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The Return to Big City Experience: Evidence from Refugees in Denmark*

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Abstract

We offer causal evidence of higher returns to experience in big cities. Exploiting a natural experiment that settled political refugees across labor markets in Denmark between 1986 and 1998, we find that while refugees initially earn similar wages across locations, those placed in Copenhagen exhibit 35% faster wage growth with each additional year of experience. This gap is driven primarily by differential sorting towards high-wage establishments, occupations, and industries. An estimated spatial model of earnings dynamics attributes an important role to unobserved worker ability: more able refugees transition to more productive establishments faster in Copenhagen than in other cities.

Keywords: Agglomeration Economies, Urban, Regional Labor Markets, Resettlement, Wage Differentials

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1. Introduction

A large number of papers across a wide range of countries find that wages are higher on average in big cities than more rural areas, even after controlling for worker level observables. A central question is whether bigger cities make workers more productive, or whether more productive workers choose to live in bigger cities.[1]

Leading empirical research attempting to answer this question uses worker fixed effects to control for selection on unobserved ability across locations (Glaeser and Mare (2001); D'Costa and Overman (2014); De La Roca and Puga (2017); Combes, Duranton, and Gobillon (2008)). An important limitation of this approach is that identification relies on individuals who migrate across locations. Selection into moving hence induces a potential source of bias in the resulting estimates, which may then also confound analysis of the determinants of the city wage premium.

In contrast, this paper uses a natural experiment in Denmark to isolate the causal effect of placement into a big city on workers' lifetime wages and earnings for a particular population, and investigates the underlying mechanisms. Between 1986 and 1998, the Danish government assigned 80,000 newly arriving refugees to municipalities across Denmark, without regard to a refugee's labor market relevant characteristics.[2] Since refugees did not choose their location, this natural experiment provides a unique setting to understand the effect of working in a big city on labor market outcomes. For our baseline analysis, we split the spatial economy of Denmark into two parts: Copenhagen, the country's capital and only big city, and the union of all remaining small cities and towns. We exploit the quasi-random assignment of refugees across these two zones and examine how labor market outcomes differed between the two groups, both initially and over time.

We first document the treatment effects of *assignment* to Copenhagen on lifetime wage and earnings paths. In contrast with previous studies, we find that refugees initially earn similar hourly wages across areas. However, individuals settled in Copenhagen see their wages and earnings grow around 35% faster with every additional year of experience relative to their counterparts settled outside of Copenhagen. This treatment effect is substantial and comparable in magnitude to the return to an additional two years of education over a working life. We then discuss the extent to which this treatment effect reflects the true differences in the return to experience between Copenhagen and elsewhere, given that some refugees migrate in the years after assignment. We derive tight bounds on the *return to big city*

[1] While spatial equilibrium requires higher wages in big cities to compensate for higher local prices (c.f. Rosen (1979); Roback (1982); Allen and Arkolakis (2014); Redding (2016)), there must be productive advantages in big cities that allow local firms to pay higher wages in the first place (see Moretti (2011)). If not, firms producing tradable goods or services would relocate to save on wages. These advantages can stem either from a more talented pool of labor or features of big cities beyond worker types. See Ahlfeldt and Pietrostefani (2019) for a recent survey of the empirical literature on the economics of density, and Rosenthal and Strange (2004) and Duranton and Puga (2004) for earlier reviews.

[2] Other papers in economics have used the exogenous variation associated with the Nordic refugee dispersal policies. Edin et al. (2003) exploits a similar policy in Sweden, while Damm and Dustmann (2014) and Dustmann et al. (2019) use the same natural experiment as we do; they all investigate research questions unrelated to ours.

experience by comparing refugees who never move to those who migrate.

Next, we investigate the mechanisms underlying the city wage growth premium. To do so, we draw on an administrative matched employer-employee data set with detailed information on the universe of workers and establishments in Denmark. We use this information to decompose the big city wage growth premium into contributions from firm and job characteristics. We find that differential sorting towards high-wage establishments, occupations, and industries explains the majority of the big city effect on wage and earnings growth. Over time, a refugee assigned to Copenhagen is increasingly likely to work at more productive firms and in skill-intensive jobs than one settled outside Copenhagen. Previous literature has, for the most part, not taken a stand on the contribution of firm characteristics to the urban wage premium. [3] Moreover, in explaining our findings, we can rule out several other mechanisms, including separate wage trends between locations, the effects of ethnic enclaves, and variation in educational take-up across space. Our results also suggest that spatial differences in assimilation have no bearing on the differences in returns to experience we identify.[4]

While the natural experiment controls for initial differences in latent worker characteristics across space, a remaining question is whether the city differentially affects workers based on these characteristics. For example, high latent ability workers may be more able to take advantage of the greater presence of high-wage firms in a big city. We explore this possibility in a spatial model of earnings dynamics that builds on the setup in Baum-Snow and Pavan (2012). We exploit the quasi-random placement to help identify the distribution of latent ability among refugees. We find that higher ability workers move to more productive firms faster in the city, accounting for more than 50% of the dynamic experience premium. Through the lens of the model, our results on the importance of sorting towards high-wage establishments also reflect the success of high latent ability workers in the city.

Recent calls for more causal inference in urban economics (Baum-Snow and Ferreira, 2015) have highlighted the need for well-identified estimates of the impact of cities on workers. Such inference often requires tradeoffs between the ability to recover parameters of interest, and the generality of the setting. Clearly, refugees are a particular population. However, the mechanisms behind the differential return to experience across cities we identify appear to be general. In particular, the greater presence of highly productive establishments, high-skill occupations, and service industries is likely to steepen the earnings paths of natives in Copenhagen in like ways. While we can say less about their quantitative importance for natives, our results suggest that future studies of the urban wage premium should focus on firm location decisions as fundamental sources of the dynamic wage benefits cities bestow.

[3] See, for example, Wheeler (2006), Gould (2007), Baum-Snow and Pavan (2012), Papageorgiou (2017), De la Roca et al. (2014), and Grujovic (2018). Notable exceptions are Combes et al. (2012), Mion and Naticchioni (2009), and Dauth et al. (2018). For papers that highlight the importance of city industry structure in explaining the urban wage premium see Davis and Dingel (2020), Baum-Snow and Pavan (2013), and Kok (2014).

[4] In fact, leading scholars on refugee dispersal programs argue that assimilation is enhanced by placement *outside* major cities (see, e.g., Damm and Rosholm (2010)), whereas we find that placement in a big city causes faster wage gains with experience.

2. Background, Data, and Sample Selection

In this section, we describe the Danish refugee dispersal policy. We also discuss the sample construction, introduce the map of Danish local labor markets, and show that refugees do not move much after assignment to a location. Our description here follows Damm and Dustmann (2014), who provide substantially more detail. We also follow their sample construction exactly as it appears in their published work.

2.1 The Danish Refugee Dispersal Policy

Before 1986, refugees arriving in Denmark chose their municipalities of residence freely. As a result, a few municipalities played host to a majority of refugees. To ensure all localities shared in the work of integrating refugees, the Danish government, through the Danish Refugee Council, initiated a *refugee dispersal policy* in 1986. The program's explicit goal was to distribute refugees across municipalities in proportion to local population.[5] Between 1986 and 1998, 76,673 immigrants were granted refugee status (Statistics Denmark 1992, 1997, 2000) and assigned to municipalities.[6]

Under the policy, arriving refugees lived in Red Cross reception centers located across Denmark until receiving asylum. With asylum granted, refugees faced no further legal impediments to labor market participation. Within 10 days of the asylum decision, refugees were assigned temporary housing in one of 15 counties in Denmark.[7] Each county assigned the refugees to a municipality within the county and helped them find permanent housing. When assigning refugees to a municipality, the council's local office had access only to the birth date, marital status, number of children, and nationality of the refugee.[8] Conditional on this information, assignment was random. Importantly, the council did not have information on years of schooling or family income, and did not meet refugees in person.

Reassignment requests were allowed only after refugees had first moved to the originally assigned municipality. This is important for our design which relies only on the randomness of the first assignment municipality. Once settled in the assigned municipality, refugees received social assistance and Danish language courses for the first 18 months in the assigned municipality. Nevertheless, although the refugees were urged to stay in the assigned municipality during the entire introductory period, there were no relocation restrictions.

Our empirical strategy exploits the initial quasi-random assignment in three steps. First, we study the

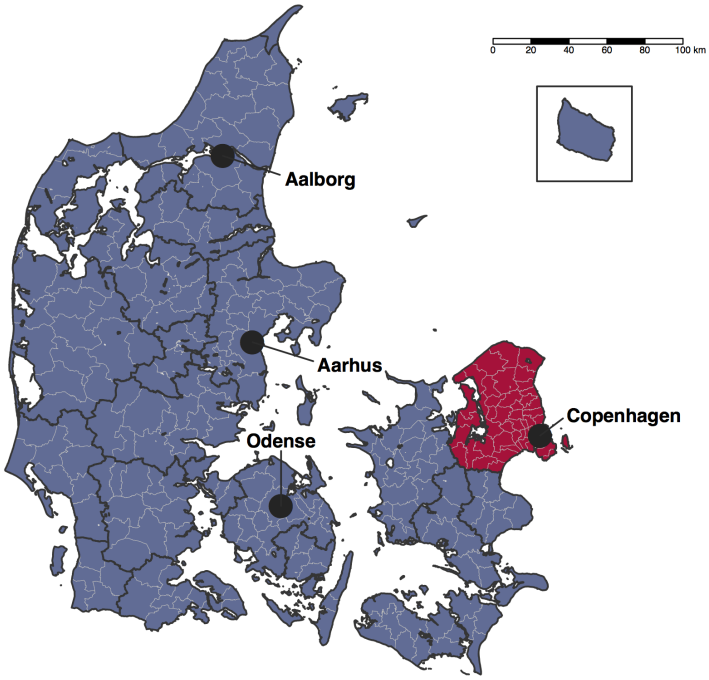
[5] We also thank Bente Bondebjerg from Dansk Flygtningehjælp, who helped run these programs and provided further details in personal communication.

[6] Other notable papers in the exogenous placement literature are Aslund et al. (2009), Shoag and Carollo (2020), Peters (2019), Chetty et al. (2016), Beaman (2011), Edin et al. (2003), Gould et al. (2004), and Imberman et al. (2012).

[7] Counties are sub-units of the geographic units (commuting zones) we consider below. Random assignment across counties hence implies random assignment across our units of analysis.

[8] The council tended to assign families with a large number of children to less populated municipalities with higher availability of family homes.

FIGURE 1: COMMUTING ZONES IN DENMARK



Notes: This figure shows 23 commuting zones (black lines) constructed by the authors using commuting flows of the entire Danish population between the 271 municipalities in Denmark in 1986 (light grey lines) following the methodology outlined in Tolbert and Sizer (1996). The Copenhagen commuting zone is highlighted in red; the non-Copenhagen assignment area is in blue. Aalborg, Aarhus, and Odense are the three largest cities in Denmark besides Copenhagen. The box in the top-right corner shows the Bornholm commuting zone, an island off the eastern shore of Denmark.

effect of initial placement in a big city on lifetime wage growth. In a second step, we use the persistence of the initial assignment to explore to what extent we can interpret our results as the return to big city experience. Lastly, we use the exogenous assignment to identify the parameters of a model of earnings dynamics to understand the source of the big city wage growth premium.

2.2 Data Description and Sample Selection

Our analysis uses administrative data provided by Statistics Denmark. Our core data set is a matched employer-employee panel covering the entire Danish population from 1986 to 2012, including all refugees from the time of being granted asylum. The data set includes detailed labor market information for each individual and year: average hourly wages, total labor earnings, 4-digit occupation codes, and current municipality of residence and work.[9] For the employing establishments, it contains industry identifiers,

[9] Annual hours worked are estimated using information on mandatory pension contributions. This is then used to construct a measure of hourly wages, potentially introducing a source of measurement error. Lund and Vejlin (2015) improve upon

employment counts, and total payroll. We enrich the matched data set with other administrative data records on gender, age, years of education, as well as family information on spouses, and the number and age of children in a household. For refugees, the data additionally contain country of origin and the year of arrival. The Online Appendix provides more detail on the data and variables used.

Following previous papers exploiting the same natural experiment (Damm and Dustmann, 2014), we restrict our sample to men between the ages of 19 and 55 arriving from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. Since family reunified immigrants were not subject to the dispersal policy unless they immigrated shortly after their spouse, we exclude spouses of refugees who immigrated to Denmark more than 12 months later.

Table 1 shows characteristics for both refugees and natives. On average, refugees are younger than Danes, have fewer children, and are less likely to be married. For refugees, the source of education information is a survey conducted upon arrival; for 19% of refugees in our sample education information is missing. In the Appendix, we confirm that years of schooling do not differ in an economically meaningful way across initial assignment regions after controlling for the information available to the council. We also compare other outcomes across assignment regions, e.g., employment rates by years in Denmark, of which we offer further discussion below.

Our final sample of refugees includes 20,493 male refugees assigned to either Copenhagen or Non-Copenhagen.

2.3 Defining Local Labor Markets in Denmark

At the end of our sample period, in 2012, Denmark had a total of 5.6 million inhabitants and Copenhagen, its capital city, housed 2 million of them in its broader metropolitan area. The remaining 3.6 million people live in three second-tier cities (Aarhus, Aalborg, and Odense), smaller towns, and rural areas. The second-tier cities are an order of magnitude smaller than Copenhagen; Aarhus, the largest, contains around 250,000 people.

We use commuting flows of all Danish workers between Denmark's 271 municipalities in 1986, together with a hierarchical clustering algorithm to construct 23 Danish commuting zones.[10] These local labor markets are shown in Figure 1.

In our analysis, we divide the economy into two locations: the Copenhagen commuting zone and everywhere else. We refer to the Copenhagen commuting zone as Copenhagen or *big city*, and all remaining commuting zones as non-Copenhagen or *elsewhere*.[11]

Statistics Denmark's estimated annual hours measure for the years 1980-2007, primarily by using additional information on time spent in sickness and leave. All results in this paper are robust to using this improved hourly wage measure.

[10] Tolbert and Sizer (1996) construct commuting zones for the U.S. labor market that are widely used in the economics literature (e.g., Autor and Dorn (2013)). We use the Tolbert and Sizer (1996) method to construct commuting zones for Denmark.

[11] In the Online Appendix, we show the commuting zones that result from using commuting flows in 1980 and 2000. All

TABLE 1: DESCRIPTIVE STATISTICS - NATIVES AND REFUGEES

	Natives	Refugees		
	1990 Cross-section	All	Copenhagen	Non-Copenhagen
Mean				
Age	36.72	28.24	28.67	28.08
Number of children	0.68	0.54	0.47	0.57
Age of youngest child	7.43	3.46	3.63	3.40
Age of oldest child	10.01	7.27	7.20	7.30
Fraction				
Married	47%	28%	28%	28%
Missing education	0%	19%	19%	19%
10 years of education	31%	27%	23%	28%
12 years of education	50%	34%	34%	34%
More than 12 years of education	17%	20%	23%	19%
Observations	1,335,545	20,493	5,530	14,963

Notes: The sample underlying the descriptive statistics of natives is a cross-section of all Danish men aged between 19-55 who were either employed or unemployed in 1990. The sample underlying the descriptive statistics of refugees includes all refugees from the full sample whose construction is outlined in Section 2.2 of the main text. The geographic delineations of the two assignment regions, Copenhagen and Non-Copenhagen, correspond to the those constructed in Section 2.3 in the main body of the paper.

As described above, assignment of refugees was done at the county and municipality level. Since our spatial units are aggregations of these assignment regions, if assignment can be considered quasi-random across these smaller units (as in Damm and Dustmann (2014)), it is also quasi-random at our higher level of aggregation. Nevertheless, we confirm that education is balanced across our two regions in the Appendix.

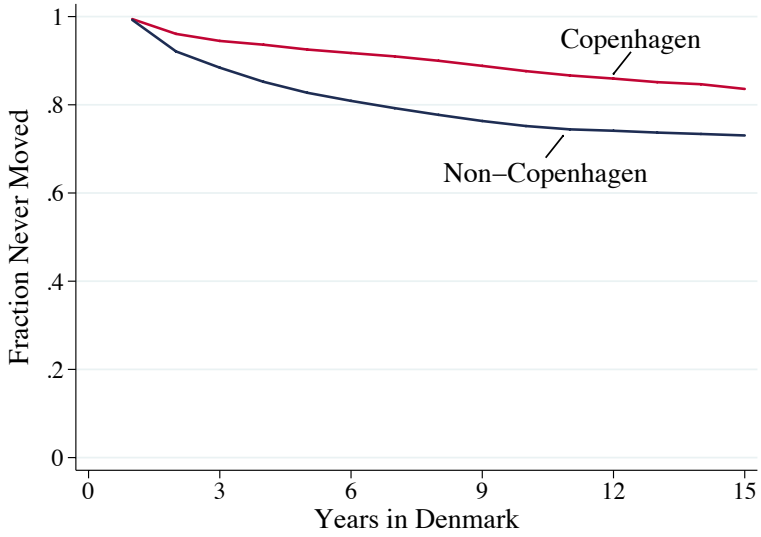
2.4 Persistence of Initial Assignment

Refugees are free to move after the initial assignment to a region. However, to understand the causal effect of working in Copenhagen on refugees' wages, persistence in the initial assignment is essential. In Figure 2, we show the fraction of refugees who have never moved from their assigned commuting zone for various years since arrival in Denmark. Even 15 years after assignment, 78% have never left their assigned regions. We discuss the implications of this persistence for the interpretation of the treatment effects in Section 3.2.[12]

results in the paper are robust to the choice of commuting zone delineation.

[12] In the Online Appendix, we show the fraction of stayers among refugees of different education levels. Refugees are less likely to *leave* Copenhagen, regardless of education level. However, refugees with at least a high school diploma are more likely to move to Copenhagen than those with less education.

FIGURE 2: PERSISTENCE OF THE INITIAL ASSIGNMENT



Notes: The sample underlying this figure includes men between the ages of 19 and 55 who arrived from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The figure shows the fraction of refugees who have never changed assignment region (Copenhagen and non-Copenhagen) out of all refugees assigned to a given region, for various years since arrival in Denmark. Years in Denmark are computed as the number of years since being granted asylum. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3.

3. THE RETURN TO BIG CITY EXPERIENCE

What is the differential effect of assignment to Copenhagen on lifetime hourly wage and earnings paths? To answer this question, we segment our sample by initial allocation, and compare the two groups: first their respective wage-experience and earnings-experience profiles, then their labor supply responses. Section 3.2 discusses the interpretation of the "Copenhagen treatment effect" on wages as *return to big city experience*.

3.1 The Effects of Initial Assignment to the Big City

In a simple log-linear setting, wage-experience profiles can differ in two ways as a function of initial assignment: intercept and slope. Refugees in one area could earn uniformly higher wages, or see their wages grow faster with experience. We refer to differences in intercept as the static treatment effect and differences in slopes as the dynamic treatment effect. These effects condition only on initial assignment, not on current location. As a result, they are specific to our context and conceptually distinct from the static and dynamic effects of cities in the literature (e.g., Glaeser and Mare (2001)). We explore their interpretation as the *return to big city experience* in the next subsection.

The only assumption needed to recover the treatment effect of assignment on wages and earnings is

random initial assignment conditional on assignment controls. Treatment effects could reflect multiple channels, including effects on the extensive margin of labor supply. We explore the channels underlying the treatment effects in a second step, below.

To document the treatment effects, we estimate the following equation:

$$y_{it} = \beta_1 Exp_{it} + \beta_2 InitCph_i + \beta_3 \left(InitCph_i \times Exp_{it}\right) + \phi' \boldsymbol{X}_{it} + \epsilon_{it}. \tag{1}$$

y_{it} is either the log hourly wage or log yearly earnings in Danish Kroner, deflated by an index of Danish nominal wage growth.[13] Exp_{it} is the number of years in which worker i has undertaken paid employment in Denmark prior to year t. $InitCph_i$ is an indicator variable that takes a value of 1 if the refugee is initially assigned to Copenhagen and 0 otherwise. $\mathbf{X}_{it}$ is a vector of controls that include cohort fixed effects, nationality fixed effects, and the variables relevant to the assignment of refugees (henceforth "assignment controls").

We report the results for hourly wages in column I.A. of Table 2. There is no significant difference in initial wages across assignment regions. However, each additional year of experience earns refugees assigned to Copenhagen an additional 0.81 percentage point wage increase over and above the return to experience of refugees assigned elsewhere.

In column I.B., we report the results for refugees who had at least a high school diploma upon arrival in Denmark. In column I.C., we do the same for refugees with either less than a high school diploma, or missing education information. The differential slope for wages is very similar across subgroups, and there are no significant differences in initial wages across assignment regions for any of them.

In the right-hand panel of Table 2, we repeat these regressions with log earnings as the dependent variable. Earnings are initially lower in the city for both of our sub-populations, but the estimated coefficient on experience implies that big city earnings catch up after about 3.5 years, and then overtake. Appendix 6 helps reconcile our findings on hourly wages and earnings. There we show that yearly hours worked are initially lower in Copenhagen, but increase faster with experience.[14]

We first rule out that differential aggregate wage trends across the two regions are driving our results. If average wages in Copenhagen grew faster than in other regions, this would affect β_3 in equation (1) above. In Figure A.1, we show average hourly wages earned by all working Danes, from 1986 to 2010, in Copenhagen and elsewhere. Wage levels in Copenhagen and Non-Copenhagen do not diverge sys-

[13] To construct an index of nominal wage growth, we use the entire population of native workers and apply our sample selection criteria from Section 2.2. We compute average hourly wages in each year relative to 1986. We use this index to deflate mean hourly wages and earnings for refugees. The results are robust to controlling for aggregate trends using year fixed effects.

[14] In the abstract and the introduction, we state that wages of refugees placed in Copenhagen grew about 35% faster with each year of experience relative to their peers assigned elsewhere. To obtain this number, we divide the coefficient on the interaction of experience and initial assignment to Copenhagen by the coefficient on years of experience in Table 2.

TABLE 2: THE RETURN TO BIG CITY EXPERIENCE

	Log Hourly Wage × 100			Log Earnings ×100		
	I.A	I.B	I.C	II.A	II.B	II.C
Years of Experience	2.177***	2.314***	1.862***	7.659***	7.754***	7.314***
	(0.144)	(0.164)	(0.132)	(0.301)	(0.339)	(0.313)
Initial Assignment to Copenhagen	0.0338	0.789	-0.837	-7.234***	-5.519**	-10.26***
	(0.996)	(0.894)	(1.338)	(1.802)	(1.889)	(2.349)
Years of Experience × Initial Assignment to Copenhagen	0.808***	0.735***	0.814***	2.116***	1.842***	2.604***
	(0.149)	(0.165)	(0.135)	(0.304)	(0.332)	(0.311)
Assignment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Nationality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.056	0.061	0.053	0.157	0.159	0.157
Observations	97,402	57,994	39,408	107,297	63,870	43,427

Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. The table shows the estimated parameters of equation 1 in the text. The dependent variables in all columns are scaled by a factor of 100 for presentational purposes. Columns I.A and II.A use the full sample. The estimates in columns I.B and II.B draw on the subset of refugees with at least a high school diploma. Columns II.C and II.C use refugees with less than a high school diploma and individuals with missing education status. Assignment controls are age at arrival, number of children at arrival, and marital status at arrival. Cohort fixed effects control for year of arrival in Denmark. Standard errors are robust, clustered at the level of initial commuting zone, and stated in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

tematically. We conclude that Copenhagen is not on a steeper growth path than the rest of the country.[15]

In the Online Appendix, we consider a range of robustness exercises. First, we show that the results are unaffected by using different definitions of the underlying commuting zones.[16] Second, we find that assignment to a second-tier city (Aalborg, Aarhus, or Odense), results in a weaker dynamic treatment effect, in accordance with intuition.[17] Third, we repeat our baseline regression non-parametrically. We continue to find no static effect for wages, and a negative static effect for earnings that is not significant.[18] Fourth, we consider an alternative specification where years of experience are interacted with assigned

[15]Note that this is not inconsistent with differential returns to experience in Copenhagen; simple models of life-cycle earnings with overlapping cohorts can exhibit differential wage-experience slopes across locations, but a stable aggregate city premium over time as older cohorts are replaced.

[16]Using the Eurostat definition of the Copenhagen commuting zone also leaves our results unchanged.

[17]Likewise, when we drop refugees assigned to second tier cities from the sample, and instead compare assignment to Copenhagen to being assigned to even smaller cities or the countryside, we find an even stronger dynamic treatment effect.

[18]In line with a large literature (see, e.g., Lagakos et al. (2018)), we find concavity in the returns to experience. However, the treatment effect itself is broadly linear in years of experience, and the size of the effect after 15 years is well captured by the linear model.

commuting zone population size (in contrast to our main binary Copenhagen specification), and find the return to experience to be increasing in population size. Fifth, we replace years of experience with years of potential experience (i.e., years since arrival in Denmark), and find a quantitatively smaller dynamic effect, reflecting that refugees take time to transition into the labor force.

We now analyze treatment effects on the extensive margins of labor force participation and labor supply. Wages and earnings are only observable for individuals who work. Differential selection into work across assignment regions could be a driver of the dynamic treatment effect on wages and earnings.

Table 3 shows the treatment effect on the extensive margin of labor force participation. As our measure of participation, we construct a dummy that takes the value 1 if a worker ever worked and 0 otherwise. We then regress this dummy on an indicator for assignment to Copenhagen, and the assignment controls from specification (1). We run the regression separately for those with at least a high school diploma in Column A, and those with less than a high school diploma in Column B. The effect of assignment to a city on labor force participation differs markedly across education groups. For those with at least a high school diploma, assignment to Copenhagen did not significantly affect participation; however, it raised the chance of never working by almost 4% for those without.[19]

This suggests that the treatment induced selection into the labor force for the less educated. This could reflect fact that those with little formal education find it particularly difficult to join the labor force in the city compared to elsewhere and that those who do join are more able than those who do not.

For the remainder of the paper, we focus on those with at least a high school diploma, who comprise 55% of our baseline sample. For these workers, we can rule out selection into the labor force as a driver of the dynamic treatment effect. Other forms of selection *within* a city, conditional on working, may be important drivers of the dynamic treatment effect on wages and earnings. We address this in Section 5 below.

3.2 Treatment Effects and the Return to Big City Experience

In the previous section, we showed that the average wage-experience profile among workers *initially assigned* to Copenhagen is steeper compared to that of those assigned elsewhere. These results are suggestive of a sizable big city experience premium. However, since some workers migrate after assignment, we cannot fully attribute their experience to the assignment region. This complicates the interpretation of the dynamic treatment effect as the statistical return to experience *earned in Copenhagen.* In this section, we discuss what the measured dynamic treatment effect reveals about the return to big city experience,

[19]Table A.3 in the Appendix shows the treatment effect on labor supply conditional on working at least one year in our sample ("the extensive margin of labor supply"). For this group, we regress an indicator for current employment on a full set of dummies for years spent in Denmark and the assignment controls. For all workers, in both assignment groups, employment rates rise steadily throughout our sample period. Regardless of education, employment rates conditional on joining the labor force are not significantly affected by assignment to the big city at conventional significance levels, and the point estimate is small.

TABLE 3: LABOR FORCE PARTICIPATION

	Indicator for Never Employed	
	A	B
Initial Assignment to Copenhagen	0.00200	0.0367***
	(0.00810)	(0.00886)
Assignment Controls	Yes	Yes
Nationality FE	Yes	Yes
Cohort FE	Yes	Yes
R-squared	0.145	0.177
Observations	11,129	9,432

Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. The dependent variable is an indicator taking a value of 1 if the individual never took up paid employment between 1986-2012. Column A uses the subsample of all refugees with at least a high school diploma and Column B the subsample with less than a high school diploma and those with missing information on years of education. Robust standard errors clustered at the level of the initial commuting zone. Standard errors in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

in the presence of worker relocation after assignment.

Suppose there are two city types indexed by c, big cities ($c = b$) and small cities ($c = s$), and that after demeaning all variables, the log hourly wage of worker i in city c at time t is given by the following equation:

$$w_{it}^c = \gamma^s \times E_{it}^s + (\gamma^b + \theta_i) \times E_{it}^b + \eta_{it}^c, \tag{2}$$

where θ_i is a scalar indexing person i's unobserved ability, E_{it}^c are years of experience accumulated in cities of type c, and γ^c is the causal return to this type of experience. η_{it}^c is a structural residual that captures other determinants of wages (e.g., good firm matches and occupation shifters), with $\mathbb{E}[E_{it}^s \eta_{it}]$ and $\mathbb{E}[E_{it}^b \eta_{it}]$ not necessarily zero.

Throughout, we assume $\gamma^b \geq \gamma^s$ so that equation (2) allows for a wage growth premium from working in a big city. Crucially, equation (2) reflects the fact that higher unobserved ability may make big city experience more valuable; this potential complementarity introduces the selection problem central to this paper.[20] If more able workers benefit more from being in a city, and hence move there at higher rates, higher wages in a city conditional on observables could be due to the city itself, or the resulting difference in average ability across locations. In particular, we define the *causal* return to big city experience, denoted by γ, as the extra return to an additional year of experience collected in a big relative to

[20]The wage process in equation (2) is similar to other specifications in the urban economics literature (see, e.g., Baum-Snow and Pavan (2012) and De La Roca and Puga (2017)).

a small city, such that $\gamma \equiv \gamma^b - \gamma^s$.

We partition the set of refugees into two subsets: $\mathcal{B}$ is the set of workers assigned to a big city, $\mathcal{S}$ that of those assigned to a smaller city. The value of the natural experiment is to ensure that average latent ability is the same across these groups, such that $\mathbb{E}[\theta_i \mid i \in \mathcal{B}] = \mathbb{E}[\theta_i \mid i \in \mathcal{S}]$ holds. This allows us to address the selection problem introduced by the complementarity between unobserved ability and city status as follows.[21]

Migration of workers across assignment regions complicates the mapping from the estimated treatment effect to the parameters of the wage process. As a benchmark, first consider an "ideal" setting without migration, by assuming that

(A.1.) Workers never move across assignment regions.

Assumption **A.1.** guarantees that all experience is accumulated in the region of assignment. As a result, treatment effects - which condition on initial assignment only - recover the *statistical* return to big city experience. To see this, consider regressing wages on experience for workers in groups $\mathcal{S}$ and $\mathcal{B}$ separately. The difference in the OLS estimates of the experience coefficients, $\hat{\gamma}^{\mathcal{B}} - \hat{\gamma}^{S}$, obeys

$$\text{plim } \hat{\gamma}^{\mathcal{B}} - \hat{\gamma}^{S} = \underbrace{\gamma^b + (\sigma_E^b)^{-2}\mathbb{E}[E_{it}^b \eta_{it}]}_{\equiv \beta^b} - \underbrace{\left(\gamma^s + (\sigma_E^s)^{-2}\mathbb{E}[E_{it}^s \eta_{it}]\right)}_{\equiv \beta^s} \equiv \beta,$$

where σ_E^c is the variance of E_{it}^c.[22] We define β as the return to an extra year of experience collected in the big relative to the small city. β is the main object of interest of the paper, and we refer to it as the *return to big city experience*. It consists of both the *causal* return to big city experience, $\gamma = \gamma^b - \gamma^s$, and terms reflecting other determinants of wages that covary with experience. For example, a large literature suggests that as workers gain experience, they work for increasingly more productive firms, i.e., they climb a job ladder (see, e.g. Baum-Snow and Pavan (2012)). If workers climb such ladders faster in big cities, this would contribute to the *return to big city experience*, β, through the $\mathbb{E}[E_{it}^c \eta_{it}]$ term.[23]

The estimated dynamic treatment effect above recovers β under Assumption **A.1**. However, Figure 2 shows that Assumption **A.1**. is too stark; most, but not all, workers stay in their assigned region. This changes the interpretation of the estimated dynamic treatment effect. To understand how, we replace **A.1**. with a stylized assumption about the process of relocation. We assume that

(A.2) A faction $\varkappa$ of workers move immediately across assignment regions, and then stay there.

[21] Equation (2) provides a simple framework to think about the interpretation of the dynamic treatment effect. In particular, the role of unobserved heterogeneity is reduced to an effect of unobserved ability on wages. Other forms of unobserved heterogeneity, including sorting on preferences for amenities, comparative advantage, and multi-dimensional types (as in Lindenlaub (2017)), are likely to be important in practice.

[22] This holds under the assumption that unobserved ability does not affect the probability of employment, i.e., $\mathbb{E}[E_{it}^s|\theta_i] = \mathbb{E}[E_{it}^s]$, and similarly for big cities b.

[23] This highlights that even in the "ideal" setting (under Assumption (**A.1.**)) we cannot isolate the causal return to big city experience, γ. To isolate γ in a natural experiment, workers would have to be randomly assigned both across space and also across firms *within* locations over their working lifetimes.

Figure 2 motivates these assumptions as a stylized description of the data: some workers move quickly after assignment.[24] Denote the set of individuals in group $\mathcal{S}$ who relocate from s to b by $\mathcal{M}_{\mathcal{S}}$, with $\mathcal{M}_{\mathcal{B}}$ defined analogously. Again running separate regressions on the two assignment groups and differencing the coefficients on experience yields:

$$\text{plim}\ \hat{\gamma}^{\mathcal{B}} - \hat{\gamma}^{S} = \beta - \underbrace{2\varkappa\beta}_{\text{Migration Bias}} - \underbrace{(\varkappa\mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{S}}] - (1-\varkappa)\mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{B}}])}_{\text{Selection Bias}}. \tag{3}$$

Equation (3) shows that relocation introduces two sources of bias into the interpretation of the dynamic treatment effect as a measure of the return to big city experience, β.

Migration bias occurs as long as *any* worker migrates upon assignment, i.e., $\varkappa > 0$ holds. In this case, the dynamic treatment effect underestimates the return to big city experience, β: movers from S would see faster wage growth after moving to the big city, but still be counted into the S stratification, and vice versa. This would shrink the estimated wage-experience profile difference between the two assignment groups, and hence the measured dynamic treatment effect would underestimate the return to big city experience, β.

Second, selection bias arises if $\mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{S}}]$ or $\mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{B}}]$ are non-zero, i.e., movers differ from non-movers in unobserved ability.[25] Suppose, as is common in the literature, more able workers are more likely to move to the city, and less able workers to leave the city, so that $\mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{S}}] > 0 > \mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{B}}]$.[26] The high-ability movers will nevertheless be counted in group $\mathcal{S}$, shrinking the gap in estimated slopes between the two treatment groups. As a result, the dynamic treatment effect further underestimates the return to big city experience.[27]

The above analysis suggests that under the assumption that $\mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{S}}] > 0 > \mathbb{E}[\theta_i | i \in \mathcal{M}_{\mathcal{B}}]$, we can construct an informative upper bound on the return to big city experience, β. To do so we re-estimate our baseline equation (1) only on individuals who *never move* across assignment regions. First, since these workers do not move, this measure does not suffer from the first source of downward bias we identified above. Second, we remove on average high-ability individuals whose wages grow faster with experience after moving to Copenhagen. This leaves on average lower ability individuals in our non-Copenhagen stratification than compared to the randomly selected sample. Likewise, the Copenhagen group earns all its experience in Copenhagen and contains those least likely to leave Copenhagen, since

[24] Further assumptions about moving over time, or different fractions moving, can easily be incorporated, without adding additional insight to the idea presented here.

[25] There is ample empirical evidence for the importance of selection in individual migration decisions, e.g., Borjas (1987), McKenzie et al. (2010), Young (2013), Lagakos and Waugh (2013), Behrens et al. (2014) and Eeckhout et al. (2014).

[26] In the Online Appendix, we show that more educated workers are more likely to move to Copenhagen than less educated ones. To the extent that educational attainment covaries with unobserved ability, this serves as empirical support for this stylized assumption.

[27] In the presence of selection on unobserved multi-dimensional types as in Lindenlaub (2017) much stronger assumptions are required to interpret our estimates.

TABLE 4: THE RETURN TO BIG CITY EXPERIENCE AMONG STAYERS ONLY

	Log Hourly Wage × 100			Log Earnings ×100		
	I.A	I.B	I.C	II.A	II.B	II.C
Years of Experience	1.937***	2.085***	1.653***	6.891***	6.976***	6.610***
	(0.148)	(0.170)	(0.156)	(0.303)	(0.375)	(0.311)
Initial Assignment to Copenhagen	-0.124	1.114	-1.860	-13.35***	-11.15***	-17.15***
	(0.936)	(0.800)	(1.339)	(1.867)	(2.123)	(2.308)
Years of Experience × Initial Assignment to Copenhagen	1.156***	1.051***	1.203***	3.234***	2.980***	3.654***
	(0.150)	(0.168)	(0.156)	(0.303)	(0.368)	(0.291)
Assignment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Nationality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.056	0.061	0.055	0.153	0.156	0.154
Observations	75,066	43,429	31,637	82,553	47,732	34,821

Notes: The full sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998 and who *never relocate across assignment regions*. The definition of the two assignment regions, Copenhagen and Non-Copenhagen, corresponds to the regions constructed in Section 2.3. The table shows the estimated parameters of the specification in equation 1 in the text. The dependent variables in all columns are scaled by a factor of 100 for presentational purposes. Columns I.A and II.A use the full sample. The estimates in columns I.B and II.B draw on the subset of refugees with at least a high school diploma. Columns II.C and II.C use refugees with less than a high school diploma and individuals with missing education status. Assignment controls are age at arrival, number of children at arrival, and marital status at arrival. Cohort fixed effects control for year of arrival in Denmark. Standard errors are robust, clustered at the level of initial commuting zone, and stated in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

it benefits them most. Jointly, this has the effect of biasing upwards our estimate of the return to big city experience, β.

The results of re-estimating the treatment regression in (1) on the stayer population are shown in Table 4. The results lend further credence to our simple selection story: the differential value of experience accumulated in Copenhagen is uniformly higher in the sample of stayers than in the full sample (see Table 2 above). Comparing the results in Tables 4 and 2 suggests that the *return to big city experience* lies in the range of $0.74\% - 1.05\%$ for the sample of refugees with at least a high school education, a relatively tight bound. For the rest of the paper, we refer to the *dynamic treatment effect* and the *return to big city experience* interchangeably.

4. Observable Determinants of the Return to Big City Experience

In this section, we use the wealth of information on workers, their jobs, and their locations available in the Danish administrative data to understand the observable determinants of the dynamic treatment effect identified above.

First, we investigate whether the types of jobs refugees work in differ across assignment regions in ways that can help explain the causal dynamic treatment effect identified above. If, for example, refugees started out in similar entry-level jobs across labor markets, but then, over time, sorted into more high-paying jobs in Copenhagen than outside of it, this could explain the return to big city experience. Second, we study other observables, including educational uptake and the effects of ethnic enclaves, and find these to be less important.

4.1 Sorting Across Job Types in Copenhagen and Outside

Previous work documented that cities host different industries and occupations than more rural areas (see, e.g., Davis and Dingel (2020) and Duranton and Puga (2005)) and that firms located in bigger cities tend to be more productive (see, e.g., Combes et al. (2012)). We provide evidence that jobs undertaken by our two groups of refugees differ by occupation, industry, and firm-type.

To this end, we group the 20 industries available in our data into three groups: low-skill services (e.g., accommodation and food services), high-skill services (e.g., professional, scientific, and technical activities), and manual industries (e.g., manufacturing or construction). Similarly, we assign the 10 occupations to three groups: high-skill occupations (e.g., managers and professionals), low-skill occupations (e.g., service and sales workers), and manual occupations (e.g., plant and machine operators).[28] We refer to firm types and establishment types interchangeably, since most firms in our sample are single-establishment operations. To create establishment types, we compute the average hourly wage among *all Danes* for each establishment in Denmark. We order establishments by the average hourly wage paid out to their workers and denote the set of establishments that (a) pay the highest hourly wages and (b) account for about 30% of Denmark-wide employment as *high-wage* establishments (or firms).

In both assignment regions, many refugees initially start their careers doing low-skill service work. However, as they gain experience, refugees placed outside of Copenhagen transition predominantly into manual work, while those placed in Copenhagen move into high-skill work. At 15 years of experience, 40% of refugees assigned to Copenhagen work in high-skill occupations compared to 27% of those assigned elsewhere. The sorting patterns across industries reflects those across occupations: as they accumulate experience, refugees assigned to Copenhagen become more likely to work in the high-skill

[28] The Online Appendix list all industries and occupations and their assigned groups.

services, while those assigned elsewhere increasingly sort into manual industries such as manufacturing.[29] In line with previous findings in the urban literature, high-wage establishments tend to be large and disproportionately present in Copenhagen (see Combes et al. (2012), Mion and Naticchioni (2009), and Dauth et al. (2018)). We also find that, as they accumulate experience, refugees assigned to Copenhagen are more likely to work at high-wage firms compared to refugees assigned elsewhere. We provide figures detailing these conclusions in the Online Appendix.

Overall, as they accumulate experience, refugees assigned to Copenhagen appear to work in different types of jobs compared to those assigned elsewhere: they increasingly sort towards high-wage firms and high-skill occupations in the high-skill service industry. We now test whether differential job choices by refugees can account for the wage growth premium associated with Copenhagen.

We re-estimate equation (1) with fixed effects for establishment types, industries, and occupation. We use the binary firm types introduced above as fixed effects for establishments, but include separate fixed effects for the full set of occupations and industries listed in Appendices A.3 and A.2. Column A of Table 5 repeats the baseline treatment regression from Table 2 above for comparison. Column B adds establishment type fixed effects, column C further includes occupation fixed effects, and column D adds industry fixed effects.

Including establishment type fixed effects reduces our baseline coefficient to 0.588%, suggesting that differential sorting across establishment types is an important component of the return to big city experience. Including occupational fixed effects in column C further decreases the coefficient on the return to big city experience to 0.409%. This is in line with the descriptive patterns above: refugees assigned to Copenhagen have a higher chance of working in high-skill jobs, and this advantage widens over time. For industries, we find that workers in Copenhagen are more likely to work in skill-intensive business services, while, outside of the capital, manual jobs in agriculture and manufacturing are more frequent. The fixed effects for 1-digit industries included in column D further decrease the return to big city experience to 0.371%. We conclude that, jointly, these observable job characteristics account for a large fraction of the return to big city experience.[30]

In summary, the results in this subsection suggests that the differential sorting into productive jobs is the key channel behind the dynamic treatment effect identified above. This suggests that theories of firm location decisions are central to understanding the fundamental determinants of wage growth differences across space.

[29] The Online Appendix shows the sorting patterns with experience for the full Danish population. As might be expected, on the whole Danes do more high-skill and less low-skill service work.

[30] For completeness, in the Online Appendix we show the estimates of the return to big city experience for all possible combinations of fixed effects. The order of inclusion of the different types of experience and industry and occupation fixed effects makes no qualitative difference.

TABLE 5: MECHANISMS BEHIND THE RETURN TO BIG CITY EXPERIENCE

	Log Hourly Wage × 100			
	A	B	C	D
Years of Experience	2.314***	1.962***	1.784***	1.601***
	(0.164)	(0.156)	(0.127)	(0.117)
Initial Assignment to Copenhagen	0.789	-0.399	0.372	0.302
	(0.894)	(0.682)	(0.553)	(0.555)
Years of Experience × Initial Assignment to Copenhagen	0.735***	0.588***	0.409**	0.371**
	(0.165)	(0.151)	(0.122)	(0.107)
Assignment Controls	Yes	Yes	Yes	Yes
Nationality FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
High-Wage Establishment FE	No	Yes	Yes	Yes
Occupation FE	No	No	Yes	Yes
Industry FE	No	No	No	Yes
R-Squared	0.061	0.132	0.185	0.221
Observations	57,994	57,994	48,183	44,135

Notes: The sample underlying this table includes men between the ages of 19 and 55 *with at least a high-school education* who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 1. The table presents the estimated parameters of the specification in equation 1 in the text. The dependent variables in all columns are scaled by a factor of 100 for presentational purposes. Column A replicates column I.B from Table 2 above. Columns B-D add establishment, occupation, and industry fixed effects to the baseline estimates in column A. Assignment controls are age at arrival, number of children at arrival, and marital status at arrival. Cohort fixed effects control for year of arrival in Denmark. The industries and occupations controlled for via fixed effects are listed in the Online Appendix. Standard errors are robust, clustered at the level of initial commuting zone, and stated in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

4.2 Other Observables and the Return to Big City Experience

There are other observable factors besides the types of work available that may lead to faster wage growth for refugees in Copenhagen. We investigate three channels that appear particularly important in our setting: acquiring more formal education, the effect of ethnic enclaves, and differences in the intensive margin of hourly labor supply.

We first test whether refugees initially assigned to Copenhagen take-up more years of education than those assigned elsewhere. Table A.1 in the Appendix shows the result of a t-test of mean differences in educational take-up between the two assignment groups. Differences in take-up across assignment regions are very small: about equal to one additional month of education on average. Including years of education in the baseline estimating equation (1) leaves the return to big city experience unchanged.

This suggests that educational take-up differences across commuting zones cannot explain the return to big city experience identified above.

If collocating with other immigrants of their own nationality in Copenhagen gradually gives refugees access to informal networks and employment opportunities not available elsewhere, this could help explain the measured return to big city experience.[31] To test this channel, we include the stock of co-nationals in the year of assignment in our baseline specification.[32] Table A.4 in the Appendix shows that our baseline estimates are unchanged. Refugees assigned to ethnic enclaves receive lower wages and earnings on average without affecting the estimated return to big city experience. This provides suggestive evidence that differences in refugee assimilation do not drive our results. In fact, Damm and Rosholm (2010) argue that one of the motivations for the refugee dispersal policy is that refugees tend to assimilate faster outside big metropolitan areas, which absent other channels would lead us to expect the opposite of our results.

Lastly, we show that for workers with at least a high-school degree the intensive margin of hours worked does not differ across assignment regions in a meaningful way, in each year since arrival in Denmark (see Figure A.3 in the Appendix). Differences in intensive margin choices cannot explain the return to big city experience.

Besides these observable channels, part of the return to big city experience could be due to dynamic selection on unobservables *within* assignment areas. This is conceptually distinct from the selection *across* assignment areas that the natural experiment helps address. The next section introduces a model of earnings dynamics to understand the role of the interaction between sorting across regions and jobs and unobserved ability in generating the observed differences in returns to experience across assignment regions.

5. Sorting on Unobserved Ability within Locations and the Return to Big City Experience

Differential sorting across observed firm types, occupations, and industries explains a significant fraction of the return to big city experience; sorting on *unobserved* ability within cities could explain another part, for at least three reasons.

First, there could be a correlation between unobserved type and the amount of experience accumulated at a high-wage firm, in a high-wage occupation, or in a high-wage industry. For example, if high-ability individuals are more likely to work at high-wage firms, and there are more high-wage firms in the city, we would misattribute some of the measured return to big city experience to the firm component.

[31] Several papers document that living in proximity to other immigrants of the same ethnicity has positive effects on the wage growth of newly arriving immigrants (see, e.g., Edin et al. (2003) and Damm (2009b)).

[32] There is variation in these stocks across the municipalities of assignment *within* the Copenhagen commuting zone.

Second, if high types experience faster general human capital accumulation, and the city raises their overall employment rates, this could raise the observed return to big city experience. Finally, suppose there are complementarities between worker and firm types, boosting the wage for a high-ability individual when employed at a high-wage firm. Then, if there is differential sorting into such firms over time within the city, this could appear to raise the premium to experience earned in the big city. We refer to these three channels as transitions, learning, and complementarities.

To distinguish the relative importance of the three channels, we estimate a model of location choice and earnings dynamics that explicitly accounts for the unobserved heterogeneity of workers. We estimate the model on our sample of refugees using a maximum likelihood strategy that exploits the quasi-random assignment of refugees across labor markets. Relative to Section 4, we now go beyond merely correlating the return to big city experience with observable job characteristics, to understand how working in these jobs affects lifetime earnings for different types of refugees. We conclude that the city mainly plays a role in sorting high-ability workers into high productivity jobs faster than occurs elsewhere; the role of learning and complementarities is comparatively minor.

5.1 Description of the Model

We adapt the canonical framework of Baum-Snow and Pavan (2012) to our setting. We make a few, minimal simplifications, and as such our description of the model here is brief; a full treatment is contained in the Online Appendix. Workers can work either in Copenhagen ($j = CPH$) or outside Copenhagen ($j = NCPH$) and be of either a high- or low-ability type, indexed by $h = \{h_L, h_H\}$. A worker can be employed at a high- or low-productivity firm, indexed by $f = \{f_L, f_H\}$.

Throughout, t indexes years of labor market participation, employed or unemployed, by a given individual since arrival in Denmark. Workers participate in the labor market for T years before retiring. After retirement, individuals live for an additional $T/2$ years at an income equal to their last annual wage. Throughout their working life, workers are employed (E) or unemployed (UE) and reside in one of the two locations. We denote by x^i_f the years of experience of worker i at firm type f and by $\mathbf{x}^i$ the vector $\{x^i_{f_L}, x^i_{f_H}\}$.

The spatial dispersal policy is modeled as initial assignment to an area. The randomness of the assignment guarantees the orthogonality of initial assignment and latent ability.

Wage Function. The wage of a worker in location j at time t, if employed, is a function of four state variables $(h^i, \mathbf{x}^i, f^i)$.[33] For notational simplicity, we suppress the worker index i on these state variables for the remainder of the model section. The log wage process of a worker, conditional on receiving an

[33]The wage function can be rationalized as the outcome of a general equilibrium islands model where refugees are a small fraction of the total population. Since we take employment decisions of Danes as given and the refugees in our experiment form a very small part of the overall Danish labor force, a general equilibrium framework does not seem appropriate in our context.

offer in location j and being employed by a firm of type f, is given by

$$\ln w(h, \mathbf{x}, f) = \bar{w} + \theta_h + \psi_f + \alpha_{h,f} + \sum_p \beta_1^{h,f_p} x_{f_p} + \beta_2 \left(\sum_p x_{f_p} \right)^2 + u. \tag{4}$$

Here, θ_h denotes the component of the wage that depends on a worker's latent type and ψ_f that component that depends on the productivity of the firm. In the empirical implementation, we set $\theta_{h_L} = \psi_{f_L} = 0$. The term $\alpha_{h,f}$ allows for a complementarity between firm and worker types. We set $\alpha_{h_H,f_L} = \alpha_{h_L,f_L} = \alpha_{h_L,f_H} = 0$ and estimate α_{h_H,f_H}. $\beta_1^{h,f}$ denotes the return to experience at type f firms, which we allow to differ depending on the latent ability of the worker. β_2 denotes a common return to total experience squared. Consequently, $\bar{w}$ is the mean wage offer of low latent ability workers from low-productivity establishments controlling for experience. We also assume there is a shock to match quality u that shifts each period's wage. We assume that u is drawn i.i.d. in every period from a type-1 extreme value distribution with mean zero and a variance denoted by σ_u.[34]

Labor Market Transitions and Worker Migration. For simplicity, we assume that workers in location j only receive job offers from firms in the same location. The arrival rate of job offers *from unemployment* for any firm type is denoted by $\underline{\lambda}_j^h$, which is indexed by latent ability type and location j. The corresponding probability of job offers *while employed* is denoted by $\bar{\lambda}_j^h$. While on the job, workers run the risk of being exogenously separated from their job and enter unemployment with probability δ_j^h, which may differ across locations and latent ability types. Workers on the job are also subject to reallocation shocks at rate μ_j^h. In this case, the worker is separated from his job and receives a new job offer without having to pass through unemployment.[35] Whenever a worker with ability h in location j obtains a job offer, the probability of that offer coming from firms of type f is denoted by $\pi_j^{h,f}$.

Each period t, agents receive preference shocks for each location j denoted by η_j. η_j is drawn i.i.d. from an extreme value type-1 distribution with mean zero and variance σ_η. If an agent decides to change labor markets, she loses her current job and searches for a new job in the destination location. Moving between locations incurs a utility cost.

Estimation Strategy. We estimate the model via maximum likelihood. For the estimation, we construct a longitudinal panel that contains the binary establishment type, the hourly wage, and the location for all refugees between 1986 and 2010.[36] In these data, we compute overall experience and firm-type-specific experience counts for each worker at each point in her labor market careers. The estimation strategy is to choose the parameters of the model to maximize the likelihood of observing the life paths

[34] The i.i.d. assumption is for convenience. It is conceptually straightforward to allow for match quality to be persistent over time for an individual who remains in the same job as in Baum-Snow and Pavan (2012), but computationally considerably more strenuous.

[35] This shock is necessary to match transition probabilities across firm types. In the data, some workers move from a high-wage firm to a low-wage firm, which is hard to rationalize without forced mobility.

[36] We define establishment types as in Section 4.

of all refugees in our sample in terms of firm types, hourly wages, locations, and stocks of experience by firm type. Estimated parameters are reported in the Appendix, an assessment of model fit is found in the Online Appendix.

We exploit the natural experiment through the lens of the model: the fraction of low-ability types in each region is the at assignment, χ_l, is the same at the time of assignment. This makes the estimation procedure significantly easier relative to Baum-Snow and Pavan (2012), who infer unobserved types in an iterative procedure.

5.2 Statistical Decomposition of the Dynamic Wage Premium

The estimated parameters reveal that high-ability workers are more likely to receive offers, to receive offers from high-wage firms, and to stay in employment once on the job. Moreover, these patterns are *amplified* by being in Copenhagen. Copenhagen not only allows all workers more of a chance to work at high-wage firms than they would elsewhere, but it also allows high-ability workers to sort into high-wage firms faster, and to stay there once they have arrived.

With the estimated model in hand, we quantify the contribution of sorting on unobserved ability in generating the return to big city experience. We consider three different "restrictions" on the estimated parameters of the model that we switch on and off in order to understand the contribution of unobserved ability to the measured return to big city experience through our three channels: transitions, learning, and complementarities. The corresponding parameter restrictions are:

Restriction A: *Transitions*. To explore the importance of differences in transition rates across types, we shut down job-ladder differences across types. In particular, we force job finding rates $\underline{\lambda}_j^h$ and $\bar{\lambda}_j^h$, job destruction rates δ_j^h, firm type f offer probabilities $\pi_j^{h,f}$, and reallocation shocks μ_j^h to be the same across worker types h within each location j. We set these parameters to the average of their respective values for the two latent ability groups (weighted by the fraction χ_L of low-ability workers).[37]

Restriction B: *Learning*. To understand the importance of differences in the return to experience for workers of different unobserved ability, we force the returns to experience to be the same across worker types, again by setting them equal to their average values across types.[38]

Restriction C: *Complementarities*. To quantify the importance of the complementarity between high-wage establishments and high-ability workers, we force the estimated complementarity between high-ability types and high-wage firms, α_{h_H,f_H}, to be zero, so that $\alpha_{h,f} = 0 \ \forall h, f$.

For each combination of these restrictions, we simulate a panel of 10^5 workers and estimate the baseline

[37]For other papers studying transitions in cities, see Gautier and Teulings (2009), Harmon (2013), Bleakley and Lin (2012), Martellini (2019), Dauth et al. (2018), and Papageorgiou (2017).

[38]For various theories of learning from other workers see Glaeser (1999), Davis and Dingel (2020), Martellini (2019), and Papageorgiou (2014). For learning outside the city context, see Eeckhout and Weng (2010), Jarosch et al. (2019), and Perla and Tonetti (2014).

TABLE 6: DECOMPOSITION OF THE RETURN TO BIG CITY EXPERIENCE

	Decrease in the Return to Big City Experience from shutting down...		
Restrictions Order	Transitions A	Learning B	Complementarities C
A→B→C	54.7%	23.0%	0.9%
A→C→B	54.7%	20.7%	3.2%
B→A→C	66.5%	11.2%	0.9%
B→C→A	52.2%	11.2%	15.0%
C→A→B	40.6%	20.6%	17.4%
C→B→A	52.2%	9.1%	17.4%
Average	53.5%	16.0%	9.1%

Notes: We generate the "model data" by simulating 10^5 agents using the parameter estimates in the Appendix. To construct the table we estimate the value of the interaction term between return to experience and placement in Copenhagen in the specification in equation 1, β_3, on various different samples. We report the percentage decrease in the return to experience, β_3, estimated on the baseline sample when re-estimating it on the alternative samples that result when sequentially introducing restrictions A, B, and C defined in Section 5.2 in the main text.

specification in equation (1) on this simulated data, conditioning only on initial location. In Table 6, we decompose the treatment effect of initial assignment to Copenhagen on wage-experience profiles into the contribution of the various sets of parameters. In each column, we report the percentage decrease in the return to experience when applying a given restriction. Due to the interactions of the three channels, the order in which we shut down worker differences matters and we report the effect of each restriction across all different permutations of introducing them.[39]

Regardless of the order, shutting down differences in worker transition probabilities across ability types has the most significant effect on the estimated return to big city experience. This suggests that the city's ability to help high-ability workers to climb up the job ladder towards high-wage firms faster than elsewhere and faster than their less able peers is an important driver of the observed return to big city experience. Differences in the actual return to experience among types of workers appear to be less important, as do worker-firm complementarities.

Given the results in Table 2, differential transitions contribute 0.4 percentage points of additional wage growth with each year of experience, generating 6% additional wage growth over 15 years for the group assigned to a big city. Learning and complementarities contribute 0.11 and 0.06 percentage points respectively.

In summary, the estimated model reveals that unobserved ability plays an important role in generating the observed return to big city experience. Differences between latent ability types in labor transition

[39]The residual percentage in each row is 21.4%. This part of the treatment effect is accounted for by differences in location fundamentals, including the fraction of high-wage firms in Copenhagen, which we do not shut down.

probabilities, learning, and complementarities with observed firm types jointly account for about 80% of the return to big city experience in the model-generated data. By far the most important effect of the city appears to be to allow high-ability workers to climb the job ladder towards high-wage firms faster than they would elsewhere and faster than low-ability workers.[40]

6. Concluding Remarks

In this paper, we provide causal evidence for higher returns to experience in big cities using a natural experiment. We further show that a large part of this premium can be accounted for by the industries, occupations, and firms present in big cities. Our findings suggest that theories of firm location decisions (e.g., Fajgelbaum et al. (2019); Bilal (2020); Slattery and Zidar (2020)) are central to understanding the fundamental determinants of wage growth differences across space. They also lend credence to the idea that moving to a big city is akin to an investment in your future earnings path (see Bilal and Rossi-Hansberg (2018)).

[40] As described in the introduction to Section 5, part of this effect is picked up by the firm-type fixed effects in Section 4, making a direct decomposition of the return to big city experience into observable and unobservable determinants impossible.

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APPENDIX

This Appendix contains additional materials and supporting evidence for the findings in the main body of the paper. Further details are contained in the Online Appendix to the paper.

Educational Take-Up

In this section, we test whether refugees initially assigned to Copenhagen take-up more years of education than refugees assigned elsewhere.

Table A.1 shows the result of a t-test of mean differences in educational take-up after assignment between the two assignment groups. The second column repeats this exercise for only those who had at least a high school diploma upon arrival. In both cases, differences in take-up across areas are very small, equivalent to about an extra month of schooling in the full sample. This suggests that differences in educational take-up across assignment regions are not an important driver behind the dynamic treatment effects identified in Section 3.1.

Parallel Wage Trends

We rule out differential *aggregate* wage trends across the two regions as driving our results. However, if such trends were driving the return to big city experience, we would also expect to see faster wage growth among all natives in Copenhagen compared to those outside. In Figure A.1 we plot average hourly wage earned by all working Danes, from 1986 to 2010, split by each worker's location in that year. The Copenhagen premium is quite stable in the aggregate, averaging 7 log points between 1986 and 2010. Most importantly, we do not observe a systematic divergence between Copenhagen and Non-Copenhagen for Danes. We infer that Copenhagen either is not on a steeper overall growth path than the rest of the country.

Balancing Tests for Education

The council officers assigning refugees to municipalities did not observe refugees' education levels. Here we test for significant differences in years of education between refugees assigned to Copenhagen and non-Copenhagen upon arrival in Denmark.

We regress years of education on a dummy for whether the refugees were assigned to Copenhagen or not. Additionally, we control for the assignment variables known to the council officers. Table A.2 presents the result.[41] After accounting for the information available to the council officers, we find a sta-

[41] The sample used consists only of refugees without missing information in the first year they appear in the data set, which is a larger fraction of the sample than the 19% for whom we never observe educational information from Table 1. We test and

tistically significant difference of 0.16 years of educational attainment between assignment groups. This amounts to an economically insignificant average difference of two months of accumulated education between those assigned to Copenhagen and non-Copenhagen.

Treatment and Employment Rates

In this section, we assess the effect of assignment to Copenhagen on employment rates, conditional on having joined the labor force. In Table A.3, we regress an indicator for being employed on the assignment variables, and 3-year dummy bins for time spent in Denmark. We restrict the sample to those who work for at least one year in our data.

In the first column we report the results for those with at least a high school diploma. We see that assignment to Copenhagen had no significant impact on average employment rates over the lifetime of the refugees for this group. This is not true for those without a high school diploma; controlling for the assignment relevant characteristics, and conditional on joining the labor force, employment rates are 2% higher if initially assigned to Copenhagen.

For workers with a high school diploma, we interact placement in Copenhagen with the years in Denmark bins, to understand more precisely the zero effect estimated in Table A.3. The estimated coefficients are reported in Figure A.2. For most years, the estimated coefficients are not significantly different from zero, suggesting that selection out of employment does not interact systematically with location of initial assignment for this group.

Effects of Ethnic Enclaves

Damm and Dustmann (2014) reports that prior to the Danish dispersal policy, immigrants and refugees were overwhelmingly clustered in Copenhagen and the other larger cities. In this section, we investigate whether the larger presence of other refugees from one's country in certain municipalities can explain part of the estimated treatment effects. We re-estimate our main specification in Table A.4 including a control for the presence of co-nationals of the refugee at the municipality level. In particular, we compute the stock of foreign-born residents in each municipality every year, for each of the nationalities studied in the paper. Then, for every refugee, we construct a variable that records the number of co-nationals residing in their municipality of assignment in their year of arrival, and include this in the regression. Table A.4 suggests ethnic enclaves are not an important explanation for differential wage-experience profiles across assignment regions. The estimate for the dynamic treatment effect is virtually the same as that reported in Table 2.[42]

reject the possibility that the fraction of refugees for whom education information is missing differs between Copenhagen and non-Copenhagen.

[42] We also re-estimated these regressions with the ethnic stock *in each year* as a control, instead of year of arrival, addressing concerns that refugees could sort into ethnic enclaves over time, and that this might assist them with employment opportuni-

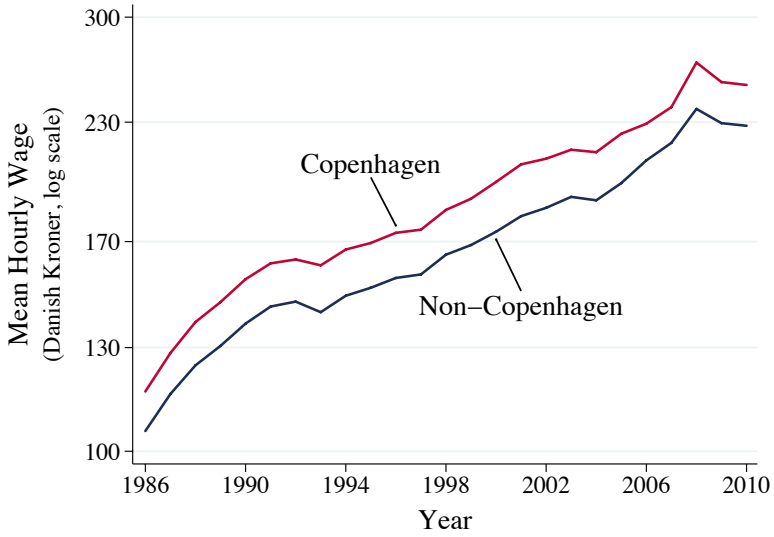
Hours Worked

In this section, we investigate the treatment effect of assignment to the city on hours worked, conditional on working. Figure A.3 shows average hours worked conditional on working that year, by assignment region and years in Denmark. Refugees with at least a high school education assigned to Copenhagen initially work fewer hours before catching up to those assigned outside Copenhagen. This accords with our finding that there is a static negative earnings premium from being assigned to Copenhagen (see Table 2 in the paper).

Estimated Model Parameters

Tables A.5 and A.6 present the parameter estimates of our maximum likelihood estimation. We allow all relevant parameters to differ by unobserved type. The only parameters we fix exogenously are the discount rate, ρ, and the log unemployment benefit b, which we set to 0.98 and 3.5, respectively. Two parameters, a_{NCPH} and τ_{NCPH}, are normalized.

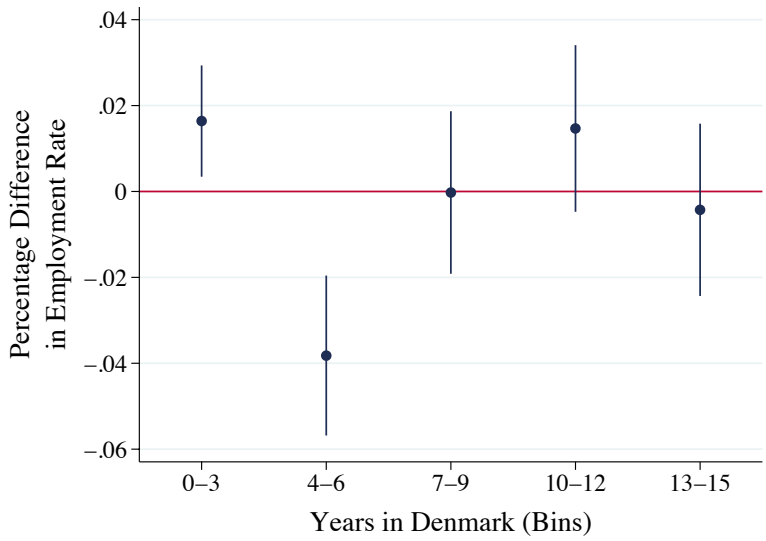
FIGURE A.1: WAGE GROWTH AMONG ALL DANES IN COPENHAGEN AND NON-COPENHAGEN



Notes: The sample underlying this figure includes all male Danes between 19 and 55 years of age. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. Log mean hourly wage is the log of the average hourly wage of an individual in a given year and a given region in Denmark.

ties. The results, available on request from the authors, are almost identical to those in Table A.4.

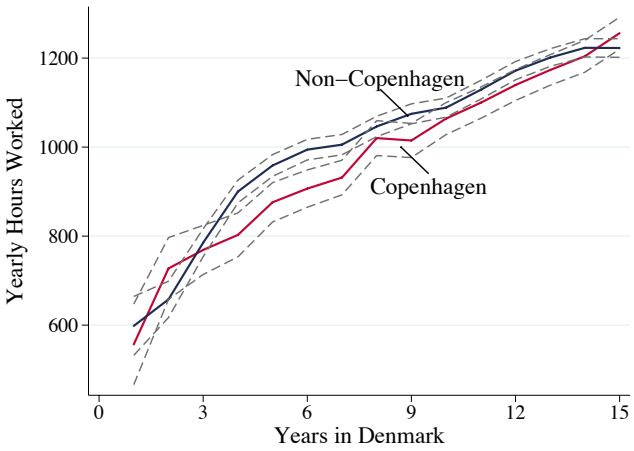
FIGURE A.2: NON-PARAMETRIC EMPLOYMENT RATE DIFFERENCES BETWEEN ASSIGNMENT GROUPS



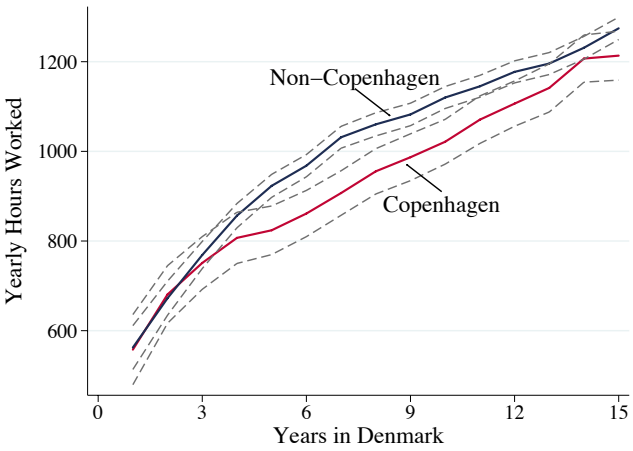
Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998 *with at least a high school diploma*. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. We regress an indicator variable taking the value 1 if the individual is in paid employment in a given year on assignment controls, 3-year experience bins, and 3-years experience bins interacted with a dummy variable that takes the value 1 if a refugee is initially assigned to Copenhagen. The figure plots the coefficients on the interaction between initial placement and experience bins for the various bins. Assignment controls are age at arrival, number of children at arrival, and marital status at arrival. Nationality and cohort fixed effects are also included. Cohort fixed effects control for year of arrival in Denmark. We also include nationality fixed effects. 95% confidence bounds shown in blue.

FIGURE A.3: MEAN YEARLY HOURS WORKED BY EDUCATION AT ARRIVAL AND ASSIGNMENT REGION

(A) AT LEAST HIGH SCHOOL AT ARRIVAL



(B) LESS THAN HIGH SCHOOL AT ARRIVAL



Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. Years in Denmark is defined as the time since asylum was granted and the refugee started appearing in the labor market data. Average hours worked are taken across all observations in the sample, conditional on employment. Figure A.3a shows average hours worked per year for all refugees with at least high school education and conditional on a given amount of years spent in Denmark. Years in Denmark is defined as the time since asylum was granted and the refugee started appearing in the labor market data. Figure A.3b shows average hours worked per year for all refugees with less than high school education and conditional on a given amount of years spent in Denmark.

TABLE A.1: T-TEST OF DIFFERENCES IN TAKE-UP OF ADDITIONAL YEARS OF EDUCATION

	Sample	
	A	B
Years of Additional Education	0.0772*	0.0942**
	(0.0309)	(0.0309)
No Take-Up of Additional Education	-0.0171*	-0.0246**
	(0.00720)	(0.00750)
Less than Two Years of Additional Education	0.00584	0.0114*
	(0.00524)	(0.00465)
Two to Four Years of Additional Education	0.00400	0.00381
	(0.00364)	(0.00479)
Four to Six Years of Additional Education	0.00726	0.00937*
	(0.00408)	(0.00397)
Observations	11,812	7,386

Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. Years of Additional Education is the years of additional education at the latest observation of an individual. All other variables are coded as an indicator of whether the individual took up a certain number of years of education. Column A uses the full sample. Column B uses the subsample with refugees with at least a high school diploma. Standard errors in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

TABLE A.2: REGRESSION OF INITIAL YEARS OF EDUCATION ON ASSIGNMENT VARIABLES

	Years of Education at Arrival		
	A	B	C
Initial Assignment to Copenhagen	0.164***	0.0980	0.0245
	(0.0491)	(0.0571)	(0.0454)
Assignment Controls	Yes	Yes	Yes
Nationality FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
R-squared	0.171	0.080	0.061
Observations	11,812	7,386	4,426

Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. The dependent variable in all columns is log years of education at arrival. The dependent variables in all columns are scaled by a factor of 100 for presentational purposes. Column A uses the full sample, Column B is restricted to refugees with at least a high school diploma, and Column C consists of refugees with less than a high school diploma. Refugees with missing education information are dropped from the regression. Standard errors in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

TABLE A.3: EMPLOYMENT RATES BY INITIAL ASSIGNMENT

	Employment Rate	
	A	B
Initial Assignment to Copenhagen	0.00992*	0.0125*
	(0.00388)	(0.00544)
3 $\leq$ Years in Denmark <6	0.252***	0.231***
	(0.00411)	(0.00488)
6 $\leq$ Years in Denmark <9	0.378***	0.318***
	(0.00418)	(0.00501)
9 $\leq$ Years in Denmark <12	0.492***	0.396***
	(0.00429)	(0.00519)
12 $\leq$ Years in Denmark <15	0.575***	0.466***
	(0.00445)	(0.00544)
Assignment Controls	Yes	Yes
Nationality FE	Yes	Yes
Cohort FE	Yes	Yes
R-squared	0.266	0.246
Observations	97,009	69,295

Notes: The full sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. The dependent variable in both columns is an indicator variable taking the value 1 if the individual is in paid employment in a given year. Column A uses the subsample of all refugees with at least a high school diploma and column B those without a high school diploma. Years in Denmark is years since arrival in Denmark, grouped into three-year bins. Assignment controls are age at arrival, number of children at arrival, and marital status at arrival. Cohort fixed effects control for year of arrival in Denmark. Standard errors in parentheses. *** indicates significant at the 1 percent level, ** indicates significant at the 5 percent level, * indicates significant at the 10 percent level.

TABLE A.4: THE RETURN TO BIG CITY EXPERIENCE WITH CONTROLS FOR ETHNIC ENCLAVES

	Log Hourly Wage × 100			Log Earnings ×100		
	I.A	I.B	I.C	II.A	II.B	II.C
Years of Experience	2.223***	2.357***	1.906***	7.765***	7.854***	7.417***
	(0.144)	(0.163)	(0.130)	(0.294)	(0.336)	(0.299)
Initial Assignment to Copenhagen	0.120	0.908	-0.815	-7.031***	-5.242**	-10.20***
	(0.866)	(0.764)	(1.257)	(1.692)	(1.736)	(2.360)
Years of Experience × Initial Assignment to Copenhagen	0.773***	0.700***	0.786***	2.034***	1.758***	2.537***
	(0.148)	(0.164)	(0.132)	(0.295)	(0.326)	(0.299)
Log Size of Ethnic Stock	-0.977***	-1.122**	-0.722***	-2.262***	-2.569**	-1.757**
	(0.195)	(0.358)	(0.162)	(0.504)	(0.769)	(0.561)
Assignment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Nationality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.057	0.062	0.054	0.158	0.161	0.157
Observations	97,402	57,994	39,408	107,297	63,870	43,427

Notes: The sample underlying this table includes men between the ages of 19 and 55 who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. Relative to Table 2, here additional controls are added for log size of ethnic stock, a variable that records for each refugee the number of co-nationals residing in their municipality in the year of assignment. The specification estimated in this table is stated in equation (1) in the text. The dependent variables in all columns are scaled by a factor of 100 for presentational purposes. The definition of the two assignment regions, Copenhagen and non-Copenhagen, corresponds to the regions constructed in Section 2.3. Columns I.A and II.A use the full sample. The estimates in columns I.B and II.B draw on the subset of refugees with at least a high school diploma. Columns II.C and II.C use refugees with less than a high school diploma and individuals with missing education status. Assignment controls are age at arrival, number of children at arrival, and marital status at arrival. Cohort fixed effects control for year of arrival in Denmark. Standard errors are robust, clustered at the level of initial commuting zone, and stated in parentheses. *** indicates significance at the 1 percent level, ** indicates significance at the 5 percent level, * indicates significance at the 10 percent level.

TABLE A.5: ESTIMATES OF MODEL PARAMETERS: BASE AND WAGE

Base Parameters			Wage Parameters		
Description	Parameter	Estimate	Description	Parameter	Estimate
Amenity in NCPH	a_{NCPH}	0 -	Base wage	$\bar{w}$	4.075 (0.002)
Amenity in CPH	a_{CPH}	-0.062 (0.003)	High-type fixed effect	θ_{h_H}	0.269 (0.002)
Moving cost to CPH	τ_{NCPH}	1 -	High firm type fixed effect	ψ_{f_H}	0.095 (0.003)
Moving cost to NCPH	τ_{CPH}	6.823 (0.073)	H-H Complementarity	α_{h_H,f_H}	0.089 (0.003)
S.D. of moving cost shock	σ_η	0.967 (0.008)	Return to Exp. for L-type at L-firm	$\beta^{h_L}_{f_L}$	0.026 (0.003)
S.D. of match quality shock	σ_u	5.816 (0.029)	Return to Exp. for H-type at L-firm	$\beta^{h_H}_{f_L}$	0.012 (0.001)
Fraction of L types	χ_L	0.518 (0.009)	Return to Exp. for L-type at H-firm	$\beta^{h_L}_{f_H}$	0.035 (0.001)
			Return to Exp. for H-type at H-firm	$\beta^{h_H}_{f_H}$	0.024 (0.001)
			Quadratic on Experience	β_2	-0.0005 (0.637)

Notes: The sample underlying this table includes men between the ages of 19 and 55 *with at least a high school education* who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. This table shows base parameters and parameters relating to the wage process for the model in Section 5 in the main body of the text. The parameters are estimated using a maximum likelihood strategy outlined in Section D.2 in the main body of the paper.

TABLE A.6: ESTIMATES OF MODEL PARAMETERS: TRANSITION PARAMETERS

	Copenhagen		Non-Copenhagen	
Description	Parameter	Estimate	Parameter	Estimate
Destruction rate for L-type	$\delta^{h_L}_{CPH}$	0.254 (0.008)	$\delta^{h_L}_{NCPH}$	0.283 (0.005)
Destruction rate for H-type	$\delta^{h_H}_{CPH}$	0.194 (0.003)	$\delta^{h_H}_{NCPH}$	0.200 (0.002)
Reallocation shock for L-type	$\mu^{h_L}_{CPH}$	0.295 (0.013)	$\mu^{h_L}_{NCPH}$	0.397 (0.021)
Reallocation shock for H-type	$\mu^{h_H}_{CPH}$	0.210 (0.013)	$\mu^{h_H}_{NCPH}$	0.220 (0.021)
Job-finding rate for L-type	$\underline{\lambda}^{h_L}_{CPH}$	0.201 (0.012)	$\underline{\lambda}^{h_L}_{NCPH}$	0.251 (0.034)
Job-finding rate for H-type	$\underline{\lambda}^{h_H}_{CPH}$	0.240 (0.007)	$\underline{\lambda}^{h_H}_{NCPH}$	0.311 (0.005)
Low-firm offer prob. for L-type	$\pi^{h_L}_{CPH}$	0.762 (0.012)	$\pi^{h_L}_{NCPH}$	0.826 (0.011)
Low-firm offer prob. for H-type	$\pi^{h_H}_{CPH}$	0.682 (0.012)	$\pi^{h_H}_{NCPH}$	0.815 (0.011)

Notes: The sample underlying this table includes men between the ages of 19 and 55 *with at least a high school education* who arrived in Denmark from Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, and Ethiopia between 1986 and 1998. This table shows base parameters and parameters relating to the wage process for the model in Section 5 in the main body of the text. The parameters are estimated using a maximum likelihood strategy outlined in Section D.2 in the main body of the paper.