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Re-evaluating the impact of immigration on the U.S. rental housing market

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A B S T R A C T

Previous studies provide evidence that immigration increases housing prices and rents. To deal with endogenous location choices of immigrants, these studies often use a shift-share instrumental variable approach. This approach, however, fails to adequately account for the natural attraction of immigrants to cities with thriving economies. High-immigration cities provide more economic opportunities and thus exhibit persistently rising housing prices and rents. This paper improves upon the traditional empirical approach by explicitly controlling for initial city characteristics that lead to both increases in immigration and the evolution of rents. Results suggest that after controlling for endogenous sorting of immigrants, the positive effect of immigration on rents is attenuated. While the impact of immigrant inflows is smaller, an extension of the main results suggests the effects of immigration on rents remains larger than that of native inflows.

1. Introduction

With average annual inflows of roughly 1 million persons over the last two decades, legal immigration has a tremendous effect on housing demand in receiving cities in the US. Since the pioneering work of Saiz (2003), many have analyzed the role of immigration on the housing market.[1] Regardless of the country of analysis, researchers typically find a significant, positive short-run impact of immigration on housing prices and rents. Results from studies on the US are fairly consistent: Saiz (2007) finds an inflow of new legal immigrants equal to 1% of the total population causes an increase of around 1% for both rents and housing values, and Ottaviano and Peri (2012) find an increase in housing prices between 1.1 – 1.6%.

The general result found in the literature is hardly debatable; a one-time increase in population should have *some* positive impact on short-run housing prices, ceteris paribus. The question, however, is whether these effects should be considered *causal*. The sorting of immigrants across localities and the resulting bias is well-documented in both the labor and migration literatures, and researchers have utilized a variety of methods to account for this phenomenon (Edin et al., 2003, 2004; Glitz, 2012). In the urban and housing literatures, studies tend to rely on shift-share estimation methods that use predicted immigration rates, based on historical settlement patterns, as an instrument for observed immigration rates. The motivation for this instrument is immigrant location choices are predictable in that the most important determinant of immigrant location choice is the share of the existing population that is foreign-born (Bartel, 1989; Bauer et al., 2005; Chiswick and Miller, 2004; Zavodny, 1999). Because this predicted rate of immigration is merely a function of historical immigrant settlement patterns and national levels of immigration, researchers suggest that the instrument is independent of city-specific shocks that directly impact rents. In other words, the identifying assumption of this approach is that while location decisions of current immigrants are endogenous to the evolution of rents today, historical migration decisions are exogenous.

This approach, however, ignores notable historic and *persistent* differences between high- and low-immigration cities that are important to the current evolution of rents. If shocks to the housing market are persistent over time, then the instrument is potentially invalid – the

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[1] Saiz (2007) and Ottaviano and Peri (2012) analyze the US housing market, Gonzalez and Ortega (2013) in Spain, Accetturo et al. (2014) in Italy, Degen and Fischer (2009) in Switzerland, and van der Vlist et al. (2011) in Israel.

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exclusion restriction would fail to hold as the instrument is correlated with current shocks through past shocks.[2] In this paper, I revisit the effects of immigration on U.S. housing rents and improve upon the shift-share estimation approach by explicitly controlling for initial city characteristics that are correlated with both increased immigration and the evolution of rents. Namely, I include controls for historical economic and housing market characteristics that both attracted immigrants in the past and predispose cities to increased future growth. This empirical approach is motivated by the knowledge that certain economic conditions in cities are persistent over time. Cities that have historically attracted in-migrants (both native and foreign-born) continue to do so in the future (Blanchard and Katz, 1992; Glaeser et al., 1995). Moreover, there exist "superstar cities" that face persistent increased relative housing price growth over time (Gyourko et al., 2013). If immigrants, both past and present, are disproportionately attracted to superstar cities or cities with thriving economies, the effect identified in the existing literature is merely correlation.

To this end, this paper makes two primary contributions to the literature. First, using annual data on housing prices and immigrant inflows, I show that the short-run impact of immigration on housing rents is significantly lower once one controls for the natural attraction of immigrants to cities with thriving economies. Findings suggest that an immigrant inflow equal to 1% of the population leads to a 0.3–0.4% increase in rental prices. This result is robust to alternate definitions of the initial city characteristics, two measures of rental prices, and different sample periods. As noted above, this result is in contrast to the existing literature, which typically estimate a roughly one-to-one impact of immigration on rents. I attribute this roughly 75% reduction in the point estimate to the bias introduced by the shift-share instrumental variable. Because high-immigration cities not only provide increased economic activity today but have done so historically, past immigrant location choices and current rent growth are shown to be positively correlated with the initial economic conditions in these cities. Omission of this relationship leads to biased (upward) and inconsistent estimates as the instrument is correlated with the error term.

An analysis of longer run changes further confirm the role of immigrant location choices, specifically the proclivity to reside in areas with inelastic housing supply, in determining the effect of immigration on housing prices and rents. Because housing supply is more elastic over time, one would expect the impact of immigration to be muted in the medium- and long-run. Medium-run estimates are remarkably consistent with the short-run estimates and, again, suggest that an immigrant inflow equal to 1% of the population leads to a 0.3–0.4% increase in rental prices. Long-run estimates, however, tell a different story. When analyzing 30-year changes in rents and immigrant inflows, the results suggest an immigrant inflow equal to 1% of the total population leads to a 0.9% increase in rents; roughly twice as large as the short- and medium-run estimates. While this result runs contrary to economic theory, I show that this result is driven by the location choices of immigrants. The effect of immigration on rents is shown to be significantly higher in regions with relatively inelastic long-run housing supply (e.g. Pacific and New England Census divisions and California). Specifically, an immigrant inflow equal to 1% of the population to a Core-Based Statistical Area (CBSA) with inelastic long-run housing supply leads to an increase in rents of 1.2–1.6% more than CBSA's located in the rest of the US. In areas with relatively elastic long-run housing supply, the estimated effect is less precise and much smaller in magnitude (again, around 0.4%).

The question remains, however, whether the magnitude of the results herein are reasonable. To frame expectations on the anticipated effect of immigration on rents, further context is needed. In the housing literature, population growth or employment growth are often included as controls in the typical housing price determination equation. The evidence of the impact of population growth on housing prices is mixed, as studies often find insignificant and wrong-signed estimates (Malpezzi et al., 1998; Poterba, 1991). Abraham and Hendershott (1996) do find a positive and statistically significant impact of employment growth on housing, but the magnitude is much smaller – around 0.3% increase in housing prices for a 1% increase in employment. These results present an empirical puzzle that has not been addressed in the literature. Why is the effect of total population growth on housing prices and rents significantly smaller and more difficult to ascertain than the effect of immigrant inflows?

Two plausible explanations exist to explain the differences in magnitudes between the effect of immigrant inflows relative to the effect of overall population growth on housing prices and rents. One explanation is that immigrant inflows and native population growth are *not* inherently different; rather, differential effects on housing prices and rents are driven by the location choices of the two groups. One such example are differences in housing supply elasticity across locations in the US (Green et al., 2005; Saiz, 2010). The elasticity of housing supply is fundamental in determining the effect of a demand shock on housing prices. If supply is relatively elastic, an increase in housing demand would have a relatively small impact on prices, as the housing stock more easily adjusts to the population inflow. If, however, supply is relatively inelastic, an increase in housing demand should lead to increased prices and rents. As such, differences in the estimated effects could be driven by the fact that immigrants disproportionately settle in areas with relatively inelastic supply, while natives tend to settle in areas with relatively elastic supply. The data seem to support this interpretation. Of the newly-arriving immigrants who entered the US between 2000 and 2010, 94% settled in metropolitan areas, 40% settled in metropolitan areas considered "superstars" by Gyourko et al. (2013), and 67% settled in metropolitan areas with highly inelastic housing supply.[3] Conversely, of natives who recently moved in the 2010 Census, 76% settled in metropolitan areas, 11% settled in superstar metropolitan areas, and 31% settled in metropolitan areas with highly inelastic housing supply.[4] It is clear that immigrants are more likely to settle in large cities with relatively more inelastic housing supply. Because past studies on the effect of immigration on rents fail to adequately control for superstar status and the elasticity of housing supply, it is unsurprising that the effects estimated by those studies are larger.

While the data suggest that differences in location choices could resolve the puzzle noted above, it is possible that immigrant inflows and native population growth do have a differential impact on the housing market. It is well known that immigrants are more likely to settle in cities with larger immigrant populations, as these communities provide cultural amenities and network externalities. If the desire to reside in these high-immigrant cities is sufficiently strong, then the increased willingness to pay of newly arriving immigrants has the potential to bid up rents and housing prices in receiving cities above and beyond an equal-sized inflow of natives (Saiz, 2007). As such, whether immigrant inflows and native population inflows have a differential effect on the housing market is purely an empirical question.

Prior studies in the literature have ignored native population flows in the estimating equation and focused solely on the inflows of immigrants. Considering that both native and immigrant inflows are housing demand shifters, this empirical approach is incomplete. In an equilibrium model of the housing market, one would expect *both* immigrant

[2] Saiz (2007) acknowledges the potential harm of this omitted relationship (p.356): "Omitted variables that are differentially present in cities with high immigration inflows, and that might account for the growth in rents in these cities (such as economic shocks), are a potential threat to my interpretation of the result."

[3] Here, "highly inelastic" is defined as those metropolitan areas in the top quartile of the Wharton Residential Land Use Regulation Index (WRLURI). Further information on this index is provided later in the paper.

[4] Author calculations using the 5-year sample of the 2016 American Community Survey. Statistics are derived from a sample of US natives who are recent movers.

Table 1
High-Immigration vs. Low-Immigration CBSA's (1990–1998).

CBSA Group	Population Growth	Real Average Weekly Wage Growth	Establishment Growth	WRLURI	Share of New Immigrants (1999–2011)
High-Immigration (Top-25 CBSA's)	11.14%	20.02%	26.06%	0.362	71.35%
Low-Immigration (Bottom-25 CBSA's)	2.17%	11.21%	11.51%	−0.759	0.11%

1. CBSA's were designated as high or low-immigration CBSA's based on the percentage of immigrants that located settled from 1999–2011. The 25 CBSA's that received the highest percent of immigrants during that period are high-immigration, while the 25 CBSA's that received the lowest percent are low-immigration. The last column shows the share of all newly arriving immigrants from 1999–2011 that located in these CBSA's.
2. The calculations for real wage growth and establishment growth come from the Quarterly Census of Employment and Wages (QCEW).
3. WRLURI is the Wharton Residential Land Use Regulatory Index. Higher values suggest a less elastic housing supply. For the entire sample, the mean and standard deviation of WRLURI is −0.120 and 0.681, respectively. Therefore, high-immigration CBSA's are roughly two-thirds of a standard deviation above the mean (less elastic) while low-immigration CBSA's are roughly one standard deviation below the mean (more elastic).

and native population flows to influence the evolution of rents. As such, a second contribution of this paper is to address the relative effects of immigrant and native populations flows on housing rents. The results of the extension are two-fold. First, the main result is robust to the inclusion of native inflows into the model. Findings again suggest that immigrant inflows equal to 1% of the population leads to a 0.4% increase in rental prices. Second, even with a properly specified model that accounts for inherent differences in economic and housing market conditions among cities, immigrant inflows have a larger impact on rents relative to native population inflows. The difference in the magnitude is highly statistically significant at the 1% level. Taken together, the results suggest that differences in location choices, while important, cannot explain the entire gap in the literature.

The rest of the paper is structured as follows. Section two presents the empirical strategy for the paper. I start by documenting the tendency for immigrants to cluster in cities with thriving economies with relatively inelastic housing supply and discuss the implications for the typical model specification used in the existing literature. Section three describes the data sources used in this analysis. A full description of each variable used can be found in the Data Appendix and summary statistics are provided in Table 2. Section four presents the main results of the paper. Section five provides empirical support for the bias introduced by the shift-share instrument without controlling for initial city characteristics and the role of housing supply. Section six provides the analysis of the effects of immigrant inflows relative to native inflows. Section seven concludes.

2. Empirical strategy

The empirical strategy is motivated by the fact that past economic and housing market conditions have a persistent long-run impact on future growth. Cities that attracted migrants in the past (both native and foreign-born) will continue to do so in the future (Blanchard and Katz, 1992; Glaeser et al., 1995). In other words, if a particular city was thriving and growing in the past, then one would expect these same cities to experience increased overall growth in economic activity, and thus housing demand, today. Evidence of this is provided by Capozza et al. (2002) who find relatively large impacts of longer-run population growth on housing prices. The authors estimate that a 1% increase in population over the last five years (their proxy for expected long-run population growth) leads to a 1.5% increase in real housing prices. While the magnitude of this result may seem to be in line with the existing literature on immigration and housing rents, I argue that this shows the potential bias in previous studies. Long-run population growth is correlated with initial city characteristics that make one city more desirable than others (i.e. amenities, labor market conditions, etc.). Thus, current period rent growth should be modeled as a function of both contemporaneous factors *and* initial city characteristics.

I motivate the empirical strategy and the importance of initial city conditions in Fig. 1 and Table 1. Fig. 1 plots average rent growth and average immigrant inflows (as a percent of lagged total population) from 1999–2011 in Core Based Statistical Areas (CBSA's). Consistent with the existing literature, there is a statistically significant positive relationship between rent growth and immigrant inflows. Absent from past models, however, is a discussion regarding where immigrants are locating (Degen and Fischer, 2009; Gonzalez and Ortega, 2013; Saiz, 2007). Note the cities in the NE region and those in the SW region of Fig. 1. Immigrants are locating in some of the largest cities in the U.S: Miami, Los Angeles, San Francisco, New York City, among others. These high-immigration cities are those with more overall economic activity that attract both firms and workers (both immigrants and natives). More importantly for the empirical strategy, these high-immigration cities had thriving economies *prior* to the entry of the immigrant wave in Fig. 1. Table 1 provides an overview of high-immigration and low-immigration CBSAs for the period 1990–1998. High-immigration cities experienced significantly higher growth in total population, real wages, and the number of work establishments prior to the immigration wave illustrated in Fig. 1. Moreover, housing supply is significantly less elastic in high-immigration cities. Thus, because of favorable economic conditions and relatively inelastic housing supply, one would expect high-immigration cities to face increased growth in housing prices relative to low-immigration cities irrespective of a new immigrant inflow.

Fig. 1. CBSA-level Rent Growth and Immigrant Inflows.

2.1. Empirical model

The typical empirical model in the literature regresses the change in rental prices on some measure of immigration penetration and a collection of explanatory variables controlling for various economic conditions. The model herein follows most closely with that of Saiz (2007).

Formally, the model is written as:

$$\Delta\ln(r_{k,j,t}) = \beta\left(\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}\right) + \alpha X_{k,t} + \pi W_{k,t-1} + \mu\Delta Z_{k,t-1} + \delta M_{k,t^*} + \theta_{j,t} + \Delta\varepsilon_{k,t}. \quad (1)$$

Consistent with Saiz (2007), the dependent variable is the annual change in the log rent in city k within region j at time t. The main explanatory variable is the lagged annual inflow of legal immigrants admitted to city k at time t-1 as a percent of the total population in period t-2, making β the coefficient of interest. The interpretation of β is as follows: an immigrant inflow equal to 1% of the total population leads to a β% chnage in rents. As noted above, one would expect a positive short-run impact of immigration on rents ($\beta > 0$); thus, it is the magnitude of β that is of interest in this paper. To control for differences in metropolitan areas, I follow the existing literature by controlling CBSA-level economic conditions. Specifically, the vector $X_{k,\,t}$ includes city-specific attributes, such as climate, crime, and land area, and the initial share of the population holding at least a bachelor's degree. $W_{k,t-1}$ is the lagged unemployment rate in the CBSA. $\Delta Z_{k,\,t-1}$ represents lagged changes in per capita income.

The model diverges from those in the existing literature with the inclusion of M_{k,t^*}. Following Glaeser et al. (1995), among others,[5] M_{k,t^*} is a vector of initial CBSA-specific, time invariant variables in some year $t^* < t$. As detailed below, these initial conditions are included to control for factors that both attracted immigrants in the past and predispose cities to increased future growth. The vector M_{k,t^*} includes rent growth from 1980–1990, the initial Fair Market Rent (FMR) level in 1990, the share of the housing stock built before 1939 in 1990, the percent of total earnings coming from farms in 1990, per capita property tax revenues in 1997, and per capita spending in retail and service establishments in 1992. Rent growth in CBSA k from 1980–1990 and the FMR level in 1990 are the main inclusions in the preferred model. The intuition behind these two variables is described in detail below; however, it should be noted that both of these variables essentially serve the same purpose: to control for the fact that certain cities are predisposed to increased future rent growth. As such, these two variables do not enter into the specification together. I estimate two variants of Eq. (1) where the initial rent growth and initial rent levels enter separately.

Rent growth from 1980–1990 controls for the possibility that immigrants are locating in "superstar" cities. Gyourko et al. (2013) show that housing price appreciation in certain cities is persistent and that superstar cities that experience increased past price growth will face higher future appreciation. The authors show that high housing price growth in superstar cities occurs even if the inherent value of a location, the elasticity of housing supply, and the willingness to pay to live in each location is held constant. The initial FMR level in 1990 is a proxy for overall economic vibrancy in a city. Cities with higher rents in 1990 were those with thriving economies experiencing positive economic shocks. When rents are higher, the values of local amenities must be higher in order to compensate for this increase in housing expenditures (Roback, 1982). As such, these cities are attractive to in-migrants, both native and foreign-born. Furthermore, population tends to flow to areas with higher housing prices and higher rents, and these population flows are persistent over several decades (Rappaport, 2004). Thus, cities with high rents in period t^* will face higher future growth in housing demand (relative to those cities with lower housing prices) in period $t > t^*$. If immigrants are inherently attracted to these same cities yet the model ignores this relationship, then one might falsely attribute accelerated future rent growth to immigrant inflows.

Per capita property tax revenue is expected to have a positive impact on future housing prices. Note that this is property tax revenues, not property tax rates. Thus, this variable is not meant to control for property taxes in the user cost of owning a home; rather, this measure is a proxy for the initial amenity level of a CBSA relative to others. Higher per capita property tax revenue suggests increased spending on public goods, namely education and police. In cities with higher property tax revenue, one expects higher amenity values of public goods, and these amenity values should be capitalized into rents. The impact of the share of the housing stock built prior to 1939 is, *a priori*, ambiguous. On one hand, an older housing stock may depress growth in housing prices. Brueckner (1982) suggests that an inverse relationship exists between the age of the housing stock and future population growth. If so, a lack of population growth will slow housing demand and, *ceteris paribus*, slow the growth of rents in the city. On the other hand, an older housing stock could have a positive impact on future housing prices if there is an incentive to revitalize the city (i.e. gentrification). The percent of total earnings coming from farms in 1990 is included as a proxy for the opportunity cost of converting agricultural land to residential land and is expected to have a positive impact on future housing price growth. Per capita consumer spending serves as a proxy for the overall economic activity in a city and should be positively correlated with future housing price growth.

The preferred model includes three controls for housing supply conditions. As noted above, supply elasticity may play are particularly important role, as immigrants seem to cluster in cities with relatively inelastic housing supply. I include controls for land area, the stringency of land use regulations, and the cost of construction. In Saiz (2007), land area of the CBSA is the lone control for housing supply. However, it has been consistently shown that a strong positive relationship exists between housing prices and the stringency of land use regulations (Gyourko et al., 2008; Ihlanfeldt, 2007; Malpezzi, 1996; Pollakowski and Wachter, 1990; among others). A city with more stringent land use regulations (i.e. zoning laws, local government interventions, etc.) will face higher future housing prices. To control for the degree of land use regulations, the vector $X_{k,\,t}$ also includes the Wharton Residential Land Use Regulatory Index (WRLURI) (Gyourko et al., 2008). The WRLURI is superior to the use of land area in that it encompasses a wide range and a large number of land use regulations. Pollakowski and Wachter (1990) suggest that analyzing the effect of land use regulations individually (i.e. land area), as opposed to collectively (i.e. WRLURI), will understate the impact of these controls on housing prices. One disadvantage, however, is that WRLURI, like land area, is time-invariant. Therefore, it must be assumed that land use regulations within a city are constant throughout the sample period. To proxy for cost of new construction I include the one period lag of the change in average construction wages.

The last addition to the model is the inclusion of region-by-year fixed effects ($\theta_{j,\,t}$) to control for regional differences in rent appreciation. Thus, β is estimated from changes in the number of newly arriving immigrants within a CBSA over time, compared to other CBSA's in the region.

2.2. Instrumental variable strategy

Because immigrant location choices are endogenous, Eq. (1) is estimated via 2SLS using the standard shift-share instrumental variable strategy used in the existing literature (Altonji and Card, 1991; Saiz, 2007; among *many* others).[6] Here, actual immigrant inflows are instrumented by predicted immigrant inflows based on historical settlement

[5] Several papers, mainly in the growth literature, use initial city conditions to explain differential growth rates among cities or metropolitan areas (Glaeser et al., 1995; Drennan et al., 1996). However, a few studies use this technique in other literatures; namely, the housing market (Engberg and Greenbaum, 1999) and the labor market (Beeson and Montgomery, 1993).

[6] Accetturo, et al (2014), Card (2001, 2009), Cortes (2008), D'Amuri and Peri (2014), Degen and Fischer (2009), Gonzalez and Ortega (2013), Hunt and Gauthier-Loiselle (2010), Lewis (2003), Ottaviano and Peri (2012).

patterns. The instrument is defined as:

$$\widehat{Immigrants}_{k,t} = \theta_{k,t^*} * I_{US,t}. \quad (2)$$

The first term on the right-hand side is the share of newly arriving immigrants that migrated to city k in some base year t^*.[7] The second term is the total number of immigrants admitted to the US in year t. Thus, it is assumed that each city will receive the same fraction of all newly arriving immigrants in every year after the base year t^*.

The identifying assumption is that while current location decisions are endogenous to current economic and housing market conditions in the city, settlement decisions of *previous* immigrant waves (θ_{k,t^*}) are uncorrelated with *current* economic conditions. This follows from the standard result that the only significant determinant of immigrant location decisions is the existing share of foreign born persons in a city. In fact, it has been shown that other factors, such as labor market conditions, do not have a discernible effect on location decisions of immigrants (Bartel, 1989; Zavodny, 1999; Chiswick and Miller, 2004; Bauer et al., 2005). Thus, one can use imputed immigrant inflows, based on historical migration patterns, to instrument for current period immigrant inflows.

3. Data

The data used in this paper are a panel of 325 Core Based Statistical Areas (CBSAs) over the period 1999–2011.[8] I use the 2013 CBSA definitions based on population estimates from the 2010 U.S. Census. The advantage of using current CBSA definitions is that metropolitan areas are no longer defined using partial counties. Thus, county-level data is easily aggregated to the CBSA-level.

Data on immigrant inflows comes from the "Immigrants Admitted to the United States" data series of the Department of Homeland Security (DHS).[9] Following the discussion of Saiz (2007), these data should be considered a "noisy indicator" of recent immigrant inflows for three reasons. First, the data do not identify the actual timing of arrival to the U.S., as there may be lags from the time a person is granted admission and actually arrives in the U.S. While the timing of arrival may be off for some, the data suggest the error is minimal. In 1995 (the year chosen for the base year of the instrument), 76% of all immigrants were admitted and arrived in the same year and more than 99% of the immigrants arrived within one year of admission.[10] Second, immigrant inflows are calculated using data on the zip code of *intended* residence. If an immigrant settles in a different location than stated in the data, then I overstate the immigrant inflow to certain CBSA's, while understating the inflow in the actual CBSA of residence. Third, as noted above, I do not observe undocumented immigrant inflows to the U.S.

Though data limitations exist, these data have the advantage of being the only available source of annual immigrant inflows to the US. The concern over undocumented immigrant flows is most relevant to this study and one that must be addressed. One concern is that undocumented immigrants may cluster differently than legal immigrants, which could occur if undocumented immigrants are more heavily concentrated in border cities due to higher transportation costs. While accurate counts of the undocumented immigrant population at the CBSA level do not exist, the state-level estimates are consistent with the legal immigrant population. Passel et al. (2004) estimate that roughly two-thirds of all undocumented immigrants live in just six states: California, Florida, Illinois, New York, New Jersey, and Texas. These six states are also the main hubs for legal immigration – 66% of all legal immigrants settled in the same six states from 1999–2011. While undocumented immigrant populations may cluster in the same states as legal immigrants, it is possible that undocumented immigrants cluster in different parts of a CBSA or that the willingness to pay to live near other immigrants may be stronger for undocumented immigrants as the benefits from ethnic enclaves are larger. Again, I do not have data at finer geographic levels and cannot account for this in the current model.

I make use of two sources of rental price data. First, following Saiz (2007), I use the Fair Market Rent (FMR) series from the Department of Housing and Urban Development (HUD). The FMR in a particular area corresponds to the market value of a vacant two-bedroom unit of standard quality. HUD reports FMR's at the county-level for each county in the U.S. For most counties in the sample, the FMR is the price of the unit at the 40th percentile of the rent distribution; however, this definition has not remained constant over time. Prior to 1996, a FMR was defined as the 45th percentile and starting in 2005, the FMR for a small sample of counties are reported as the 50th percentile of the rent distribution. Thus, I normalize the rental housing price measure throughout the sample, by adjusting all FMR's in all years to 40th percentile estimates.[11] In all specifications, I use changes in real rents as the dependent variable.

The second source of rental price data is from the interarea rental cost panel of Carrillo et al. (2014) – hereafter, the CEO index. The CEO index measures the cost of renting a unit of average quality in a given area. The authors utilize housing data from the HUD's 2000 Section 8 Customer Satisfaction Survey to estimate an interarea housing price index for the year 2000. Then, using BLS time series price indices, the authors create a panel of interarea prices.[12] The CEO index has several advantages over the FMR measure used in prior studies. First, unlike the FMR definition, the CEO index provides a consistent panel of rents over time. Because the FMR definition changes over time, the FMR data series must be linearly extrapolated to form a consistent series. As can be seen in Fig. 2, sharp decreases in average rent growth occur in these two adjustment periods (1995 and 2005).

A second advantage of the CEO index is that it uses a much richer set of data to estimate the average quality of identical units in each geographical location. As noted by Carrillo et al. (2014), the procedures used to construct the FMR do not attempt to estimate the rent of *identical* units in different locations. In fact, the FMR in an area is a gross rent estimate of a unit of standard quality. Standard quality units, as defined for FMR, have the following attributes: tenants pay cash rent, the unit is on 10 acres of land or less, the unit has full plumbing and full kitchen facilities, the unit is more than two years old, and meals are not included in the rent.[13] From this definition, it is clear that standard quality housing units are likely to differ along many other dimensions.

[7] 1995 is used as the base year of the instrument. I chose 1995 as it is a central date for which data on initial conditions are available. Ultimately, the choice of 1995 as the base year was an arbitrary one as all results hold when different base years are used. Results using alternate base years for the instrument are available upon request.

[8] There are 377 CBSA's defined in the 2013 definitions (less CBSA's in AK and HI); however, I only have complete data for 325 of these CBSA's. This will not impact the analysis as it compares to Saiz (2007) because most (if not all) of the 52 omitted CBSA's were not included in Saiz's sample.

[9] During the sample period analyzed in Saiz (2007), this data series was under the control of the Immigration and Naturalization Service (INS). While these data (1999 – 2012) are now managed by the Department of Homeland Security, the structure of the data is the same. While these data are from the same source as used in Saiz (2007), one difference should be noted. Due to increased security measures, the DHS does not provide the micro-data files of these data. These data are publicly available on the DHL website, but MSA definitions are not constant across years. Thus, the custom data I received were aggregated using the most current (2013) CBSA definitions.

[10] I am unable to make use of these admission data because I do not have access to the individual-level microdata for the years 1999-2011.

[11] I outline this process for the 2005 adjustment in the data appendix. The adjustment of rents prior to 1996 follow the same methodology.

[12] For a full description of the estimation methodology, I direct interested readers to Carrillo et al. (2014). HUD uses a similar methodology when updating and trending forward their baseline estimates.

[13] The Department of Housing and Urban Development, Office of Policy Development and Research. (2007, July). *Fair Market Rents For The Section 8 Housing Assistance Payments Program*. Retrieved from https://www.huduser.gov/portal/datasets/fmr.html.

Table 2
Descriptive Statistics (2010).

Variable	Obs	Mean	Std. Dev.	Min	Max
Total Population	325	777,053.50	1691,680	55,212	19,567,410
Real FMR (Constant 40th Percentile)	325	\$784.97	\$202.55	\$546.16	\$1656
Real FMR (Unadjusted)	325	\$781.90	\$197.50	\$546.16	\$1656
CEO Index	311	1.259	0.271	0.852	2.494
Immigrants	325	3005.59	12,889.28	22	186,086
$Immigrants_{k,t-1}/Population_{k,t-2}$	325	0.0021	0.0018	0.00017	0.0154
Immigrant Share (1995)	325	0.0027	0.0134	0	0.2144
% of Pop with Bachelor's (1990)	325	0.1905	0.0621	0.0896	0.4214
Murder Rate, per 1000 population	325	4.3391	3.1873	0	20.8321
Land Area (sq miles)	325	2700.79	2880.46	145.59	27,278.47
Average January Temperature	325	35.9846	12.1993	4.4	66.8
Average July Humidity	325	56.8031	16.1934	14	80
Unemployment Rate	325	9.46%	2.72%	3.8%	26.2%
Per Capita Income	325	\$36,340.77	\$6205.52	\$20,946	\$71,768
% Housing Stock Built Pre-39 (1990)	325	0.1639	0.1044	0.0072	0.4993
% Total Earnings from Farms (1990)	325	0.0248	0.0321	0.0005	0.2256
Rent Growth (1980–90)	325	0.0386	0.1290	−0.5517	0.3693
Log Per Capita Prop Tax Rev (1997)	325	6.6783	0.4648	5.1394	7.8753
Log Per Capita Sales (1992)	325	10.9068	0.3051	9.4086	12.0878
FMR (1990)	325	\$795.36	\$179.01	\$454.43	\$1640.88
Price-to-Rent Ratio (1990)	325	166.52	42.08	104.06	348.93
Change Real Average Construction Wages	325	−0.0070	0.0504	−0.3243	0.3412
WRLURI	325	−0.2169	0.7507	−1.7647	4.3353

1. All dollar values are 2010-constant dollars, adjusted using the CPI-U.

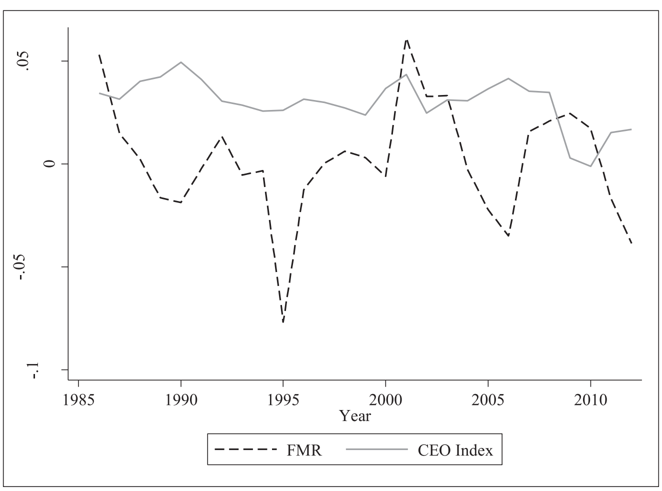

Fig. 2. Growth Rates of FMR and CEO Index, 1985–2011.

Thus, while Carrillo et al. (2014) show that the two rent measures are highly correlated, they are far from proportional – the FMR tends to be significantly higher than the CEO index for areas where the CEO index is the highest. The mean absolute deviation between the CEO index and the FMR is roughly 7% for all areas and the largest absolute deviation is 37% (Carrillo et al., 2014; Table 2).

While the CEO index has many advantages over FMR, there are potential disadvantages. First, the CEO index is not reported for fourteen CBSAs in the sample. While this is not ideal, the omitted CBSAs are some of the smallest CBSAs in terms of population and the immigration impact variable.[14] While I lose some observations each year, the CEO index is available for a longer panel. The second potential disadvantage of the CEO index stems from the use of Section 8 housing data in constructing these baseline rent indices. One may be concerned that rents for Section 8 housing differ from unsubsidized housing; however, prior research suggests this is not the case and that rents paid on voucher units are almost identical to the rents paid on unsubsidized units (Wallace et al., 1981; Leger and Kennedy, 1990). A second concern relates to the quality of Section 8 housing and the propensity of immigrants to rent similar quality housing units. That is, do immigrants tend to rent housing units of similar quality to Section 8 housing units? While feasible, if immigrant-induced demand shocks are concentrated on housing units of similar quality to Section 8 housing units, then this would actually *increase* the estimated impact of immigration on rents. Thus, any bias generated from the "under-placement" of immigrants (in terms of quality of housing) would actually work against the interpretation of this paper.

Per capita personal income are from the BEA Regional Information Systems (REIS) and converted to real terms using the CPI-U. Other explanatory variables come from a variety of sources and follow directly from Saiz (2007). Civilian labor force and unemployment rate data are from the Bureau of Labor Statistics (BLS). Climate data are from the United States Department of Agriculture Economic Research Service Natural Amenities Scale Database. Violent Crime and murder data are from the FBI Uniform Crime Reports (UCR).[15] Initial MSA-specific conditions come from the 1994 County and City Data Book and the 1990 Economic Census. Full definitions of the variables used can be found in the Data Appendix, while summary statistics are reported in Table 2.

4. Results

The discussion in section two suggests that past results may have suffered from specification error as they omit fundamental factors that impact rent growth, independent of immigration. To see the effects of these omitted variables, I start by replicating the specification in Saiz (2007) using annual data from 1999–2011. The dependent variable is the change in the log of real FMR. Again, this specification excludes initial city characteristics (M_{k,t^*}), WRLURI, a control for changes in

[14] The omitted CBSA's when using the CEO index include: Albany (OR), Beckley (WV), Bloomsburg (PA), Carbondale (IL), Chambersburg (PA), Daphne (AL), East Stroudsburg (PA), Gettysburg (PA), Grand Island (NE), Hammond (LA), Hilton Head Island (SC), Midland (MI), Sierra Vista (AZ), and Staunton (VA). The average population for CBSA's without a measure for the CEO index was 119,683. For those with the CEO index, average population was 734,451.

[15] Some states did not consistently report crimes to the FBI. For these states (i.e. FL, IL, KS, MN, etc.), individual state Uniform Crime Reports were used.

Table 3
Immigration and Rents – Replication of Saiz (2007).

VARIABLES	(1) OLS Δr_{ijt}	(2) 2SLS Δr_{ijt}
Immigrants$_{k,t-1}$/Population$_{k,t-2}$	1.425***	1.316***
	(0.347)	(0.428)
Unemployment Rate (t-1)	−0.126***	−0.123***
	(0.033)	(0.034)
Δ Per Capita Income (t-1)	0.013	0.013
	(0.031)	(0.031)
% Pop with at least Bachelor's (1990)	−0.012	−0.010
	(0.009)	(0.009)
Murder Rate (2000)	0.0002	0.0002
	(0.0002)	(0.0001)
Log Land Area (1990)	0.0004	0.001
	(0.0006)	(0.001)
Log Mean January Temperature	0.008***	0.008***
	(0.001)	(0.001)
Log Mean July Humidity	0.001	0.001
	(0.001)	(0.001)
Initial CBSA Variables (M_{k,t^*})	No	No
Year Fixed Effects	1999–2011	1999–2011
Region-by-Year Fixed Effects (θ_{jt})	No	No
Observations	4221	4221
R-squared	0.158	0.158

1. Each column represents a unique specification: column (1) presents the OLS estimates while column (2) presents the 2SLS estimates where observed immigrant inflows are instrumented with the predicted immigrant inflows from the shift-share instrumental variable. The dependent variable is the change in the log of FMR of CBSA *k* at time *t*. Year fixed effects are included in both specifications; however, these point estimates are omitted for the sake of brevity. Robust standard errors clustered by CBSA are included in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

construction costs, and region-by-year fixed effects. I present the OLS and 2SLS estimates in columns (1) and (2) of Table 3, respectively.

Two important results emerge from Table 3. First, the results in Table 3 serve as an appropriate and comparable baseline for past studies. The coefficient of interest in the OLS (2SLS) model suggests that an immigrant inflow equal to 1% of the total population will cause rents to increase by 1.43% (1.32%). While the magnitude of the point estimate is similar to past studies, so too is the fact that the magnitudes of the OLS and 2SLS coefficients are essentially the same.[16] This is concerning and could occur if the shift-share instrument is "too strong" (Jaeger et al., 2018). In other words, if the instrument predicts actual immigrant inflows too closely, it is likely that the instrument is subject to the same endogenous variation as actual immigrant inflows. I further explore the exogeneity of the instrument in Section 5.

The second notable result from Table 3 is that most of the other control variables are not statistically significant. Most importantly, the two controls included to account for differences in economic and housing supply conditions – the share of the population with a Bachelor's degree and land area – appear to have no effect on rent growth. As this runs contrary to expectations, two possible explanations exist for this result. On one hand, it could be specification error – both variables are simply poor controls for differences between cities and have no explanatory power for rent growth. On the other hand, the controls and the underlying expectations of their effects are solid, yet the dependent variable is measured with considerable error. In other words, the share of the population holding a college degree and land area are important predictors of rent growth, but changes in FMR is a poor measure of changes in rents in a city. I address both of these concerns below.

I address the possibility of misspecification and omitted variable bias by estimating preferred 2SLS specification in Eq. (1) using the same sample as above and report the estimates in Table 4. Each column in Table 4 represents a unique specification with columns differing along two dimensions. First, odd-numbered columns include FMR in 1990 as the control of initial economic vibrancy, while even-numbered columns include rent growth from 1980 to 1990. Second, I estimate the model with and without region-by-year fixed effects. Because of the relatively short panel, concern arises that region-by-year fixed effects may "soak up" too much of the variation in the independent variable of interest.

The effect of city-specific factors is evident in Table 4. Regardless of specification, the coefficient of interest is lower when initial city characteristics are included in the model, suggesting past estimates were biased upwards. The reduction in the point estimate is quite large – the effect of immigration on rents falls by around 75% from the baseline in column (2) of Table 3. While the point estimates in Table 4 are not statistically significant, they are statistically different from the replication estimates in column (2) of Table 3 at the 5% level. Moreover, the new controls for initial city characteristics perform reasonably well. Both measures for economic vibrancy, initial FMR in 1990 and rent growth from 1980 to 1990 are highly statistically significant and take the expected sign: CBSA's with higher initial rent levels or higher rent growth in the past experienced accelerated rent growth in the future.[17,18] Once I control for the fact that some cities are predisposed to higher future rent growth, the effect of immigrant inflows on rents is considerably lower.

Overall, the results seem to confirm the presence of omitted variable bias in past studies; however, this is inconclusive, as the effect of immigration on rents is no longer statistically significant. The lack of precision in the point estimate of interest is concerning. While I return to this point in more detail in Section 5, the lack of statistical significance is not due to a weak instrument problem. As seen in Table A3 of the Appendix, the instrument performs well in the first stage. Predicted inflows have a statistically significant impact on actual immigrant inflows, and partial F-test critical values for the excluded instruments are sufficiently high. The problem lies in the reduced form – the OLS regression of changes in log FMR on the instrument and all other exogenous variables in the model. The reduced form estimates are presented in Table 5. Column (1) presents the reduced form for the baseline 2SLS model in Table 3, while column (2) presents the reduced form estimate for the preferred 2SLS model in column (4) of Table 4. It is clear that the preferred model suffers from a weak reduced form as the estimated impact of predicted immigrant inflows has no statistically significant effect on rent growth and the R-square is relatively low.[19]

One potential explanation for the lack of significance in the reduced form is that FMR is a poor measure of rental costs Carrillo et al., 2014). As such, I re-estimate the model using the changes in the log CEO index as the dependent variable and report estimates in Table 6. Columns ((1) and (2) again show estimates of the baseline model without control-

[16] Saiz (2007) reports a point estimate on the immigration impact variable of 1.028 (0.995) for OLS (2SLS) estimation

[17] I also estimated the model where each element of the vector M_{k,t^*} is added independently to determine which variables are important in the reduction in the point estimate. These results are presented in Table A1 of the appendix and confirm that initial economic vibrancy (initial FMR or initial rent growth) is the driving factor in the reduction of the point estimate.

[18] I also estimated the model using different definitions of economic vibrancy for the city. These results are presented in Table A2 of the appendix and show that the results are not sensitive to varying definitions of initial economic vibrancy. Specifically, I re-estimate (1) using the following proxies in place of initial rent level and initial rent growth: FMR growth from 1983-90, initial median gross rent in 1990, the average commute in 1990, and the price-to-rent ratio in 1990. The first three proxies follow directly from the discussion in section 2. The price-to-rent ratio is included as it has been shown to be positively correlated with future capital gains (Capozza and Seguin, 1996) and future rent growth (Clark, 1995; Gallin, 2008). The intuition is that when the price-to-rent ratio is high in year *t-k*, owner-occupied housing is overvalued. As such, rents increase in future periods as the market works to correct itself.

[19] I estimated the reduced form for all specifications listed in Table 4. The results are qualitatively similar, as each specification suffers from a weak reduced form.

Table 4
Immigration and Rents – Preferred Model.

VARIABLES	(1) 2SLS Δr_{ijt}	(2) 2SLS Δr_{ijt}	(3) 2SLS Δr_{ijt}	(4) 2SLS Δr_{ijt}
$Immigrants_{k,t-1}/Population_{k,t-2}$	0.258	0.264	0.257	0.179
	(0.506)	(0.511)	(0.476)	(0.504)
Unemployment Rate (t-1)	−0.137***	−0.136***	−0.139***	−0.139***
	(0.038)	(0.038)	(0.049)	(0.051)
Δ Per Capita Income (t-1)	0.009	0.012	0.032	0.034
	(0.031)	(0.030)	(0.031)	(0.031)
% Pop with at least Bachelor's (1990)	−0.018**	−0.016*	−0.020**	−0.020**
	(0.009)	(0.009)	(0.010)	(0.010)
FMR (1990)	0.013***		0.010***	
	(0.003)		(0.004)	
Rent Growth (1980–90)		0.017***		0.015***
		(0.005)		(0.006)
Per Capita Sales (1992)	0.002	0.003*	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Per Capita Prop Tax Rev (1997)	−0.001	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
% Housing Stock Built Pre-39 (1990)	0.011**	0.016***	0.009	0.014**
	(0.006)	(0.005)	(0.007)	(0.007)
% Total Earnings from Farms (1990)	0.032*	0.030*	0.019	0.016
	(0.017)	(0.016)	(0.016)	(0.016)
WRLURI	0.001	0.001*	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Δ Average Construction Wages (t-1)	0.012	0.012	0.010	0.010
	(0.016)	(0.016)	(0.015)	(0.015)
Include Controls from Baseline Model	Yes	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	Yes	Yes	Yes	Yes
Year Fixed Effects	1999–2011	1999–2011	No	No
Year-by-Region Fixed Effects	No	No	1999–2011	1999–2011
Observations	4221	4221	4221	4221
R-squared	0.160	0.160	0.229	0.229

1. Each column represents a unique specification. The dependent variable is the change in the log of real FMR of CBSA *k* at time *t*. Column (1) presents the estimates of the preferred model controlling for initial FMR in 1990 without region-by-year fixed effects. Column (2) present the estimates of the preferred model controlling for initial rent growth without region-by-year fixed effects. Column (3) presents results of the preferred model controlling for initial FMR and region-by-year fixed effects. Column (4) presents estimates of the preferred model controlling for initial rent growth and region-by-year fixed effects. All other controls from the baseline are included; however, the point estimates are omitted for the sake of brevity. Robust standard errors clustered by CBSA are reported in parentheses (***p<0.01, ** p<0.05, * p<0.1).
2. The variables FMR (1990), Rent Growth (1980–90), Per Capita Sales (1992), Per Capita Property Tax Rev (1997),% Housing Stock Built Pre-39 (1990),% Total Earnings from Farms (1990), WRLURI, and the change in Average Construction Wages (t-1) are new additions to the baseline model.

ling for initial city conditions and region-by-year fixed effects. Similar to past studies, the 2SLS estimates suggest that an immigrant inflow equal to 1% of the total population will increase the rental cost index by 1.065%. Columns (3) and (4) show estimates of the preferred specification and, as in Table 4, the point estimate is attenuated when one includes controls for initial city conditions – the point estimate falls by around 50% from the baseline in column (2). When using the CEO index as the measure for rental costs, however, the estimated effect is now highly statistically significant. Overall, the CEO index performs well and seems to fit the data better than FMR. In all specifications, the R-square is higher and the other controls have the expected significant impacts on rent growth, namely changes in per capita income. Moreover, this model performs well in both the first stage Table A4 of the Appendix) *and* in the reduced form (columns 3 and 4 of Table 5). Column (4) of Table 5 presents the reduced form for the preferred model using CEO index in the dependent variable. Compared to the preferred model using FMR (Column 2 of Table 5), the R-square is much larger (0.814 vs. 0.229) and the instrument has a statistically significant effect on the dependent variable.

Another advantage of the CEO index is that it is available starting in 1982. A potential concern with the above results is the relatively short panel (1999–2011). With such a short panel, one may be concerned that there is insufficient temporal variation to identify the coefficient of interest. With a consistent measure of rental costs over time, however, I am able to re-estimate the model using a panel of metropolitan areas from 1985–2011.[20] These results are presented in Table 7, while first stage estimates are presented in Table A5 of the Appendix. When using the longer panel the same pattern emerges. The baseline model estimates an impact of immigration of about one-to-one; however, once I control for initial city conditions, the effect of immigration on rents falls considerably. The results in columns (3) and (4) suggest that an immigrant inflow equal to 1% of the total population will increase rents by 0.3–0.4% – a reduction in the point estimate of about 75%.

To this point, I have ignored an obvious specification to test whether inherent differences between cities are important omitted factors in the

[20] FMR is also available for the earlier periods. However, due to changing definitions throughout the sample period, measurement error may pose a serious concern. From 1980-2012, the definition changes from the 40th percentile to the 45th percentile, then again to the 50th percentile for a subset of metropolitan areas. The CEO index does not have this problem, which makes it better suited for use in a longer panel.

Table 5
Reduced Form Estimates.

VARIABLES	(1) *Baseline* Δr_{ijt}	(2) *Preferred* Δr_{ijt}	(3) *Baseline* ΔCEO_{it}	(4) *Preferred* ΔCEO_{it}
Imputed Immigration Inflow	1.080***	0.134	0.871***	0.424***
	(0.361)	(0.388)	(0.136)	(0.101)
Unemployment Rate (t-1)	−0.122***	−0.139***	−0.068***	−0.053***
	(0.034)	(0.052)	(0.011)	(0.013)
Δ Per Capita Income (t-1)	0.007	0.033	0.053***	0.018**
	(0.031)	(0.032)	(0.008)	(0.007)
% Pop with at least Bachelor's (1990)	−0.007	−0.020**	−0.007*	−0.009***
	(0.009)	(0.010)	(0.004)	(0.003)
Murder Rate (2000)	0.0002	0.0003*	−0.000	0.000
	(0.0002)	(0.0002)	(0.000)	(0.000)
Log Land Area (1990)	0.001	0.001	−0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Log Mean January Temperature	0.009***	0.009***	0.002***	0.001*
	(0.001)	(0.002)	(0.0004)	(0.0004)
Log Mean July Humidity	0.001	0.001	0.002***	0.003***
	(0.001)	(0.002)	(0.0004)	(0.001)
WRLURI		0.001		−0.0003
		(0.001)		(0.0002)
Δ Average Construction Wages (t-1)		0.010		0.008*
		(0.016)		(0.005)
Rent Growth (1980–90)		0.015***		0.004***
		(0.006)		(0.001)
Per Capita Sales (1992)		0.003*		0.000
		(0.002)		(0.000)
Per Capita Prop Tax Rev (1997)		−0.001		−0.000
		(0.001)		(0.000)
% Housing Stock Built Pre-39 (1990)		0.013*		−0.000
		(0.007)		(0.000)
% Total Earnings from Farms (1990)		0.016		−0.000
		(0.016)		(0.000)
Year Fixed Effects	1999–2011	No	1999–2011	No
Year-by-Region Fixed Effects	No	1999–2011	No	1999–2011
Observations	4221	4221	4039	4039
R-squared	0.156	0.229	0.670	0.814

1. Each column presents the reduced form estimates of different specifications. Again, the reduced form is the regression of the dependent variable (rent growth) on the instrument and all other exogenous variables in the model. Columns (1) and (2) present the reduced form estimates when using the changes in log of real FMR as the dependent variable. The reduced form estimates correspond to column (2) of Table 3 and column (4) of Table 4, respectively. Columns (3) and (4) present the reduced form estimates when using the changes in log CEO index as the dependent variable. The reduced form estimates correspond to columns (2) and (4) of Table 5, respectively. Robust standard errors clustered by CBSA are presented in parentheses (${}^{***}p < 0.01$, ${}^{**}p < 0.05$, ${}^{*}p < 0.1$).

model. Given the interpretation of this paper, including CBSA fixed effects seems like a natural extension. There are, however, two problems with this approach. First, as mentioned above, the prior results were estimated from a relatively short panel. Without sufficient temporal variation, it is unlikely such a specification would identify the coefficient of interest. Second and most importantly, CBSA fixed effects cannot be included in the 2SLS specification as identification of β comes from cross-sectional variation, not variation *within* a CBSA. Given the longer panel provided by the CEO index, however, I can include these fixed effects in the baseline OLS specification as a further test of the relative importance of city specific factors. I estimate the baseline OLS model (without vector M_{k,t^*} or region-by-year fixed effects) with CBSA fixed effects and report the estimates in column (5) of Table 7. Comparing the point estimate of the immigration share variable in columns (1) and (5) confirm that omitted city-specific factors are important. When CBSA fixed effects are included to the baseline model, the estimated effect of immigration becomes less statistically significant, falling by roughly half.

5. The role of immigrant location decisions

In the previous section, I show that the effect of immigration on rents is sensitive to the inclusion of initial city economic. The question that remains is why does the past literature estimate such large effects of immigration on rents? Recall that Table 1 provides evidence that high-immigration and low-immigration cities are strikingly different in two key areas: economic prosperity and housing supply conditions. Specifically, high-immigration cities have and continue to have thriving economies that offer more economic opportunities and face relatively inelastic housing supply. In this section, I expand on the role of location choices of immigrants and provide evidence that it is these choices and the failure to adequately account for them in the empirical model that are driving the results in the existing literature.

5.1. Consistency of the shift-share instrument

The previous results suggest that current period rent growth is positively correlated with initial economic conditions in the city. Most notably, initial FMR and initial rent growth have a statistically significant positive impact on current period rent growth. Once one accounts for these characteristics, the impact of immigration on rent decreases by about 75%. One possible explanation for this is that the shift-share instrument introduces bias. Recall, the instrument is defined as:

$$\widehat{Immigrants}_{k,t} = \theta_{k,t^*} * I_{US,t}. \tag{3}$$

Table 6
Immigration and Rents (CEO Index).

VARIABLES	(1) OLS ΔCEO_{it}	(2) 2SLS ΔCEO_{it}	(3) 2SLS ΔCEO_{it}	(4) 2SLS ΔCEO_{it}
$Immigrants_{k,t-1}/Population_{k,t-2}$	1.020***	1.065***	0.616***	0.567***
	(0.153)	(0.171)	(0.121)	(0.123)
Unemployment Rate (t-1)	−0.068***	−0.070***	−0.055***	−0.054***
	(0.011)	(0.011)	(0.013)	(0.013)
Δ Per Capita Income (t-1)	0.057***	0.057***	0.019***	0.020***
	(0.009)	(0.009)	(0.007)	(0.007)
% Pop with at least Bachelor's (1990)	−0.009**	−0.010***	−0.009***	−0.009***
	(0.004)	(0.004)	(0.003)	(0.003)
FMR (1990)			0.001	
			(0.001)	
Rent Growth (1980–90)				0.004***
				(0.001)
Per Capita Sales (1992)			−0.0005	−0.0004
			(0.0007)	(0.001)
Per Capita Prop Tax Rev (1997)			−0.0004	−0.001
			(0.0004)	(0.0003)
% Housing Stock Built Pre-39 (1990)			−0.0005	0.0003
			(0.003)	(0.003)
% Total Earnings from Farms (1990)			−0.009	−0.010*
			(0.006)	(0.006)
WRLURI			−0.0003	−0.0003
			(0.0002)	(0.0002)
Δ Average Construction Wages (t-1)			0.008*	0.008*
			(0.005)	(0.005)
Include Controls from Baseline Model	Yes	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	Yes	Yes	Yes	Yes
Year Fixed Effects	1999–2011	1999–2011	No	No
Year-by-Region Fixed Effects	No	No	1999–2011	1999–2011
Observations	4039	4039	4039	4039
R-squared	0.672	0.672	0.814	0.814

1. Each column represents a unique specification. The dependent variable is the change in the log of the CEO index of CBSA k at time t. Columns (1) and (2) present OLS and 2SLS estimates of the baseline model, respectively. Columns (3) and (4) present estimates of the preferred model using either initial FMR or initial rent growth as a control. Robust standard errors clustered by CBSA are reported in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$). Some point estimates are omitted for the sake of brevity.

As discussed above, this is a "good instrument" in that it performs well in the first stage. However, concern would arise if either θ_{k,t^*} or $I_{US,\,t}$ are correlated with initial economic conditions that were shown to be positively correlated with future rent growth. If so, past estimates relying on the shift-share instrument are biased and inconsistent. To test the exogeneity of the first term, I estimate the determinants of this initial immigrant share via the following model:

$$\theta_{k,t^*} = \alpha X_{k,t} + \pi W_{k,t-1} + \mu \Delta Z_{k,\,t-1} + \delta M_{k,t^*} + \varepsilon_{k,t}. \tag{4}$$

The dependent variable is the share of total immigrants that entered CBSA k at base year t^*, and the independent variables include all exogenous variables from the preferred specification in Eq. (1). I estimate Eq. (4) using several different base years as a robustness check and report the results in Table 8. Panel A includes initial rent levels in 1990 as a control, while Panel B includes initial rent growth. The results confirm the bias introduced by the shift-share instrument. Initial FMR level and initial rent growth are both positively correlated with initial immigrant shares (θ_{k,t^*}), regardless of the choice in base year. Newly-arriving immigrants in t^* were attracted to large, vibrant superstar cities with high rent levels that were predisposed to increased future rent growth. As these initial conditions were shown to have an independent positive impact on future rent growth in Section 4, this suggests that the instrument is, in fact, correlated with the error term. The omission of this relationship explains the large estimates in previous models.[21]

Similarly, the total annual inflow of immigrants to the US ($I_{US,\,t}$) is taken as exogenous in previous studies. However, if one considers immigrant inflows over the past 10 years, it is clear that immigrant inflows are somewhat cyclical. Fig. 3 plots inflows of legally admitted immigrants to the U.S as a percentage of lagged total population from 2003–2012. The data suggest that immigrants do respond to overall economic conditions in the U.S. Legal immigration steadily increased through 2006; however, after the start of the Great Recession around 2008, immigration stagnated and has actually decreased in recent years. This trend is not unique to legal immigrants. Passel et al. (2013) show that, during the Great Recession, the growth of the illegal immigrant population also slowed considerably.

While the broader trend suggests immigrants are responsive to economic conditions in the U.S., this finding, by itself, has few implications for the exogeneity of the instrumental variable. In other words, an overall reduction in immigration would not be problematic for the construction of the instrument so long as the distribution of those who do immigrate is identical to previous years. If the distribution of newly arriving immigrants across the U.S. changes with economic conditions, this would provide further evidence against the exogeneity of the shift-share instrument. To see this, Fig. 4 plots weighted average immigrant inflows as a percent of total population for 1) the ten states most

[21] To further test this, I estimate the baseline model (without the vector M_{k,t^*}) for different subsamples of cities based on initial FMR, initial rent growth (1980-90), and WRLURI. For each of these three measures, I estimate the impact of immigrant inflows on CBSA's that are: 1) above the CBSA average, 2) below the CBSA average, 3) in the top 25% of the CBSA distribution, and 4) in the bottom 25% of the CBSA distribution. The results are presented in Table A6 of the appendix and confirm that the point estimates from the baseline model are driven by inflows into vibrant cities with inelastic housing supply.

Table 7
Immigration Impact on CEO Index, 1985–2011.

VARIABLES	(1) OLS ΔCEO_{it}	(2) 2SLS ΔCEO_{it}	(3) 2SLS ΔCEO_{it}	(4) 2SLS ΔCEO_{it}	(5) OLS ΔCEO_{it}
$Immigrants_{k,t-1}/Population_{k,t-2}$	1.055***	1.261***	0.413***	0.343***	0.523**
	(0.146)	(0.163)	(0.094)	(0.096)	(0.216)
Unemployment Rate (t-1)	−0.061***	−0.066***	−0.029***	−0.027***	−0.108***
	(0.009)	(0.009)	(0.007)	(0.007)	(0.014)
Δ Per Capita Income (t-1)	0.036***	0.037***	0.013**	0.012**	0.028***
	(0.007)	(0.007)	(0.005)	(0.005)	(0.006)
% Pop with at least Bachelor's (1980)	−0.012***	−0.015***	−0.0051**	−0.006**	
	(0.004)	(0.004)	(0.002)	(0.002)	
Rent Growth (1970–80)			0.002*		
			(0.001)		
FMR (1983)				0.002**	
				(0.001)	
Include Controls from Baseline Model	Yes	Yes	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	No	No	Yes	Yes	No
Year Fixed Effects	1985–2011	1985–2011	No	No	1985–2011
Year-by-Region Fixed Effects	No	No	1985–2011	1985–2011	No
CBSA Fixed Effects	No	No	No	No	Yes
Observations	8708	8708	8708	8708	8708
R-squared	0.606	0.605	0.795	0.795	0.645

1. Each column represents a different specification. The dependent variable in each specification is the change in log of the CEO index. The independent variable of interest is the same immigration impact variable as before. The specifications differ based on the inclusion of initial CBSA variables and fixed effects. In this specification, the vector of initial city characteristics are taken from years prior to 1983. Specifically, the excluded variables are: temperature, humidity, WRLURI, violent crime rate in 1981, log land area in 1980, log per capita sales in 1977, log per capita property tax revenue in 1977, the percent of the housing stock built before 1939 in 1980, and the percent of income received from farms in 1980. Due to data limitations, this specification omits the change in construction wages. Robust standard errors clustered by CBSA reported in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).
2. The 2SLS specifications use the same instrument as before; however, the base year of the instrument is now 1983.

Table 8
Determinants of Immigrant Shares in Base Year.

VARIABLES	(1) $\theta_{k,1995}$	(2) $\theta_{k,1994}$	(3) $\theta_{k,1993}$	(4) $\theta_{k,1992}$	(5) $\theta_{k,1991}$	(6) $\theta_{k,1990}$
Panel A						
Initial FMR (1990)	0.012**	0.012**	0.011**	0.009**	0.005**	0.005**
	(0.005)	(0.005)	(0.005)	(0.004)	(0.002)	(0.002)
% Housing Stock Built Pre-39 (1990)	0.020**	0.022**	0.019**	0.017**	0.008**	0.010**
	(0.010)	(0.010)	(0.010)	(0.008)	(0.004)	(0.005)
% Total Earnings from Farms (1990)	−0.012	−0.013	−0.018	−0.013	−0.006	−0.008
	(0.014)	(0.014)	(0.015)	(0.012)	(0.006)	(0.007)
Per Capita Sales (1992)	0.005**	0.005**	0.005**	0.004**	0.002*	0.00230*
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Per Capita Prop Tax Rev (1997)	0.000	−0.000	−0.001	−0.000	−0.000	−0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	4221	4221	4221	4221	4221	4221
R-squared	0.185	0.183	0.185	0.178	0.170	0.169
Panel B						
Rent Growth (1980–90)	0.016**	0.017**	0.017**	0.014**	0.007**	0.008**
	(0.008)	(0.008)	(0.008)	(0.006)	(0.003)	(0.004)
% Housing Stock Built Pre-39 (1990)	0.025**	0.027**	0.025**	0.021**	0.011**	0.012**
	(0.012)	(0.013)	(0.012)	(0.010)	(0.005)	(0.006)
% Total Earnings from Farms (1990)	−0.016	−0.018	−0.022	−0.017	−0.008	−0.010
	(0.014)	(0.015)	(0.016)	(0.013)	(0.006)	(0.008)
Per Capita Sales (1992)	0.006**	0.006**	0.006**	0.005**	0.002**	0.003**
	(0.002)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)
Per Capita Prop Tax Rev (1997)	0.000	−0.000	−0.001	−0.000	−0.000	−0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	4221	4221	4221	4221	4221	4221
R-squared	0.183	0.181	0.184	0.177	0.168	0.169

1. Each column presents the results from a unique specification. All models are estimated using the original 1999–2011 panel. The dependent variable is the immigrant share of the population in CBSA k in base year t^*. For example, the dependent variable in column (1) is the immigrant share of the population in 1995.
2. Each specification includes the full set of exogenous controls from the preferred specification in Eq. (1); however, I omit some point estimates for the sake of brevity. Panel A includes initial FMR as the measure of economic vibrancy, while Panel B includes initial rent growth. Robust standard errors clustered by CBSA are reported in parentheses (***$p <$ 0.01, **$p < 0.05$, *$p < 0.1$).

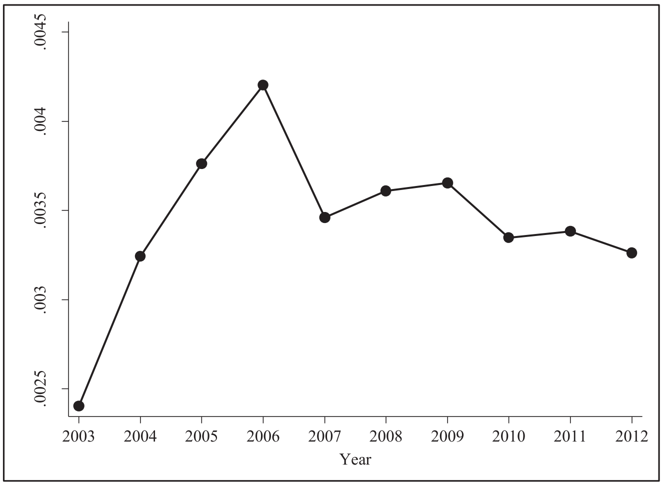

Fig. 3. National Immigrant Inflows as a Share of Total U.S. Population.

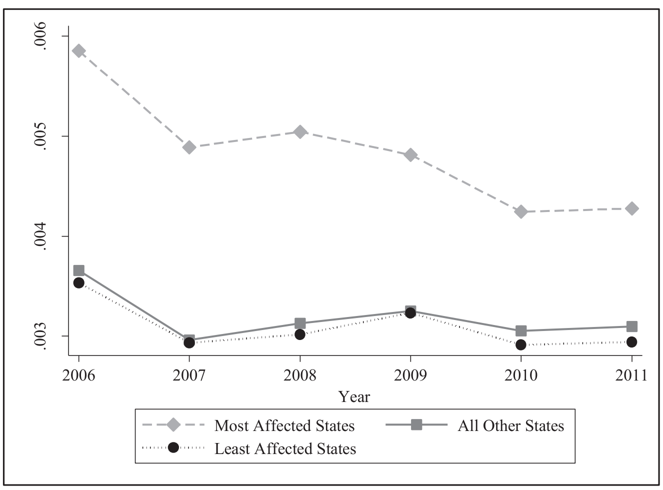

Fig. 4. Average Immigrant Inflows by State ESI Groups.

adversely affected by the Great Recession, 2) the ten states that were least affected by the Great Recession and 3) all other states from 2006–2011. To define these groups, I use the ten states with the highest Economic Security Index (Hacker et al., 2014), which is defined as an integrated measure of insecurity that captures the prevalence of large economic losses among households. Fig. 4 shows that immigrant inflows slowed in states that were most affected by the recession and this decline was much more pronounced than in the other two groups. Perhaps more importantly, California and Nevada are two states included in the group that were most impacted by the recession. As both also have high shares of foreign-born populations (in 2000, California was ranked first and Nevada fifth), the data contradict the theory that the lone determinant of immigrant locations is the existing share of foreign-born populations.

The above analysis suggests that the widely-used shift-share instrumental variable strategy introduces bias as the exclusion restriction fails to hold. Immigrants in the base year located in thriving cities that provided the best economic opportunities. These same cities were predisposed to higher future rent growth. If one believes that the lone determinant of immigrant location choices is the share of existing population that is foreign-born, then new immigrants settle in these same cities in search of the cultural amenities. Without explicitly controlling for the relationship between past location decisions and future rent growth, however, one falsely attribute this increased rent growth to immigration. Moreover, Figs. 3 and 4 suggest immigrants' preferences may be influenced by overall economic climate, not just the share of the population that is foreign-born. As such, a more likely interpretation is that all immigrants, both past and present, choose locations that afford them the best economic opportunities.

5.2. *The role of housing supply*

As seen in the above section, failure to account for past economic conditions leads to biased estimates of β. In this section, I document the role of housing supply elasticity in estimating the effect of immigration on housing prices and rents by analyzing the longer-run impacts of immigration. *A priori*, one may expect the effect of immigration on housing prices to be muted in the medium- and long-run as housing supply is more elastic in the future. If housing supply were perfectly elastic in the long-run, immigration-induced demand shocks should have no effect on rents; however, if long-run housing supply is upward sloping, then the effect on price should be positive (β>0).

I estimate the preferred specification using the full sample (1985–2011) and long changes in both the CEO index and immigrant inflow variables and report the estimates in Table 9 below. From columns (1)–(4), the short-run (1 and 2 year) and medium-run (5 and 10 year) impacts of immigration on rents are remarkably consistent: an immigrant inflow equal to 1% of the total population leads to a roughly 0.4% increase in rents. In column (5), I present the long-run effects of immigration (30-year changes) on rents. Here, the estimated effect of immigration is twice as large as the short-run and medium-run effect. At first glance, this result seems counterintuitive as long-run housing supply is thought to be relatively elastic; however, it has been shown that long-run housing supply is sensitive to geographic location (Saiz, 2010; Rosenthal, 2014).

Saiz (2010) estimates long-run housing supply elasticities at the MSA-level and shows that for a collection of MSA's, long-run housing supply is indeed *inelastic*. Most important for the present paper are the cities that are found to be inelastic (Saiz, 2010; Table VI): Miami, San Francisco, Los Angeles,

New York City, and San Diego, just to name a few. As these MSA's would also be classified as high-immigration in Table 1 and Fig. 1, this suggests that the long-run effect of immigration on housing prices should be positive. Rosenthal (2014) presents similar findings. Annualized real housing price growth was relatively flat for most of the US over the last 3 decades except for the Pacific and New England Census divisions which saw positive growth (roughly 2% per year). Taken together, this suggests that the medium- and long-run effects of immigration should be positive and rent growth in the Pacific and New England divisions should be driving this effect.

To test this, I estimate the model by interacting the immigrant inflow variable with different indicator variables for the location of the CBSA: column (6) interacts the immigration variable with a dummy equal to one if the CBSA is in the Pacific division, column (7) interacts the immigration variable with a dummy equal to one if the CBSA is in the Pacific or New England division, and column (8) interacts the immigration variable with a dummy equal to one if the CBSA is located in California. The results of the interaction specifications confirm the role of location choice in the estimated effect of immigration on rents and show that CBSA's in areas with inelastic housing supply are driving the results in column (5). When the interaction is included, the main effect is less precisely estimated and similar in magnitude to the short-run and medium-run effects; however, the interaction term in all three specifications is significant in terms of both magnitude and statistical significance. Depending on the specification, the results suggest an immigrant inflow equal to 1% of the population in a CBSA located in a region with inelastic long-run housing supply leads to an increase in rents of

Table 9
Impact of Immigration on Rents by Time Horizon.

VARIABLES	(1) 1-year ΔCEO_{it}	(2) 2-year ΔCEO_{it}	(3) 5-year ΔCEO_{it}	(4) 10-year ΔCEO_{it}	(5) 30-year ΔCEO_{it}	(6) 30-year ΔCEO_{it}	(7) 30-year ΔCEO_{it}	(8) 30-year ΔCEO_{it}
Immigrant Inflow Rate (1-year)	0.413***							
	(0.094)							
Immigrant Inflow Rate (2-year)		0.393***						
		(0.093)						
Immigrant Inflow Rate (5-year)			0.363***					
			(0.090)					
Immigrant Inflow Rate (10-year)				0.441***				
				(0.115)				
Immigrant Inflow Rate (30-year)					0.855***	0.451**	0.431*	0.499**
					(0.214)	(0.230)	(0.235)	(0.219)
*Immigrant Inflow Rate (30-year)*Pacific*						1.598***		
						(0.281)		
*Immigrant Inflow Rate (30-year)*Pacific/NE*							1.592***	
							(0.271)	
*Immigrant Inflow Rate (30-year)*California*								1.179***
								(0.423)
Observations	8708	4354	1555	933	311	311	311	311
R-squared	0.795	0.808	0.834	0.818	0.809	0.828	0.829	0.829

1. Each column presents the point estimate of interest from a unique specification that differ with respect to the time horizon of the effect of immigration on rents. In all specifications, the dependent variable is the change in the log of the CEO index. The independent variable of interest, named *Immigrant Inflow Rate*, is defined as: $\sum_{t=1}^{n}(\text{Immigrants}_{k,t-1})/\text{Population}_{k,t-2}$ where n is the length of the time horizon. Columns (1) and (2) analyze short-run impacts via annual changes and 2-year changes, respectively. Columns (3) and (4) analyze medium-run impacts: 5-year and 10-year changes, respectively. Column (5) analyzes long-run impacts via 30-year changes. Columns (6) and (7) show the long-run impacts of immigration when the independent variable of interest is interacted with the Pacific division dummy (column 6), a Pacific or New England division dummy (column 7), and a California dummy (column 8). Robust standard errors clustered by CBSA are reported in parentheses (***p<0.01, ** p<0.05, * p<0.1).
2. Each specification includes the full set of independent variables of the preferred specification. These point estimates are omitted for the sake of brevity but are available upon request. Two slight differences in the variable construction are worth noting. Lagged unemployment rates and changes in lagged per capita income are included as in other specifications in the paper; however, these definitions differ slightly in the above specifications depending on the time horizon of the analysis. For example, when analyzing two-year changes, lagged unemployment rates are the unemployment rates in period *t-2* and changes in per capita income is the one period lagged percentage change in per capita income in period *t-2*.

1.2–1.6% more than CBSA's located in the rest of the US.[22] Thus, as above, location matters. Immigration does have a sizeable impact on rents, but this effect is driven by housing market conditions in the destination city. Once one controls for differences across cities, the effect of immigrant inflows on rents is attenuated.

6. Extension: Immigrant inflows vs. Native inflows

The results in the previous section suggest the true effect of immigration on rents is smaller than the one-to-one impact estimated in prior studies. Like much of the existing literature, however, the analysis above is limited in that it focuses solely on immigrant inflows, ignoring the effects of native population demand shocks on housing rents. In this section, I expand the empirical analysis to explicitly account for native population inflows. This is important for at least two reasons. First, because Eq. (1) analyses immigrant *inflows,* the lack of an additional control for native population *inflows* could mean the model is misspecified. If immigrant inflows and native inflows are correlated yet one omits native inflows from the model, then the estimates of β from Eq. (1) are biased.

Second, by including native population inflows, one can test whether immigrant and native inflows have a different effect on housing rents. Recall that there are notable differences in the estimated effects of immigrants and overall population growth on housing prices and rents. Prior studies consistently estimate relatively large statistically significant effects of immigration on rents, while the effects of total population or employment growth are often small and statistically insignificant. Two potential explanations for this gap were introduced in Section one. First, it could be the case that the differences are driven by the location decisions of immigrants compared to natives. Second, the positive externalities associated with ethnic enclaves may result in newly arriving immigrants having a higher willingness to pay and relatively inelastic demand for living in high-immigrant cities. If so, immigrant inflows could potentially bid up rents in receiving cities above and beyond an inflow of natives (hereafter, the willingness to pay hypothesis). By including native inflows in the model, a test for statistical differences between the coefficients allows one to discern between the two theories. Given that the present model includes explicit controls for differences across cities (both in terms of economic vibrancy and housing supply conditions), statistically different coefficients would lend credence to the willingness to pay hypothesis.

Using data from 1991–2011, I estimate the following model via 2SLS:[23]

$$\Delta\ln(r_{kt}) = \beta\left(\frac{Immgigrants_{k,t-1}}{Population_{k,\ t-2}}\right) + \gamma\left(\frac{Natives_{k,t-1}}{Population_{k,\ t-2}}\right) + \alpha X_{k,t} + \pi W_{k,t-1} + \mu\Delta Z_{k,\ t-1} + \delta M_{k,t*} + \theta_{k,t} + \Delta\varepsilon_{k,t}. \quad (5)$$

The dependent variable is change in the log of the CEO index in CBSA k at time t. The two independent variables of interest are immi-

[22] I also provide estimates of longer changes using Census data on median gross rents and foreign-born populations in Table A7 of the Appendix. The results are qualitatively and quantitatively similar.

[23] I do not use the full sample (1985-2011) due to the nature of the instrumental variable for native population growth (described in detail in the Data Appendix). Because the Bartik shift-share instrument relies on industry-level data, the shift from using SIC codes to NAICS codes is problematic. To construct this instrument, I use data from the Quarterly Census of Employment and Wages of the Bureau of Labor Statistics. These data provide NAICS-classified data starting in 1990. As such, I limit the sample to only include years with NAICS-defined industries.

Table 10
Immigrant Inflows vs. Native Inflows.

VARIABLES	(1) Native *Population* Inflow Rate (IRS) ΔCEO_{it}	(2) Native *Household* Inflow Rate (IRS) ΔCEO_{it}	(3) Domestic Migration Rate (Census) ΔCEO_{it}
Panel A: Results from main specification			
$Immigrant\ Inflows_{k,t-1}/Population_{k,t-2}$ (β)	0.357***	0.357***	0.470***
	(0.087)	(0.087)	(0.088)
$Native\ Inflows_{k,t-1}/Population_{k,t-2}$ (γ)	0.054	0.053	0.197
	(0.059)	(0.059)	(0.232)
Panel B: First-stage estimates			
Imputed Immigrant Inflow	0.421***	0.421***	0.421***
	(0.024)	(0.024)	(0.024)
Oil_{kt}	−0.402***	−0.391***	−0.093
	(0.101)	(0.114)	(0.075)
$Growth_{kt}$	0.558***	0.563**	0.136**
	(0.211)	(0.235)	(0.063)
Panel C: Wald Test: $\beta = \gamma$			
Test Statistic	9.50	9.44	1.44
P-Value	0.002	0.002	0.23
Include Controls from Baseline Model	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	Yes	Yes	Yes
Year-by-Region Fixed Effects	1991–2011	1991–2011	1991–2011

1. Each column represents a unique specification estimated via 2SLS. In all specifications, the dependent variable is the change in the log of the CEO index. The specifications differ with respect to the measurement and calculation of native inflow rates. Columns (1) and (2) presents estimates using the native population inflow rates and native household inflows rates (from the IRS data), respectively. Column (3) presents estimates using the domestic ***net*** migration rate from the US Census. All specifications include the full set of controls specified in Eq. (5); however, the point estimates are omitted for the sake of brevity. Robust standard errors clustered by CBSA are reported in parentheses (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).
2. Panel A presents the estimates of the coefficients of interest. Panel B presents the coefficient estimates on the instruments. Panel C presents the results of the Wald test for equality of coefficients from the main specification.

grant population inflows, $\frac{Immgigrants_{k,t-1}}{Population_{k,\ t-2}}$, and native population inflows, $\frac{Natives_{k,t-1}}{Population_{k,\ t-2}}$. All other variables in (5) are defined analogously to the model specification in Eq. (1).

As in previous sections, immigrant inflows are constructed using data from the INS. To construct native population inflows, I make use of two data sources. First, I calculate CBSA-level native population and native household *inflow* rates from IRS county-to-county migration data. These data report the total number of tax returns filed and the total number of exemptions at the county level. The total number of returns are used as a proxy for the number of households and the total number of exemptions as a measure of the population. A person (or household) is considered an in-migrating native if they moved to a county in the U.S. from an address in any U.S. state. The advantage of using the IRS data is the ability to construct a true *inflow* rate of native persons and households. This is ideal when making direct comparisons to the estimates in the previous section, as the variable of interest throughout the paper is the impact of newly-arriving immigrant *inflows*. The disadvantage of this measure is that only persons who successfully filed tax returns in consecutive years are recorded in the data. As such, this measure surely understates the true native inflow rate into a given CBSA. While I acknowledge this clear limitation, these measures of native inflows should serve as an appropriate proxy for native inflows. For the second measure of native inflows, I construct CBSA-level domestic migration rates using county population estimates reported by the U.S. Census Bureau.[24] While these data have the advantage of being representative of the entire population, not just those who file tax returns, the disadvantage is that these are *net* migration rates. As such, direct comparison to the estimated impact of immigrants in prior tables is less clear.

I estimate Eq. (5) via 2SLS and report the results in Table 10. As before, observed immigrant inflows are instrumented with predicted inflows from the shift-share instrument. Just as immigrant inflows are likely endogenous, so too are native population inflows. As such, I follow the existing literature and use labor demand shifters as instruments for observed native population inflows (Bartik, 1991; Davis et al., 1997; Gallin, 2004).[25] Though common in the labor economics literature as an instrument for local employment growth, this shift-share style instrument is also used in urban settings as an instrument for changes in housing demand (Quigley and Raphael, 2005; Saks, 2008).

Each column in Table 10 represents a unique specification that differ with respect to the construction of the native population inflow rate: native *population* inflow rate from the IRS data, native *household* inflow rate from the IRS data, and domestic migration rate from the Census data. Panel A presents the coefficient estimates of the variables of interest and Panel B presents the first-stage coefficients for the instruments. The estimated impact of immigrant inflows is statistically significant and similar in magnitude to the estimates in the prior section, providing further evidence that the impact of immigration on rents is significantly less than previous estimates. The estimated impact of native inflows, however, is less precise. For all three measures of native inflow rates, the estimated impact is not statistically significant and very close to zero. Recall that while there is a consensus that immigration has a statistically significant positive effect on the housing market, the effect of native population is notoriously difficult to uncover. The results in Table 10 are consistent with these findings.

While the effect of native inflows on rents is imprecisely estimated, a more relevant comparison for this paper is the difference between the estimated effect of native inflows and immigrant inflows (Panel C of Table 10). In the preferred specification where native inflows are mea-

[24] Components of Population Change from 1980 to 1990, 1990 to 2000, and 2000 to 2010.

[25] For a complete discussion on the instruments and how they were calculated, please refer to the data appendix.

sured using IRS data (columns 1 and 2), the differences in the estimated effects are shown to be highly statistically significant at the 1% level. When using the domestic migration rate as the measure for native inflows (column 3), the difference is not statistically significant. This result, however, seems to be driven, at least in part, by a weak first stage (Panel B, Column 3).

So what can one conclude from the results in Table 10? On one hand, it cannot be overlooked that the main result survives – the effect of immigrant inflows on rents is significantly lower than previously estimated in the literature – and this result is robust when 1) considering a different sample period and 2) native inflows are included in the model. On the other hand, even with a properly specified model that accounts for the endogenous sorting of immigrants in thriving cities, there is a statistically significant difference between the effect of immigrant inflows and the effect of native inflows on rents. This suggests that differences in location choices, while important, cannot explain the entire gap in the literature.

Overall, the differences in the estimated impacts is largely unsurprising. The added control variables to the model (M_{k,t^*}) are included to control for CBSA-specific differences in economic vibrancy and housing supply conditions. The model cannot, however, control for differences in individual-specific preferences and willingness to pay. Moreover, the focus on the rental housing market likely exacerbates this issue, as immigrants are more likely to rent compared to natives. In 2017, 49% of immigrant households were renters compared to just 32% for native households.[26] As such, given an equal-sized inflow of immigrants and natives to a city (say, equal to 1% of the total population), the immigrant-induced shock to rental housing demand would be larger relative to the native-induced shock, even when differences in economic and housing market conditions are held constant.

7. Conclusion

While one would expect a one-time population shift to increase housing prices and rents, specification error in previous models makes causal inference difficult. In this paper, I show that prior estimates of the impact of immigration on housing rents are biased upward due to a lack of controls for city-specific characteristics that 1) attract immigrants and 2) predispose these cities to higher rent growth. With a properly specified model, the effect of immigration is significantly attenuated. Findings suggest that an immigrant inflow equal to 1% of the total population leads to a 0.3–0.4% increase in housing rents. The magnitude of this result is robust to two measures of rent, several different sample periods, and different definitions of the initial economic conditions of cities. Although point estimates are imprecisely estimated when using FMR as the measure for rental costs, estimates using the CEO index make clear that the true impact of immigration on rents is significantly less than the one-for-one impact reported in the existing literature. Rent growth is larger in high-immigration cities relative to low-immigration cities; however, this relationship is driven by relative differences in economic and housing conditions.

An analysis of medium- and long-run impacts of immigration provides support for this interpretation and confirm the role of housing supply elasticity in estimating the effects of immigration on housing rents. The medium-run effect is similar to short-run estimates. The long-run effect, however, differs by region of residence. When the effect of immigrant inflows are allowed to vary by region in the interaction specification, the main effect was again around 0.4%; however, the effect of immigration in regions known to have long-run inelastic housing supply are 1.2–1.6% higher. Taken together, these results further suggest that differences between high- and low-immigration cities are driving the relationship found in the literature.

While the paper provides evidence that estimates in the existing literature were biased upward due to spurious correlation, an extension of the main results suggests that immigrant and native inflows do have statistically different effects on the rental housing market. However, it cannot be overlooked that, similar to prior studies cited above, the estimated effect of native inflows on rents is not statistically nor economically significant. Given that the extension reports short-run estimates, this result is surprising and remains a puzzle that must be addressed before a definitive conclusion can be drawn regarding the relative effects of immigrants and natives.

Overall, while this paper provides evidence that the common result in the literature is biased upward due to spurious correlation, it would be incorrect to assert that the findings of this paper recover the *true* effect of immigration on rents. Given the direction of the bias in previous studies, however, the results herein likely represent an upper bound of the true effect. Still, the implications of the results are far-reaching as the source of this bias is shown to be the oft-used shift-share instrumental variable strategy. Recall, the main identifying assumption of the shift-share instrument is that immigrant inflows in the base year are not driven by omitted variables that are correlated with future rent growth. However, the positive correlation between the initial economic conditions and immigrant location choices in the base year suggests that past immigrants were also attracted to large, growing cities with relatively inelastic housing supply. Without proper controls, this instrumental variable estimation strategy fails to identify a causal effect of immigration. In addition, the common result in migration literature is that the main determinant of immigrant settlement decisions is the fraction of the existing population that is foreign-born. It has been shown here that both past and present immigrants are attracted to cities with thriving economies with growing wages, housing prices, and rents. Thus, a more likely interpretation is that immigrants, like their native counterparts, choose locations that afford them the best economic opportunities.

[26] Author calculations from the 5-year estimates of the 2017 American Community Survey.

Appendix

Table A1
Added Variables Specification.

VARIABLES	(1) Δr_{ijt}	(2) Δr_{ijt}	(3) Δr_{ijt}	(4) Δr_{ijt}	(5) Δr_{ijt}	(6) Δr_{ijt}	(7) Δr_{ijt}	(8) Δr_{ijt}	(9) Δr_{ijt}
$\text{Immigrants}_{k,\ t\text{-}1}/\text{Population}_{k,\ t\text{-}2}$	1.316***	1.246***	1.209***	1.192***	1.246***	0.569	0.679	0.425	0.403
	(0.428)	(0.458)	(0.469)	(0.426)	(0.425)	(0.478)	(0.466)	(0.443)	(0.450)
Per Capita Prop Tax Rev (1997)		0.001							
		(0.001)							
Per Capita Sales (1992)			0.002						
			(0.002)						
% Housing Stock Built Pre-39 (1990)				0.014***					
				(0.005)					
% Total Earnings from Farms (1990)					0.015				
					(0.015)				
Initial FMR (1990)						0.014***		0.010***	
						(0.003)		(0.003)	
Rent Growth (1980–90)							0.020***		0.014**
							(0.005)		(0.006)
Include Controls from Baseline Model	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Housing Supply Conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Region Fixed Effects	No	No	No	No	No	No	No	1999–2011	1999–2011
Observations	4221	4221	4221	4221	4221	4221	4221	4221	4221
R-squared	0.158	0.158	0.158	0.158	0.158	0.160	0.159	0.228	0.228

1. Each column represents a different specification of the 2SLS model using the sample 1999–2011. For all columns, the dependent variable is the change in the log of real FMR. Column (1) is the baseline model and the results are the same as in Table 3. Subsequent columns add one control variable from the vector of initial conditions ($M_{k,\ t^*}$). For example, column (2) is the baseline model with the property tax revenue variable added; column (3) is the baseline model with just the per capita sales variable added; and so on. Robust standard errors clustered by CBSA are reported in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A2
Alternate Proxies for Initial Economic Conditions.

VARIABLES	(1) FMR Growth Δr_{ijt}	(2) Med Gross Rent Δr_{ijt}	(3) Commute Δr_{ijt}	(4) Price/Rent Δr_{ijt}
$\text{Immigrants}_{k,\ t\text{-}1}/\text{Population}_{k,\ t\text{-}2}$	0.486	0.037	0.074	0.236
	(0.455)	(0.530)	(0.500)	(0.522)
FMR Growth (1983–90)	0.001			
	(0.006)			
Initial Median Gross Rent (1990)		0.015**		
		(0.006)		
Average Commute (1990)			0.001***	
			(0.0002)	
Price-to-Rent Ratio (1990)				0.005*
				(0.003)
Include Controls from Baseline Model	Yes	Yes	Yes	Yes
Other Initial CBSA Variables (M_{k,t^*})	Yes	Yes	Yes	Yes
Year-by-Region Fixed Effects	1991–2011	1991–2011	1991–2011	1991–2011
Observations	4221	4221	4221	4221
R-squared	0.228	0.229	0.229	0.228

1. Each column represents a different specification of the 2SLS model using the sample 1999–2011. For all columns, the dependent variable is the change in the log of real FMR. Each specification includes the full set of exogenous controls and region-by-year fixed effects with one exception. The lone difference in the above specifications and the preferred specification is that I replace initial FMR and initial rent growth with an alternate definition of economic vibrancy. Column (1) presents the full model with FMR growth from 1983–1990 as the measure of economic vibrancy. Column (2) presents the full model with Initial Median Gross Rent in 1990 as the measure of economic vibrancy. Column (3) presents the full model with Average Commute time (in minutes) in 1990 as the measure of economic vibrancy. Column (4) presents the full model Price-to-Rent Ratio in 1990 as the measure of economic vibrancy. While other exogenous controls are included, their point estimates are omitted for the sake of brevity. Robust standard errors clustered by CBSA are reported in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A3
First Stage Regressions.

VARIABLES	(1) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$	(2) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$	(3) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$
Imputed Immigration Inflow	0.781***	0.708***	0.699***
	(0.027)	(0.028)	(0.029)
F-Stat	178.79	63.85	64.92
Prob > *F*	0.0000	0.0000	0.0000
Include Controls from Baseline Model	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	No	Yes	Yes
Year Fixed Effects	1999–2011	No	No
Year-by-Region Fixed Effects	No	1999–2011	1999–2011
Observations	3831	3831	3831
R-squared	0.715	0.763	0.763

1. Each column presents the first stage estimates of the shift-share instrument. In all cases, the shift-share instrument is calculated using 1995 as the base year. Column (1) is the baseline specification in column (2) of Table 3. Columns (2) and (3) are the first stage estimates for columns (3) and (4) of Table 4, respectively. Robust standard errors clustered by CBSA are reported in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A4
First Stage, CEO Index.

VARIABLES	(1) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$	(2) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$	(3) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$
Imputed Immigration Inflow	0.818***	0.752***	0.748***
	(0.023)	(0.026)	(0.027)
F-Stat	269.40	87.74	88.89
Prob > *F*	0.0000	0.0000	0.0000
Include Controls from Baseline Model	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	No	Yes	Yes
Year Fixed Effects	1999–2011	No	No
Year-by-Region Fixed Effects	No	1999–2011	1999–2011
Observations	4039	4039	4039
R-squared	0.782	0.822	0.822

1. Each column presents the first stage estimates of the shift-share instrument for Table 6. In all cases, the shift-share instrument is calculated using 1995 as the base year. Column (1) corresponds to the baseline specification in column (2) of Table 6. Columns (2) and (3) are the first stage estimates for columns (3) and (4) of Table 6, respectively. Robust standard errors clustered by CBSA are presented in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A5
First Stage, CEO Index, Extended Panel.

VARIABLES	(1) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$	(2) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$	(3) $\frac{Immigrants_{k,t-1}}{Population_{k,t-2}}$
Imputed Immigration Inflow	0.450***	0.435***	0.430***
	(0.025)	(0.027)	(0.025)
F-Stat	175.69	672.35	622.75
Prob > *F*	0.000	0.000	0.000
Include Controls from Baseline Model	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	No	Yes	Yes
Year Fixed Effects	1985–2011	No	No
Year-by-Region Fixed Effects	No	1985–2011	1985–2011
Observations	8708	8708	8708
R-squared	0.749	0.795	0.797

1. Each column presents the first stage estimates of the shift-share instrument for Table 7. In all cases, the shift-share instrument is calculated using 1983 as the base year. Column (1) corresponds to the baseline specification in column (2) of Table 7. Columns (2) and (3) are the first stage estimates for columns (2) and (3) of Table 7, respectively. Robust standard errors clustered by CBSA are presented in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$).

Table A6
Immigration Impact by CBSA subsample.

VARIABLES	(1) *Full Sample* ΔCEO_{it}	(2) *CBSA's Above Average* ΔCEO_{it}	(3) *CBSA's Below Average* ΔCEO_{it}	(4) *CBSA's in top 25% of distribution* ΔCEO_{it}	(5) *CBSA's in bottom 25% of distribution* ΔCEO_{it}
Panel A – FMR in 1990					
$Immigrants_{k,\ t\text{-}1}/Population_{k,\ t\text{-}2}$	1.020***	1.330***	0.199	1.061***	0.101
	(0.153)	(0.235)	(0.180)	(0.245)	(0.126)
Observations	4039	1920	2119	1036	1001
R-squared	0.672	0.644	0.768	0.653	0.804
Panel B – Rent Growth (1980–90)					
$Immigrants_{k,\ t\text{-}1}/Population_{k,\ t\text{-}2}$	1.020***	1.190***	0.220	1.212***	0.386
	(0.153)	(0.196)	(0.226)	(0.230)	(0.239)
Observations	4039	1961	2078	1027	986
R-squared	0.672	0.651	0.722	0.633	0.711
Panel C – WRLURI					
$Immigrants_{k,\ t\text{-}1}/Population_{k,\ t\text{-}2}$	1.020***	1.177***	0.558	1.243***	0.006
	(0.153)	(0.222)	(0.357)	(0.298)	(0.386)
Observations	4039	1842	2197	971	1001
R-squared	0.672	0.641	0.760	0.627	0.812

1. Each column represents a different specification of the baseline 2SLS model. The baseline model does not control for initial city conditions, region-by-year fixed effects, or the other housing supply measures. For all columns, the dependent variable is the change in the log of the CEO index. Column (1) is the baseline model and the results are the same as in Table 6. Columns (2) – (5) are estimates of the baseline model for different subsets of CBSA's. Column (2) estimates the effects of immigration on rents in CBSA's above the national average of FMR in 1990 (Panel A), Initial Rent Growth from 1980–90 (Panel B), and WRLURI (Panel C). Columns (3) – (5) are structured similarly. Column (3) limits the sample to CBSA's below the national average, column (4) limits the sample to CBSA's in the top 25% of the distribution, and column (5) limits the sample to the CBSA's in the bottom 25% of the distribution.
2. In Panel C, above average WRLURI and the top 25% of the WRLURI distribution reflect metropolitan areas with more inelastic housing supply.
3. The point estimates of the other explanatory variables are omitted for the sake of brevity. Robust standard errors clustered by CBSA are reported in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Table A7
Immigration Impact over Time, Census Data.

VARIABLES	(1) 10-year Preferred Δr_{ijt}	(2) 10-year Preferred Δr_{ijt}	(3) 30-year Preferred Δr_{ijt}	(4) 30-year Preferred Δr_{ijt}
Immigrant Inflow Rate	0.258**	0.061		
	(0.115)	(0.117)		
*Immigrant Inflow Rate*Pacific/NE*		0.780***		
		(0.188)		
Immigrant Inflow Rate			0.196**	0.061
			(0.0796)	(0.056)
*Immigrant Inflow Rate*Pacific/NE*				0.731***
				(0.181)
Include Controls from Baseline Model	Yes	Yes	Yes	Yes
Initial CBSA Variables (M_{k,t^*})	Yes	Yes	Yes	Yes
Decade-by-Region Fixed Effects	1980–2010	1980–2010	1980–2010	1980–2010
Observations	1126	1126	374	374
R-squared	0.595	0.607	0.559	0.590

1. Each column presents the point estimate of interest from a unique specification that differ with respect to the time horizon of the effect of immigration on rents. Here, the dependent variable is the change in the log real median rent. The independent variable of interest, named *Immigrant Inflow Rate*, is defined as: $\sum_{t=1}^{n}(Immigrants_{k,t-1})/Population_{k,t-2}$ where n is the length of the time horizon. Columns (1) and (2) analyze 10-year changes while columns (3) and (4) analyze 30-year changes.
2. All specifications include the full set of controls from the preferred specification. Point estimates from other controls in the model are omitted for the sake of brevity. Robust standard errors clustered by CBSA are reported in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$).

Data appendix

Variable Descriptions and Sources		
Variable	Description	Table
FMR	The FMR is reported at the county-level by the HUD. The CBSA-level data are population-weighted averages of the corresponding county data. Prior to aggregating to the CBSA-level, all county-level data are adjusted (as described in section 2.3) to 40% FMR estimates.	T3-T5, A1-A3
CEO Index	Interarea rental cost panel from Carrillo et al., 2014	T5 – T7, T9, T10, A4-A6
Immigrants (1983–2011)	Customized data from the Department of Homeland Security (1999–2011) and from the INS Immigrants Admitted to the US dataset (1983–1998). These data were aggregated to 2013 CBSA definitions.	T1-T10, A1-A7
Native Population Flows	IRS County-to-County migration flows; US Census Components of Population Change.	T10
Per Capita Personal Income	County-level data from the Bureau of Economic Analysis' (BEA) Regional Economic Information System (REIS).	T3-T10, A1-A7
Unemployment Rate	County-level employment data from the Bureau of Labor Statistics (BLS) aggregated to 2013 CBSA definitions.	T3-T10, A1-A7
January Average Temperature	The average temperature (measured in Fahrenheit degrees) over the years 1941–1970. From the United States Department of Agriculture (USDA) Economic Research Service (ERS) Natural Amenities Scale Database. County-level data is aggregated to CBSA.	T3-T10, A1-A7
July Average Humidity	The average relative humidity over the years 1941–1970. From the United States Department of Agriculture (USDA) Economic Research Service (ERS) Natural Amenities Scale Database. County-level data is aggregated to CBSA.	T3-T10, A1-A7
CBSA Land Area	County-level data derived from the US Census Bureau Censtats database, aggregated to 2013 CBSA definitions.	T3-T10, A1-A7
% of population with a Bachelor's degree	County-level data derived from the US Census Bureau Censtats database, aggregated to 2013 CBSA definitions.	T3-T10, A1-A7
Murder Rate (2000)	County-level murder statistics from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) database. As certain states do not report to the FBI (i.e. Florida, Illinois, etc.), these data are obtained from state run databases.	T3-T6, T8, A1-A3, A6
Violent Crime Rate (1981)	County-level data from the National Archive of Criminal Justice Data (NACJD). Aggregated to 2013 CBSA definitions.	T6, T8, T9, A4, A5, A7
Initial Rent Growth	Constructed using county-level median gross rent data from the U.S. Census. I calculate weighted average median gross rents for each CBSA, where weights are the number of rental-occupied housing units.	T4-T10, A1, A3-A5, A7
% of Housing Stock Built Pre-1939	County-level data from the 1994 County and City Data Book	T4-T10, A1-A5, A7
% of Total Earnings from Farms	County-level data from the 1994 County and City Data Book. The ratio of earnings from farms to the total earnings.	T4-T9, A1-A5, A7
Per Capita Sales	This is per capita sales in private retail and service establishments. County-level data obtained from the 1992 Economic Census.	T4-T9, A1-A5, A7
Per Capita Property Tax Revenue	County-level data from the 2000 County and City Data Book. Use the variables total tax revenue and percent of total revenue from property taxes to construct this variable.	T4-T9, A1-A5, A7
Price-to-Rent Ratio	Constructed from county level census data. Calculate weighted average house values and rents, where the weights are owner-occupied units and renter-occupied, respectively	A2
WRLURI	The Wharton Residential Land Use Regulatory Index. This index is given for a Census-defined place. I then construct CBSA-level estimates as population-weighted averages of each place.	T1, T4-T10, A1-A5, A7
Change in Average Construction Wages	Constructed from county-level wage data from the QCEW. All employment in wages in NAICS industry 23.	T4 – T6, T8, A1 – A3
Labor Demand Shifters (OIL and GROWTH)	Described in detail below.	T10
FMHPI	Freddie Mac Housing Price Index.	Available Upon Request
HPI	FHFA Housing Price Index	Available Upon Request

Normalization of fair market rent series

As discussed in Data Section, the definition of what constitutes a fair market rent (FMR) has changed over time. For most counties in the sample, the FMR is the price of this unit at the 40th percentile of the rent distribution; however, starting in 2005, the FMR for a small sample of counties are reported as the 50th percentile of the rent distribution. Thus, I normalize the FMR series throughout the sample by adjusting 50th percentile estimates to 40th percentile estimates. Formally, I use the following procedure.

1. For each CBSA k, I use the observed 40th percentile FMR data for years prior to 2005 and linearly extrapolate to recover the predicted 40th percentile estimate in 2005 ($\widehat{FMR}_{k,\ 40\%,2005}$). I then take the ratio of actual 50th percentile estimate in 2005 to the predicted 40th percentile estimate in 2005. This ratio is defined as: $r_{k,t} = (\frac{FMR_{k,\ 50\%,\ 2005}}{\widehat{FMR}_{k,\ 40\%,\ 2005}})$.
2. For each CBSA k, I use the 50th percentile FMR data for the subsequent years to linearly extrapolate the 50th percentile rent estimate in 2004 ($\widehat{FMR}_{k,50\%,\ 2004}$). I then take the ratio of predicted 50th percentile estimate in 2004 to the actual 40th percentile estimate in 2004. This ratio is defined as: $r_{k,t} = (\frac{\widehat{FMR}_{k,\ 50\%,\ 2005}}{FMR_{k,\ 40\%,\ 2005}})$.
3. Next, I construct an adjustment factor (A_k) equal to the average of the previous ratios in steps 1 and 2.
4. Finally, I use the adjustment factor to deflate 50% FMR estimates to reflect 40% FMR estimates: $\widehat{FMR}_{40} = \frac{1}{A_k} * FMR_{50}$.[27]

[27] In 1995, HUD began to report FMR as a 40% estimate. Thus, Saiz (2007) had to adjust FMR to reflect 45% rent estimates for the years 1996-1998. The difference, however, is that both 40th and 45th percentile estimates were reported in 1995 and the ratio of these two estimates were used to adjust 45th percentile FMRs to 40th percentile FMRs. While the methodology in the present paper may seem like a crude treatment of the data, the results are not sensitive to this adjustment. Results using unadjusted FMR as the dependent variable are available upon request.

Calculation of oil and growth instruments

To instrument for observed native population flows, I follow the existing literature by using a Bartik-style shift share instrument for labor demand shocks (Bartik, 1991). Although this empirical strategy is common in studies analyzing local employment growth, these labor demand shifters have also been used in studies of the housing market as instruments for changes in housing demand (Quigley and Raphael, 2005; Saks, 2008).

The intuition of this instrumental variable strategy is that if a given industry is growing at the national level, then one can expect CBSA's with large shares of employment in said industry to experience increased employment growth as well. As such, the traditional Bartik-style instrument uses the industrial mix of a CBSA and national industry-level employment growth to construct CBSA-specific predicted employment growth rates. The identifying assumption is that while actual CBSA-level employment growth is likely correlated with local conditions, a national shock to employment levels is likely exogenous with regards to these local conditions.

To construct the instruments, I use county-level annual files from the Quarterly Census of Employment and Wages (QCEW) of the Bureau of Labor Statistics (BLS) from 1989–2012. The county-level files that report average employment for each industry in each county in the US. These county-level data are then aggregated to 2013 CBSA definitions. Following Davis et al. (1997) and Gallin (2004), I assume that national-level employment growth in each industry is driven by two sources: oil price shocks and "everything else".

The analysis focuses on 10 broadly-defined industries: Mining; Government, Construction; Primary Metals; Services; Motor Vehicles; Finance, Insurance, and Real Estate (FIRE); Other Manufacturing; Trade; and Transportation, Communications, and Public Utilities. Then, for each individual industry (i), I estimate:

$$E_{it} = \alpha_0 + \alpha_1 OIL_t + \alpha_2 OIL_{t-1} + \varepsilon_{it},$$

where E_{it} is the employment growth rate in industry i at time t, OIL_t is the growth rate of the producer price index (PPI) for crude oil relative to the PPI for all finished products at time t. PPI data on crude oil and all finished products comes from the Federal Reserve Economic Data (FRED) of the St. Louis Fed.

To construct the instruments, I calculate the weighted average of each industry's employment response to oil shocks and everything else, using the industry employment share for each CBSA (s_{ikt}) as weights. Specifically, the instruments for each CBSA (k) at time t are:

$$Oil_{kt} = \sum_i \left(\alpha_1 OIL_t + \alpha_2 OIL_{t-1}\right) s_{ikt}$$

$$Growth_{kt} = \sum_i \varepsilon_{it} s_{ikt}$$

To construct the instruments for native population inflows used in estimating Eq. (5), I then demean OIL_{kt} and $GROWTH_{kt}$ from their CBSA and year means. To ensure that oil shocks have the appropriate sign in the first stage, I follow Gallin (2004) and use the negative of the demeaned Oil_{kt}.

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