white and non-white individuals [1] over time, we only see pre-treatment significance at 7 years before treatment. This can still be interpreted as having causal significance because pre-treatment effects do not seem to persist at all. LIHTC tracts increase (decrease) the share of non-white individuals (white individuals) by 1.17 percentage points and 1.47 percentage points about 8 and 9 years after treatment respectively. Effectively LIHTC is driving a higher share of minorities within neighborhoods approximately 8-9 years after LIHTC units are placed in service.

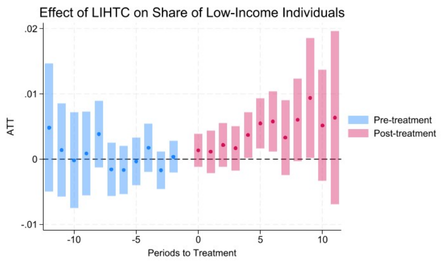
Again, I cannot derive any causal effects of LIHTC on the share of high-skilled and low-skilled jobs over time. This is because we see significant and persistent pre-treatment effects. This does not rule out that movement of low-income individuals and minorities over time could happen due to changes in job composition, but simply rules out making any causal claims on job composition.

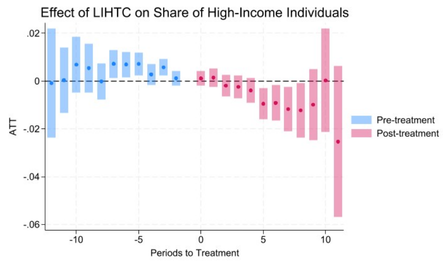
[1] The results are just negative of each other because the share of white and non-white individuals adds up to 1

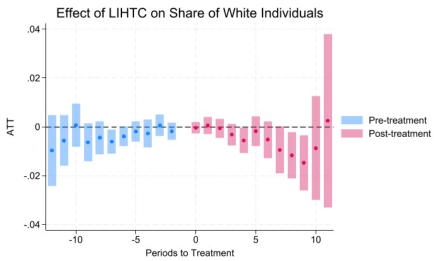
With housing prices, pre-treatment effects are significant and persistent just before treatment, although the effect drops quite substantially (from 0.1125 standard deviations away from the mean to 0.0622 standard deviations from the mean). This is not something that is observed for the pre-treatment effects on high-income residents and job composition. With some caveats then, we can interpret the post-treatment estimates as indicating that LIHTC developments decrease house prices by 0.1173, 0.1196, and 0.1889 standard deviations from the mean at 5, 6, and 7 years after treatment respectively.

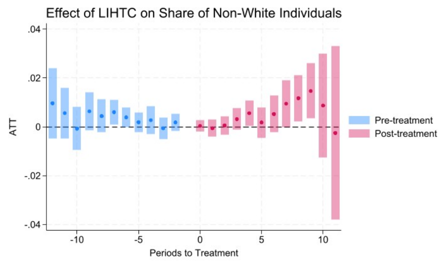
Overall, these results indicate that LIHTC developments do not contribute to gentrification. There are still some threats to causal identification that will be expanded upon in the Conclusion section.

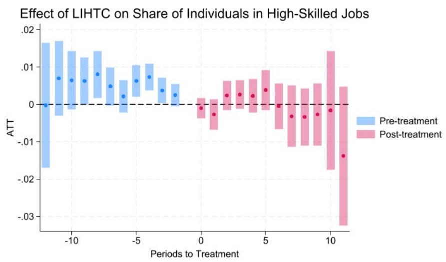
All the results showing in Table 2 are displayed in the following event study diagrams below:

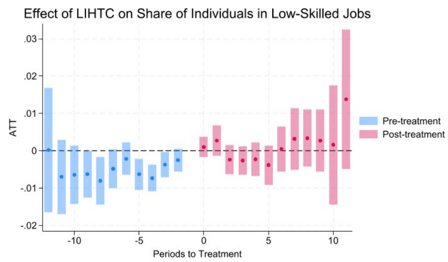


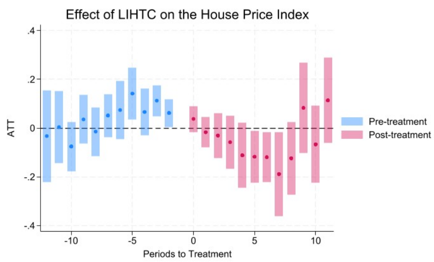












Conclusion

This paper provides evidence that is particularly important to the policy debates surrounding the Low-Income Housing Tax Credit (LIHTC). In particular, the difference-in differences (DID) approach developed by Callaway and Sant'Anna (2021) is adopted here to understand dynamic heterogeneous effects of the LIHTC on several gentrification measures. These measures include the share of low and high income indvidiuals, the share of white and non-white individuals, the share of high skilled and low skilled jobs, and a standardized house price index. I find that LIHTC increases the share of low-income residents by 0.55 percentage points 5 years after treatment and by 0.58 percentage points 6 years after treatment. Additionally, I find positive effects on the share of non-white individuals by approximately 1.17 percentage points and 1.47 percentage points about 8-9 years after treatment, and a decrease in house prices by 0.1173, 0.1196, and 0.1889 standard deviations from the mean at 5, 6, and 7 years after treatment respectively.

The reason we see that the effects seem to happen a few years after a census tract gets LIHTC units is probably just because neighborhood change takes time to happen. Additionally, I also model changes within a census tract and do not consider how nearby census tracts might change as a result of some LIHTC developments. This is a complication that is considered by Voith et al. (2022) and by An et al. (2023), so an extension could consider how other nearby census tracts are affected over time. I also do not consider how effects might change with concentration of LIHTC developments. This could be important for better causal identification as future LIHTC developments are also determined in part by the concentration of LIHTC units in a neighborhood.

I stated in my introduction that I believed that the gravity model and specifically local labor market conditions would drive my results. I do not seem to be able to make this conclusion. This does not necessarily mean that this mechanism is invalid however. It could be the case that from the set of covariates X that I used in my model, I used a set of "bad" controls, essentially I may have introduced collider bias into the estimation. For example, when looking at the share of individuals in low-skill jobs over time, I used the share of low-income individuals as a covariate. Now, if LIHTC developments are causing the increase in the share of low-income individuals over time, its possible that the share of individuals in low-skills jobs is also affecting the share of low-income individuals. A simple way to check this is to see whether conditional parallel trends holds for different sets of covariates. Due to the large number of variables and estimates that I had, this was beyond the scope of this paper. The covariate specification could have also been an issue for the estimation of the Doubly-Robust ATT as it uses the covariates to estimate likelihood of treatment and control.

Despite these vast limitations, I think that my results imply that LIHTC developments in Texas have not contributed to any meaningful gentrification over time, and in fact have stlightly degentrifying effects. This is important to policymakers as it suggests that maybe targeted LIHTC policy seems to (at least, eventually) work in postive ways for the low-income households that it is targeted to. An obvious extension would be to consider neighborhood demographics to capture unemployed and vulnerable populations and to use another state as a control group or to look at the effects of LIHTC nationwide to inform appropriate reform.

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