The Economic Geography of Lifecycle Human Capital Accumulation:
The Competing Effects of Labor Markets and Childhood Environments

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Abstract

We examine how place shapes the production of human capital across the lifecycle. We ask: do those places that most effectively produce human capital in childhood also have local labor markets that do so in adulthood? We begin by modeling wages across place as driven by 1) location-specific wage premiums, 2) adult human capital accumulation due to local labor market exposure, and 3) childhood human capital accumulation. We construct estimates of location wage premiums using AKM-style estimates of movers across US commuting zones and validate these estimates using evidence from plausibly exogenous out-migration from New Orleans in response to Hurricane Katrina. Next, we examine differential earnings trajectories among movers to construct estimates of human capital accumulation due to labor market exposure. We validate these estimates using wage changes of multi-time movers. Finally, we estimate the impact of place on childhood human capital production using age variation in moves during childhood. Crucially, our estimates of location wage premiums and adult human capital accumulation allow us to construct estimates of the causal effect of place during childhood that are not confounded by correlated labor market exposure. Using these estimates, we show there is a tradeoff between those places that most effectively produce human capital in childhood and the local labor markets that do so in adulthood. We find that each 1-rank increase in earnings due to adult labor market exposure trades off with a 0.43 rank decrease in earnings due to the local childhood environment. This pattern is closely linked to city size, as adult human capital accumulation generally increases with city size, while childhood human capital accumulation falls. These divergent trajectories are associated with differences in both the physical structure of cities and the nature of social interaction therein. There is no tradeoff present in the largest cities, which provide greater exposure to high-wage earners and higher levels of local investment. Finally, we examine how these patterns are reflected in local rents. Location wage premia are heavily capitalized into rents, but the determinants of lifecycle human capital accumulation are not.

1 Introduction

This paper investigates how place shapes economic opportunity throughout the lifecycle. Evidence suggests that children raised in a given US commuting zone (CZ) are highly likely to live there in adulthood. For example, approximately ∼70% of young adults reside at age 26 in the same commuting zone where they were raised (Sprung-Keyser et al. 2022). As a result, children are highly likely to be exposed to both the childhood environment and to the local labor market in their hometown. Is it therefore natural to ask: Do the places that produce human capital during childhood also produce human capital during adulthood? Is there a tradeoff between them? And, what are the characteristics of place that help explain this relationship?

We begin with a simple empirical framework where wages have three determinants across place: 1) location wage premia, 2) human capital accumulation in adulthood, and 3) human capital accumulation in childhood. Here, location wage premia measure the impact of place on wages when holding an individual's level of human capital fixed. It can be conceptualized as a measure of the "price" of labor in each location. Adult human capital accumulation captures the extent to which exposure to (and experience in) a given local labor market causes an individual's wages to change over time. Childhood human capital accumulation captures the impact of exposure to a given childhood location (and its associated environment) on wages earned in adulthood. Formally, the goal of this paper is to measure the empirical relationship between these latter two factors: place-driven human capital accumulation in childhood versus adulthood.

It is theoretically ambiguous which way this relationship should go. One possibility is that successful places produce human capital throughout the lifecycle. It could be that, consistent with theories of agglomeration, large and highly educated environments increase human capital accumulation during adulthood. Those same environments could then produce positive spillovers onto kids as tax revenues rise, local investments increase, and social connections form across the income distribution. By contrast, places with high levels of adult human capital accumulation may be more physically and socially fragmented. For example, large and dynamic labor markets may have more income segregation, longer commute times, less social engagement, and fewer social connections across the income distribution. In such a scenario, we could see negative spillovers from adult human capital production onto childhood environments. It is with these two divergent models of the world in mind that we seek to estimate the empirical relationship between human capital accumulation in childhood and adulthood (and examine the characteristics of place that can help to explain this relationship.)

In order to conduct that comparison, we need to estimate each of the three place-based determinants of

wages in turn. We begin by estimating the location wage premium in each US commuting zone. We use movers across place to construct AKM-style estimates in the spirit of recent work by Card et al. (2022). Consistent with that work, we find that high location wage premia are concentrated in the largest CZs, such as the CZs in the area known as the Northeast Megalopolis, and in energy producing areas such as the Bakken region in North Dakota and Montana. Our estimates have a variance of approximately 3.7 income ranks, which, for a point of reference, is approximately 35–45% as large as the variation in upward mobility across place.[1,2] We seek to validate that our estimates capture the causal effect of place on wages rather than selection effects. We do so by introducing a new test for selection that draws upon plausibly exogenous location choices in response to Hurricane Katrina.[3] We use a sample of out-migrants from New Orleans and calculate the location wage premia in their various destinations. We then compare those estimates to location wage premia calculated on a sample of individuals originating outside New Orleans.[4] The alignment of these two sets of estimates helps confirm that our baseline estimates using typical movers are not subject to meaningful selection bias.

Next, we estimate the impact of place on human capital accumulation in adulthood. Rather than examining an initial wage change at the time of move, we focus instead on the trajectory of wages over time. We build upon techniques developed in De La Roca and Puga (2017), which measure the dynamic effects of experience in large cities.[5,6] We once again use a sample of movers across place, but now estimate a series of fixed effects that capture the impact of year-by-year exposure in each CZ. We seek to validate our estimates by using evidence on the wage trajectories of multi-time movers. In particular, we look to rule out selection effects where the differential wage trajectories across place are driven by the particular wage trajectories of individuals who choose to remain in their current location for an extended period of time.[7] We do so by con-

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[1] We present our results regarding location wage premia using individual income ranks, but we show our results are robust to the use of alternate income measurements such as family income ranks and log income.

[2] In this context, upward mobility refers to the predicted wage outcomes among children who grew up in a given location and had parents at the 25th percentile of the income distribution.

[3] We complement this with use of symmetry tests, which have been developed in the prior literature (Card et al. 2013; Card et al. 2016). These tests compare wage changes associated with leaving a location to the wage changes associated with arriving there. As we note in Section 4.2, these tests rule out certain forms of bias, but fail to rule out selection effects that have an equal and opposite effect on wages when arriving to and departing from a location.

[4] The logic of this test is that location choices made during a time of great distress after Hurricane Katrina are not likely to be motivated by the same considerations that would motivate location decisions in normal times. Consequently, they are unlikely to driven by the factors which might produce selection bias in typical estimates (time-varying human capital changes correlated with location choices, match quality, etc).

[5] This work itself builds on earlier research using longitudinal data and fixed effects to capture static measures of agglomeration effects across place (Glaeser and Maré, 2001; Combes et al., 2008).

[6] Here, the notion of adult human capital accumulation is quite broad. As we discuss in Section 5, wage gains over time could be driven by a number of mechanisms such as knowledge spillovers (within or across firms) or behavioral changes by workers. We argue that the broad patterns of our results are consistent with knowledge spillovers.

[7] We seek to address this concern because our primary estimates are constructed on a sample of individuals who make a single move and then remain in their destination location for at least 7 years. Formally, this selection concern arises if long-term

structing estimates of human capital accumulation using movers who remain in a destination for a extended period of time and show those estimates can be used to predict the wage changes of individuals who make multiple moves.[8] We then use our estimates to test for common theories of agglomeration, which suggest that human capital accumulation is higher in large, dense CZs and in places with high levels of educational attainment. We find confirmation of both of these patterns. For example, exposure to a top-50 largest CZ has a clear linear effect on wage ranks over time.[9] We estimate that after 6 years of exposure to a top-50 CZ, wages rise by an average of 0.28 ranks. This is around 27% as large as the average location wage premium in those CZs. We find similar evidence of human capital accumulation for individuals living locations with large numbers of highly educated individuals.

Finally, we estimate the impact of place on human capital accumulation in childhood. We examine the effect of exposure to childhood environments using age variation among children at the time of parental moves. Our estimation strategy builds on the approach developed in Chetty and Hendren (2018a;b). The key contribution of our analysis is to disentangle the effects of correlated exposure to childhood environments and adult labor markets. As previously noted, young adults are highly likely to live and work in the CZ where they were raised. We show that when parents move across CZs, those additional years spent in a destination CZ increase their children's probability of working in that CZ in adulthood. As a result, variation in child ages at the time of parental moves is associated with both a change in childhood environment and a potential change in labor market exposure. Simply examining the impact of childhood moves on adult outcomes does not distinguish between two effects. For that reason, we construct "hockey stick" graphs in the spirit of Chetty and Hendren (2018a;b) that separately visualize the role of childhood exposure and adult labor market exposure.[10] We then use our existing estimates of location wage premia and adult human capital accumulation to isolate the role of childhood environment. We construct a measure of wages net of local labor market exposure and use age variation in the timing of parental moves to construct causal estimates of exposure to childhood environment.[11]

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stayers are on different wage trajectories than typical movers, and that difference in trajectories itself differs across locations.

[8]As we discuss in Section 5, we also rule out certain selection concerns by controlling for pre-move wage trajectories and showing they do not impact our results.

[9]For the sake of conceptual consistency with estimates of childhood human capital accumulation, our primary measure of CZ size is derived from Chetty et al. (2020) and is the number of individuals born in each CZ between the 1978 and 1983 cohorts.

[10]These double and triple hockey stick graphs, as we refer to them in Section 6.1, also serve as an out-of-sample validation of our existing estimates. They show that our estimates of location wage premia and adult human capital accumulation estimated among a sample of adult movers serve to well approximate wage outcomes for a distinct population of movers during childhood. This is a particularly useful validation as many individuals whose parents move across CZs during their childhood do not themselves leave their parent's destination CZ in young adulthood. This means that our estimates constructed using movers across labor markets serve to well approximate the outcomes of labor market stayers.

[11]Consistent with existing work, we construct these estimates focusing on children raised by parents at the 25th percentile of

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Having constructed estimates of our three place-based determinants of wages, we then return to the central question of the paper: Do the places that produce human capital during childhood also produce human capital during adulthood? In other words, do places with strong agglomeration effects and dynamic labor markets produce human capital throughout the lifecycle, or do those places have childhood environments that actually inhibit the production of human capital during childhood?

We show that while adult human capital accumulation is positively correlated with aggregate upward mobility, adult human capital accumulation and childhood human capital accumulation trade off with one another.[12] In other words, the places that are better for human capital production in adulthood are, in general, worse for the production of human capital in childhood. We quantify that tradeoff across place by regressing our estimates of human capital accumulation in childhood on our estimates of human capital accumulation in adulthood.[13] We estimate that each 1-rank increase in earnings due to adult labor market exposure trades off with a 0.43 rank decrease in earnings due to local childhood environment.

We then explore the characteristics of place that help explain this tradeoff. We show that the tradeoff is closely linked to CZ size: adult human capital accumulation tends to rise with CZ size, but childhood human capital accumulation declines. This size-related pattern persists across nearly the full distribution of CZs, but reverses in the top 50 largest CZs, where human capital accumulation in childhood begins to rise with CZ size. We examine what it is about the nature of large and small cities that might explain these patterns.

While we do not have causal variation in place-level characteristics, we seek to identify traits that have the right "heat signature" to be plausible drivers of this relationship. In particular, we identify characteristics that vary with CZ size, have a significant relationship with the determinants of human capital, and have a strong conceptual reason for impacting human capital accumulation. Here, we find support for the hypothesis that while larger places have more adult human capital accumulation, they are also more socially and physically fragmented, which can result in negative spillovers onto children. For example, both commute times and income segregation within a CZ are linked closely to CZ size. Shorter commute times and lower levels of income segregation also a have a strong positive relationship with childhood human capital accumulation.

the income distribution. In Section 7 we show the robustness of our results to variation in the choice of income percentiles.

[12]Section 7.1 cross-walks between these two findings. Aggregate upward mobility measures the predicted outcomes of individuals growing up in a given location, conditional on the incomes of their parents. As we discuss in detail, it is driven not only by the childhood environment in a CZ, but also by the impact of the CZ on labor market exposure, and a selection effect due to parental sorting across CZs. In conducting the cross-walk, we first adjust upward mobility to net out the role of location wage premia. We find that the positive correlation falls. Next, when we remove the role of adult human capital accumulation, the correlation becomes weakly negative. Finally, when we use our causal estimates to purge the impact of selection, our coefficient of interest continues to fall and becomes even more negative.

[13]In practice, we use split sample IV to account for potential measurement error in estimates of adult human capital accumulation.

Similarly, we explore measures of social interaction such as the fraction of individuals in a CZ that are religious adherents.[14] This measure has a strong relationship with city size and an opposite-signed relationship with human capital accumulation in childhood versus adulthood. Consistent with a story of fragmentation, it appears that the physical structure of cities and the nature of the social interactions therein help to explain why places with high levels of adult human capital accumulation have worse environments for the production of human capital in childhood.

As for the reversal of this tradeoff in the largest CZs, we present evidence that it may be linked to both the presence of high-income individuals and greater levels of local investment in those CZs. For example, levels of educational attainment and area average income both rise with CZ size and are both associated with higher levels of human capital accumulation. Moreover, we examine estimates from Chetty et al. (2022a) that capture the extent to which low-income individuals have social connections to high-income individuals. This measure of social connectedness has a U-shaped relationship with CZ size. Importantly, that U-shape mirrors the relationship between childhood human capital production and size. Both measures decline over much of the size distribution of locations before rising again in the largest CZs. This reinforces the notion that social connectedness can help to explain the primary tradeoff across most CZs and the lack of a tradeoff in the largest ones. We also find similar evidence of a U-shaped relationship with size when we examine per-pupil school spending, which is a measure of local investment. While these patterns do not formally establish a causal relationship with childhood human capital accumulation, they provide insight into potential mechanisms through which strong agglomeration effects may produce positive spillovers on childhood environments. They help characterize the types of places where human capital accumulation in adulthood can coincide with human capital accumulation in childhood, rather than trading off with one another. They show that weak childhood environments are not an immutable feature of successful labor markets.

We conclude by examining how place-based determinants of wages are reflected in local prices and migration decisions. We estimate the relationship between CZ-level prices, measured using local rents, and each of our three components of wages. Location wage premia are reflected more than dollar-for-dollar in prices. This more-than-complete capitalization suggests that high location wage premia are also correlated with high levels of local amenities.[15] By contrast, the contribution of place to adult human capital accumulation

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[14]We follow Chetty et al. (2014) in using this measure of social capital.

[15]This is consistent with recent findings in Diamond and Moretti (2021) and Card et al. (2022). We show that outside the

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and childhood human capitalization do not appear to be reflected in prices. While we do not have a definitive explanation for this phenomenon, it may be that individuals are unaware of human capital effects and therefore have no discernible willingness to pay for them. Consistent with this pattern, we examine whether place-based determinants of wages are reflected in migration decisions. As young adults age, they migrate toward locations with lower location wage premia and smaller increases in adult human capital accumulation, but they do not switch to destinations that produce more childhood human capital. In other words, it does not appear that young adults make lifecycle migration decisions driven by the tradeoff in lifecycle human capital production that is present across place.

Relation to Existing Literature   Our work lies at the intersection of three primary strands of literature. The first area is work on wage premia across place. As noted above, we construct our estimates of location wage premia using an AKM-style approach in the spirit of recent work by Card et al. (2022). The approach from Card et al. (2022) itself builds on an extensive literature estimating the wage premia of firms. Those techniques were initially developed by Abowd et al. (1999) and have been applied to address a wide variety of topics such as time variation in firm wage premia and their contribution to inequality (Card et al. 2013; Song et al. 2018; Bloom et al. 2018).[16]   Our primary contribution in this space is the development of a new test to validate whether our AKM-style estimates are capturing the causal effect of place rather than selection.[17] Here, we use plausibly exogenous out-migration in response to Hurricane Katrina, which is a source of identifying variation that has previously been used to examine outcomes such as long-run wage changes and mortality (Deryugina et al., 2018; Deryugina and Molitor, 2020). The test itself is inspired by a broader literature on forecast testing that is used in a wide range of contexts such as measuring teacher value added or the mortality effects of health insurance plans (Angrist et al., 2017; Abaluck et al., 2021).

Our work relates to a second strand of literature on the role of place in creating human capital accumulation via the labor market. Here, we build most directly on recent literature using individual fixed effects and wage changes over time to measure the impact of work experience in large cities (De La Roca and Puga,

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100 largest CZs location wage premia are priced in at 79 cents on the dollar. This variation with CZ size suggests that location wage premia are nearly fully capitalized into prices across all CZs and, consistent with what one might expect, larger CZs have higher levels of amenities.

[16]It is worth noting that there is also a robust econometric literature regarding firm wage premia estimates and concerns such as limited mobility bias (Andrews et al., 2008; Bonhomme et al., 2020; Kline et al., 2020). Our focus is on estimating a limited number of location fixed effects relative to the number of observations in our sample (only 741 CZs rather than hundreds of thousands or millions of firms). This helps us to side-step these limited mobility concerns, but we discuss them in more detail in Section 4.

[17]We also seek to provide a public good by releasing commuting zone level measures of location wage premia for use in future work.

2017; Card et al., 2023).[18] Our CZ-level estimates allow us to build upon previous analyses in a number of key ways. It allows us to test theories of agglomeration by examining the relationship with other place-based characteristics such as educational attainment. Here, our findings relate to a broader literature on theories of agglomeration which highlight the role of both city size/density and area level educational attainment (Ciccone and Hall, 1996; Rauch, 1993). Additionally, they allow us to directly estimate the pairwise relationship between childhood human capital production and adult human capital production across CZs. Our use of multi–time movers to test for selection once again builds on the forecast testing literature to provide a novel way to test the validity of these causal estimates.

Finally, our work relates to literature on the role of place in contributing to upward mobility via childhood exposure. We construct measures of upward mobility across place using methods developed in Chetty et al. (2014) and Chetty et al. (2020). We then use techniques developed in Chetty and Hendren (2018a;b) to measure exposure to childhood environment using variation in child age at the time of parental moves. Our key contribution relative to their approach is to separately estimate the role of exposure to childhood environment as distinct from exposure to labor markets in adulthood.[19] As we examine the relative contribution of labor markets and childhood exposure, our work is also related to a literature on mediation analysis in the context of upward mobility (Bolt et al., 2021; Rothstein, 2019; Zohar and Dobin, 2023).

The rest of the paper proceeds as follows. Section 2 provides a simple conceptual framework that outlines the place-based determinants of wages. Section 3 describes the data and sample used to construct our estimates. Section 4 estimates location wage premia across US commuting zones and validates those estimates. Section 5 estimates the impact of place on adult human capital accumulation and uses the results to test various theories of agglomeration. Section 6 estimates the causal effect of childhood environment on human capital accumulation. Section 7 estimates the empirical relationship between child and adult human capital accumulation across place and examines the characteristics of place that explain the tradeoff. Section 8 concludes.

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[18] As noted above, this work itself builds on earlier research using longitudinal data and fixed effects to capture static measures of agglomeration effects across place (Glaeser and Maré, 2001; Combes et al., 2008) It also relates to a broader literature on the determinants of the urban/city size wage gap (Baum-Snow and Pavan, 2012; Behrens et al., 2014; Combes and Gobillon, 2015; Gould, 2007).

[19] This exercise has a connection to Card and Krueger (1992), which estimates the return to school quality and seeks to distinguish that effect from differential labor market returns to schooling across place. It also has a connection to a broader literature focused on the role neighborhoods environment on economic outcomes (Case and Katz, 1991: Chyn, 2018).

2 Conceptual Framework

Our goal is to understand the determinants of wage across place. In order to do so, we start with a simple empirical framework. Consider an individual with a level of earnings w_i. Let their wages be determined both by their level of individual human capital, θ_i, and a premium paid to individuals living in their current location l, LM_l. This second term is known as the location wage premium in location l, and it can be conceptualized as a measure of the "price" of labor in location l. For simplicity, we let $w_i = \theta_i + LM_l$.[20]

We then further evaluate the role of human capital by isolating its place-based determinants. In particular, we write individual human capital in the following manner:

$$\theta_{it''} = \sum_{0}^{t'} CHC_{l(i,t)} + \sum_{t'+1}^{t''} AHC_{l(i,t)} + \gamma_i \qquad (1)$$

Here, CHC_l represents the human capital that a person accumulates during childhood as the result of living for a year in location l. In other words, it is the impact of exposure to that childhood environment on subsequent earnings of an individual in adulthood.[21] An individual raised in location l accumulates CHC_l from birth until the end of their childhood, t'. Here, AHC_l represents the causal effect of working in location l on the human capital that a person accumulates during adulthood.[22] It captures the impact of an additional year of working (and potentially learning on the job) in location l on earnings in subsequent years. Individuals accumulate those local labor market effects from the beginning of their working lives up through the time of income measurement, t''.

Putting this together with our initial wage equation, we get a single expression for wages in terms of their place-based determinants:

$$w_{it''} = \sum_{0}^{t'} CHC_{l(i,t)} + \sum_{t'+1}^{t''} AHC_{l(i,t)} + LM_{l(i,t'')} + \gamma_{it''} \qquad (2)$$

We also provide a simple graphical visualization of this wage process across the lifecycle in Figure 1.[23]

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[20]In the analysis to follow, we measure wages in terms of income ranks. We do so in part because we are comparing wage changes throughout the lifecycle, and the presence of rank stability across ages serves to simplify that analysis. In that context, we find that this additively separable structure does well to fit the data. An additively separable structure would also be appropriate if we considered log wages, as is the case in previous work on location wage premia (Card et al. 2022; 2023).

[21]In the spirit of Mincer (1958), we conceptualize these human capital effects as accruing due to exposure over time. Mincer highlights childhood human capital accumulation as the result of years of education and adult human capital accumulation as the result of labor market experience. Our approach replaces those terms with location-specific estimates corresponding to the effect of childhood or adult exposure on human capital.

[22]Here, the notion of human capital accumulation is quite broad by design. It captures any impact of adult location on the annual income rank of the individuals who work there. As we discuss more in Section 5, it could capture any type of skill accumulation due to experience in a given labor market. It could also capture behavioral changes due to labor market experience. In practice, our estimates show linear exposure effects over time and vary across place in a manner that is consistent with common theories of agglomeration. This suggests we are primarily measuring the accumulation of skills over time.

[23]In particular, Figure 1 shows the wage trajectories by age for a hypothetical individual over time. It considers an individual

Equation 2 serves as the motivating expression for our analysis to follow. Formally, the goal of the paper is to estimate and understand the empirical relationship between CHC_l and AHC_l. Do they tradeoff with each other? Do they covary positively? What are the characteristics of place that help explain this relationship?

As noted above, it is not ex-ante obvious how these sources of human capital production should vary across place. One possibility is that successful places produce human capital throughout the lifecycle. It may be, that, consistent with common theories of agglomeration, large and highly educated environments result in dynamic labor markets with positive skill spillovers that increase human capital during adulthood (Ciccone and Hall, 1996; Rauch, 1993; Glaeser and Resseger, 2010). At the same time, those same agglomeration effects could also produce positive spillovers onto childhood environments. As skills of existing workers rise and high-skilled workers sort into certain locations, that could increase the local tax base (Koh et al., 2013; Jofre-Monseny, 2013) and allow for greater levels of local investment in areas such as schooling.[24] Similarly, the presence of those highly-paid workers could result in a greater degree of social connectivity across the income distribution (Chetty et al., 2022a; Chetty et al., 2022b). Those networks could allow for transmission of skills during childhood.

By contrast, it is also possible that places which produce human capital during adulthood actually inhibit the production of human capital during childhood. While urban environments have dynamic labor markets, they also have high levels of residential segregation on the basis of both income and race.[25] They may have concentrated business districts which employ workers who commute long distances (Burd et al., 2021;Gordon et al., 1989).[26] They may also be socially fragmented with lower levels of community engagement and fewer social connections across income groups (Putnam, 2000). If agglomeration effects result in such negative spillovers, we should expect there to be a tradeoff across place between our two objects of interest, CHC_l and AHC_l.

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who is raised in location o, spending both their childhood and their early working life there. It visualizes the increase in latent wages associated with a childhood in location o and the accumulation of adult human capital as the result of labor market experience in location o. It then shows the change in the individual's wage trajectory as they move to a new location d. It first shows the jump in wages associated with the difference in location wage premia between locations d and o. It then shows the accumulation of adult human capital due to labor market experience in location d.

[24]There is also some suggestive evidence in a developing country context that these effects are reflected in local investments (Giang et al., 2017).

[25]There is an extensive literature on the role of residential segregation in worsening outcomes for non-White residents of cities (Charles, 2003; Cutler and Glaeser, 1997; Massey and Denton, 1993).

[26]It is worth noting that while there is a clear positive relationship between city size and commute times, the relationship itself is an equilibrium object determined by the decisions of employers and employees, and by evolving city characteristics such as transportation infrastructure (Angel and Blei, 2016a; Angel and Blei, 2016b)

3 Data and Sample

We conduct our analysis using de-identified administrative and survey data from the U.S. Census Bureau. We begin with a sample of US born individuals drawn from the Census Numerical Identification Database (Numident) of Social Security Number holders. Our estimates of location wage premia are constructed using a sample of individuals who migrate across commuting zones and are aged 25-44 in the years before their move. We also restrict our primary sample to moves that occur between 2010 and 2016. Our analysis of migration in response to Hurricane Katrina also considers individuals who move in 2005.[27]

We determine the locations of individuals by drawing upon a Residential History File (RHF) developed for use in Sprung-Keyser et al. (2022). The RHF draws location information from the Form 1040 when such information is available. It then supplements with locations reported on information returns (W2s and 1099s). Finally, it draws upon location information from the Department of Housing and Urban Development (HUD), which tracks residence in public housing or use of housing vouchers.[28,29] When we identify movers across commuting zones, we focus on individuals who remain in their destination location for at least 3 years. Formally, we identify a set of individuals who are observed in an origin location in year t, who move to a new commuting zone in year t+1 and then remain in that commuting zone in years t+2 and t+3. We place this restriction to ensure that our measure of earnings captures a full calendar year where an individual remains in the same commuting zone.[30]

Our primary measure of income is individual income ranks. We construct individual income as the sum of all W-2s earnings in a given year. Individuals are assigned an income of 0 if they did not receive a W-2. We construct ranks within cohort and within calendar year, following the approach in Chetty et al. (2020).[31] We

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[27]When we estimate the impact of place on adult human capital accumulation, we increase our available sample size by including moves that occurred between 2007 and 2009. This sample change has no meaningful impact on our estimated location wage premia. And, as we demonstrate in Section 7.1 with the use of CZ-by-year fixed effects, the inclusion of the Great Recession in our sample period does not impact our primary findings.

[28]In some instances, an individual may have multiple information returns with conflicting addresses. If one of those locations is consistent with an individual's next Form 1040 location and that location is continuously observable until the year of the 1040 filing, we prioritize that location. If there is no such continuously observable location, we prioritize the newest address to appear in the information returns. (We use a 3-year window to identify new addresses.) We focus on new locations because individuals may still receive information returns associated with previous locations. For example, if they fail to provide their bank with updated location information, they may receive a 1099-DIV at their old address.

[29]In some cases, full location information is not available from any of these sources but ZIP code information is still collected during filing of the Form 1040. In that case, we use that ZIP code to identify a filer's commuting zone.

[30]When an individual files a Form 1040 for the tax year t, the location on that form is their residence at the timing of filing. Such filing likely occurs between February and April of year $t + 1$, but it may occur at any point between January and October if the individual files an extension. So, if an individual moves from origin o to destination d between their tax filings in year t and year $t + 1$, that means they were likely residing in origin o in the first months of year $t + 1$ and were living in destination d by the middle of year $t + 2$. Their move to destination d could have occurred as late as February or March of year $t + 2$, and so their earnings in year $t + 3$ might not capture a full year of earnings in that new destination. That is why we require that an individual is also observed in destination d in year $t + 3$.

[31]All individuals without observed income are assigned the mean rank of that group. Put another way, if 10% of individuals

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also report results using family income ranks, where family income is defined as adjusted gross income (AGI) found on the Form 1040.[32] We show that our results are not sensitive to our choice of income measurement. In cases where the existing literature has tended to report results in terms of log income, we also show the robustness of our results to such an approach.

When constructing estimates of childhood exposure to place, we rely on a sample of children born in the 1978–1990 cohorts. We link children to parents using dependent claiming on the Form 1040 and construct matches using a five-year claiming window centered at age 16.[33] When we construct a sample of parental movers, we use parents who are observed changing location a single time before their child turns age 28. When we construct a sample of parental stayers, we use parents who are not observed changing location before their child turns age 28. In both cases, we measure parent wages in the year in which we construct our parent-child data linkages. (As noted above, this is done in a small window centered around the time the child turns age 16.) We measure the outcomes of their children at age 28.

Our demographic controls and heterogeneity analysis uses information on the race and ethnicity of movers across CZs. We get that self-reported information from the Decennial Census and the American Community Survey (ACS). We prioritize information from the 2010 Decennial Census, supplementing with information in the 2000 Decennial Census and finally the 2008–2018 ACS.

Our analysis in Section 7 examines how CZ-level characteristics can help to explain our results. We draw some of those estimates from existing work (Chetty et al. 2014; Chetty et al. 2022a; Chetty et al. 2022b). We construct measures of other CZ characteristics, such as area-level college graduation rates and area-level rents, using data from the 2015 ACS. In one instance, we construct a measure of local income segregation using a Numident sample of all US born adults ages 25–54 between 2006 and 2018.

4 Location Wage Premia

4.1 Estimating Location Wage Premia

In order to evaluate the relationship between the place-based determinants of human capital, we begin by estimating the location wage premium in each US commuting zone. The location wage premium captures

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have no 1040 or W-2 earnings, they are each assigned a rank of 5. This imputation is motivated by the findings in Chetty et al. (2020). They use the American Community Survey (ACS) to examine average self-reported income among non-filers without W-2 income. They find that sample of individuals has a median self-reported income of just $5,000.

[32]Our analysis of migration in response to Hurricane Katrina uses family income as the primary income measurement because W-2 incomes are not available before 2005.

[33]Consistent with the approach in Sprung-Keyser et al. (2022), we prioritize linking at age 16 before seeking to link children using the following age order: 15, 17, 14, 18.

the impact of place on an individual's wages, holding their level of human capital constant. We conduct our analysis on a sample of movers across CZs. In the spirit of Card et al. (2022), we use an AKM-style approach and estimate a fixed effect that captures the wage premium in each CZ.[34]

The intuition behind this approach can be seen mostly clearly if we return to the data generating process outlined in Equation 2:

$$w_{it''} = \sum_0^{t'} CHC_{l(i,t)} + \sum_{t'+1}^{t''} AHC_{l(i,t)} + LM_{l(i,t'')} + \gamma_{it''}$$

When individuals move across place, there is a jump in their wages associated with the change in the location wage premia between their origin, LM_o, and their destination, LM_d. (We visualize this in Figure 1, where we show the wage process for a hypothetical individual over time.) Therefore, in order to estimate $LM_{l(i,t'')}$, we want to zoom in on the period around a move and examine wages on either side of the move.[35]

In particular, we estimate:

$$w_{it} = \alpha_i + \sum_m \beta_m [\mathbb{1}(l(i,t) = m) + \vec{\Upsilon} \cdot X_{it} + \eta_{it} \tag{3}$$

Here, α_i is an individual fixed effect that absorbs all characteristics of the mover pre-determined before the time of the move. In the context of the DGP in equation 2, it absorbs $\bar{\gamma}_i + \sum_0^{t'} CHC_{l(i,t)} + \sum_{t'+1}^t AHC_{l(i,t)}$.[36,37] Next, X_{it} captures the time-varying impact of demographic characteristics. In other words, it allows for the time-varying component of individual human capital, $\gamma_{it''} - \bar{\gamma}_i$ to vary based on demographic characteristics. The β_m are our coefficients of interest. They capture the location wage premium in each location m.[38]

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[34]This stands in contrast to the context in which the AKM approach was originally developed and for which it is most commonly used, measuring firm wage premia using movers across firms.

[35]For a move that occurs in year $t+1$, we use period t to measure an individual's pre-move wages and period $t+3$ to measure their post-move wages. Given the nature of our data, $t+3$ is the first year that we can ensure an individual has lived in their destination location for 12 months. Much of our geographic information is derived from locations on the Form 1040. If an individual is filing their taxes for the tax year t, they fill out that form between January and April of the year $t+1$. (They may file as late as October if they receive an extension.) The location information provided on the Form 1040 is their location at the time of filing out the form. So, if that tax return in year t+1 shows a different address than the return in year t, that individual may have moved as late as April of year $t+2$. This means that their income for the year $t+2$ might have been split between their origin location and their destination location. If the individual files their year $t+3$ tax returns and it contains the same address information as years $t+1$ and $t+2$ we can be sure that they spent the full year $t+3$ in the destination location.

[36]Here $\bar{\gamma}_i$ is the average of the individual component of human capital. This leaves a time-varying component $\gamma_{it} - \bar{\gamma}_i$ that we discuss in Section 4.2 below because it is key for addressing selection concerns in our estimation of β_m.

[37]Zooming in on the period around the move allows us to assume that an individual's adult human capital accumulation does not vary in the window in which we construct our estimates. So, for an individual who moves at t" and has their post-move income measured at t, the assumption here is that $t \approx t''$.

[38]Card et al. 2023 argue that location wage premia should be estimated by constructing firm wage premia for all firm movers and then aggregating up to the CZ level. They argue that using CZ-movers alone understates the variance across place because movers to high location wage premium locations tend to move to low wage premium firms, and vice versa. In order to address this, we also construct our estimates using movers across firms. We find that the variance of CZ movers is actually higher in our sample. As we report in Appendix Table 1, when we regress our firm-movers estimates on our CZ-movers estimates we get a coefficient of 0.86. We believe that the main reason for this finding is the discrepancy across our two samples. Card et al. 2023 uses quarterly data from the LEHD while we rely on annual data. Given the annual nature of our data, in order to construct estimates of firm movers, we need to identify individuals who are found in a given firm in year t, move to a new firm in year $t+1$ and remain in that firm until year $t+3$. This means we conduct our analysis on a subset of individuals who consistently remain attached to the labor force but only make one firm change in a four year period. The restriction to this subset of individuals appears to drive down the variance of our estimates.

The map of our estimates across CZs can be found in Figure 2.[39,40] Consistent with the findings in Card et al. (2023), we find that high location wage premia are concentrated both in large urban areas and in energy producing locations. There is, for example, a large swath of high-location premium CZs along the eastern seaboard in what is sometimes referred to as the Northeast Megalopolis. There are similarly high location wage premia locations in the San Francisco Bay Area and in the large cities of the Pacific Northwest. As for the energy producing locations, location wage premia are highest in the Bakken region and other oil producing locations such as West Texas. In Appendix Figure 2, we formally investigate how these location wage premia vary with CZ size. We see evidence of a U-shaped pattern where wage premia are highest in the largest cities but also rise slightly in the smallest (and potentially most remote) cities.[41] This is consistent with the notion that location wage premia may capture both differences in productivity but also differences in compensating differentials.[42] That U-shaped pattern stands in stark contrast to the evidence on adult human capital accumulation found in Section 6 below. Across all CZs the variance of our estimates is approximately 3.7 income ranks.[43,44] For a point of comparison, this is approximately 35–45% as large as the variance in upward mobility across place.

4.2 Validating Location Wage Premia Estimates

Having constructed our estimates of location wage premia, we then seek to validate that these capture the causal effect of place. In the context of our estimation strategy in Equation 3, the key question is whether the choice of destination d is correlated with the error term, η_{it}. In the context of our data generating process, the question is whether the choice of place is correlated with the time-varying component of individual human

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[39]For reasons of privacy protection, we follow the general methodology used by the US Census Bureau's Small Area Estimates Program and apply Empirical Bayes shrinkage to our estimates. For CZs of above median size, we shrink toward the mean of all above-median sized CZs. We take the symmetric approach for CZs of below median-size. The details of the approach can be found in Appendix A.

[40]Our primary results show the location wage premia calculated among the full population of movers. In Table 2 we show that these patterns look relatively similar across demographic groups. In particular, we compare fixed effects for men versus women and Black movers versus White movers. When we regress these estimates upon each other, we find coefficients in excess of 0.75 and often quite close to 1.

[41]Appendix Figure 2 shows a similar U-shaped pattern when plotting location wage premia against commuting zone density.

[42]Here, any productivity gains associated with location wage premia are static productivity gains. They are shifts in worker wages associated with working in certain local labor markets. It is important to distinguish between these static productivity gains and the dynamic productivity gains we explore in Section 5. As we discuss below, our measure of adult human capital accumulation focuses on dynamic wage gains that appear to be driven by skill accumulation.

[43]There is a recent literature that outlines how the plug-in estimates of the variance of fixed effects estimates may be upwardly biased when examining fixed effects each estimated with a substantial amount of noise. This does not pose a concern in our context, where we consider only 741 fixed effects (relatively small by typical AKM standards.) We show this formally by following the approach of Kline et al. 2020 and estimating the variance using their leave-out method. As we show in Table 1, this adjustment reduces our variance by less than 1%.

[44]We also estimate these location wage premia using family income rather than individual incomes. We find that the variance differs by approximately 5%. This strongly suggests our results are not sensitive to the measure of income used.

14

capital, $\gamma_{it''}$.[45] We implement two methods to test for this.

First, we begin by using symmetry tests, a method typical in the literature (Card et al., 2016; Card et al., 2023). This approach compares the wage changes of people leaving a given location to the wage changes of the people arriving there. This approach serves to rule out asymmetric bias across place, where a given location receives a selected set of in-migrants or out-migrants. For example, consider a case where individuals who have learned how to use GPT-4 all choose to move to location l. Those individuals have a change in their individual human capital level, γ_{it}. The change is concurrent with their choice of location and so will introduce upward bias in our estimate of location $l's$ wage premium. That said, as long as that GPT-effect doesn't also result in out-migration from location l among individuals who saw a sudden reduction in their skills, such a scenario would break symmetry. As a result, the symmetry test is able to rule out such an occurrence.

We implement this test by constructing separate origin and destination fixed effects for each CZ.[46] In particular we estimate:

$$\Delta w_{it} = \alpha_i + \sum_m \beta_m^o [\mathbb{1}(o = m) + \sum_m \beta_m^d [\mathbb{1}(d = m) + \overrightarrow{\Upsilon} \cdot X_{it} + \mu_i^{\Delta w} \tag{4}$$

Here, $\hat{\beta_m^o}$ captures the location wage premium in location m as measured using the change in wages among individuals who depart that location. $\hat{\beta_m^d}$ captures the location wage premium in the same location as measured using the change in wages among individuals who arrive in that location. We test for symmetry by regressing these two sets of fixed effects upon each other and examining whether we get a coefficient close to 1. In practice, that is exactly what we see. When we regress origin fixed effects on destination fixed effects we get a coefficient of 1.01 (0.013), and when we conduct the regression in the opposite direction, we get a coefficient 0.95 (0.012). Appendix Figure 1 shows a binned scatterplot of this relationship. We

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[45]In the context of our DGP, we begin with a time-varying component of individual human capital, γ_{it}. Our individual fixed effects absorb the average of individual level human capital $\bar{\gamma}_i$. We also allow for demographic characteristics X_{it} to explain the time-varying component of human capital. As a result, we are left with a time varying component of individual human capital that can't be explained by observable characteristics, $\gamma_{it} - \bar{\gamma}_i - \overrightarrow{\Upsilon} \cdot X_{it}$. Our concern is about whether it is correlated with the choice of destination d. It is also worth noting that here we define human capital broadly to include any determinant of individual wages. So an individual can increase their human capital by accumulating new skills, but they could also simply choose to work more or work harder and increase their earnings in that manner. Any of those factors could impact time-varying individual human capital.

[46]We implement this regression approach rather than simply comparing mean wages of individuals who move from O to D or D to O. We do so because it allows us to implement demographic controls. While these controls do not meaningfully impact results, they do provide some evidence of differential Roy sorting across ages. In particular, Appendix Figure 3 shows the wage changes among movers who switch between CZs that differ in their location wage premium by less than 0.5 ranks. We find that movers under age 40 have a slight wage rank increase after their move. Older movers have a slight wage decrease. This Roy selection pattern does not bias our results as long as the degree of sorting is identical across place, but it serves as the motivation for the inclusion of our demographic controls.

also repeat these symmetry tests for a series of alternate measures of income such as log income and income ranks for individuals above the 20th income rank. The symmetry results remain consistent across those specifications.[47]

While these symmetry tests rule out certain forms of selection, they do not rule out all forms of bias in our estimates. In particular, they do not rule out bias that is symmetric when arriving to or leaving from a location. For example, let's consider a case where the types of individuals who choose to become more career oriented also choose to move location l. Let's assume that people who become less career oriented often choose to leave location l. In such a scenario, that change in attitude among movers to or from location l may not be due to the causal effect of place, but it will still produce symmetric bias in our estimates of origin and destination effects.

We introduce a new approach to help us rule out symmetric bias of this nature. We do so using evidence from plausibly exogenous migration decisions in response to Hurricane Katrina. Figure 3 Panel A shows the rates of out-migration from New Orleans in the years around the hurricane. We look at the rate that individuals leave New Orleans and resettled elsewhere, and we find those rates increase by a factor of more than 6 in the year immediately following the storm.[48] This pattern suggests that nearly all of these movers were forced to make their moves in response to the storm and would not have done so otherwise.[49] Given that these moves were made in a time of great distress, it seems likely that movers chose their destinations based on considerations such as the presence of family, rather than considerations that are more likely to produce selection bias, such as the quality of their firm match. With that intuition in mind, we use the wage changes of out-migrants from New Orleans to test for selection in our estimates among other movers.

We start by considering a sample of individuals who originated outside New Orleans in 2005 and moved to some other non-New Orleans location. We estimate the location wage premia in each of their destination locations using the destination fixed effects among these typical movers, $\hat{\beta}^d$. We then consider a sample of individuals who originated in New Orleans and left in 2005. We then use this sample to estimate the location

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[47]It is worth noting that when we restrict to individuals earning more than $15,000 per year and consider log income, we do see some slight violations of symmetry. In that case, the coefficient from regressing destination fixed effects on origin is around 1.15 as compared to 0.79 the other way around. We consider this suggestive evidence that income ranks do a better job capturing the income process at play here.

[48]For the years before the storm we only have location information from the Form 1040, rather than having location from information returns as well. As a result, we construct these estimates only using locations and family income from the Form 1040. In Appendix Figure 5 we show that our migration rate estimates are relatively similar if we use post-move locations derived from information returns.

[49]The fact that these individuals would almost certainly not have moved in the absence of the storm means that we construct our estimates on a population of individuals that differs from a set of typical movers. For that reason, if the location wage premia calculated among typical movers results in a LATE that differs substantially from the ATE across the full population, our test should suggest the presence of such a divergence.

16

wage premia in each of their destination locations, $\hat{\lambda}^d$. Regressing our fixed effects estimates among the New Orleans movers, $\hat{\lambda}^d$ on the fixed effects estimates for the non-New Orleans movers $\hat{\beta}^d$ provides a measure of the bias in our original location fixed effects estimates.[50] Subject to some mild assumptions, the coefficient in that regression gives us a lower bound on the fraction of the variance in $\hat{\beta}^d$ that comes from the treatment effect rather than selection.[51] The intuition here is that we have a set of location wage premia estimated among New Orleans out-migrants that we think are subject to a limited degree of selection bias. If those fixed effect estimates align closely with fixed effects among a set of typical movers, it suggests the estimates from the typical movers are unlikely to be subject to a substantial degree of selection bias. As we show in the binned scatterplot in Figure 3 Panel B, we estimate a coefficient of 0.993 (0.134). This indicates that there is little or no selection bias in fixed effects estimates constructed using typical movers. As a result, it appears that our baseline estimates are subject to little or no selection.

We further validate these patterns by considering the year-by-year wage trajectories of individuals leaving New Orleans. We construct an event study where we take the year-by-year wages of movers who leaves New Orleans and we regress them on the corresponding fixed effects estimated among non-New Orleans movers. As we show in Figure 3 Panel C, this produces coefficients of approximately 0 in the years before the move and coefficients of approximately 1 in the years after the move.[52] This serves to further validate estimates of location wage premia constructed using typical movers across place.

5 Location-Specific Adult Human Capital Accumulation

Next, we estimate the impact of place on human capital accumulation in adulthood. We do so using wage trajectories of movers, rather than examining initial changes in their wages at the time of a move, as we did

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[50]As noted in Section 1, this approach is related to a broad literature on forecast testing that has been used in a wide range of contexts such as measuring teacher value added and measuring the impact of health insurance plans on health outcomes (Angrist et al. , 2017; Abaluck et al., 2021).

[51]As we show in Appendix B there are two key assumptions here. The first is that our exogenous variable, $\hat{\beta}^d$, is captured without noise due to measurement error. We address this by using a split sample to construct our estimates of $\hat{\beta}^d$ and then using split sample IV in our regression of $\hat{\lambda}^d$ on $\hat{\beta}^d$. The second assumption is that bias has a non-negative correlation with the treatment effect. If there is no correlation then our regression coefficient directly captures the fraction of variance from the treatment effect. If the correlation is positive, then the regression coefficient serves as a lower bound. The only case that is a potential cause for concern is one in which the correlation is negative. In that instance, our coefficient will serve as an upper bound. While such an occurrence is certainly possible, the bias in typical selection stories tends to run the opposite direction. In order for the covariance to be negative, individuals moving to locations with high location wage premia need to experience large relative wage declines at the time of their move.

[52]We validate these estimates using a few alternate specifications. Appendix Figure 6 Panel A uses a single destination fixed effect calculated in the year after the move and compares that to the wages of New Orleans movers in each year. This is different from our primary specification where, consistent with our approach in Section 5 below, we allow the fixed effects among non-New Orleans movers to vary in the years after the move. Appendix Figure 6 Panel B uses pre-move fixed effects among non-New Orleans movers to test for pre-trends. This also lies in contrast to our baseline approach where we use post-move fixed effects to test for selection.

in the last section. We validate these estimates by using the wage trajectories of multi-time movers to address potential selection concerns. We conclude this section by using our estimates to test (and find evidence in favor of) existing theories of agglomeration.

5.1 Estimating Adult Human Capital Accumulation

We seek to estimate the causal effect of labor market exposure on human capital accumulation. In the context of our visualization in Figure 1, this corresponds to the trajectory of an individual's wages as the result of experience in a given labor market.

Once again, we begin with a sample of movers across place, just as we did in Section 4. Now, we focus on a primary sample of individuals who remained in their destination CZ for at least 7 years after their move. (We also consider the robustness of our results to alternative restrictions.) Rather than estimating a single fixed effect associated with their post-move wages, we estimate a series of fixed effects that capture the impact of place on wages in each year after the move. These fixed effects reflect the trajectory of wage growth associated with yearly exposure to a given labor market.

In the context of our data generating process in Equation 2, the goal is to capture $AHC_{l(i,t)}$. We let $\alpha_i = \bar{\gamma}_i + \sum_0^{t'} CHC_{l(i,t)} + \sum_{t'+1}^{t} AHC_{l(i,t)}$ and $\gamma_{it} = \bar{\gamma}_i + \overrightarrow{\Upsilon} \cdot X_{it} + \eta_{it}$. We can then consider the wages of an individual in each of the j years after they make a move and write their wages in the following manner:

$$w_{i,j} = \alpha_i + \sum_{j=1}^{7} \sum_l \beta_l^j [\mathbb{1}(l(i,j) = l) + \overrightarrow{\Upsilon} \cdot X_{ij} + \eta_{ij} \tag{5}$$

Here, β_l^1 measures the initial location wage premium associated with residing in location l. It captures the initial wage change associated with a move to that location and corresponds to our estimates of $\hat{LM}_l$ in Section 4. The subsequent fixed effects β_l^j capture the impact of continued exposure to location l. These are CZ-by-years-of-experience fixed effects.

In order to see how these fixed effects, our β_l^j's, vary across place, we average each of them across the 50 largest CZs and then plot those year-by-year averages. The results, shown in Figure 4 Panel A, demonstrate that in the first year after the move the average fixed effect, β_l^1, is 1.06 ranks. This is our estimate of the average location wage premia in the 50 largest CZs. Over the subsequent 6 years, the fixed effect estimates increase from 1.06 ranks to 1.36 ranks. These effects appear approximately linear over time, which we can further validate by directly constructing a linear estimate of yearly exposure to each CZ. Those linear

estimates align almost exactly with our year-by-year fixed effects. We estimate that each additional year spent living in a top 50 largest CZ is associated with wages that are 0.047 higher ranks. After 6 years this effect is 27% as large as the initial location wage premium.[53]

It is important to note that the place-driven notion of human capital accumulation here is quite broad. It refers any impact of place on the trajectory of annual earnings among the individuals who reside there. For the purposes of our analysis, we remain mostly agnostic about the causal mechanism behind the human capital accumulation we observe. It could be the result of direct knowledge spillovers within and across firms (Arzaghi and Henderson, 2008; Rosenthal and Strange, 2004; Roche et al., 2022). It could be the result of location-specific job ladders (Moscarini and Postel-Vinay, 2018; Henning and Kekezi, 2023). It could also the result of place-induced behavioral changes that impact the trajectory of an individual's earnings.[54] Our estimation strategy does not rule out any of these types of effects. That said, the pattern of our results suggests that we are primarily measuring skill accumulation over time. We find clear linear exposure effects over time and, as we'll see in a moment, we find variation across place that is consistent with theories of agglomeration. Those patterns suggest we are likely isolating place-driven skill accumulation over time.

5.2 Validating Adult Human Capital Accumulation Estimates

Having estimated fixed effects that capture the wage trajectories of individuals across place, we then seek to validate that these estimates capture the causal effect of place on wages. In other words, returning to Equation 2, we seek to rule out a selection bias where the trajectory of the individual-specific determinant of wages, $\{\gamma_1, \gamma_2, \gamma_3, ...\}$, is correlated with the choice of location. We do this by addressing two forms of potential selection. The first is the possibility that individual location choices are correlated with persistent wage trajectories in the years before a move. In other words, the concern is that the types of individuals

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[53]For context, the magnitudes of these effects are quite modest relative to existing estimates in the literature. Work by De La Roca and Puga (2017) suggests that workers who move to the five largest cities in Spain have yearly wage gains between 1.4 log points in the 3rd-5th largest cities and wage gains of 2.9 log points in the 2 largest cities. Results from Card et al. (2023) suggest that each additional year spent in a top 25 largest CZ is associated with 1.6 log points higher wages. Over the course of 6 years that corresponds to over 17.5 log points in the largest cities in Spain and 9.6 log points in the largest US cities. If we assume that young workers earn $25,000 per year, this corresponds to a wage increase of approximately $2,500 to more than $4,500. As noted, we estimate a 6-year effect of being in the top 50 largest CZs of approximately 0.28. A rank corresponds to $900-1500 in young adulthood depending on the age of income measurement, and so our effect falls meaningfully below those other estimates. In interpreting our results, it is also helpful to consider differences in aggregate upward mobility across place. Estimates from Chetty et al. (2020) suggest that upward mobility in the top 50 largest CZs is approximately $1000 larger than the mean and upward mobility in the top 25 CZs is approximately $1,500 larger than the mean. If it were the case that six years of exposure to these locations resulted in wages that were at least $2,500 higher, that effect would be nearly twice as large as the aggregate difference in upward mobility.

[54]Evidence suggests that changes in worker productivity due to peer effects sometimes stem from motivational changes associated with working with high productivity peers, rather than true learning effects (Mas and Moretti, 2009; Brune et al., 2022).

who chose location l might be on a steadily increasing wage trajectory. We alleviate this concern by adding controls for pre-period wage changes and showing they do not attenuate our estimates. For example, in the context of Figure 6 below, we estimate the relationship between our estimates and log CZ size. Inclusion of pre-period wage controls changes the estimate by less than 0.5%. The second form of selection we seek to rule out is a correlation between the choice of location and variation in wage trajectories that depends on the length of time that individuals stay in a given location. In particular, we construct our estimates using the wage trajectories of individuals who remain in a given location for 7 years after their initial move. It could be that long-term stayers stick around in a location because they are on a particularly good wage trajectory. If that pattern is quantitatively significant, and also differs heterogeneously across locations, such a selection effect could introduce bias in our estimates.[55]

We address this potential selection concern by examining the wage trajectories of multi-time movers. We implement a four-step procedure for this test. First, we consider a sample of individuals who move to some location d and remain in that location for 7 years after a move. We estimate both the location wage premium in each commuting zone, β_d and a linear exposure effect associated with an additional year of residence in d, ρ_d. Second, we construct a sample of individuals who move to location d and then make a second move to a new location d' after t' years. (We restrict t' to 3–5 years so as to allow a meaningful amount of time in each location.) Third, we use the estimates from the long-term stayers to construct predictions for the wage changes of these two-time movers. In particular, we construct an estimate for the change in the location wage premium between locations d and d', using the estimates from our long-term stayers sample $\beta_{d'} - \beta_d$. We also construct an estimate for their predicted change in adult human capital accumulation based on the time they spend in each location, $t'\rho_{d'} - (7 - t')\rho_d$. Finally, we regress the observed wage change for the multi-time movers on the these two predicted wage changes. We estimate:

$$\Delta w_i = \kappa_{LM}(\beta_{d'} - \beta_d) + \kappa_{AHC}[t'\rho_{d'} - (7 - t')\rho_d] + \mu_i^{\Delta w}. \tag{6}$$

Our coefficients of interest κ_{LM} and κ_{AHC} allow us to determine whether estimates constructed using long-term stayers can predict the wage changes of multi-time movers. Figure 5 shows the binned scatterplots corresponding to the two coefficients. They are 0.984 (0.026) and 1.107 (0.076) respectively, indicating that

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[55]In our opinion, it is this second condition, the heterogenous variation across locations, that makes this form of selection somewhat improbable. If it is the case that individuals who leave a given location have bad wage draws in the years before they leave, such an effect will not bias our results. That pattern will be picked up by our controls for years relative to one's move. This will be a kind of universal Roy sorting that does not bias our estimates. We will only find bias in our estimates if the choice to be a long-term stayer rather than a short-term stayer has a differential relationship with wage trajectories across different places. For example, it will introduce bias if the choice to remain in some City A is observationally associated with large steady increases in wages while the decision to remain in City B has no such pattern.

changes in predicted wages using long-term stayers map essentially one-for-one on the observed wage changes for multi-time movers.[56] This serves to rule out differential selection across place correlated with the length of time someone stays there.

5.3 Agglomeration and Human Capital Accumulation

We now consider the characteristics of place associated with strong human capital growth. Our search is motivated by the existing literature on agglomeration, which suggests that learning in cities is linked both to the size and/or density of those locations and to the concentration of individuals with high levels of human capital. In order to test this, we repeat our exercise from Figure 4 Panel B. Now, we examine the exposure effect of the 50 CZs with the highest college graduation share, rather than the effect of the 50 largest CZs. The results, presented in Panel B, show an initial location wage premium of 1.74 ranks and an effect that rises to 2.15 ranks over 6 years. These exposure effects of place are all measured relative to a national mean, so the presence of locations with strong adult human capital growth implies the presence of locations where human accumulation is comparatively weak. Panel C plots the estimates for rural locations (as measured in Chetty et al. (2014)) and finds that wages fall by 0.521 ranks after 6 years exposure to these locations.

Rather than estimating these effects across groups of CZs, such as the top 50 largest locations, we can estimate these effects linearly over the full population of commuting zones. Panel A of Figure 8 shows the results of a regression of adult human capital accumulation on city size.[57] There is a clear upward sloping relationship that persists over the full distribution of CZs. We estimate that for each 1-log increase in CZ size, the impact of a year of employment on individual wages is approximately 0.03 ranks. Panel B shows the relationship between log CZ size and the share of college graduates in a CZ. The pattern is broadly similar, showing a clear upward slope across the full distribution. We estimate that for each 10 percentage point increase in the college graduation share in a given CZ, the impact of a year of employment on additional wages is ∼.08 ranks. These results are consistent with the notion that large and highly-educated locations are ideal environments for the accumulation of human capital in adulthood.

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[56]In Table 3 we report an alternate version of this test where we incorporate CZ-by-year fixed effects into our construction of exposure estimates and our predictions. This is designed to rule out bias introduced by time trends. In that case, we need to expand the sample so that CZ-by-year fixed effects are not collinear with exposure effects. We expand the sample back to 1998 by switching to family income and deriving locations only from the form 1040. We examine multi-year movers and construct a prediction of wages in each year after the move as determined by the location wage premium and exposure effects among the long-term stayers. Our regression of observed income on predicted income yields a coefficient of 1.032 (.055), but when eliminating the exposure effect from our prediction the coefficient drops to .198 (.057).

[57]While we use CZ population as our primary outcome, Appendix Figure 8 Panel C reports the relationship with respect to commuting zone density and shows the same relationship.

6   Location-Specific Childhood Capital Accumulation

Having estimated location wage premia and adult human capital accumulation across place, we turn our attention to the role of place in childhood human capital accumulation. In order to do so, we begin by adopting a popular estimation strategy: we use a set of children who moved during their childhood, and we examine the impact of those moves on the adult outcomes of those children. These estimates are often interpreted as capturing the impact of exposure to a place's childhood environment. That said, we argue these estimates incorporate the effect of correlated exposure to both childhood environment and local labor markets.

The key consideration here is that children born in a given CZ are highly likely to live there in adulthood. For example, nearly 70% of children found in a given CZ at age 16 are still living there at age 26 (Sprung-Keyser et al., 2022). This means that children who are exposed to the childhood environment of a given location are also highly likely to be exposed to the labor market in that location. We show that when parents move across CZs, that move is associated with change in subsequent labor market exposure for their children. In this section, we use our existing estimates of location wage premia and adult human capital accumulation to disentangle the effects of childhood environment and labor market exposure.

6.1   Disentangling Correlated Exposure to Childhood Environments and Labor Markets

As noted, we begin by adopting an estimation strategy first developed in Chetty et al. (2018a;b), and examining the impact of child environment using age variation among children at the time of parental moves. For the purpose of illustration, we initially set aside the role of labor markets and implement the canonical version of this approach. We examine a sample of children whose parents move once during their childhood and consider the wages they earn in adulthood. For a child whose parents moved at age m, we estimate:

$$w_i = \sum_m b_m [\mathbb{1}(m_i = m)\Delta\omega_{odp} + \overrightarrow{\Upsilon} \cdot X_{it} + \eta_{it} \tag{7}$$

Here, w_i is the income rank of the child at age 28. $\Delta\omega_{odp}$ is the change in location quality between the child's origin and destination locations. We measure location quality using the wage outcomes in adulthood among children who lived in a single location for their entire childhood and had parents with income rank p. In this context, ω_{lp} is a measure of upward mobility for children raised in each location.[58] X_{it} includes

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[58]Formally, this is the predicted wage outcomes for children whose parents are at the *pth* percentile of income distribution. That prediction is formed by constructing a place-specific slope and intercept that maps parental income ranks onto child income ranks in adulthood.

controls for cohort fixed effects, parental income ranks, and origin quality.

Figure 7 Panel A plots our coefficients $\hat{b_m}$ across each age at move m. This reproduces the typical "hockey stick" structure found in the childhood exposure graphs from Chetty and Hendren (2018a). The downward sloping region indicates that, as children are exposed to a given destination for a longer period of time, their adult outcomes converge toward the outcomes of children who spent their whole childhood there. This suggests there is a clear causal exposure effect of place. The flattening of the estimates at older ages is typically interpreted as evidence of selection. If the migration decisions of parents when their child is age 31 serve to predict the wage outcomes of those children at age 28, that suggests migration decisions are correlated with unobserved determinants of wages.

As noted above, while these estimates use aggregate upward mobility as a proxy for quality of the childhood environment to which children are exposed, they do not distinguish between the role of childhood exposure and the role of labor market exposure. When a child moves across CZs, they are exposed to a new childhood environment. It could also be, however, that they are also exposed to a new labor market. That correlated exposure to childhood and adult environments could impact our estimates from Equation 7. In particular, if age variation in the timing of parental moves impacts the labor markets in which the children of movers ultimately work, then our $\hat{b_m}$ will also capture the effect of changes in exposure to the local labor market in adulthood.[59]

We can test for this in the data by examining how adult locations vary with the age at which children move. Figure 7 Panel B reports the adult locations among children whose parents moved during their childhood. In particular, it plots children by their age at the time of their parents' move and reports the probability that they are observed living in their parents' origin or destination location. It shows that the timing of parental moves has a clear impact on the labor markets in which their children reside. For parental moves that occur before a child turns age 16, young adults have an approximately 65% chance of living in their childhood destination and less than 5% chance of living in their CZ of origin. These probabilities are approximately constant across all 16 years. When we consider parental moves at older ages, we see a sharp decline in the probability that a child ultimately ends up in their parents' destination location. For parental moves at age 22, children are approximately equally likely ($\sim$32-33%) to be found at age 28 in either their parents' origin

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[59]While existing work has established that young adults are highly likely to reside in the location where they were raised, the migration pattern discussed here does not necessarily follow from those findings. We need to test it directly in our data because it relies on a particular claim about the relationship between the timing of parental moves across locations and the locations in which children ultimately reside.

or destination location. These patterns suggest that age variation in parental moves is associated both with changes in the duration of exposure to place and in the probability of ending up in a given labor market. As noted, such a phenomenon may have an impact on the interpretation of our childhood exposure estimates $\hat{b_m}$. Consequently, we seek to re-estimate these exposure effects while accounting for the role of future labor market exposure in adulthood.

For simplicity, we begin by trying to isolate the role of location wage premia before turning next to the role of adult human capital accumulation. We consider a modified version of Equation 7 where we add in a separate term that captures the effects of location wage premia. We estimate:

$$w_i = \sum_m a_m [\mathbb{1}(m_i = m)\Delta LM_{od} + \sum_m b_m[\mathbb{1}(m_i = m)\Delta\tilde{\omega}_{od\tilde{p}} + \overrightarrow{\Upsilon} \cdot X_{it} + \eta_{it} \tag{8}$$

Here, we've added ΔLM_{od} interacted with an indicator for age at time of move. This captures the change in location wage premia associated with these moves.[60] We've also adjusted our measure of location quality, $\Delta\tilde{\omega}_{od\tilde{p}}$ to purge out the effect of location wage premia. We've used observed locations at the time of income measurement to adjust both for the location wage premia of parents and for the location wage premia of children at age 28. In essence, we are constructing a new measure of upward mobility that is calculated net of location wage premia.[61]

Figure 7 Panel C plots the results from Equation 8. The result is a "double hockey" pattern that is fully consistent with the evidence on migration rates found in Panel B of the same Figure. The exposure effect of location wage premia is approximately flat before age 16 because moves at younger ages do not result in differential location decisions at age 28. Starting from age 16 onward, the downward sloping pattern is consistent with a decreasing likelihood that an individual will be found in their parent's destination CZ. [62]The

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[60]For this sample, and for the analysis to follow, we construct our measure of location wage premia using a weighted average of location wage premia across ages. Alongside our primary sample of movers, who are ages 35-44 in the years before their move, we also consider a sample of individuals who are ages 25-26 in the years before their move. We construct the location wage premia for those younger movers and give them a weight equal to the probability that an individual will have left their origin CZ before age 28. We give the remaining weight to the wage premia calculated among movers ages 35-44. This approach is motivated by our analysis in Appendix Table 8 and Appendix Figure 8. Together they show that location wage premia are higher among younger movers and that young first-time movers have higher destination fixed effects than origin fixed effects. In other words, it appears that initial moves may be subject to higher location wage premia (maybe as the result of particularly good firm matches) but location wage premia settle down in older ages. This motivates the use of older age fixed effects to approximate the fixed effect for stayers and younger age fixed effects to capture the fixed effect for movers. We return to the role of weighting in Section 7.1 and show that our primary results are not sensitive to the choice of specification.

[61]Here, we can think of this adjustment as capturing upward mobility in the space of human capital, rather than income. Consider two sets of parents, who are observed in different locations, a and b, but have the same level of nominal income y. If location a has a higher location wage premia than location b, then location a is doing more to increase the nominal wages of its residents than location b. In the absence of that location effect, the parents in location b would be earning more. When seeking to construct a measure of human capital that potentially could be transmissible between parent and child, this suggests we want to net out the contribution of those labor market effects and use wages net of location wage premia as our proxy for human capital.

[62]In Appendix Figure 7, we show an alternate version of this double hockey stick graph where our estimates of location wage premia on the right-hand side of Equation 8 are replaced with the location wage premia corresponding to the true location in

hockey stick pattern for location quality remains very similar to our previous estimates. To the extent that it shifts slightly, the slope is marginally steeper and the magnitude of the intercept (the estimated selection effect) is slightly smaller. These two adjustments are consistent with what we might expect when removing the effect of location wage premia. Removing a component of upward mobility that is flat under age 16 should serve to steepen the remaining graph. Similarly, the migration graphs are relatively flat after age 28 because a parental destination after age 28 has predictive information about their child's location at age 28. As a result, some of the intercept in Figure 7 Panel A may be the result of correlated location decisions rather than a selection effect.

Having accounted for the role of location wage premium, we can then repeat the same basic exercise to further account for the role of adult human capital accumulation. We consider a modified version of Equation 8 and estimate the following:

$$w_i = \sum_m a_m[\mathbb{1}(m_i = m)\Delta LM_{od} + \sum_m b_m[\mathbb{1}(m_i = m)\Delta \tilde{\tilde{\omega}}_{od\tilde{p}} + \sum_m c_m[\mathbb{1}(m_i = m)\Delta AHC_{od} + \overrightarrow{\Upsilon} \cdot X_{it} + \eta_{it} \quad (9)$$

Here, ΔAHC_{od} captures the impact of the move on adult human capital exposure. $\Delta \tilde{\tilde{\omega}}_{od\tilde{p}}$ further modifies our definition of location quality to adjust not only for the location wage premia in each place, but also for the adult human capital accumulation among stayers in that place.[63] It provides us a measure of upward mobility that is net of all adult labor market exposure.

Figure 7 Panel C reports the results of our three location exposure variables.[64] The result is a "triple hockey stick" graph. The exposure effect of the location wage premium remains essentially unchanged as compared to Panel B. It is flat before age 16 and then declines through age 28. Although the estimates of adult human capital accumulation are slightly noisier, they show the same basic pattern. They also have a flat region followed by a steeper one between ages 16 and 28.[65] The final series, our measure of location quality net of labor market effects, shows a clear downward sloping pattern before flattening off around age

which each childhood mover is observed at age 28. Consistent with the presence of sorting, we find coefficients close to 2. Parents who move to places with high location wage premia have kids who are themselves higher earning individuals in adulthood. The graph also shows a slight upward slope for moves after age 16, indicating a small degree of differential selection by age among later-age movers. This helps to explain why the downward slope on our estimates of location wage premia in Figure 7 Panel C is slightly shallower than the observed change in migration probabilities in Panel B.

[63]Here we used the observed labor market history of these individuals between ages 23 and 28.

[64]We group our move ages into sets of two years in order to account for the loss of power associated with estimating adult human capital accumulation in each CZ.

[65]This figure also serves as a form of out-of-sample validation for our estimates in Sections 4 and 5. The set of children in this sample impacted by parental moves is only minimally overlapping with the set of movers used to estimate location wage premia and adult human capital accumulation. The fact that the hockey stick structure shows the appropriate downward sloping pattern helps to confirm the validity of our estimates. The slopes we observe are particularly reassuring because many of the children who moved during childhood likely did not move in young adulthood. So, while our estimates of location wage premia and adult human capital accumulation were constructed on a population of movers, this suggests that they do well to capture the impact of place on stayers.

24. This presents strong evidence for an effect of childhood environment that scales with the amount of time spent in each location.[66]

Taken together, these results demonstrate that parental moves across locations often result in both a change in childhood environment and a correlated change in labor market exposure. Estimation strategies which use age variation in the timing of parental moves to measure the impact on child outcomes may be actually measuring the combination of childhood environment and labor market effects. By accounting directly for the role of labor market exposure, both in terms of location wage premia and adult human capital accumulation, we are able to modify this estimation strategy to evaluate the impact of childhood environment alone.

6.2 Constructing Estimates of Childhood Environment Across Place

Having examined the exposure effects of childhood environment, our goal now is to construct CZ-specific estimates of the causal effect of childhood exposure to place. The hockey stick patterns from Panel C of Figure 7 demonstrates that our current measure of childhood environment, $\tilde{\tilde{\omega}}_{l\tilde{p}}$, upward mobility net of labor market exposure, has a clear exposure effect over time. That said, this does not necessarily capture the causal effect of childhood environment because we have not yet accounted for the potential role of selection in these estimates. It could still be that the types of parents who sort into certain locations have particularly high wage children, even conditional on their level of parental income. In that case, $\tilde{\tilde{\omega}}_{l\tilde{p}}$ will provide us a biased measure of childhood environment.

We can see the role of this bias more formally by revisiting our data generating process from Equation 2. If we take an individual's income and net out the impact of location wage premia and adult labor market exposure, we are left with childhood human capital accumulation, $\sum_0^{t'} CHC_{l(i,t)}$ and other human capital, γ_i. Our measure of childhood environment (not yet purged of selection), $\tilde{\tilde{\omega}}_{l\tilde{p}}$, is constructed by calculating individual wages net of labor market exposure effects and taking an average with respect to place and parental human capital.[67] We can therefore write $\tilde{\tilde{\omega}}_{l\tilde{p}}$ as the sum of three terms:

$$\tilde{\tilde{\omega}}_{l\tilde{p}} = \sum_t CHC_{l,\tilde{p}} + HC_{\tilde{p}}^{par} + HC_{l,\tilde{p}}^{select}. \tag{10}$$

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[66]This also shows that the location quality as measured among the population of parental stayers serves as a good proxy for the location quality experienced by the sample of childhood movers. The alignment of the treatment effect across these groups helps to validate the general applicability our estimates to follow where we use movers across CZs to produce causal estimates of the effect of childhood environments.

[67]Just as in Section 6.1, parental human capital is defined as parental income net of location wage premia. We use parental locations at the time of income measurement to identify that location wage premia.

Here, $CHC_{l,\tilde{p}}$ is our object of interest. It captures the impact of a place l on childhood human capital accumulation. While our initial wage equation assumed that CHC_l was homogenous within place, we now extend this to allow childhood human capital accumulation to vary heterogeneously with parental human capital, $\tilde{p}$. This allows us to align with specifications from Chetty and Hendren (2018a;b). Here, $HC_{\tilde{p}}^{par}$ represents the average wage outcomes for individuals with parental human capital $\tilde{p}$. This is just a measure of national-level upward mobility.[68] Finally, $HC_{l,\tilde{p}}^{select}$ is any residual variation in wages among individuals with parental human capital $\tilde{p}$ who are raised in location l. It is the selection term that we need to account for. This term captures parental sorting across destinations. Formally, $HC_{l,\tilde{p}}^{select} = E[\gamma_i | \tilde{p}, l] - E[\gamma_i | \tilde{p}]$. It captures the fact that parents who sort into certain locations themselves have children with subsequent wage outcomes ($E[\gamma_i | \tilde{p}, l]$) that differ systematically from the average wage outcomes of children with parental human capital, $\tilde{p}$, ($E[\gamma_i | \tilde{p}]$).

In the analysis to follow in Section 7, we will often use $CHC_{r,\tilde{p}} + HC_{r,\tilde{p}}^{select}$ as a noisy measure of childhood human capital accumulation. This allows us to demonstrate when our results are sensitive to adjusting for selection. That said, we are primarily interested in estimating the role of childhood human capital accumulation $CHC_{r,\tilde{p}}$ as distinct from the role of parental selection $HC_{r,\tilde{p}}^{select}$. Our current estimates of childhood environment, $\tilde{\tilde{\omega}}_{l\tilde{p}}$ combine these effects and so we need an estimation strategy that isolates $CHC_{r,\tilde{p}}$.

Motivated by the hockey stick design in Figure 7, we once again return to an evaluation of parental movers across place. Drawing upon the approach in Chetty and Hendren (2018a;b), we estimate the impact of an additional year of exposure in each CZ on outcomes in adulthood.

Here, we build upon the original approach in Chetty and Hendren 2018b by measuring wage outcomes net of labor market exposure effects. This allows us to identify the impact of an additional year of childhood exposure as distinct from the impact of labor market exposure. We do so by estimating the following regression:

$$\widetilde{w}_i = \xi_{od\tilde{p}} + m_i(\mu_{o(i)} + \mu_{o(i)}^{\tilde{p}}\tilde{p}_i) + (T - m_i)(\mu_{d(i)} + \mu_{d(i)}^{p}\tilde{p}_i) + \overrightarrow{\Upsilon} \cdot X(\tilde{p}, t) + \eta_i \tag{11}$$

Here, $\widetilde{w}_i$ is the wage outcomes of individual i net of both the location wage premia in their age 28 CZ, and net of the human capital they accumulated in adulthood as the result of their labor market experience. $\mu_{l(i)} + \mu_{l(i)}^{\tilde{p}}\tilde{p}_i$ are our objects of interest, as they measure the impact of an additional year of exposure in location l. They capture both a level and slope effect of exposure that varies with parental human capital.

[68]Formally, $HC_{\tilde{p}}^{par} = E[\gamma_i | \tilde{p}]$. It is worth noting that interpretation of this term as measuring upward mobility requires that we normalize the place-specfic determinants of wages to be mean-zero.

$\xi_{od\tilde{p}}$ is the origin-by-destination wage effect for someone with parental human capital $\tilde{p}$. It is measured as the sum of an origin-by-destination fixed effect and an origin-by-destination effect interacted with parental human capital. $X(\tilde{p}, t)$ captures our demographic controls, including a control function that varies with the interaction of parental human capital and time.

In order to construct our estimates of $\mu_{l(i)} + \mu_{l(i)}^{\tilde{p}}\tilde{p}_i$ we follow Chetty and Hendren (2018b) and use a two-stage procedure. We begin by measuring the exposure effect at the origin-by-destination level. We then project those estimates onto a single measure of exposure in each sample. We conduct all our analysis on a sample of individuals whose parents move once during their childhood. Consistent with the approach above, we measure income ranks at age 28. We adjust for labor market exposure using the location wage premium of an individual's age 28 location, and we adjust for adult human capital accumulation using their labor market history from ages 23-28.[69],[70]

It is worth noting that unlike the analysis of the hockey stick graphs in Section 6.1, this method does not rely on any assumption about the relationship between the wage outcomes of parental stayers and the wage outcomes of parental movers. Instead, it directly measures the effect of an additional year of exposure to a given CZ. This means that parental selection on destination will not bias our estimates.[71] This approach merely requires that the degree of selection is not associated with the child's age at the time of move. (This is an assumption that has been validated in previous work (Chetty and Hendren, 2018a;b).)

We can therefore use this parental movers estimation strategy to construct estimates of the causal effect of childhood exposure in each CZ. This allows to produce a measure of a causal impact of childhood environment that can then be compared to the other place-based determinants of wages.

7 Relationship Between Place-Specific Determinants of Wages

Having estimated the place-based determinants of wages outlined in Section 2, we now examine the empirical relationship between the impact of place on human capital accumulation in childhood versus adulthood. In particular, we consider three key questions: First, do the places that produce human capital during childhood also produce human capital during adulthood? Second, what are the characteristics of place that explain that relationship? Third, how are these patterns reflected in prices and in migration decisions?

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[69] In Section 7.1 below we explore the robustness of our results to alternate durations of labor market histories.

[70] For context, the results presented in tripartite hockey stick graphs showed the projection of these causal estimates on our observational measure of location quality net of labor market exposure, $\tilde{\omega}_{r\tilde{p}}$.

[71] This is particularly useful because the flat portion of the hockey stick graph in Figure 7 suggests that selection on destinations is present.

7.1 Estimating the Relationship Between Childhood and Adult Human Capital Accumulation

In order to answer these questions, we build toward an evaluation of childhood versus adult human capital effects across place. Figure 8 presents our findings. We begin by examining the relationship between adult human capital accumulation and aggregate upward mobility. (Consistent with our analysis above, aggregate upward mobility is measured using predicted wage outcomes in adulthood among children with parents at the 25th percentile of income.[72]) In the first bar on Figure 8, we regress upward mobility in each CZ on our adult human capital estimate for that location. We normalize these estimates so that they measure the effect of an additional year of exposure to place. We implement this regression with a split sample IV to account for measurement error in the construction of our adult human capital estimates. We find that for each 1 rank in adult human capital accumulation, aggregate upward mobility rises by 0.15 ranks. On average, places with higher levels of adult human capital accumulation have higher levels of upward mobility.

If it were the case that aggregate upward mobility were solely determined by childhood exposure, this would be our coefficient of interest. The positive sign of this coefficient would suggest that adult human capital accumulation and childhood human capital accumulation covary positively. We've established, however, that aggregate upward mobility also incorporates the impact of exposure to labor markets (alongside an effect of selection). With that in mind, we seek to account for these factors.

In the second bar of Figure 8 we regress upward mobility net of location wage premium on adult human capital accumulation. This causes a reduction in our coefficient from 0.15 to 0.09 ranks. It suggests that part of the reason why adult human capital accumulation is positively related to aggregate upward mobility is that places with high location wage premia also have higher levels of adult human capital accumulation. In the third bar we regress upward mobility net of both location wage premia and adult human capital exposure[73] on adult human capital accumulation. This, once again, causes a reduction in our coefficient from 0.09 to -0.11 ranks. In the fourth bar we estimate our coefficient of interest: the causal effect of place on childhood human capital exposure, CHC_l, regressed on the causal effect of adult human capital exposure, AHC_l. Here, we use the estimates from Section 6.2, which measure the causal effect of childhood environment on human capital development. We find a coefficient of -0.43 (.11). This means that each 1-rank increase

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[72]Consistent with the estimates of upward mobility in Section 6, this is estimated at age 28 and estimated among individuals who remain in the same commuting zone throughout their childhood. In other words, it is calculated on our sample of parental stayers rather than parental movers.

[73]As noted above, this expression, $w_i - \sum_t AHC_{l(l,\tilde{p},t)} - LM_l - HC_{\tilde{p}}^{par}$ can be re-written as $\sum_t CHC_{l,\tilde{p}} + HC_{l,\tilde{p}}^{select}$.

in earnings due to adult labor market exposure trades off with a 0.43 rank decrease in earnings due to local childhood environment. In order to facilitate an interpretation of these magnitudes we can do a net present value calculation that considers a tradeoff between parents and their children. As we outline in Appendix C, we consider a case where a parent raises their child while living in a location that produces high levels of human capital in adulthood. The parent benefits from the wage gains for their full working life. By contrast, their child earns lower wages as the result of the weaker childhood environment in which they were raised. If we discount back earnings at a 3% rate we find that each additional $1 in parental earnings trades off with ∼$0.43 in child earnings. This lines up very closely with our coefficient measuring the effect of yearly exposure.

While this serves as our primary specification, we also examine the robustness of our results across a wide range of specifications. In Appendix Figure 10 Panel A we examine whether our findings are sensitive to our method for calculating childhood human capital accumulation. We vary the control function, $X(p, t)$, to add individual year fixed effects and to include a full set of second order terms. We vary the number of years of adult labor market exposure, showing that the pattern remains consistent regardless of whether we adjust for labor market histories starting at ages 22, 23 or 24.[74] We also vary the ages of children over which we examine parental moves[75] and the age ranges over which one-time movers are defined.[76] In each of these cases, our basic finding of a negative relationship between CHC_l and AHC_l continues to hold.

In Appendix Figure 10 Panel B we examine how our specific estimates for location wage premia impact our results. We consider how our findings vary across demographic groups, separately estimating results for men versus women and for White versus Black young adults. In each of those cases our results remain similar. We split the sample by parental income and cannot reject that the effects are the same. (The tradeoff appears slightly stronger among those with below median parental income.) We also vary the age at which we calculate our location wage premia showing that our results do not change when using a location wage premia calculated exclusively among younger movers (ages 25-26).

Finally, in Appendix Figure 10 Panel C we consider the joint effect of varying our location wage premia

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[74] As expected, adjusting for longer labor market histories results in a more substantial estimate of the tradeoff because we are subtracting from our adult outcomes a measure that is highly correlated with adult human capital accumulation. That said, the relationship is not fully mechanical because the employment location of an individual at age 22-24 may differ from their ultimate location at age 28.

[75] In our primary specification we focus on moves that occur before a child turns age 22. We examine the robustness to alternate cutoffs such as moves before 24.

[76] In particular, in our primary sample we define one-time movers as parents who move once before their children are 24. In robustness analysis we adjust that to consider parents who only move once before their children are 28.

estimates and our adult human capital accumulation estimates. Once again, we examine our results across demographic groups and find no statistically different effect for Black versus White young adults or men versus women. We re-estimate our results using family income rather than individual income and find the same basic tradeoff. We also adjust our estimates of adult human capital accumulation to incorporate alternate controls such as calendar year fixed effects and see no discernible impact on our findings.

7.2 Identifying Characteristics of Place

Next, we look to identify the characteristics of place that help explain this tradeoff. Is there something about the nature of certain locations that make them good labor markets for the production of human capital but also inhibit human capital production during childhood?

In the conceptual framework outlined in Section 2 we outlined why high levels of adult human capital accumulation could theoretically be associated with either higher or lower levels of childhood human capital accumulation. In one scenario, we posited that adult human capital accumulation could be driven by strong agglomeration effects which themselves produce a spillover onto childhood environments. We noted potential mechanisms such as social connections to high-income individuals and increased local investment. In another scenario, we posited that strong agglomeration effects could produce negative spillovers onto childhood environments. We noted potential mechanisms such as greater levels of physical and social fragmentation that emerge in agglomeration economies.

In order to test these competing theories, we begin by examining the relationship between commuting zone size and the human capital tradeoff we observe. We have already established that increases in CZ size are associated with higher adult human capital production, potentially through an agglomeration channel. We find the opposite pattern when we examine the relationship between size and childhood human capital development. Figure 9 Panel A shows the distribution of childhood human capital estimates (measured inclusive of selection) relative to CZ size. The binned scatterplot shows a strong downward relationship across the vast majority of commuting zones. In particular, childhood human capital development is highest in the smallest commuting zones and declines until around the 50th largest CZ. At that point, the downward slope reverses and the relationship is weakly positive in the top-50 CZs. (Figure 9 Panel B shows this relationship in the top 50 largest CZs.) We repeat this exercise showing our estimates of childhood human capital effects purged of selection and we find that same relationship. Figure 10 Panel A shows a downward

relationship present in all but the 50 largest CZs, and Panel B shows the slight reversal in the largest cities.

We return in a moment to discuss the pattern in the largest CZs, but it is clear that, across the vast majority of commuting zones, adult human capital production is positively related to size and childhood human capital production is negatively related. We confirm this further in Appendix Figure 9 where we estimate the tradeoff in all but the 50 largest CZs. In that sample, we find that the decline in childhood human capital production is 0.63 ranks for each 1 rank increase in adult human capital production. (This is a meaningful increase relative to our baseline estimate of 0.43 ranks.)

Having established that the primary tradeoff we observe is associated with commuting zone size, we turn our attention to the characteristics of large versus small CZs that can explain this relationship. Here we test the hypothesis that the tradeoff occurs because agglomeration economies are associated with a greater degree of physical and social fragmentation. While do not have exogenous variation in CZ-level characteristics, we test whether these characteristics have the right "heat signature." We examine whether our CZ characteristics of interest vary with CZ size, have a significant relationship with the determinants of human capital, and have a strong analytical justification as to why they contribute to the tradeoff.

Using this basic approach, we see evidence that relationship between the human capital tradeoff and CZ size is associated with both the physical structure of cities and the nature of interaction therein. The evidence is consistent with the hypothesis that physical and social fragmentation may drive these patterns. In the context of physical structure, Appendix Figure 12 Panel A shows that income segregation increases consistently and strongly with CZ size. (Here, income segregation is measured using estimates of tract-level segregation from Chetty et al. (2014).) Appendix Figure 12 Panel B shows the prevalence of short commute times decreases rapidly with CZ size. (Here, we plot the fraction of individuals within commute times less than fifteen minutes against log CZ size.) In both of these cases, we can then examine the relationship with the place-based determinants of human capital. We do so by regressing our place-based determinant of human capital on these characteristics of place. Figure 11 shows that short commute times are strongly positively related to childhood human capital, and that income segregation is strongly negatively related to the same measure. For example, each 1 standard deviation increase in the fraction of individuals commuting less than fifteen minutes is associated with a 0.07 rank increase in yearly childhood human capital accumulation. There is no such connection between these CZ characteristics and adult human capital accumulation. Short

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commuting distances have a de minimus positive effect[77] and higher levels of income segregation have a positive relationship with adult human capital accumulation.[78] These patterns suggest that the physical structure of small CZs is associated with greater opportunities during childhood but weaker adult labor market opportunities. It suggests the opposite pattern for larger CZs.

We also find suggestive evidence that the nature of social interactions in cities contribute to the tradeoff we observe. For example, Appendix Figure 12 Panel C shows a strong negative relationship between log CZ size and the fraction individuals who are religious in each CZ. (We borrow this measure of social environment from Chetty et al. (2014). It measures the number of religious adherents in each place, which can be conceptualized as the set of people who take part in their local religious community.) We map this back to the place-based determinants of human capital and find a strong positive relationship with childhood human capital production but a negative relationship with adult human capital production.

These patterns regarding physical and social interaction help to explain the tradeoff across the vast majorities of CZs, but they don't yet explain the lack of a tradeoff in the largest CZs. When it comes to that reversal of the pattern, we find evidence consistent with positive spillovers from agglomeration economies onto childhood environments. In accordance with the initial theory outlined above, the results suggest that exposure to high-wage individuals and associated increases in local investment help to explain the positive covariance between childhood and adult human capital production.

Figure 12 Panel A plots the mean income rank of each CZ against log CZ size. There is a positive relationship across the full distribution and a positive relationship with the childhood human capital production as well. This suggests that the concentration of high wage individuals in large cities, and any associated spillover benefits on kids, may serve to swamp the negative effects associated with concentrated agglomeration economies. We can further explore this mechanism using a direct measure of social connectedness to high-wage individuals constructed in Chetty et al. (2022a). Their measure, known as economic connectedness, uses data on friend connections from Facebook to measure the extent to which low-income individuals have Facebook friend connections to high-income individuals. When we plot this connectedness measure across

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[77]While we do not have definitive evidence on this point, there are a number of conceptual reasons why short commute times may be associated with higher levels of childhood human capital development but not adult human capital development. Such commute times may have a direct causal effect on parental investment as parents are able to spend more time with children. They may also be associated with smaller, tighter-knit communities in which other community members make more direct investments in children.

[78]In Figure 11 we seek to focus on the determinants of the tradeoff, and so we exclude the top 50 largest CZs when we plot these relationships with the determinants of human capital. In Appendix Figure 11 we plot the relationship across all CZs and we find the same basic pattern. In fact, including the top 50 CZs results in a weakly negative relationship between adult human capital and short commute times, rather than the insignificantly positive effect found in our baseline estimates.

the distribution of CZs by size, we see a distinctive U-shaped pattern. It declines across a large fraction of CZs before increasing in the largest places. That U-shaped pattern matches the reversal in childhood human capital accumulation we see in the data. Moreover, economic connectedness has a positive relationship with our measure of childhood human capital accumulation, helping to further validate this explanation.[79] Taken together, this suggests that the presence of high wage individuals may have a positive effect on the production of childhood human capital and may help to explain the reversal pattern in the largest CZs.

We also see evidence that variation in local investment may help explain our results. In particular, we examine real per-pupil school spending.[80] When we plot rates of school spending across the distribution of CZs by size, we once again see a U-shaped pattern. Spending initially declines with CZ size but then reverses itself in the largest of places. As previously suggested, this may be the result of a larger tax base and higher levels of local investment in thriving large cities with successful labor markets and higher numbers of high-wage individuals. It is important to note, however, this does not mean that school spending is causally responsible for the increase in childhood human capital production in the largest places. Rather, it is the types of places that invest more in schools that also see better outcomes for children. It is those types of places that break from the dominant trend and manage to produce higher levels of adult human capital accumulation without sacrificing human capital accumulation during childhood.

7.3 Exploring Prices and Migration

Having explored the tradeoff across place between human capital accumulation in childhood versus adulthood, we conclude by examining how these effects are reflected in local prices and migration flows. If individuals are aware of this variation in outcomes across place, we should expect that to be reflected in prices and in migration decisions.

We begin with prices. We calculate a measure of local prices in each commuting zone using local rents paid by young adults aged 26-32. We calculate these rents using data from the ACS and, following Diamond and

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[79] We also validate this approach with our measure of exposure to high wage individuals. In particular, we consider individuals who are living in tracts where more than 50 percent of individuals are below the 25th percentile of family income. We then measure the fraction of all individuals within a given radius who are above the 50th or 75th percentile. We find that a higher fraction of high-income neighbors within 5 miles is positively associated with higher levels of childhood human capital development. This suggests cities that are better for child human capital production are cities with relatively proximate population of high wage individuals. But, it is important to note that this effect does not appear to be related linearly with proximity. We find that the effect is actually slightly weaker at a radius of 3 miles (and is essentially non-existent at 1 mile where there may be too little variation to identify an effect.)

[80] Here, we use National Center for Educational Statistics (NCES) yearly per-pupil school spending estimates, as used in Chetty et al. (2014). We convert these nominal measures to measures of real spending using our location wage premia estimates. (This allows us to make a wage adjustment across locations rather than simply using a price adjustment.) We scale our adjustment by the fraction of school expenditures spent on salaries and benefits, which the NCES estimates to be 80%.

Moretti 2021, we make a hedonic adjustment to account for cross-CZ differences in housing characteristics. We then regress our measure of CZ-level prices on each of the three place-based determinants of wages: location wage premia, adult human capital accumulation and childhood human capital accumulation. We estimate: $price_l = \beta_1 LM_l + \beta_2 CHC_l + \beta_3 AHC_l + \epsilon_l$. We implement this with a split sample IV in order to account for measurement error in our place-based determinants of wages. We also use a rank to dollar conversion so that all estimates are measured in dollars. We scale our human capital estimates to account for the fact that human capital earned in a given year serves to increase an individual's wages in all subsequent years in the labor force.[81] In particular, we do a simple net present value calculation that examines the discounted impact on long-term earnings associated with an additional year of childhood or adult human capital accumulation. Consequently, we scale our 1-year adult human capital estimates by a factor of ~ 22 and scale our 1-year childhood human capital estimates by a factor of ~ 17.[82] Here, LM_l simply measures the level effect of place on yearly wages, and so there is no exposure component to it.

The results presented in Figure 13 show that location wage premia are heavily capitalized into local prices while the human capital components of wages are not. In particular, our baseline regression shows that for each \$1 increase in location wage premia, prices rise by \$1.63. The presence of some capitalization effect is not surprising, as we would expect landlords to capture some of the benefits of location-specific wages (Moretti, 2011). Moreover, the large magnitude of this capitalization effect is consistent with previous evidence (Diamond and Moretti, 2021; Card et al., 2022).[83] It is important to note, however, that the magnitude of our estimate does not mean that location wage premia have a causal effect on prices that is more than 1-1. We are examining cross-sectional variation in location wage premia and it may be that such premia are correlated with amenities.[84] If it is the case that places with high levels of amenities also have

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[81]In other words, even if human capital were fully mobile and migration were costless, we should expect prices to capture the value of an additional year of access to a given labor market. If migration were not costless, then we might expect some fraction of historical human capital accumulation effects to be priced in as well.

[82]For the purposes of these calculation we assume that the individual's change in childhood human capital occurs when they are age 11. We assume that they enter the labor force 11 years later and then continue to work through age 65. For an individual who benefits from additional adult human capital accumulation we align with the age at which prices are measured and assume they gain that benefit at 28. They also continue in the labor force through age 65. In both cases benefits are discounted at 3%. For the sake of simplicity (and in order to be conservative) we assume that the effect of place is constant in dollar terms throughout the lifecycle rather than increasing with age. The adjustment factor would be larger if we accounted for lifecycle earnings growth.

[83]Diamond and Moretti (2021) find that wage differences across place conditional on observable characteristics are heavily capitalized into prices. In particular, they find they are fully capitalized among high-income earners and more than fully capitalized among low-income earners. As they note, if individuals sort into locations on the basis of unobservable wage characteristics (i.e., location wage premia are smaller than cross-sectional wage differences conditional on observables) then these estimates will understate the extent of capitalization. Card et al. (2022) examine how location wage premia vary with city size, and also examine how prices across place vary with city size. They find the elasticity of prices is as great or greater than the elasticity of location wage premia, depending on the income of the individual being evaluated. This once again suggests full or more than full capitalization.

[84]We are by no means the first to make this observation. Diamond and Moretti (2021) and Card et al. (2022) both discuss

location wage premia, we could see more than complete capitalization into prices. We test this hypothesis by examining our price regression among a subset of cities. In particular, we re-run analysis after dropping large cities, which are often regarded as high amenity locations. When we drop the 50 largest CZs from our regression, the coefficient falls to 0.96. When we drop the 100 largest CZs, the coefficient drops to 0.79. There are two implications from these results. First, the decline in our estimates suggests that correlation of high location wage premia with high levels of local amenities is driving our price effects in above 1. Second, the fact that the coefficient is still nearly 0.8 outside the top 100 cities suggests that location wage premia are heavily capitalized into prices. Even when we restrict our attention to places where disamenities may drive down our estimates, wage premia are still almost fully capitalized into prices.[85]

The results show far more limited capitalization of the human capital effect of place into prices. For example, the coefficient on our measure of adult human capital accumulation is 0.028 across all CZs. In other words, for individuals living and working in a given CZ at age 28, less than 3% of the wage gains due to their human capital accumulation is priced in.[86] And, when we consider that same regression excluding the top 50 or top 100 largest CZs, the effect falls below zero, suggesting any positive effect here may be the result of a spurious correlation with amenities. It is not entirely clear why such human capital effects are not capitalized into prices. One reason may be that individuals simply do not have knowledge of the human capital effects of place. If individuals living in a given city are unaware of its effect on human capital accumulation, their willingness-to-pay will not reflect such effects.

This same explanation regarding lack of information may also help explain our findings regarding the price of childhood human capital accumulation. We see no evidence that differences in place-based childhood human accumulation are reflected in prices. We find that childhood human capital accumulation has an insignificant but negative relationship with prices across the full sample of CZs. The effect becomes slightly more negative (and statistically significant) when excluding the 50 or 100 largest CZs. It could be that some of this negative relationship is the result of correlation with location amenities, but there is no evidence that childhood human capital effects are meaningfully capitalized into prices.

the role of amenities as a potential explanation for more than complete capitalization.

[85]In Appendix Figure 8 we also explore how these results vary with the age of price measurement. In particular, we repeat the exercise with rents measured at ages 32-37. In that case, we still find evidence of near complete capitalization, but the coefficients fall to 1.24 in the full sample and 0.74 when dropping the top 100 cities. It is hard to conduct an apples to apples comparison of these regression effects across ages because characteristics such as home ownership rate may vary. That said, these patterns appear to be driven by more limited age variation in rents as opposed to age variation in location wage premia.

[86]We can also rescale our estimates to examine what fraction of historical adult human capital accumulation is priced in at age 28. If we assume those individuals have 6 years of labor market experience, then only 11% of their historical adult human capital accumulation is priced in.

We can further explore these patterns by examining the migration patterns of young adults. In particular, we can see whether people move to locations which are better for certain place-based determinants of wages. In Figure 14, we consider a sample of individuals who move between ages 25 and 45. We plot the difference between their destination and their origin for each of our three variables of interest. We find that individuals in their mid 20s move to locations with higher location wage premia while individuals around 30 and older move to locations with lower location wage premia. For example, the average mover at age 25 ends up in a location with a location wage premia ∼0.2 ranks higher than their origin. By contrast, an average mover at age 40 ends up in a location ∼0.2 ranks lower than their origin. When we look at changes in adult capital accumulation we see that young movers switch to places with higher levels of human capital accumulation, while older movers switch to places with nearly identical levels of human capital production. As for childhood human capital accumulation, we find that movers at every age shift to locations that are worse for childhood human capital accumulation. Moves in the late 20s and early 30s are associated with comparatively smaller childhood human capital declines, but declines are present at all ages. We repeat this same analysis in Appendix Figure 14 examining moves relative to the birth of a first child and we find qualitatively similar patterns with sharper adjustments in the years right around the birth of a child.[87]

These patterns suggest that movers may be unaware of the impact of place on childhood human capital accumulation. We might expect individuals leaving young adulthood to migrate toward places with higher levels of childhood human capital accumulation rather than places with similar or lower levels of such human capital growth. That said, the evidence here once again reinforces that moves which improve human capital accumulation in adulthood ultimately trade off with human capital accumulation in the next generation.

8   Conclusion

We show that there is a tradeoff across place between the US commuting zones that produce human capital in childhood versus those that produce human capital in adulthood. In order to conduct that comparison we estimate three distinct determinants of wages: 1) location wage premia, 2) the impact of place on adult human capital accumulation and 3) the impact of place on childhood human capital accumulation. We estimate the first component by constructing AKM-style estimates using movers across place. We validate those estimates

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[87]We see declines in location wage premia are largest for moves immediately prior to the birth of the first child. While there is a slight rebound in subsequent years, the effect remains negative up the oldest child age in our sample, age 10. We find that moves immediately prior to birth are approximately neutral for childhood human capital accumulation but moves in all other years are associated with declines in childhood human capital accumulation.

using plausibly exogenous out-migration in response to Hurricane Katrina. We estimate the impact of place on adult human capital accumulation by examining the effect of experience in a commuting zone on the trajectories of an individual's wages. We address selection concerns by examining the wage trajectories of multi-time movers. We use our estimates to validate classic theories of agglomeration, showing that adult human capital accumulation rises in large, dense locations and in places with large shares of highly educated individuals. We estimate the impact of place on childhood human capital accumulation using age variation in the timing of parental movers. We show that this estimation strategy must account for correlated exposure to childhood environments and adult labor markets. We use our existing estimates on location wage premia and adult human capital accumulation to disentangle the impact of that correlated exposure.

We find that while aggregate upward mobility is positively related to adult human capital accumulation, childhood human capital accumulation trades off with adult human capital accumulation. The labor markets that produce strong agglomeration effects on average produce worse outcomes for children. We quantify this relationship and estimate that each 1 rank increase in adult human capital accumulation in a given place is associated with a 0.43 rank decline in childhood human capital accumulation.

We show that this tradeoff occurs throughout nearly the full distribution of CZs but disappears in the largest CZs. We present evidence for the characteristics of place that help explain this tradeoff. We show that the tradeoff is linked both to the physical structure of cities and the nature of the social interactions that occur there. It appears that living in a smaller place with a more physically and socially connected populous is conducive to human capital accumulation in childhood. It appears that the lack of a tradeoff in the largest cities may be linked to presence of a large number of high-income individuals. Those high-income locations provide more opportunities for social connections between high and low income residents, and they also have higher levels of local public investment. We regard this exploration of mechanisms as merely a starting point in understanding the drivers of the central tradeoff that we document.[88] We also present evidence that these human capital effects are not capitalized into prices (nor do they have a clearly observable impact on lifecycle migration decisions), suggesting that individuals may not be aware of these potential human capital gains.

Importantly, the absence of a tradeoff in the largest CZs suggests that producing a low level of childhood human accumulation is not an immutable feature of successful labor markets. It suggests instead that there

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[88]It is worth noting that our findings also have implications for local public finance. If there is a divergence between the places where human capital is accumulated in childhood and those places where it is accumulated in adulthood, it suggests a divergence between those places where potential tax revenue is generated and tax revenue is collected. This may inhibit further investment in children among locations with strong childhood environments but weak adult labor markets and high rates of out migration.

is room for cities to serve as thriving labor markets while also encouraging the production of human capital during childhood.

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FIGURE 1: Identification Strategy Visualization
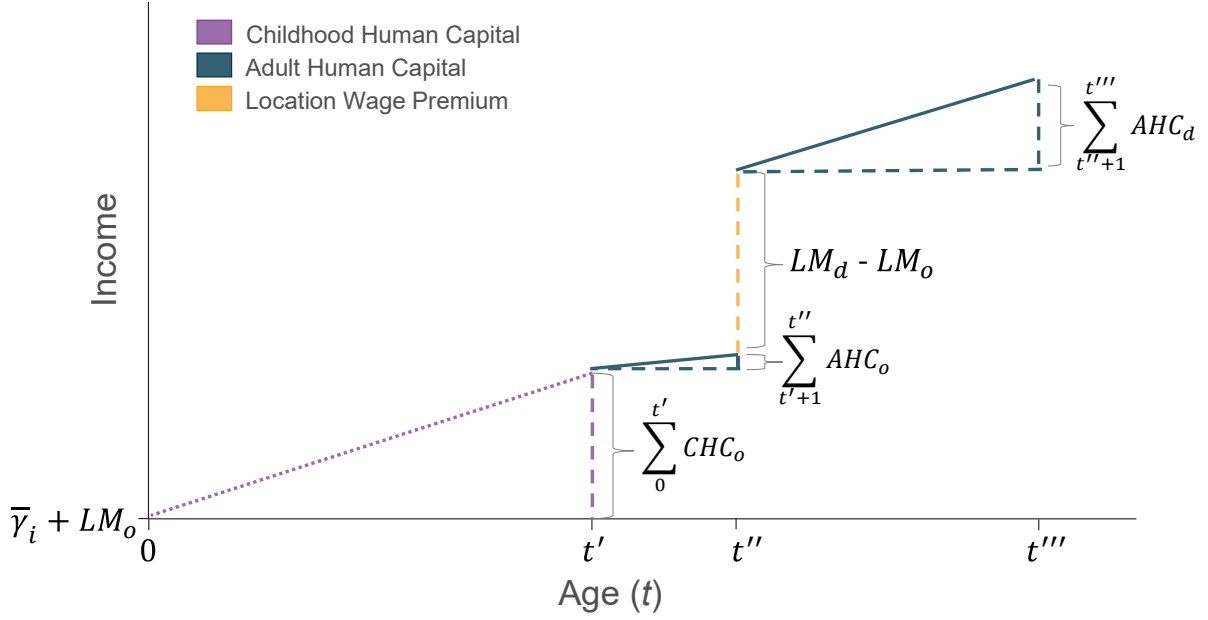
Notes: This figure provides a hypothetical wage trajectory that shows the impact of place on wages for an individual who is born in some location o, resides in that location through the early part of their working life, and then moves to a location d. An individual begins at income level $\bar{\gamma}_i + LM_o$. This corresponds to their average level of individual human capital, $\bar{\gamma}_i$, plus the location wage premium in their origin location, LM_o. That is the location wage premium they would receive if they were to remain in location o through adulthood. From time 0 through time t', the figure shows the impact of exposure to the individual's childhood location on their latent wages, $\sum_0^{t'} CHC_o$. At time t' the individual enters the workforce, and they remain in the workforce in location o until time t''. This labor market experience results in the accumulation adult human capital in that location, $\sum_{t'+1}^{t''} AHC_o$. At time t'' the individual moves to location d. Their wage increases by the difference in the location wage premia between their destination d and their origin o, $LM_d - LM_o$. From that period forward the individual continues to work in location d, accumulating the yearly adult human capital associated with residence in d, $\sum_{t''+1}^{t'''} AHC_d$ Here we remain agnostic about the units of income. In practice, when we estimate each of the components of wages shown on this graph, we do so using individual income ranks.

FIGURE 2: Location Wage Premia Across US Commuting Zones
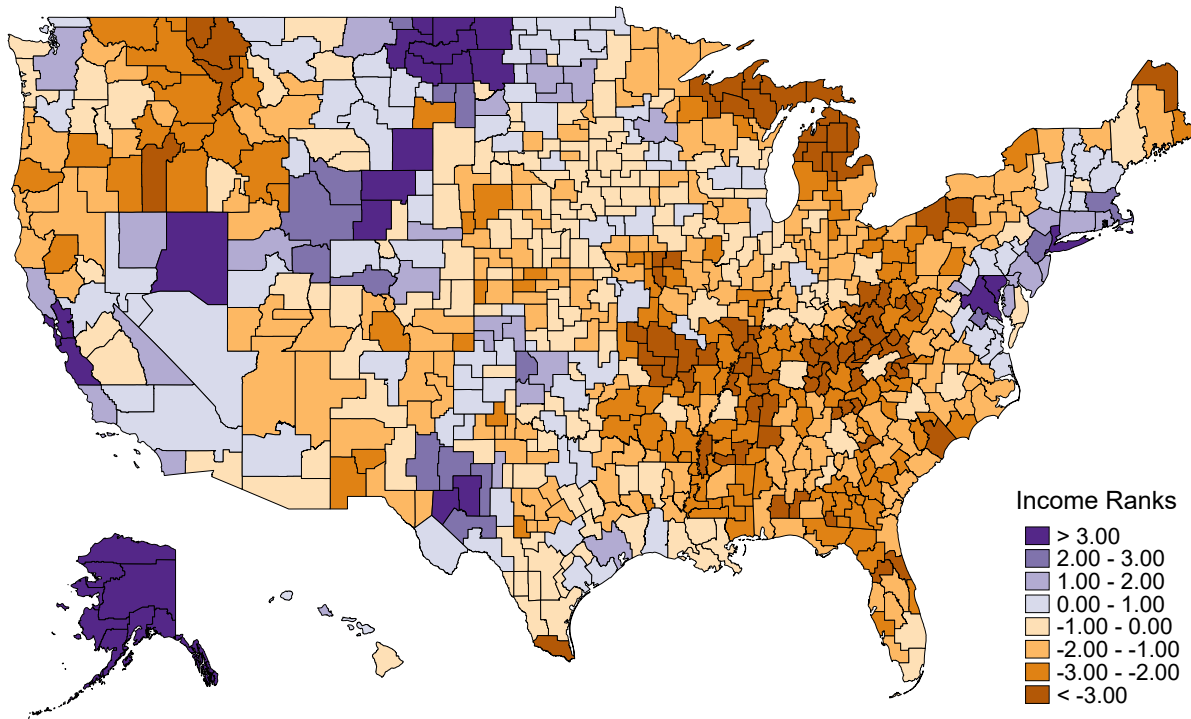
Notes: This figure presents estimates of the location wage premia in each US commuting zone. As outlined in Equation 3, these estimates are constructed using movers across CZs. They are constructed using a primary sample of individuals who are ages 35-44 in the years before their move. For privacy protection, the estimates here are subject to Empirical Bayes shrinkages. The method for shrinkage is outlined in Appendix A.

FIGURE 3: Selection Tests Using Out-Migration Due to Hurrican Katrina

A. Out-Migration from New Orleans
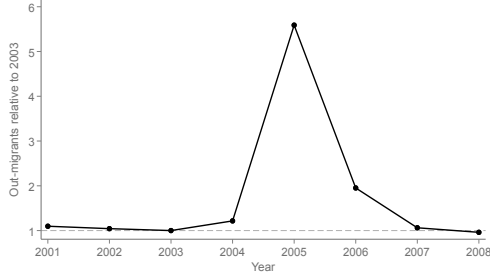
B. Binned Scatterplot Selection Test

Slope = 0.993 (0.134)
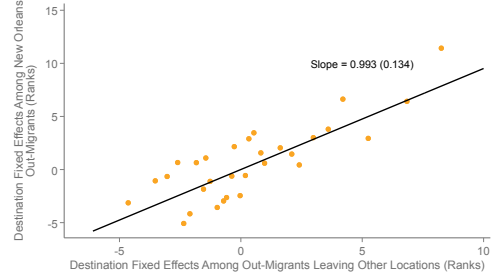
C. Event Study Selection Test
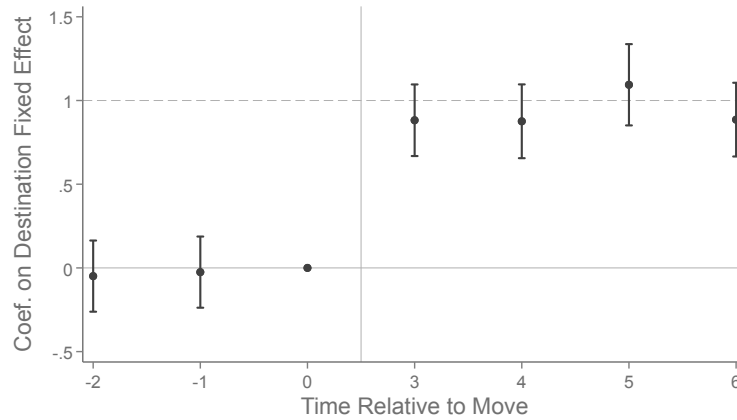
Notes: This figure presents evidence on our selection test from Section 4.2, which uses out-migration from New Orleans after Hurricane Katrina to test for selection in location wage premia estimates. Panel A plots the rate of out-migration from New Orleans in each year around the Hurricane Katrina. Panel B presents a binned scatterplot from a regression of location wage premia calculated among New Orleans out-migrants on location wage premia calculate among non-New Orleans out-migrants. The binned scatterplot corresponds to a simple OLS using precision (inverse-squared standard error weights.) We report the regression coefficient from the regression in the micro-data. Panel C presents the results of an event study where the year-by-year wages of out-migrants from New Orleans are regressed on the location wage premia estimated among non-New Orleans migrants. Here the fixed effects for non-New Orleans movers are allowed to vary based on the years relative to the move. This event study is implemented using split-sample IV to account for potential measurement error in the fixed effects among non-New Orleans movers.The regression is weighted using population count weights corresponding to the number of individuals arriving in the destination who left New Orleans. For both Panels B and C the sample is restricted to destinations to which at least 10 individuals in our sample arrive from New Orleans.

FIGURE 4: Average Adult Human Capital Accumulation by Years Exposure

A. Top 50 By CZ Population

B. Top 50 By College Share

C. Rural CZs
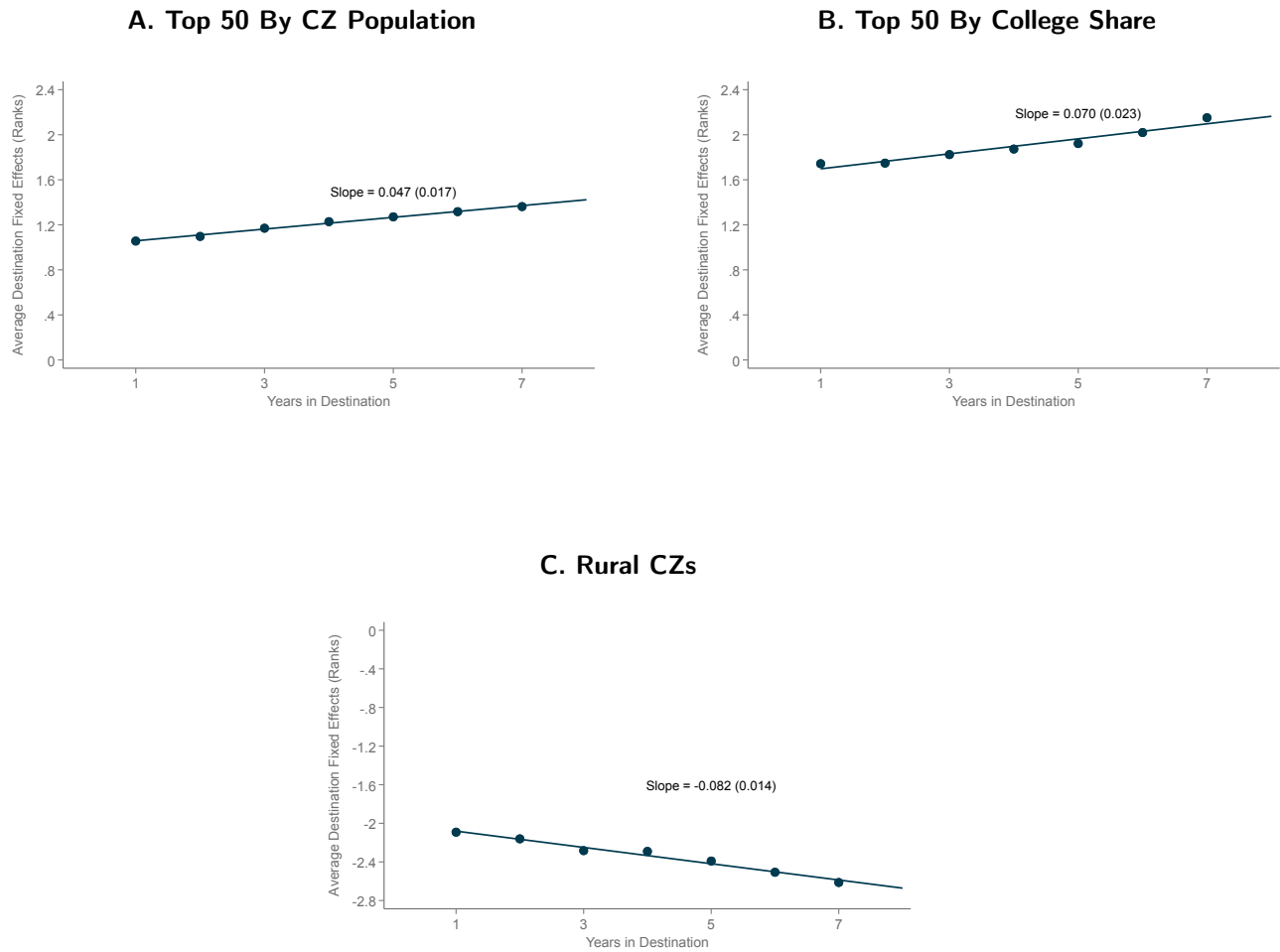
Notes: This figure presents evidence on adult human capital accumulation across place using variation in location fixed effects measured up to 7 years after a move. Panel A plots the average year-by-year fixed effects for the 50 largest CZs. Panel A also plots the slope corresponding to the average linear effect of exposure to the top 50 largest CZs. We report the regression coefficient from the regression in the micro-data. Panel B repeats the same exercise for the top 50 cities as measured by their share of college graduates. Panel C repeats the same exercise among rural CZs. We derive our measure of urban versus rural from Chetty et al. (2014). Both the individual fixed effects and the slopes are weighted based on the number of individuals in the sample moving to each destination.

FIGURE 5: Multi-Time Mover Selection Test

A. Predicted Location Wage Premia

Slope = 0.984 (0.026)

B. Predicted Human Capital Accumulation

Slope = 1.107 (0.076)
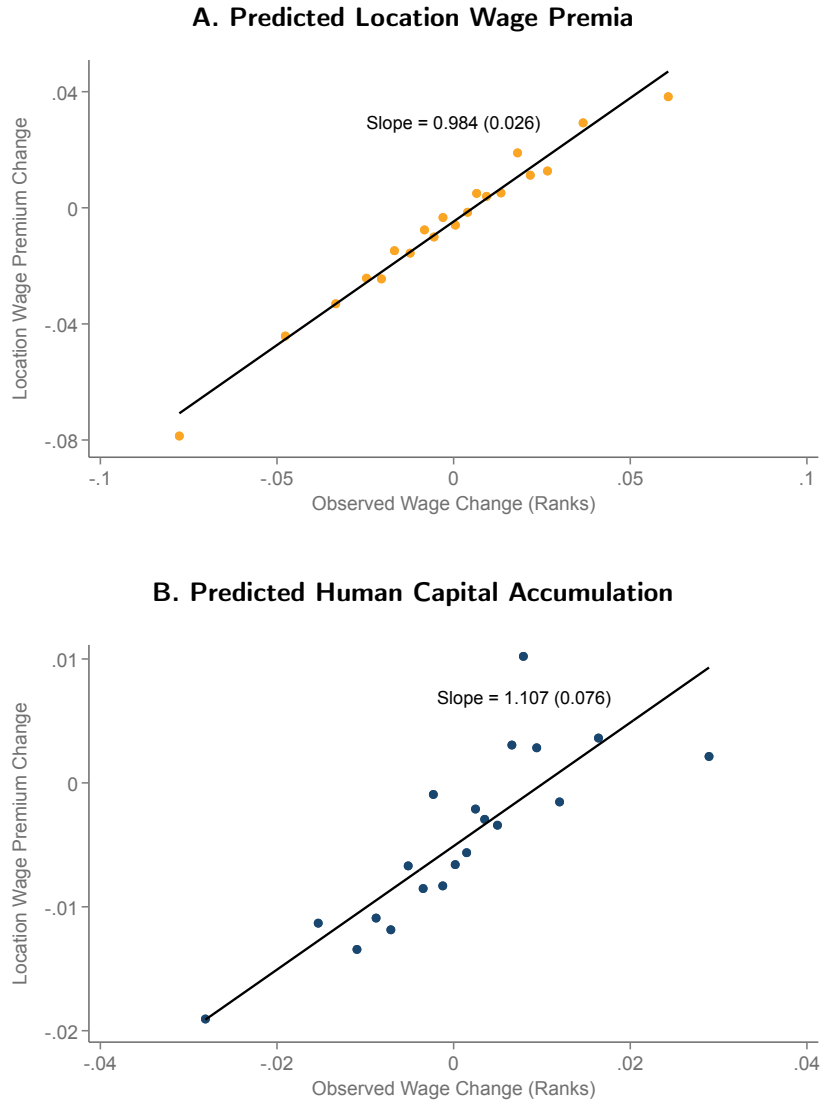
Notes: This figure reports the results of our multi-mover selection tests, as outlined in Section 5.2. Both Panels A and B show binned scatterplots that report the results of difference coefficients in the same primary regression. We regress observed 7-year wage changes of multi-time movers. The first year of income is our first full year of income measurement after their initial move. Their second year of income measurement is 7 years later, which is after the second move. We regress this wage change on our prediction of the individual's change in location wage premia, . $\beta_{d'} - \beta_d$, and change in adult human capital accumulation, $t'\alpha_{d'} - (7 - t')\alpha_d$. Both predictions are constructed using the observed location decisions of the multi-time movers but predicted values formed based on a sample of one-time movers who remained in their destination for 7 years. Panel A shows the binned scatter plot corresponding to our coefficient on the location wage premia and Panel B shows the binned scatterplot corresponding to the coefficient on adult human capital accumulation. Both panels report the regression coefficients from the regression in the micro-data.

FIGURE 6: Adult Human Capital Accumulation by CZ Characteristics
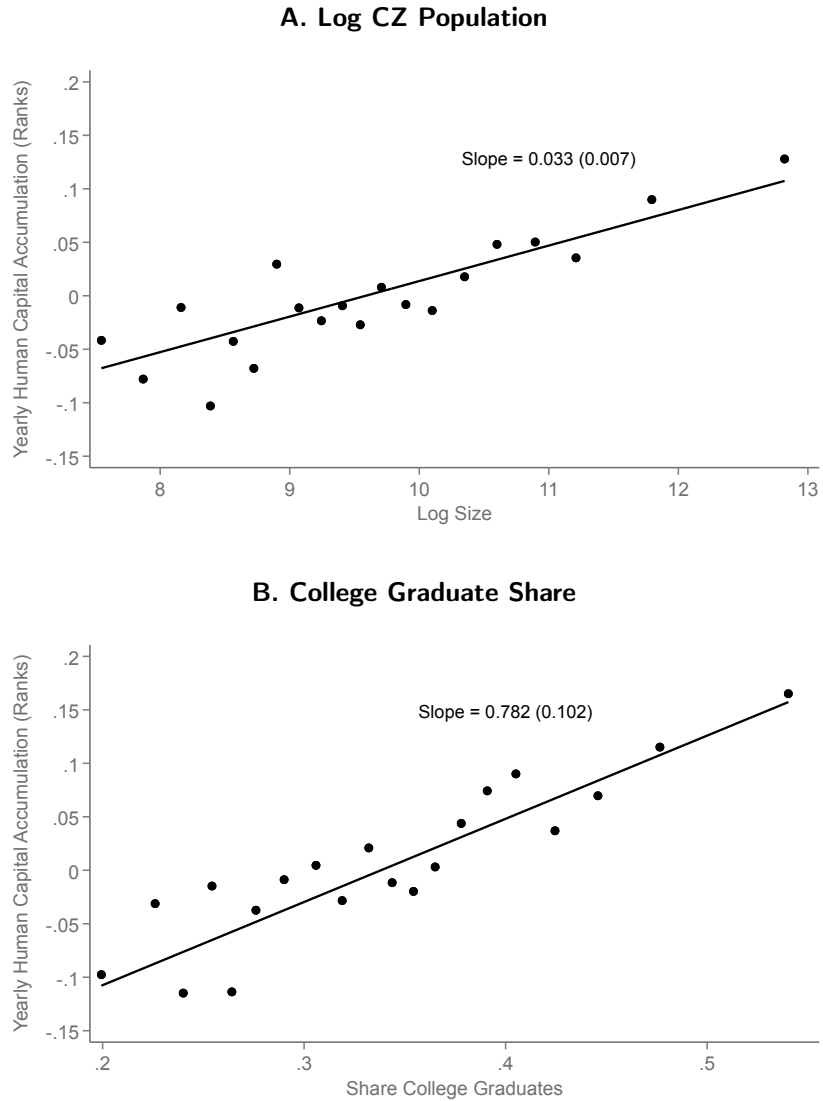
A. Log CZ Population

Slope = 0.033 (0.007)

B. College Graduate Share

Slope = 0.782 (0.102)

Notes: This figure plots the relationship between our estimates of adult human capital accumulation across place and characteristics of commuting zones. We measure adult human capital accumulation as the impact of an additional year of exposure to a place on an individual's wage ranks.. Panel A shows a binned scatterplot comparing our adult human capital accumulation estimates and log CZ size. Panel B shows the relationship with the share of individuals in a CZ who are college graduates. Both panels report the regression coefficients from the regression in the micro-data. For the sake of consistency with subsequent estimates, both panels are restricted the 641 largest CZs, dropping the 100 smallest CZs from the sample. These figures are unweighted across all CZs, but the binned scatter plots corresponding to the weighted relationship can be found in Appendix Figure 8.

FIGURE 7: Childhood Environment Exposure Estimates
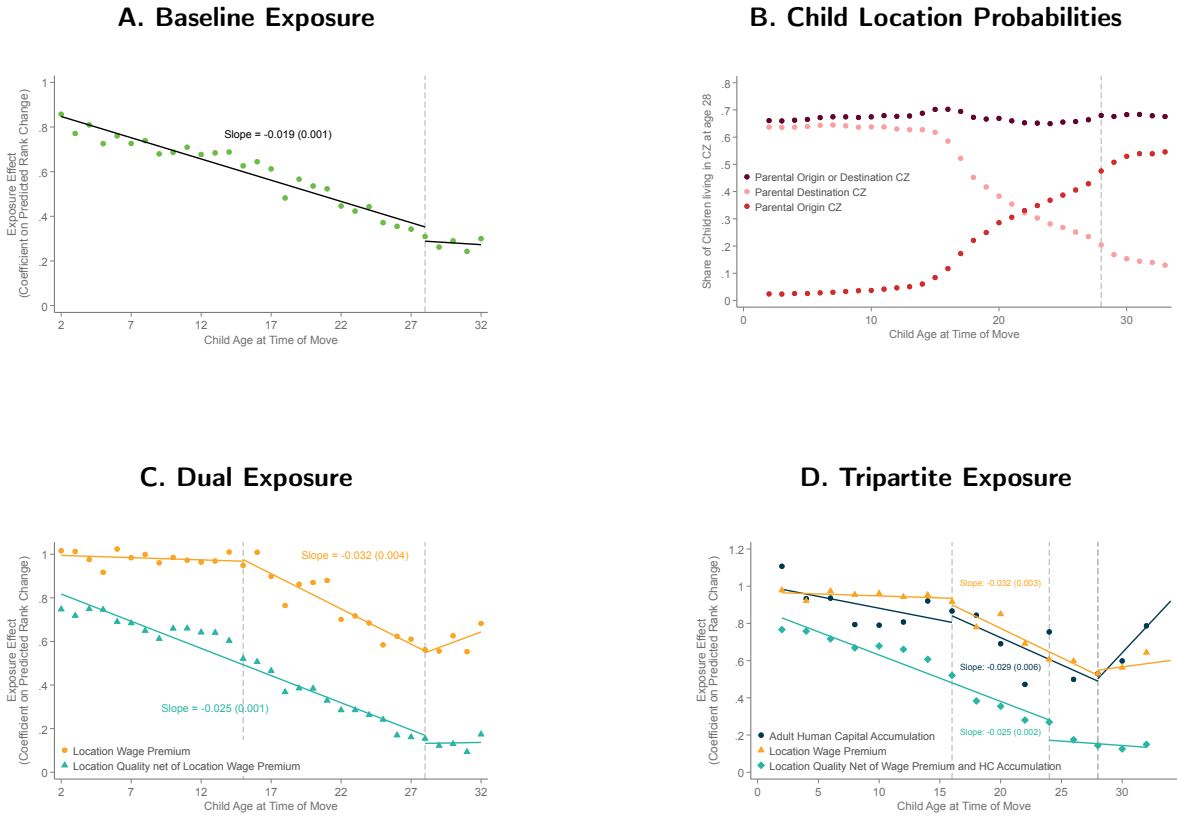
A. Baseline Exposure

B. Child Location Probabilities

C. Dual Exposure

D. Tripartite Exposure

Notes: This figure presents evidence on childhood human capital accumulation across place using variation in child age at the time of parental moves. Panel A plots the results of the regression outlined in Equation 7. This regression splits movers by their age at the time of parental moves. Individual's wage outcomes at age 28 are regressed on the change in the location quality due to the move. Here, location quality is measured using average upward mobility among children who spent their entire childhood in each location. This is the typical "hockey stick" graph from Chetty and Hendren 2018a. Panel B reports the adult locations (measured at age 28) of children whose parents moved once during their childhood. It separates out movers by their age at the time of move and plots the probability that they are reside in the origin location from which they moved and the destination location to which they arrived. Panel C modifies the regression in Panel A to examine the dual effects of exposure to differences location wage premia across locations and differences in location quality net of location wage premia. This is outlined in Equation 8. Panel D modifies the regression in Panel C to examine the tripartite effects of exposure to differences in location wage premia across locations, differences in adult human capital accumulation across locations and differences in location quality net of both location wage premia and adult human capital accumulation. This corresponds to the estimation outlined in Equation 9.

FIGURE 8: Relationship Between Place-Specific Determinants of Wages
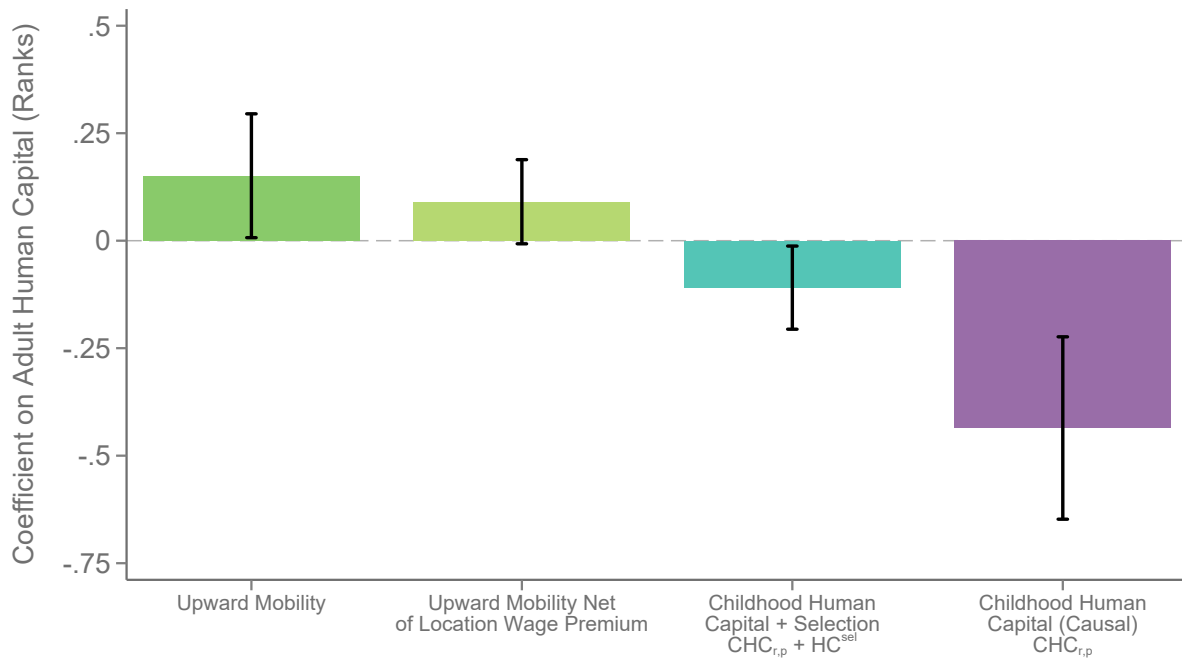
Notes: This figure plots the relationship between our place-specific determinants of wages. The first bar reports the results of a regression of aggregate upward mobility in each location on the impact of place on adult human capital accumulation. Upward mobility is calculated on a sample of individuals who remained in one CZ during childhood. Wage outcomes are measured at age 28. Upward mobility is measured at the 25th percentile of parental income. Both effects are normalized to measure the effect of one additional year of childhood exposure. For upward mobility, this means dividing the aggregate effect of place by 22 to correspond to 22 year of exposure. In each of the four bars, we use a split sample IV to account for potential measurement error in our measures of adult human capital exposure. In each of the first three bars, the observations are weighted using population counts that correspond to the number of individuals in the sample raised in the corresponding CZ. In the fourth bar the observations are weighted using precision weights (inverse standard error weights) estimated using the standard errors constructed when estimating the causal effect of place on childhood human capital. The estimates are constructed using a stacked regression across both splits of the data and so standard errors are clustered at the CZ level. The second bar reports the results of a regression of upward mobility net of location wage premia on adult human capital accumulation. Upward mobility is calculated by starting with the same sample of CZ stayers during childhood. Wage outcomes for both children and adults are adjusted to account for the location where they are observed at the time income is measured. Upward mobility is then recalculated on that sample of individuals and recalculated at the 25th percentile of percentile ability. (We refer to this as parental ability rather than parental income because it is adjusted for location wage premia.)The third bar reports the results of a regression of upward mobility net of location wage premia and adult human capital accumulation. The final bar reports the results of a regression of our estimates of the causal effect of place on childhood human capital accumulation on our estimates of the causal impact of place on adult human capital accumulation.

FIGURE 9: Childhood Human Capital (Plus Selection) and CZ Size
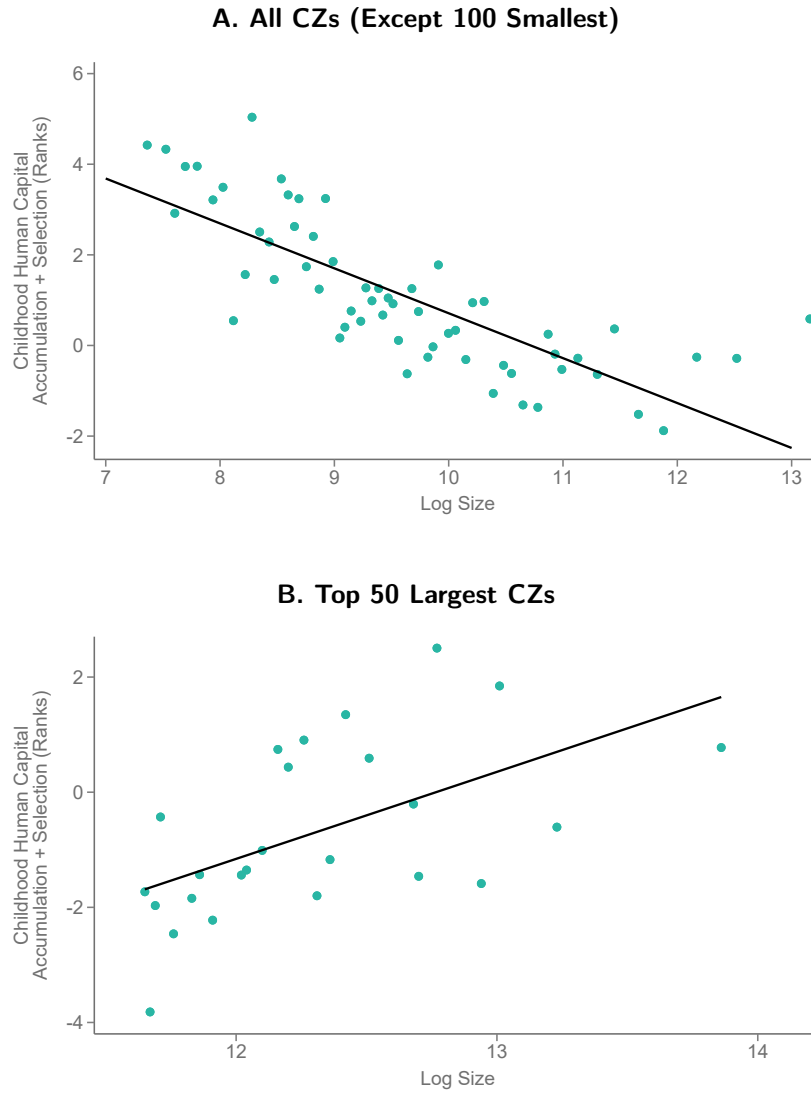
A. All CZs (Except 100 Smallest)

B. Top 50 Largest CZs

Notes: This figure plots a binned scatterplot of the impact of place on childhood human capital production against log CZ size. Panel A plots that relationship across all CZs except the 100 smallest. We omit the 100 smallest because in the analysis to follow in Figure 9, we place a restriction on the minimum cell size of 20 for the construction of our causal estimates. This causes the omission of nearly all CZs in the very bottom of the distribution and so we place a cutoff at the 641st largest CZ. Panel B plots the relationship within the top 50 largest CZs. In both cases our measure of childhood human capital production here also includes potential selection, $CHC_{r,p} + HC_{r,p}^{select}$. We construct that my calculating upward mobility in each location net of the labor market exposure effects of place.

FIGURE 10: Childhood Human Capital (Causal Estimates) and CZ Size
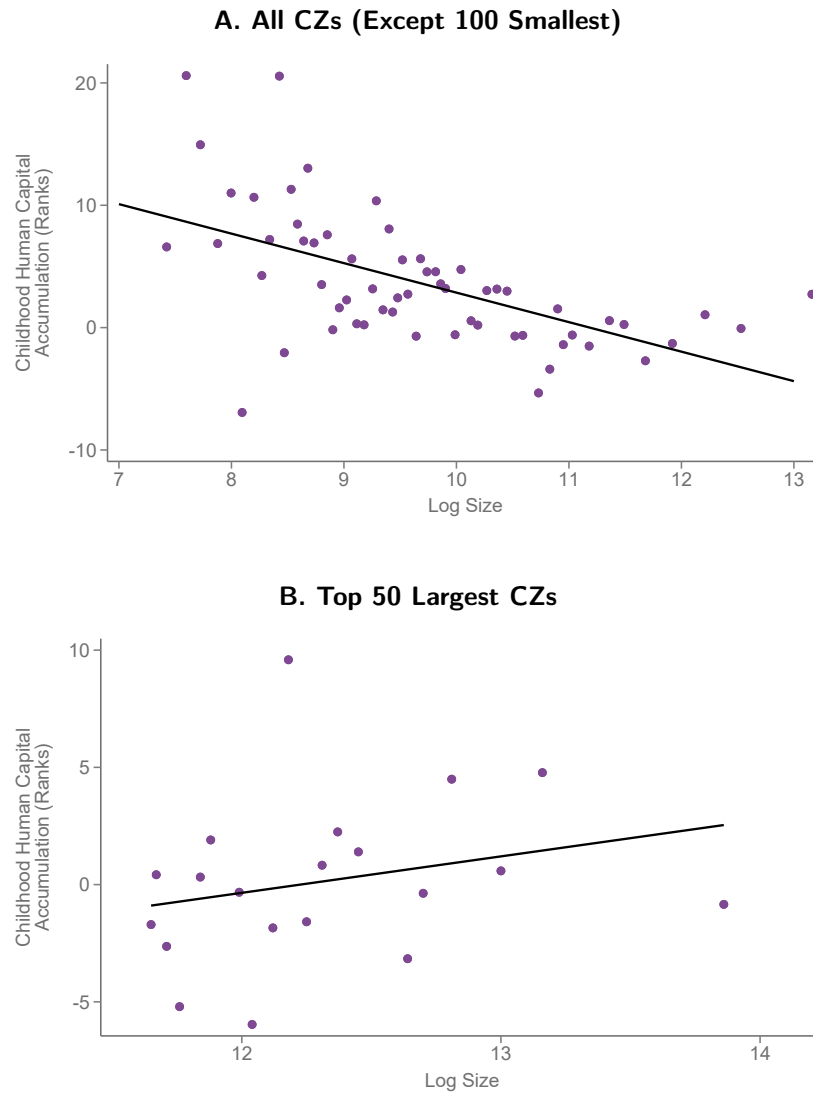
A. All CZs (Except 100 Smallest)

B. Top 50 Largest CZs

Notes: This figure plots a binned scatterplot of the impact of place on childhood human capital production against log CZ size. Panel A plots that relationship across all CZs except the 100 smallest. We omit the 100 smallest because in our analysis we place a restriction on the minimum cell size of 20 for the construction of our causal estimates. This causes the omission of nearly all CZs in the very bottom of the distribution and so we place a cutoff at the 641st largest CZ. Panel B plots the relationship within the top 50 largest CZs. In both cases our measure of childhood human capital production is our causal estimate constructed using age variation in the timing of parental moves. Our original causal estimates correspond to the impact of 1 year of childhood exposure. Here we plot the impact of 22 years of exposure.

FIGURE 11: Relationship between CZs Characteristics and Primary Tradeoff
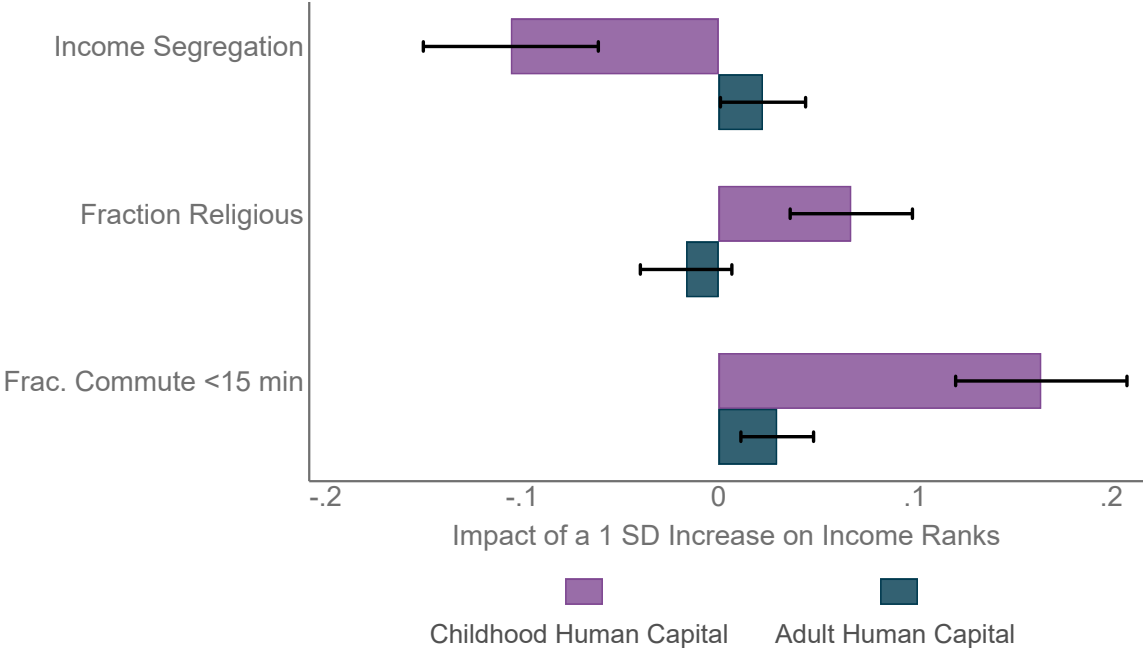
Notes: This figure the results of regressions of our place-based determinants of human capital on three CZ characteristics: income segregation, fraction religious, and fraction with a commute less than 15 minutes. The CZ characteristics are normalized by their respective standard deviations. All human capital effects are normalized to capture the effect of one year of additional exposure to place. Reported regression coefficients are constructed using regressions on the micro-data. Regressions evaluating childhood human capital accumulation are weighted using precision (inverse squared standard error weights) based on the standard errors associated with our causal human capital estimates. Regressions evaluating adult human capital accumulation are weighted using count weights corresponding to the number of individuals in our sample originating in the respective CZ. Consistent with the evidence on the tradeoff by CZ size, these results are reported excluding the 100 smallest CZs. The corresponding regression results that also exclude the 50 largest CZs can be found in Appendix Figure 9.

FIGURE 12: Relationship between CZs Characteristics and Tradeoff Reversal

A. Mean Parent Income Rank by CZ Size
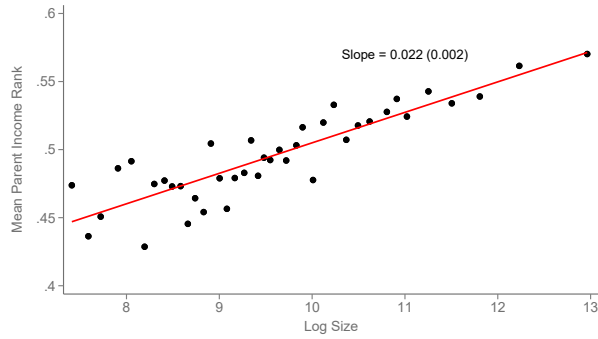
B. Proportion College Grads by CZ Size
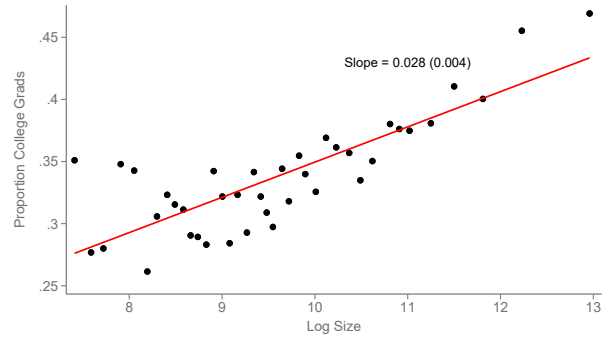
C. Per-Pupil School Spending by CZ Size
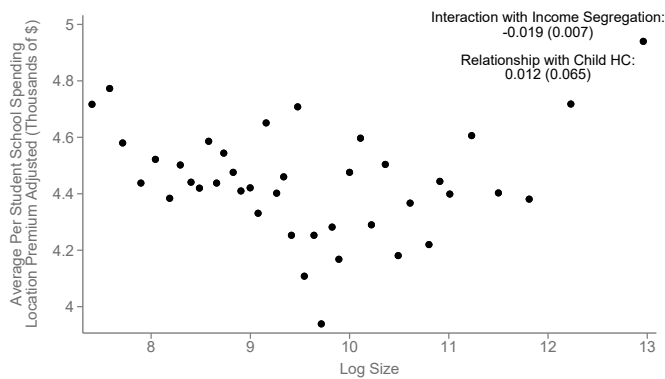
D. Economic Connectedness by CZ Size
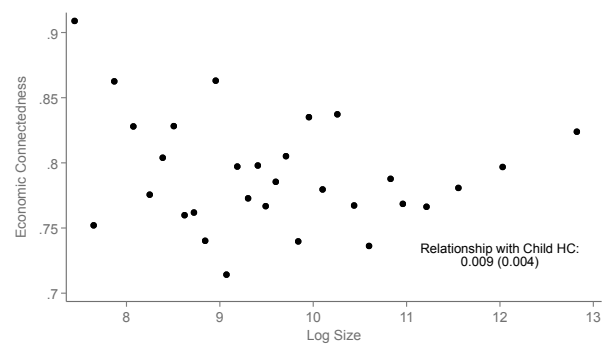
Notes: Panel A of this figure shows the relationship between mean parent income rank and log CZ size. Plot B plots the relationship between the proportion of CZ residents that are college graduates and log CZ size. Panel C shows the relationship between per-pupil school spending and log CZ size. School spending estimates are derived from Chetty et al. (2014), which uses information from the NCES. We convert these to real expenditures using our location wage premia estimates. We only apply that adjustment to 80% of school expenditures, consistent with NCES estimates that 80% of school expenditures are directed toward wage costs. Reported regression coefficients are constructed using regressions on the micro-data. Panel D plots the relationship between economic connectedness, as measured in Chetty et al. 2022a;b, and log CZ size. When reporting a relationship with human capital, that is the coefficient of a regression of the impact of place on childhood human capital production on our covariate of interest. Our covariates normalized (divided) by their own standard deviation so that the coefficient reports the rank increase associated with a 1-standard deviation increase in the covariate. Regressions evaluating childhood human capital accumulation are weighted using precision (inverse squared standard error weights) based on the standard errors associated with our causal human capital estimates.

FIGURE 13: Local Prices and Place-Based Determinants of Wages
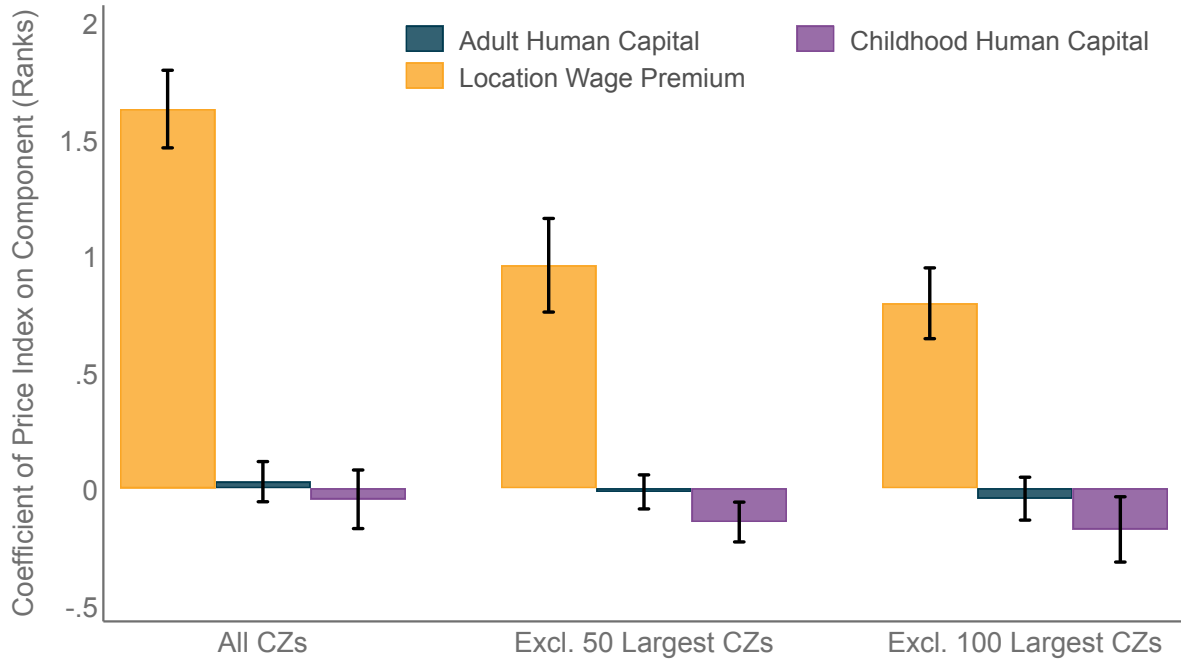
Notes: This figure plots the relationship between local prices and place-based determinants of wages. We measure local prices using local rents from the ACS. In particular we calculate rents for individuals aged 26-32 and perform a hedonic adjustment to account of unit characteristics following the approach in Diamond and Moretti (2021). We use a rank-to-dollar conversion so all calculations are done in dollar terms. In order to capture the total wage gains associated with a year of exposure to childhood or adult environments, we use a simple net present calculation to calculate the value of earning an additional dollar in all subsequent years of labor force participation. This results in a scaling of our adult human capital effects by a factor of approximately 23 and our childhood human capital effects by approximately 17. We then regress our measure of local prices on each of the three place-based determinants of wages. We use a split sample IV to account for potential measurement error in our estimates.

FIGURE 14: Change in Place-Specific Wage Characteristics by Age at Move
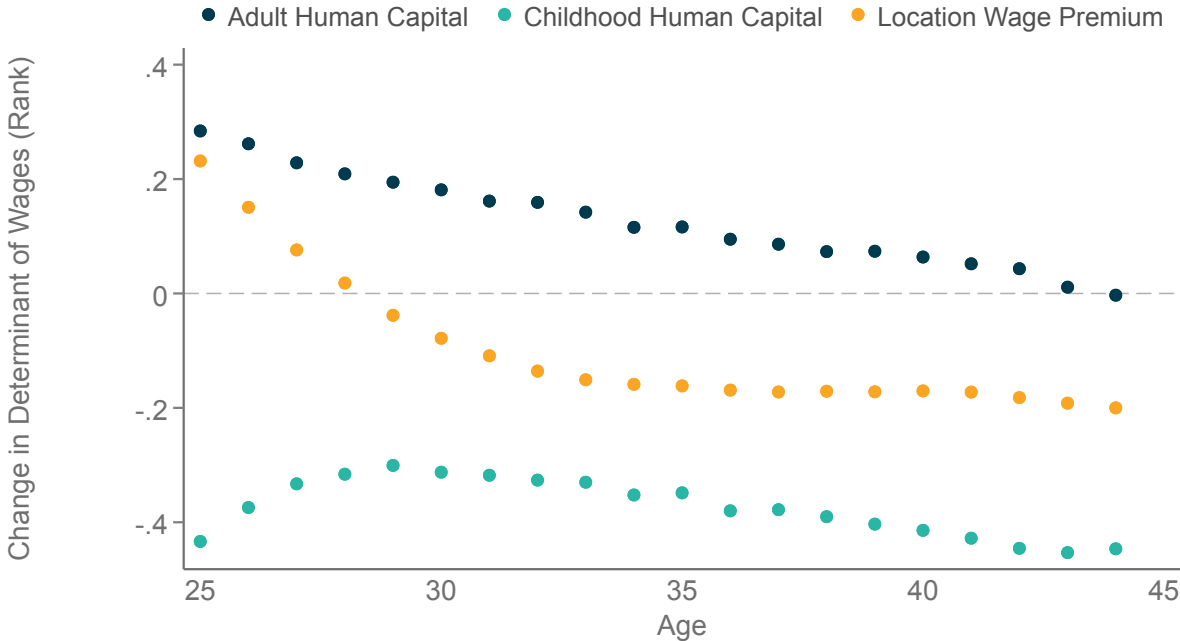
Notes: This figure plots individuals by the age at which they moved and examines the change in place-based wage determinants associated with their moves. The first series shows the change in the location wage premia between their origin and their destination. The second series shows the change in adult human capital accumulation and the third series shows the change in childhood human capital accumulation. Just as in Figure 13, these exposure effects are normalized to capture the net present value of an additional year of exposure in a given location.

Table 1: Symmetry Tests

Panel A: Symmetry Tests, Alternate Income

Income Measure:	Rank			Ranks > 20	Log	Log, Income > $15k	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D on O	0.985	1.026	0.931	1.011	0.990	1.149	1.145
s.e.	0.018	0.018	0.010	0.012	0.018	0.015	0.013
O on D	1.010	0.910	0.996	0.954	0.955	0.794	
s.e.	0.013	0.017	0.011	0.011	0.017	0.010	
Weight	Count	SE	Count	Count	Count	Count	Count
Sample	Aligned	Aligned	Full	Aligned	Aligned	Aligned	Full

Panel B: Variance Comparisons

Variance of Fixed Effects	Corrected Variance (KSS 2020)	Estab. Movers on CZ Movers
0.000375	0.0003725	0.8564

Notes: This table reports additional tests corresponding to our symmetry tests in Section 4. First it shows how our baseline symmetry tests differ when adjusting the weighting used in our symmetry regressions. We show the results using both count weights, indicated by Count, and using precision-weights (inverse square of standard error weights) , indicated by SE. The difference between our aligned and full sample corresponds to our previous symmetry tests where origin and fixed effects are measured in identical calendar years in the aligned sample. The table also reports results from the symmetry test among a subset of individuals with income ranks above the 20th percentile both before and after their move. It also reports the results of the same test using log income (and consequently excluding individuals with 0 income) and when using log income among a population of individuals with annual income levels above $15,000 both before and after their move. The bottom of the table reports the variance of our baseline fixed effect estimates and shows the variance using the correction from Kline et al. (2020). It also shows the regression coefficient when we regress our baseline estimates on estimates constructed as the mean of establishment-level fixed effects aggregated up to the CZ level. This establishment level aggregation is the technique used in Card et al. (2023).

Table 2: Location Wage Premia

	Demographics (Location FE)				Age 25-26 (Orig and Dest FE)		Above on Below Median Parental Income (Location FE)	
	Dep: Black, Ind: White	Dep: White, Ind: Black	Dep: Female, Ind: Male	Dep: Male, Ind: Female	First Moves	Non-First Moves	Age 25-26	Age 33-34
Location FE	0.749	0.852	0.811	0.953			1.326	1.164
s.e.	0.049	0.025	0.019	0.022			0.071	0.055
Dep: Dest Ind: Orig					1.066	0.738		
s.e.					0.164	0.054		
Dep: Orig Ind: Dest					0.419	0.826		
s.e.					0.040	0.055		

Notes: The first part of this table shows the relationship between our location wage premia estimates across demographic groups. We calculate the location wage premia among men versus women and among white versus Black movers. This table shows the regression coefficient when male location wage premia are regressed on our female location wage premia and vice versa. It does the same for location wage premia among white versus Black individuals. The table then focuses on location wage premia among individuals ages 25-26 in the year before their move and regresses origin on destination fixed effects and vice versa for individuals who are classified as making a first move or non-first move. Individuals are classified as making a first move here if they are moving away from the original location that they were observed in when they were linked to their parents (around age 16.) They are making a non-first move if they are moving from any other location. Finally, the table focuses on the location wage premia for individuals with above versus below parental income. It regresses those sets of fixed effects upon one another for individuals who are ages 25-26 and 33-34 in the years before their move.

Table 3: Multi-Mover Wage Prediction Test

Exposure Measure:	Primary		Alt Exposure		Yearly Predicted Income	Yearly Predicted Income, No ACH
	(1)	(2)	(3)	(4)	(5)	(6)
Predicted Δ Wage Premium	0.708	0.984	0.716	1.005		
s.e.	0.016	0.026	0.015	0.024		
Predicted ACH	0.744	1.107	0.747	1.108		
s.e.	0.068	0.076	0.067	0.076		
Predicted Origin Wage Premium	-0.068	-0.153	-0.072	-0.162		
s.e.	0.014	0.024	0.014	0.023		
Predicted Family Income					1.030	0.191
s.e.					0.055	0.057
Ages	25-34	35-44	25-34	35-44	35-44	35-44

Notes: This table provides supplemental evidence on our multi-mover tests. Column (1) corresponds to our baseline multi-mover tests as outlined in Section 4.2. Column (2) corresponds to the same tests estimated among younger individuals who are ages 25-34 in the years before their move. Columns (3) and (4) correspond to estimates that make a slightly different assumption regarding exposure over time. In particular, they assume that an individual get the adult human capital accumulation of their destination location for the full year in which they are observed there, rather than our baseline assumption that they receive 6 months of adult human capital accumulation in each location. Columns (5) and (6) show the results of a slightly modified prediction problem where we predict year by year income among multi-time movers using the location wage premia and adult human capital accumulation among long-term stayers. In both cases, wages are estimated here using AGI and locations are exclusively measured using the form 1040 in order to extend the sample. This analysis also includes year-by-cz fixed effects to control for time trends. Column (5) shows the regression coefficient on yearly predicted income where predictions are formed using both location wage premia and adult human capital accumulation. Column (6) shows the regression coefficient on yearly predicted income if adult human capital accumulation is excluded from that analysis.

APPENDIX FIGURE 1: Location Wage Premia Symmetry Tests

A. Aligned Sample
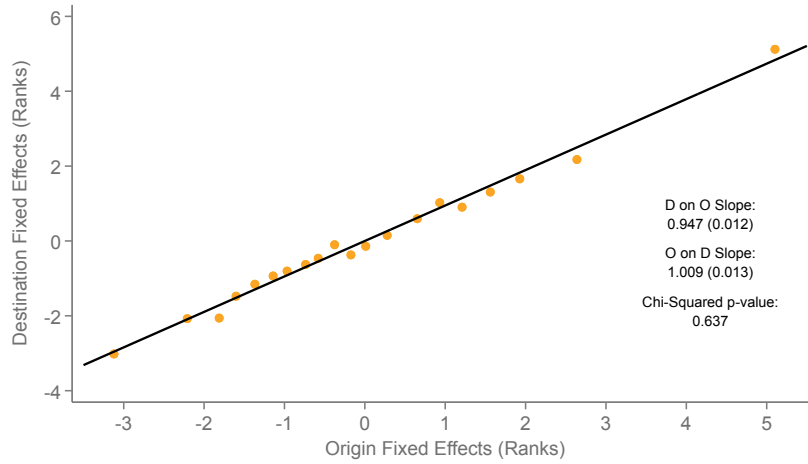
B. Full Sample
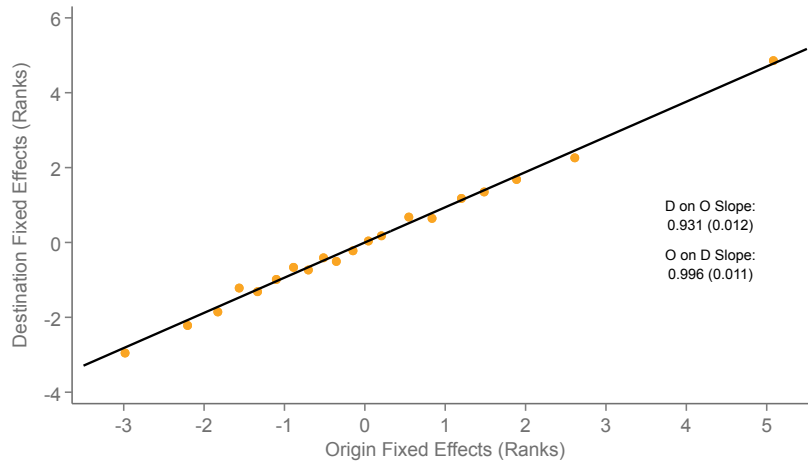
Notes: This figure conducts the location wage premia symmetry tests outlined in Section 4.2. In each CZ, an origin fixed effect and a destination fixed effect are calculated in order to estimate the location wage premia in those CZs. Both Panels A and B showed the binned scatterplot of the destination fixed effect regressed on the origin fixed effect. The panels also report the regression coefficient from the micro-data corresponding to the regression of the destination fixed effect on the origin fixed effect and the regression of the origin fixed effect on the destination fixed effects. Both sets of estimates are constructed on a sample of individuals who are ages 35-44 in the years before their move. Panels A is constructed using an "aligned sample" where the origin fixed effects and destination fixed effects are measured in the same calendar years, 2012. This alignment is done by estimating those effects on two distinct samples of movers. This panel also reports the p-value from a Chi-squared test of equality across the coefficients. Panel B shows the symmetry tests for a full sample of individuals who move between 2010-2016.

APPENDIX FIGURE 2: Location Wage Premia by CZ Characteristics
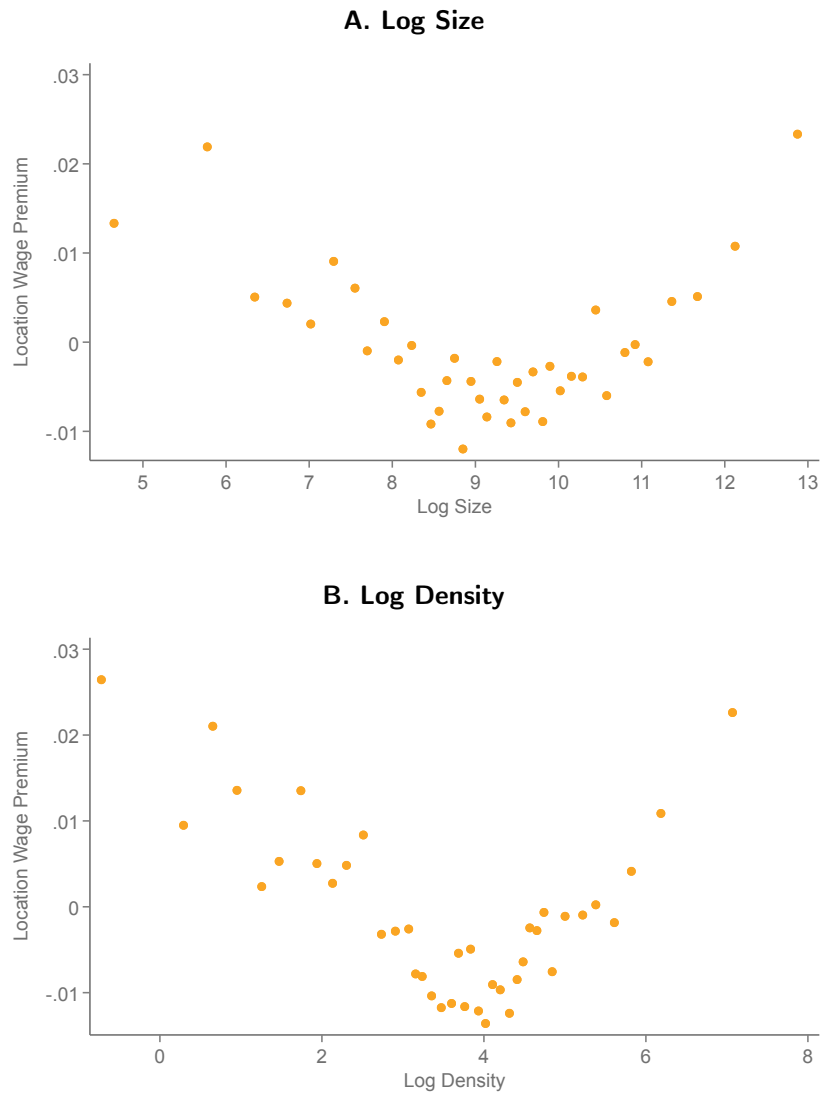
A. Log Size

B. Log Density

Notes: This figure plots the relationship between our estimates of location wage premia and CZ-level characteristics. Panel A presents a binned scatterplot that plots location wage premia against log CZ size.Panel B plots location wage premia against log CZ density, where density is measured as the density of the densest county in the CZ.

APPENDIX FIGURE 3: Roy Sorting By Age

A. Similar CZ Moves
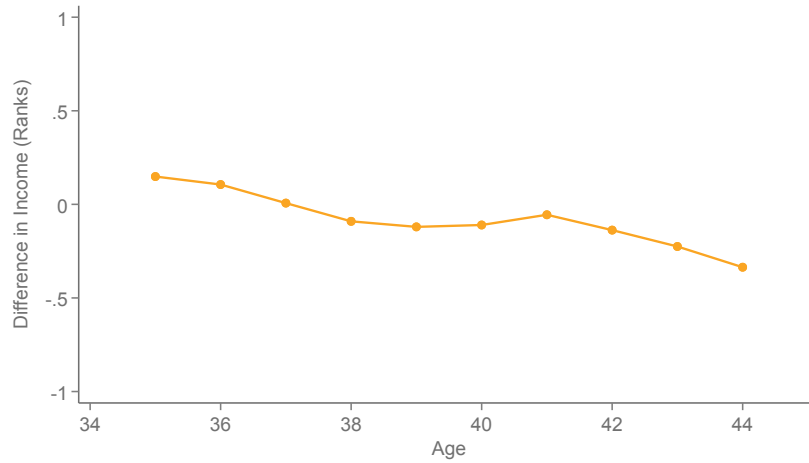
B. Moves Across CZ Quartiles
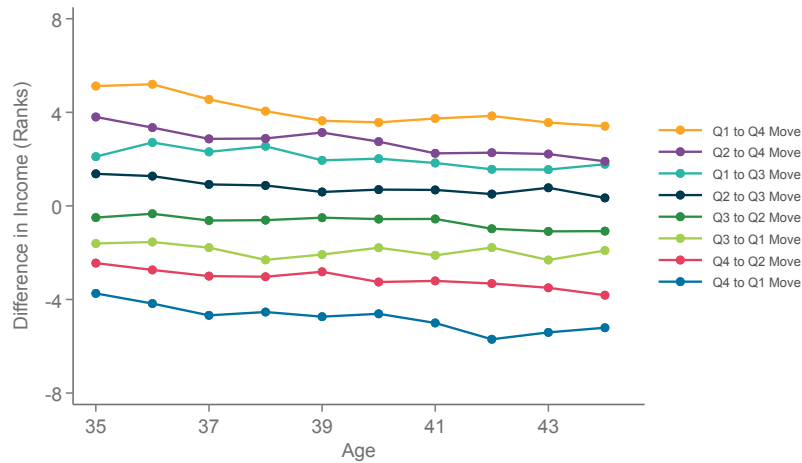
Notes: This figure examines the extent of Roy sorting in moves by age. Panel A examines the wage changes for a set of individuals who migrate across CZs but move to locations with similar location wage premia. This is calculated using a split sample so the that the location wage premia are constructed using one half of the sample and the differences in wages across movers are constructed using the other half of the sample. This figure plots the wage changes among those movers by age. In order to construct Panel B, we separate locations into quartiles of the distribution of location wage premia. Panel B shows the wage changes of individuals as they move across those various quartiles.

APPENDIX FIGURE 4: Variance of Location Wage Premia by Age

A. Ranks

B. Log Wages
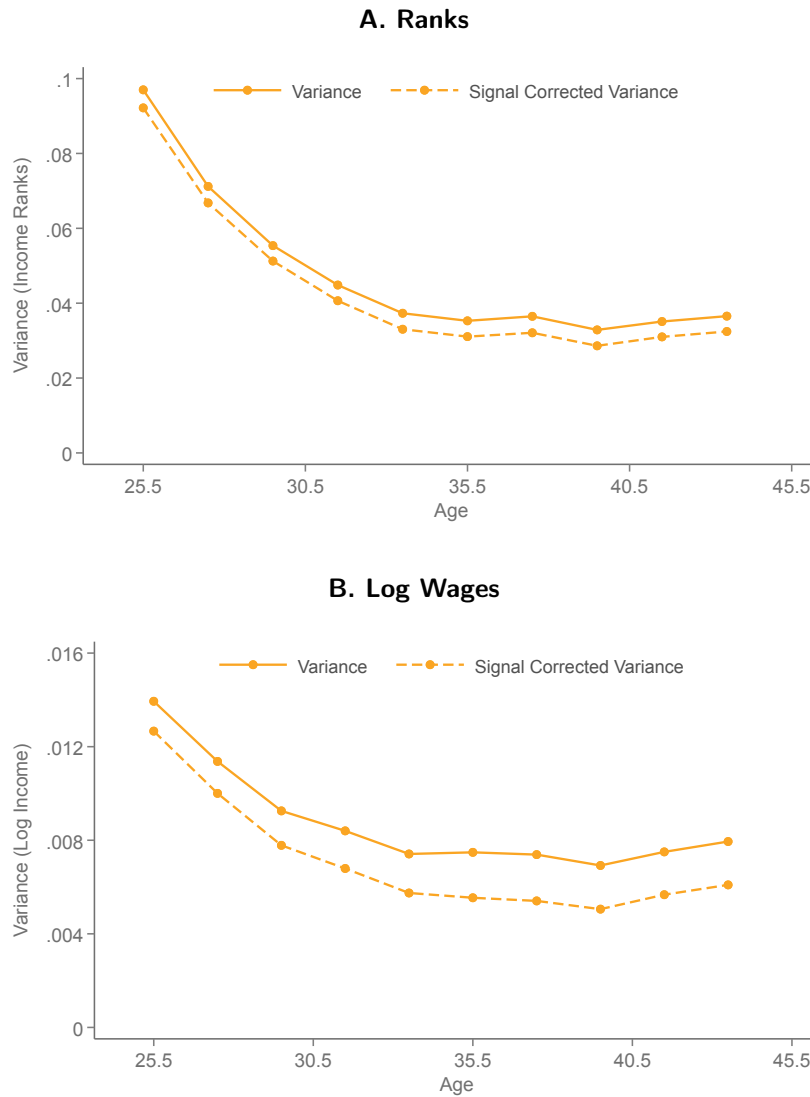
Notes: This figure plots the variance of location wage premia estimated among movers of different ages. In Panel A, our wage outcome of interest is individual income ranks and in Panel B our outcome of interest log income. Individuals are categorized by their age before their move occurred and they are grouped into 2-year groupings of ages. In both panels we plot the variance of the initial estimates and we plot a signal corrected variance. That signal corrected variance is constructed by using the mean of the square of the standard errors of the individual estimates in order to calculate the noise in the estimates. Estimates are adjusted using the resulting signal to noise ratio.

APPENDIX FIGURE 5: New Orleans Out-Migration Rates using W-2 Location
Information
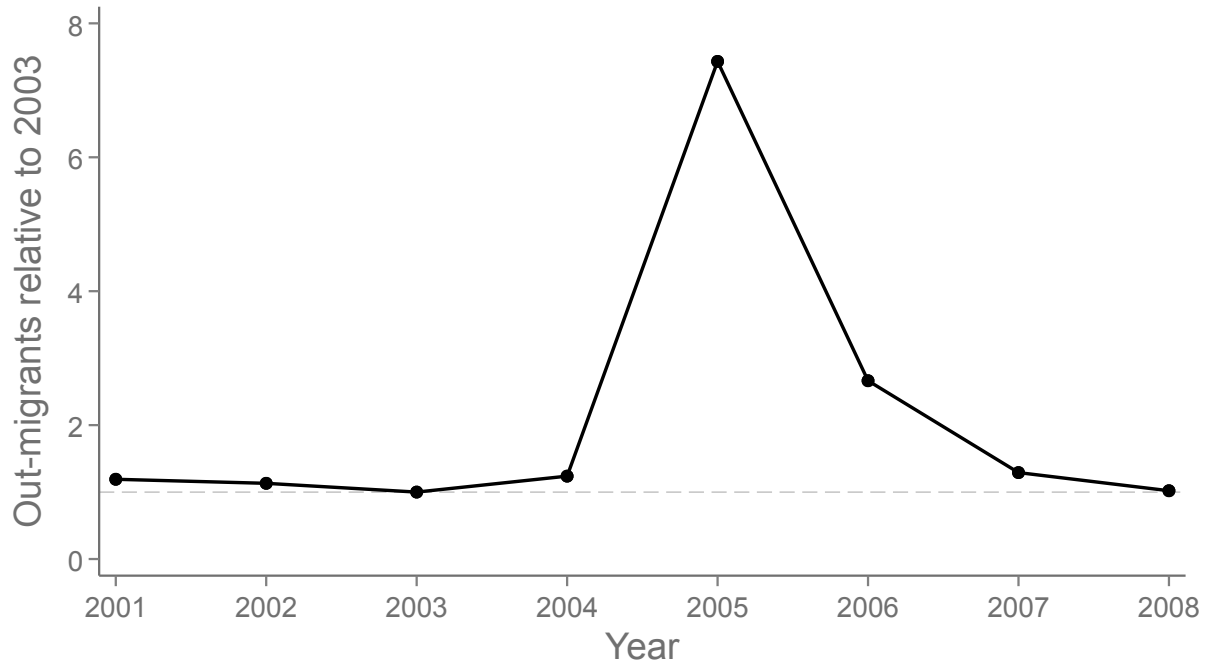
Notes: Just as in Figure 3 Panel A , this figure plots the rate of out-migration from New Orleans in each year around the
Hurricane Katrina. In this case migration rates are calculated using location information from information returns alongside
location information from tax form 1040 filings. This sample is calculated among individuals who are ages 36-44 in the years
before their move.

APPENDIX FIGURE 6: Hurricane Katrina Out-Migration Event Studies, Alternate Specifications

A. Single Destination Fixed Effect

B. Pre-Period Predictions
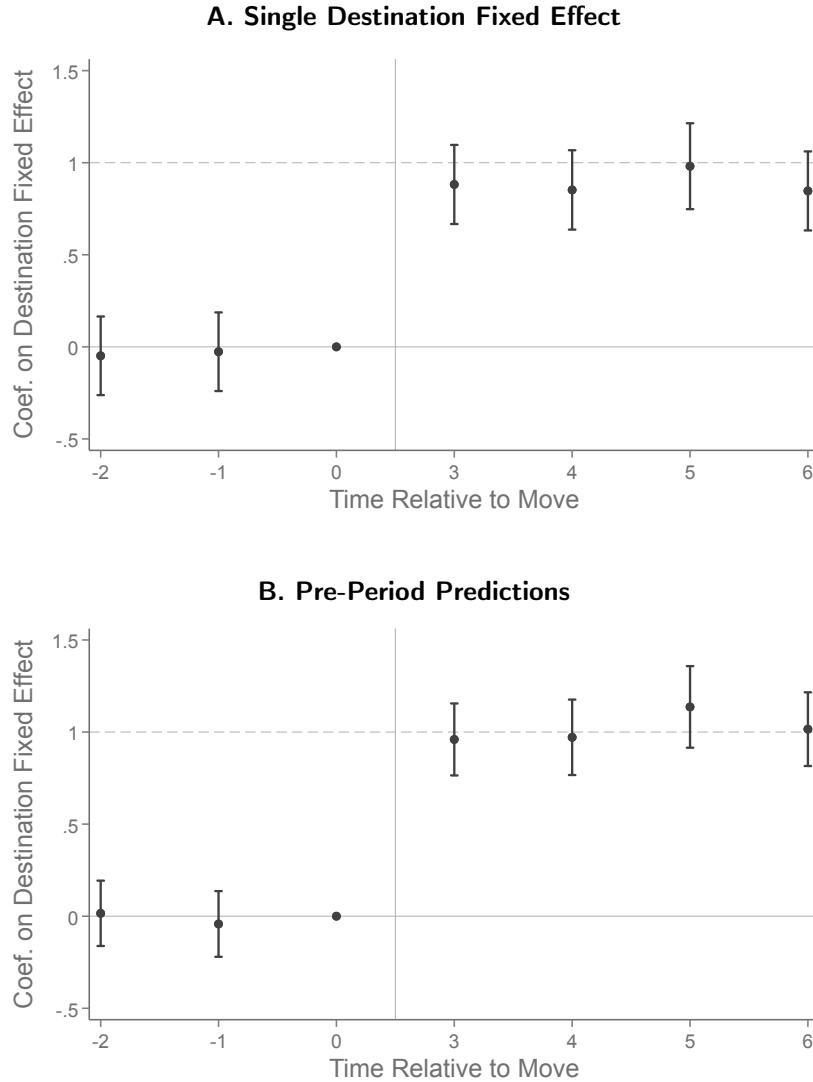
Notes: Just as in Figure 3 Panel C, this figure presents the results of an event study where the year-by-year wages of out-migrants from New Orleans are regressed on the location wage premia estimated among non-New Orleans migrants. In Panel A, the wage trajectories of New Orleans movers are compared to a single location fixed effect estimated among non-New Orleans movers. That fixed effect is estimated in the first full year of arrival in the destination locations. In Panel B, the wage trajectories of New Orleans movers are compared to location wage premia among non-New Orleans migrants are allowed to vary based on the years relative to the move. In particular, this specification uses pre-move fixed effects among non-New Orleans movers to test for pre-trends. This lies in contrast to our baseline approach where we use post-move fixed effects to test for selection. This event study is implemented using split-sample IV to account for potential measurement error in the fixed effects among non-New Orleans movers.The regression is weighted using population count weights corresponding to the number of individuals arriving in the destination who left New Orleans. For both Panels A and B the sample is restricted to destinations to which at least 10 individuals in our sample arrive from New Orleans.

APPENDIX FIGURE 7: Childhood Environment Exposure, Realized Location Wage Premia
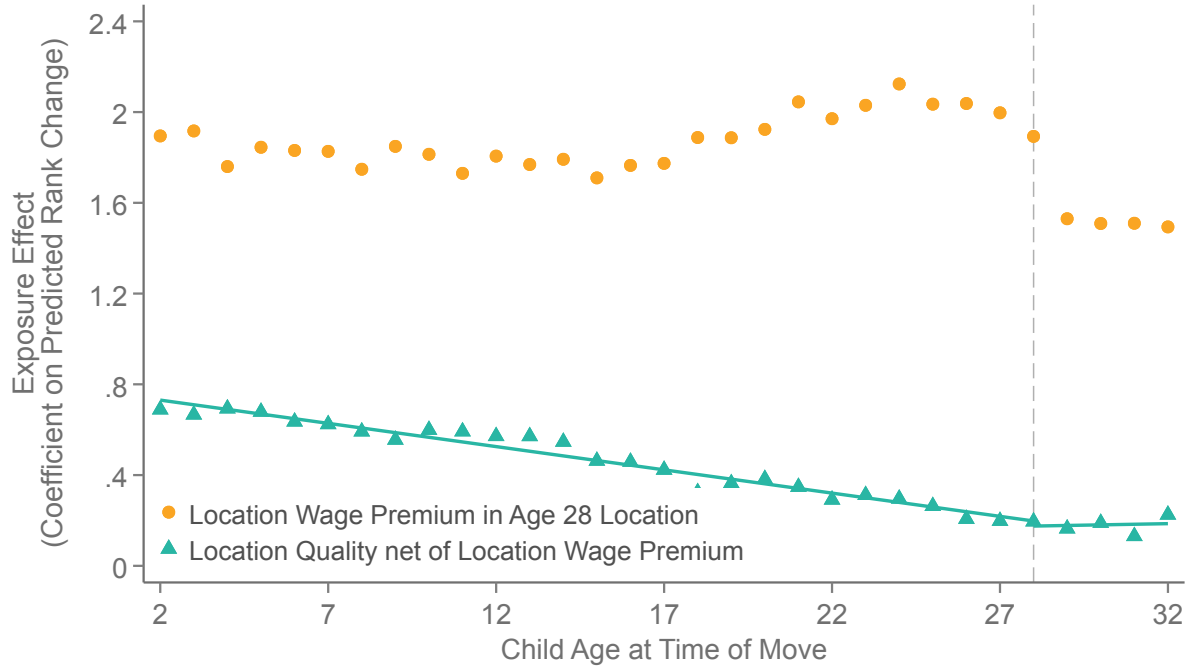
Notes: Just as in Figure 7, this figure presents evidence on childhood human capital accumulation across place using variation in child age at the time of parental moves. It presents a modified version of the regression presented in Panel B. In this case, rather than regressing observed wages on the difference in location wage premia between a child's parent's origin and a child's parent's destination, it is regressed directly on a measure of the location wage premia in the place where the child is observed at age 28. This measure of location wage premia is plotted alongside a measure of location wage quality, which is measured upward mobility net of location wage quality.

A. Log CZ Size, Weighted
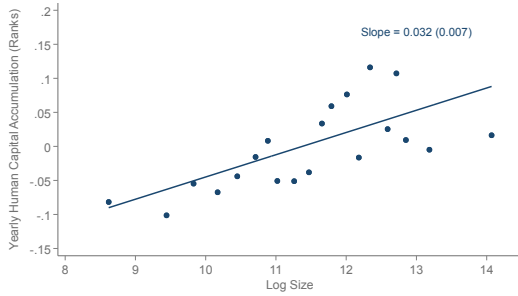
B. Share College Grads, Weighted
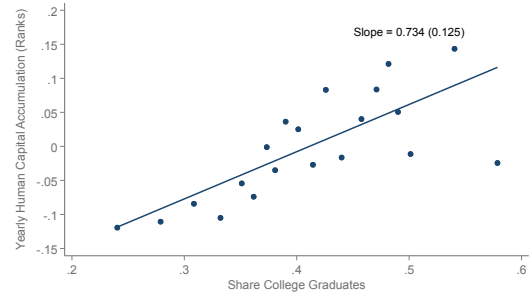
C. Log CZ Density
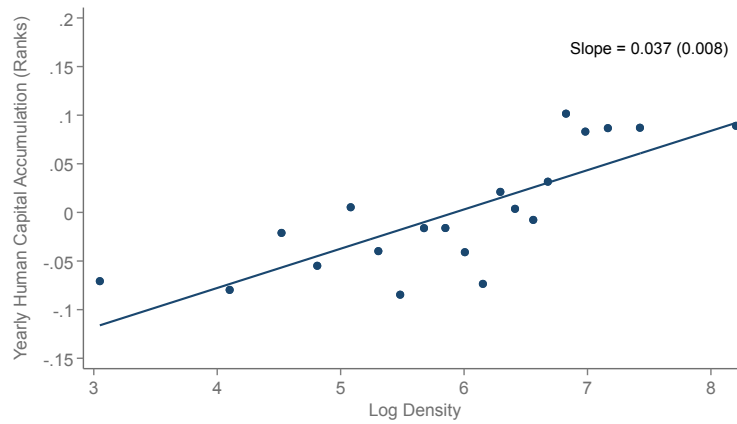
Notes: Just as in Figure 6, this figure plots the relationship between our estimates of adult human capital accumulation across place and characteristics of commuting zones. We measure adult human capital accumulation as the impact of an additional year of exposure to a place on an individual's wage ranks.. Panel A shows a binned scatterplot comparing our adult human capital accumulation estimates and log CZ size. This binned scatterplot is weighted using CZ size. Panel B shows the relationship with the share of individuals in a CZ who are college graduates. This binned scatterplot is weighted using CZ size. Panel C shows the relationship with adult human capital accumulation and log CZ density, where density is measured using the density of the densest county in the CZ. All panels report the regression coefficients from the regression in the micro-data.

APPENDIX FIGURE 9: Relationship Between Place-Specific Determinants of Wages, Excluding Top 50 Largest CZs
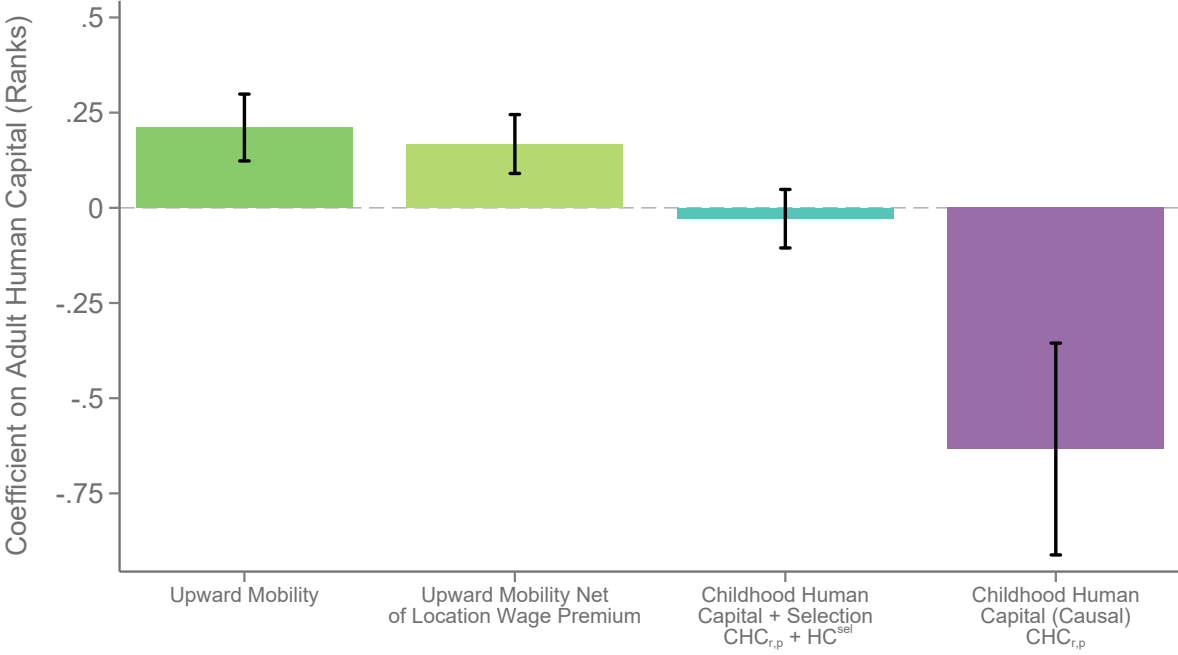

Notes: Just as in Figure 8 this figure plots the relationship between our place-specific determinants of wages. The key modification is that it excludes the 50 largest CZs. The first bar reports the results of a regression of aggregate upward mobility in each location on the impact of place on adult human capital accumulation. The second bar reports the results of a regression of upward mobility net of location wage premia on adult human capital accumulation. The third bar reports the results of a regression of upward mobility net of location wage premia and adult human capital accumulation. The final bar reports the results of a regression of our estimates of the causal effect of place on childhood human capital accumulation on our estimates of the causal impact of place on adult human capital accumulation.

APPENDIX FIGURE 10: Relationship between Adult and Childhood Human Capital
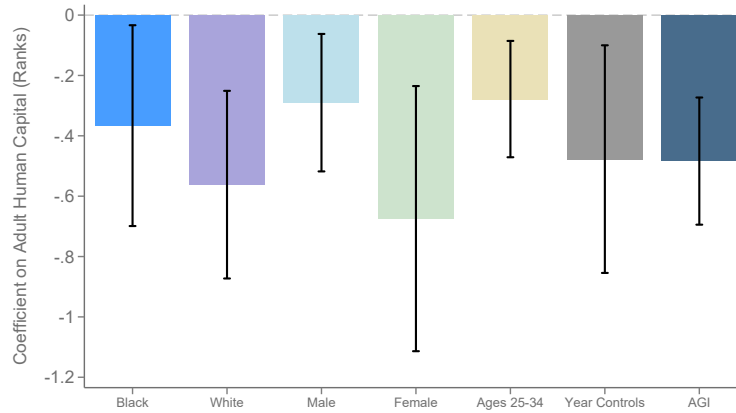Accumulation, Alternate Specifications

A. Alternate CHC Estimation Strategy
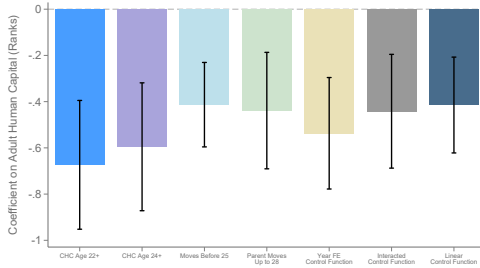

B. Alternate Location Wage Premia
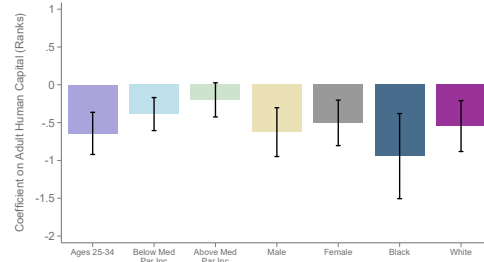
C. Alternate Location Wage Premia and Adult Human Capital Accumulation

Notes: This plots the relationship between our estimates of childhood human capital accumulation and adult human capital accumulation. Panel A shows how our results change when changing our estimation strategy used to construct our estimates of the causal effect of place on childhood human capital. In particular, it shows the effect if we vary the numbers of years of adult human capital exposure that are adjusted for. It shows how the results change when we place alternate sample restrictions, such as calculating one time-movers among parents who only move once before their child turns 28 (rather than 24 in our baseline.) It also shows how the results change under alternate controls functions in Equation 11. Panel B shows how results change when we use alternate samples to construct our location wage premia. In particular, it shows the results among Black versus White individuals and men versus women. It also shows our results when we vary the age of location wage premia measurement and restrict our analysis to individuals with above or below median parental income. Panel C shows how results change when we use alternate samples to construct our estimates of location wage premia and adult human capital accumulation. It shows variation in our estimates across demographic characteristics as well as variation in the age used to calculate our location wage premia and adult human capital estimates. It shows variation in our estimates with cz-by-year fixed effects are added to estimates of adult human capital accumulation. Finally, it shows our results calculated using family income rather than individual income ranks.

APPENDIX FIGURE 11: CZ Characteristics and Place-Based Determinants of Human Capital, Include Top 50 Largest CZs
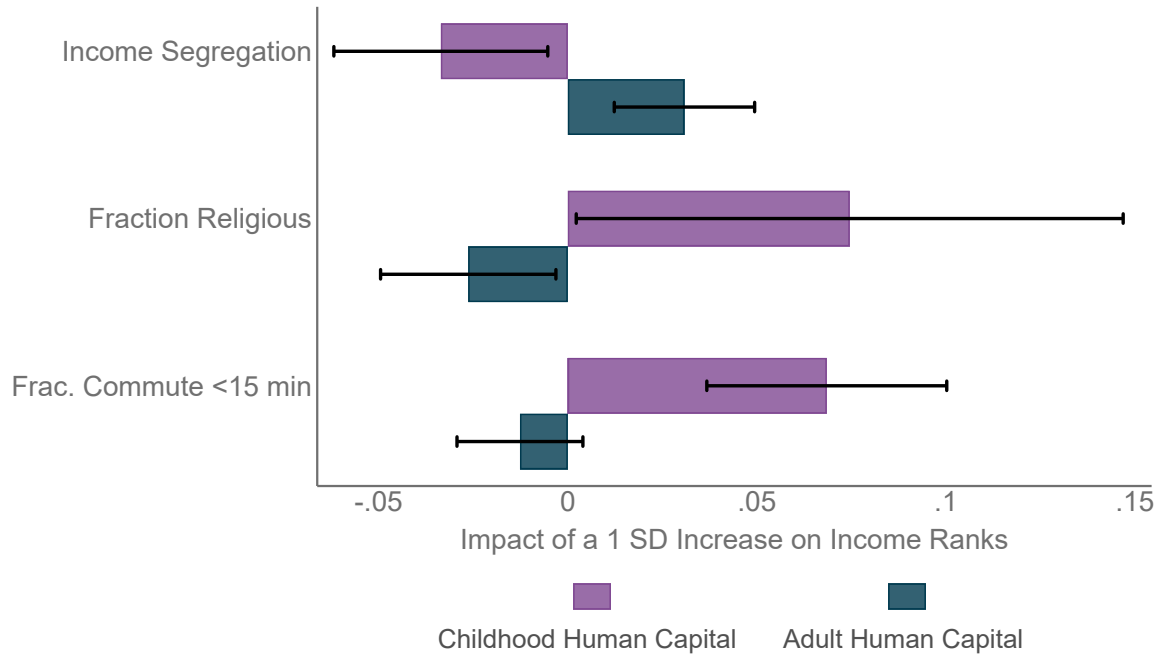
Notes: Just as in Figure 11, this plots the results of regressions of our place-based determinants of human capital on several key CZ characteristics. The CZ characteristics are normalized by their respective standard deviations. All human capital effects are normalized to capture the effect of one year of additional exposure to place. This regression includes all commuting zones, rather than excluding the top 50 largest CZs.
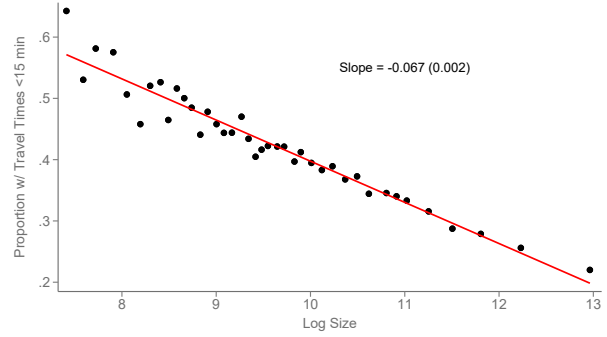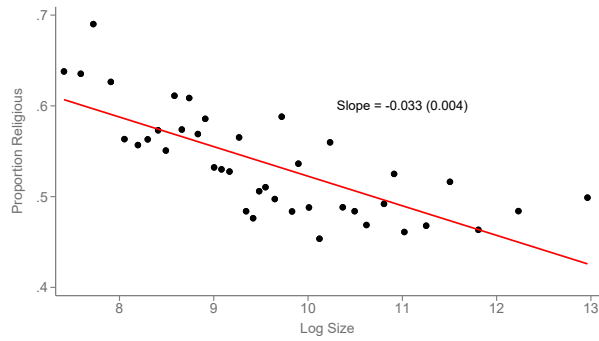
APPENDIX FIGURE 12: Binscatters of CZ Covariates

A. Income Segregation by Log CZ Size


Slope = 0.020 (0.001)
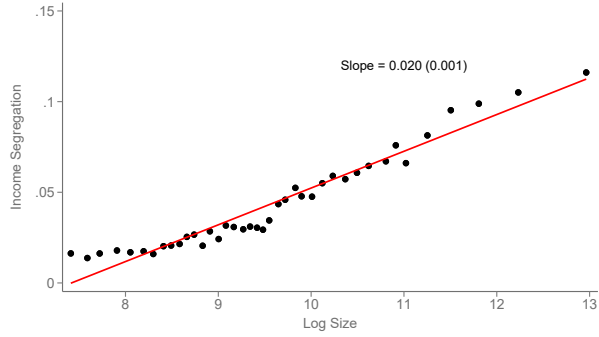
B. Commute Time by Log CZ Population

Slope = -0.067 (0.002)

C. Fraction Religious by Log CZ Size

Slope = -0.033 (0.004)

Notes: This figure expands on Figure 11 by exploring the relationship between the primary tradeoff result we find in Figure 8 and CZ characteristics in the form of binned scatterplots. Panel A shows the relationship between tract level income segregation and log CZ size. Plot B plots the relationship between the fraction of commute times less than 15 minutes and log CZ size. Panel C shows the relationship between the fraction of the population in each CZ that is religious and log CZ size. (The measures of income segregation, commute time come and religious observance come from Chetty et al. (2014).)

APPENDIX FIGURE 13: Prices and Place-Specific Determinants of Wages, Alternate Specifications

A. Childhood Human Capital Estimates with Selection


B. Ages 32-37 Prices
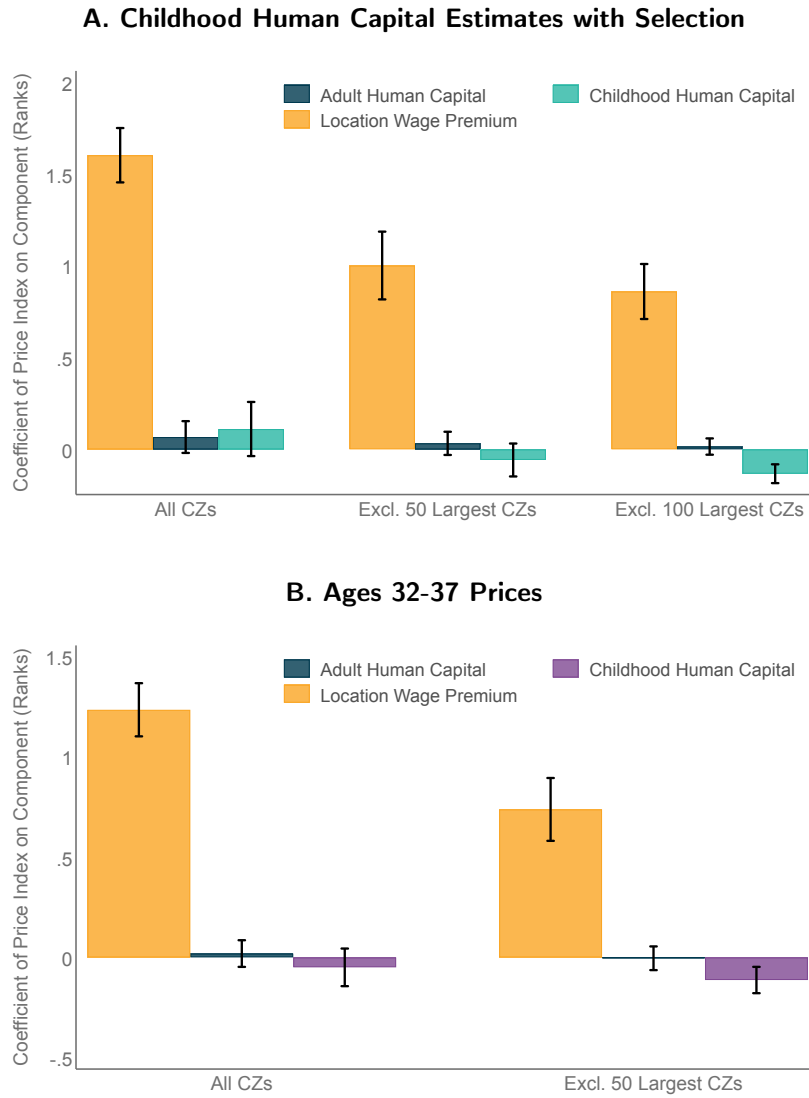
Notes: Just as in Figure 13 plots the relationship between local prices and place-based determinants of wages. We measure local prices using local rents from the ACS. In Panel A we modify our estimate of childhood human capital accumulation to use our estimates which are inclusive potential section effects. In Panel B we modify our analysis to measure prices using the rents of individuals aged 32-37. In all cases we make hedonic adjustment to account for unit characteristics following the approach in Diamond and Moretti (2021). We then regress our measure of local prices on each of the three place-based determinants of wages. In order to align with our age of income measurement, adult human capital accumulation is normalized to measure the effect of 6 years of exposure, ages 23 through 28 and childhood human capital accumulation is normalized to measure the effect of exposure through age 22. We use a split sample IV to account for potential measurement error in our estimates.
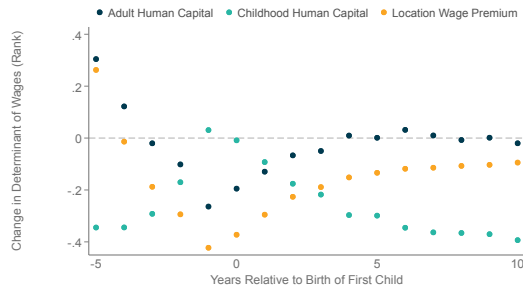
A. Moves Relative to Birth of First Child
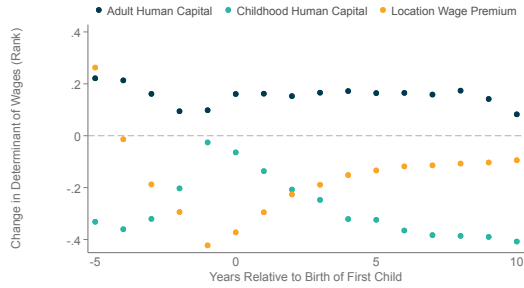
B. Parent Moves, Alt. Adult Human Capital Estimate
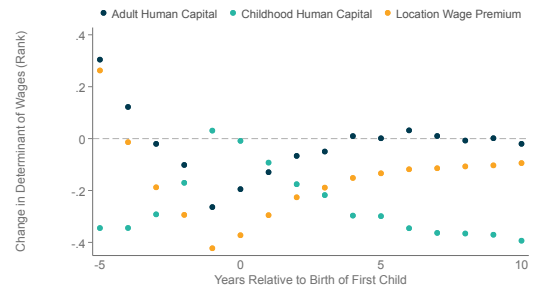
C. Alt. Adult Human Capital Estimate

Notes: Just as in Figure 14 this figure plots individuals by the age at which they moved and examines the change in place-based wage determinants associated with their moves. The first series shows the change in the location wage premia between their origin and their destination. The second series shows the change in adult human capital accumulation and the third series shows the change in childhood human capital accumulation. Panel A shows those migration patterns among a sample of parents. It shows the patterns relative to the age at which they have their first child. Panel B once again shows adult migration by age. It uses an alternate measure of adult human capital accumulation that includes CZ-by-year fixed effects to account for any potential macroeconomic trends in the data. Panel C reports rates of migration relative to the birth of a first child, now using the modified adult human capital accumulation series that includes CZ-by-year fixed effects.

Online Appendix

A   Empirical Bayes Procedure

In Figure 2 we display estimates of location wage premia across US commuting zones. The estimates displayed in that Figure are subject to Empirical Bayes shrinkage. In this appendix we outline the method used to construct those shrunk estimates.

Using the regression outlined in Equation 4 we have an estimate for the location wage premia in each commuting zones, $L\hat{M}_l$ and we have an associated heteroskedasticity-robust standard error for each estimate, $\hat{\sigma}_l$. Ultimately, our approach will shrink CZs toward one of two means. For CZs of above median size we will shrink them toward the mean location wage premia of CZs of above median size. For CZs of below median size we will shrink them toward the mean location wage premia of CZs of below median size.

We assume that our estimates of location wage premia $L\hat{M}_l$ are distributed in the following manner. $L\hat{M}_l \sim \mathcal{N}(LM_l, \hat{\sigma}_l^2)$. Here LM_l is the true value of the location wage premia in location l and $\hat{\sigma}_l^2$ is the square of the standard error. As noted, we allow true values of LM_l to be drawn from one of two different distributions, depending on whether location l is a CZ of above or below median size. In other words, let A be the set of CZs of above median size and B be the set of CZs of below median size. We assume the true distribution of location wage premia is normally distributed such that $LM_l | l \in A \sim \mathcal{N}(LM_A, \sigma_A^2)$ and $LM_l | l \in B \sim \mathcal{N}(LM_B, \sigma_B^2)$.

Next, we need to construct estimates of the means and variances of our hyperdistribution. These are given by $LM_A, LM_b, \sigma_A^2, \sigma_b^2$. We construct our estimates of LM_A using the mean of the location wage premia for CZs within set A. Formally, this is $E[L\hat{M}_l | l \in A]$. (We take this approach because we assume that our estimated location wage premia are unbiased estimates of the true location wage premia.) We can then use the same procedure to estimate LM_B. When it comes to estimating σ_A^2 and σ_B^2 we need to account for the fact that the variance of our estimated location wage premia will also incorporate sampling error. This will procedure excess dispersion in the estimates and increase the variance. We estimate a noise corrected variance using the law of total variance. Here we solve for $\hat{\sigma}_A^2$ using the fact that $\hat{\sigma}_A^2 = Var(L\hat{M}_l | l \in A) - Var(\hat{\sigma}_l^2 | l \in A)$. We do the same symmetrically for the hyperdistribution variance of CZs in set B. With our estimates of the means and variance of the hyperdistribution in hand, we can then construct our shrunk estimates.

Given an assumption of normal priors and normal signals we use the standard Bayesian updating pro-

cedure. For a CZ in set A, our estimate for the posterior mean is given by $\frac{\hat{\sigma}_A^2}{\hat{\sigma}_A^2 + \hat{\sigma}_i^2} LM_i + \frac{\hat{\sigma}_i^2}{\hat{\sigma}_A^2 + \hat{\sigma}_i^2} LM_A$.
The same is true symmetrically for CZs in set B. Our credible interval for CZs in Set A is given by
$\frac{\hat{\sigma}_A^2}{\hat{\sigma}_A^2 + \hat{\sigma}_i^2} LM_i + \frac{\hat{\sigma}_i^2}{\hat{\sigma}_A^2 + \hat{\sigma}_i^2} LM_A \pm 1.96 \sqrt{\frac{\hat{\sigma}_i^2 \hat{\sigma}_A^2}{\hat{\sigma}_A^2 + \hat{\sigma}_i^2}}$. The same is true symmetrically for CZs in set B.

B   Hurricane Katrina Estimation Selection Bias Test

In Section 4.2 we seek to validate our estimates of location wage premia using evidence from plausibly exogenous out migration in response to Hurricane Katrina. In particular, we construct a set of location wage premia estimated on a sample of individuals who migrated out of New Orleans in 2005. We also construct a set of location wage premia estimated on a set of individuals who originated in a location out of New Orleans. We argue that the location wage premia estimated among the New Orleans movers are subject to little or no selection. These were individuals who made their migration decision under duress. They likely chose their location based on considerations of convenience such of the presence of family, rather than choosing based on the types of considerations that are likely to induce bias into our estimates (a match between location quality and a change in individual-level human capital in the year of a move.) In order to test for selection we regress the New Orleans movers location wage premia on the typical movers location wage premia. We argue that, subject to mild assumptions, the regression coefficient captures the fraction of total variance in our typical movers estimates that are due to treatment rather than selection. Our logic is as followers.

Consider two sets of location wage premia estimates:

$$LM_{typical,l} = T_l + S_l + a_l$$

$$LM_{noselect,l} = T_l + b_l$$

Here our first set of estimates, $LM_{typical,l}$ are estimated on a typical set of movers and $LM_{noselect,l}$ are estimated without selection. Here, T_l is the true treatment effect in location l. It is the true location wage premia associated with residence in location l. S_l is any potential selection bias in our estimates of the location wage premia in location l. a_l and b_l is place-specific noise in our estimates.

In our test, we regress our estimates without selection on our estimates constructed using typical movers. We estimate: $LM_{noselect,l} = \beta LM_{typical,l} + \pi$. For the moment we make two assumptions. First, $Cov(T_l, S_l) = 0$, our treatment effects T_l are uncorrelated with the degree of selection in our estimates, S_l. Second, our

estimates of location wage premia contain no place-specific noise, $a_l = 0$ and $b_l = 0$.

Under these assumptions we can solve for our regression coefficient β in terms of the variance of T_l and S_l. In particular:

$$\beta = \frac{Var(T_l)}{Var(T_l) + Var(S_l)}$$

This means that our coefficient of interest captures the fraction of the total variance in our typical estimates that is the result of treatment effect rather than selection. In the context of our primary results, it means that our typical location wage premia estimates are almost entirely driven by treatment effects rather than selection.

But, how should we evaluate the validity of the two assumptions we made? As for the covariance, let us consider the case where $Cov(T_l, S_l) \neq 0$. The typical selection story told about location wage premia often suggests that this covariance is positive. For example, it may be suggested that estimates in high-wage cities are biased upward because the typical movers to those locations are positively selected in some manner. Maybe the type of person who moves to New York City just had an increase in their individual human capital that coincides with the decision to move to New York. In that case, if $Cov(T_l, S_l) > 0$ then β actually serves as a lower bound on the fraction of total variance due to selection. This is because the positive covariance term is found in both the numerator and denominator of our expression for β: $\beta = \frac{Var(T_l)+Cov(T_l,S_l)}{Var(T_l)+Var(S_l)+2Cov(T_l,S_l)}$. For a positive values $Cov(T_l, S_l)$ and baseline values of $\frac{Var(T_l)}{Var(T_l)+Var(S_l)}$ above 0.5, this reduces the value of the fraction below the true fraction of variance due to selection. In the context of our results, this bound adjustment is not all that relevant. Our estimated coefficient is so close to 1 that the interpretation of our results don't change if we regard β as a lower bound.

It is also worth acknowledging the flip side of this covariance condition. If it were the case that $Cov(T_l, S_l) < 0$ then our coefficient would be an upper bound on the fraction of variance that comes from selection and the strength of that bound would depend on $Cov(T_l, S_l)$. One possible way for such a negative covariance to occur is if there a substantial degree of negative selection across locations. Card et al. (2023) suggest one potential mechanism when they argue that movers from high location wage premia CZs to low location wage premia CZs may move from firms with lower location wage premia to firms with higher location wage premia. If that were the case, and if the negative covariance is quite substantial, it could drive up our estimated coefficient.

We also established a second condition in our equation for β when we set estimates of place-specific noise equal to zero. While we do not have the ability to estimate our location wage premia without any sampling noise, we are able to use split sample IV to account for any potential sampling noise. That's exactly what we do when we implement our regression in practice, and it accounts for the impact of $Var(a_l)$ on our regression coefficients. If needed, we could also correct for that noise ex-post by using our standard errors from our estimates of $LM_{typical,l}$ to estimate the fraction of the total variance of $LM_{typical,l}$ due to sampling error. In either case, we are able to side-step any bias due to sampling noise and simply consider the relative contributions of selection and treatment.

C Net Present Value Magnitude Calculation

In our primary results, presented in Figure 8, we show the relationship between adult human capital accumulation and childhood human capital accumulation. In particular, we regress our estimates of childhood human capital accumulation on our estimates of adult human capital accumulation. Both estimates measure the impact, in wage ranks, of spending an additional year in a given location. While this rank-rank comparison is useful in quantifying the tradeoff across place, it is also useful to think about the magnitude of these effects in lifecycle terms.

As a result, we conduct a net present value calculation in order to help provide intuition for the magnitude of our estimates. In particular, we consider the change in wages that occur if a parent locates in a place that has strong adult human capital but weak childhood human capital accumulation. We evaluate the tradeoff between them and their children.

Consider a parent who locates in a given location where each additional year of residence increases wages by \$1 due to adult human capital accumulation. Let us assume that they reside in that location starting in the year in which their child is born. Let us assume that they are 30 at the time of the birth of their child. Let us assume that they remain in that location through the time that their child turns 22. After that point, let us assume that they move to a neutral location regarding adult human capital accumulation. Let us assume that they work through age 65. Let us assume that the effect of place is constant in dollar terms throughout the lifecycle of the individual. (This is slightly more conservative than our primary assumption of rank stability, but avoids the need for a lifecycle rank-dollar conversion in this back-of-the-envelope calculation.) In other words, this is an individuals who spent 22 years during their child's upbringing in a place that was

better for adult human capital accumulation. If we discount future earnings at 3%, then discounted back to the first year of residence, living in this high adult human capital location increase the individuals wages by approximately $273.

Now let us consider the impact on the child in this scenario. Our estimates suggest that for each 1 rank increase that a place has an adult human capital accumulation that trades off on average with 0.43 rank decrease in childhood human capital accumulation. Let us therefore assume that each year the child spends in their hometown, it reduces their future wages by an additional $0.43. Again we've made a rank to dollar adjustment for the sake of simplicity here, understating the magnitude of the future losses as individuals age. We assume that the child spends 22 years in the location with low childhood human capital accumulation before entering the labor force. We assume that their adult labor location is uncorrelated with their childhood location. (If they remained in the location with low childhood human capital accumulation and high adult human capital accumulation they would serve to recoup some of what they lost.)We assume that they remain in the labor force through age 65. When discounted back to their year of birth at a 3% rate, their childhood exposure reduces their future earnings by approximately $116.

When we compare our two sets of estimates we find that $273 in additional earnings due to increased adult human capital accumulation trades off with approximately $116 due to decreased childhood human capital accumulation. Or, when evaluating the ratio of these two, each $1 in increased wages due to adult human capital accumulation trades off with approximately $0.43 in reduced earnings due to childhood human capital accumulation. Somewhat surprisingly, this lines up nearly identically with our rank-rank estimates.

Now it is important to acknowledge that is $0.43 figure is meant to give a sense of magnitude rather than provide a precise figure. There are a number of ways that one could modify this calculation. First, this calculation is done for a single parent and a single child. It could be modified to account for any different sets of family structure. Second, this calculation converts ranks to dollars and then holds them constant throughout the lifecycle. Our estimates suggest that there is relative rank stability in the size of the adult and childhood human capital accumulation estimates. As a result, the dollar magnitude of those effects should increase over the lifecycle. The parent at age 35 should receive a greater benefit in dollar terms than the decrease in wages experienced by the child when they first enter the labor market. But, the magnitude of the child effect should catch up over time. Third, this calculation assumes that individuals reside in neutral locations in all years other than those in which the child is ages 0-22. While this may be the case, it may

also be the case that both the parent and the child are likely to stick around in the high adult human capital location after the child enters the labor force. While that would tend to reduce the extent of the tradeoff observed here, the child sticking around could also extend the tradeoff into the next generation. They remain in the high adult human capital location but have a child there within 10 years and the cycle starts over again. Finally, these calculations have assumed that there is no impact of the parental adult human capital accumulation on the wage outcomes of the child. If the gains experienced by the parent were transmitted to the child with an intergenerational earnings elasticity of approximately 0.3, that would serve to offset a meaningful portion of the earnings losses experienced by the child.[89]

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[89]This adjustment also provides another way to think about the implication of our results. Typically an increase in parental earnings would be associated with an improvement in child outcomes as well. But, when it comes to CZ level differences in adult human capital accumulation those effects are more than offset by the weaker levels of childhood human capital accumulation in those places.