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Essays in Urban and Labor Economics

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ABSTRACT OF THE DISSERTATION

Essays in Urban and Labor Economics

by

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This dissertation explores topics in urban and labor economics, separately and together.

In Chapter 1, I explore human capital spillovers between workers and across neighborhoods. I first show that college-educated and non-college workers tend to work in the same Census tracts. I then estimate an economic geography model and find a positive spillover effect of nearby college workers but a negative spillover effect of nearby non-college workers on a worker's income. Both spillover effects decay very quickly, having no effect beyond three miles. I conduct counterfactual exercises to assess the benefits of a Los Angeles policy. The exercise shows that policies increasing density of college jobs provide benefit to both the targeted and surrounding areas, suggesting an important margin for urban policymakers to influence worker productivity in local areas.

Chapter 2 studies the Civilian Conservation Corps (CCC) to provide the first comprehensive assessment of the short- and long-term effects of means-tested youth employment programs. We use digitized enrollee records from the CCC program in Colorado and New Mexico and matched these records to the 1940 Census, WWII enlistment records, Social Security Administration records, and death certificates. Overall, we find significant long-

term benefits in both longevity and earnings, suggesting short and medium-term evaluations underestimate the returns of training programs, as do those that fail to consider effects on longevity.

Chapter 3 examines the housing market effects of inclusionary zoning policies (IZPs). IZPs have been implemented to spur construction of below-market housing to tackle the issue of housing affordability. They are popular with local governments because the direct costs of creating affordable housing are borne by developers, but their effects are theoretically ambiguous. I implement an empirical strategy that exploits variation in geography and timing in a difference-in-discontinuity framework to examine the effect of New York City's mandatory IZP on housing supply and prices. I find that, while transaction prices increase following the policy, building activity also increases. This suggests that models considering IZP as a tax on development are too simplistic and further research is required to disentangle these findings.

The dissertation of Keyoung David Lee is approved.

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To my family, both the one I started with

and the one I gained along this journey;

as a scholar I stood on the shoulders of giants,

but as a man I leaned on you.

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CHAPTER 1

Benefits of Working Near College Workers: Human Capital Spillovers Across Neighborhoods

1.1 Introduction

When Los Angeles opened new metro stations in 2016, the city council proposed a new zoning scheme to almost triple the allowable density near some of its stations and make the new developments more suitable for types of jobs that involve high-skill workers. Across the nation in recent years, there has been a newfound focus of firms and urban policymakers on dense, high human capital developments (Katz and Wagner 2014). Google, once a firm well-known for its large suburban campus Googleplex, is opening new offices in London, expanding in Manhattan, and taking up space in Los Angeles' Westside Pavillion Mall. Florida and Mellander (2014) document that venture capital firms have been flocking to urban centers with high human capital. Even in the suburbs, developments are favoring dense, transit-oriented neighborhoods. Meanwhile, some firms have transformed areas nearby by attracting more jobs and workers close to them. For example, Quicken Loans in Detroit and Amazon in Seattle have transformed their respective cities' downtowns.

What drives firms and workers to locate in dense employment clusters and co-locate in high-density, high human capital areas? This chapter examines neighborhood-level human capital spillovers of density as a possible reason. For example, do workers benefit from high density of high-skill workplaces nearby? Are there similar benefits from nearby low-skill workplaces? And what is the spatial extent of these benefits? These are the questions that

I answer in this chapter.[1]

To examine these questions, I proceed in three steps. First, I document that people of different education levels work in geographical proximity using LEHD[2] Origin Destination Employment Statistics (LODES) data in 2010. I provide new stylized facts that show how college and non-college jobs[3] are co-located within American cities: 1) college and non-college workplaces are co-located unlike college and non-college residences, 2) college and non-college workplaces are both very concentrated, but college workplaces more so, and 3) denser work locations tend to have higher college share of workers. These stylized facts provide preliminary evidence on neighborhood-level agglomeration economies within and between skill levels.

With these stylized facts in mind, I investigate whether workers who work in areas with higher nearby college or non-college worker density earn more using individual-level data with income and detailed workplace geography. I find descriptive evidence that there is a positive association of nearby college employment density on income, while there are negative effects of nearby non-college employment density. Moreover, both of these effects decay very quickly, becoming statistically insignificant beyond 3-5 miles. Quantitatively, a 1% increase in college workers 0-3 miles away is associated with an increase in wages by 0.075%. The same increase 3-5 miles away is only associated with a wage increase of 0.026%. In contrast, a 1% increase in non-college workers 0-3 miles away is correlated with a 0.086% decrease in worker's wages, while for 3-5 miles away, with a 0.035% decrease. These effects are similar whether the worker herself is college educated or not.

Identifying causal effects of nearby worker density is challenging given the limitations of

[1]Throughout the paper I use the term "neighborhood" and "local-level" to mean effects across Census tracts. Census tracts are on average 15 square miles in urban areas and contain 600 to 3,000 residents.

[2]Longitudinal Establishment-Household Dynamics.

[3]I use the terms college jobs and college workplaces to denote jobs held by workers with a bachelor's degree at a location. Similarly, I use the terms non-college jobs and non-college workplaces as jobs or workplaces held by workers without a bachelor's degree.

the individual-level dataset. In particular, I only observe individuals in California in the cross-section at 2010. Selection of productive individuals into productive tracts is one source of endogeneity. Another source is unobservable tract-level characteristics that are spatially correlated, such as a train station that increases the productivity of all nearby tracts.

To identify the spillovers, I extend an economic geography model and solution methods developed by Ahlfeldt et al. (2015) to include college and non-college workers in both production and density spillovers across space. In the model, workers make a workplace-residence pair location decision given idiosyncratic Frechét preference draws on each pair. This Frechét distribution assumption on the preference shock allows me to recover the wages that should be paid at each workplace location to justify the observed distribution of workers across workplaces and residences in the data. In addition, the model assumes a representative firm in each location with spillovers from nearby college and non-college workers enhancing its productivity.

The model assumptions allow me to use sparser data to estimate the parameters of interest. I use information on workplace counts, residence counts, bilateral distance, land area, and floor price, which I can obtain for multiple years and for multiple cities in the U.S., to form estimating equations on differences over 2010-2015. The estimation differences out time-invariant tract-level unobservables as well as city-level fixed effects that affect all tracts within a city.

The parameters of interest in the model are the strength and sign of the spillover effects and how fast they decay over distance. In order to estimate the parameters, I need instruments that are uncorrelated with tract-level changes in fundamental productivity but correlated with the distribution of workers across the city. I use Bartik instruments that are constructed using past industry composition in each zip code and industry-level human capital growth outside each city. I also use historical residential segregation. Using these instruments and the structural residuals recovered from the model, I estimate the parameters using non-linear GMM.

The estimates are qualitatively similar to what I find in the descriptive OLS regressions: nearby college workers have a positive spillover effect while nearby non-college workers have a negative spillover effect. The estimated decay of these spillover effects are faster than in my descriptive results, decaying to half by only around 0.75 mile for nearby college workers and 0.6 miles for nearby non-college workers. These estimates suggest that the spillover effects of human capital are again very localized. This is in line with the literature estimating other types of agglomeration at the local level. For example, Ahlfeldt et al. (2015) estimates that local productivity spillovers from nearby total employment density decay to about 0 after 15 minutes of travel time.

Magnitudes of the spillover are on the lower end compared to estimates of human capital spillovers on the whole city. For example Moretti (2004c) estimates that a one percentage point increase in college share in the city leads to about 1.9% increase in wages of high-school drop-outs, 1.6% increase for high-school graduates, and 0.4% for college graduates. At the mean value of college share across tracts (1/3) and assuming that the one percentage point increase in college share is a result of an equal increase in college and decrease in non-college workers, my estimates imply an increase of roughly 0.4% in wages following a one percentage point increase in the college share of tracts 0-3 miles away. However, they are larger than another study of more local-level, human capital spillovers. Fu (2007) estimates that in the Boston metropolitan area, a one percentage point increase in college share 0-1.5 mile away is associated with a 0.14% increase in wages, and 1.5-3 miles away associated with a 0.1% increase in wages.

Finally, I use the model estimates to examine the impact of Exposition Corridor Transit Neighborhood Plan in Los Angeles, a proposed policy to re-zone areas near the city's new light rail stations. The plan seeks to add density by increasing the floor area ratio (FAR), which determines how tall you can build, and changes the land use from light industrial to hybrid industrial, aiming to convert warehouses into office spaces to attract high-productivity jobs. I find that the combination of increased density and re-purposing the area to attract college jobs have a significant spillover effect that not only benefit the targeted area itself, but also the neighbors of the targeted area. These findings have important policy implications. Because localities wield significant decision power in land use and density, these spillover effects of density and human capital should be taken into account to make accurate assessments on the efficacy of local-level policies.

This chapter proceeds as follows. In the rest of this section, I review the literature related to the paper and summarize my contribution. In Section 1.2, I provide some stylized facts about workplace location and its relation to human capital distribution within the city. In Section 1.3, I provide descriptive evidence on positive, fast-decaying human capital spillovers that come from density of surrounding college workplaces. In Section 1.4, I outline the model framework. In Section 1.5, I provide details on the model estimation procedure and present parameter estimates. Finally, in Section 1.6, I use the estimated model to explore the effects of Los Angeles policy.

1.1.1 Literature Review

This chapter contributes to two main strands of literature in urban economics. First, I contribute to the literature on agglomeration economies. Many empirical papers estimate the effect of density on productivity of cities by regressing wages on city-level population or employment density (Combes and Gobillon 2015). The estimates range from 0.04-0.07, indicating that a 1 percent increase in the city-level density affects wages by around 0.05 percent, or alternatively, a doubling of density increases productivity by about 4%. A more recent review by Ahlfeldt and Pietrostefani (2019) corroborates these estimates and summarizes the evidence on density's effect on more outcomes such as rent, external welfare, and amenity provision.

Papers utilizing finer spatial definitions have found that agglomeration economies exist even at a smaller spatial scale. Henderson and Argazhi (2006) document extremely localized agglomeration effects in Manhattan's advertising industry, where the effect disappears beyond 200m. Ahlfeldt et al. (2015) uses an economic geography model with the division and reunification of Berlin as an exogenous shock and find that productivity agglomeration spillovers decay to about 0 by 15 minutes of travel time.

I contribute to the agglomeration literature by introducing the human capital margin at a smaller spatial scale. The agglomeration spillover estimates have either used total worker counts (Ahlfeldt et al. 2015) or assumed that there are only certain industries that benefit from spatial spillovers (Berkes and Gaetani 2019). In this chapter, I instead separate out the educational component of local agglomeration spillovers, and find that there is a meaningful distinction between the two education groups in how they generate spillovers across space. This chapter's focus is similar to Rosenthal and Strange (2006), who also separate out agglomeration spillovers between college and non-college workers. However, their paper uses Place-of-Work Public Microdata Area (PWPUMA) as the geographic definition of workplace and assumes that any count of workers in each PWPUMA is evenly distributed within the area. This is a very crude measure of worker counts, especially given that I document the uneven distribution of workplaces within larger spatial units.

Second, I contribute to the literature on city-level human capital spillovers. Moretti (2004a,b) find that there exist social benefits of high-skill workforce within the city. He finds that an increase in college share leads to positive wage gains for all workers, but especially uneducated ones. Ciccione and Peri (2006) are more skeptical about the extent of human capital within cities. Controlling for the composition effect, they find negligible effect of city-level average years of schooling on city-level productivity. Diamond (2016) also provides estimates of city-level cross-skill spillovers, especially from college to non-college workers. My estimated fast-decay of spillovers provide an additional dimension of the effect of human capital. Due to the very local nature of the human capital spillovers that I estimate, it is possible that even when there are strong effects in the city as a whole, the effect may be unevenly distributed.

Fu (2007) is one of the few papers examining local human capital spillovers at a spatial scale as fine as mine. He explores neighborhood-level spatial spillovers of various agglomeration mechanisms using confidential Decennial Census data for the Boston metropolitan area, which has detailed geographical information on each respondent. Like other papers exploring human capital spillovers, he focuses on the share of college workers in nearby workplaces. He finds a very localized average education spillover effect that decays rapidly down to about 0 by 3 miles, similar to the results provided in this chapter.

I rely on the modeling and solution methods developed in Ahlfeldt et al. (2015). Ahlfeldt et al. (2015) develops an economic geography model incorporating joint residence-workplace location decision, commuting, and agglomeration spillovers across neighborhoods. I include a CES production technology with college and non-college workers and agglomeration spillovers with college and non-college nearby density separately. To the best of my knowledge, this paper is the first paper to estimate human capital agglomeration spillovers across neighborhoods in the U.S. in such an economic geography model.

1.2 Stylized Facts

While residence patterns by education group within cities have received bulk of the attention in the urban literature (Florida and Mellander 2015), the distribution of jobs by education group has been rarely studied. In this section, I provide novel summary measures on the distribution of workplaces by skill across *Census tracts* within cities. To do so, I use data on workplace location by educational attainment from Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program's LEHD Origin-Destination Employment Statistics (LODES).

LEHD collects administrative data from unemployment insurance database from each state for private jobs and government employee statistics for public jobs. It contains data on the employee-employer link for each job that is in the unemployment insurance system, which is about 95% of all private wage or salary jobs (Graham et al. 2014). LODES data counts the number of jobs and residences by counting the worker in each Census block. It also contains data of these counts by four levels of educational attainment: high school dropout, high school or equivalent, some college, and bachelors degree or advanced degree.[4] I refer to workers with bachelor's degrees or above as college (high-skill) workers, and workers without bachelor's degree as non-college (low-skill) workers.[5] I aggregate the counts from Census block to the Census tract-level. Additionally, I use data on residential counts of workers by college status by Census tract from the American Community Survey 5-year Summary Files.

A number of new patterns of density and co-location of workplaces emerge. First, workplaces tend to be co-located by skill level even while residences are segregated. Figure 1.1 shows the distribution of residences (1.1a) and workplaces (1.1b) within the Los Angeles-Long Beach-Anaheim Core-Based Statistical Area (CBSA) as an example, with each shade of color indicating bins of 20 quantiles. Figure 1.1a shows a familiar picture of residential segregation. We see that there is a cluster of non-college residents in South Central near downtown Los Angeles, while there is a cluster of college residents by the beach and on the Westside. Although there is some overlap in the patterns of residence, there is mostly a considerable level of separation between where college residents and non-college residents locate. In fact, the index of dissimilarity,[6] a common measure of segregation in the literature, is 0.42 in the CBSA.

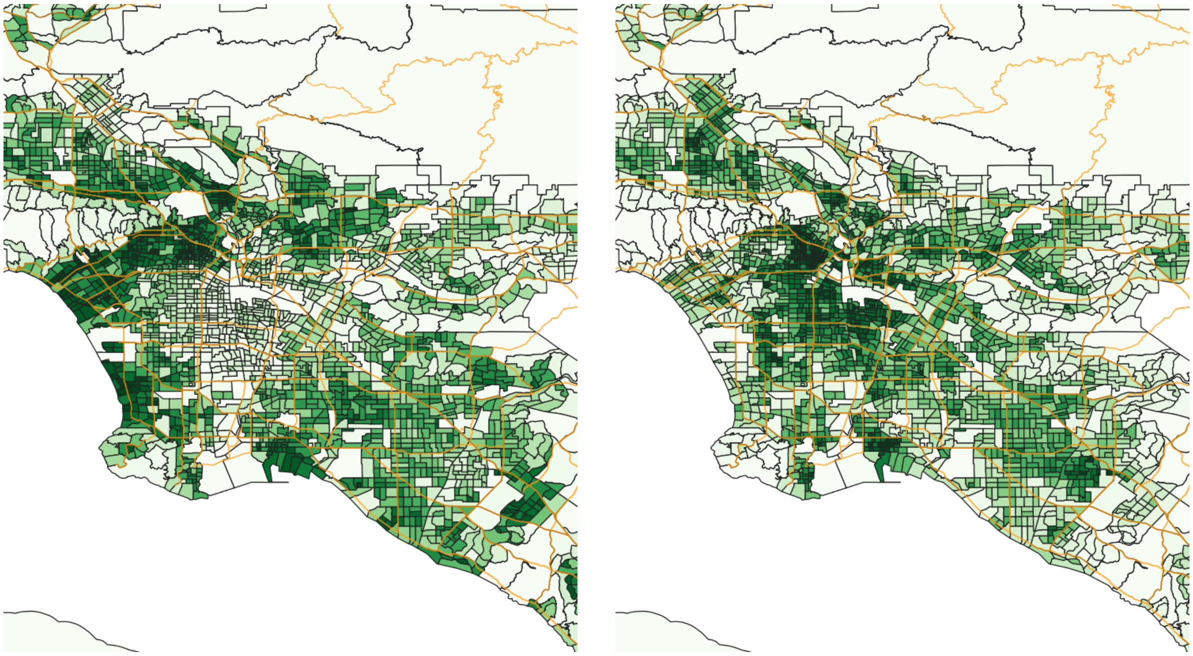
In contrast, Figure 1.1b shows a different picture. We see that the workplace heat map for college and non-college workplaces are very similar. The index of dissimilarity calculated with workplaces is 0.18, less than half of the residence index. These two figures show that

[4]Some of the demographic details and workplace location are imputed. I verify the data quality by checking against different sources of data and find a high degree of correlation with other available datasets. See Section 1.5.2 for more details.

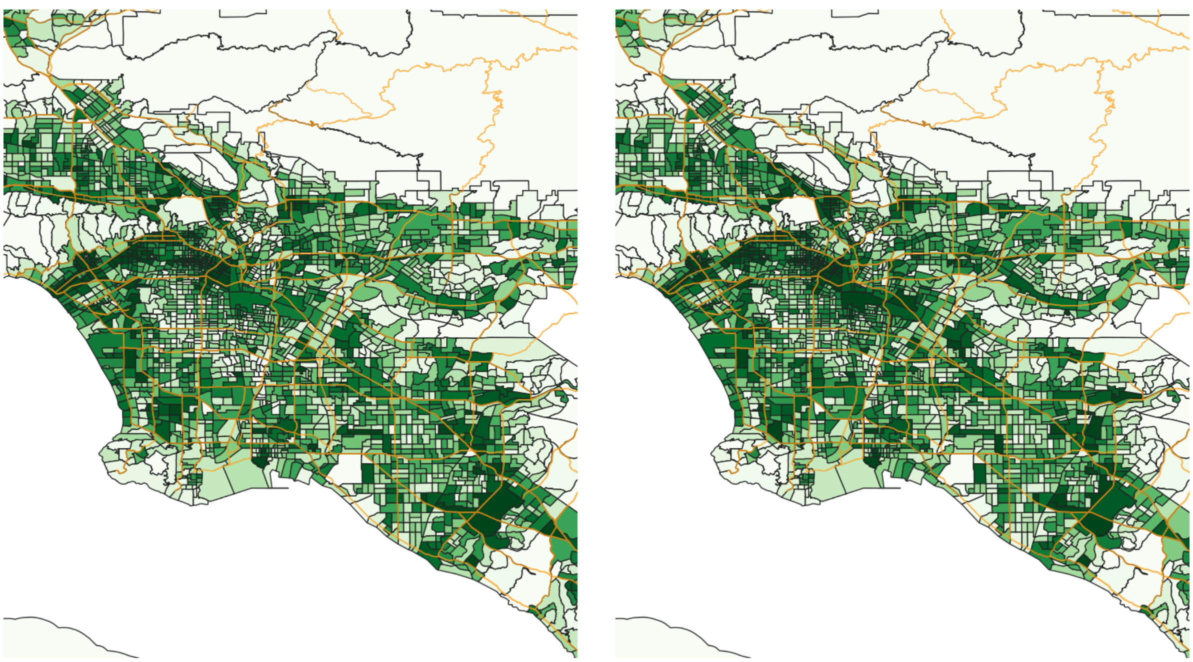
[5]This definition of college and non-college workers is the margin that matters the most in recent redistribution of workers across and within cities (Diamond 2016, Baum-Snow and Hartley 2017, Couture et al. 2017)

[6]Index of Dissimilarity for two groups A and B is $\frac{1}{2}\sum_i \left|\frac{a_i}{A} - \frac{b_i}{B}\right|$, where a_i, b_i are group A and B population in tract i and A, B are total population of group A and B in the city. It has the interpretation as the share of people that need to be moved to have equal share of residents by group in all areas.

Figure 1.1: Residential and Workplace Location in Los Angeles-Long Beach-Anaheim CBSA



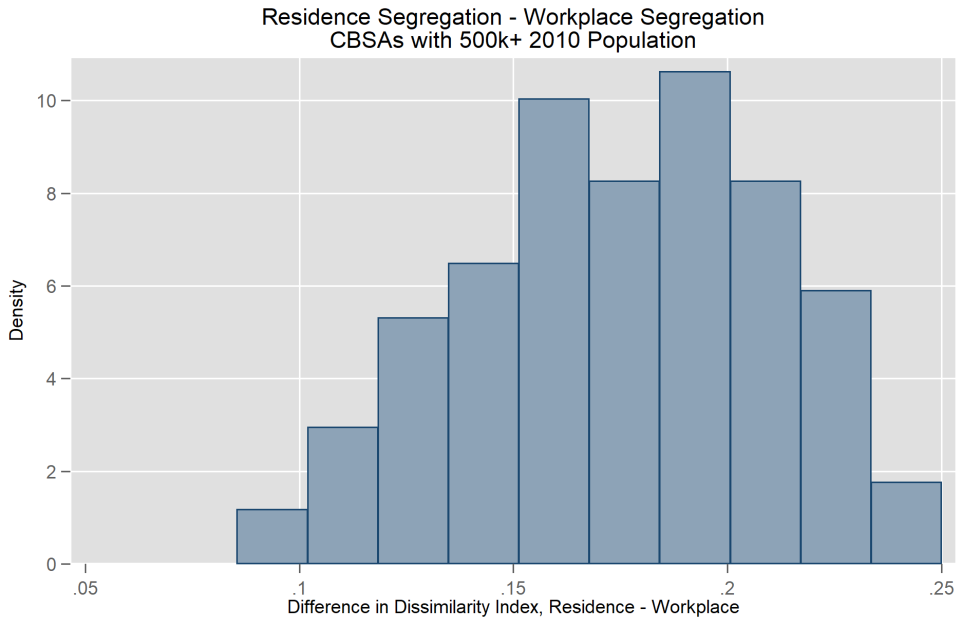
(a) Residence quantiles of college (left) and non-college (right) residents



(b) Workplace quantiles of college (left) and non-college (right) workplaces

Plots the distribution of residences (a) and workplaces (b) for workers with bachelor's degree or higher (college) and without bachelor's degree (non-college). Darker shades are higher quantile categories, where 20 quantiles were used for both graphs.

Figure 1.2: Histogram of Difference Between Residence and Workplace Segregation



The difference plotted is calculated as (index of dissimilarity for residence) - (index of dissimilarity for workplace), where the indices of dissimilarity are calculated across college and non-college residents/workers in CBSAs with more than 500,000 residents in 2010 Census.

for the Los Angeles metropolitan area, residences tend to be much more segregated than workplaces. This pattern holds in general for the CBSAs with more than 500,000 people in 2010.[7] Figure 1.2 is the histogram of the difference between residence and workplace dissimilarity indices. It shows that in virtually all cities residential segregation is more pronounced than workplace segregation.

Another way to illustrate the fact that workplaces tend to be more co-located is to run rank-rank regressions of tracts by their relative rank in college versus their relative rank of non-college employment. First, I rank each *tract* from highest to lowest college employment count, calling this $Rank_{col}$. Then I rank each tract from highest to lowest non-college employment count, calling this $Rank_{non}$. Regressing $Rank_{col}$ on $Rank_{non}$ provides a

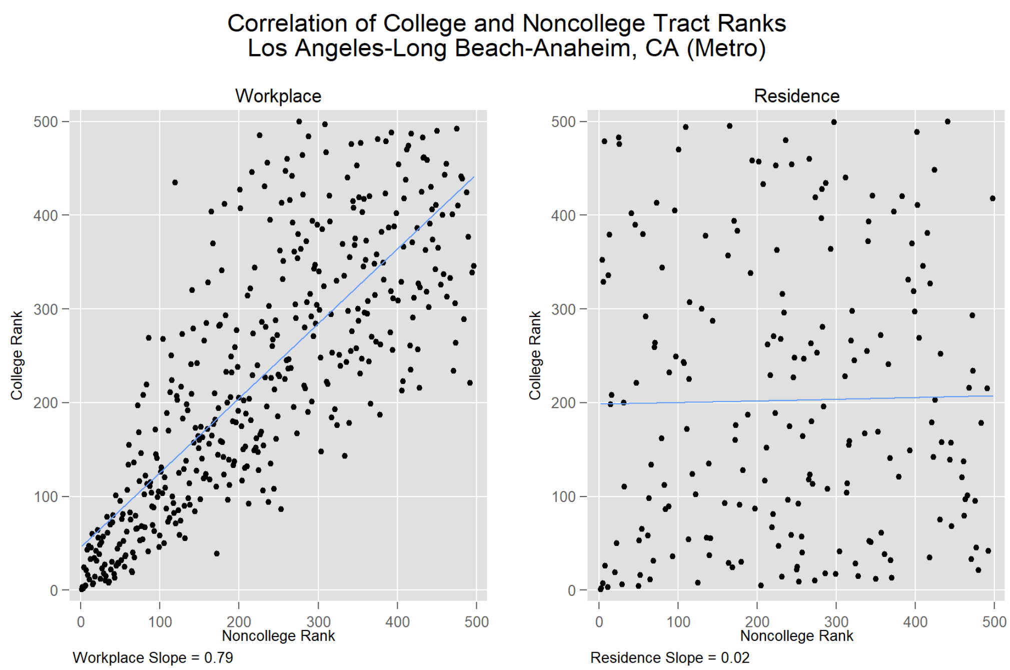
[7]The population cutoff of 500,000 in CBSA roughly corresponds to Top 100 CBSAs (it is top 104), so I use this number throughout

summary measure of how correlated the rank by skill groups are. I do this for residences as well. Figure 1.3a provides an example for Los Angeles metropolitan area, and 1.3b provides the distribution of these rank-rank regression coefficients for both workplace and residence in CBSA with more than 500,000 inhabitants.

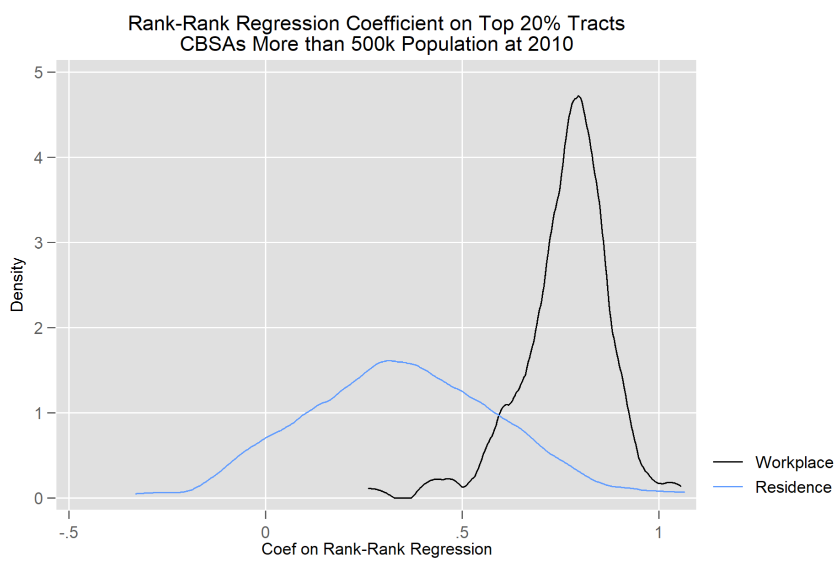
We can see in Figure 1.3a that correlation of college and non-college ranking of tracts for workplace is very high. This shows that tracts with more college jobs relative to the entire city are also tracts with relatively more non-college jobs. Meanwhile, in the residential rank-rank regression, no such pattern emerges. Looking at the distribution of these coefficients across different metro areas in Figure 1.3b, we see two other interesting patterns. First, workplace rank-rank regression coefficients are larger than residence rank-rank regression coefficients. This pattern is expected from workplaces being much more co-located than residences. Second, workplace regression coefficients are much more concentrated than residence coefficients. This seems to suggest that while residential co-location patterns differ by city, workplace co-location patterns tend to be universal across cities.

Finally, I show that 1) both college and non-college employment tends to be highly concentrated, but college employment more so and 2) high density areas tend to have higher college share of employment. Figure 1.4 gives one summary measure of the concentration of college and non-college employment across CBSAs: it shows the distribution of the share of employment in the top 10% of area by CBSA. To calculate this number for college workplaces, I use $Rank_{col}$ and calculate the cumulative share of area these tracts occupy in each CBSA. I take the tract at which the cumulative share of area is around 10% of CBSA total area and examine how much cumulative share of employment that tract contains. So, for example, if this number is 30% in one CBSA, it means that 30% of the CBSA's college employment is located within 10% of its area. I do this for non-college workplaces as well, and plot the distribution of the share of employment for both college and non-college jobs. Figure 1.4 shows that non-college workplaces tend to be less concentrated than college workplaces, with average share being around 45% for non-college and 56% for college workplaces.

Figure 1.3: Rank-Rank Regression Coefficients



(a) Rank-Rank Scatterplots for Los Angeles-Long Beach-Anaheim CBSA



(b) Kernal Density Estimate of Workplace and Residence Rank-Rank Coefficients

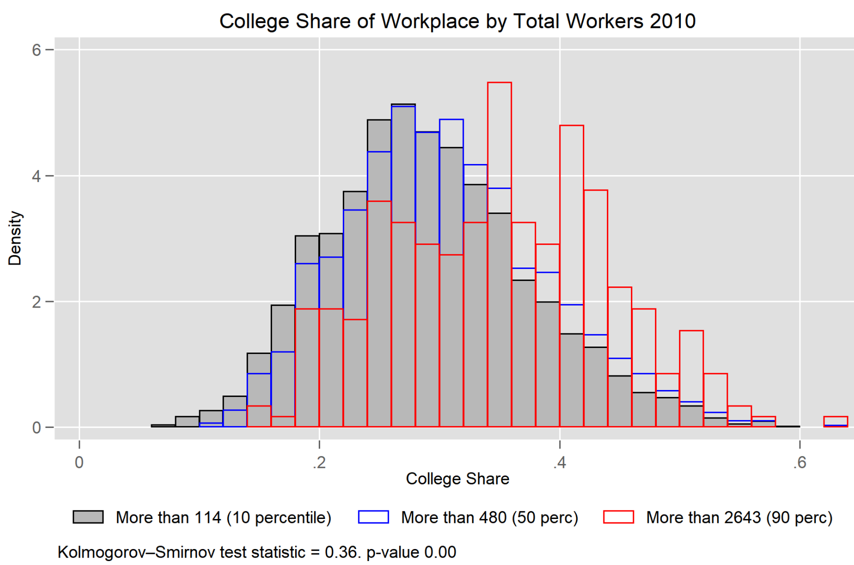
Coefficients from a regression of rank of tract by college worker counts on rank of tract by non-college worker counts, and similarly for residence counts. The regression is plotted for Los Angeles (a) and distribution of coefficients is plotted for all cities above 500,000 population in metro area (b).

Figure 1.4: Distribution of Top 10% Concentration of Employment

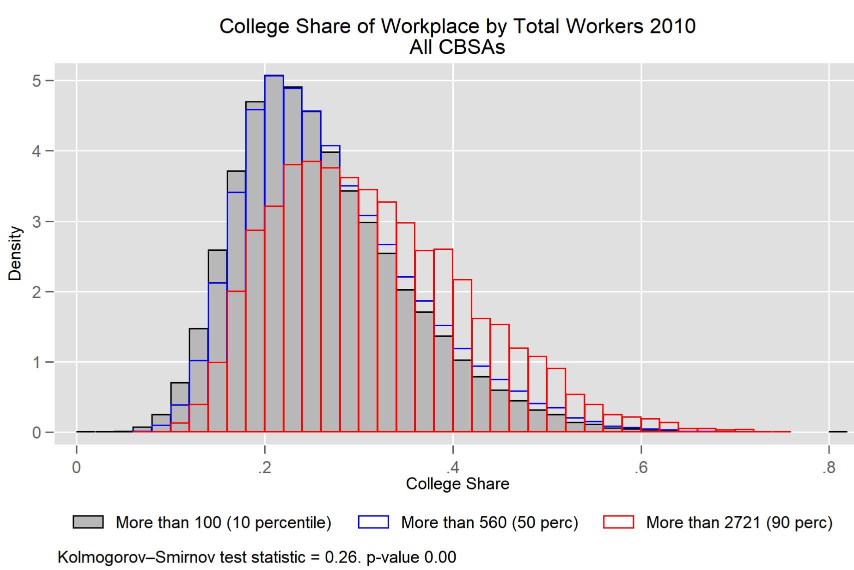
Distribution of a measure of concentration of jobs in CBSAs with more than 500,000 population in 2010 Census. The measure is the share of employment in top 10% of tracts by area in the CBSA. The mean for non-college workplaces is 45% and for college workplaces is 56%.

On the other hand, Figure 1.5 shows that denser areas tend to be occupied by more college workers relative to non-college workers. For example, in Figure 1.5a, I plot three histograms for Los Angeles CBSA. The first histogram in black restricts the sample to tracts above the 10th percentile of tracts with by the number of total workplaces. The second histogram in blue restricts the sample to those above the 50th percentile of tracts, and the third histogram in red restricts the sample to those above the 90th percentile. Thus, the histograms sequentially restrict the sample to denser areas to examine whether there tend to be higher college share of total workplaces for denser tracts. And indeed, we see that denser workplace locations (by count of total number of workers) tend to have higher college

Figure 1.5: Histogram of College Share by Number of Workers in Tract



(a) Los Angeles



(b) All CBSAs with Population 500,000+

Histogram of college share of jobs in tracts in (a) Los Angeles and (b) all CBSAs with more than 500,000 population in 2010 Census. The histogram is plotted restricting the sample to tracts with more total employment than 10th percentile of tracts by total employment (black), 50th percentile (blue), and 90th percentile (red). Kolmogorov-Smirnov test statistic for the null that the distribution above 10 percentile is equal to the distribution above 90th percentile.

share, as can be seen from the right-shifted red histogram compared to the black histogram. Figure 1.5b shows that this pattern holds across all CBSAs with the Kolmorogorov-Smirnov test statistic of 0.26 and a p-value near 0.[8] This is in line with qualitative evidence of higher concentration of college workers, and dense locations becoming more concentrated by high human capital firms (Katz and Wagner 2014).

This set of stylized facts indicate a high degree of co-location and concentration for college and non-college workplaces. Urban economists have traditionally seen the density of firm location as a sign of agglomeration economies and co-location of firms as a sign of spillovers. Therefore, these stylized facts provide preliminary suggestive evidence on the existence of very local agglomerative spillover effects of both college and non-college workers. I now turn to exploring this evidence further.

1.3 Descriptive Evidence

In this section, I provide descriptive evidence on the existence of spatial spillovers by skill level using individual-level data on workplace and income. I estimate ordinary least squares (OLS) regression coefficients of worker wages on nearby college and non-college worker density, providing preliminary evidence on the existence and fast decay of agglomeration spillovers by skill level.

1.3.1 Estimation Framework

I use a standard Mincerian approach used in the agglomeration literature, taking wages as an indirect measure of productivity. Suppose each location j produces goods using a Cobb-Douglas production function $Y_j = A_j N_j^{\alpha} F_j^{1-\alpha}$ with Total Factor Productivity (TFP), A_j, inputs using floor (or office) space F_j and CES labor aggregator $N_j = \left(a_{jL} L_j^{\frac{\sigma-1}{\sigma}} + a_{jH} H_j^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$

[8]The null of the Kolmorogorov-Smirnov test is equality of two distributions, one distribution for tracts above the 90th percentile in total worker counts and one for below the 90th percentile.

that aggregates college and non-college workers. The first-order condition and the profit-zero condition yields the following expression for wage w_{jg} at location j for skill group g:

$$\log w_{jg} = \log a_{jg} + \frac{1}{\sigma}\log\frac{N_j}{G_j} - \frac{1-\alpha}{\alpha}\log q_j + \frac{1}{\alpha}\log A_j \tag{1.1}$$

where G_j denote labor for group g and q_j is floor price at j.

Suppose now that the productivity A_j is determined by a function of own-location productivity a_j and surrounding employment density of college $H_{j'}$ and non-college $L_{j'}$ workers in tracts $j' \neq j$:

$$\log A_j = \log a_j + f_H(\{H_{j'}\}_{j'\neq j}) + f_L(\{H_{j'}\}_{j'\neq j}) \tag{1.2}$$

where $f_H(\cdot), f_L(\cdot)$ are functions that determine how surrounding density affects productivity at j.

Combining equations (1.1) and (1.2), we arrive at the following equation,

$$\log w_{jg} = \tilde{\gamma}_L \log L_j + \tilde{\gamma}_H \log H_j + \tilde{\gamma}_F q_j + \tilde{f}_H(\{H_{j'}\}_{j'\neq j}) + \tilde{f}_L(\{H_{j'}\}_{j'\neq j}) + \log \varepsilon_{jg} \tag{1.3}$$

where $\tilde{\gamma}_L, \tilde{\gamma}_H, \tilde{\gamma}_F$ are reduced-form parameters that combine various model parameters and functional form assumptions, and ε_{jg} combines the two unobservable fundamentals a_j, a_{jg}, the productivity terms at location j and for location-group $j \times g$.

I augment the estimating equation to include individual worker o characteristics. Following the literature, individual characteristics are thought of as augmenting individual's productivity, where her final productivity is a function of her own observable characteristics X_o and unobserved characteristics ε_o. So the productivity worker o working in location j is given by $\log A_{jgo} = \log A_j + \Gamma X_o + \varepsilon_o$.

Finally, assume a functional form for $\tilde{f}$ to be log-additive in the sum of employment in concentric rings around tract j of varying radii. So for example, H_{j2-3} sums all high-skill workplaces in tracts that are 2-3 miles away from workpalce tract j. With this assumption, equation (1.2) becomes,

$$\log A_j = \log a_j + \sum_{d>0} \gamma_{Hd} \log H_{jd} + \sum_{d>0} \gamma_{Ld} \log L_{jd} \tag{1.4}$$

where d denote rings. Therefore, the final estimation regresses individual wages on characteristics of individuals, tract, and surrounding workers in concentric circles,

$$\begin{aligned} \log w_{jgo} = \tilde{\gamma}_L \log L_j + \tilde{\gamma}_H \log H_j + \tilde{\gamma}_F q_j + \sum_{d>0} \gamma_{Hd} \log H_{jd} + \sum_{d>0} \gamma_{Ld} \log L_{jd} \\ + \Gamma X_o + \log \varepsilon_{jg} + \log a_j + \varepsilon_o \end{aligned} \tag{1.5}$$

The parameters of interest are γ_{Hd}, γ_{Ld} and how they vary with distance. I cluster the standard errors of these parameters on the workplace tract-level in my OLS specifications.

There are two main sources of endogeneity in the current regression framework. First, high skill individuals may also select into more productive tracts, which could bias the estimates. For example, high ability individuals may receive higher wages *ceteris paribus*, but also select into more productive workplaces. This will bias the estimates for nearby density away from 0. I include individual and household controls to mitigate these concerns as much as possible, but there may still be remaining selection on unobservables. Rosenthal and Strange (2009) argue that as long as the selection effect is constant across sources of productivity increases (e.g., first ring versus second ring, etc.), comparing the coefficient estimates between different rings should still give a consistent estimate of the decay. If we make that assumption, we can get an unbiased estimate of relative magnitude of productivity increases in the presence of selection even though the absolute magnitude of productivity increases from surrounding density may be biased.

Second, there may be omitted tract-level variables that may be correlated across space. For example, consider a newly built train line with multiple stations nearby. Suppose this train station offers an identical access to a productivity-enhancing technology and easier access for workers to two nearby tracts. These will each show up as an increase in own-tract productivity and increase in nearby workers. However, this enhanced productivity is not due to spillovers, but a common benefit from a third source. Therefore, anything that affects productivity of locations across space will raise both the wages in tract j while raising employment in surrounding tracts j'. This is a crucial concern. Addressing this issue in the current framework is extremely difficult: it would require an instrument for each ring with independent sources of variation for both college and non-college workers. For now, I include possible tract-level characteristics such as public transport accessibility to control for shared spatial characteristics as much as possible.

1.3.2 Data

I use 2010 California Household Travel Survey (CHTS) data to get information on individual's workplace and household income. Because LODES does not have data on wages at each location, I cannot use it directly to examine the question of productivity across locations. CHTS is a publicly available dataset from a travel survey conducted on a sample of households. Crucially, the data contains information on income and workplace tract of the individuals, which makes it one of the only publicly available individual/household-level sources with such geographic precision *and* income information. The survey also contains information on various demographic and work-related variables, which I include in the regression as controls to proxy for selection effects.

To obtain information on the surrounding density of college and non-college employment, I use information from LODES on tract-level employment by skill group. I form concentric rings of various radii around the centroid of each tract, summing up the number of jobs in each concentric circle rings. For example, for tract j, I sum the college employment in tracts j' whose centroids are within 3-5 miles from the centroid of tract j. This gives me the college measure for the 3-5 mile ring. I compute the measures for college and non-college workplaces within 0-3 miles (excluding the own-tract), 3-5 miles, 5-10 miles, 10-25 miles, 25-50 miles, and 50-100 miles.

In the regression, I choose to include up to and including 10-25 miles, which was chosen by sequentially adding the rings, computing the Aikake Information Criterion (AIC), and finding the specification minimizing the AIC. Bayesian Information Criterion (BIC) gives a slightly more parsimonious specification, excluding one ring at 10-25 miles. I choose to include up to 10-25 miles in favor of completeness. Therefore, there are total of 4 rings in the regression specification for each skill group.[9]

I use household income bins as the dependent variable. The bins are defined as follows: \$0 – \$9,999, \$10,000 – \$24,999, \$25,000 – \$34,999, \$35,000 – \$49,000, \$50,000 – \$74,999, \$75,000 – \$99,999, \$100,000 – \$149,999, \$150,000 – \$199,999, \$200,000 – \$249,999, and \$250,000 or more. Because income is only observed in bins, I estimate both an interval regression model directly using the bins and an OLS model with midpoints of each bin as the outcome. In addition, I include four sets of controls in the regressions. First, I include controls relating to the individual's demographics: race, Hispanic status, nativity status, whether the person has a valid driver's license, age indicator variables in bins, college status, and male indicator. Second, I include individual's employment characteristics: full-time status, hours per week, industry and occupation of employment. Third, I include some household controls: whether the household owns the home and the total worker count of the household.

Last, I include characteristics of the workplace tract: 1) the median residential housing rent, 2) total road network density, 3) aggregate frequency of transit service within 0.25 miles of the boundary 4) log working age population residing within 45 minutes of driving time, and 5) CBSA fixed effects. Median residential housing rent data is from 2006-2010 American Community Survey 5-year Summary File. The three accessibility measures are from EPA's SmartLocationDb. The median housing rent is a proxy for floor price faced by firms locating in tract j. Total road network density and aggregate frequency of transit

[9] In the appendix, I present results for a specification disaggregating the first 5 miles into 0-1, 1-2, 2-3, 3-4, 4-5 miles.

service both measure the tract's infrastructure. While roads are infrastructures that is both productivity- and commute-enhancing, transit service may be thought of as commute-enhancing but not necessarily a productivity-enhancing. Working age population residing within 45 minutes of driving time measures the accessibility of workers for a firm but also is a measure of geographic centrality within the city.

Summary statistics for the estimation sample is presented in Table 1.1 for all workers, non-college workers, and college workers. The two education-specific sample restricts age to 26+ in order to avoid including workers who may still be completing their education. As expected, college workers on average belong to higher household income groups, have higher share of natives, valid driver's licenses, and home ownership. In addition, a higher share of college workers are white and Asian compared to non-college workers, and a significantly lower share of college workers are Hispanic. However, the age groups are distributed similarly, and workers of both groups work similar number of hours.

Comparing workplace tract characteristics of the two groups reveal interesting patterns. First, college workers seem to work in more connected workplaces, measured by total road network density, aggregate frequency of transit nearby, and working age population within 45 minutes of car travel. It is particularly interesting that college workers tend to work in areas that are more accessible by public transit, although almost all of them have valid driver's licenses. Second, college workers work in areas with higher residential floor price. These summary statistics go hand-in-hand with the styled fact I presented in Section 1.2 that shows that college workers tend to work in denser areas.

1.3.3 Results

As described above, I estimate two different models: 1) an OLS specification with income bins converted to log income level by using the midpoint of the bins, and 2) an interval regression specification, which uses information on the endpoints of the intervals to estimate an ordered multinomial probit model without the need to estimate the cutoffs of the intervals.

Table 1.1: Summary Statistics for CHTS Estimation Sample

	Sample		
	All	Non-college	College
Individual/HH Characteristics			
Household income category	6.01	5.08	6.77
Income, median of bracket	107,733	77,707	131,784
Male	0.52	0.52	0.51
College	0.54	0.00	1.00
Nativity	0.80	0.74	0.82
Valid Driver's License	0.96	0.94	0.99
Hours Per Week	37.20	37.58	38.49
Full Time (Hours > 35)	0.73	0.75	0.77
Number of Workers in HH	1.89	1.84	1.79
Home Ownership	0.80	0.73	0.85
White	0.73	0.67	0.77
Black	0.03	0.04	0.03
Native American	0.04	0.07	0.02
Asian	0.08	0.05	0.11
Other	0.09	0.15	0.05
Hispanic	0.20	0.32	0.10
Age Group			
Age Group 16-25	0.09		
Age Group 26-35	0.12	0.15	0.12
Age Group 36-45	0.19	0.20	0.21
Age Group 46-55	0.28	0.32	0.30
Age Group 56-65	0.25	0.26	0.29
Age Group 66-79	0.05	0.04	0.06
Workplace Tract Characterisitcs			
Total road network density	19.37	18.25	20.30
Agg. freq transit within 0.25 mi	133.60	107.20	159.00
Working age pop. within 45 min	1,212,000	1,176,000	1,245,000
Residential Floor Price	83,495	78,621	87,519
N	29,576	11,474	15,507

Means presented for each sample. The sample are restricted to have no missing values for all variables and are identical to the estimation sample. Non-college and College columns only include those with ages 26+ to avoid including workers who are still completing education.

Interval regressions are better than ordered probit models because the coefficient estimates have a straightfoward meaning of x's partial effect on y, just like in linear regressions.

The parameter estimates of the regressions are presented in Table 1.2. I present results for both specifications on three different samples: 1) the whole sample of workers, 2) only non-college workers aged 26+, and 3) only college workers aged 26+. Therefore, we can consider the estimates on different samples as estimating cross-skill and own-skill spillover effects from surrounding skill densities. I also plot the coefficients on distance by different sample of workers using the OLS specification (columns 2 and 3 in Table 1.2) in Figure 1.6.

For example, column (3) tells us that an increase of 1% in own-tract college workers is associated with a 0.12% increase in worker's household income for if the worker is college-educated. This effect seems to decrease with distance rapidly, with a 1% increase in college workers in 0-3 mile radius having 0.07% increase in household income, and only 0.02% increase for 3-5 miles. Also, the estimates are no longer statistically significant beyond 3-5 miles. Meanwhile, there is an opposite effect of nearby non-college workers. A 1% increase of non-college workers in the own tract decreases college worker household income by 0.1%. College worker income decreases by about 0.06% and 0.03% in 0-3 miles and 3-5 mile radii, respectively. Again, the estimates are not statistically significant beyond 3-5 miles. These estimates are stable between OLS and interval regression specifications.

These fast decaying effects of surrounding density is more easily visible when the coefficients are plotted against distance in Figure 1.6. We can see that for both college and non-college workers, the effect of surrounding density from college workers are positive and decays to close to 0 by 3-5 mile radius. On the other hand, the effect of surrounding density of non-college workers is negative and also close to 0 by 3-5 mile radius. Also, note that the R-squared is much larger for non-college workers than for college workers, suggesting more heterogeneity of workplace location choice for college workers than for non-college workers.

The regression results display two patterns. First, the density effect of nearby college workers seem similar on both college and non-college workers and the density effect of nearby

Table 1.2: Descriptive Regression Results

Model	*OLS*			*Interval Regression*		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Non	Col	All	Non	Col
Col Own	0.0925***	0.0446**	0.124***	0.0998***	0.0498***	0.130***
	(0.0120)	(0.0191)	(0.0151)	(0.0103)	(0.0170)	(0.0133)
Col 0-3 mi	0.0923***	0.0760**	0.0711**	0.0883***	0.0652**	0.0723***
	(0.0222)	(0.0322)	(0.0284)	(0.0186)	(0.0292)	(0.0249)
Col 3-5 mi	0.0257	0.0190	0.0235	0.0329*	0.0258	0.0282
	(0.0201)	(0.0320)	(0.0263)	(0.0196)	(0.0310)	(0.0260)
Col 5-10 mi	-0.0154	0.00141	-0.0286	-0.0170	0.00274	-0.0306
	(0.0265)	(0.0420)	(0.0344)	(0.0248)	(0.0398)	(0.0330)
Col 10-25 mi	0.0143	-0.00955	0.0455	0.0200	-0.000841	0.0509
	(0.0320)	(0.0533)	(0.0400)	(0.0308)	(0.0484)	(0.0412)
Non Own	-0.0766***	-0.0214	-0.110***	-0.0837***	-0.0275	-0.115***
	(0.0134)	(0.0214)	(0.0169)	(0.0115)	(0.0189)	(0.0150)
Non 0-3 mi	-0.0858***	-0.0771**	-0.0588*	-0.0815***	-0.0660**	-0.0594**
	(0.0244)	(0.0351)	(0.0316)	(0.0209)	(0.0328)	(0.0280)
Non 3-5 mi	-0.0350	-0.0343	-0.0259	-0.0424*	-0.0416	-0.0301
	(0.0226)	(0.0361)	(0.0297)	(0.0221)	(0.0352)	(0.0292)
Non 5-10 mi	0.0329	0.0366	0.0342	0.0357	0.0357	0.0376
	(0.0298)	(0.0473)	(0.0387)	(0.0280)	(0.0448)	(0.0373)
Non 10-25 mi	0.00399	0.0209	-0.0250	-0.00267	0.00994	-0.0299
	(0.0350)	(0.0591)	(0.0437)	(0.0339)	(0.0535)	(0.0453)
Constant	10.53***	10.14***	11.16***	10.60***	-0.593***	-0.648***
	(0.263)	(0.429)	(0.327)	(0.240)	(0.387)	(0.00628)
Observations	29,576	11,474	15,507	29,576	11,474	15,507
R-squared	0.451	0.438	0.304			
Std. errors	*Clustered (employment tract)*			*White HAC*		

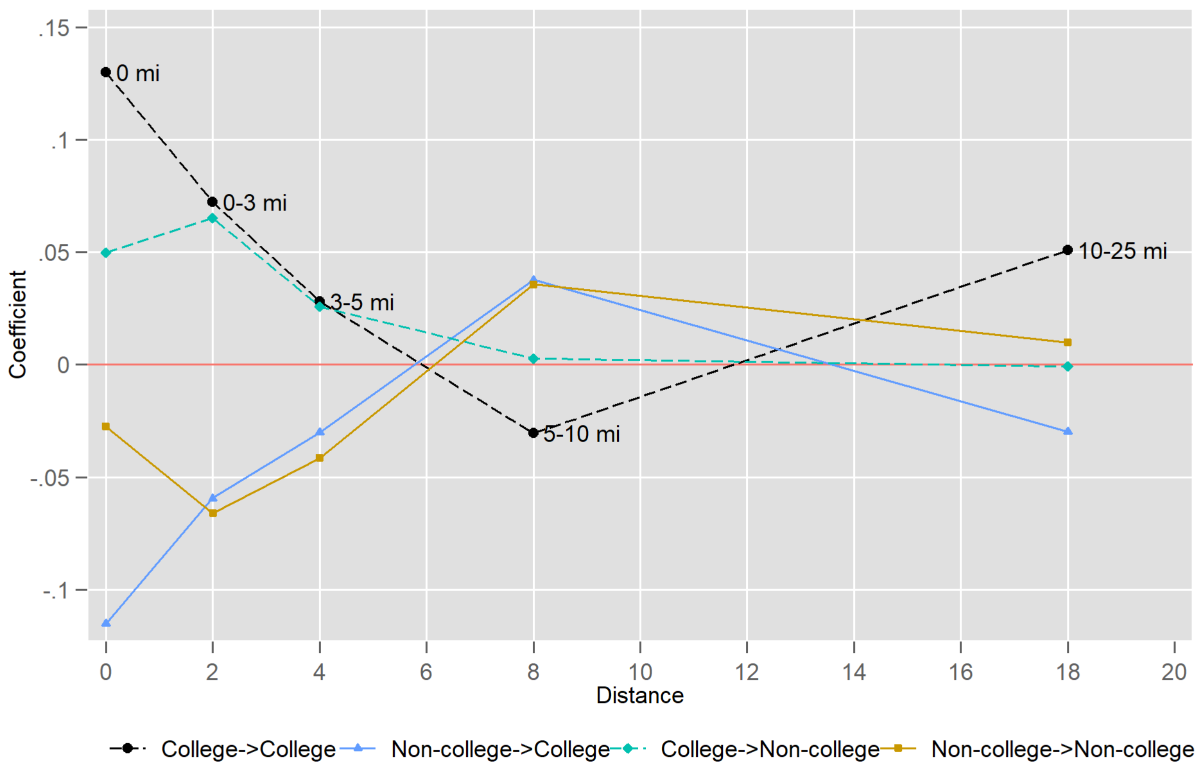
Clustered standard errors at the employment tract level in parentheses in OLS and White HAC standard errors in parentheses in interval regression. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Independent variables displayed are in logs. Columns (1)-(3) present results from the OLS specification, while columns (4)-(6) present results from the interval regression specification. Columns (1) and (4) use the entire sample, while columns (2), (5) and columns (3), (6) estimate the model only using non-college and college workers in the regression sample, respectively.

Table 1.3: OLS Regression Table with Varying Controls

Controls	(1) FE	(2) Add Ind	(3) Add Emp	(4) Add HH	(5) Add Tract
Col Own	0.323***	0.141***	0.0987***	0.0991***	0.0925***
	(0.0159)	(0.0130)	(0.0126)	(0.0117)	(0.0120)
Col 0-3 mi	0.161***	0.0782***	0.0803***	0.0942***	0.0923***
	(0.0293)	(0.0241)	(0.0230)	(0.0211)	(0.0222)
Col 3-5 mi	0.0144	0.0122	0.00858	0.0174	0.0257
	(0.0290)	(0.0237)	(0.0223)	(0.0197)	(0.0201)
Col 5-10 mi	-0.0274	-0.0265	-0.00371	0.00388	-0.0154
	(0.0361)	(0.0305)	(0.0290)	(0.0253)	(0.0265)
Col 10-25 mi	-0.00493	0.0333	0.0433	0.0150	0.0143
	(0.0444)	(0.0355)	(0.0343)	(0.0313)	(0.0320)
Non Own	-0.298***	-0.115***	-0.0859***	-0.0846***	-0.0766***
	(0.0174)	(0.0142)	(0.0141)	(0.0129)	(0.0134)
Non 0-3 mi	-0.170***	-0.0789***	-0.0823***	-0.0915***	-0.0858***
	(0.0330)	(0.0269)	(0.0257)	(0.0233)	(0.0244)
Non 3-5 mi	-0.0399	-0.0282	-0.0229	-0.0307	-0.0350
	(0.0327)	(0.0268)	(0.0251)	(0.0221)	(0.0226)
Non 5-10 mi	0.0429	0.0372	0.0106	0.00800	0.0329
	(0.0405)	(0.0346)	(0.0328)	(0.0284)	(0.0298)
Non 10-25 mi	0.00547	-0.0261	-0.0295	-0.00600	0.00399
	(0.0484)	(0.0389)	(0.0376)	(0.0342)	(0.0350)
Constant	11.10***	10.91***	10.92***	10.64***	10.53***
	(0.258)	(0.207)	(0.199)	(0.175)	(0.263)
Observations	34,388	33,941	33,085	33,085	29,576
R-squared	0.096	0.290	0.338	0.452	0.451
FE	CBSA	CBSA	CBSA	CBSA	CBSA

OLS regression with log of midpoint income of each bin as outcome variable. Clustered standard errors at the employment tract level in parentheses. Sample is all workers. *** p<0.01, ** p<0.05, * p<0.1. Independent variables displayed are in logs. The regressors included are: (1) density measures and CBSA FE, (2) individual controls, (3) individual employment characteristics, (4) household-level controls, (5) workplace tract controls.

Figure 1.6: Plot of Attenuation



Coefficient estimates on the concentric circles plotted over distance. Dotted lines are effect of surrounding college density on college (black) and non-college (green) workers. Solid lines are effect of surrounding non-college density on college (blue) and non-college (brown) workers. Distance is the mean of distance band, rounded up to the nearest integer.

non-college workers is also similar on college and non-college workers. Second, the spillover effect decays very quickly over space, in line with the estimates that are available in the literature. In particular, Fu (2007) estimates that in Boston, the college share of nearby locations has an effect that disappears past 3 miles, which is similar to what I find here. Therefore, these regressions suggest that there exist symmetric, fast-decaying spillover effects of distance.

In terms of the magnitudes, these estimates are on the lower end of Moretti's (2004b) city-level estimates but larger than Fu's (2007) tract-level estimates using only data from Boston. Moretti estimates a model with wages as the dependent variable and college share in the city as the independent variable. He finds that a one percentage point increase in the college share leads to about a 1.9% increase in wages of high-school drop-outs, 1.6% increase for high-school graduates, and 0.4% increase for college graduates. If I convert my estimates by assuming the mean value of college share across tracts (1/3) and increase the college share purely by increasing college workers, my estimates imply about 0.4% increase in household income for a one percentage point change in college share 0-3 miles away. However, the estimates are much larger than Fu's estimate, which shows that a one percentage point increase in college share 0-1.5 miles away is associated with a 0.14% increase in wages.

In the appendix, I present several specifications for robustness analysis. First, I use a finer subset of distances in the first 0-5 mile radius by disaggregating the range into 0-1, 1-2, 2-3, 3-4, 4-5 miles. The resulting coefficients are plotted in Figure 1.A.1. In Figure 1.A.1a I plot the original specification using 0-3 and 3-5 mile circles for all workers. In Figure 1.A.1b I plot the specification with disaggregated concentric circles. The qualitative results are similar, although the decay pattern is not as clear-cut in the earlier miles. However, the estimates in the earlier miles are not statistically distinguishable from each other. Moreover, the decay of the effect to 0 still holds beyond 3 miles. In Table 1.A.1, I present coefficient estimates for varying worker sample definitions. The estimates show similar results across sample definitions.

To examine some of the concerns of endogeneity, in Table 1.3, I present results of the OLS specification, sequentially adding in various controls. Column (1) only includes the density measures and CBSA-level fixed effects in the regression. Column (2) adds the individual controls, column (3) adds individual's employment characteristics controls, column (4) adds household-level controls, and finally column (5) adds tract-level controls. We see that in column (1) when no controls are included, the coefficients on the density measures are a lot larger than in column (2), which includes individual-level controls. This seems to suggest that the individual controls are controlling for some positive selection effect, on both own and surrounding tracts. However, the relative decay of the spillover coefficients are still qualitatively similar to before, with the spillover effect going to near zero by 3-5 miles.

Moreover, once the individual-level controls are included, additional controls do not seem to change the estimates by very much, and the coefficients are stable across specifications. This is suggestive evidence that perhaps selection effect is the most relevant bias in the estimation of the spillover effects. This suggests that further research using mover-design in Glaeser and Maré (2001) with individual-level panel data might be beneficial in exploring causal impact of neighborhood-level spillover effects.

Given the remaining concerns of endogeneity that I cannot fully deal with using the data at hand, I use an economic geography model framework using tract-level data that allows me to recover characteristics of workplaces that are unobserved. This introduces identification conditions that are less stringent to estimate parameters of interest, at the cost of imposing modeling assumptions, allowing me to provide additional evidence of human capital spillovers across neighborhoods and run counterfactual policy simulations. In the next sections I introduce the model, estimate it, and run policy simulations.

1.4 Model

The model is heavily based on Ahlfeldt et al. (2015), henceforth ARSW, and its multi-city extension by Berkes and Gaetani (2019). I augment the model in ARSW by incorporating skill groups and agglomeration spillovers of each skill employment across space.[10]

[10] Although Tsivanidis (2018) has a similar extension of CES aggregator of college and non-college workers, he does not consider spillovers across space. Moreover, because he does not observe college and non-college workplace directly in his data (Bogotá, Colombia), he has to use an additional assumption of constant shares of college workers in industries across space. Berkes and Gaetani (2019) incorporate two industry groups. However, they do not consider cross-spillovers in production, but only in amenities. In addition, the two industries are separate economies and therefore do not interact with each other in production, while in my model, the workers of two types are imperfect substitutes in the CES aggregator.

1.4.1 Workers

Within each city, a worker o of skill group $g \in \{H, L\}$ (high or low-skilled) living in tract i and working in tract j has the following utility,

$$U_{ijgo} = C_{ijgo} = \frac{B_{ig} z_{ijgo}}{d_{ij}} \left(\frac{c_{ijgo}}{\beta} \right)^{\beta} \left(\frac{h_{ijgo}}{1-\beta} \right)^{1-\beta} \tag{1.6}$$

where B_{ig} is residential amenities, d_{ij} is disutility of commuting from i to j, z_{ijgo} is the idiosyncratic preference shock on residence-workplace pair (i, j), c_{ijgo} is goods consumption, and h_{ijgo} is housing consumption. z_{ijgo} is distributed Frechét, independent across locations,

$$F(z_{ijgo}) = e^{-T_i z_{ijgo}^{-\varepsilon_g}} \tag{1.7}$$

where $T_i > 0$ is the average utility living in block i, and $\varepsilon_g > 1$ is the dispersion parameter of the idiosyncratic utility and varies by skill group.

Consumers face the following budget constraint:

$$c_{ijgo} + Q_i h_{ijgo} \leq w_{jg} \tag{1.8}$$

where Q_i is housing cost at location i and w_{jg} is wages received from working at j for worker of skill g. Here the assumption is that the only object a worker values at the workplace tract is the wage, which is her only source of income.

The utility form in (1.6) leads to indirect utility,

$$u_{ijgo} = \frac{z_{ijgo} B_{ig} w_{jg} Q_i^{\beta-1}}{d_{ij}} \tag{1.9}$$

which, combined with Frechét preference shocks, leads to the following commute probability equations,

$$\pi_{ij|g} = \frac{T_i(d_{ij}Q_i^{1-\beta})^{-\varepsilon_g}(B_{ig}w_{jg})^{\varepsilon_g}}{\sum_{r=1}^{S}\sum_{s=1}^{S}T_r(d_{rs}Q_r^{1-\beta})^{-\varepsilon_g}(B_{rg}w_{sg})^{\varepsilon_g}} \equiv \frac{\Phi_{ijg}}{\Phi_g} \tag{1.10}$$

$$\pi_{Ri|g} = \sum_{j=1}^{S}\pi_{ij|g} = \frac{\sum_{j=1}^{S}\Phi_{ijg}}{\Phi_g} \tag{1.11}$$

$$\pi_{Mj|g} = \sum_{i=1}^{S}\pi_{ij|g} = \frac{\sum_{i=1}^{S}\Phi_{ijg}}{\Phi_g} \tag{1.12}$$

$$\pi_{ij|ig} = \frac{\pi_{ij|g}}{\pi_{Ri|g}} = \frac{(w_{jg}/d_{ij})^{\varepsilon_g}}{\sum_s (w_{sg}/d_{ij})^{\varepsilon_g}} \tag{1.13}$$

where (1.10) is the probability that a worker of group g chooses to live in i and work in j, (1.11) is the probability that a worker lives in i, (1.12) is the probability that a worker works in j, and (1.13) is the conditional probability that a worker living in i chooses to work in j. Let L_{Mjg} denote count of workers of skill g employed in j and L_{Rig} denote count of workers of skill g living in i. Then,

$$L_{Mjg} = \sum_i \pi_{ij|ig}L_{Rig} = \sum_i \frac{(w_{jg}/d_{ij})^{\varepsilon_g}}{\sum_s (w_{sg}/d_{ij})^{\varepsilon_g}}L_{Rig} \tag{1.14}$$

I parametrize the disutility of commuting $d_{ij} = e^{\kappa_g \tau_{ij}}$ as standard in the commuting literature, where κ_g is the disutility parameter for group g and τ_{ij} is the distance between tracts i and j.

The reservation level of utility for group g is therefore,

$$\bar{U}_g = \Gamma\left(\frac{\varepsilon_g - 1}{\varepsilon_g}\right)\left[\sum_{r=1}^{S}\sum_{s=1}^{S}T_r\left(d_{rs}Q_r^{1-\beta}\right)^{-\varepsilon_g}(B_{rg}w_{sg})^{\varepsilon_g}\right]^{1/\varepsilon_g} \tag{1.15}$$

where $\Gamma(\cdot)$ is the Gamma function.

1.4.2 Production

I modify the production function by adding in a CES labor aggregator over skill groups. Final goods at location j is produced by a representative firm with technology,

$$y_j = A_j N_{Mj}^{\alpha} H_{Mj}^{1-\alpha} \tag{1.16}$$

$$N_{Mj} = \left(\sum_{g \in \{H,L\}} a_{jg} L_{Mjg}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}} \tag{1.17}$$

where N_{Mj} is the CES labor aggregator given by (1.17), σ is the elasticity of substitution between skill groups, and H_{Mj} is floor space used in production in j. Firms are perfectly competitive. The wage and labor demand at each location is given by

$$w_j = \left(\sum_g a_{jg}^{\sigma} w_{jg}^{1-\sigma} \right)^{\frac{1}{1-\sigma}} \tag{1.18}$$

$$L_{Mjg} = \left(\frac{w_{jg}}{a_{jg} w_j} \right)^{-\sigma} N_{Mj} \tag{1.19}$$

$$H_{Mj} = \left(\frac{w_j}{\alpha A_j} \right)^{1/(1-\alpha)} N_{Mj} \tag{1.20}$$

$$q_j = (1-\alpha) \left(\frac{\alpha}{w_j} \right)^{\alpha/(1-\alpha)} A_j^{1/(1-\alpha)} \tag{1.21}$$

where (1.18) is the wage index of location j, (1.19) relates the skill group labor demand to the total labor demand, (1.20) relates the labor demand with demand for floor space, the wage index, and productivity, and (1.21) is the floor price.

1.4.2.1 Production Spillovers Across Space

I incorporate production spillovers across space in the Hicks-neutral productivity term A_j,

$$A_j = a_j \Upsilon_{jH}^{\lambda_H} \Upsilon_{jL}^{\lambda_L} \tag{1.22}$$

$$\Upsilon_{jg} = \sum_s e^{-\delta_g \tau_{js}} \left(\frac{L_{Msg}}{K_s} \right) \tag{1.23}$$

where K_s is the land area of location s, so L_{Msg}/K_s is the density of employment of skill group g in location s. The spillover of nearby density to productivity decay with distance τ_{js}, with δ_L, δ_H governing the speed of decay for low- and high-skill workers nearby. Finally, λ_L, λ_H determine the contribution of low- and high-skill worker density on productivity in location j. In other words, a location's productivity benefits from a distance-weighted sum of different worker types across space.

In addition, there are no differential cross-skill spillovers but spillovers occur both to college and non-college workers at the same level. Fajgelbaum and Gaubert (2018) incorporate spillover effects on the share parameters of the CES labor aggregator, which incorporates differential cross-skill productivity spillovers. However, motivated by my descriptive exercise, I choose to model spillovers on the TFP term instead. So all spillovers from one worker type affects the whole production, and there are no type-differential benefit of one worker type on another.

1.4.3 Land Market

Floor space is produced by a competitive sector using land and capital as inputs. As in ARSW, I assume that the production function for floor space is Cobb-Douglas. With the assumption that capital prices are the same across tracts and cities, total floor space produced in i is $H_i = \varphi_i K_i^{1-\mu}$, where φ_i could be thought as the density of development. Land market imposes a no-arbitrage condition between residential use with price Q_i and commercial use with price q_i. Denoting the share of floor space for commercial use in location i as θ_i,

$$\begin{aligned} \theta_i &= 1 \text{ if } q_i > Q_i \\ \theta_i &\in [0,1] \text{ if } q_i = Q_i \\ \theta_i &= 0 \text{ if } q_i < Q_i \end{aligned} \tag{1.24}$$

and observed price is $r_i = \max\{q_i, Q_i\}$.

The floor space market clearing condition is therefore as follows,

$$\left(\frac{(1-\alpha)A_i}{q_i}\right)^{1/\alpha} N_i = \theta_i H_i \tag{1.25}$$

$$(1-\beta)\frac{\sum_g E[w_{sg}|i]L_{Rig}}{Q_i} = (1-\theta_i)H_i \tag{1.26}$$

where $E[w_{sg}|ig] = \sum_s \pi_{is|ig} w_{sg}$, with $\pi_{is|ig}$ defined by (1.13). The market clearing condition (1.25) comes from first-order conditions of the firm's profit maximization problem and (1.26) comes from first-order conditions of the utility maximization problem, taking expectation over the wage of residence area i.

1.4.4 Equilibrium

Given parameters $\{\beta, \kappa_L, \kappa_H, \varepsilon_L, \varepsilon_H, \alpha, \sigma, \lambda_L, \lambda_H, \delta_L, \delta_H\}$[11], reservation utilities $\{\bar{U}_g\}$, and exogenous location characteristics $\{H_i, T_i, B_i, \varphi_i, K_i, \tau_{ij}, a_{jL}, a_{jH}\}$, an equilibrium is defined by prices $\{w_{jL}, w_{jH}, q_j, Q_j\}$ and endogenous quantities $\{L_{RiL}, L_{RiH}, L_{MiL}, L_{MiH}, \theta_i, A_j\}$ that solve the following:

1. **Labor market clearing**: labor supply (commuting decisions) of each group (equation (1.14)) is consistent with labor demand for each group (equation (1.19))

[11] {Consumption share, disutility of commuting, Frechét parameters, labor share in production, elasticity of substitution between groups in production, spillover elasticities, spillover spatial decays}

2. **Floorspace market clearing**: commercial floor space market clearing condition in equation (1.25) and residential floor space market clearing in equation (1.26) hold and floor space allocation satisfies equation (1.24)

3. **Spatial Spillovers**: productivity is determined by equation (1.22)

4. **Spatial Equilibrium**: everyone receives their group's respective reservation utility level as in equation (1.15).

1.4.5 Recovering Endogenous Quantities

The central benefit of the model is to use the model inversion technique in ARSW to recover the endogenous (unobserved) model quantities of the model, $\{w_{jL}, w_{jH}, A_j\}$ (low-skill wage, high-skill wage, productivity) given data on $\{L_{MjL}, L_{MjH}, L_{RiL}, L_{RiH}, \tau_{ij}, K_s, q_i\}$, (Low-skill employment, high-skill employment, low-skill residence, high-skill residence, bilateral travel distance, area, and floor price). In the next section, I detail the inversion and estimation procedure for the model.

Since I am interested in the productivity spillovers in the production, for now I abstract from residential amenities. Because the productivity and residential spillover parameters are estimated independently, excluding residential amenities does not affect parameter estimates of productivity spillovers.

1.5 Estimation

In this section, I detail the procedure for estimating parameters without fully solving the model and describe instruments and their construction for the GMM estimation. I estimate the central parameters of interest—agglomeration externalities in the production by skill, λ_L, λ_H, and the decay of spillovers δ_L, δ_H—using GMM specification laid out in Section 1.5.1.5. However, for other parameters, I either use moments in the data (β), take estimates from the literature (α, μ, σ), or estimate them using model-derived specifications without fully solving for the model (ν_g, ε_g).

1.5.1 Model Inversion and Moment Condition

The model inversion allows me to recover unobserved variables of the model. At the final step, it allows me to form the moment condition for a GMM estimation procedure.

1.5.1.1 Obtaining $\{\nu_L = \kappa_L \varepsilon_L, \nu_H = \kappa_H \varepsilon_H\}$ for each city

I estimate the gravity equation for commuting to get ν for each city using LODES data, which provides commute flows between tracts. However, LODES does not have commuting flows by education so I cannot compute ν by education. Instead, I use the rough cuts of income group in LODES ($3,333+/month and $1,250 – $3,333/month) to first calculate the ratio of higher-to-lower income νs, then take the average of these ratios as a universal scaling factor.

The assumption is that 1) the relative commute semi-elasticity between college and non-college worker is constant across cities, and 2) the difference in the commute semi-elasticity between the cuts of income group can roughly estimate the relative commute semi-elasticity between college and non-college workers. The procedure gives a scaling factor of around 0.95, which suggests a lower commute semi-elasticity for college workers. This is consistent with Tsivanidis (2018), where he uses actual college and non-college commuting flow data from Bogotá, Colombia. So I estimate the gravity equation using all workers to obtain υ, set $\nu_L = \nu$, $\nu_H = \nu_L * 0.95$ for each city.[12] The semi-elasticities that are estimated from LODES has an average of about 0.04 across cities.

[12] As an additional verifying exercise, I calculate the relative commute elasticities between college and non-college workers using Zip Code commute flows in CHTS and find that the median value to be around 0.91.

1.5.1.2 Given $\{\nu_L, \nu_H\}$, get wages

From equation (1.14), for each $g \in \{H, L\}$,

$$L_{Mjg} = \frac{\left(w_{jg}^{\varepsilon_g} / e_{ij}^{\nu_g \tau_{ij}}\right)}{\sum_s \left(w_{sg}^{\varepsilon_g} / e_{is}^{\nu_g \tau_{ij}}\right)} L_{Mig} \tag{1.27}$$

I can recover $\omega_{jg} \equiv w_{jg}^{\varepsilon_g}$ uniquely up to a normalization via Lemma S.7 in ARSW.

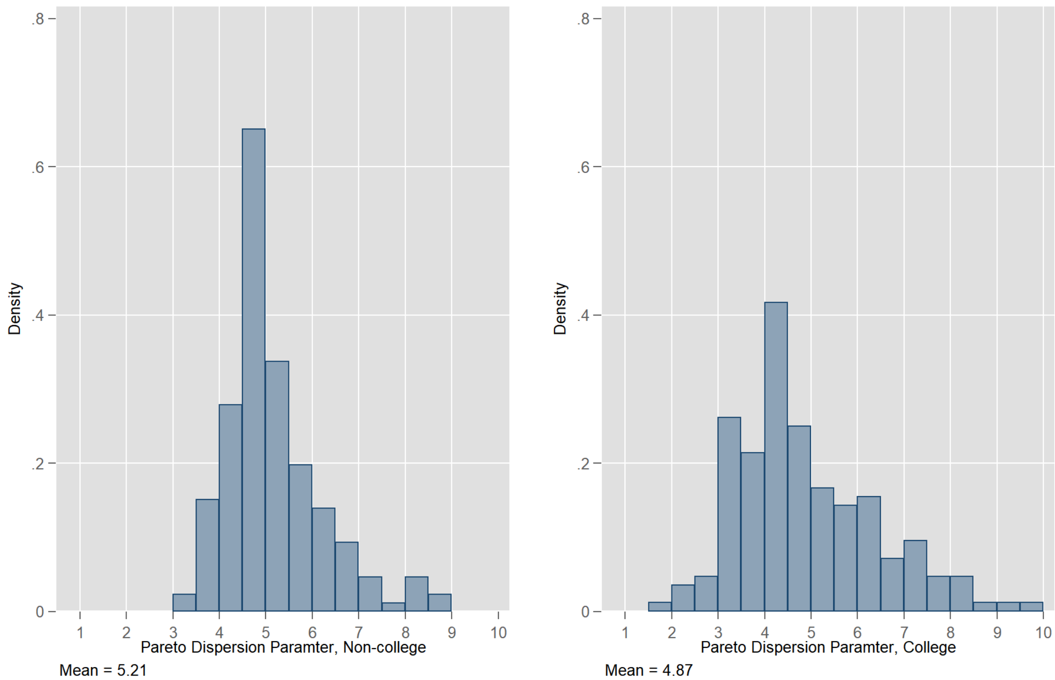
In order to obtain estimates of ε_g, I use the relation $Var(\ln \omega_{jg}) = \varepsilon_g^2 Var(\ln w_{jg})$ and so set the value of ε_g to make $(1/\varepsilon_g^2)Var(\ln \omega_{jg})$ equal to the variance of observed wages in the data for each city and each skill group, as in ARSW. The problem here is that I do not observe the variance of wages *across workplace* districts, which is what the model requires to compute ε. The approach I take is to use the dispersion of median wages across residential districts. The assumption I have to make is that the dispersion of residential district-level wages is similar to dispersion of workplace district-level wages.[13]

The Frechét parameters $\varepsilon_H, \varepsilon_L$ that I estimate from this method has an average of about 5.21 for non-college workers and 4.87 for college workers. This is consistent with educated workers having greater dispersion of residence-workplace pair choices in the literature, and therefore can serve as a validity check for the procedure. Figure 1.7 shows the distribution of the ε for college and non-college workers. An interesting pattern emerges: ε across cities for non-college workers is more concentrated than for college workers. This suggests that there is more across-city heterogeneity in the dispersion of college idiosyncratic shocks than for non-college idiosyncratic shocks.

A final normalization needs to be done for the recovered wages within cities. They are

[13] I could also assume a value of $\kappa = 0.01$ from Ahlfeldt et al. (2015) and Tsivanidis (2018) and calculate ε from estimated νs. However, I take this approach for following reasons. 1) I can use information on college and non-college wages separately. 2) The estimated epsilon values line up better with Ahlfeldt et al. (2015) (around 6.7). 3) Mean kappa values from this calculation is around 0.01 without imposing it. 4) Comparing residential log wage variance with workplace log wage variance from the Zip Code Business Patterns annual wages per employee, the average difference is about 17% of the variance. Therefore, I take the approach to be the best way out of the limited ways to get the values of epsilon.

Figure 1.7: Distribution of ε for College and Non-college Workers



Histogram of recovered ε_g parameters. Commute data was used to estimate the gravity equation to estimate the semi-elasticity of commuting. Then, ε_g were estimated using distribution of median wages across residences.

normalized such that the mean of each skill group's recovered wages in each city is equal to the observed counterpart in the data. This is necessary because the recovered wages are only unique up to a normalization.

1.5.1.3 Given $\{\alpha, \sigma\}$ and $\{w_{jg}\}$, recover normalized A_j

I can retrieve A_j as in ARSW by using equation (1.21),

$$q_j = (1-\alpha)\left(\frac{\alpha}{w_j}\right)^{\alpha/(1-\alpha)} A_j^{1/(1-\alpha)}$$

In order to recover the productivity terms A_j, I need to first recover the wage index w_j. The FOCs of the production-side yields the familiar wage index expression from a CES aggregator,

$$w_j = \left(a_{jL}^{\sigma} w_{jL}^{1-\sigma} + a_{jH}^{\sigma} w_{jH}^{1-\sigma}\right)^{\frac{1}{1-\sigma}} \tag{1.28}$$

I need to recover a_{jL} and a_{jH} to recover w_j. Given the recovered wages from the previous step, w_{jL}, w_{jH}, the FOCs with respect to high- and low-skill labor yield the following expression,

$$\frac{a_{jH}}{a_{jL}} = \frac{w_{jH}}{w_{jL}} \left(\frac{L_{MjH}}{L_{MjL}}\right)^{\frac{1}{\sigma}} \tag{1.29}$$

I normalize the share parameters such that $a_{jL} = 1 - a_{jH}$. The above equation yields an expression for a_{jH} given $\sigma, L_{MjH}, L_{MjL}, w_{jL}, w_{jH}$. With a_{jH} recovered, I can calculate w_j, and in turn I can recover A_j using equation (1.21).

1.5.1.4 Given $\{\lambda_g, \delta_g\}$ and A_j, recover a_j

From the productivity spillover equation (1.22),

$$a_j = A_j \Upsilon_{jH}^{-\lambda_H} \Upsilon_{jL}^{-\lambda_L}, \text{ where,}$$
$$\Upsilon_{jg} = \sum_s e^{-\delta_g \tau_{js}} \left(\frac{L_{Msg}}{K_s}\right)$$

The a_j terms are fundamental productivities of location j and are the structural residuals of the model. These are the central terms that are used to form the moment conditions for estimation.

1.5.1.5 Constructing Moments

The above procedure allows us to express the normalized structural residuals a_j in terms of observed data and model parameters for all years and all locations in the data. I use the time-differenced and demeaned expression:

$$\Delta \log \frac{a_{jt}}{\bar{a}_t} = (1-\alpha)\Delta \log \frac{Q_{jt}}{\bar{Q}_t} + \alpha \Delta \log \frac{w_{jt}}{\bar{w}_t} - \lambda_H \Delta \log \frac{\Upsilon_{Hjt}}{\bar{\Upsilon}_{Ht}} - \lambda_L \Delta \log \frac{\Upsilon_{Ljt}}{\bar{\Upsilon}_{Lt}} \tag{1.30}$$

where $\bar{x}_t$ denotes geometric mean over space.[14] This equation is an augmented version of ARSW with measures of density in surrounding areas for both college and non-college workers. The expression in (1.30) could be re-written as:

$$\alpha \Delta \log \frac{w_{jt}}{\bar{w}_t} = \lambda_H \Delta \log \frac{\Upsilon_{Hjt}}{\bar{\Upsilon}_{Ht}} + \lambda_L \Delta \log \frac{\Upsilon_{Ljt}}{\bar{\Upsilon}_{Lt}} - (1-\alpha)\Delta \log \frac{Q_{jt}}{\bar{Q}_t} + \Delta \log \frac{a_{jt}}{\bar{a}_t} \tag{1.31}$$

The error term $\Delta \log \frac{a_{jt}}{\bar{a}_t}$ is the fundamental productivity of tract j that does not depend on surrounding density of college and non-college workers. It is similar to the error term in equation (1.5) in the OLS estimation in the previous section. We need instruments that are correlated with the distribution of workers but uncorrelated with the fundamental productivities. However, due to the imposed structure of the model that allows me to recover the adjusted wages at each location, I can use multiple years that allows me to difference out the common city-level terms (from the geometric mean in the denominator) and difference over time. This makes the identification conditions weaker, at the expense of imposing model assumptions.

Given a set of instruments Z, we can form the following moment conditions:

$$E\left[Z \cdot \Delta \log \frac{a_{jt}}{\bar{a}_t}\right] = 0 \tag{1.32}$$

Note that because the wage and density measures are both non-linear functions of the data and model parameters, these moment conditions constitute a non-linear GMM estimating equation. Denoting the moment functions as $m(X_i, \Theta)$ with inputs of data X_i for observation

[14] Units are determined from the choice to set the geometric mean of floor space and wage index equal to one in New York CBSA.

(tract) i and parameters $\Theta = (\delta_L, \delta_H, \lambda_L, \lambda_H)$, the GMM estimator $\hat{\Theta}$ solves,

$$\hat{\Theta} = \arg\min \left(\frac{1}{N} \sum_i m(X_i, \Theta)' \right) W \left(\frac{1}{N} \sum_i m(X_i, \Theta) \right) \tag{1.33}$$

where W is the GMM weight matrix. I use a two-step GMM procedure, first estimating the parameters using an identity matrix as the weight matrix, then using the efficient weight matrix calculated at the parameter values in the first step. Finally, White heteroskedasticity and autocorrelation consistent (HAC) variance-covariance matrix of the parameter estimates are used to calculate the standard errors on the parameters.

I use three types of variables as instruments in the GMM estimation. First, I use Bartik-instruments using industry shares in 1994 from Zip Code Business Patterns (ZBP) data and growth in college employment, non-college employment, and college share in each industry. More specifically,

$$Bartik_{Lj} = \sum_{ind} \frac{Emp_{j,ind,1994}}{TotEmp_{j,1994}} (L_{ind(-c)2010} - L_{ind(-c)1990}) \tag{1.34}$$

where L_j is low-skill employment. Similarly, for H_j, high-skill Bartik is calculated.[15]. The requirement for instrument validity here is that 1) the industry shares in 1994 are uncorrelated to changes in tract-level productivity from 2010-2015, while still being relevant for the distribution of high- and low-skill workplaces and 2) the industry-level growth shock in college or non-college workers outside the city is uncorrelated with tract-level productivity changes from 2010-2015.[16]

For (1), to the extent that there are industry-share correlates that persist through the estimation period, 2010-2015, the estimates may be biased. However, unlike other literature

[15]For growth in college share, I use $\frac{H_{ind(-c)2015}}{H_{ind(-c)2015}+L_{ind(-c)2015}} - \frac{H_{ind(-c)2010}}{H_{ind(-c)2010}+L_{ind(-c)2010}}$

[16]There currently is a lively debate about Bartik instruments, whether the identification assumption should be placed on the shares or on the shocks. See Borusyak et al. (2018) and Goldsmith-Pinkham et al. (2018). I do not take a stance here, and assume that both needs to hold for the instrument to be valid.

utilizing Bartik instruments, I use the *past* shares of industry composition, not the baseline shares. This means that industry mix 15 years ago in a tract must have effects that cannot be differenced out between 2010-2015. That is a strong assumption, unless there is a persistent shock that is changing over time and correlated with the past industry shares. For (2), even though the mechanical correlation own-city growth is removed, if there are shocks that are correlated across the U.S., then the instruments may not satisfy the exclusion restriction. However, for this to happen, the national shocks must be correlated differentially across tracts within the city, as the city-level variation is absorbed by the geometric mean.

Second, I use city-level historical residential segregation in 1980 as an instrument. The idea is that historical residential segregation affects the distribution of workers through the combination of residential choices and commute costs, but does not affect the changes in residual *productivity* of individual tracts. It is unlikely that historical residential segregation will have a direct effect on production fundamentals. Additionally, there indeed is a relationship between historical residential segregation and current workplace segregation, with a coefficient of about 0.2 and F-stat of about 17 (not shown). This provides evidence that historical segregation is related to the current distribution of workplace location.

Third, in some specifications I use elevation within tracts as another instrument. The idea comes from Saiz (2010), who uses the fact that the share of land above 15% slope is deemed "undevelopable" by architectural guidelines and therefore restricts density in these areas. Therefore, I calculate for each tract the share of land that is above 15% slope. In essence, the elevation instruments capture the extent to which the tracts can sustain a level of density. The assumption then is that this pre-determined geographical feature does not affect changes in productivity of locations over-time, while affecting the distribution of workers via available density. In the next section, I describe the data I use for estimation.

1.5.2 Data

For model estimation, I need tract-level data on the following: 1) workplace and residence counts of workers by education level, 2) floor price, 3) land area, and 4) distance between tracts. For instrument construction, I need historical shares of industry, industry-level growth rates in college and non-college employment, and elevation. In this section, I detail the sources of data.

As before, I use LODES data for information on the residence and workplace counts of workers by educational attainment and tract location. Consistently with the previous sections, I designate workers without bachelor's degree as non-college, or low-skill, workers and those with bachelor's or above as college, or high-skill, workers. I take two years of data, 2010 and 2015, and use Primary Jobs as the measure of workplaces, which tries to measure workers, as one worker could potentially hold multiple jobs. Massachusetts and Wyoming are missing 2010 and 2015 data, respectively, so I exclude CBSAs from the two states in my analysis. Additionally, I only include cities with Census 2000 population more than 500,000. The final count of CBSAs is 89.

There are two crucial limitations of the LODES data. First, except for Minnesota, unemployment insurance data only contain information to the State Employer Identification Number (SEIN) level, which is insufficient to link workers to establishment location for multi-establishment firms. Therefore, for multi-establishment firms, LEHD imputes links of workers to establishments by first linking workers to firms, then distributing those workers across establishments using a firm-establishment manifest and a probabilistic matching model. Second, some demographic data such as education is also missing in the state unemployment insurance files. Therefore, LEHD links about 1/6 of workers to the Census and imputes the educational attainment for the remainder. Details of the imputation procedures is available in Abowd et al. (2005). Despite these imputations, comparing LODES to other available sources of data for college and non-college residence/workplace distribution shows high degrees of correlation.[17] Additional data on residence locations by education for 1970-2010 is provided by Geolytics. Geolytics compiles Decennial Census long-form data (for 1970-2000) and ACS 5-year data (for 2010) into consistent-boundary tract-level data, matched to tract definitions in 2010. The historical data is used to construct historical residential segregation indices of dissimilarity.

I also obtain tract-level floor price, demographics, and income information on residents from Census Bureau's ACS 5-year Summary Files for years 2010 and 2015 by using the 5-year files from 2006-2010 and 2011-2015, respectively. These files contain data at the tract-level. I also obtain tract-level residential median income for each major educational group (high school dropout, high school graduate, some college, bachelors and above). For floor price data, I use median rent at each tract. Because I need information on floor price of housing, the ideal data would include the housing rent or value per square feet. However, that data is not available at the tract-level. In order to mitigate concerns that the median rent is uncorrelated to rent per square feet, I compare the measure to average price per room, calculated by using aggregate rent and aggregate rooms by tract from the ACS. The idea is that I can use the total number of rooms in each tract as a proxy for total housing area. The median rent measure and the housing rent per room measure are highly correlated at around 0.91, so I use the median rent measure instead because it has better coverage than aggregate rent and rooms. Additionally, I compare my floor price measure to Zillow's Median Rent Index Per Square Feet by ZCTA. I aggregate the tract-level floor price measure from the ACS to the zip code-level and compare the measure to Zillow's measure, and find a high correlation of 0.77. Therefore, it seems that median rent can sufficiently capture the patterns of floor price per square feet.

[17] To mitigate the concern that the imputations are causing significant issues in data quality, I compare LODES residential data to the ACS, and workplace data to the CHTS, which is limited to California. Correlation coefficients are presented in the Appendix, Table 1.A.2. Overall, there is a high degree of correlation between measures provided by LODES and alternative sources. ACS-LODES correlation is 0.85 for college and 0.79 for non-college residents, while CHTS-LODES correlation is 0.86 for college and 0.71 for non-college workplaces.

For instrument construction, I use Zip Code Business Patterns (ZBP) data to calculate the industry shares in each zip code in 1994. I use 3-digit SIC codes (and the equivalent 4-digit NAICS codes) to define an industry. Because the ZBP data in 1994 uses SIC codes while the newer data sources use NAICS codes, I map the 3-digit SIC code to 4-digit NAICS codes. For increase in college and non-college employment in these respective industries, I use ACS 1-year data from University of Minnesota's Integrated Public Use Microdata Series (IPUMS) for years 1990, 2000, 2010, and 2015. For tract-level elevation data, I obtain the share of area above 15% slope by processing NASA's 1-arcsecond Digital Elevation Model (DEM) data. The data contains information on elevation at the 30m×30m grid-level, which is finer than what Saiz (2010) used for his calculation (3-arcsecond or 90m×90m).

I use three sources of data for geography. First, GIS shapefiles for various Census geographies are obtained from Census Bureau's TIGER products. TIGER also provides interior points (latitude/longitude), land area, and water area of each geography. These shapefiles and geography data are used for GIS processing and calculation of various statistics. Second, concordance between different levels of geographies are obtained from University of Missouri Census Data Center's Geocorr 2014 database. Third, Zip Codes to Zip Code Tabulation Area (ZCTA) concordance is obtained from UDS Mapper to match Zip Codes in ZBP to ZCTA which is used by rest of the data sources.

1.5.3 GMM Estimation

I first need to set values for parameters that I am not estimating from the full model. First, the housing share of consumption, $(1 - \beta)$, is set at 0.25. This value comes from spending share on shelter, household operations, and housekeeping supplies from the 2010-2011 Consumer Expenditure Survey (CES). I set a constant value across skill groups because the share of consumption on housing for various education groups are stable in the in the CES. Because CES only has separate table for 21 cities, data limitations prevent me from setting a different value for each city. However, the standard deviation of the share on 21 cities that

Table 1.4: Paramter Estimates from GMM Estimation

	Preferred	Robustness Checks	
Parameter	(1)	(2)	(3)
δ_L	1.179***	1.98	0.5938***
	(0.423)	(2.419)	(0.171)
δ_H	0.882***	0.8758*	0.7831***
	(0.197)	(0.543)	(0.244)
λ_L	-0.4415***	-0.3653***	-0.502***
	(0.078)	(0.1722)	(0.0763)
λ_H	0.497***	0.5225***	0.5186***
	(0.078)	(0.2175)	(0.111)

White HAC standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Preferred estimates in Column (1) use instruments of historical residential segregation and Bartik instruments. Column (2) use instruments of residential segregation and static Bartik instruments with employment at 2010 instead of employment growth from 1990-2010. Column (3) uses the current specification with elevation instruments.

are available is only about 3 percentage points. Also, the value is consistent with estimates from Davis and Ortalo-Magné (2011). I set the floor space share in production, $(1-\alpha)$, to 0.2. This value comes from ARSW, who get the number from Valentinyi and Herrendorf (2008). Finally, I set the elasticity of substitution between high- and low-skill workers, σ, to 1.3, from Card (2009), as in Tsivanidis (2018). As described in the previous section, Frechét dispersion parameters ε_g, and commute disutilities κ_g are estimated separately.

I construct the moments in Section 1.5.1.5 and use MATLAB's `patternsearch` algorithm to estimate the parameters. To verify that the estimates are not sensitive to initial guesses, I run the estimation algorithm for various initial values within the range of parameter values suggested by the literature. I confirm that the resulting estimates are robust to starting points.

1.5.4 Results

Results from the GMM estimation are presented in Table 1.4. In column (1), I present my preferred estimates using historical residential segregation and Bartik instruments. Columns (2) and (3) present results from robustness checks with alternative instrument specifications.

In all specifications, the elasticity of productivity to density measures, λ_L, λ_H, is negative for non-college density and positive for college density, just like in the previous regressions. These parameter estimates are actually large compared to traditional agglomeration estimates using total employment density. For example, Ahlfeldt et al. (2015) estimates range around 0.07 for all workers and Berkes and Gaetani (2019) estimates an elasticity of 0.12 for "knowledge-intensive sectors". However, when college and non-college elasticities are combined, the two elasticities form an estimate of about 0.06, which is in line with estimates of agglomeration in the city-level literature and with estimates from Ahlfeldt et al. (2015).[18]

In addition, the decay parameter for both college and non-college workers is very high, implying a very fast decay rate. In Panel A of Table 1.5, I document the implied elasticity of density on production spillovers for each mile from a given tract using the preferred estimates. For example, the row with mile 3 has numbers 0.026 for college and -0.013 for non-college employment. This implies that if employment of college workers 3 miles away increases by 1% holding worker count in other distances fixed, productivity of the tract increases by about 0.026%; if employment of non-college workers 3 miles away increases by 1%, productivity of the tract decreases by about 0.013%.

These numbers show how quickly externalities fade away as distance increases. We can see that by mile 2, the effect of increase in college worker density more than halves compared to the effect in mile 1. Moreover, the effect goes to virtually 0 by mile 5. For non-college externalities, the decay is even faster, disappearing to almost 0 by mile 3. This decay is in line with the preferred estimate from Ahlfeldt et al. (2015) for all workers in Berlin, where their estimate implies production externalities decaying to almost 0 by 15 minutes of travel time, which is about 3 miles of straight-line distance in the United States by automobile. Their higher-end of the estimate of overall decay from total employment is in line with my college decay speed.

[18]This value would come from an equal percentage rise in the density measure.

Table 1.5: Implied Elasticity Decay over Distance

Panel A: Elasticity		
Mile	Col	Non
1	0.153	-0.135
2	0.064	-0.042
3	0.026	-0.013
4	0.011	-0.004
5	0.005	-0.001

Panel B: Comparison		
	OLS	
0-3	0.092***	-0.086**
3-5	0.025	-0.035
	Model-equiv	
0-3	0.151	-0.136
3-5	0.017	-0.008

Panel A shows the distance decay of 1% increase at each mile employment, assuming that initial employment is distributed uniformly over distance. Panel B compares the descriptive regression estimates (Table 1.2 column 1) to the model estimates, where Model-equivalent estimates are from sums of elasticity parameters of each mile parameters.

The decay parameter estimates imply faster decay than what was estimated in the reduced-form regressions. Panel B of Table 1.5 presents the estimates from 0-3 and 3-5 mile rings again, and compares with a crude measure that is model-equivalent by averaging the effects within the bin. For example, for 0-3 mile ring, I take the elasticity effects of miles 1, 2, 3 and take the average of the three to get the model-equivalent effect.[19] We see that the model-equivalent and OLS elasticities are similar for 0-3 miles. The model-implied elasticities falls sharply below the OLS estimates by the outer ring, although the OLS estimate is not statistically significant.

These parameter estimates are somewhat robust to alternative instrument specifications. I run two additional specifications 1) with residential segregation instrument and static Bartik, where I use the employment at year 2010 instead of growth of employment, and 2)

[19]Starting from 1 mile is appropriate because most tracts in the descriptive estimates have tracts with significant employment starting at the 1-mile radius

my current specification with elevation instruments. The college decay parameter δ_H stays stable with a range of 0.78-0.88 and college elasticity parameter λ_H also stays stable with a range of 0.49-0.52. Non-college elasticity parameter λ_L is less stable, with a range of -0.37 to -0.5 but are within reasonable bounds. Non-college decay parameter δ_L, however is the least stable, ranging from 0.6-1.9, with my preferred estimate being around the middle. Still, the estimates imply the same qualitative results with positive spillovers from nearby college density, negative spillovers from nearby non-college density, and fast speed of decay.

1.5.5 Over-Identification Checks

In this section, I evaluate the fit of the model against moments in the data that I did not use for estimation. First, I compare the recovered wages obtained from the model to the crude data on wages per employment in ZBP data. ZBP contains data on the total first quarter payroll as well as total mid-march employment of businesses for each zip code. Taking the ratio of the payroll to employment gives a measure of wage per job in the particular zip code. To compare this measure to the model, I take the tract-level recovered wages from the model, link the tracts to ZCTA, and compute a measure of total wages by computing the weighted sum of college and non-college recovered mean wage in the ZCTA. I then plot the standardized measures of the two variables in Figure 1.8. Overall, although they do not coincide perfectly, they have a high degree of correlation (0.51). It seems that the model-recovered wages can replicate the data quite well.

Another over-identification check that I do is to compare the actual floor space density in tracts with the model-implied density. I have information on approximate floor space in four areas: New York City, Chicago, Philadelphia, and the county of Los Angeles.[20] In New York, Philadelphia, and Los Angeles County, I obtain data on the height and area of building outlines. I use $height * area$ as a measure of total available floor space of the

[20] These are not CBSAs, but rather the city or county boundaries. Together, these three cities and one county constitute the center of 4 out of top 5 CBSAs by 2010 population.

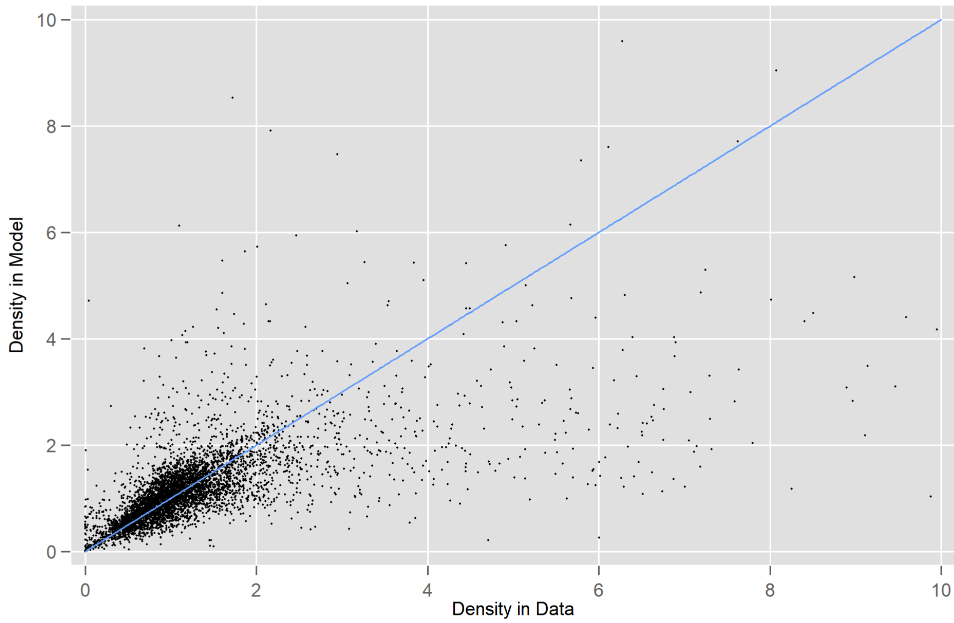
Figure 1.8: Over Identification Check: Wages

Plots wages per employment in Zip Code Business Patterns (Wages in Data) against recovered wages in the model (Wages in Model). 45-degree line is also added for comparison. Correlation between two measures is 0.51.

building. In Chicago, I obtain data on the number of stories and area of building outlines. I use $(\#of stories) * area$ as a measure of total available floor space of the building. I match each building to tract, sum all the building floor space within the tract and divide by tract area to get a measure of floor space density.

These measures are not perfect measures of floor space: the actual floor space available can only be measured using information on each floor's area of a building. For New York, Philadelphia, and Los Angeles County, I assume that building height proxies for number of floors. For both all cities, I assume that the area of usable space is the same for each floor. In reality, each floor on a building can have different areas of usable space. Nevertheless, Figure 1.9 shows that the model measure and the data measure of density line up relatively well with a correlation coefficient of 0.59. To mitigate the concern that the three geographies have different information on buildings in the data, I present separate figures for the areas

Figure 1.9: Over Identification Check: Floor Space



Plots model-implied density of floor space (Density in Model) and available data measure of floor space by area in New York City, Chicago, and Los Angeles County (Density in Data). Both are normalized by their geometric mean within geography. Correlation between two measures is 0.59. The figure plots only data points below 10 for each measure for visibility, but the correlation coefficient is computed using whole data. The cutoff excludes 64 (1.2%) observations from the plot.

in Figure 1.A.2. The results hold separately as well.

Using the preferred estimates, I now utilize the model and run a policy experiment to analyze a Los Angeles Policy to increase density and improve productive externalities within the city.

1.6 Counterfatual Analysis

In 2016, the City of Los Angeles completed the construction of new Metro stations on their light rail transit, the Exposition Line (Expo Line). Along with the new stations, Los Angeles proposed to re-zone areas around the newly built Exposition Metro Line stations to allow for higher-density commercial, industrial, residential developments to attract a high-skill workforce to these areas. The resulting Exposition Corridor Transit Neighborhood Plan (henceforth Expo Plan) was approved and finalized in 2018. In this section, I use my model and parameter estimates to simulate the impact of this plan on productivity, floor space prices, employment, and residence in the targeted and surrounding areas under various scenarios of development.

From the viewpoint of the model, the Expo Plan does two things. First, it increases the density of development of the targeted tracts. Second, it shifts the composition of the workforce in targeted tracts toward college employment. The first effect can be thought of as an increase in the density of development parameter φ_j, while the second effect can be thought of as in increase in the college share parameter in the CES aggregator of each location, a_{jH}.

To assess the impact of the two effects separately, I first simulate a policy where I only increase the density parameter in the targeted locations. Then, I increase both the density parameter and the college share parameter to examine the total effect of the policy. Lastly, because I implement the policy simulation by changing the exogenous parameters φ_j and a_{jH}, I compute multiple counterfactuals by changing the parameters by different amounts to quantitatively assess the effect of the policy under different scenarios of development.

1.6.1 Details of Exposition Corridor Transit Neighborhood Plan

The Expo Plan implements two different urban policy instruments. First, the Plan significantly increases maximum floor-area ratios (FAR), which determines the number of floors that is allowed per area of plot.[21] Increasing FAR therefore allows denser development in the target areas. Second, it is re-zoning the target areas to permit office spaces, different from what is currently allowed. Last, it is giving "density bonuses" to developers who include

[21] By "per area of plot", I mean given the area of land and the building coverage ratio (BCR). Fixing the area of land and BCR, increase in FAR determines the increase in floor density

Table 1.6: New Zoning Policies

Current Zoning	**New Zoning**	**Max FAR**	**Purpose**
Light Industrial	Hybrid Industrial	1:1.5 $\rightarrow$ 4	Attract skill-intense jobs
General Commercial	Community Commercial	1:1.5 $\rightarrow$ 3.6	Increase live-work space

Summary of the zoning changes in the Expo Plan.

affordable housing units within the new developments.[22] Figure 1.10 shows the geographic extent of the proposed policy.

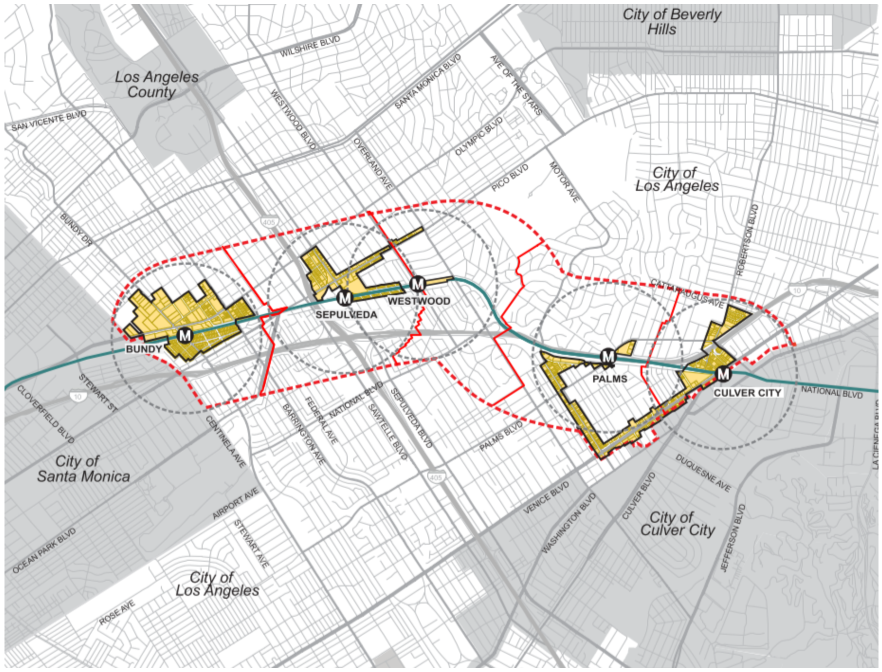
More specifically, the Expo Plan targets two types of areas for re-zoning: areas currently zoned Light Industrial and General Commercial. Both area types currently have a low FAR of 1.5, which means that for every 1 square mile of buildable area, the permissible amount of floor space is 1.5 square miles. An example target area can be seen in Figure 1.11. This area is currently occupied by low-rise warehouse buildings and low-density businesses such as gas stations. A summary of new zoning regulations compared to old regulations is listed in Table 1.6. The city expects to attract an additional 3,369 units of housing, 7,124 residents, 10,521 jobs, 3,230,060 of non-residential square footage by 2035.[23]

The counterfactual equilibrium is computed using the technique in ARSW. First, change the relevant location-specific fundamentals of the model. Then, calculate equilibrium objects with the changed fundamentals, starting as guesses the values of endogenous variables from the observed data. Update the guess as a weighted average of the new equilibrium values and the previous equilibrium values, with a larger weight on the previous guess. Repeat until the old and new values converge. This algorithm gives the interpretation that we are selecting the new equilibrium that is closest to the observed equilibrium. As described above, the model fundamentals that I am interested in changing are 1) the density of development, φ_j,

[22]I do not explicitly model the density bonus policy. The policy's guarantee to allow higher FAR for developments with affordable housing units can have two effects. It can mitigate the effect of house price increases in the surrounding area, while depressing the house price in the target tracts even further.

[23]Source: "Exposition Corridor Transit Neighborhood Plan" (Terry A. Hayes Associates Inc. 2017).

Figure 1.10: Targeted Tracts and Re-zoning



Highlighted areas of proposed change, with "Project Area" boundary in dotted red. Source: "Environmental Impact Report", Los Angeles Transit Neighborhood Plan.

and 2) the college share in the CES labor aggregator, a_{jH}.[24]

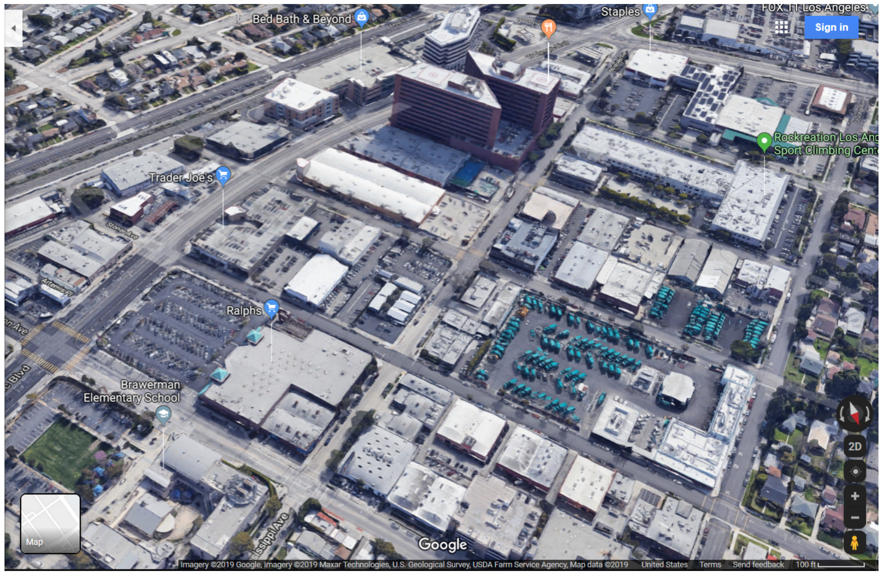
1.6.2 Policy Simulations

In accordance with the proposed plan, I conduct policy simulations under three different scenarios. First, I increase the density of development parameter in the proposed targeted tracts by $4/1.5 \approx 2.6$ times in current Light Industrial Zones and by $3.6/1.4 = 2.5$ times in current General Commercial Zones.[25] Second, in addition to increasing the density of development parameter, I increase the college share parameter in the labor CES aggregator to increase the college to non-college employment ratio by 50% in current Light Industrial Zones. This captures the effect of replacing warehouses with office spaces suitable for higher

[24] The share of density devoted to commercial use (θ_j) is endogenous and left to vary according to the model.

[25] Ratio of new to old FAR.

Figure 1.11: Current View of an Example Area Zoned Light Industrial



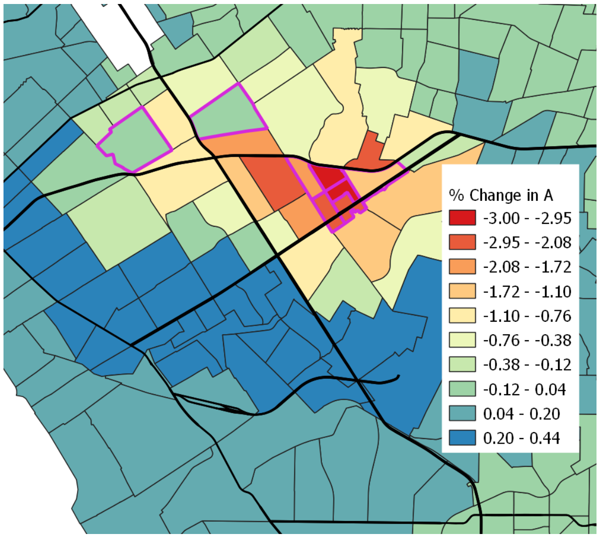
Area around Barrington Ave. and Olympic Blvd. in Los Angeles, CA. This area is currently zoned Light Industrial and will be rezoned Hybrid Industrial. Map data: Google, Maxar Technologies, U.S. Geological Survey, USDA Farm Service Agency.

college-share workplaces. The interpretation of an increased college share parameter in the CES function is a change in the production technology favoring college labor input. Since the current General Commercial Zones do not benefit from change in land use but only in increased density, I keep the college share parameter the same in these zones.

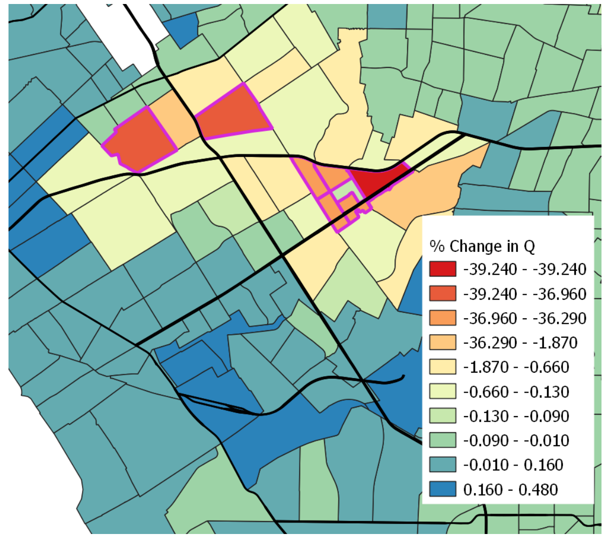
Figure 1.12 shows the results of the first exercise, where I only increase density in the target areas. The targeted tracts are in purple borders. The immediate affect of increasing the density parameter is the change in employment and residence of the targeted tracts. I take as an example Tract 2676.00 by Bundy Ave and Exposition Ave, which is the Northwest targeted tract and is also the example tract shown in Figure 1.11. As can be seen in Panels 1.12c and 1.12d, there is a large increase of college workers (40%) and non-college workers (42%) in the tract. In addition, as shown in Panel 1.12b there is a decrease in floor space price (-39%), driven by a large increase supply.

Panel 1.12a shows the decrease in productivity in the tract and surrounding areas. There

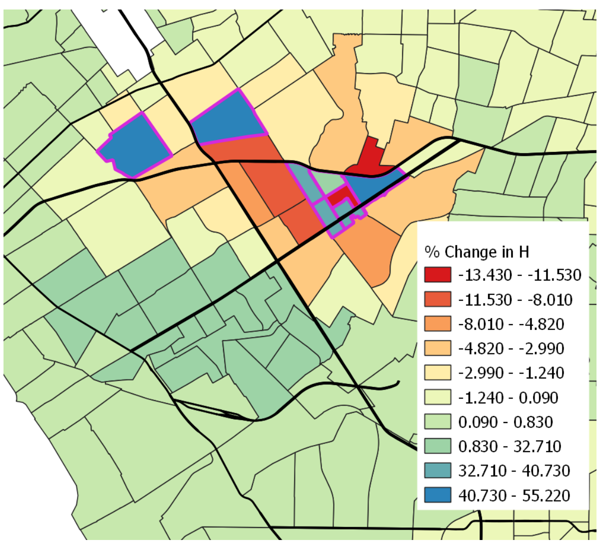
Figure 1.12: Policy Simulation with Only Density Increase



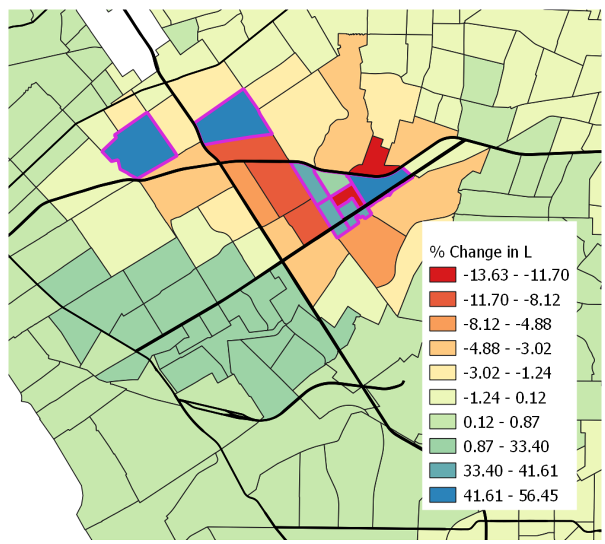
(a) Productivity



(b) Floor Price



(c) College Employment



(d) Non-college Employment

The effect of the policy simulation with only density increase to the maximum FAR on each tract's (a) productivity, (b) floor price, (c) college-educated worker employment, and (d) non-college worker employment. Purple boundaries indicate the targeted tract.

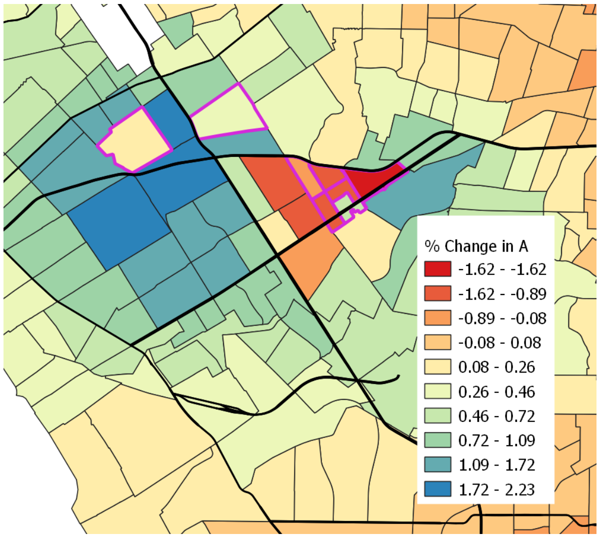
is very little productivity spillover effect of such a policy. In fact, while there are negative effects in the targeted tracts, in the surrounding tracts there is almost no effect. This is because these zones are currently occupied by low-skill workplaces in the observed equilibrium so the increase in density of these locations lead to a higher count of low-skill workers relative to high-skill workers. Due to the negative spillover effects, this increased density of low-skill workers in the target tracts decreases the productivity of the immediately surrounding areas. However, the overall magnitude of the spillovers are very small due to the fast decay of non-college workplace density spillovers.

Figure 1.13 shows the results of a policy increasing both the density of development and the share parameter for college workers in the CES function. Here, as before, the density of development parameter is increased by the amount of FAR increase and the share parameter is increased by amount that implies a 50% increase in college to non-college employment ratio. It can be immediately noticed that 1) the effect on the targeted tract is different from the previous policy experiment, and 2) the spillover effects are much larger, coming from increased college worker density.

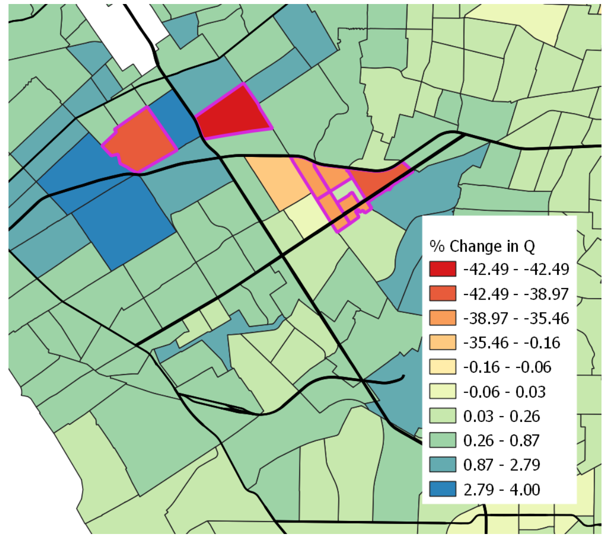
In targeted tracts, we see a large shift of worker composition from non-college to college workers. Going back to our example tract 2676.00, there is around a 50% increase in college workers, but only a 7% increase in non-college workers. However, resident composition is preserved and there is an equal percentage increase of college and non-college residences (not shown). Floor prices fall at the targeted areas again (42%). There are also pronounced spillover effects affecting tracts that are not directly targeted. For example, one adjacent tract 2712.00 next to our example tract experiences around 30% rise in both college and non-college employment, while a 5% increase in both college and non-college residence. The share of floor space for commercial use increases by 1 percentage point, and college and non-college wages go up by 2%. Housing prices rise by 3%.

Overall, these policy experiments suggest that indeed, policies that merely increase the density of development are not enough to produce meaningful spillover effects, unless these

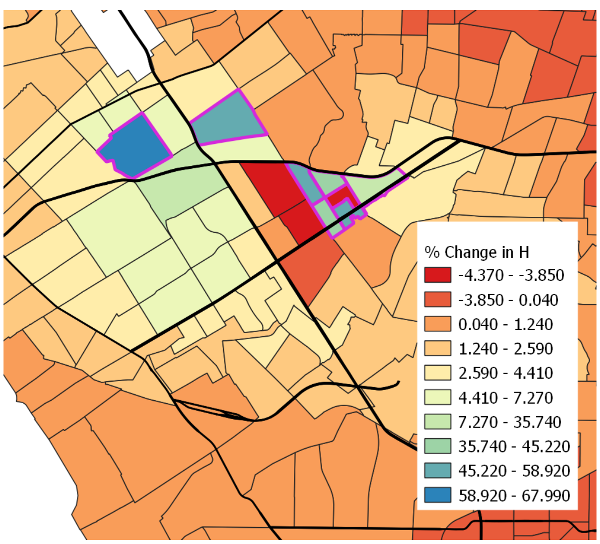
Figure 1.13: Policy Simulation with Both College Share and Density Increase



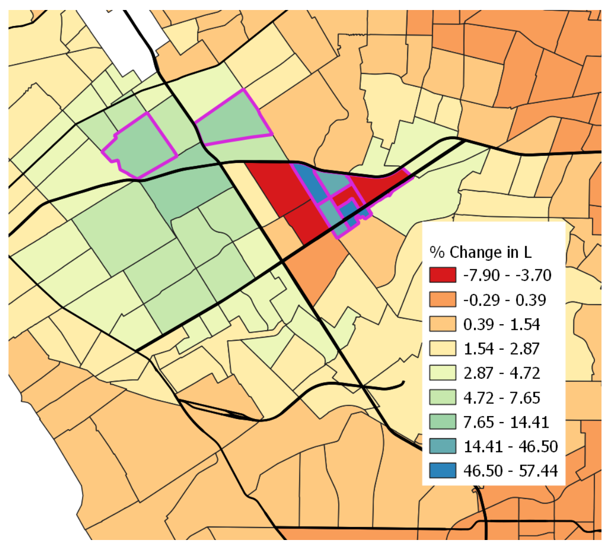
(a) Productivity



(b) Floor Price



(c) College Employment



(d) Non-college Employment

The effect of the policy simulation with both density increase to the maximum FAR and change in share of college workers in tracts re-zoned to Hybrid Industrial on each tract's (a) productivity, (b) floor price, (c) college-educated worker employment, and (d) non-college worker employment. Purple boundaries indicate the targeted tract.

areas are already occupied by a large share of college workers. When incorporating policies that increase the density of areas, it is best to either target locations with a healthy mix of college workers or be used in combination with land use policy to attract college workplaces.

The counterfactual policy simulations presented here are limited in a few ways. First, the density policies pertain to "allowable" density via zoning regulations, not the actual density of development. However, in the simulations I assume that the target areas get developed to the fullest allowable extent. In reality, the actual density will be an endogenous decision made by developers, dependent on the demand for office spaces in the area. Similarly, the extent to which the new developments in the policy area will be occupied by college jobs also depend on the endogenous decisions of firms, while in the policy exercises, these values are assumed. To address the two concerns, I simulate the policy for different values of density of development and college share increase and characterize the quantitative effect of the policy under different scenarios. Specifically, I conduct experiments with some combination involving: 1) density increase to maximum of the FAR increase, 2) density increase to half of the FAR increase, 3) college-to-non-college employment ratio increase by 50%, and 4) college employment ratio increase by 25%.

Table 1.7 summarizes the effect of different policy scenarios on changes in college employment, non-college employment, average productivity, college wages, and non-college wages for each policy scenario in the project area.[26] It also includes the City's projection on total employment increase. The simulations show that different assumed increases in fundamentals have non-monotonic effects. For example, fixing the college share increase by 50%, comparing the scenarios with half the density increase (fourth row) and full density increase (third row), we see that the average productivity increases slightly more in the case with half the increase. This is because the smaller density increase limits the absolute increase of non-college workers, in turn producing less negative spillover effects and causing a larger average productivity gain.

[26]Project area includes neighboring tracts as well as the target areas shown in Figure 1.10.

Table 1.7: Policy Simulation Effect on Employment and Productivity in Project Area

	Effect On				
Simulated Policy	Col Emp	Non Emp	Avg Prod	Col Wage	Non Wage
Density Max	21.78	21.32	-0.66	5.47	4.96
College Ratio by 50%	5.42	-7.58	0.92	1.70	-2.78
Density Max + College Ratio 50%	30.08	9.52	0.68	7.73	2.16
Density Half + College Ratio 50%	19.39	2.08	0.78	5.34	0.21
Density Max + College Ratio 25%	26.18	14.03	0.14	6.70	3.37
Density Half + College Ratio 25%	16.08	5.89	0.31	4.41	1.42
Projection From City	27.85				

The values indicate percentage change if the simulated policy is implemented. Density Max is if policy induces maximum increase of density laid out by the plan. Density Half is if policy only induces half the intended density increase. College Worker 50% is if the change in zoning increases a_{jH} such that college worker ratio increases by 50%; similarly for College Worker 25%. Last row is Los Angeles' own prediction on the gains to total employment (Soruce: "Environmental Impact Report", Table 3-1, percentage change calculated with *Difference Between Current Plan and Proposed Plan* and *Existing Conditions*). Changes in productivity and wages is calculated by taking the weighted average of increases in each tract, weighted by the intial total employment share for productivity, college employment share for college wages, and non-college employmentshare for non-college wages.

Another implication of the policy simulations is that the density increase acts as an equalizer of benefits across skill groups. For example, comparing the scenarios with maximum increase in density with half the increase with 50% rise in college employment ratio (Table 1.7, rows 3 and 4), we see that college wages rise by around 6% in both scenarios but the increase in non-college wages between the two scenarios is quite different. With only half the density increase, non-college wages experience a negligible increase, while with the full density increase, non-college wages increase by about 2%. This difference is even more pronounced when compared to no density increase (row 2), which decreases non-college wages by about 3%. This is due to the fact that with an increase in the floor space, an increase in college employment does not have to replace non-college employment in the area. Therefore, combining the change in land use policy with an increase in density can mitigate unbalanced development across skill groups.

One margin the policy exercises does not take into account is workplace congestion from increased commuting. Increased jobs in the targeted areas may create congestion effects on certain commute modes. Although the city is targeting areas around newly built transit stations to lessen the load on automobile traffic, congestion in public transportation could affect commuter utilities and make these workplaces less desirable. Therefore, the gains from the policy in the model will miss the possible benefits (less automobile traffic) and possible costs (public transport commuting congestion).

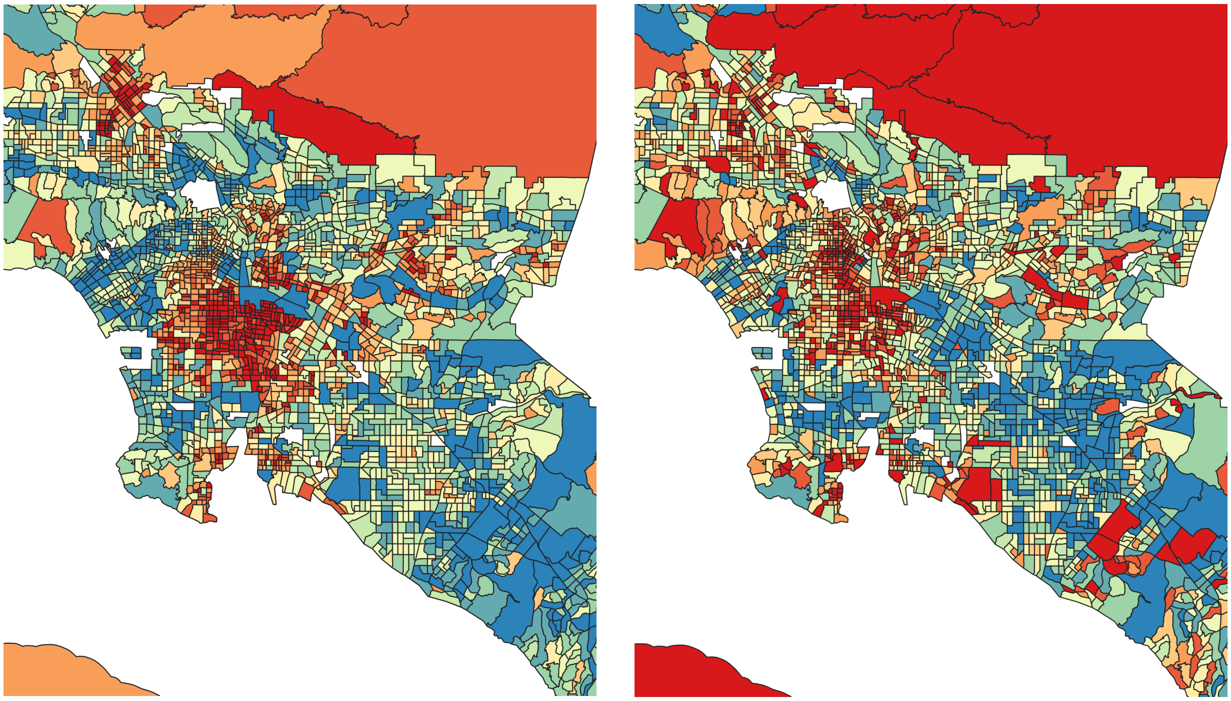
Overall, the policy experiments show two important margins of the policy. First, density increase alone does not produce meaningful spillover effects because keeping the land use the same would mostly increase non-college workplaces in the area. Combining the density policy with changes in land use to induce more college workplaces can produce spillover effects that benefit both college and non-college workers. Second, change in land use alone produces a large negative effect to non-college workers that currently occupy the project area. Increasing density in these areas in conjunction allows the benefits to accrue to both college and non-college workers. Thus, using both policy instruments and making sure that the policy attracts the intended level of density and college jobs is crucial to the success of such plans.

1.6.3 Re-locating of the Expo Plan

I perform another counterfactual exercise to examine whether there are differential effects to welfare of implementing a policy like the Expo Plan on other tracts within Los Angeles. I examine the increase in ex-ante expected utility of college and non-college workers from increasing the current density of development by around threefold and the college ratio of employment by 50% in each tract within Los Angeles. Essentially, this exercise seeks to understand the optimal location of such a policy to increase the welfare of college and non-college workers.

I present the results in Figure 1.14. In the Figure, each tract's color represents the magnitude of the percentage increase in expected utility of the whole city of an education

Figure 1.14: Expected Utility Gain of Targeting FAR increase in College Share in Each Tract



(a) College Welfare Gain (b) Non-college Welfare Gain

The welfare gain of increasing FAR threefold and college ratio of employment by 50% in each tract to (a) college and (b) non-college workers. Red denotes lower gain, and blue denotes higher gain.

group when implementing the policy on the tract. Blue represents a higher welfare gain and red represents a lower welfare gain. Figure 1.14a shows the welfare change of college workers and Figure 1.14b shows the welfare change of non-college workers. Since the policy is a relatively mild change to the supply of density and employment in the view of total supply of workplaces within the city, the expected utility gain is very mild, ranging from -0.05% to 1% gain in expected utility. However, there is meaningful differences in impact across location and skill groups.

Two interesting patterns emerge from these figures. First, there are both similarities and differences on the welfare gains of the two education groups for the same target location. For example, certain tracts near downtown Los Angeles benefit college workers more than non-college workers, while tracts on the edge of South Los Angeles benefit non-college workers while being worse for college workers. Meanwhile, where Expo Plan is being implemented seems to be beneficial for both college and non-college workers to a similar degree.

Second, there seems to be an interesting relationship between location of residence, workplace, and the optimal location of such policies. For example, tracts that are traditionally occupied by mostly non-college and poorer workers—such as those in South Central Los Angeles and in the center of South Los Angeles—seem to be bad locations for a density policy for both college and non-college worker welfare. Also, tracts far from college worker residences seem to be worse locations for density policies. Non-college workers are particularly worse-off when density policies are located in areas of high non-college worker residence density, probably due to displacing of non-college residences. The largest benefit seems to come from tracts that are currently occupied mostly by workplaces rather than residences.

1.7 Conclusion

This chapter documents the very local human capital spillovers within cities. College employment density raises the productivity of workers around them, while non-college employment density is associated with congestive forces that decrease the productivity of workers around them. Moreover, both of these effects only occur within 2-3 miles, so the reach is limited. I started with some stylized facts on the geographical distribution of workplaces within cities that the literature has not yet shown. Then, I presented both descriptive evidence of this phenomenon and framed the results using an economic geography model that takes into account commuting, agglomeration, and human capital spillovers across tracts. These rapidly decaying human capital spillovers are consistent with previous papers estimating local spillovers of density of all workers (Ahlfeldt et al. 2015), of certain industries (Arzhagi and Henderson 2006, Berkes and Gaetani 2019), and of human capital (Fu 2007, Rosenthal and Strange 2008).

With model estimates at hand, I explored the consequences of a Los Angeles policy enhancing density and re-zoning to allow for higher-quality office spaces to attract high-skill jobs into certain neighborhoods. By varying the level of density of development and increase in college share, I find that if development follows the desired density alone, there will be a fall in average productivity due to an increase in non-college workplaces in the area. When coupled with policies to encourage the creation of more high-skill jobs, these policies create spillover effects that benefit beyond the targeted locations.

The estimates of local spillover effects of human capital presented in this chapter are relevant to urban policymakers who must consider where to place employment centers and the effect density on the surrounding areas. The parameter estimates and model framework presented in this chapter can inform policymakers as to what would be the general equilibrium effects of human capital density, and how they affect the targeted and surrounding areas. However, the fast decay of these effects will also limit the reach of density and growth in the city. Especially worrisome is the recent trend in the loss of accessibility to job clusters for the urban poor as gentrification picks up across the United States (Kneebone and Holmes 2015). These fast-decaying spillovers imply that the loss of accessibility to jobs near high human capital centers will exacerbate the welfare loss of the urban poor in the coming years.

I end this chapter with some avenues for extension. First, better individual-level data with finer geography and a panel structure, such as the underlying LEHD data, could be used to better empirically estimate the human capital spillover effects. Second, incorporating the endogenous response of developers and firms in a model with spatial spillovers will be important; unlike the counterfactual exercises presented in this chapter, such a model will better predict outcomes from changes in zoning. Third, exploring potential mechanisms behind human capital spillovers I document will provide the right context to interpret the estimates of this chapter.

1.A Appendix Tables and Figures

Table 1.A.1: Robustness Checks for Different Sample Definitions

	(1)	(2)	(3)	(4)
Sample	One Worker in HH	Two Workers One Full-time	Two Workers in HH	Full-time
Log Col Own	0.102***	0.104***	0.0958***	0.106***
	(0.0197)	(0.0200)	(0.00702)	(0.0105)
Log Col 0-3 mi	0.0972***	0.0610	0.0782***	0.0805***
	(0.0341)	(0.0365)	(0.0189)	(0.0240)
Log Col 3-5 mi	-0.00863	0.0634**	0.0216	0.0602***
	(0.0381)	(0.0246)	(0.0151)	(0.0213)
Log Col 5-10 mi	-0.0320	-0.0371	-0.0186	-0.0535
	(0.0372)	(0.0289)	(0.0200)	(0.0381)
Log Col 10-25 mi	-0.0276	-0.00600	-0.00258	0.0500
	(0.0647)	(0.0397)	(0.0395)	(0.0395)
Log Non Own	-0.0735***	-0.0825***	-0.0778***	-0.0890***
	(0.0192)	(0.0205)	(0.00802)	(0.0116)
Log Non 0-3 mi	-0.107***	-0.0596	-0.0722***	-0.0712**
	(0.0375)	(0.0414)	(0.0212)	(0.0278)
Log Non 3-5 mi	0.00340	-0.0799***	-0.0295	-0.0740***
	(0.0412)	(0.0271)	(0.0198)	(0.0228)
Log Non 5-10 mi	0.0608	0.0687*	0.0404*	0.0749*
	(0.0400)	(0.0356)	(0.0221)	(0.0398)
Log Non 10-25 mi	0.0405	-0.000457	0.0180	-0.0407
	(0.0777)	(0.0483)	(0.0450)	(0.0428)
Constant	10.56***	10.05***	10.08***	10.34***
	(0.453)	(0.579)	(0.328)	(0.426)
Observations	9,848	10,617	24,591	21,565
R-squared	0.492	0.491	0.483	0.461

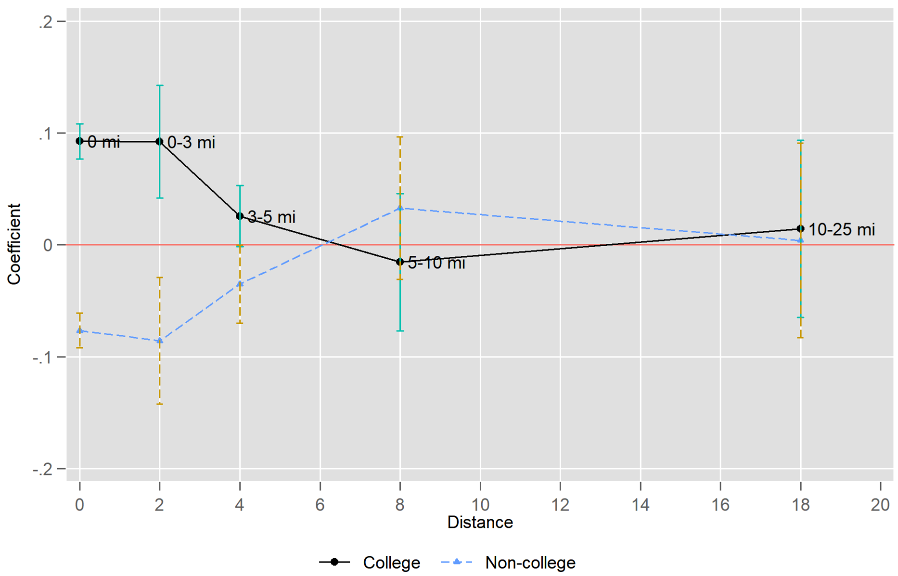
OLS specification with clustered standard errors at the employment tract level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Column (1) uses only households with one worker in the sample. Column (2) uses only "primary earners" defined by the only full-time worker in households with two workers. Column (3) uses only households with 2 workers or less. Column (4) uses only full-time workers.

Table 1.A.2: Correlation of LODES Between Different Data Sources

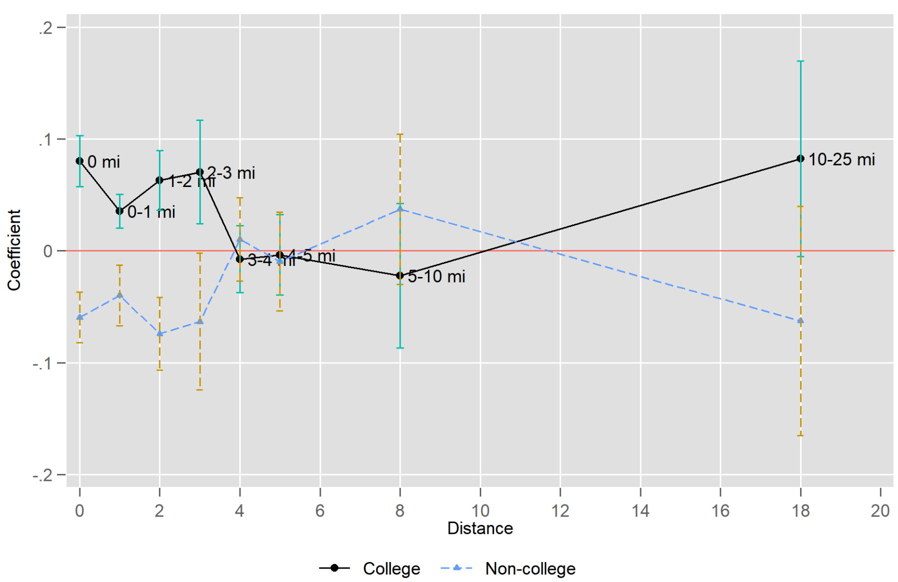
	Employment vs CHTS	Residence vs ACS
College	0.863	0.847
Non-college	0.713	0.791

Correlation coefficients of LODES dataset against different data sets. First column checks employment location against CHTS and second column checks residence location vs American Community Survey 2010. Both data restricts samples to workers (employeed).

Figure 1.A.1: OLS with Finer Spatial Definition



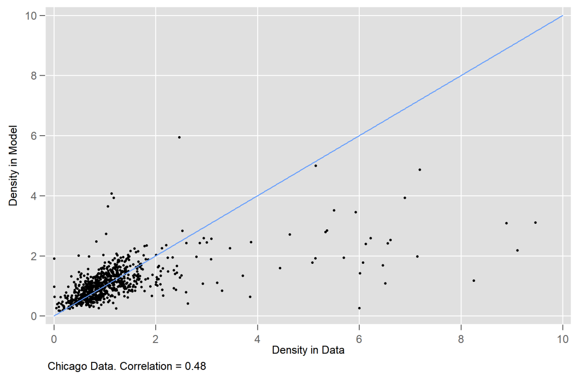
(a) OLS with Same Specification As Paper



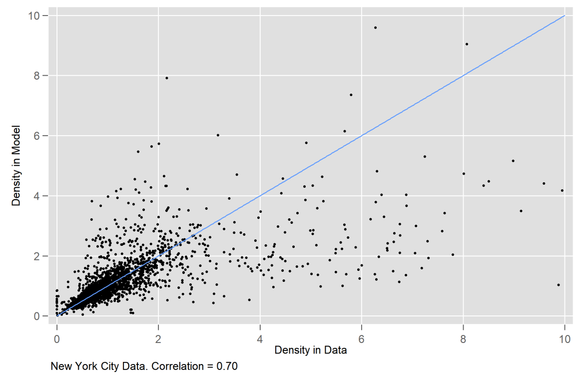
(b) OLS Disaggregating 0-5 miles

Coefficient estimates and 95% confidence intervals from pooled OLS regression specification as in Table 1.2 column (1) with varying distance ring definitions. Figure 1.A.1a plots the specification from column (1) and Figure 1.A.1b plots the specification with 0-3 miles and 4-5 miles disaggregated to 0-1, 1-2, 2-3, 3-4, 4-5 mile rings.

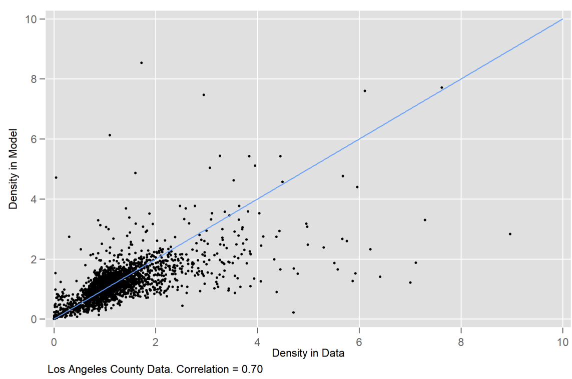
Figure 1.A.2: Over-Identification Check for Floor Space by Area



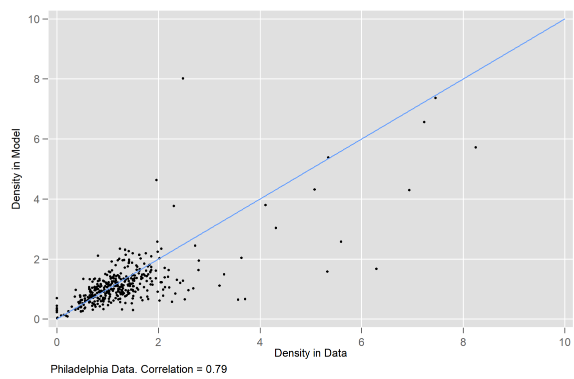
(a) Chicago



(b) New York



(c) Los Angeles



(d) Philadelphia

Over-identification checks for density of development separately by area. Chicago, New York, Philadelphia data are for the city boundaries. Los Angeles data is for the County of Los Angeles. Correlation coefficients for Chicago, New York, Los Angeles, Philadelphia are 0.48, 0.7, 0.7, 0.79, respectively.

CHAPTER 2

Do Youth Employment Programs Work? Evidence from the New Deal[1]

2.1 Introduction

Unemployment rates are typically highest among the young, particularly those from poor backgrounds and during recessions. At the height of the Great Recession, unemployment rates for those over age 25 peaked at 8.4% in 2010 but were as high as 19.6% for those aged 16-24 (US Bureau of Labor Statistics 2018). To address youth unemployment, government-run employment training programs specifically target young adults. However, the short run effects of these programs have been shown to be modest, at best, and there is very limited evidence of their effectiveness over the long run. There is also very limited evidence on the effects of these programs on non-labor market outcomes and on the mechanisms by which

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labor market effects operate (Card, Kluve, and Weber 2018, Barnow and Smith 2015, Crepon and van den Berg 2016).

We re-evaluate the *short-* and *long-run* effects of means-tested employment and training programs targeted at young adults by studying the impact of the Civilian Conservation Corps (CCC). The CCC was the first and largest employment program in U.S. history and was implemented during a period of profound levels of youth unemployment—the Great Depression. Unemployment rates among young adults during the Depression were estimated to be as high as 60 percent, depending on how partial employment is counted.[2] To address high youth unemployment, the CCC was created in 1933 by the Roosevelt Administration. It employed young men aged 17 to 23 in unskilled, manual labor. Under the Army's supervision, enrollees were sent to work in camps in rural areas where they were also fed, housed and given access to medical treatment. In addition to work experience, the CCC provided academic and vocational courses as well as cash transfers to the families of poor unemployed youths. The CCC also helped enrollees obtain employment upon completion. Enrollment in the CCC was voluntary and enlistment periods lasted 6 months with an option to re-enlist up to three times. Between 1933 and 1942, the CCC had three million enrollees and operated about 2,600 camps. Several programs in existence today such as Job Corps, Youth Conservation Corps, JobsFirstNYC, and CalWORKs are modeled after the CCC.[3]

We collect a new, large individual-level data set of CCC participants and their long-term outcomes. We digitize administrative records from the CCC program in Colorado and New Mexico covering the population of men in the CCC program between 1938 and 1943. Our data include dismissal records on more than 25,000 men and details their demographic characteristics, compensation, enlistment duration and reasons for leaving the program. We matched these enrollee records to 1940 Census records, WWII enlistment records, Social

[2] Salmond (1967) reports that in 1932, 25 percent of youths were unemployed, and another 29 percent were only employed part-time. Rawick (1957) estimates that about 20% of youths were unemployed and another 30% were working part-time.

[3] Levine (2010).

Security Administration records, and individual death certificates. These data allow us to investigate the effects of the CCC on important long-run outcomes and mediators including education, health, geographic mobility, employment, earnings and longevity.

To estimate the effect of the program, we exploit variation in the service duration of the enrollees. Treatment duration varied from a few days to more than two years with the average enrollee participating for approximately nine months. We show that the determinants of duration are complex and that those who trained for long periods were not necessarily from higher or lower SES backgrounds. Moreover, many ended their training for arbitrary reasons. We confirm these observations by investigating the reasons for dismissal. To assess the validity of our approach, we use the rich data from Colorado to perform some placebo tests. We find that duration does not predict pre-CCC labor outcomes or health, though we do find some effects on education. We then explicitly control for many individual and aggregate characteristics that predict participation and long-term outcomes and assess the sensitivity of our results to adding these covariates, informally and formally, as suggested by Oster (2017).

We find that individuals who trained longer in the CCC also lived longer. These gains appear to be driven by the improved health of the participants (measured by height and weight) as well as their increased geographic mobility towards richer areas, and their larger lifetime incomes. These effects are larger among Hispanics, and for those serving in times of high unemployment. We also find modest increases on educational attainment and in the probability of serving in WWII. In the short run, we find no evidence that their labor force participation, employment, or wages increased—these effects are very small and statistically insignificant. Overall, the results are consistent with the hypothesis that the program provided important in-kind goods and services to disadvantaged populations in a time of need, improving their long-term health and survival. They are also consistent with the program having returns in the labor market.

To further investigate the internal and external validity of our findings, we make use of publicly available experimental data from the Job Corps (JC) program, the largest job training program in the US targeting youth with an annual budget of $1.7 billion. The JC experiment followed randomly assigned participants for four years.[4] With these data, we are able to follow Lalonde (1986), using experimental data to shed light on the internal validity of a study based on observational data. Although the JC data pertains to youth training that took place in the 1990s, the program was modeled after the CCC and so retained many similar features. We focus on men that participated in the RCT for comparability. We document that JC participants are quite similar to CCC participants with regard to socio-economic characteristics (with some notable exceptions), and that they train for similar durations and quit for similar reasons.

The estimated treatment effects of training from the JC RCTs are similar in both direction and magnitude to the effects of duration in a simple OLS model that controls for basic observables at baseline, suggesting that our estimation strategy is internally valid. The results also speak to external validity. The original JC RCT reported that the program increases education levels, has small effects on employment rates and has positive, but statistically insignificant, effects on wages among those employed. We replicate these findings for men. We also document that JC and CCC both increased geographic mobility and improved health. Our results from CCC are similar in the short-term to the effects of JC, except for employment and wages.

This suggests that our long-run estimates of job training based on the CCC are likely informative about the long-run effects of JC particularly for health. There does exist a single study examining the effects of JC on labor market outcomes over 20 years using administrative tax data. Schochet (2018) finds no employment or earnings effects in the overall sample, though there are some positive effects for individuals who were older at baseline. They also report a 40% reduction in SSDI benefits, suggesting JC improved health, consistent with our longevity results. Using data from the Social Security Administration, we find CCC resulted

[4]There was a longer 9-year follow-up as well but these data are not publicly available.

in a 3.9% increase in pension amounts, which are a function of individuals' highest 35 years of earnings. This corresponds to an increase of roughly 6% in lifetime earnings. These effects are larger than the 2% (imprecise) increase Schochet (2018) documented, suggesting that the 20-year evaluation underestimates the returns of the program, or alternatively, that the economic conditions prevailing in the 20 years after the training took place have large effects on its return.

Our results suggest that JC participants today may live longer as a result of the program. As such, job training evaluations that focus only on the labor market impact of the program may underestimate the overall benefits. Our findings also suggest that there are in fact positive returns to investing in young adults, contrary to the commonly stated findings that returns on human capital investment are low after age 18. Our conclusion differs from that of Hendren and Sprung-Keyser (2020), who report low values for JC, because we are able to incorporate large increases in longevity, as well as increases in lifetime earnings into the benefits of the program.

This paper also contributes to the broader evaluation of the New Deal programs developed during the Great Depression. The Great Recession of 2008 renewed interest in understanding whether and for whom government programs deployed during large economic crises can be effective. Fishback (2017) provides a comprehensive survey of the literature on the effects of New Deal programs, and reports that studies show New Deal programs increased internal migration, lowered crime and reduced mortality in the short run. (See also Fishback, Haines and Kantor, 2007 and Vellore 2014.) Our results are consistent with these findings for migration and health. To our knowledge, there have not been any statistical studies of the long-term causal effects of the CCC program or of any other New Deal program on individual lifetime outcomes. Our results suggest that cost benefit analysis that do not include such outcomes may generate incorrect estimates.

2.2 Background: The CCC Program

Program Overview. The CCC, which was signed into law on March 31, 1933, was created by President Franklin Delano Roosevelt by executive order "for the relief of unemployment through the performance of useful public work and for other purposes."[5] The CCC had two objectives: 1) to provide relief to unemployed youth; and 2) to preserve and enhance natural resources. Because of the prevailing view at the time that the provision of work would be more beneficial to the unemployed than the receipt of cash transfers "relief through work" rather than "direct relief" was a basic tenet of all the work programs in the New Deal. There was also a belief that idle youth would commit crimes and cause social disturbances (Brock 2005).

The untapped work capacity of idle youth was to be used to create national parks and forests, and to help cope with the Dust Bowl. One of the primary appeals of the CCC was that the work of enrollees would not directly compete (in terms of labor) with private sector activities. As the program evolved, it added education components, which became mandatory in 1937. The nature of the program changed again in 1941 when military training was added to the program as a result of growing tension in Europe during World War II.[6]

Size and allocation of projects and enrollees. The federal government commissioned the CCC to build national parks, preserve forests and irrigate land. Within weeks of the creation of the CCC program, 1,250 projects had been submitted and 749 camp sites had been approved by the director of the CCC and the President.[7] Camp locations were chosen to be

[5]The program was extended in 1935, 1937 and 1939, and ended in 1942 when Congress voted against another renewal, despite prior efforts to make the program permanent. In addition, the program was originally called the Emergency Conservation Work Program, but its name was changed in 1937 to Civilian Conservation Corps, its popular name. Data Appendix Figure 1 contains a timeline describing the major changes to the program throughout its existence.

[6]Although perhaps unintended, and due to the fact that the military was in charge of running the camps, another perceived benefit of the CCC program was that "enrollees made splendid soldier material" (McEntee 1942).

[7]US Department of Labor Report, 1933.

close to work sites, and to minimize the distance to communities that would supply them. Most camps had 200 enrollees at a time. Many smaller "side camps" were also created to allow for work in remote locations.[8]

Eligibility. Only unmarried unemployed men, ages 17 to 25, who were American citizens, were eligible.[9] Preference was given to those in greater need—in practice, CCC enrollees were often selected from families already enrolled in relief programs.[10] Government reports at the time confirm that enrollees were poorly educated, with little work experience, and undernourished (McEntee 1942).[11] Enrollees had to present in good physical condition (an examination was required at enlistment) and have no history of criminal activity.[12] Finally, they had to be willing to send a substantial portion of their wages to an assigned family member and to move to the designated camp location for the duration of the enrollment period. After the enrollee signed the contract there was a two-week conditioning period, after which enrollees were sent to a camp.[13]

Compensation and program cost. Enrollees were required to work 40 hours per week

[8] Local labor could be employed when there were needs for specific skills to complete a project. Although initially some communities were concerned with possible increases in crime resulting from nearby camps, most communities eventually welcomed and moreover demanded camps be placed nearby, with the notable exception of black-only camps, and camps with a large share of Hispanics. The CCC program was popular and many communities welcomed the camps and the monies that it brought (Parham, 1981). A nation-wide poll in 1936 showed that more than 80 percent supported the continuation of the program, and this support was larger in the Rocky Mountain states (Paige 1985). However, there were racial tensions (Rawick 1957).

[9] There were some changes to these initial criteria, importantly age eligibility of juniors was modified twice. Data Appendix Figure 1 documents some of the important changes in the history of the program.

[10] In 1935 when the program was expanded, it became a requirement that enrollees be drawn from relief rolls, though in practice this was not always the case. In 1937 this requirement was eliminated.

[11] For example, in 1939 and 1940, about 52% had 8 years of schooling or less (Annual Report 1940).

[12] Enrollees were vaccinated against typhoid, paratyphoid and smallpox at enlistment.

[13] In addition to accepting "juniors"—that is youth 18-25 to be trained, the CCC program also made veterans eligible. There was also a large CCC program for American Indians, which operated under somewhat different rules and was managed by the Bureau of Indian Affairs. Finally, the CCC also enrolled LEM "local enlisted men" which had skills and knowledge not available among its Army personnel. The total number of men training in the CCC was reported to be 3.2 million, LEMs accounted for 263,000, Indians 127,000, and veterans. There was a small separate program for women started in 1936 which eventually served about 8,500 women nationwide in about 80 camps.

and paid \$30 per month, of which \$25 was sent home to a designated family member.[14] The government also paid for the transportation to and from the camp, provided housing, uniforms, food, dental and medical care, and workers' compensation insurance. Thus, it is estimated that the real monthly wages of CCC enrollees was \$66.25 per month.[15] CCC administration estimated that on average a CCC camp would spend about \$5,000 per month in local markets.[16]

Duration of enrollment. Individuals initially enrolled for a six-month period, and were allowed to re-enroll, for a maximum of two years (4 terms). Although the *average* enrollee worked for 9 months, there is large variation. CCC contracts could be terminated unilaterally by the government, based on governmental needs, at any point. Many individuals deserted, resigned or were expelled prior to completing their contract. Enrollees could leave early if they had secured employment, were enrolled in a formal schooling program or for "urgent and proper call" reasons, for instance the death of a parent or some other personal emergency. Enrollee turnover was costly, and efforts were made to keep it low.

Education and training components. Vocational training and skill provision were always a part of the program. In addition to on-the-job training, camps offered several vocational courses. Attendance was voluntary. Soon after the creation of the CCC, there was a realization that an educational component would be needed as a large number of enrollees were illiterate or had education levels so low it prevented them from performing their assigned tasks at the camp.[17] An education program was put into place by March of 1934, and the 1937 extension of the CCC program included an important requirement that the CCC

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[14] Later in the program, a portion was retained as savings and given to enrollees upon dismissal.

[15] See BLS (1941). Levine (2010) reports this program was considerably more expensive than Works Progress Administration as it was estimated to cost approximately \$800 per enrollee. Critics of the program pointed out that direct relief would have cost an estimated \$250 per year instead (McEntee 1942). The value of the training and of the work achieved in terms of conservation is of course not considered in this estimate.

[16] Paige (1985).

[17] Britton reports than in Northern camps an average of 3 to 5 percent of enrollees were illiterate, but as many as 25% were illiterate in Virginia camps.

provide at least ten hours a week of general or vocation training.[18] Participation was not mandatory unless the enrollee was illiterate.

2.2.1 The CCC in Colorado and New Mexico

We study the program using administrative data from Colorado (CO) and New Mexico (NM). Both CO and NM were relatively poor states during the Great Depression, though NM was poorer and arguably one of the poorest states at the time. Estimates from National Income Accounts for 1930 suggest that per capita annual personal income was $571 in CO, and $329 in NM, while the nationwide average was $618.[19] About a quarter of the population in CO was on relief in 1933; New Mexico had the highest share of the population on relief in the nation (Hinton 2008).[20]

Due to the large number of parks and forests in these states, and the severe impact of the Dust Bowl, CO and NM had disproportionate participation in the CCC Colorado and New Mexico had disproportionate participation in the CCC program. In a given year, on average, there were 34 main camps operating in CO and 32 in NM in operation in a given year.[21] The number of individuals training in CO and NM was disproportionately large. In CO, a total of 57,944 men served, of which 35,000 came from CO. In NM, a total of 54,500 served of which 32,300 came from NM.[22] Enrollees in Colorado and New Mexico were disproportionately Hispanic.[23]

[18] Act of June 28, 1937, Public No 163, 75$^{\text{th}}$ Congress.

[19] Bureau of Economic Analysis NIPA 1929-today. SA1-3

[20] Census of relief 1933. Table 9.

[21] Final report. This number does not include the so-called side camps, which were smaller in size than typical camps, whose population hovered around 200 men.

[22] Cohen (1980).

[23] New Mexico also had a large share of Native Americans. Native Americans had their own CCC programs which operated separately within Indian reservations and were administered by the Bureau of Indian Affairs. See Parman (1971) for details. We have no data on the Indian CCC program.

2.3 Estimation Strategy and Estimation Issues

We estimate the effect of the program on lifetime outcomes by comparing outcomes for those who served longer and shorter periods among individuals who served. This strategy is similar to what Flores et al. (2012) do to estimate the returns to the number of courses taken in JC and to Lechner et al. (2011), who evaluate impacts of short and long training programs in Germany. The intuition behind this approach is simple: if training increases skills through some standard production function, then more training should result in greater skills, though the rate of increase might change with the level of training.

We use the following specification,

$$Y_{ibj} = c + b * (\textit{duration of CCC service}_{ibj}) + X_{ibj}B + e_{ibj} \tag{2.1}$$

where Y_{ibj} is an outcome, such as employment or age at death for individual i born in year b training in CCC camp j, and X_{ibj} includes individual-level and camp-level covariates. The independent variable of interest is $\textit{duration of CCC service}_{ibj}$, the duration of training in years. We estimate equation (2.1) clustering the standard errors at the application county and enrollment year-quarter level, though the results are not sensitive to this choice.[24]

The coefficient b identifies the causal effect of duration on a given outcome only if duration is uncorrelated with other determinants of the outcome, conditional on the observables. There are several threats to identification. First, duration is measured with error because dates are often incomplete or missing, possibly causing downward bias in the estimates. Second, there is a possible omitted variable bias: it may be that individuals with higher abilities trained longer because they benefitted more from the program and were able to better adapt to military life in camps (positive selection). Alternatively, poorer individuals

[24]We also experimented with alternative approaches and estimate results clustering at the application county, enrollment year level. Overall, we found these alternatives do not materially impact our conclusions, and the evidence suggests that there is little correlation across individuals in the data.

may have had stronger incentives to train in the CCC because they were more in need of the payment that they and their families received (negative selection). Third, it is also possible that camp characteristics are omitted. For example, individuals might have stayed longer in camps with good weather, and good weather could improve long-term health (positive selection). Demand for work might have been greater in places where the dust bowl hit, leading enrollees to stay longer in unhealthy locations (negative selection). In these cases, the coefficient on duration would be biased.

To address these concerns, we take multiple approaches. First, we investigate the determinants of duration to determine the extent of possible selection issues. We also make use of the reasons why individuals dropped out to understand who leaves early and why. Then, to account for selection on observables, we explore how the inclusion of individual- and camp-level covariates affect the estimates of the effect of duration. We estimate bounds using the method proposed by Oster (2017). For a subset of the data we also conduct placebo tests to see if duration predicts pre-CCC enrollment outcomes (education, labor market experience, height and weight). Finally, we use the data from JC to investigate whether our approach generates biases in the estimates by comparing OLS type estimates to the estimates derived from the RCT.[25]

2.4 Data and descriptive statistics

2.4.1 Data Collection

Colorado (CO) Enrollees. We digitized the entirety of CCC records contained at the State Archives of Colorado. These records include original applications of all individuals who ap-

[25]We also investigated a large number of IVs to instrument for individual duration including the use of weather, camp closures, measures of the intensity of the Dust Bowl and leave-out duration at the camp. Unfortunately, most of the IV estimates we produced had large standard errors and suffered from weak IV problems.

plied.[26] The entire collection, which includes 21,538 individuals, accounts for the population of individuals who trained between 1937 and 1942 but not for those who enrolled prior to 1937.[27] The applications contain the following: name, address, date of birth, place-of-birth, height, weight, race, and social security number (SSN), marital status, whether the father or mother is living, number of brothers, number of sisters, number of family members in household, rural status, farm ownership, occupation of main wage earner in household, educational details, employment status and history. With the exception of information on height, weight and race, which were collected upon medical examination, the rest was self-reported. In addition, previous CCC enrollment information was collected, and information on the designated allottee(s) (the family member who would receive the allotment from the CCC): name, relationship and amount allotted, for up to two allottees. If the individual was rejected, it is noted in the file. Otherwise we observe the discharge information detailing the company and camp the individual attended, reason for dismissal, the date of dismissal, and whether the dismissal was honorable.

New Mexico (NM) Enrollees. We digitized the entirety of CCC records from the New Mexico State Records Center, which has the entire set of discharge forms for the state from 1938 to 1942. These records include information on 9,699 individuals, covering the population of individuals that trained in state from 1938 to 1942.[28] For each individual, the records contain the following: name, date of birth, address, family information (head of family, address of family, and relationship to enrollee), allottee information (name, address and relationship to allottee, for up to two allottees), enrollment date, assigned camp, date and reason for dismissal and whether the dismissal was honorable. Because enrollment forms

[26]Of the 35,000 that trained in CO and came from CO, about 30,000 were junior and veterans, and 5,000 were non-enrolled personnel (hired from local population), and about 500 were part of the Indian CCC program.

[27]We established based on published reports from the CCC that the records account for the complete population of records starting in 1937 (see Data Appendix Figure 4).

[28]We established based on published reports from the CCC that the records account for the complete population of records starting in 1938 (see Data Appendix Figure 4).

are unavailable, NM records contain substantially less information on participants than CO records.

Camp-level Data. We collected information on the exact location of camps. In particular, each camp was assigned to a zip code within a county using post-office codes. Then, we coupled camp location information (latitude and longitude) with historical weather patterns (temperature and precipitation), which come from PRISM Climate Group. Additionally, we retrieve longitude and latitude information of closest towns and individual's residence cities from the United States Board of Geographic Names, and use them to compute (Euclidian) distances to the closest towns and to each enrollee's hometown. Using the camp name, we can construct indicators for the agency (and thus the type of work) that created the camp. We use our records to construct average characteristics of enrollees (such as the fraction under age 18) in each camp and point in time. Finally, we match camps to census county-level information about the county in which it was located, such as unemployment rates.

Death Records. The administrative data from CO and NM was matched to death records (including the Social Security Death Master File and state-level death records) to identify the date of death and social security number of each enrollee. This match was done manually by trained genealogists at BYU, who found CCC enrollees in the collection of records kept by Ancestry.com and FamilySearch.org. A summary of this process is available in Appendix 6. We find death dates for 88% of CO recipients and 75% of NM recipients, representing much higher match rates than typically found in the literature.[29] We use these data to compute the age at death using the date of death in the death certificate and date of birth in the CCC

[29] Our match rates are higher than those typically found in the literature (which range from 20 to 50%) for two reasons (Bailey et al. 2017, Abramitzky et al. 2019). First, administrative records contain information not just on individuals but also on their family members. This greatly improves our ability to find individuals by using information from family trees and various vital registration records. Second, the death records come from various sources. Most commonly these come from the Death Master File (DMF) which includes the universe of death certificates in the US starting in the mid 1970s. But the collection also includes records from other sources, including state vital registration sources, deaths during WWII, and gravestones. A few individuals are observed as dying during CCC training.

application.[30] We also match the data using automated methods as a robustness check.

1940 and WWII records. We match our records to the Federal Census of 1940 and to WWII Enlistment Records. These matches are made using the Abramitzky, Mill, and Perez (2018) algorithm. Details of the procedure are available in Data Appendix D and E. The 1940 census includes location, demographics (race and ethnicity, marital status, place of birth, household information), and labor market information (employment occupation and wages). We successfully match 44% of individuals to the census, and about 29% to WWII enlistment records. This lower match rate to WWII records is to be expected: not all individuals enlisted or served in WWII, even when they were eligible. Also, not all records of those who served survived.[31]

Social Security Records. We match our data to the Master Beneficiary Record File (MBR) in the Social Security administration, which contains information on individual lifetime earnings, disability, and retirement. (More details are available in Data Appendix 1F.) We merge these data on SSNs.[32] We are able to match 52% of our records to the MBR records. But only those that apply for benefits (social security pensions or disability) appear in the MBR. We have information on 80% of individuals who survived to age 65, so our match rate for the targeted population is high. In these records we can observe the Primary Insurance Amount (PIA), which is a proxy for lifetime earnings. The PIA corresponds to the pension a person

[30]Mortality information is missing for some individuals for several reasons. First, some individuals died prior to 1975, which is the first year of complete death records in the Social Security Death Master File (For more information about coverage of the DMF, refer to Hill and Rosenwaike (2001). In this case, we might find a death record for them if one exists in state vital records. Second, some individuals might still be alive, so the age at death is censored. Based on SSA life tables we compute that about 1.1% of individuals born in 1920 (our median birth year) would be expected to be alive by 2017. Lastly, we might not have found individuals who died in the 1975-2017 interval due to measurement error and matching errors. The key issue for estimation will be whether missing data is differentially missing for those that trained for linger durations.

[31]Several cards were lost to fire or were unreadable. See https://aad.archives.gov/aad/series-description.jsp?s=3360&cat=all&bc=sl

[32]We only observe SSN if they person reported it in the application in CO, or if it is available in the death certificate. However, SSNs are not available for anyone who died after 2008 (these are masked for privacy reasons) or for those who died young and never applied for a SS card.

receives if they start receiving retirement benefits at his/her normal retirement age. The PIA is a non-linear transformation of the AIME (average indexed monthly earnings), which computed as the average of the highest 35 years of earnings after adjusting for inflation.

2.4.2 Sample Selection

For our analysis, we restrict attention only to individuals for whom we can observe duration of training, camp, and the outcome of interest. Therefore, we drop individuals who have no birth year, enrollment year, discharge year or application county, as well as those whose entire discharge records are missing. This results in a sample of 23,722 men out of 26,292. Appendix Table 2.A.1 details the number of observations that are lost due to missing data.

For the mortality analysis, we make additional restrictions. We include only individuals with age of death information but investigate the effects of missing data and also use imputations in alternative specifications. The final mortality sample contains information on 17,639 men. This estimation sample generally is representative of the initial data (Table 1) except that, by construction, the age at death is significantly higher. For the lifetime outcomes from the SSA, our sample includes 12,455 individuals, 64% of the original analytic sample. Again, this sample is relatively representative of the initial full sample in many dimensions (duration, YOB, age, height, weight, education, father alive, mother alive, household size, farm) with some notable exceptions (Table 1). By construction, the age at death in this sample is higher because only those who survive to at least 62 are eligible to apply for pensions. We also see fewer Hispanics, more people who lied about their age, and more people who sent money to their mothers. But these differences are not too large. We investigate the extent of sample selection further below.

Table 2.1a: Summary Statistics From Enrollment Records

	Analytic Sample			Mortality Sample			Analytic Sample (matched to MBR)		
	N	mean	sd	N	mean	sd	N	mean	sd
Characteristics in Enrollment Application									
Birth year	23,722	1,920	3.712	17,639	1,920	3.649	12,455	1920	3.546
Age at enrollment	23,488	18.75	2.122	17,449	18.73	2.170	12,330	18.74	2.242
Enrollment year	23,722	1,939	1.902	17,639	1,939	1.894	12,455	1939	1.889
Reported age younger than DMF*	23,722	0.0888	0.284	17,639	0.113	0.317	12,455	0.130	0.336
Reported age older than DMF*	23,722	0.167	0.373	17,639	0.219	0.413	12,455	0.253	0.435
Age is 17 or 18	23,488	0.564	0.496	17,449	0.535	0.499	12,330	0.513	0.500
Not Eligible	23,722	0.0151	0.122	17,639	0.0143	0.119	12,455	0.014	0.117
Allottee is father	23,722	0.334	0.472	17,639	0.332	0.471	12,455	0.330	0.470
Allottee is mother	23,722	0.466	0.499	17,639	0.475	0.499	12,455	0.475	0.499
Non-junior	23,722	0.00628	0.0790	17,639	0.00675	0.0819	12,455	0.007	0.082
Hispanic (imputed using hispanic index)	23,722	0.484	0.500	17,639	0.451	0.498	12,455	0.432	0.495
Additional information in CO records									
Highest grade completed	14,507	8.592	2.109	11,235	8.674	2.081	8,225	8.700	2.055
Household size excluding applicant	7,870	4.745	2.600	6,283	4.763	2.591	4,730	4.725	2.575
Live on farm?	8,101	0.248	0.432	6,460	0.253	0.435	4,846	0.252	0.434
Height (Inches)	8,141	67.80	3.089	6,475	67.88	3.083	4,860	67.92	3.053
Weight (100 pounds)	8,234	1.385	0.171	6,561	1.390	0.172	4,922	1.391	0.171
Body Mass Index	8,115	21.21	2.178	6,461	21.23	2.174	4,849	21.230	2.190
Underweight	8,115	0.0694	0.254	6,461	0.0689	0.253	4,849	0.068	0.253
Overweight	8,115	0.0450	0.207	6,461	0.0461	0.210	4,849	0.046	0.210
Father Living	7,943	0.799	0.401	6,339	0.803	0.398	4,765	0.806	0.396
Mother Living	8,006	0.850	0.357	6,391	0.855	0.352	4,808	0.855	0.352
Tenure in county (years)	5,432	12.66	6.483	4,326	12.68	6.504	3,353	12.59	6.522
Ever had a paid regular job?	8,841	0.375	0.484	7,022	0.386	0.487	5,256	0.394	0.489
Male White Unemp. / Male White Pop 1937	23,709	0.0885	0.0397	17,629	0.0864	0.0388	12,450	0.085	0.038
Male White Unemp. / Male White Pop 1940	23,709	0.0710	0.0308	17,629	0.0696	0.0299	12,450	0.069	0.029

Service Characteristics									
First allottee amount (dollars per month)	22,970	21.63	3.772	17,088	21.67	3.721	12,097	21.697	3.683
Duration of service (yrs)	23,722	0.821	0.706	17,639	0.826	0.708	12,455	0.816	0.701
Ever Rejected?	23,722	0.0194	0.138	17,639	0.0201	0.140	12,455	0.020	0.140
=1 if disabled	23,722	0.00847	0.0917	17,639	0.00686	0.0825	12,455	0.007	0.083
Gap in service (more than 3 months)	23,722	0.160	0.366	17,639	0.173	0.378	12,455	0.180	0.384
Reason ended: End of term	23,722	0.379	0.485	17,639	0.379	0.485	12,455	0.372	0.483
Reason ended: Employment	23,722	0.116	0.320	17,639	0.124	0.329	12,455	0.125	0.331
Reason ended: Convenience of the government	23,722	0.145	0.352	17,639	0.151	0.358	12,455	0.154	0.361
Reason ended: Urgent and Proper Call	23,722	0.117	0.321	17,639	0.122	0.327	12,455	0.125	0.330
Reason ended: Deserted	23,722	0.222	0.416	17,639	0.206	0.404	12,455	0.205	0.404
Reason ended: Rejected upon examination	23,722	0.00915	0.0952	17,639	0.00754	0.0865	12,455	0.007	0.083
Reason ended: No Record	23,722	0.0128	0.112	17,639	0.0120	0.109	12,455	0.012	0.109
Honorable Discharge	23,722	0.767	0.423	17,639	0.785	0.411	12,455	0.786	0.410
Camp Characteristics									
Distance from home to camp in miles (derived)	22,405	154.8	207.1	16,645	157.2	208.0	11,740	159.474	209.104
1st closest city distance form camp (miles)	23,480	26.68	22.50	17,454	26.57	22.26	12,322	26.404	22.063
2nd closest city distance form camp (miles)	23,480	49.86	22.49	17,454	49.33	22.32	12,322	48.713	22.170
Mean precipitation in camp 1933-1942	23,202	33.43	9.281	17,253	33.52	9.321	12,174	33.662	9.382
Mean min temp in camp 1933-1942	23,202	1.459	3.474	17,253	1.382	3.457	12,174	1.265	3.450
Mean max temp in camp 1933-1942	23,202	17.51	4.114	17,253	17.39	4.108	12,174	17.243	4.106
Camp Mean Hispanic (imputed using hisp. index)	23,722	0.482	0.313	17,639	0.462	0.312	12,455	0.430	0.329
Camp Type: Department of Grazing	23,671	0.135	0.341	17,593	0.132	0.339	12,455	0.131	0.337
Camp Type: Federal Reclamation Project	23,671	0.0553	0.229	17,593	0.0566	0.231	12,455	0.056	0.230
Camp Type: Fish and Wildlife Service	23,671	0.0118	0.108	17,593	0.0111	0.105	12,455	0.011	0.102
Camp Type: National Forest	23,671	0.295	0.456	17,593	0.290	0.454	12,455	0.292	0.454
Camp Type: National Monument	23,671	0.0191	0.137	17,593	0.0184	0.134	12,455	0.019	0.136
Camp Type: National Park	23,671	0.105	0.307	17,593	0.108	0.310	12,455	0.108	0.310
Camp Type: Soil Conservation	23,671	0.307	0.461	17,593	0.311	0.463	12,455	0.306	0.461
Camp Type: State Park	23,671	0.0524	0.223	17,593	0.0527	0.223	12,455	0.054	0.226
Camp Type: Other	23,671	0.0202	0.141	17,593	0.0206	0.142	12,455	0.021	0.145

Basic sample includes records with duration (begin and end date of enrollment), camp id and enrollment county. The analytical sample for the mortality analysis only includes those not missing death age and death age more than 45. When multiple records were found for a single individual we use the information in the first enrollment record. *Reported age being younger (older) than DMF OR than the oldest (youngest) reported if the individual has multiple enrollment spells.

Table 2.1b: Summary Statistics From Death Certificate, 1940 and WWII Records

	Analytic Sample			Mortality Sample			Analytic Sample (matched to MBR)		
	N	mean	sd	N	mean	sd	N	mean	sd
Death Certificate Data									
Age at death	19,377	69.82	16.84	17,639	73.62	12.03	12,348	74.76	9.25
=1 if missing age at death	23,722	0.183	0.387	17,639	0	0	12,455	0.009	0.092
Survive at 70	19,377	0.587	0.492	17,639	0.644	0.479	12,348	0.706	0.456
P(70), imputed to 0 if missing	23,722	0.479	0.500	17,639	0.644	0.479	12,455	0.700	0.458
Imputed Prob of Survival at 70 Using Age at Discharge	23,718	0.589	0.446	17,636	0.644	0.479	12,455	0.705	0.454
1940 Census Data									
Matched to 1940 Census	23,722	0.449	0.497	17,639	0.479	0.500	12,455	0.487	0.500
Panel a: those that served before 1940									
Year of birth	4,217	1,918	3.836	3,410	1,918	3.803	2,451	1918	3.559
Age at last birthday (in years)	4,217	21.77	3.836	3,410	21.75	3.803	2,451	21.74	3.559
Hispanic	4,217	0.279	0.449	3,410	0.258	0.438	2,451	0.245	0.430
White	4,217	0.991	0.0933	3,410	0.992	0.0903	2,451	0.991	0.092
In labor force	4,217	0.909	0.288	3,410	0.912	0.283	2,451	0.909	0.288
Working, conditional on labor force	3,833	0.711	0.453	3,110	0.718	0.450	2,228	0.711	0.453
Wage, conditional on working	2,983	405.3	361.0	2,424	401.8	337.4	1,764	410.8	360.7
Lives in CO	4,217	0.776	0.417	3,410	0.787	0.409	2,451	0.790	0.407
Lives in NM	4,217	0.166	0.372	3,410	0.152	0.360	2,451	0.144	0.351
Years of educ	4,159	8.770	2.477	3,363	8.842	2.445	2,415	8.873	2.420
Moved Residence Counties	4,215	0.299	0.458	3,408	0.291	0.454	2,450	0.296	0.457
Panel b: those that served after 1940									
Year of birth	636	1,920	3.486	532	1,920	3.493	418	1921	2.621
Age at last birthday (in years)	636	19.66	3.486	532	19.62	3.493	418	19.45	2.621
Hispanic	636	0.365	0.482	532	0.340	0.474	418	0.330	0.471
White	636	0.994	0.0791	532	0.992	0.0865	418	0.995	0.069
In labor force	636	0.879	0.326	532	0.883	0.321	418	0.880	0.325
Working, conditional on labor force	559	0.719	0.450	470	0.711	0.454	368	0.712	0.453
Wage, conditional on working	440	253.8	167.2	366	258.6	172.1	282	252.9	149.6
Lives in CO	636	0.855	0.352	532	0.868	0.338	418	0.864	0.344
Lives in NM	636	0.134	0.341	532	0.122	0.328	418	0.129	0.336
Years of educ	629	8.347	2.135	526	8.390	2.114	413	8.370	2.097
Moved Residence Counties	636	0.145	0.352	532	0.139	0.346	418	0.136	0.344

WWII Records									
Matched to WWII records	23,722	0.306	0.461	17,639	0.338	0.473	12,455	0.347	0.476
Birth year	7,263	1,920	2.810	5,954	1,920	2.831	4,321	1920	2.815
Enrollment year	7,262	1,942	1.424	5,954	1,942	1.439	4,321	1942	1.45
Years of education	7,263	9.395	1.787	5,954	9.404	1.785	4,321	9.399	1.766
Height in inches*	5,971	67.52	6.089	4,876	67.70	6.098	3,510	67.73	6.164
Weight in lbs**	5,641	138.6	26.19	4,595	138.7	25.70	3,327	139.4	27.17
BMI	5,466	21.55	4.500	4,451	21.50	4.101	3,214	21.55	4.399
Ever Married	7,256	0.215	0.411	5,947	0.221	0.415	4,316	0.224	0.417
Home State CO	7,232	0.591	0.492	5,928	0.605	0.489	4,300	0.617	0.486
Moved Residence Counties	7,215	0.303	0.460	5,914	0.296	0.457	4,290	0.303	0.46
Home State NM	7,232	0.319	0.466	5,928	0.305	0.460	4,300	0.289	0.453
Birthplace CO	7,215	0.444	0.497	5,913	0.451	0.498	4,295	0.462	0.499
Birthplace NM	7,215	0.322	0.467	5,913	0.309	0.462	4,295	0.292	0.455
Birthplace Rest of US	7,215	0.230	0.421	5,913	0.237	0.425	4,295	0.244	0.429

Basic sample includes records with duration (begin and end date of enrollment), camp id and enrollment county. The analytical sample for the mortality analysis only includes those not missing death age and death age more than 45. When multiple records were found for a single individual we use the information in the first enrollment record. *Reported age being younger (older) than DMF OR than the oldest (youngest) reported if the individual has multiple enrollment spells. * Dropped values below 40. ** Dropped values below 90 and over 350.

2.4.3 Summary Statistics: CCC Training and Lifetime Outcomes

Pre-CCC Characteristics. Characteristics of the men in our data are presented in Table 2.1a and 2.1b. The average CCC enrollee enlisted around 1939 and was 18.7 years old, but many enrollees appear to have misrepresented their age: 22% overstated their age (their age in the death certificates suggest they were younger than they reported), and another 11% understated their age. While some of these discrepancies might be due to errors in matching individuals to death certificates, they might also indicate that many men, particularly the young ones, were quite desperate to train and lied about their age to gain eligibility.[33]

As expected, more detailed data for CO suggest that the enrollees were relatively disadvantaged. On average, enrollees completed 8.7 years of schooling and came from a household of about 5 individuals. About 25% came from a farm, 20% had a deceased father and 15% had a deceased mother. Despite height and weight examinations to exclude the unhealthy, about 7% were underweight. Imputing the ethnic origin of the participants, we estimate that about 45% were Hispanic.[34] In the Online Appendix we show that these young men came from poorer counties than the average males of the same age in CO and NM in the 1930 and 1940 census, consistent with them being recruited from relief rolls. Consistent with the fact that CO and NM were very poor states, CO and NM enrollees were even more disadvantaged than the average CCC enrollee in the nation—they are substantially younger, shorter, weigh less, have more dependents, and more of them have fewer than 4 years of schooling.[35] Data Appendix Figure 6 documents this graphically. Data on the camps suggest that they were typically rural in nature and as such, located relatively far from the enrollees' hometowns

[33] A few of the men are not junior (less than 1%) which can also explain a small fraction of the violations in the age criteria. Individual accounts of CCC participants include accounts of lying and over-eating in order to qualify, see Melzer (2000).

[34] See Data Appendix for method of imputation.

[35] We check this by comparing the means in our estimation sample to the published national means. These were published in *Annual Report of the Director of the Civilian Conservation Corps: Fiscal Year Ended June 30, 1937* Appendix H: Census of Civilian Conservation Corps Enrollees.

(150 miles on average).

Post CCC outcomes. Table 2.1b shows the mean outcomes for CCC enrollees after they left the program. The average enrollee eventually lived to be 70 years old, below what SSA cohort life tables predict for male cohorts born in 1920 who survived to age 17 (71). In our estimation sample, conditioning for dying after 45, the average enrollee lived to be 73.6 years old, which is also lower than 74.5 from the SSA cohort life tables. This evidence is consistent with the fact CCC men were poor and came from poor states. Among those in the SSA records, the average PIA was around 430 dollars per month. In 1940, 91% of those who had already completed their training were in the labor force, and 72% were working conditional on being in the labor force, making about 400 dollars in annual wages. A substantial fraction (29%) were living in a different county from their prior county of residence. Similar patterns are observed in the WWII enlistment data.[36]

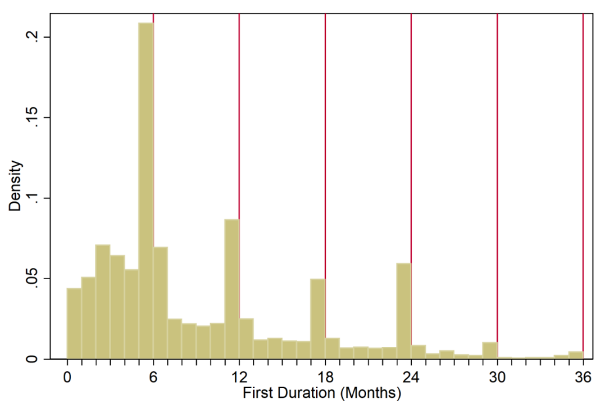
2.5 Determinants of Training Duration.

We start by investigating the determinants of enrollment duration. On average enrollees in our estimation sample trained for 9.8 months (S.D. 0.7) or .82 years. Aggregate data on the national CCC program from a 1937 CCC Census shows that the distribution of duration in our states (using CO) is skewed slightly towards shorter durations than the national distribution (Data Appendix Figure 6).

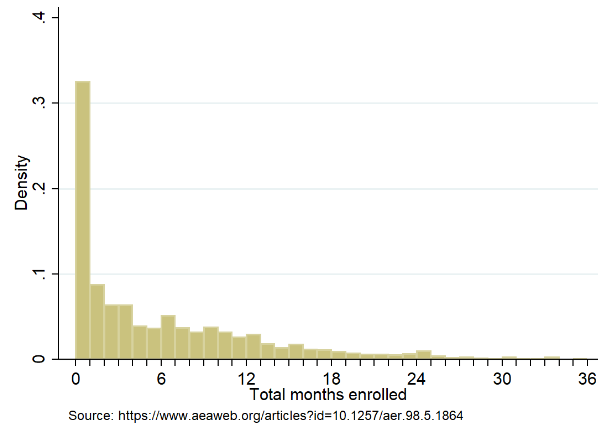
There is large variation in the duration of training. Figure 2.1, Panel A shows the histogram of duration in months. It shows spikes exactly at 6, 12, 18 and 24 months, corresponding to 1, 2, 3 and 4 terms. However, most individuals (62%) dropped out in the middle of their assignment (Table 2.1a, 38% ended due to "end of term"). And there is significant variation in duration among those serving partial terms: 9% of individuals trained

[36]At the time of WWII enlistment (around 1942) 30% were living in a different county from their prior county of residence.

Figure 2.1: Distribution of Service Duration in the CCC Records and Jobs Corps



(a) CCC



(b) Jobs Corps

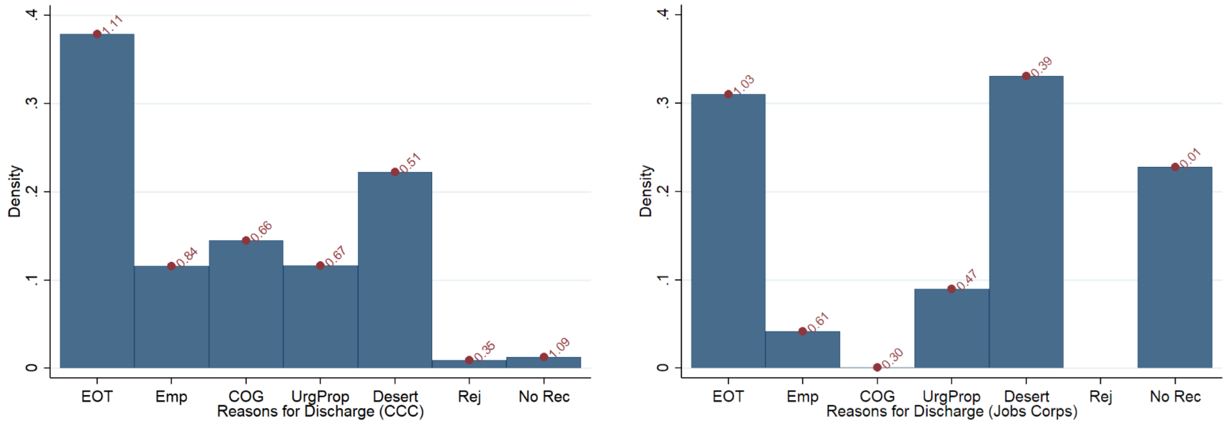
Notes: We exclude durations greater than 3 years (less than 1% of the observations) in this figure. Mean duration is 9.44 months (s.d. 7.47) for CCC and 5.8 months (s.d. 6.6) for Jobs Corps.

fewer than 2 months and a few individuals (about 1%) trained for more than 3 years despite program rules. Figure 2.2 Panel A shows that among those who left before completing their term, 21% deserted, 15% were dismissed "for the convenience of the government" (e.g., the camp closed), 12% left for a job, and another 12% left because of an "urgent and proper call" (e.g., a family member was sick, though the specific reason is not generally noted).

Figure 2.2 also shows that those leaving before completing their term tend to have shorter average durations. Individuals with honorable discharges trained for longer, suggesting positive selection into duration. However, among those who quit early, the results are more ambiguous: individuals with "urgent and proper calls" trained less than those who deserted. Furthermore, those who were rejected upon further examination trained for just as long as those who were dismissed for the convenience of the government. Thus, short durations may have resulted from either positive or negative circumstances.

To investigate the determinants of duration we estimate simple OLS regressions of the duration of training as a function of individual, family, and camp characteristics. We include year-of-birth fixed effects (YOB) because different cohorts were eligible to train for different

Figure 2.2: Distribution of Reason for Discharge



(a) CCC (b) Jobs Corps

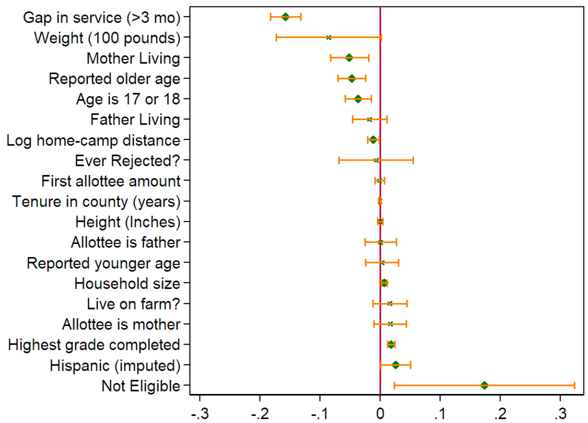
Note: Values on top of the bar graph are mean duration (in years) for each category: EOT (End of Term), Emp (employment outside the program), COG (Convenience of the Government), UrgProp (Urgent and Proper Call), Desert, Rej (Rejected), No Rec (No record). Reasons for Jobs Corps was harmonized to match with CCC's reasons for discharge.

amounts of time (Data Appendix Figure 5). We include county-of-enlistment by quarter-of-enlistment (CQE) fixed effects for two reasons. This addresses the fact that the number and types of camps that were opened varied over time and space, affecting where individuals ended up serving and potentially the duration of training. It also addresses differential selection based on location and time over the program years because the type of individuals who apply for training (and other government benefits) varies substantially with economic conditions (Mndez and Seplveda. 2012).

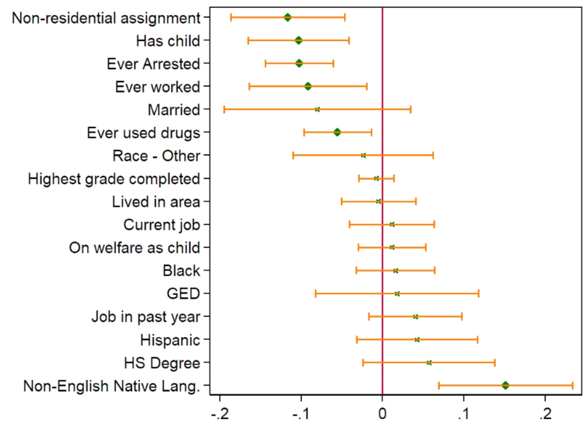
Figure 2.3 shows the results for selected characteristics, with full results shown in Appendix Table 2.A.2. In examining the relationship between personal characteristics and duration, no clear relationship emerges. Individuals who reported being older than they truly were trained for shorter durations whereas those who were older trained for longer durations. Those who were farther away from home also trained for shorter.[37] Surprisingly,

[37]Other traits predict durations: e.g. those who were paid more and those that were not juniors trained longer.

Figure 2.3: Determinants of Duration



(a) CCC



(b) Jobs Corps

Note: Estimates and 95% confidence intervals plotted for coefficient estimates on selected variables from regressing duration on various individual, camp, and peer characteristics. Coefficients in diamond are statistically significant at the 95% level. Mean duration for the estimation sample is 0.84 years for CCC and 0.49 years for Jobs Corps. Full results of the regression estimates are shown in Appendix Table 2.A.2.

individuals with a higher weight, who were presumably healthier individuals, trained for *shorter* durations. Height, which is a marker of improved nutrition and health during the growing years, does not predict training duration. Those with more education trained for longer but so did those who came from larger households or whose parents were deceased.[38]

This evidence is not consistent with a single narrative of selection. There appear to be three groups of enrollees. First, some who served for longer because they were positively selected, such as those with more education, older, or honorably dismissed. A second group seems to be negatively selected and in need of the CCC payments, such as those from more disadvantaged backgrounds. Third, some appear to have more or less random reasons due to good or bad luck, such as a job appearing, a camp closing or having an emergency at home.

[38]These results are qualitatively similar if we estimate regressions separately for CO and NM (see Appendix Table 2.A.2) but some coefficients are only significant in one state. Notably Hispanics were more likely to train longer in NM but not in CO. Individuals who were older than they reported trained longer in CO but not in NM. Weather is a significant predictor in CO but not in NM. There are no cases in which the coefficients are statistically significant and of opposite signs.

The evidence also suggests that, conditional on individual characteristics and place and time of enrollment, camp conditions mattered, as shown in Appendix Table 2.A.2. For instance, in places with less rain and milder weather, individuals trained for longer, as did those assigned to camps farther from cities. Peer characteristics also mattered. Durations were longer in camps with larger Hispanic shares of the population or with more men under 18, but shorter in camps with many men who misrepresented their age or sent smaller amounts to their families.

In sum, the primary evidence shows that desirable traits in an enrollee or in a camp did not necessarily lead to longer durations, and there is no single narrative of selection.

2.6 The Long-Term Effect of CCC Training on Mortality and Lifetime Earnings

We now investigate the effect of duration of enrollment on lifetime outcomes, namely mortality and earnings.

2.6.1 Mortality results

For this analysis, we restrict attention to individuals who died after age 45 to avoid WWII related deaths and who have been linked to a death certificate. The results are not sensitive to these restrictions. Figure 2.4 shows the relationship between average duration of training and mean age at death among CCC men: the longer an enrollee trained, the longer he lived. The relationship is positive and linear. Figure 2.5 shows the estimated density of the age at death for individuals who trained for less than one term, between 1 and 2 terms, and more than three terms. The distribution of the age at death appears to shift to the right for those who trained for longer.

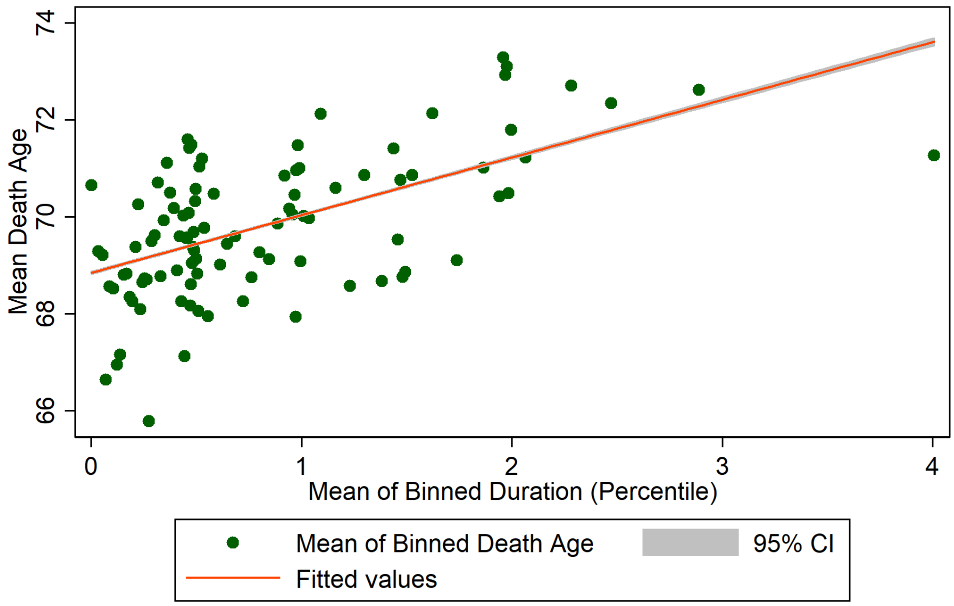
Next, we estimate an accelerated failure time model of the age at death on duration

Table 2.2: Effect of Service Duration on Longevity and Lifetime Earnings

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
Panel A: longevity for the full sample							
Duration of service (yrs)	0.013***	0.013***	0.011***	0.011***	0.013***	0.013***	0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Observations	17,086	17,086	17,086	17,086	17,086	17,086	10,944
R-squared	0.003	0.117	0.126	0.127	0.128	0.138	0.149
Mean Dep	73.62	73.62	73.62	73.62	73.62	73.62	73.30
Implied AIME Increase	-5.23	67.83	62.17	61.62	59.31	53.38	48.31
Panel B: what is the effect of duration on PIA in the MBR sample? (Claimed 1979 and later)							
Duration of service (yrs)	-1.675***	21.706***	19.893***	19.717***	18.979***	17.083***	15.459***
	(2.869)	(3.743)	(3.827)	(3.841)	(4.284)	(4.636)	(5.414)
Observations	10,241	10,241	10,241	10,241	10,241	10,241	6,525
R-squared	0.000	0.200	0.215	0.216	0.218	0.233	0.254
Mean Dep	437.70	437.70	437.70	437.70	437.70	437.70	449.34
Panel C: what is the effect of duration on PIA in the MBR sample? (Claimed earlier than 1979)							
Duration of service (yrs)	13.075***	12.552**	12.692**	10.713*	8.819	8.792	8.088
	(3.857)	(6.107)	(6.313)	(6.481)	(7.394)	(10.585)	(11.020)
Observations	1,562	1,562	1,562	1,562	1,562	1,562	1,284
R-squared	0.007	0.456	0.503	0.507	0.511	0.557	0.526
Mean Dep	314.02	314.02	314.02	314.02	314.02	314.02	317.41
Implied AIME Increase	40.86	39.23	39.66	33.48	27.56	27.47	25.28

Robust standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$.Sample is restricted only to those that died after age ≥ 45.

Figure 2.4: Longevity Increases with CCC Service Duration



Each mean of death age and duration was calculated on percentile bins of duration

Notes: figure plots the linear fit of mean death age within each percentile bin of duration. Data: Administrative records matched to death certificates. See text for more details.

in which we add controls for the characteristics of the enrollees and the camps to examine whether and how our estimates change in response. The first column of Table 2.2 with no controls shows a very precise coefficient on duration of 0.013. Controlling for cohort fixed-effects and county-of-enrollment-quarter-of-the-year (CQE) fixed-effects (column 2) does not change the coefficient estimate. Including family and individual characteristics (column 3) lowers the coefficient to 0.011. Adding camp characteristics (column 4), peer characteristics (column 5), or camp fixed effects (column 6) changes the coefficient very little. The magnitudes imply that one more year of training increased the age at death by one year (roughly 1.3 percent of 73.6 years of life). When we limit our sample to CO where the records contain a lot more important baseline information, such as education, height, etc., the results are again similar (column 7).[39]

[39] For NM and for CO records with missing data we impute using the mean and include a series for dummies to indicate when the covariate is missing.

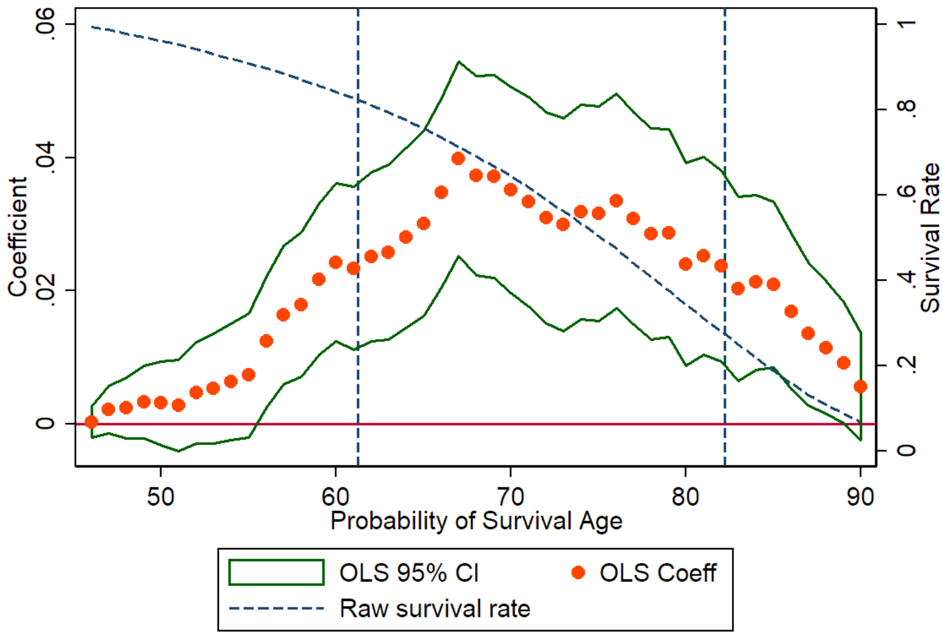
Figure 2.5: CCC Enrollees Who Served More Terms Lived Longer

Notes: this figure plots the estimated density of the age at death, separately for those serving less than one term, 1-2 terms and 3 terms or more.

The fact that the coefficient is essentially unchanged from columns 1-7 suggests that selection bias may be small. However, to more formally assess the magnitude of the omitted variable bias, we re-estimate these coefficients under various assumptions about the unobservables following Oster (2017). If delta (the proportionality value) is assumed to be 1 (i.e., unobservables as important as observables) then our coefficient would be 0.0136. Alternatively, if delta is assumed to be -1, we would estimate 0.0127. Thus, one more year of training would increase the age at death between 0.96 and 1.02 years.

Coefficients on other covariates (shown in Appendix Table 2.A.3) are interesting and shed some light on the issue of selection. They show that variables that predict longer duration do not always predict longer lives, providing additional evidence that selection into duration is unlikely to drive our results. More educated individuals trained longer and lived longer as well. Similarly, individuals who were accepted but eventually rejected trained for shorter durations and lived shorter lives, consistent with accounts that these shorter durations were

Figure 2.6: Effect of Service Duration on the Probability of Survival to Different Ages



Notes: On the left y-axis, this figure reports the coefficients (and standard errors) from running linear regressions of the probability that the person survived to a given age a on duration, where age ranges from age 45 to age 90. The regressions use the administrative data we collected and control for all observables at baseline (see Table 2.2 for details). On the right y-axis we plot the survival rate.

mostly related to physical disabilities. On the other hand, individuals who were older than they reported, trained for longer durations but lived shorter lives. Similarly, those who lived far away trained longer but lived shorter lives.

Finally, to examine possible non-linearities, Figure 2.6 shows the results of the regression of probability of survival to age x on duration for every age between 45 and 90. The coefficients are small and statistically insignificant at younger ages, when the survival is very high. They become positive and statistically significant starting at age 56 and continue to increase and peak between ages 68 and 78, and then decline thereafter. As a function of the baseline survival rate, which is declining throughout, the effects rise until age 67, and then decline.

Sample attrition. About 20% of the original sample is missing information on age at death. We assess whether missing age at death is systematically related to training duration (with or without conditioning on covariates). Table 2.3 shows that, without controls, the missing rates are not a function of training duration. But conditional on camp, family and individual characteristics, age at death is about 9% *less* likely to be missing for those who trained for an additional year. This suggests that differential attrition could bias our OLS estimates. To address this issue, we estimate survival models where we make various assumptions about the missing data. The results in Appendix Table 2.A.4 show that our findings are robust to various imputation approaches.

Quality of the longevity data. Our main results use the information found by trained genealogists from multiple sources to determine the age at death. To assess the quality of the data and whether the hand matching procedure introduces unknown biases, we replicate the results using machine matches only. To do this we use the EM algorithm to match our records to the Death Master File. The results in Appendix Table 2.A.8 show that we still obtain a positive and statistically significant coefficient of duration on age at death that is very similar in magnitude to our main estimates.

2.6.2 Lifetime income results

We examine the effects of the program on lifetime income by investigating the effects of duration on the Primary Insurance Amount (PIA), our proxy for lifetime earnings. The PIA is the amount of Social Security pension an individual would receive if they retired at the normal retirement age. To compute PIA, the SSA takes the average of an individuals' best 35 years of earnings, known as the AIME (Average Indexed Monthly Earnings). The SSA transforms the AIME into pension amounts using a non-linear formula, where each additional dollar earned is weighted by a smaller factor as earnings increase, with the weights tapering to zero. Thus, the formula compresses the distribution of PIA compared to the distribution of earnings. Because the PIA computation changed in 1979, we focus on results post 1979, but show results for the pre-1979 sample as well.

Table 2.3: Effect of Service Duration on Missing Data and Sample Selection

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
Panel A: Does duration predict whether longevity is missing?							
Duration of service (yrs)	0.001	-0.017***	-0.020***	-0.020***	-0.017***	-0.015***	-0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	22,964	22,964	22,964	22,964	22,964	22,964	14,116
R-squared	0.000	0.111	0.196	0.197	0.198	0.206	0.200
Mean Dep	0.18	0.18	0.18	0.18	0.18	0.18	0.15
Panel B: does duration predict being in the MBR sample?							
Duration of service (yrs)	-0.006	0.004***	0.010*	0.011*	0.009	0.005	0.002
	(0.005)	(0.001)	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)
Observations	22,980	22,980	22,980	22,980	22,980	22,980	14,116
R-squared	0.000	0.102	0.205	0.206	0.206	0.212	0.187
Mean Dep	0.53	0.53	0.53	0.53	0.53	0.53	0.57
Panel C: is the effect of duration on longevity for the MBR sample the same as in the full sample?							
Duration of service (yrs)	0.013***	0.010***	0.009***	0.009***	0.012***	0.011***	0.014***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Observations	11,953	11,953	11,953	11,953	11,953	11,953	7,913
R-squared	0.005	0.157	0.169	0.169	0.170	0.185	0.190
Mean Dep	74.81	74.81	74.81	74.81	74.81	74.81	74.78

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

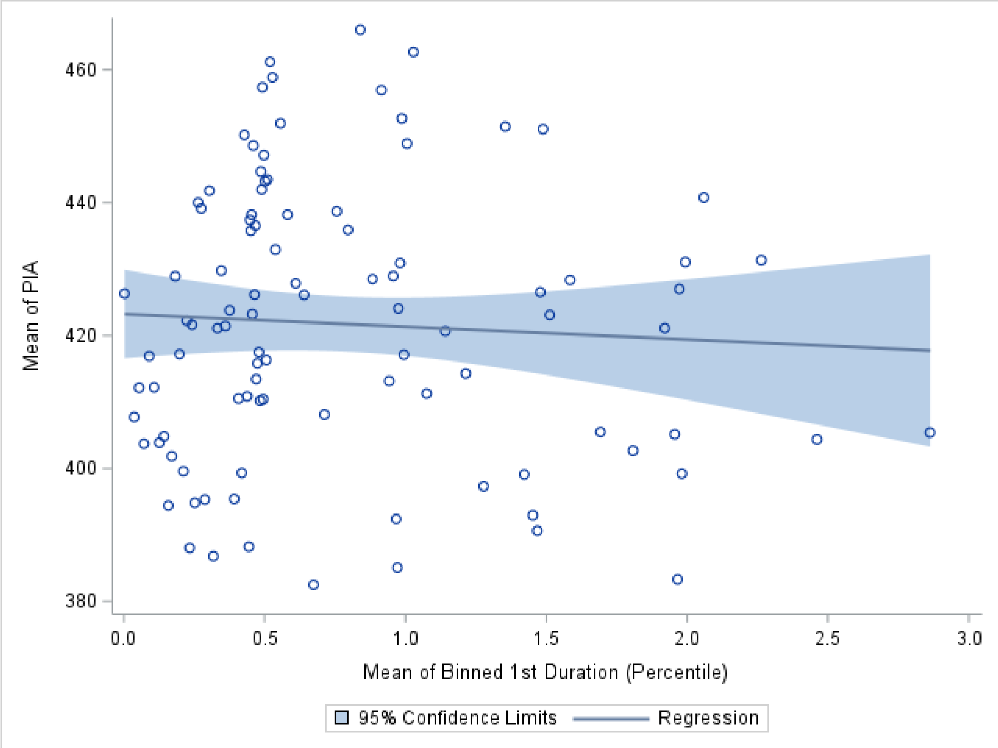
We plot the mean PIA as a function of duration for the sample claiming after 1979 and find a flat or slightly negative relationship between duration and PIA (Figure 2.7). We investigate these results further by estimating OLS regressions. Without covariates there is indeed a negative and statistically significant relationship that is however very small in magnitude (Table 2.2 Panel B, column 1). This relationship reverses and becomes positive and statistically significant when we add controls for birth cohort and for quarter and county of enlistment (column 2). It remains stable thereafter regardless of the additional controls we add: one more year of training increases the pension amount by \$17 per month, or 3.9%. If we convert this to the effect on the AIME, it corresponds to a 5.9% increase relative to the mean. Panel C shows we obtain similar results in the sample who claimed before 1979: increasing training by a year raises earnings by 2.7%, though this is not statistically significant. If we compute the weighted average between the two samples, we arrive at a 3.7% increase in earnings for the entire sample.

These results do not appear to be driven by sample selection or attrition in the SSA data. This can be seen from Table 2.3 Panel B, which shows that there is no effect of duration on whether we match an enrollee to MBR. As an additional check, Table 2.3 Panel C shows that, even when restricting the sample to those matched to the MBR, the effect of duration on longevity is very close to the results from the full sample.

We can compare the size of the gains by comparing our returns to the returns to schooling. The coefficient of years of schooling in the regression of column 6 of Panel B is 10.57 (s.e. = 1.4) so the effect of a year of school on the PIA is smaller than the \$17 we estimate for one year of JC, though schooling is measured with error, potentially causing downward bias in the estimate of schooling. Alternatively, OLS estimates of the returns to schooling for our cohorts range from 5% (Goldin and Katz 2000) to 8% (Clay et al 2012). Based on these estimates, the returns to one year of CCC training are roughly equivalent to a year of school or a bit larger.

For reference, the latest evaluation of JC, which tracks individual tax records 20 years

Figure 2.7: CCC Duration and PIA



Notes: Authors computation based on administrative program data matched to the Master Beneficiary Records.

after the program, finds that participation in JC had a statistically insignificant increase in wages of 2%, with our effects well within their confidence interval [-4%; 8%]. There are at least two reasons why the returns are lower for JC:The JC evaluation only uses 15 years of labor market outcomes, whereas we use 35 years. The shorter length of the evaluation may lower the estimated returns. Alternatively, the post-WWII economy was better for low-skilled labor than the economy of the early 2000s, which had stagnant wages for low-income groups (Piketty, Saez and Zucman 2018).

2.6.3 Treatment Effect Heterogeneity

A recent meta-analysis of 200 training programs around the world by Card, Kluve, and Weber (2018) suggests substantial heterogeneity in estimated impacts. Other recent reviews

(Barnow and Smith 2015, Crpon and van den Berg 2016) come to similar conclusions. While we cannot evaluate gender differences (women were ineligible for the CCC), we can investigate other differences that have been found important, such as age, SES and economic conditions at the time of enlistment (Appendix Table 2.A.5).[40]

We find that the poorest and most disadvantaged benefitted more, provided they were in good health. The effects were also larger when unemployment was higher. These findings are consistent with Card et al. (2018)'s finding of larger effects of job training in recessions and among the more disadvantaged. Our results differ in one important way from existing work: we find larger pension gains for the young, and significant benefits for Hispanics.[41]

2.7 Short-Term Outcomes: Evidence from the 1940 Census and WWII Enlistment Records

What might explain the long-run effects? To investigate, we examine the impact of training on short-run outcomes. First, we investigate the effects on employment and wages, the standard outcomes that are typically assessed in job training programs. Next we investigate other mechanisms that include formal education, health improvements, and geographic mobility.

2.7.1 Labor market outcomes: Evidence from the 1940 census

Table 2.4 shows estimated effect of training duration on outcomes as measured in the 1940 census. We constrain our sample to 9,623 men who participated in CCC before January 1$^{\text{st}}$,

[40] Effects are generally larger for women partly explaining why our results are more modest than those found by Attanasio et al. (2017) (or Kugler et al. 2015) who evaluate programs in Colombia after ten years and find large effects on earnings of about 11% for men and 18% for women. But our estimates for men are still lower than what these studies find.

[41] We suspect these differences are due to several factors: 1-we compute Hispanic ancestry and do not rely on self-reports, b-our enrollees are from only 2 states with large number of Hispanics in the population; 3-the country of origin among our enrollees differs substantially from today.

1940, of whom we find 43% percent in the 1940 census.[42] On average these men had left the CCC two years before the 1940 census.

CCC training duration appears to have little effect on the short-run labor market outcomes of CCC enlistees. Most men (91%) are in the labor force, and longer CCC training had at best a very small effect on this outcome: a 1.5% increase relative to the mean of 0.91. We observe no effect on employment (conditional on labor force participation) during the week prior to the Census. There appears to be a small, negative and imprecise effect of duration on weeks worked and earnings.[43] Overall, our results are consistent with the observation in recent reviews that the labor market effects are more positive in the long run than in the short run.

2.7.2 Health and military service: Evidence from WWII enlistment records

Table 2.5 presents results on other short-run outcomes. Because these outcomes are observed at different points in time, in these regressions we include age at enlistment dummies. Duration does predict whether we find enrollees in these records. Each year of CCC training leads to about a 3-percentage point increase in the probability we find the individual in the WWII enlistment records, about a 10% increase relative to the mean, robust and statistically significant. This result is not surprising: the army organized and administered life in the camps, and CCC men who trained for a long time were well acquainted with military life. Some men (2% in our data) ended their CCC engagement to enlist in the military directly, particularly toward the end of the program in 1942. Given that we have not found differential matching rates in any of our other data, we do not believe differential matching explains this result. Rather, we conclude that the program made men more likely to serve.

[42] Duration does not predict whether we find an enrollee in the 1940 census once we include birth cohort and county-quarter fixed effects (Table 2.4 top panel).

[43] For example, the largest coefficient for weeks worked is -0.937 which corresponds to 3.4% change relative to the mean of 28 weeks worked. Similarly, we observe a negative but statistically insignificant effect on earnings, corresponding to about a 3% decrease in wages at the mean.

Table 2.4: Effect of Service Duration on Labor Market Outcomes Observed in the 1940 Census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	No Controls	Add Birth, County-qtr Dummies	Add Indiv Controls	Add Camp Chars	Add Peer Chars	Add Camp FE	CO Only
Census							
Found in Census Records						Mean Dep	0.43
Duration of service (yrs)	-0.015**	0.009	0.007	0.009	0.006	0.012	0.011
	(0.007)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)
Observations	9,518	9,518	9,518	9,518	9,518	9,518	7,553
R-squared	0.001	0.137	0.152	0.154	0.155	0.166	0.154
In Labor Force						Mean Dep	0.91
Duration of service (yrs)	0.014**	0.013*	0.013*	0.015**	0.016*	0.019*	0.018*
	(0.006)	(0.007)	(0.007)	(0.007)	(0.009)	(0.010)	(0.011)
Observations	4,052	4,052	4,052	4,052	4,052	4,052	3,374
R-squared	0.001	0.272	0.279	0.280	0.280	0.305	0.286
Working In Census Week — Labor Force						Mean Dep	0.71
Duration of service (yrs)	0.006	-0.004	-0.005	-0.004	-0.010	-0.015	-0.011
	(0.011)	(0.014)	(0.014)	(0.014)	(0.019)	(0.022)	(0.023)
Observations	3,684	3,684	3,684	3,684	3,684	3,684	3,067
R-squared	0.000	0.265	0.279	0.283	0.286	0.310	0.295

Weeks Worked in 1939ˆ						Mean Dep	27.88
Duration of service (yrs)	0.669	-0.691	-0.911	-0.937	-0.896	0.265	0.227
	(0.732)	(1.044)	(1.049)	(1.029)	(1.082)	(1.199)	(1.213)
Observations	2,360	2,360	2,360	2,360	2,360	2,360	2,208
R-squared	0.000	0.314	0.345	0.351	0.354	0.383	0.361
Total Annual Wage in 1939ˆ						Mean Dep	383.71
Duration of service (yrs)	16.773	-12.266	-18.948	-20.038	-21.185	-14.497	-14.633
	(16.061)	(23.145)	(23.911)	(23.533)	(25.577)	(26.389)	(26.652)
Observations	2,148	2,148	2,148	2,148	2,148	2,148	2,011
R-squared	0.001	0.318	0.352	0.357	0.359	0.391	0.376
Ln Total Annual Wage — Workingˆ						Mean Dep	471.25
Duration of service (yrs)	0.047	-0.035	-0.047	-0.042	-0.051	-0.014	-0.012
	(0.039)	(0.052)	(0.051)	(0.052)	(0.058)	(0.062)	(0.062)
Observations	1,749	1,749	1,749	1,749	1,749	1,749	1,649
R-squared	0.001	0.396	0.447	0.452	0.454	0.487	0.456

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sample are those whose first term in CCC is before 1940 and are not enrolled in 1940. The 1940 Census was taken on April 1, 1940. ˆSample are those whose first term in CCC is before 1939 and are not enrolled in 1939. Census asks labor force and work status on the week before the Census enumeration, while wage information and weeks worked is asked for the year before the Census 1939.

Table 2.5: Effect of Service Duration on WWII Service, Health and Education Observed in WWII Enlistment and 1940 Census

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
WW2							
Found in WWII Records						Mean Dep	0.31
Duration of service (yrs)	0.018***	0.036***	0.034***	0.035***	0.038***	0.038***	0.042***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)
Observations	22,963	22,963	22,963	22,963	22,963	22,963	14,116
Enlistment Year						Mean Dep	1942.24
Duration of service (yrs)	-0.181***	0.976***	0.975***	0.976***	0.966***	0.962***	0.964***
	(0.025)	(0.008)	(0.008)	(0.008)	(0.009)	(0.010)	(0.011)
Observations	7,018	7,018	7,018	7,018	7,018	7,018	4,785
Height						Mean Dep	67.55
Duration of service (yrs)	-0.022	1.098***	1.098***	1.097***	1.161***	1.143***	1.207***
	(0.103)	(0.190)	(0.191)	(0.190)	(0.209)	(0.221)	(0.276)
Observations	5,770	5,770	5,770	5,770	5,770	5,770	3,816

BMI						Mean Dep	21.53
Duration of service (yrs)	-0.134**	0.789***	0.829***	0.822***	0.874***	1.018***	1.157***
	(0.064)	(0.191)	(0.191)	(0.190)	(0.195)	(0.204)	(0.265)
Observations	5,287	5,287	5,287	5,287	5,287	5,287	3,454
Combined WW2 Census							
Education						Mean Dep	9.23
Duration of service (yrs)	-0.072**	0.299***	0.185***	0.186***	0.188***	0.169***	0.115***
	(0.035)	(0.041)	(0.035)	(0.036)	(0.038)	(0.040)	(0.043)
Observations	9,586	9,586	9,586	9,586	9,586	9,586	6,907

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sample are those found in WWII records. WWII: additionally includes the age at enlistment dummies. Combined: additionally includes age at observation dummies, where if observed in Census, the age is 1940 - birth year.

We observe measured height and BMI in the WWII enlistment data for about 7,300 observations. We find that one more year of training translated into roughly 1 more inch of height—this result is statistically significant and relatively robust to the inclusion of covariates once cohort dummies are included as controls.[44] While this effect is small relative to the mean (about 1.5%), it is large by historical standards: for example, it took British men 100 years for their average height to increase by 6 inches (Fogel 1994). This result holds conditional on height at enlistment, so it corresponds to additional growth rather than initial differences in height.

It might seem surprising that the program increased heights given that these enrollees' average age is 19. However, undernourished populations grow more slowly and achieve their final adult height at older ages (Steckel 1986) and our results are consistent with this. Also note that our effects are consistent with national reports of the CCC program that the average height gain was half an inch (McEntee 1942). Our estimates are a bit larger, possibly because our population is more disadvantaged than the average CCC enrollee. Also recall that individuals in the CCC received food and medical care, including vaccinations, as part of their participation in the program, likely improving their nutritional status. Finally recall that many individuals (9% in our estimation) were likely younger than they reported.

Consistent with this, the results for BMI, which is a commonly used indicator of short-term nutrition, also show statistically significant increases, across specifications, implying gains of about 5-6% on BMI depending on the specification. The final CCC report documents an average weight gain of enrollees during the program of 11 pounds (McEntee 1942), and our results suggests that 40-60% of these gains persisted.[45]

[44] Controlling for cohort matters a lot because individuals born in more recent cohorts are taller but less likely to serve for long, since the program ended with WWII. This possibly explains the negative correlation in the raw data.

[45] For an average enrollee in our sample, adding 11 pounds would translate to a gain of 8%, so our results suggest that about 40-60% of the weight gain obtained during the program persisted.

2.7.3 Effects on education, and geographic mobility

We conclude by showing results on formal years of schooling and geographic mobility, which are observed in both the Census of 1940 and WWII Enlistment Records. For these outcomes, we combine information from the two sources to maximize sample size.[46] We control for the time since discharge (or equivalently the year of observation) to account for the fact the outcomes are not observed at the same time.

We find a positive and statistically significant effect of duration on years of schooling of about 0.18 years, relative to a mean of 9.4 years of schooling and controlling for education at baseline (Table 2.5). When we restrict our analysis to those with non-missing baseline education, the estimate declines to 0.12 and remains significant at the 5% level.[47] Though small in absolute terms, this represents one tenth of the standard deviation of schooling in WWII records, and it is larger than the effect of many education policies, such as child labor laws, on educational attainment during the early 20$^{\text{th}}$ century.[48]

This magnitude is somewhat larger than what one would expect based on the number of individuals that gained formal education during their CCC enlistment and suggests that perhaps individuals obtained school after participating in the CCC. CCC reports indicate that 8% of men obtained additional schooling during the program.[49] Assuming 8% obtained

[46] Because the WWII records contain the latest information, we take information from WWII if the enrollee can be found in WWII record and 1940 Census if cannot be found in WWII record and discharged before 1940. For education and marriage, we take the value at WWII, which is later than 1940, if observed in WWII and the value at 1940 Census if only observed in the Census. For moving, we code some as moved if they moved counties in either 1940 or in WWII. The results are not qualitatively different if we run the regressions separately although they are less frequently statistically significant as a result of the smaller sample size. Results available upon request.

[47] Results available upon request.

[48] For example, see Lleras-Muney (2002) or Goldin and Katz (2008). One more year of compulsory schooling led to about 0.05 years of schooling.

[49] The final report states that over one hundred thousand enrollees (3%) were taught how to read and write in the CCC program, 4% of men received primary school degrees (8$^{\text{th}}$ grade), 0.6% got their high school diplomas and a handful (270 out of more than 3 million) obtained college degrees. Thus, about 7-8% obtained some schooling.

one more year of school, this would result in a gain in years of schooling of 0.08, below but close to our estimate. Given that about 3.5% of enrollees in our data cited education as an explicit reason for leaving the program, post-CCC education gains likely accounts for the rest of the effect.

Finally, we look at the relationship between duration and short and long-term geographic mobility. In Table 2.6, we compare the county of individuals in their original CCC application with the county of residence indicated in the 1940 Census records, the WWII records and in the death certificates. Thirty five percent of participants moved in the short term. Training for more time in the CCC substantially increased the likelihood of moving. The coefficient on duration is positive and statistically significant in many specifications, hovering around 0.05; thus one more year of training increases the chance of moving by about 15%. This is substantial particularly during this period, which was characterized by historically low migration nationwide, at least across states.[50] Moreover, when CCC men moved, they moved to locations with higher paying weekly or annual wages in 1940, and thus potentially better economic opportunities, as well as lower mortality, measured by the average county level mortality from 1950 to 1968.[51] Over the long run, however, most individuals moved and the effect of duration on mobility fades.

In sum, enrollees who served longer had better health, more schooling and greater short-term mobility towards healthier, richer places. But in the short run, there appear to be no effects on labor market outcomes.[52]

[50] In the 1940 census 12% of people report living in a different county than in 1935. https://www.census.gov/dataviz/visualizations/010/

[51] We use this measure instead of the county mortality from 1940 onwards because of the disruptions that occurred during the WWII.

[52] For CO we have baseline measures for several outcomes: height, weight, education and prior labor market experience. In our main results we control for these. However, this allows us to test if duration predicts these pre-labor market outcomes. Appendix Table 2.A.6 shows that duration does not predict these pre-CCC outcomes, except for education. These results suggest that by in large our approach produces unbiased estimates of the effects of the program.

Table 2.6: Effect of Service Duration on Geographic Mobility Over the Lifetime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	No Controls	Add Birth, County-qtr Dummies	Add Indiv Controls	Add Camp Chars	Add Peer Chars	Add Camp FE	CO Only
Panel a: Short-term geographic mobility (Combined WW2 and Census)							
Moved to a Different State						Mean Dep	0.09
Duration	-0.014***	0.023***	0.024***	0.023***	0.028***	0.026***	0.033***
	(0.004)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)
Observations	9,568	9,568	9,568	9,568	9,568	9,568	6,891
Moved to a Different County						Mean Dep	0.33
Duration	0.006	0.053***	0.054***	0.053***	0.062***	0.057***	0.067***
	(0.007)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.012)
Observations	9,568	9,568	9,568	9,568	9,568	9,568	6,891
New County Has higher Yearly Wage than Sending County						Mean Dep	0.59
Duration	-0.005	0.046**	0.049**	0.047**	0.062**	0.077**	0.079**
	(0.020)	(0.020)	(0.020)	(0.021)	(0.026)	(0.034)	(0.035)
Observations	1,452	1,452	1,452	1,452	1,452	1,452	1,209
New County Has Above Median Mortality Rate (1950-1968)						Mean Dep	0.38
Duration	-0.061***	-0.071***	-0.068***	-0.072***	-0.068***	-0.065***	-0.064**
	(0.012)	(0.019)	(0.019)	(0.020)	(0.021)	(0.024)	(0.026)
Observations	3,003	3,003	3,003	3,003	3,003	3,003	2,403

Panel b: Long-term geographic mobility							
Died in a Different State						Mean Dep	0.5
Duration	-0.016*	-0.020*	-0.025**	-0.025**	-0.026**	-0.029*	-0.027*
	(0.008)	(0.012)	(0.012)	(0.012)	(0.013)	(0.015)	(0.015)
Observations	7,235	7,235	7,235	7,235	7,235	7,235	4,784
Died in a Different County						Mean Dep	0.8
Duration	0.003	-0.002	-0.004	-0.003	0.003	0.005	0.002
	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)
Observations	7,231	7,231	7,231	7,231	7,231	7,231	4,781
New County Has Above Median Mortality Rate (1950-1968)						Mean Dep	0.25
Duration	-0.030***	0.004	0.006	0.005	0.003	0.006	0.009
	(0.008)	(0.012)	(0.012)	(0.012)	(0.013)	(0.015)	(0.016)
Observations	5,313	5,313	5,313	5,313	5,313	5,313	3,678

We assume that the person lived in the county of application when definining wheter a person moved. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sample are those found in WWII records. WWII: additionally includes the age at enlistment dummies. Combined: additionally includes age at observation dummies, where if observed in Census, the age is 1940 - birth year.

2.8 Internal and External Validity: Comparisons to Modern Job Corps Program

To shed some light on the internal and external validity of our results, we analyze data from the modern Federal Job Corps program (JC hereafter), which was modeled after the CCC.[53] Using publicly available data from a randomized evaluation of the JC program conducted in 1994-1996, we first compare JC and CCC enrollees along a number of dimensions, including prior schooling and training duration. We then compare our estimated treatment effects (using OLS methods) with JC estimates based on randomization to assess the validity of our research design. We then compare estimates of duration in JC and the CCC on short run outcomes.

2.8.1 Comparing CCC and JC Enrollees.

JC participants differ from CCC participants in two key respects: JC includes women and married individuals, whereas the CCC excluded both. If we restrict attention to men in JC, Table 2.7 shows that overall, JC and CCC participants are similar. Both are young (19 years old on average) and have relatively few years of schooling. JC participants have completed 10 years of schooling, compared with 8.5 for the CCC enrollees, and 19% have graduated from high school compared with 12% of the CCC enrollees. Our sample has considerably more Hispanics, due to the fact we concentrate on CO and NM, whereas the JC data is national.

Participants are also similar in terms of duration of enrollment and reasons for unenrolling. Mean duration is 9.44 months (s.d. 7.47) for CCC and 5.8 months (s.d. 6.6) for JC.

[53]The current website (https://www.doleta.gov/job_corps/) states that "The program helps eligible young people ages 16 through 24 complete their high school education, trains them for meaningful careers, and assists them with obtaining employment." "Students can earn a high school diploma or the equivalent, and college credits. Job Corps also offers tuition-free housing, meals, basic health care, a living allowance, and career transition assistance."

Table 2.7: Characteristics of Eligible Job Corps Applicants and Comparison to CCC

	Jobs Cors Data		CCC
	All	Males	Males
Characteristic	Applicants	Only	Only
Baseline Characteristics			
Duration (in years, only positive durations)	0.67	0.652	0.819
Male	0.6	1	1
Age at application	18.8	18.728	18.75
White, non-Hispanic	0.3	0.304	NA
Black, non-Hispanic	0.5	0.451	NA
Hispanic	0.2	0.169	0.484
Other	0.1	0.076	NA
Years of education	10.2	10.042	8.581
High school diploma or more (including GED)	0.2	0.19	0.12
Ever arrested	0.3	0.332	NA
Had a job in the past year	0.6	0.662	NA
Ever had job	0.8	0.808	0.375
Average earnings in the past year (dollars)	2974.9	3255.74	NA
Mean for outcomes			
Duration for treated (years, duration > 0)	0.67	0.652	0.826
Duration for treated (years)	0.483	0.487	0.819
Years of school	11.145	11.07	9.403
Employment (in week of the survey)ˆ	0.606	0.631	0.71
Weeks worked in previous year	30.62	32.17	27.88
Total ann. earnings in prev. yr	10538.31	11947.78	382.43
Total ann. earnings in prev. yr (weeks worked > 0)	12990.85	14471.77	466.69
Movedˆˆ	0.198	0.207	0.34
Self-reported health status in 12 monthsˆˆˆ	1.786	1.733	NA
Self-reported health status in 30 monthsˆˆˆ	1.799	1.739	NA
Self-reported health status in 48 monthsˆˆˆ	1.809	1.757	NA
Self-reported health excellent or good (12-month)*	0.838	0.855	NA
Self-reported health excellent or good (30-month)*	0.838	0.856	NA
Self-reported health excellent or good (48-month)*	0.828	0.842	NA
Reason ended: End of term	0.31	0.302	0.378
Reason ended: Employment	0.042	0.038	0.116
Reason ended: Convenience of the government	0.001	0	0.145
Reason ended: Urgent and Proper Call	0.09	0.056	0.116
Reason ended: Deserted	0.331	0.373	0.223
Reason ended: Rejected upon examination	0	0	0.0101
Reason ended: No Record	0.228	0.232	0.0127
Observations: Baseline	14327	8646	NA
Observations: Outcomes	11313	6528	NA

Source: Baseline data. ˆemployment is not conditional on labor force participation. ˆˆfor Job Corps it is defined as living more than 20 miles away from baseline residence. For CCC it is defined as living in a different county than the county of residence at the time of enrollment. For Job Corps, employment is defined as having a job during the 208th week after the baseline survey (four years). ˆˆˆSelf-reported health status with 1 = excellent health, 2 = good, 3 = fair, and 4 = poor health. *Constructed variable that is equal to 1 if self-reported health of 1 (excellent health).

The main reason for the lower duration of the JC participants is that 20% never serve (Figure 2). Conditional on training, the duration among the treated group in JC is 7.8 months. Rates of completion are similar across the two programs as are the reasons for leaving.[54] Finally, and perhaps most importantly, when we try to predict duration in the JC, we also find evidence of both positive and negative selection into duration, as well as evidence that duration might have ended at a random time, just as we found in the CCC data (Figure 3).[55]

2.8.2 Comparison of experimental and non-experimental estimates.

We reproduce the JC randomized evaluation results in Schochet et al. (2008) using only the sample of males (Table 2.8).[56] In the first column we present estimates that compare the outcomes of those assigned to treatment to those of the control. In the second column we present the implied effects of training duration by estimating the implied 2SLS effect of duration using the randomized treatment status as an instrument. Thus, these estimates represent the causal effect of duration under a certain set of assumptions.[57] The third and fourth columns show the results of our OLS strategy for JC and for CCC, respectively, which we discuss below.[58]

[54]About 30% of JC enrollees complete the program, compared with 38% of the CCC. And of those who leave before completing, 30% in the JC and 22% in the CCC "deserted" while 12% and 4%, respectively, left because of employment opportunities.

[55]We find that education, Hispanic ethnicity, non-native speakers trained longer and individuals with a criminal history or those with shorter work histories trained for shorter periods of time (Figure 3 Panel B). As in the CCC, participants that found employment and those that deserted, were rejected or had urgent and proper calls also served shorter durations compared to those that completed their term.

[56]The results in the first column are almost identical to those in Schochet et al. (2008) except that we are restricting the sample to males and we constructed a few new outcomes (years of education, mobility and marriage). We can reproduce the full RCT results very closely using the full sample.

[57]Assuming that there are no heterogeneous treatment effects, and that the effect of training duration on the outcomes is linear starting at 0.

[58]Appendix Table 2.A.7 shows that the treated and control groups are balanced among males only suggesting that the RCT results for this subsample are valid. However, we show both groups since the original RCT was not designed or powered to estimated effects among males only.

Table 2.8: Comparison to Job Corps

	Jobs Corps Data			CCC
	RCT		OLS	OLS
	Coefficient on Treatment Dummy (ITT)	2SLS Instrument Duration with Treatment	Coefficient on Duration (years)+	Coefficient on Duration (years)
Years of school	0.184***	0.393	0.360***	0.169***
	(0.039)	(0.084)	(0.041)	(0.040)
N	6,280	6,280	3,407	9,620
Employment (in week of the survey)^	0.026**	0.056	0.060***	0.006
	(0.013)	(0.027)	(0.015)	(0.025)
N	6,022	6,022	3,285	2,686
Weeks worked in previous year	1.615***	3.443	2.629***	0.434
	(0.536)	(1.142)	(0.610)	(1.203)
N	6,235	6,235	3,382	2,383
Total Annual Earnings in previous year	969.765***	2,083.466	1,055.435***	-16.226
	(280.804)	(603.598)	(336.311)	(26.061)
N	6,081	6,081	3,317	2,168
ln(Earnings) (weeks worked>0)	0.038	0.080	0.078**	-0.010
	(0.027)	(0.057)	(0.031)	(0.061)
N	5,009	5,009	2,753	23,103
Moved^^	0.018*	0.038	0.060***	0.054***
	(0.011)	(0.023)	(0.014)	(0.011)
N	6,301	6,301	3,419	9,603

	Jobs Corps Data			CCC
	RCT		OLS	OLS
		2SLS		
	Coefficient on Treatment Dummy (ITT)	Instrument Duration with Treatment	Coefficient on Duration (years)+	Coefficient on Duration (years)
Self-reported health exc. or good (12-month)^^^	0.035***	0.073	0.020*	
	(0.009)	(0.020)	(0.010)	
N	5,920	5,920	3,234	
Self-reported health exc. or good (30-month)^^^	0.018*	0.037	0.020*	
	(0.010)	(0.020)	(0.011)	
N	5,458	5,458	2,944	
Self-reported health exc. or good (48-month)^^^	0.016*	0.034	0.013	
	(0.010)	(0.020)	(0.011)	
N	6,279	6,279	3,407	
Duration of training in months			5.829	
Individual controls?	No	No	Yes	Yes

Sample is males only. +Sample includes all treated, including those with zero duration. Controls include year and quarter of baseline, year and quarter of 48-mo followup survey, whether individual was enrolled in non-residential program and baseline characteristics such as whether individual had child, was ever arrested, had ever used drugs, had a job, had a job in the previous year, ever had a job, race, native language, on welfare as a child, education, baseline marital status and others. ^Employment is not conditional on labor force participation. ^^For Job Corps it is defined as living more than 20 miles away from baseline residence. For CCC it is defined as living in a different county than the county of residence at the time of enrollment. For Job Corps, employment is defined as having a job during the 208th week after the baseline survey (four years). Earnings conditional on employment only includes the earnings of individuals employed during the 208th week after the baseline survey. ^^^Constructed variable that is equal to 1 if self-reported health of 1 (excellent health).

OLS as a reasonable approximation of experimental estimates in the JC data. We find that OLS estimates are a reasonable approximation of the causal impact of duration on short run outcomes in the JC data. For example, the 2SLS estimate of duration on years of schooling using assignment to treatment as an instrument is 0.39. When we use data only from the treated group and estimate the effect of training duration on education using OLS, the coefficient we would estimate is 0.36, statistically indistinguishable from the experimental estimate. We find similar effects in the OLS and RCT settings for employment, weeks worked, and log earnings conditional on employment. The OLS approach underestimates annual earnings (including zeros) and over-estimates mobility, but the results are still similar given the estimated standard errors.

Comparing CCC estimates with the JC estimates. We find that the JC and the CCC programs both had positive and statistically significant effects on education, mobility and self-reported health. The latter two outcomes had not previously been examined, although the 20-year evaluation did report that a 40% reduction in SSDI benefits among JC participants. We find that JC increases the likelihood that respondents report being in excellent or very good health. Thus the JC evidence on health is similar to CCC findings for height and BMI.[59]

However, we find different effects of JC and CCC training on labor market outcomes. While small positive effects on employment, weeks worked, and annual earnings are found in the JC program, we find that the CCC program had no significant effects on employment and earnings. The differences might be due to the effects of experience: the labor market outcomes are measured on average only two years after leaving the CCC program, but they are measured 3 years out for JC. The differences could also be driven by the fact that labor market conditions differed at the time of the evaluation and were still quite dire in the 1930s and early 1940s.

[59]Results are similar if we use the entire scale or only look at whether the respondents are in excellent health alone.

Overall, we conclude that CCC participants are comparable in some important dimensions to JC participants: they are young and uneducated, and they participate in training for about 7 to 9 months. In the short run, both programs appear to raise educational attainment and geographic mobility and improve health. JC has more beneficial labor market effects in the short run. But CCC appears to have increased lifetime earnings, suggesting the 20-year evaluation might underestimate the effects of the program. This suggests that in the long-term JC participants will benefit from JC by living longer lives and enjoying greater pensions.

2.9 Discussion

In the long run, we find that individuals who participated longer in CCC had increased longevity by about 0.7 years. In the short run (within 2 years of training), we find no significant effects of training duration on labor force participation, employment, or earnings. We do however find significant income effects in the long-term despite no short-term effects, consistent with Card et al.'s (2018) findings. We also find small improvements in education and large increases in geographic mobility, height, and BMI.

Our longevity finding is consistent with the literature on the determinants of mortality. Height and normal BMI are both associated with longevity (Fogel 1994), and both indicators of health improved with CCC duration. The education of the men (formal and informal) also increased, and education is associated with longevity (Cutler et al. 2006). Greater lifetime earnings are also associated with lower mortality (Chetty et al. 2016). Finally, the enrollees moved to locations with lower mortality, which Finkelstein et al. (2019) and Deryugina and Molitor (2019) show also increase longevity.

Was the program worth it? We calculate the Marginal Value of Public Funds (MVPF) to answer the question, following the approach by Hendren and Sprung-Keyser (2019). CCC program costs we include are 1) upfront cost of the program and 2) increases in social security payouts from enrollees both living longer and having increased PIA. These costs are mitigated by tax increases from earnings benefits of the program. The program benefits include: 1) willingness to pay (WTP) for life extensions, 2) increase in after-tax earnings, 3) $30 per month wage paid (most of which went to families), and 4) the value of other services received by enrollees during the program, such as room and board. The MVPF is estimated to be 2.13 including the WTP for life increase, and 0.96 excluding the WTP for life increase.[60] Thus our conclusions are similar to Hendren and Sprung-Keyser (2019)[61] in that the MVFP is below one when computed based on earnings, but they differ once we incorporate longevity gains.

Historical accounts suggest that the program may have positively affected other "soft skills," improved mental health, and enlarged social networks, but we have no data to assess these effects. For example, enrollees reported making many life-long friendships and experiencing improvements in their state of mind. Additionally, the Army ran the CCC camps and imposed rules of behavior that were likely unusual for most individuals and may have been beneficial. Criminality is an important outcome which may have been affected as well. Though we do not observe these outcomes directly, we do observe that the CCC increased the probability that young men served in the Army, consistent with a change in either discipline or attitudes towards national service. Also, worth noting is that the program likely

[60] Assumptions made and details of calculation are presented in the Online Appendix. Some of the increases in life expectancy could lead to greater government spending through Medicare, potentially lowering the marginal value of public funds (Hendren and Sprung-Keyser 2019). Families received transfers, which could have benefitted them but also potentially distorted their behaviors. We do not have estimates of these effects. We do not assess the general equilibrium effects of job training programs. Recent research suggests these effects could be substantial and possibly offset the benefits to individuals (Crepon et al. 2013). Relatedly, the CCC program had impacts not just on the individuals that participated in the program but also on the communities where the CCC operated. In the short run these effects were driven in part by the economic and social activity that the camps generated, bringing men and resources to nearby towns. These short-term effects ended with the end of the program and the closure of the parks. But the changes in the landscape related to the building of national parks and forests, and the irrigation of the land, might have affected these communities more permanently. We do not incorporate these. Van der Berg et al. (2016) discuss this extensively—full cost benefit evaluations of training programs are rare and difficult to do.

[61] They compute an MVFP of 0.18 for JC. This computation does not incorporate the lower SSDI claims or the potential life extensions we compute here.

benefitted not just enrollees and their families, but the communities and the landscape where the CCC operated. Future work should attempt to measure these outcomes and conduct a more extensive cost-benefit evaluation of the program.

Our results have important implications for evaluations of job training programs. The vast majority of evaluations focus on labor market outcomes in the short- to medium-term and find small and/or insignificant effects. We confirm these findings in our data. But we observe large changes in lifetime outcomes that are not usually studied, namely health, military service, and geographic mobility. As previous scholars have noted, these findings suggest that it is essential to evaluate multiple mechanisms and indicators of well-being when assessing the impacts of various interventions.

Individuals entering the labor force today will begin their job search in the midst of high unemployment rates not seen since the Great Depression. Our results suggest that job training during periods of high unemployment has the potential to generate significant long-term benefits for participants, particularly on health outcomes. Thus, the results may be highly applicable to more modern periods with high rates of unemployment, such as the Great Recession of 2009 and the 2020 global pandemic. Our findings demonstrate these programs can improve both health and labor market outcomes. However, the contrast between our findings and the long-term evaluation of the JC program also underscores that the labor market benefits of training programs may depend heavily on the economic conditions that prevail in the decades that follow the program. Indeed, this point has been raised by Rosenzweig and Udry (2019) who have documented that the causal impact of multiple types of investment (including education) can vary significantly over time with changes in aggregate economic conditions. Whether and how the returns to modern job training programs varies with both current and future economic conditions is an important area for future monitoring and study.

2.A Appendix Tables

Table 2.A.1: Sample Selection

Sample Restriction	Itself	Sequential
All	26290	26290
Camp Exist	25165	25165
Enrollment Exist	24832	23943
Duration Exist	26050	23722
Final analytic sample	**26050**	**23722**
Death Age Exist	21457	19377
Death Age Restrict	24386	17639
Final analytic sample for mortality	**24386**	**17639**

The rows show many observations survive after dropping for each restriction. Itself column shows how many observations survive if we drop for just the restriction in the row. Sequential column shows the final observations that survive when we drop for each reason sequentially. Our working sample is 23,889, where we additionally lose observations to Death Age Exist for death age analysis

Table 2.A.2: Determinants of CCC Service Duration

	(1)	(2)	(3)	(4)	(5)	(6)
	Indiv Controls	Camp Controls	Indiv and Camp	Add County-qtr FE	CO Only	CO Non-missing Only
Individual characteristics						
Ever Rejected?	-0.201***		-0.020	-0.007	-0.009	0.060
	(0.033)		(0.034)	(0.031)	(0.034)	(0.038)
=1 if disabled	-0.446***		-0.464***	-0.328***	-0.363***	-0.237*
	(0.055)		(0.055)	(0.050)	(0.061)	(0.127)
Non-junior	0.834***		0.840***	0.509***	0.574***	0.005
	(0.122)		(0.119)	(0.097)	(0.127)	(0.235)
Reported Age Younger than DMFˆ	0.033*		0.026	0.003	0.003	-0.005
	(0.019)		(0.019)	(0.014)	(0.020)	(0.024)
Reported Age Older than DMF	0.081***		0.089***	-0.047***	-0.029*	-0.033
	(0.015)		(0.015)	(0.012)	(0.016)	(0.025)
Not Eligible	0.300**		0.265*	0.174**	0.186*	0.662***
	(0.139)		(0.141)	(0.077)	(0.106)	(0.134)
Age is 17 or 18	0.100***		0.103***	-0.037***	-0.045***	-0.020
	(0.014)		(0.014)	(0.011)	(0.014)	(0.021)
Allottee amount	0.058***		0.060***	-0.001	0.009	0.026***
	(0.004)		(0.005)	(0.004)	(0.006)	(0.009)
Allottee is father	0.045***		0.045***	0.001	0.001	-0.003
	(0.017)		(0.017)	(0.013)	(0.019)	(0.027)
Allottee is mother	0.045***		0.045***	0.017	0.030	0.012
	(0.017)		(0.016)	(0.014)	(0.019)	(0.027)
Gap in service	-0.201***		-0.156***	-0.158***	-0.126***	-0.113***
	(0.016)		(0.015)	(0.013)	(0.016)	(0.020)
Log distance from home to camp (miles)	-0.016***		-0.013**	-0.011**	-0.015***	-0.021**
	(0.005)		(0.005)	(0.005)	(0.006)	(0.008)
Hispanic (imputed using hispanic index)	0.078***		0.058***	0.026**	-0.014	0.007
	(0.014)		(0.014)	(0.013)	(0.017)	(0.019)
Highest grade completed (CO only)	0.024***		0.021***	0.019***	0.016***	0.007*
	(0.003)		(0.003)	(0.003)	(0.003)	(0.004)
Household size excluding applicant (CO only)	0.012***		0.013***	0.007***	0.008***	0.007***
	(0.003)		(0.003)	(0.002)	(0.002)	(0.003)
Live on farm? (CO only)	0.053***		0.053***	0.016	0.012	0.017
	(0.016)		(0.017)	(0.014)	(0.015)	(0.017)
Height (Inches) (CO only)	0.002		0.001	0.000	-0.000	-0.001
	(0.003)		(0.003)	(0.002)	(0.002)	(0.002)
Weight (100 pounds) (CO only)	-0.189***		-0.154***	-0.085*	-0.113**	-0.019
	(0.054)		(0.052)	(0.045)	(0.047)	(0.045)
Father Living (CO only)	-0.054***		-0.055***	-0.018	-0.015	-0.006
	(0.019)		(0.019)	(0.015)	(0.015)	(0.017)
Mother Living (CO only)	-0.088***		-0.095***	-0.051***	-0.056***	-0.032
	(0.021)		(0.021)	(0.016)	(0.017)	(0.024)
Tenure in county (years) (CO only)	-0.001		-0.001	-0.001	-0.001	-0.001
	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)

Camp characteristics						
=1 if camp is in enrollment state		-0.094***	0.053	0.154***	0.165***	-0.027
		(0.034)	(0.051)	(0.058)	(0.059)	(0.066)
Mean precipitation in camp 1933-1942		-0.001	-0.001	-0.004***	0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Mean min temp in camp 1933-1942		0.010	0.014**	0.030***	0.027***	0.012
		(0.006)	(0.006)	(0.008)	(0.008)	(0.010)
Mean max temp in camp 1933-1942		-0.018***	-0.021***	-0.034***	-0.022**	-0.006
		(0.006)	(0.006)	(0.007)	(0.009)	(0.011)
Camp Type: Department of Grazing		0.131***	0.123***	-0.075	0.117	-0.052
		(0.044)	(0.041)	(0.063)	(0.087)	(0.116)
Camp Type: Federal Reclamation Project		0.118**	0.099**	-0.055	0.147	0.031
		(0.047)	(0.045)	(0.070)	(0.096)	(0.120)
Camp Type: Fish and Wildlife Service		0.106**	0.024	-0.383***		
		(0.051)	(0.048)	(0.131)		
Camp Type: National Forest		0.008	-0.006	-0.106*	0.024	-0.091
		(0.043)	(0.041)	(0.060)	(0.078)	(0.109)
Camp Type: National Monument		0.145*	0.121	-0.303***	-0.265*	-0.166
		(0.088)	(0.084)	(0.090)	(0.147)	(0.179)
Camp Type: National Park		0.069	0.060	-0.117*	-0.012	-0.165
		(0.044)	(0.042)	(0.063)	(0.079)	(0.101)
Camp Type: Soil Conservation		0.121***	0.100***	-0.075	0.092	-0.070
		(0.040)	(0.038)	(0.059)	(0.080)	(0.108)
Camp Type: State Park		-0.031	-0.041	-0.119*	-0.078	-0.176
		(0.054)	(0.050)	(0.069)	(0.090)	(0.147)
Log distance to closest city (miles)		-0.007*	-0.007**	0.011**	0.000	0.022**
		(0.004)	(0.004)	(0.005)	(0.007)	(0.008)
Log distance to 2nd closest city (miles)		0.028	0.035*	-0.017	-0.044*	0.012
		(0.019)	(0.019)	(0.022)	(0.025)	(0.037)
Peer Char: Hispanic at enrollment		0.386***	0.239***	0.249***	0.015	0.051
		(0.044)	(0.047)	(0.070)	(0.071)	(0.098)
Peer Char: Age at enrollment		-0.200***	-0.235***	-0.319***	-0.313***	0.052
		(0.021)	(0.023)	(0.034)	(0.035)	(0.041)
Peer Char: Reported Age Younger than DMF		0.483***	0.381**	-0.607***	-0.579**	0.478*
		(0.170)	(0.169)	(0.211)	(0.254)	(0.262)
Peer Char: Reported Age Older than DMF		-0.276**	-0.452***	-1.025***	-0.814***	0.397
		(0.127)	(0.137)	(0.200)	(0.236)	(0.318)
Peer Char: Not Eligible (First enrollment)		1.861***	1.587***	1.349***	-0.295	1.949*
		(0.256)	(0.273)	(0.389)	(0.452)	(1.041)
Peer Char: Allottee amount		0.083***	0.030***	-0.255***	-0.360***	-0.305***
		(0.005)	(0.007)	(0.017)	(0.024)	(0.018)
Peer Char: Allottee: Father		-0.083	-0.120	0.019	-0.040	0.088
		(0.126)	(0.122)	(0.149)	(0.177)	(0.198)
Peer Char: Allottee: Mother		-0.163	-0.117	-0.032	-0.078	-0.221
		(0.126)	(0.128)	(0.133)	(0.147)	(0.202)
Peer Char: Gap in service		-0.931***	-0.692***	-0.652***	-0.156	-1.462***
		(0.098)	(0.099)	(0.133)	(0.140)	(0.191)
Constant	-1.457***	3.342***	2.800***	12.992***	14.686***	6.747***
	(0.458)	(0.518)	(0.569)	(0.868)	(0.991)	(0.807)
Observations	17,639	17,086	17,086	17,086	10,944	3,013
R-squared	0.181	0.160	0.222	0.574	0.482	0.465
Mean Dep	0.83	0.84	0.84	0.84	0.76	0.67
FE	BD	BD	BD	BD,CYQ	BD,CYQ	BD,CYQ
Sample	All	All	All	All	CO	CO
Reason	N	N	N	N	N	N
Number of County-Quarter Groups				1,789	1,231	477

Robust standard errors in parentheses *** p¡0.01, ** p¡0.05, * p¡0.1. Only Duration ¡= 3 years, death age ¿= 45 are included in regression. Variables imputed if missing and missing dummies included. County Unemployment is from ICPSR compilation of County statistics from 1937 Census of Unemployment and 1940 Decenniel Census. Those values are given to enrollment years 1937, 1938 for 1937 Census and 1939-1942 for 1940 Census. ˆ=1 if reported age in CCC documents is smaller than in the DMF, or maximum of all reported age for enrollee.

Table 2.A.3: Full Regressions of Log Death Age on Duration

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
Duration of service (yrs)	0.013***	0.013***	0.011***	0.011***	0.013***	0.013***	0.009**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
Ever Rejected?			-0.031***	-0.031***	-0.031***	-0.030***	-0.031***
			(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
=1 if disabled			-0.006	-0.006	-0.006	-0.004	0.008
			(0.016)	(0.016)	(0.016)	(0.016)	(0.023)
Non-junior			0.002	0.004	0.003	-0.000	-0.034
			(0.018)	(0.019)	(0.019)	(0.019)	(0.025)
Reported age younger than DMF^			-0.019***	-0.019***	-0.019***	-0.019***	-0.010*
			(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Reported age older than DMF			-0.022***	-0.022***	-0.022***	-0.022***	-0.017***
			(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Not Eligible			0.010	0.011	0.010	0.011	0.014
			(0.017)	(0.017)	(0.017)	(0.017)	(0.022)
Age is 17 or 18			0.007*	0.007*	0.007*	0.007*	0.004
			(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
First allottee amount (dollars per month)			0.000	0.000	0.000	0.000	0.000
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Allottee is father			0.008*	0.008*	0.008*	0.008	0.003
			(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Allottee is mother			0.001	0.001	0.001	0.001	-0.001
			(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Gap in service (more than 3 months)			0.001	0.001	0.001	0.001	-0.006
			(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Log distance from home to camp			0.001	0.002	0.002	0.002	0.002
			(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Hispanic (imputed using hispanic index)			0.018***	0.018***	0.018***	0.019***	0.018***
			(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
Highest grade completed (CO only)			0.004***	0.004***	0.004***	0.005***	0.005***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household size excluding applicant (CO only)			0.003***	0.003***	0.003***	0.003***	0.003***
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Live on farm? (CO only)			0.011*	0.011*	0.011*	0.011*	0.011**
			(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Height (Inches) (CO only)			0.001	0.001	0.001	0.001	0.001
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Weight (100 pounds) (CO only)			-0.042**	-0.041**	-0.041**	-0.041**	-0.041**

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
			(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Father Living (CO only)			0.000	0.001	0.000	-0.000	-0.000
			(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mother Living (CO only)			0.008	0.008	0.008	0.008	0.007
			(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Tenure in county (years) (CO only)			-0.001	-0.001	-0.001	-0.001	-0.001
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
=1 if camp is in enrollment state				-0.015	-0.017		
				(0.012)	(0.012)		
Mean precipitation in camp 1933-1942				0.000	0.000		
				(0.000)	(0.000)		
Mean min temp in camp 1933-1942				-0.002	-0.002		
				(0.001)	(0.002)		
Mean max temp in camp 1933-1942				0.000	0.001		
				(0.001)	(0.001)		
Camp Type: Department of Grazing				-0.020	-0.019		
				(0.024)	(0.024)		
Camp Type: Federal Reclamation Project				-0.017	-0.019		
				(0.025)	(0.026)		
Camp Type: Fish and Wildlife Service				-0.012	-0.013		
				(0.032)	(0.033)		
Camp Type: National Forest				-0.015	-0.013		
				(0.024)	(0.025)		
Camp Type: National Monument				-0.006	-0.001		
				(0.028)	(0.028)		
Camp Type: National Park				-0.021	-0.017		
				(0.024)	(0.025)		
Camp Type: Soil Conservation				-0.010	-0.007		
				(0.024)	(0.024)		
Camp Type: State Park				-0.013	-0.012		
				(0.024)	(0.025)		
Log distance to closest city				-0.002**	-0.002**		
				(0.001)	(0.001)		
Log distance to 2nd closest city				0.003	0.005		
				(0.006)	(0.006)		
Peer Char: Hispanic at enrollment					0.002	-0.024	-0.010
					(0.014)	(0.021)	(0.023)
Peer Char: Age at enrollment					0.011**	0.014**	0.011
					(0.005)	(0.006)	(0.007)
Peer Char: Reported Age Younger than DMF					0.006	-0.031	-0.058
					(0.043)	(0.057)	(0.066)

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
Peer Char: Reported Age Older than DMF					-0.017	-0.007	-0.054
					(0.029)	(0.037)	(0.040)
Peer Char: Not Eligible (First enrollment)					-0.029	-0.070	-0.188*
					(0.051)	(0.077)	(0.098)
Peer Char: Allottee amount					0.002	-0.000	0.004
					(0.002)	(0.003)	(0.004)
Peer Char: Allottee: Father					-0.050*	-0.079**	-0.081*
					(0.030)	(0.038)	(0.044)
Peer Char: Allottee: Mother					-0.004	0.003	0.019
					(0.025)	(0.031)	(0.036)
Peer Char: Gap in service					-0.025	-0.026	0.008
					(0.026)	(0.033)	(0.033)
Reason for discharge (code) = 2, Emp							-0.006
							(0.006)
Reason for discharge (code) = 3, COG							-0.007
							(0.006)
Reason for discharge (code) = 4, UrgProp							-0.009
							(0.006)
Reason for discharge (code) = 5, Desert							-0.017
							(0.019)
Reason for discharge (code) = 6, Rej							-0.016
							(0.031)
Reason for discharge (code) = 7, No Rec							0.001
							(0.019)
Honorable Discharge							0.007
							(0.019)
Constant	4.274***	4.391***	4.308***	4.294***	4.063***	4.363***	4.309***
	(0.002)	(0.137)	(0.159)	(0.168)	(0.206)	(0.162)	(0.183)
Observations	17,086	17,086	17,086	17,086	17,086	17,086	10,944
R-squared	0.003	0.117	0.126	0.127	0.128	0.138	0.149
Mean Dep	73.62	73.62	73.62	73.62	73.62	73.62	73.30
FE	None	BD,CYQ	BD,CYQ	BD,CYQ	BD,CYQ	BD,CYQ,Camp	BD,CYQ,Camp
Sample	All	All	All	All	All	All	CO
Number of groupayq		1,789	1,789	1,789	1,789	1,789	1,231

Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$, Only Duration $\leq$ 3 years, death age $\geq$ 45

Table 2.A.4: Effect of Service Duration on Survival Rates by Age

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Survival to age 70, mean: 0.65						
Duration	0.030***	0.032***	0.028***	0.035***	0.030***	0.030***
	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)
Observations	17,086					
Panel B: Survival to age 70 missing imputed, mean: 0.64						
Duration	0.022***	0.026***	0.023***	0.028***	0.023***	0.016**
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)
Observations	21,269					
Panel C: Survival to age 70 missing imputed to 0, mean: 0.52						
Duration	0.024***	0.037***	0.037***	0.040***	0.034***	0.020***
	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)
Observations	21,269					
County-Quarter FE	N	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
Peer + Camp Controls	N	N	N	Y	Y	Y
Camp FE	N	N	N	N	Y	Y
Type of Dismissal	N	N	N	N	N	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors (clustered at the application county and enrollment year-quarter level) in parentheses. Sample only includes death ages ≥ 45. Panel B imputes survival probability using the age at discharge, birth year, and life tables from SSA. Panel C imputes 0 for missing survival probability. Panel D implements the bounding procedure explained in the text.

Table 2.A.5: Heterogeneity in OLS effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	CO	NM	Age≤18	Age>18	Allottee Mother	Allottee Father	Allottee Other	Urate above median	Urate below median
Panel A: Log Death Age									
Duration of service : Enrollment 1	0.013***	0.014**	0.014***	0.013***	0.018***	0.009	0.011	0.017***	0.013
	(0.003)	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)	(0.009)	(0.004)	(0.009)
Observations	11,148	6,243	8,042	9,349	8,253	5,801	3,337	8,238	2,742
Panel B: PIA									
Duration of service : Enrollment 1	15.677***	19.203**	25.859***	11.645	12.425*	15.848*	31.316*	29.252***	15.337*
	(5.375)	(9.202)	(6.878)	(7.209)	(6.956)	(9.045)	(17.003)	(9.434)	(7.893)
Observations	6,641	3,779	5,680	4,740	5,077	3,536	1,807	3,415	3,520
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Sample	Hispanic	Non-Hispanic	BMI<18 (CO)	BMI 18.5-25 (CO)	BMI≥25 (CO)	Phase 2 (1935-1937)	Phase 3 (1937-1940)	Phase 4 (1940-1942)	Random-ized
Panel A: Log Death Age									
Duration of service : Enrollment 1	0.018***	0.009**	0.008	0.013**	0.098	0.022***	0.022***	0.015	0.020***
	(0.005)	(0.004)	(0.071)	(0.007)	(0.137)	(0.006)	(0.005)	(0.009)	(0.005)
Observations	7,864	9,527	433	5,627	290	3,852	7,256	6,049	5,170
Panel B: PIA									
Duration of service : Enrollment 1	19.052**	19.827***	-17.382	21.666**	-724.479	6.741	23.239***	40.099***	17.739**
	(7.672)	(6.285)	(127.386)	(9.369)	-	(9.789)	(7.662)	(14.172)	(8.346)
Observations	4,720	5,700	309	3,943	204	1,739	4,597	4,049	3,097

Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sample is restricted only to those that died after age ≥ 45 and restrictions described by column headings. The specification uses the most restrictive specification with Camp FE, which was the specification used in Table 2, Column 6.

Table 2.A.6: Placebo Tests for CO Only

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
Education	Mean Dep	8.72					
Duration of service (yrs)	0.223***	0.225**	0.261***	0.257***	0.216**	0.212*	0.212*
	(0.080)	(0.095)	(0.091)	(0.090)	(0.107)	(0.118)	(0.118)
N	2,987	2,987	2,987	2,987	2,987	2,987	2,987
Height	Mean Dep	67.94					
Duration of service (yrs)	-0.035	-0.218	-0.062	-0.054	-0.162	-0.209	-0.209
	(0.125)	(0.170)	(0.146)	(0.149)	(0.179)	(0.186)	(0.186)
N	2,334	2,334	2,334	2,334	2,334	2,334	2,334
Weight (100 pounds)	Mean Dep	1.40					
Duration of service (yrs)	-0.012*	-0.016	-0.008	-0.008	-0.005	-0.002	-0.002
	(0.007)	(0.011)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
N	2,067	2,067	2,067	2,067	2,067	2,067	2,067
Ever had a paid job	Mean Dep	0.45					
Duration	-0.007	-0.018	-0.048	-0.061	-0.065	-0.048	-0.048
	(0.032)	(0.051)	(0.048)	(0.047)	(0.049)	(0.059)	(0.059)
Observations	1,104	1,104	1,104	1,104	1,104	1,104	1,104

Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables are pre-program characteristics of individuals. Each column's specification corresponds to column specifications in Table 5. Regressions Do Not include imputed values

Table 2.A.7: Balance Test of Baseline Characteristics for Job Corps Applicants

	Full sample				Males only			
Characteristic	Treatment	Control	Difference		Treatment	Control	Difference	
Male	0.591	0.599	-0.008	(0.009)				
Age	18.861	18.826	0.035	(0.038)	18.735	18.717	0.018	(0.047)
White - Non-Hispanic	0.274	0.265	0.009	(0.008)	0.309	0.295	0.014	(0.01)
Black - Non-Hispanic	0.476	0.478	-0.002	(0.009)	0.45	0.452	-0.002	(0.011)
Hispanic	0.174	0.181	-0.007	(0.007)	0.163	0.178	-0.015*	(0.008)
Non-English Native Language	0.141	0.143	-0.001	(0.006)	0.14	0.144	-0.004	(0.008)
Has Child	0.181	0.179	0.002	(0.007)	0.106	0.108	-0.002	(0.007)
Childhood Household Head - Mother	0.483	0.49	-0.007	(0.009)	0.45	0.467	-0.016	(0.011)
Highest Grade Completed - Mother	11.516	11.539	-0.022	(0.051)	11.678	11.658	0.02	(0.062)
Highest Grade Completed - Father	11.471	11.578	-0.107	(0.064)	11.605	11.608	-0.003	(0.079)
Never on Welfare During Childhood	0.47	0.459	0.012	(0.009)	0.489	0.485	0.004	(0.012)
Highest Grade Completed	10.069	10.081	-0.012	(0.027)	9.953	9.969	-0.016	(0.032)
High School Degree	0.178	0.182	-0.004	(0.007)	0.139	0.142	-0.003	(0.008)
GED	0.047	0.055	-0.008*	(0.004)	0.05	0.052	-0.001	(0.005)
Ever Worked	0.8	0.788	0.011	(0.007)	0.812	0.801	0.011	(0.009)
Worked in Past Year	0.649	0.64	0.009	(0.008)	0.666	0.655	0.012	(0.01)
Currently has Job	0.215	0.208	0.007	(0.007)	0.221	0.204	0.017*	(0.009)
Months Worked in Past Year	6.055	6.127	-0.072	(0.092)	6.028	6.067	-0.039	(0.113)
Earnings in Past Year (if employed during past year)	3019.377	2903.822	115.556	(103.731)	3319.099	3156.064	163.035	(137.756)
Typical Hours Worked (if employed during past year)	35.635	35.344	0.291	(0.348)	36.922	36.73	0.192	(0.44)
Typical Wage (if employed during past year)	5.062	5.078	-0.017	(0.033)	5.167	5.194	-0.027	(0.042)
Received AFDC	0.316	0.316	-0.001	(0.009)	0.244	0.242	0.002	(0.01)
Received Food Stamps	0.437	0.446	-0.009	(0.009)	0.37	0.378	-0.008	(0.011)
Received Any Welfare	0.578	0.585	-0.007	(0.009)	0.511	0.518	-0.007	(0.012)
Ever Used Drugs	0.386	0.376	0.01	(0.009)	0.43	0.423	0.007	(0.011)
Ever Arrested	0.264	0.266	-0.001	(0.008)	0.337	0.326	0.011	(0.01)
Non-residential Job Corps Participant	0.137	0.141	-0.004	(0.006)	0.067	0.072	-0.005	(0.005)
Obs	8813	5514	14327		5036	3610	8646	

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Notes: Data source is baseline data for Job Corps program from Schochet et al. (2008). If employed during past year is measured as the individual worked for at least 2 weeks in the previous year.

Table 2.A.8: The Effect of Service Duration for Machine-Matched Sample

VARIABLES	(1) No Controls	(2) Add Birth, County-qtr Dummies	(3) Add Indiv Controls	(4) Add Camp Chars	(5) Add Peer Chars	(6) Add Camp FE	(7) CO Only
Panel A: Longevity from CCC for the machined-matched sample							
Duration of service (yrs)	0.013***	0.011***	0.010***	0.010***	0.011***	0.010**	0.017***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Observations	8,833	8,833	8,833	8,833	8,833	8,833	5,904
R-squared	0.003	0.186	0.192	0.194	0.195	0.212	0.220
Mean Dep	72.64	72.64	72.64	72.64	72.64	72.64	72.41
Panel B: Longevity from DMF for the matchine-matched sample							
Duration of service (yrs)	0.013***	0.011***	0.010***	0.010***	0.012***	0.012***	0.019***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Observations	9,175	9,175	9,175	9,175	9,175	9,175	6,071
R-squared	0.003	0.181	0.186	0.188	0.189	0.205	0.214
Mean Dep	72.65	72.65	72.65	72.65	72.65	72.65	72.42
Panel C: Does duration predict whether they are machine-matched to DMF?							
Duration of service (yrs)	0.015***	0.024***	0.026***	0.026***	0.024***	0.022***	0.020**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)
Observations	22,964	22,964	22,964	22,964	22,964	22,964	14,116
R-squared	0.000	0.110	0.153	0.153	0.154	0.161	0.165
Mean Dep	0.41	0.41	0.41	0.41	0.41	0.41	0.44

Robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. In Panel A, we use death age calculated from CCC birth year and death age from hand-matched sources. In Panel B we use death age calculated from DMF birth date and death date from the machine match. Sample is restricted only to those that died after age ≥ 45 for Panels A and B.

CHAPTER 3

The Effect of Inclusionary Zoning Policy on Local Housing Markets[1]

3.1 Introduction

Housing affordability has come to the forefront of urban policy debate in recent years. Secular increases in housing prices and gentrification have made even traditionally affordable neighborhoods into expensive places to live. Federal and local policy tools, such as Low-Income Housing Tax Credit (LIHTC) or housing vouchers, tackle housing affordability for the poor and try to encourage the creation of income-mixed neighborhoods.

In this chapter, I focus on one particular local policy measure: Inclusionary Zoning Policies (IZPs). IZPs incentivize or require property developers to set aside a certain proportion of units in new developments at below-market price. Termed "inclusionary" to directly contrast ordinary zoning policies that are designed to exclude certain types of properties, IZPs take on different forms across the U.S. Perhaps because IZPs vary widely across jurisdictions or because they do not exist at the scale of some federal programs, there are few papers examining their effectiveness in providing low-income housing.

Nonetheless, IZPs have been adopted or recently expanded in many municipalities, in-

[1]Data is provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of Zillow Group. Where noted, map tiles are provided by OpenStreetMap under Open Database License and cartography licensed as CC BY-SA. Please refer to https://www.openstreetmap.org/copyright for more details.

cluding America's three largest cities, New York, Los Angeles, and Chicago. It is likely these policies will spread, because they generally involve no direct expenditure of public funds, appear to directly address concerns about generating affordable housing, and provide a path forward for developers. Moreover, with the increased severity of the affordability crisis, jurisdictions are turning to stronger IZP tools. New York expanded the scope of IZPs in 2016. Los Angeles, which was limited by a legal decision striking down the enforceability of voluntary IZPs,[2] is exploring re-enforcing IZPs following a recent revision to a key California law. Chicago has had IZP in place since 2007, but revised the policy in 2015 and is currently considering significant revisions.

In this chapter, I analyze the causal impact of IZPs on housing supply and prices in New York City. I first introduce a model of developer profit with and without IZPs to analyze the determinants of developer participation and housing market response. Standard microeconomic theory predicts that any zoning policy that seeks to restrict land use should act as a tax on new developments, therefore dampening housing supply and increasing prices in the area, ultimately undermining housing affordability. In contrast, my theoretical analysis suggests that the effect crucially depends on local housing market characteristics and policy variations of different jurisdictions.

I then quantitatively explore the causal impact of IZPs on housing prices and supply using a differences-in-discontinuity design by exploiting the timing and variation in mandatory IZP in New York City. New York implemented mandatory IZP in 2016 requiring new developments within certain parts of the city to provide affordable units. Comparing the mandatory IZ areas to neighboring areas before and after the policy allows me to net out unobserved housing market characteristics that plague identification faced by existing studies in the literature. In particular, I extend the own-lot and external effects border-discontinuity design by Turner et al. (2014) to a differences-and-differences framework. This is an important contribution to the literature, as the approach in the existing literature, which compares

[2] *Palmer/Sixth Street Properties, L.P. v. City of Los Angeles*

jurisdictions with and without IZPs, is inadequate in dealing with potential endogeneity problems arising from unobserved, time-varying housing market characteristics, which are very likely to exist when comparing across jurisdictions (such as counties and cities).

My empirical results show that, while prices have increased following the policy, supply has also increased in response. My causal estimates suggest about a 18% short-term increase in prices and a 15% short-term increases in supply. Moreover, the policy does not seem to have meaningful spillover effects to nearby areas. These results are inconsistent with simple models of IZPs as a tax on new housing development. However, it is also inconsistent with increased housing affordability, which makes a straightforward interpretation of the policy's impact difficult. This suggests that a more careful consideration of the impact of IZPs on local housing markets is needed.

The rest of this chapter is structured as follows. In the rest of this section, I provide background on IZPs and review the literature surrounding the evaluation of IZPs on local housing markets. In Section 3.2, I explore a theoretical foundation on the developer's decision to take on IZ projects in the framework of density bonuses. In Section 3.3, I describe my empirical strategy based on Turner et al. (2014). In Section 3.4, I describe the data source that will be used in estimation. In Section 3.5, I present my findings on MIZ's impact on building activity and housing prices.

3.1.1 Background on IZPs and New York's Implementation

Usually, IZPs require or incentivize developers to set aside x% of new units to households earning y% of Area Median Income (AMI) in exchange for extra density or tax benefits. However, IZPs take on a variety of flavors and utilize a wide set of policy tools (Schwartz et al. 2012, Schuetz, Meltzer, Been 2009):

1. **Mandatory status**: whether developer compliance to set-aside rules are voluntary or mandatory

2. **Qualifying developments**: what kinds of developments are subject to IZ (e.g., buildings with more than 10 units)

3. **Set-aside rule**: how many or what percentage of units must be set aside as affordable units

4. **Incentives**: what kinds of benefits developers receive in exchange for participating

5. **Target Households**: which households (e.g. those earning 80% of Area Median Income) are eligible to buy/rent the affordable units, and how these households are found or recruited

6. **Tenure**: whether the affordable units are sold or rented

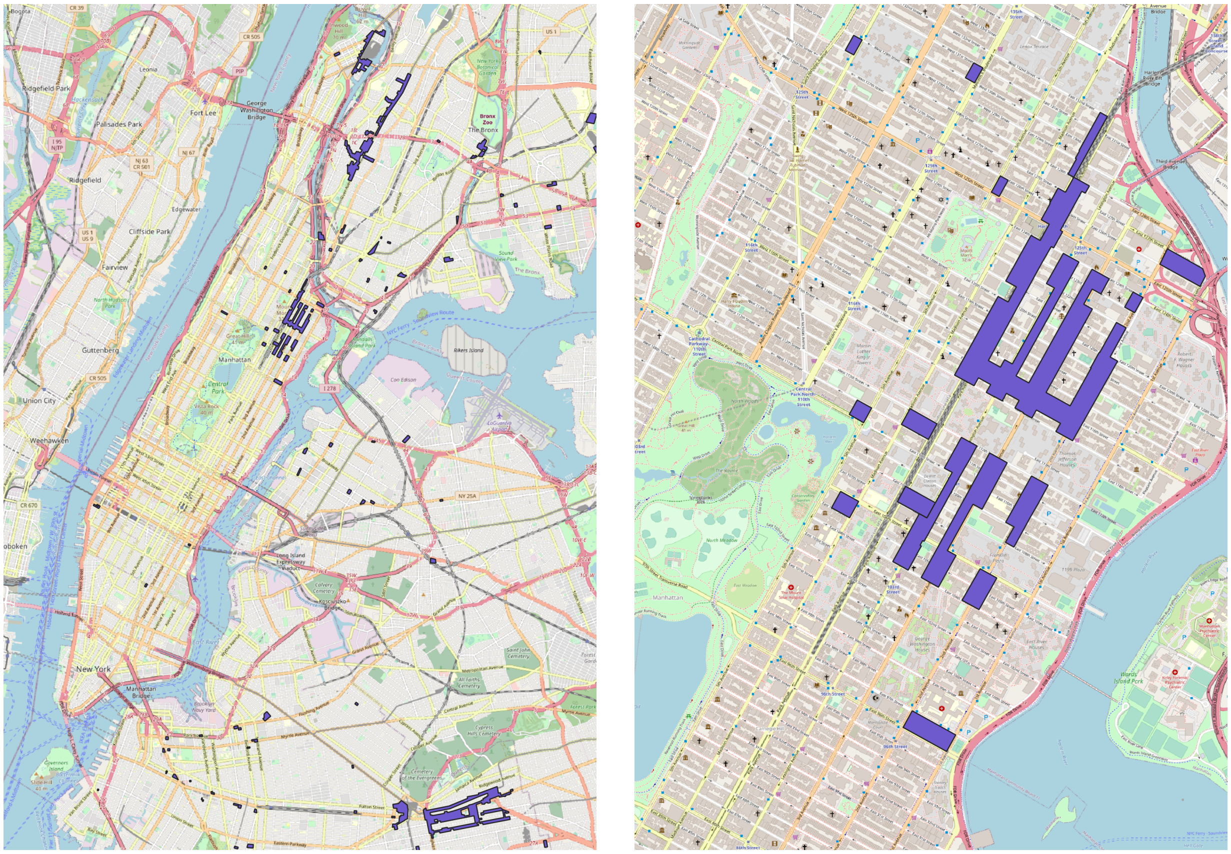
7. **Cost offsets**: whether developers can use in-lieu options to pay to opt out of providing affordable units

8. **Continued affordability**: whether the program contains long-term affordability guarantees or mechanisms

In this chapter, I focus on New York City's recently implemented Mandatory Inclusionary Zones (MIZ). In 1987, New York City adopted voluntary inclusionary zoning for R10 zones, the city's highest-density residential zones. The policy allowed developers up to 20 percent in extra floor area ratio (FAR) from 10 to 12 if they included affordable housing in their developments. In 2005, the city expanded their voluntary IZP to other "designated areas" with varying degree of bonuses available to developers. During 2005-2013, within these designated areas 19% of new units produced were affordable units, well-aligned with the average density bonus participating developers receive.

Owing to the success of voluntary IZP and facing a stronger need for affordable housing, in 2016, the city adopted a mandatory inclusionary zoning policy. The city designated several MIZs scattered throughout the city: Figure 3.1a shows an overview of MIZs in New York

Figure 3.1: New York City's Mandatory Inclusionary Zones



(a) New York City (b) East Harlem

Mandatory Inclusionary Zones (MIZ) are shown in purple at (a) larger view of the city and (b) zoomed into East Harlem in Manhattan. Although Staten Island is not in view, all five boroughs have MIZ areas. Map provided by OpenStreetMap. ©OpenStreetMap and contributors.

City, and Figure 3.1b details the MIZs in East Harlem, Manhattan. Any new development producing 10 or more units within MIZs is required to generate affordable units under one of three options:

- Option 1: 25% affordable units at an average of 60% AMI
- Option 2: 30% affordable units at an average of 80% AMI
- Deep Affordability Option: 20% affordable units at an average of 40% AMI

Moreover, small projects may contribute to an affordable housing fund or provide affordable housing off-site in lieu of providing the units within the development itself.

3.1.2 Literature Review

A simple theoretical framework for IZPs was laid down by an influential article by Ellickson (1981). Using basic economics, he argued that IZPs are indeed a tax on new development as it forces developers to sell some of their units at below the market price, and were thus unlikely to have net positive effects on housing affordability without additional government subsidies. Dietderich (1996) provides a more nuanced view of IZPs by arguing that housing markets without IZPs are not free markets, but are characterized by market failure so that a well-defined IZP can act as a corrective tax to push the market output to the socially optimal output. Rubin and Seneca (1991) analyze the density bonus mechanism of IZPs and show that the effectiveness of density bonuses as an incentive depends on local housing market conditions. In Section 3.2, I build on these papers and provide a framework that is consistent with these theoretical contributions. Overall, the simplistic model of Ellickson (1981) cannot capture the intricacies of IZPs that my model can.

Studies examining the effectiveness of IZPs on producing affordable units have documented a large variation across jurisdictions. This is expected as IZPs are not uniform and can take on many different forms, as explained above. Schuetz et al. (2009) find that in the San Francisco Bay and Washington D.C. areas, IZPs were more successful in producing IZ units. In suburban Boston, however, IZPs were less effective. Schwartz et al. (2012) examine 11 jurisdictions with data on IZ units that were produced under IZPs. They find that, in these jurisdictions, most IZ units are located in areas with relatively low concentration of poverty. Thus, they posit that IZPs are effective at generating affordable housing in areas traditionally cut off from the poor.

Few empirical studies have sought to assess the impact of IZPs on local housing markets, and all of them compare jurisdictions with or without IZPs in their analysis. Bento et al. (2009) analyze jurisdictions in California in a regression framework with city and year fixed effects. They find a modest increase in prices (2-3%) but no effect on housing starts. They also find that IZPs dampened the effect of new single-family housing starts. This is consistent with my findings that New York's MIZs had price but no supply effects, and also that the composition of new housing starts are skewed towards multifamily housing. However, their price analysis is limited to single-family homes. Means and Stringham (2012) also implement a fixed effects model for jurisdictions in California and find a 20% price effect and -6% supply effect. However, they only use decennial data between 1980-2000, which makes their analysis more limited.

Schuetz et al. (2011) uses a fixed effects strategy with time trends on a panel data of IZ units in the San Francisco Bay Area and suburban Boston. They find that years IZ has been in place interacted with an indicator for rising housing market status is negatively related to permit activity but positively related to prices in suburban Boston. However, the finding does hold in the San Francisco Bay Area. The effects overall are small and demonstrates considerable heterogeneity across housing markets.

Hollingshead (2015) uses a difference-in-differences framework, exploiting the timing of California's court decision in 2009 that weakened the enforceability of mandatory IZPs. Comparing jurisdictions that previously implemented mandatory IZPs with those that did not, she finds that following the court decision, there were stronger positive effect on prices for jurisdictions with IZPs compared to those without, which runs counter to Ellickson's (1981) claim that IZPs are a tax.

Overall, jurisdiction-level analysis in these studies are subject to severe endogeneity problems, as I will detail in Section 3.3. First, jurisdictions vary in many characteristics that may be related to their decision to adopt IZPs. Even well-understood confounding variables such as the price elasticity of demand for housing is incredibly difficult to observe and measure, making across-jurisdiction analyses subject to omitted variable bias. Second, the jurisdictions are comprised of many housing sub-markets and not internally homogeneous, both in IZP implementation and in housing market characteristics. An obvious example is New York's mandatory IZs only applying to specific blocks, but many other jurisdictions have variations in IZ policy variables across neighborhoods. I contribute to the literature by providing an analysis involving within-jurisdiction variation in IZP, thereby dealing with many endogeneity problems within the existing literature.

3.2 Theoretical Framework

I present a theoretical framework to analyze the developer's problem under exclusionary and inclusionary zoning. By comparing the profits under the two regimes, I show how IZP can affect housing production in a neighborhood. Suppose firms face a demand curve $P(q)$ and cost curve $C(q)$ over square footage of housing q. IZP requires a share $s \in (0, 1)$ of floor space be set aside to be sold at below the market price φP, where P is the market price and $\varphi \in (0, 1)$. Finally, suppose the current zoning laws set a limit to density of $\bar{q}$, but developers who set aside units receive density bonus, $\delta > 1$, which allows them to build up to $\delta\bar{q}$.

I make several assumptions in this set-up. First, floor space is not distinguished by its quality and so the cost and demand for housing does not depend on its type—whether affordable or market-rate. The identical cost assumption may be reasonable, as many developers find it difficult to lower development cost for specific units. However, the identical demand assumption is a strong one and assumes that 1) there exists, between affordable and market-rate units, an undifferentiated market for housing and affordable units are perfect substitutes for market-rate units, or 2) households targeted for market-rate housing and households targeted for affordable units have identical demand. Second, I assume that the incentives are given as a density bonus, which is the most common incentive for IZPs. This is not an innocuous assumption, as different incentives can distort developers' profit in significantly different ways.

Given the above notation and assumptions, profit under exclusionary zoning regime without density restrictions is simply its revenue minus cost, $\pi_E(q) = P(q)q - C(q)$. Profit under IZP with a set-aside rule $\{s, \varphi\}$ constitutes three parts: 1) revenue from selling $(1-s)$ units at market price P, 2) revenue from selling s units at below the market price at φP, and 3) cost of producing q. Thus, profit under IZP is, $\pi_I(q) = (1 - s(1-\varphi))P(q)q - C(q)$.

Comparing the two profit functions, we can see immediately that the set-aside rule acts as a tax on revenue, where the "tax rate" is given by $\tau = s(1-\varphi)$. This is the graphical analysis done by Ellickson (1981), who claimed that IZPs merely act as taxes on new development. Here, the tax rate is increasing in s, the share of units that need to be set-aside for affordability, and in $(1-\varphi)$, the rate of discount for households eligible for affordable units. For example, a set-aside rule with 20% of units sold at 80% of market rate would be equivalent to a $0.2 * (1 - 0.8) = 4\%$ tax rate. This is an intuitive result demonstrating developers will under-produce with IZPs.

However, density restrictions, which are almost always present under exclusionary zoning, is unaccounted for in this simple framework. Moreover, jurisdictions often understand the dampening effect of set-aside rules and provide incentives to counteract them, commonly used being density bonuses. Therefore, the choice set of developers is different with IZP and without IZP. We can define maximized profit under exclusionary zoning regime with density restrictions, π_E^*, and maximized profit under inclusionary zoning regime with density bonus, π_I^* as follows:

$$\pi_E^* = \max_{q \leq \bar{q}} P(q)q - C(q) \tag{3.1}$$

$$\pi_I^* = \max_{q \leq \delta\bar{q}} (1-\tau)P(q)q - C(q) \tag{3.2}$$

In this framework, $\bar{q}$ defines the density restriction, and $\delta > 1$ is the degree of the density bonus that allows developers to build above $\bar{q}$. The profit of the developer under exclusionary and inclusionary regimes ultimately depend on the optimal quantity q_E^* and q_I^* that solve (3.1) and (3.2), respectively, which depends on the restrictiveness of $\bar{q}$.

To see this, define q_E and q_I as the solution to (3.1) and (3.2) *without the density restrictions*. If $q_I, q_E \leq \bar{q}$ or $q_I < \bar{q} \leq q_E$, then $q_E^* = \bar{q}$, $q_I^* = q_I$, and we are back to the previous case where IZP merely acts as a tax. In words, if the density restriction is not very binding then the density bonus under IZP is useless to the developer. In essence, this is the argument made in *Palmer/Sixth Street Properties, L.P. v. City of Los Angeles*, where the developer contended that density bonus under L.A.'s mandatory IZP conferred no meaningful benefit and therefore IZP acted as local rent control.[3]

Suppose now that $\delta\bar{q} < q_I \leq q_E$. This is the case when the original density restriction is binding enough both with and without the tax from set-aside and affordability rules. Then, $p_E^* = \bar{q}, p_I^* = \delta\bar{q}$, and so $\pi_E^* = P(\bar{q})\bar{q} - C(\bar{q})$ and $\pi_I^* = (1-\tau)P(\delta\bar{q})\delta\bar{q} - C(\delta\bar{q})$.[4] The difference between profits is thus,

$$\begin{aligned}
\pi_I^* - \pi_E^* &= (1-\tau)P(\delta\bar{q})\delta\bar{q} - C(\delta\bar{q}) - [P(\bar{q})\bar{q} - C(\bar{q})] \qquad (3.3)\\
&= [P(\delta\bar{q}) - P(\bar{q})]\bar{q} + [P(\delta\bar{q})(\delta-1)\bar{q}] - [\tau P(\delta\bar{q})\delta\bar{q}] - [C(\delta\bar{q}) - C(\bar{q})]\\
&= \underbrace{\left[-\frac{1}{\varepsilon_\delta}\frac{P(\bar{q})}{(\delta-1)\bar{q}}\right]}_{\text{(a)}} + \underbrace{[(1-\tau)P(\delta\bar{q})(\delta-1)\bar{q}]}_{\text{(b)}} - \underbrace{[\tau P(\delta\bar{q})\bar{q}]}_{\text{(c)}} - \underbrace{[C(\delta\bar{q}) - C(\bar{q})]}_{\text{(d)}} \qquad (3.4)
\end{aligned}$$

where $\varepsilon_\delta = -\frac{\delta\bar{q}-\bar{q}}{\bar{q}}\frac{P(\bar{q})}{P(\delta\bar{q})-P(\bar{q})}$ is the price elasticity of demand for housing from $\bar{q}$ to $\delta\bar{q}$.

The expression in (3.4) has an intuitive interpretation with four parts in its sum: (a) loss from decreased price from increased supply, (b) gain from selling extra quantity $(\delta - 1)$ from the density bonus at the new net price $(1-\tau)P(\delta\bar{q})$, (c) loss from selling original units $\bar{q}$ at the decreased net price from the set-aside rule ("tax"), and (d) the loss from increased cost of production. The loss from (a) is larger with lower elasticity of demand because the developer would rather produce less and charge a higher price when facing a

[3] California's Second District Court of Appeal sided with the developer in the case. As a result, inclusionary zoning was unenforceable in California, until Costa-Hawkins was revised by the State legislature in 2017 (Chau Yager 2017, Phalon, Briseño, Bradish 2017).

[4] This holds even as long as $\bar{q} < q_I \leq q_E$, as we can redefine $\delta = q_I^*/\bar{q}$.

lower elasticity. On the other extreme, a developer facing a perfectly elastic demand ($\varepsilon = \infty$) could only increase his profit by producing more and therefore the density bonus becomes more attractive. This means that the likelihood of developers choosing an IZ project is lower when facing relatively inelastic demand, a situation that can occur, for instance, when a neighborhood has few substitutes in residents' choice set. The gain from (b) is larger with a higher density bonus, and the loss from (c) is larger with a higher tax rate. Therefore, with stronger set-aside rules and smaller density bonuses, developers will be less willing to pursue IZ projects. Finally, (d) depends on the cost structure of the developers. The higher the increase in costs of producing extra housing, the lower the likelihood of developers taking on IZ projects.

The theoretical framework in this section demonstrates that the empirical strategies of the existing literature is subject to omitted variables bias. As shown above, whether IZPs lower the supply of housing depends on i) the extent to which $\bar{q}$ is binding, ii) the elasticity of demand, iii) the cost structure, and iv) the policy variables. While (iv) has been well explored as a source of across-jurisdiction variation on IZP's effect on housing supply, (i)-(iii) has been less explored. For example, previous studies do not take into account the bindingness of original density policies without IZP. If a jurisdiction's decision to adopt IZP is related to its pre-existing density policy, a simple comparison of jurisdictions with and without IZP will be biased. Moreover, elasticity of housing demand and cost structure of developers are nearly impossible to measure, making analysis of IZPs across jurisdictions extremely difficult.

One important outcome that I do not explore here is the effect of IZPs on neighborhood characteristics. For example, does the production of affordable units actually lead to income-mixing? This is a crucial outcome of interest to policymakers and requires a principal-agent model of how developers choose to comply with city policy. Many jurisdictions specify the income level of households that are eligible for affordable units, but they do not specify how these households should be found. Discussions with housing advocates reveal that some developers take advantage of the incentives of IZP but purposely seek out tenants who, while technically fitting the income requirement (e.g., recent college graduates), are not the target demographics of IZPs.

3.3 Empirical Strategy

The central challenge of the IZP literature has been to identify the causal effect of IZPs on the housing market, both on prices and supply. The literature has relied on comparisons across jurisdictions with or without IZPs, or alternatively, across jurisdictions with varying implementations of IZPs. The main source of endogeneity of this approach is that jurisdictions often differ in characteristics which may be related to their subsequent policy choices. For example, cities with less developable land may choose to adopt IZP due to fast-rising housing costs. But even in the absence of IZP, those cities will face a shortage of development due to the lack of land. Merely comparing housing starts in cities with or without IZPs will mistakenly attribute the adoption of IZP as the source of low housing starts. In general, one cannot understand the effect of IZP without taking into consideration the existing local zoning and housing market characteristics as seen in Section 3.2.

Additionally, jurisdiction-level analysis is troublesome because jurisdictions are not internally homogeneous. For example, a residential zone in one neighborhood can have different FAR than a zone in another neighborhood. Neighborhoods within jurisdictions also vary in their housing market and demographic characteristics, making it difficult to "control for" them as I discuss in Section 3.2. Previous literature has focused on broad measures such as population density or housing prices for the whole jurisdiction which are inadequate controls.

To account for these shortcomings in the cross-jurisdiction analysis, I use an expanded version of the border-discontinuity by Turner et al. (2014), who use a regression discontinuity design at the border of municipalities to examine the effect of land use regulation on the value of land. The idea is that very close to the border of municipalities, everything other than the land regulation remains the same as residents can commute for services or amenities very easily.[5] This amounts to constructing an environment where the "but-for" test can be held: everything but-for land regulation is the same at or near the border, therefore any differences in housing neighborhood characteristics must arise due to the differences in regulation. Their innovation is recognizing that the regulation can have both own-lot and external (spillover) effects, since a stringent regulation on one side of the border can affect the other side as well. They construct a framework where they can analyze the two effects.

Consider, for example, the effect of land regulation on land prices. Very close to the border, only land regulation distinguishes one and another side of the border. As we move away from the border deeper into one municipality's territory, the land regulation remains the same but the spillover effects from another municipality's land regulation declines. Measuring the difference in prices between the two municipalities at the border measures both the own-lot effect of the regulation on prices and the spillover effect from the other municipality's land regulation. Meanwhile, measuring the price differences within one municipality but between areas close and far from the border measures the effect of spillovers, assuming that spillovers fade away by a certain distance. Thus, we can measure the pure effect of regulation on prices by taking into account both measures.

I adopt this empirical strategy to examine the effect of IZP. In the context of New York City's Mandatory IZ policy, own-lot measures can be constructed by comparing the prices of properties inside MIZ to those that are near MIZ. External measures can be constructed by comparing the prices of properties near but not inside MIZ to properties that are further away from MIZ. Taking account of both effects can therefore give us the pure effect of IZP on housing prices.

While Turner et al. (2014) only used a cross-sectional variation in land regulation, I expand the strategy by also taking advantage of the time variation on IZP enactment. In

[5] This may not hold for certain amenities like school districts, however.

New York, the MIZ areas were implemented in 2016. In Chicago, the 2015 revision of its IZP created three zones that have differing in-lieu fees. Therefore, instead of comparing prices in the cross-section, I apply a difference-in-differences regression for each measure.

The regression equation is,

$$y_{izt} = \alpha + \gamma_t + \gamma_z + \beta_t * D_{iz} + \Gamma_H H_{iz} + \Gamma_X X_{zt} + \varepsilon_{izt} \tag{3.5}$$

where i is property, z is location, t is time. γ_t is time fixed effects, γ_z is zone fixed effects, H_{iz} is a vector of housing characteristics, X_{zt} is a vector of neighborhood characteristics. For the own-lot effect regression, D_{iz} is an indicator for properties belonging to MIZs. For the external-effects regression, D_{iz} is an indicator for locations near but not in MIZs. The coefficients of interest are β_t, the time-varying difference-in-differences coefficient. I restrict the sample to be properties within x_o meters of MIZ for the own-lot effect regression and restrict the sample to be properties within x_e meters but not in MIZ for the external effect, where x_o, x_e can vary.

Identification fails when there are time-varying differences in characteristics between IZP and non-IZP areas that violate the parallel trends assumption. Given the discontinuity design, these time-varying characteristics must be geographically specific between MIZ and non-MIZ zones at the border. This is a much more stringent condition than the original, cross-sectional strategy, where any fixed differences in characteristics between IZP areas can lead to an identification failure. It is also much more stringent than previous studies' strategies of comparing jurisdictions with or without IZPs, where any differences in housing market characteristics across jurisdictions can be confounding factors.

3.4 Data

In order to implement the empirical strategy in Section 3.3 to examine the effect of IZP on housing prices and supply, I need two types of data. First, I need zoning data that describes not only the IZP and where it operates, but also the zoning policy of nearby areas. This data needs to detail within-jurisdiction variation of IZP. Second, I need fine-geography data on housing prices and development to measure price and development activity in and near IZ areas with enough precision to distinguish properties within x_o, x_e meters of IZ zones.

For this pilot study, I draw from three principal data sources. First, I obtain zoning map of New York City that has information on the MIZs within the city. I use the zoning map to construct an indicator variable of whether observations from other data are located within MIZ, and subsequently within x meters of MIZ, with varying levels of x. I do this by creating a x-meter buffer around MIZ zones and calculating whether the observations belong in the buffered zones.

Second, I use Zillow ZTRAX data that has information on property transactions and assessment histories, as well as detailed geographical information. The data is built from publicly-available county assessment and deed transfers data, combined with mortgage, foreclosure, auction, property tax delinquency data for about 2,700 counties in the U.S. Information on the property's exact address allows me to geocode the properties. One major shortcoming of the ZTRAX data is that it does not include rent data.

I obtain ZTRAX data for five counties that form New York City: Bronx, Kings, New York, Queens, and Richmond Counties. I exclude deed filings that do not have any sales price information, which excludes foreclosure and mortgage deeds, as well as special deeds, such as intra-family transfers. I only include transactions involving residential buildings and lot size below 10,000 square feet. As some transactions involve multiple buildings, I restrict my sample to transactions involving only one building. Finally, I exclude transactions below the 5th and above the 95th percentile in sales price.

Table 3.1: ZTRAX Summary Statistics on Selected Sample

	N	Mean	Std Dev	Median
Transaction Information				
Document Date Year	684,681	2006.01	7.51	2005
Sales Price	685,483	423,978	304,018.82	360,000
Loan Amount	356,847	171,902	519,867	158,650
Property Characteristics				
Year Built	681,869	1,945.03	32.99	1,935
Number of Units	685,483	1.75	0.94	2
Number of Stories	685,164	2.74	3.83	2
Lot Size Sqft	685,483	2,999	1,541.84	2,500
Building Area Sqft	685,471	3,626	13,444.37	1,800
Smaller Family (4 Units or Less)	685,483	0.90	0	1
Larger Family (5+)	685,483	0.06	0	0
Values From Assessment				
Land Assessed Value	685,477	76,599	543,385	8,515
Improvement Assessed Value	685,472	179,980	1,272,881	18,753
Total Assessed Value	685,477	256,578	1,816,088	27,729
Land Market Value	685,474	365,682	1,195,306	200,000
Improvement Market Value	685,198	862,580	2,811,271	407,000
Total Market Value	685,474	1,227,914	3,997,690	612,000
Location Relative to MIZ				
Within MIZ	685,483	0.00	0.05	0.00
Within 250m of MIZ	685,483	0.05	0.21	0.00
Within 500m of MIZ	685,483	0.11	0.32	0.00
Within 750m of MIZ	685,483	0.19	0.39	0.00
Within 1000m of MIZ	685,483	0.26	0.44	0.00
Within 1500m of MIZ	685,483	0.39	0.49	0.00
Within 2000m of MIZ	685,483	0.51	0.50	1.00
Within 2500m of MIZ	685,483	0.62	0.48	1.00

Sample includes transactions of residential buildings with price information, lot size below 10,000 square feet, involving only one building, and between sales price 5th and 95th percentiles.

Table 3.1 shows summary statistics of key variables in the ZTRAX data for the selected sample, according to the criteria mentioned above. Some summary statistics are worth mentioning here. First, we see that there is a very large variation in sales price, where the mean is around \$423,000 and the standard deviation is around \$304,000. The same is true for loan amount and assessment valuations, although we can see that only about half of the property transaction have loans attached to them. The average year built of properties transacted is around 1945. This is not to say that the average building year is 1945 in New York, because some properties may be transacted more than once and some may never be transacted at all. As is suspected from the specificity of MIZ locations in New York, very few transactions are actually within MIZs. Specifically, 1,665 property transactions belong inside MIZs. However, as we enlarge the radius around MIZs, more transactions are observed. About 26% of the sample belongs within 1km of MIZs.

Second, I use city-level housing permit data that is publicly available through city open-data portals. Most importantly, I obtain two sources of data crucial for my analysis: zoning and building permits data. New York City has building permits data that go back to 1989, although coverage is limited in the earlier years. I only select building permits for new buildings and significant alterations that require use changes. The rest are permits for small changes. For example, electrical work constitutes a bulk of all permits applied for in New York. The sample selection results in around 212,088 permits, of which 210,960 have address and location information.

Table 3.2 shows summary statistics from the building permits data. We see that around 57% of the permits in the sample are for significant alterations and 43% are for new buildings. The permit data also distinguishes between 1-2-3 Family buildings and others, which include larger multifamily buildings. About 63% of the sample are 1-2-3 Family building types. Again, because of the specificity of the MIZ areas in New York, very few permits (1,321) are inside the MIZ. About 33% of building permits are within 1km of MIZs.

Table 3.2: Building Permits Summary Statistics

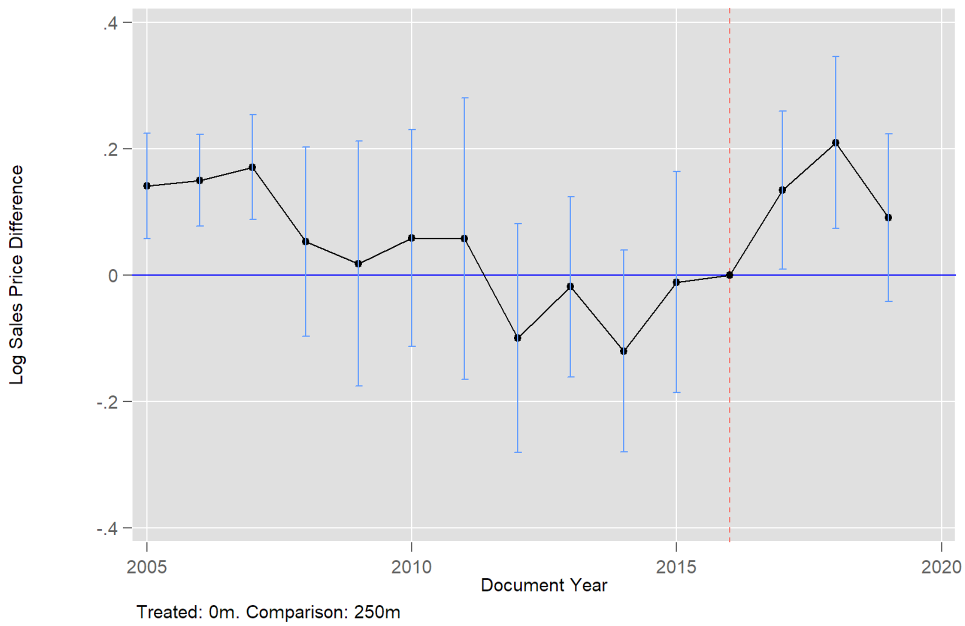
	N	Mean	Std Dev	Median
Permit Characteristics				
Job: Significant Alterations	212,088	0.57	0.50	1
Job: New Building	212,088	0.43	0.50	0
Building Type: 1-2-3 Family	212,084	0.63	0.48	1
Building Type: Other	212,084	0.37	0.48	0
Filing Year	212,088	2004.45	7.45	2004
Location Relative to MIZ				
Within MIZ	210,960	0.01	0.08	0
Within 250m of MIZ	210,960	0.07	0.26	0
Within 500m of MIZ	210,960	0.16	0.36	0
Within 750m of MIZ	210,960	0.25	0.43	0
Within 1000m of MIZ	210,960	0.33	0.47	0
Within 1500m of MIZ	210,960	0.47	0.50	0
Within 2000m of MIZ	210,960	0.59	0.49	1
Within 2500m of MIZ	210,960	0.68	0.47	1

3.5 Results

In this section, I present results from the specification I describe in Section 3.3. For permits, however, because the outcome of interest are in counts, a direct regression is difficult to implement. Therefore, I instead use tabulations of permit counts in each area and manually calculate difference-in-difference estimates of own-lot effect and external effect. I first present the results for housing transactions and then present results for permits.

Figure 3.2 shows the result from the own-lot regression on sales prices. The specification includes log square footage of lot size and year built as housing controls. Each point on the graph represents the relative difference of means between treated and control units from the base year (2016), which was the year MIZ was enacted; blue ranges are 95% confidence intervals. I constrain the sample to be properties that are within 250m of MIZ. Treated units are those that are in MIZs, while control (or comparison) units are those that are outside but within 250m of MIZs. Therefore, the data point 0.16 for year 2017 represents that the

Figure 3.2: Own-lot Effect of MIZ



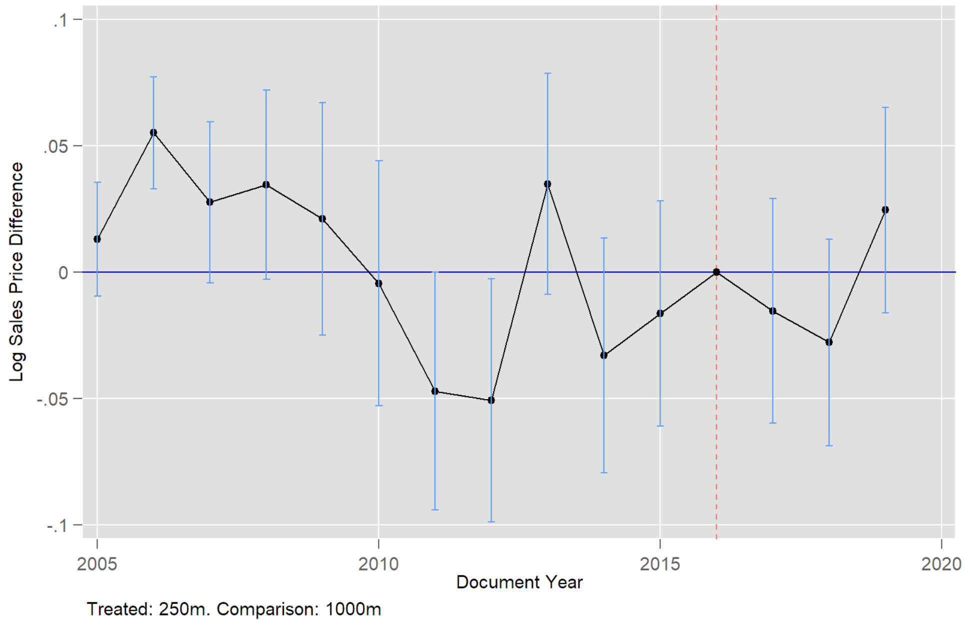
Difference-in-Difference regression coefficients on treated*year, with base year 2016. Treated units are those within MIZ and control units are those within 250m but not in MIZ. Blue ranges are the 95% confidence intervals.

difference in average prices between treated and control units in 2017 is around 16% higher than the difference in average prices between treated and control units in 2016. Similarly, Figure 3.3 shows the result from the external-effects regressions on sales prices. In this specification, treated units are properties within 250m but not in MIZs, while comparison units are properties within 1000m but not within 250m of MIZs.

These two figures combined tell us a picture of the policy's impact on transaction prices. First, Figure 3.2 demonstrates that there is a statistically significant relative increase in prices in MIZ locations compared to those very near, while the results in Figure 3.3 suggest that there were no notable spillover effects of policy onto nearby areas. This suggests that the policy has caused about a 18% increase in prices in MIZs. This could be consistent with the idea that MIZ is reducing the supply of housing in these locations.

However, looking at the results for permit activity, that does not seem to be the case.

Figure 3.3: External Effect of MIZ

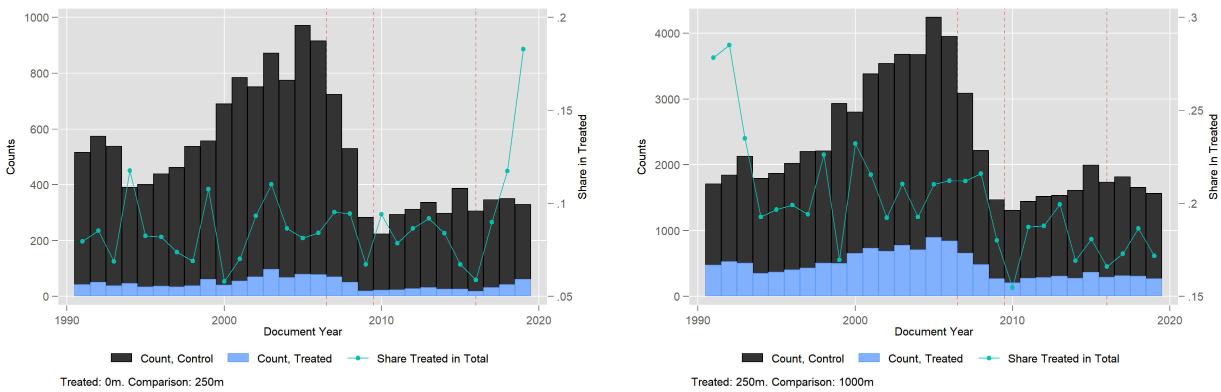


Difference-in-Difference regression coefficients on treated*year, with base year 2016. Treated units are those within 250m but not in MIZ and control units are those within 1000m but not within 250m of MIZ. Blue ranges are the 95% confidence intervals.

Figure 3.4 shows both the own-lot effect (Figure 3.4a) and external effect (Figure 3.4b) of MIZ on permit activity. By looking at differences between 2016 and later in permit activity within treatment area as a share of total permit activity within treatment and comparison areas, we can measure the relative difference in permit activity between MIZ and non-MIZ ares over time. We can see a fairly dramatic uptick, from 5% in 2016 to 17% in 2019, in the share of total development activity occuring in MIZs compared to near but not within MIZs. However, there is no apparent difference in the share of development activity comparing within-250m MIZ and within-1000m MIZ areas. This suggests a strong own-lot effect but a weak external effect of MIZ on development activity.

In Figure 3.5, I present the analysis done separately by permit types. Figures 3.5a and 3.5b show results for new building permits and Figures 3.5c and 3.5d show results for 1-2-3 Family types. An interesting finding is that permit activity results in Figure 3.4 is driven by

Figure 3.4: Own-lot and External Effects of Permit Activity



(a) Own-lot Effects (b) External Effects

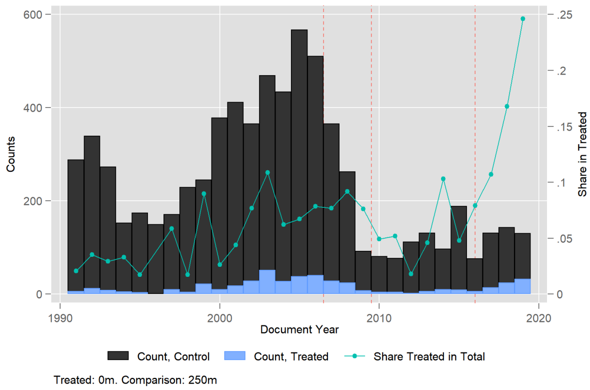
Black bars are total activity for treated and comparison areas, and blue bars are activity only for treated areas, thus representing share of total development activity. Green line is the share of development activity in treated areas as a fraction of activity in both treated and control areas. (a) restricts treated units to be in MIZs and control units to be within 250m but not in MIZs. (b) restricts treated units to be within 250m but not in MIZs and control to be within 1000m but not within 250m of MIZ.

new building permits but not by 1-2-3 Family permits, indicating that the results are driven by new construction and multifamily units, the kinds of developments targeted by IZPs.

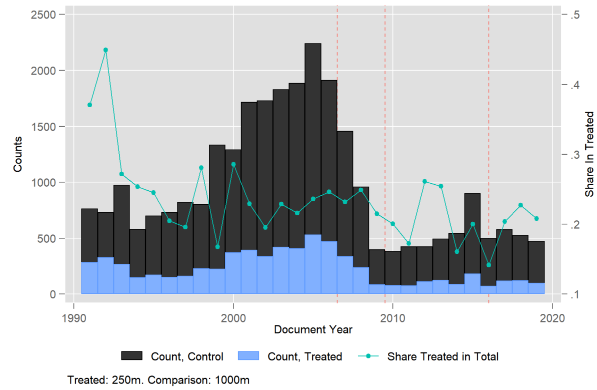
There are several caveats that must be mentioned. One potential problem is selection on trends. If the neighborhoods that were designated for MIZs were specifically targeted for reasons that are related to the evolution of prices over time, the parallel trends assumption does not hold. There is some evidence this may be true. Figure 3.6 plots the evolution of prices over time for properties within 250m and in MIZ and the rest of New York City. Properties within 250m and in MIZ start out with lower price, but catch up by mid-2000s, and finally overtake the rest of New York around 2015. This is suggestive of the fact that neighborhoods receiving MIZ may have been those that have experienced fast price growth relative to others, and policymakers might have targeted these neighborhoods to slow gentrification or housing unaffordability.

However, own-lot regressions should net out any neighborhood-level differences in trend growth, unless the MIZ boundaries are drawn specifically delineating the fast-growth areas

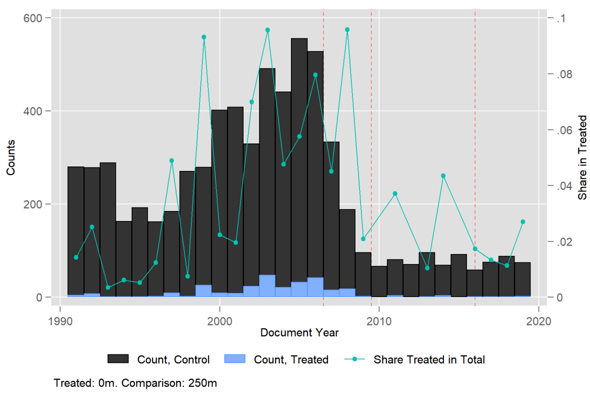
Figure 3.5: Own-lot and External Effects By Permit Type



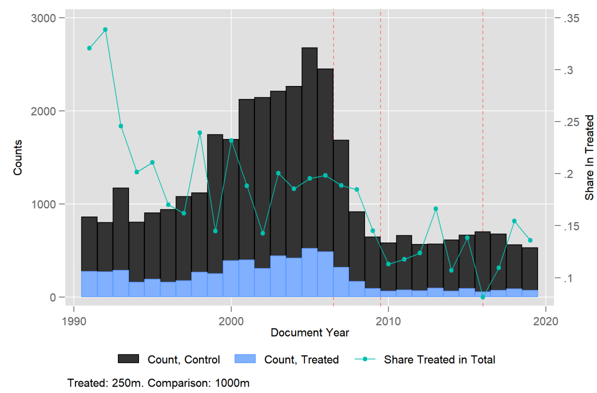
(a) New Buildings: Own-lot Effects



(b) New Buildings: External Effects



(c) 1-2-3 Family: Own-lot Effects



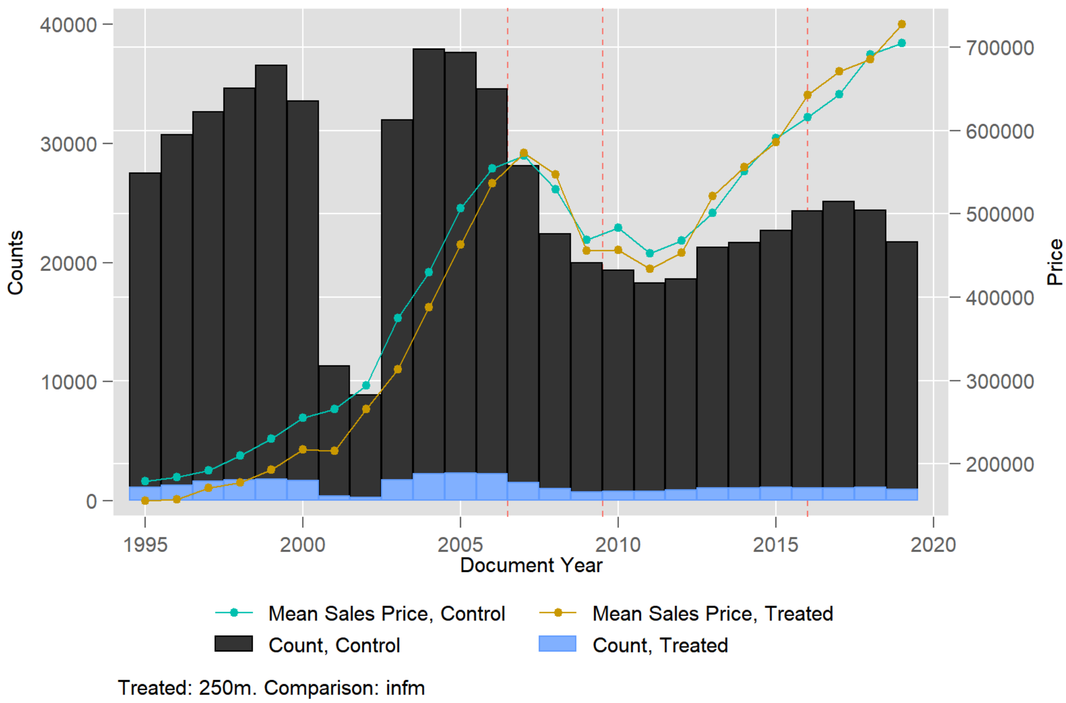
(d) 1-2-3 Family: External Effects

See footnotes for Figure 3.4 for description of the difference between own-lot and external effects figures. (a) and (b) restricts the sample to be permits for new buildings, and (c) and (d) restricts the sample to be permits for 1-2-3 Family type.

by the block from the rest of the neighborhood. Still, external-effect regressions, which take a larger area into consideration (between 250m and 1000m) may be subject to these biases from neighborhood targeting.

Another problem is the relatively low number of observations inside MIZs. This is reflected in the large standard errors and subsequently, the wide 95% confidence intervals. The large variance in pre-trend (for example in Figure 3.3), although not statistically significantly different from 2016 baseline, is worrisome as this might signify differences in trend-growth.

Figure 3.6: Average Sales Price Over Time



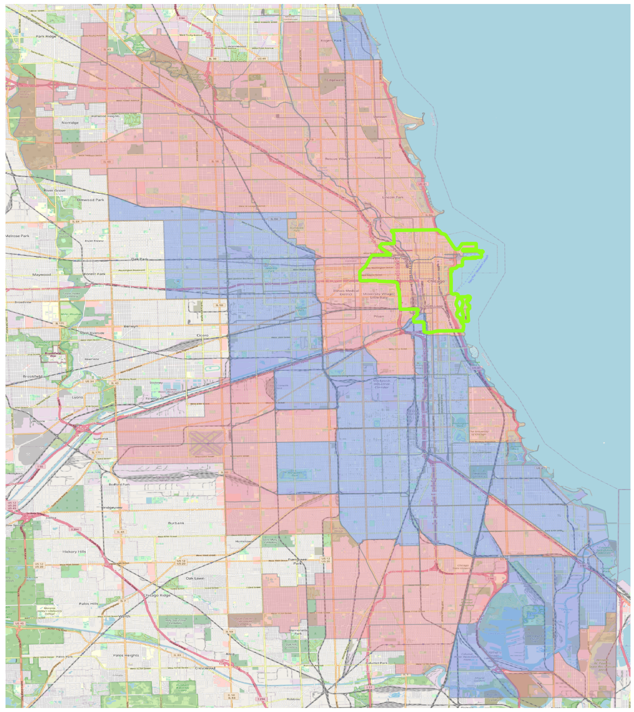
Average sales prices and counts of properties transacted for observations within 250m and in MIZ and the rest of New York City.

3.6 Conclusion

In this chapter, I explored the impact of inclusionary zoning policies (IZPs) by analyzing the effect of New York City's Mandatory IZ on housing supply and prices. I exploited the timing and geographic variation of MIZ in New York by using detailed-geography data on building permits and housing transactions. I showed that although prices rose following the policy, building activity also rose, mostly spurred by new residential and larger multifamily developments. These results suggest that although property transaction prices rose in these locations, this is not due to stifled development activity.

I end this chapter with some suggestions on how to expand the scope of this chapter. First, one extension would be to include more cities with within-jurisdiction variation of IZ.

Figure 3.7: MIZ Variation in In-Lieu Fee by Neighborhood in Chicago



Each color represents a different in-lieu fee amount for mandatory inclusionary zoning policy in Chicago. Red are high-income areas and require a higher in-lieu fee while blue are low-income areas and require a lower in-lieu fee. The green outline is the downtown-area which has a more stringent IZ policy. Map provided by OpenStreetMap. ©OpenStreetMap and contributors.

Chicago, Washington DC, and Los Angeles seem to be good candidates in this regard. For example, Figure 3.7 shows the variation in IZ policy within the city of Chicago. Chicago has a cost offset policy that allows developers to pay an in-lieu fee instead of including affordable units within the development. The in-lieu fee differs across neighborhoods, which provides a clean monetary variation of IZ policy within the city. My hope that the strategy described in this chapter can be used for properties that are close to the border of differing in-lieu fee zones within Chicago.

Second, using American Community Survey 5-year Summary Files (ACS5SF) to capture demographic and neighborhood characteristics might be a promising avenue for research. ACS5SF data are available from 2005-2009 to 2014-2018. One could obtain count of population in race, income bin, age, renter/owner status, employment status, occupation, and educational status categories for each census tract. The biggest challenge using this data is that tracts often do not line up with zoning districts, and they frequently cross zoning boundaries.

One major challenge is the availability and quality of data in different jurisdictions. For example, the quality and quantity of ZTRAX observations vary across counties due to record-keeping issues and the massive effort required to obtain and digitize the data. Building permits data, as well as data on affordable units produced, have different coverage and availability for different cities. For example, Chicago, Washington DC have building permits data available online, but Washington DC's permits data begin only in 2009, Los Angeles data starts in 2013, and Chicago data starts in 2006. The challenge will be to combine these varying data and zoning policies to conduct a coherent analysis.

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