Like A Good Neighbor: The Role of Neighbors in Career Choice*

Michael Andrews† Ryan Hill‡
Joseph Price§ Riley Wilson¶

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Abstract

There is growing evidence that where a child grows up has consequences for long-run economic mobility. We explore the role of neighbors in this place-based transmission. Using linked historical census records for over 6 million boys and 4 million girls, we reconstruct neighborhood microgeography to estimate how growing up next door to someone in a particular occupation affects a child's probability of working in that occupation as an adult, relative to other children who grew up farther away on the same street. Living next door to someone as a child increases the probability of having the same occupation as them 30 years later by about 10 percent. High income, high education occupations are more transmissible. This occupational transmission has real economic consequences. Children who grow up next to neighbors in high income occupations, such as doctors or lawyers, see gains in income and education, even relative to other children living on the same street, suggesting that neighborhood networks significantly contribute to economic mobility. We additionally document larger occupational transmission in more connected neighborhoods and when next door neighbors are the same race, or ethnicity and have children of similar ages, consistent with information and exposure channels.

Keywords: Neighborhood networks, peer effects, occupational transmission
JEL Codes: J24, J62, N32

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†University of Maryland Baltimore County, Email: mandrews@umbc.edu
‡Northwestern University, Email: ryan.hill@kellogg.northwestern.edu
§Brigham Young University, Email: joseph_price@byu.edu
¶Brigham Young University, CESifo, IZA, Email: riley_wilson@byu.edu.

1 Introduction

Growing evidence suggests that the neighborhood in which a child grows up influences their future earnings and economic mobility (Chetty et al., 2018, 2014). The "moving to opportunity" literature finds that some neighborhoods have a causal role in improving future economic outcomes (Bergman et al., n.d.; Chetty and Hendren, 2018a,b; Chetty et al., 2016; Chyn, 2018; Haltiwanger et al., 2020; Katz et al., 2001; Kawano et al., 2024; Kling et al., 2007). But why do neighborhoods matter? Previous work has focused on the role that neighborhoods play in providing access to education, health, and safety amenities and infrastructure that augment investments in children (Laliberté, 2021). In this paper, we focus on another potentially important mechanism: the role of neighbors. In addition to amenities, neighborhoods foster person-to-person interactions that potentially generate human capital spillovers from peers, mentors, and role models. More specifically, we study the influence of nearby adult neighbors on the future career choices of children, a key decision impacting lifetime earnings.

Existing work finds strong intergenerational correlations between parents and children's later career outcomes (Bell et al., 2019; Corak and Piraino, 2011; Fairlie and Robb, 2007; Hvide and Oyer, 2018). But other adult connections in the neighborhood beyond parents may also play a role in shaping children's futures. As Chetty and Hendren (2018b) and Chetty et al. (2022) show, the demographic makeup of neighborhoods and the level of connection between high-SES and low-SES individuals are some of the strongest predictors of neighborhood intergenerational mobility effects. Many neighborhood effects are highly localized (Aliprantis, 2017; Billings et al., 2022; McCartney and Shah, 2022; Redding and Sturm, 2024; Shah and McCartney, 2023), and local, neighborhood employment networks are strong (Bayer et al., 2008; Hellerstein et al., 2011; Tan, 2022). The ability to easily interact face-to-face, as is possible between nearby neighbors, facilitates the spread of ideas (Andrews, 2019; Andrews and Lensing, 2024; Arzaghi and Henderson, 2008; Atkin et al., 2022; Catalini, 2018;

Moretti, 2021). Other studies find that geographically proximate peers influence children's schooling choices (Avdeev et al., 2023; Barrios-Fernández, 2022; Matta and Orellana, 2022).

To identify the impact of nearby adult neighbors on occupation choice, we exploit within-street variation in exposure to different careers. Our empirical strategy can be illustrated with a simple thought experiment. Suppose that Max lives next door to Dr. Smith. Carl lives five doors away from Dr. Smith on the same street. Is Max more likely to become a doctor than Carl? We scale this thought experiment to estimate exposure effects for all children who lived near doctors (and many other occupations) in the 1910 U.S. population census. Importantly, we use narrow geographic fixed effects to focus our analysis on the comparison of children living on the same census manuscript sheet (which is typically a subset of a single street), but are exposed to next door neighbors with different occupations. Our analysis relies on the identification assumption that while selection into neighborhoods may not be random, selection of immediate next door neighbors (sorting of households within a particular subset of a street) is as-good-as-randomly assigned. We assess this assumption using a variety of tests in our data. This strategy is analogous to work exploring the effects of neighbor's race on neighborhood choice (Bayer et al., 2022).

We study the effect of adult neighbors on career choice using historical US Census data. We take advantage of two key features of the de-anonymized historical census data and related data sets. First, we use modern machine learning methods and user-contributed linkages that allow us to track over 10 million children across censuses, from their childhood neighborhood into their adult careers. These approaches offer a large improvement over alternative linking methods, especially for girls. Second, we exploit the fact that, prior to 1970, historical U.S. censuses were collected by enumerators going door-to-door. By examining the ordering of households on census manuscript pages we can reconstruct the microgeography of a neighborhood.

To begin, we illustrate our approach by studying the effects of growing up next door to one exemplary occupation: doctors. Several studies use doctors as a case study of inter-

generational transmission of occupations from parents to children (Lentz and Laband, 1989; Polyakova et al., 2020; Ventura, 2024). We find that boys who live next door to doctors in 1910 are 41% more likely to be doctors as adults in 1940 than are other boys residing on the same census manuscript sheet but farther away from the doctor. To put this magnitude into perspective, having a doctor as a next door neighbor is about one-thirtieth as predictive that a child will become a doctor as is having a doctor in the child's own household; having a doctor in the same household makes a child 12.4 times more likely to become a doctor than other children on the same sheet. We show that these conclusions are robust to several alternative specifications and samples of the data.

Next, we extend this approach to examine the top 50 largest non-farm occupations for men in the 1910 census and the top 25 largest occupations for women.[1] We first estimate each occupation separately, similar to our doctor analysis. While there is substantial heterogeneity across occupations, the point estimate on next door neighbors is positive for all but four of the male occupations, is statistically significantly greater than zero in 26 out of the 50 occupations, and is never statistically significantly negative. Because fewer women were participating in the labor force in the early 1900s, we have less power to estimate precise transmission effects for women, but we find similar positive (albeit lower in magnitude) effects for girls. We then combine information on all 50 occupations into a single "stacked regression" in which each child appears as an observation in the regression multiple times for each occupation that appears on their census sheet. A boy is about 10% more likely to enter into the average occupation when they live next door to an individual in that occupation than are other children on the same census sheet. A girl is about 6% more likely to enter their neighbor's occupation compared to other girls on the street. As in our results using only doctors, we show that these findings are robust to numerous alternative specifications and subsamples.

The stacked regression approach allows us to examine heterogeneity across occupational

[1]Because women's employment was highly concentrated in both 1910 and 1940 we restrict our analysis to fewer occupations.

characteristics. Here we focus on boys because of the higher demands on statistical power for estimating heterogeneity. Children are more likely to go into the occupations of their next door neighbors when their neighbor is in a high income occupation. This suggests that selective careers are difficult to enter without access to someone with experience in that occupation. These relationships might be key for passing information, opportunities, or general mentoring. Lower skilled occupations may have lower barriers to entry and therefore not require as much direct influence from a mentor.

Importantly, the occupation composition of childhood neighbors has real economic implications. Boys growing up next door to a high-income, high-education worker, such as a doctor or lawyer, have significantly higher education and earnings as adults relative to other boys on the same census sheet. Growing up next door to a lower income occupation such as a porter, truck driver, or laborer actually results in lower income for the children 30 years later. We find evidence that, in addition to making neighbors more likely to enter their own occupation, neighbors also convey more general human capital to neighboring children. For example, growing up next to a teacher increases the probability of working in other high-education occupations like doctors and lawyers, many of which have higher average incomes than teachers. Meanwhile, growing up next to a doctor increases the probability that a boy chooses a different high-income, high-education occupation, but not a high-income, low-education occupation, even relative to other children on the same 1910 census sheet. Results on economic outcomes are particularly striking for girls. The growth of professionalized occupations, such as teachers, nurses, and stenographers, meant that exposure to these new and promising career paths was an important influence on girls educational attainment and future income.

To further understand why childhood neighbors matter, we document how heterogeneity across neighborhood and person-specific characteristics alters the magnitude of estimated exposure effects. Consistent with neighborhood connectedness, boys are more likely to adopt their next door neighbors' occupation in rural areas relative to urban areas, and in places

where a smaller share of residents were born in other places. We also find strong patterns of homophily between neighbors. Boys are more likely to adopt their neighbor's occupation when they share a birthplace, are of the same race, are the same gender, or when the families have children of similar ages. Overall, these results suggest that children are influenced in their career choices by their interactions with people in their social networks. This influence is highly correlated with physical proximity and connectedness. This could be because the formation of social links is more likely with nearby neighbors, and also because the nature of the relationship between the children and their neighbors is stronger with closer proximity.

This work adds to our understanding of neighborhood effects, intergenerational mobility, and occupation choice. Although connectedness is a strong predictor of intergenerational mobility (Chetty et al., 2022), there is a dearth of evidence to causally identify this mechanism. We are able to provide causal estimates of how adult neighbors affect children's long-run trajectories. Most studies of adults' influence typically investigate the correlation between parents and children's later career outcomes (Bell et al., 2019; Corak and Piraino, 2011; Fairlie and Robb, 2007; Hvide and Oyer, 2018). Studies of parents' influences, however, are typically unable to disentangle the effects of environment and informational spillovers from genetic endowments and intra-family transfers such as inheriting a family business.[2] By estimating the effect of nearby neighbors, we remove these last two channels and investigate the importance of information and exposure spillovers. This paper also joins a recent literature focused on the role of information, exposure, and mentoring in children's (especially girls') decisions to enter high-income and high-education careers (Andrews and Zhao, 2025; Breda et al., 2023; Mertz et al., 2024; Olivetti et al., 2020; Porter and Serra, 2020).

This work also adds context to a growing economic history literature. Agresti (1980) and Logan and Parman (2017a) pioneered the use of census sheet order to construct new measures of residential segregation by race. These measures have subsequently been used to

[2]Some exceptions are Dal Bó et al. (2009) and Greenberg et al. (2024) which use regression discontinuities in vote shares and AFQT scores to isolate plausibly exogenous variation in the parent's probability of being a politician or serving in the military, respectively.

study the relationship between residential racial segregation and lynching (Cook et al., 2018), homeownership (Logan and Parman, 2017b), mortality (Logan and Parman, 2018), and present-day neighborhood-level economic mobility (Andrews et al., 2017). Similar techniques have been used to measure residential segregation of immigrants (Eriksson and Ward, 2019).[3] Rather than measuring local segregation, we use census sheet proximity to measure chilren's exposure to different occupations. We advance this literature by utilizing historical maps in conjunction with a close examination of census manuscript images to document several potential errors with existing measures of census sheet proximity, along with modifications of these proximity measures to minimize these errors.

The paper proceeds as follows. Section 2 discusses the census data, in particular how we construct links from children in 1910 to adults in 1940 and how we construct measure of microgeographic proximity. Section 3 lays out our identification strategy and presents evidence for why it is plausible. Section 4 presents our baseline results for doctors, for the 50 largest male occupations separately, the 25 largest female occupations, and for the stacked regressions. Section 5 explores heterogeneous treatment effects in the stacked regression. Section 6 disentangles how exposure to next door neighbors can affect economic outcomes of children when they are adults. Section 7 explores mechanisms of transmission, focusing especially on homophily within neighborhoods . Finally, Section 8 briefly concludes.

2 Data

Existing work has relied on occupation case studies (e.g., congressional legislatures (Dal Bó et al., 2009), the military (Greenberg et al., 2024)) or administrative employment records, like the Longitudinal Employer Household Dynamics linked to census records (Staiger, 2023), to document intergenerational transmission of occupation. Outside of the US, Norwegian registry data has been used to explore the propensity of sons to follow the occupation choice

[3]Other have used census sheet to identify potential neighbors. Quincy (2022) uses census sheet to identify individuals who receive veterans' bonuses and compare them to nearby neighbors, while Tan (2022) shows that workers' industry composition is more concentrated within census sheets.

of fathers (Hvide and Oyer, 2018). However, even these datasets are not suited to answer the question at hand: does a child's exposure to adult neighbors affect their eventual career choice. To answer this question, we must not only be able to link a child to their parents' occupations, but must simultaneously be able to observe the occupations of all their geographically proximate neighbors. Long-running surveys like the Panel Study of Income Dynamics do not contain this information on neighbors and even recent innovations in modern census record linking through the Personal Identification Key (PIK) either restrict analysis to survey samples or the PIKed full-count censuses do not span enough time to observe the children as adults.

For this reason, we exploit individual-linked full count census data for 1910 to 1940. The US Census Bureau releases personally identifiable information from the decennial census after 72 years. Digitized versions of the original census sheets, filled out by hand by Census enumerators, are publicly available and include individuals' names, sex, birth year, birth place, occupation, and address.[4] Each individual's information is recorded on a census line with a family identifier, thus allowing us to connect families within a given point in time.

We use three main data sources: the 1910 full count census, the 1940 full count census, and the Census Tree database of individual links, initially developed in Price et al. (2021) and described in more detail in Buckles et al. (2023).

2.1 Full Count Census Data

We obtain full count census micro data for 1910 and 1940 from Ancestry. This includes all of the digitized information on a census sheet including state, city, enumeration district, address (when recorded), household id, census sheet number, census line number, name, sex, relation to head-of-household, marital status, year of birth, place of birth, employment status, and occupation. We merge the de-identified full count census data to IPUMS records using historical IDs (HISTID) to work with cleaned, categorized measures of occupations.

[4] As discussed in detail below, address was not recorded for every individual.

Using the 1940 census, we can observe each child's own occupational choice as an adult, 30 years later. In some robustness analyses, we also use 1920 and 1930 full count census micro data, to explore the dynamics of occupation transmission over time.

2.2 Census Tree Links

The Census Tree is a dataset that provides over 700 million links for individuals across historical US census records. This dataset was originally created by Price et al. (2021) and further expanded and refined in Buckles et al. (2023). The Census Tree builds on family tree data from one of the world's largest internet genealogy platforms, FamilySearch.org. FamilySearch users add and link historic records to the public profiles of their own relatives. These profiles are connected together through family relationships into a large interconnected network of profiles called the Family Tree. FamilySearch users often have private information that allows them to link records that would not be possible for a trained research assistant or machine learning algorithm. Since the the Family Tree is an open-edit wiki-style platform, mistakes made by one user can be edited by other users. The Family Tree can be used to create links between census records by looking for pairs of census records that are attached to the same profile. The census-to-census links for men alone number more than 158 million, greater than the number of conservative links in the Census Linking Project (Abramitzky et al., 2022).

The Census Tree uses the hand-linked records from the Family Tree as training data to develop a new machine learning algorithm to identify additional linkages. The Census Tree then combines these new machine learning links with links from the Census Linking Project (CLP) and the Multigenerational Longitudinal Panel (MLP), as well as the Family Tree links and a set of machine learning links created by FamilySearch. One of the key innovations of the Census Tree is to combine together links from multiple methods and then use a set of decision rules to adjudicate disagreements across the different linking methods. The final result is a dataset with 391 million links for men and 314 million links for women across the

1850 to 1940 censuses.

To construct our analysis sample we start with the universe of children between the ages of 5 and 18 in the 1910 full count census. The children observed in 1910 are then linked to outcomes in the 1940 census, when they are between 35 and 48 years old. In the 1940 census we observe the individual's occupation, along with other outcomes, such as employment, educational attainment, and wage income. To link individuals from 1910 to 1940 we use all available Census Tree links. This yields a sample of approximately 10.5 million, including 6,346,719 men and 4,182,461 women. As seen in Table 1, this represents 40.2 percent of all children observed in 1910.[5] Table 1 shows that our linked sample comes from household settings that are broadly representative of the full population, although our linked sample is more white than the full population, which is perhaps unsurprising given the difficulty linking Black individuals during this time period.

Because women historically change their last name upon marriage, traditional linking methods perform poorly when linking women. For this reason researchers often exclude women or focus on subsamples of women that are easier to follow over time. Our ability to evaluate outcomes for girls is therefore an innovation relative to the prior literature. The patterns and opportunities for employment were quite different for boys and girls in 1910, however. In 1910, only 29.3 percent of women 18-54 were in the labor force, compared to 95.5 percent of men. These gendered patterns were largely unchanged by 1940, when the labor force participation rate of women 18-54 was only 31.7 percent (90.7 percent for men). Given these patterns, we view the occupational choice effect of boys and girls as both interesting, but distinct, questions. As such, the set of occupations we consider and the estimated effects will be sex-specific to correctly interpret patterns in the historical context.

[5]This likely understates the true linking rate as some children in 1910 have likely died or emigrated by 1940.

2.3 Measuring Neighbors in the Full Count Census

Our empirical approach relies on identifying geographically proximate households using the 1910 census records. Importantly for us, census enumerators were given explicit instructions on how to collect information. "It is your duty *personally* to visit every family and farm within your district... Canvassing of blocks should go in order around the block, not switching back and forth across the street" (U.S. Bureau of the Census, 1910, emphasis in original). As specified in the 1910 enumerator instruction guide, the intent was to record families on a census sheet *neighbor-by-neighbor*. Because the enumerator records households in the order that one would walk down the street, the order of households in the census sheet likely captures the kind of proximity most relevant for frequent social interactions. We use information on the enumeration district, sheet (i.e., page) number, line number, and, in some cases, street address to identify likely neighbors with fine geographic precision.

We estimate occupational transmission for many occupations, but for clarity we describe our data creation process just looking at doctors. First, we flag all households across all 1910 census sheets that include a child age 5-18 and all households across all 1910 census sheets that include an individual with the target occupation: physicians and surgeons (1950 occupation code 075). We then collapse the data to include one observation per household, preserving the geographic measures (city, state, enumeration district, census sheet number, and street addresses when available) and the within-sheet ordering of households. If household members spread across multiple sheets we reassign them to the census sheet of the household head. For each household we then construct a set of binary indicators. The first equals one if there is a doctor in one's own household. The second equals one if there is a doctor on the same sheet and one household above or one household below one's own household ("next door"). The last set of indicators equal one if there is a doctor on the same sheet and exactly 2, 3, 4, or 5 households above or below one's own household, each as a separate indicator. Thus when we limit the sample to focal children we can observe if they have a doctor in their own household, one next door, or one further down the sheet. For our

baseline analysis we do not restrict the gender of the next door neighbor in the target occupation. However, we also explore gender homophily in Section 7. We repeat this process for each target occupation separately. We focus on the 50 largest occupations in terms of 1910 male workforce for boys and the 25 largest occupations in terms of 1910 female workforce for girls. In both cases we exclude farmers and farm laborers.[6]

Given census instructions for enumeration, sheet ordering should allow us to accurately identify geographically proximate neighbors. Focusing on the subset of households where street addresses have been recorded and transcribed, we find this to be true in general. As seen in Figure 1, the probability of being on the same street falls and the gap between house numbers (e.g., 32 Mulberry Street vs 34 Mulberry Street) increases for households that are more lines apart on the census sheet.

While the sheet order should accurately capture proximity in general, there may be cases in which this is not the case. For example, if no one was home when the enumerator stopped at a house, that household was revisited later and added to a supplemental sheet at the end of the enumeration district. As such we observe cases where households with addresses are not in order on the census sheet. This will introduce measurement error in our geographic proximity explanatory variables.[7] Unfortunately, for most households in the census it is impossible to know if the sheet ordering leads us to mis-assign neighbors. In the 1910 census, 35% percent of households contain no address information. Even among those that do record addresses, we cannot verify the sheet ordering is correct without street maps from the time period to verify that addresses are listed in order. However, we can quantify

[6]We exclude farmers from this set of occupations for four reasons. First, farmers often live in communities where most households have the same occupation; it is likely that farmers who live on a street with no other farmers, for instance a lone farmer in an urban neighborhood, are very different from the average farmer. Second, the nature of farming changed substantially between 1910 and 1940. Third, because farmers often lived on large farms, the sheet ordering is less tied to geographic proximity. Finally, farmers often report no wage income; in the analyses in subsequent sections, we consider heterogeneity across occupations in terms of income, among other dimensions, making it difficult to know how to classify farmers. We present results including farmers in Appendix A.

[7]Besides laborers, we find that occupation is not strongly predictive of appearing on the last few pages of an enumeration district, which are likely to be supplemental sheets where individuals are recorded who were not home when the enumerator first stopped by.

the level of measurement error for a subset of observations using historical Sanborn Fire Insurance Maps (Sanborn Map Company, Various Years). To the best of our knowledge, we are the first to attempt to assess the extent to which census sheet order successfully measures the proximity of neighbors.

During the early 20th century, the Sanborn Fire Insurance company constructed detailed maps of streets with house addresses in communities they serviced. To quantify the level of measurement error in the sheet order definitions of neighbors, we first restrict the sample to focal child on the same census sheet as a doctor. We then choose 1 percent of doctors (on 383 sheets) at random to verify by hand. About one quarter of these census sheets have addresses and can be matched to fire insurance maps provided by the Library of Congress. We then hand check each observation and construct a measure of neighbors based on the addresses on census image and the fire insurance maps. For this subsample, we find that the sheet-order based next door neighbor measure is correct 91 percent of the time. In robustness exercises, we provide further evidence that our estimates are robust to measurement error concerns by excluding individuals on the last sheets in each enumeration district (to eliminate supplemental sheets where the sheet ordering does not reflect neighbor proximity), and by using street address information (where available) to determine when two households live next door to one another.[8]

3 Identification Strategy

Intuitively, identification relies on the idea that, while households select their cities or towns of residence, their neighborhoods, and even the street on which they live, based in part on how they expect it to affect their children's later economic outcomes, they have little ability to choose their immediate neighbors. In many cases individuals are unable to observe the occupations of potential neighbors before moving into a residence. Moreover, when deciding

[8]See Appendix B for a more detailed description of census sheet neighborhood reconstruction and possible sources of measurement error.

where to live, households can only move into available housing, limiting their ability to choose immediate neighbors' occupations. In other words, conditional on living in the same portion of a street, a household's distance to a neighbor in a particular occupation is as good as random.

To see this idea more formally, consider a simple linear model of children occupational choice:

$$\text{Occ1940}_{is} = \alpha \text{Amenities}_s + \sum_{d=1}^{D} \beta_d \text{AdultOcc1910}_{id} + \nu_i, \tag{1}$$

where Occ1940_{is} is an indicator equal to one if a child in household i that we observe in 1910 has a particular occupation in 1940. Amenities_s capture any amenities or other local characteristics that may change the probability that a child enters a particular occupation as an adult. AdultOcc1910_{id} is equal to one if the adult living distance d from household i in 1910 is in that occupation. ν_i capture any individual- or household-level idiosyncratic characteristics that affect the probability that a child enters a particular occupation.

We are interested in estimating the importance of exposure to adult neighbors in a particular occupation, given by β_d. The challenge is that we lack data on ν_i, and likely also lack data on relevant local amenities, and both are likely to be correlated to the distance to neighbors in a particular occupation. For an extreme example, children growing up in coal mining towns are more likely to become coal miners than other children observed in the full count census because that is the primary occupation in their town; they are also more likely to live close to coal miners, but this is likely driven as much by proximity to coal mines as to exposure to adult coal miners. As another example, wealthy households, where many of the adults hold occupations like doctors or lawyers, may sort into the same high prestige neighborhoods; then children of high income families are more likely to go into the occupations of adults living in their neighborhoods than the average child, even if true causal exposure effects were zero, since high income parents have the resources and familial

social capital to encourage their children to enter high income occupations.

Access to local amenities should be constant when we compare households who are on the same census manuscript sheet (on average 7.6 households). In other words, households that live very close to one another should have approximately the same exposure to local coal mines, good schools, access to transportation, etc. Furthermore, if, as we argue above, households cannot select the occupations of their immediate neighbors, then idiosyncratic factors should also be uncorrelated with distance to a neighbor in a particular occupation when comparing households on the same census sheet. Our identification assumption is therefore that

$$\text{Cov}(\text{AdultOcc1910}_{id}, \text{Amenities}_s | s) = \text{Cov}(\text{AdultOcc1910}_{id}, \nu_i | s) = 0, \tag{2}$$

where we condition on the census manuscript sheet s. Similar identification has been used in housing transaction data in a modern context (Bayer et al., 2022). We discuss estimation of this model below.

3.1 Plausibility of the Identification Assumption

Our identification strategy would fail if households sort *within census manuscript sheets* in ways that are correlated with both their neighbor's occupation and their child's proclivity to work in that occupation. In the data, this would likely result in individuals in the same occupation living next to each other or individuals in similar occupations (along dimensions such as industry, skill-level, or income) living next to each other.

We think this is unlikely for several reasons. First, we find little evidence that adults having the same occupation are able to sort into adjacent houses. For the 50 largest, non-farm occupations in the 1910 census, among the sheets that contain at least one individual with each occupation, a large fraction of sheets contain only one occurrence of that occupation. With the exception of resource-based occupations that are geographically constrained like

mining, most people are the only person in their occupation on their census sheet. In the results below, we show that our results are robust to focusing on only the sheets that have one of each occupation, in which case it is by construction impossible for households with the same occupation to sort to be adjacent to one another. Occupation-specific measures of segregation, analogous to measures of racial segregation in Logan and Parman (2017a), show similar results. Across occupations, occupation-specific segregation is close to zero (consistent with as good as random placement) and substantially lower than racial segregation or even ethnic segregation for nearly all occupations.[9] See Appendix C for a detailed analysis of neighborhood sorting.

Second, we use the rich information in the census to further condition on features that may predict within-sheet sorting. Results are similar when we control for a suite of household-level characteristics or include fixed effects for the occupation of the household head in each child's household. To minimize concerns about within-sheet variation in access to amenities, we can include fixed effects for the position of each child's household on the census sheet and compare outcomes for even smaller subsets of census sheets. We discuss these results in more detail in the robustness section below.

Finally, we conduct our analysis for a broad range of occupations that vary across skill-level, income-level, and prestige, and find similar patterns of transmission. Once we move from particular occupations (such as doctor) to the full set of large occupations, the kind of sorting necessary to invalidate our identifying assumption becomes more nuanced. Remaining threats to identification must be match-specific sorting where families sort into homes next to people in particular occupations because their child has an idiosyncratic proclivity for that particular occupation in a way that is unique and not driven by general characteristics like occupational income or prestige.

Although this level of sophisticated sorting seems unlikely, we are further able to rule it out using an alternative strategy that exploits neighbors' untimely deaths. If our estimated

[9]The exception is once again for resources based occupations like farming, mining, and lumbermen.

effects were driven by match-specific sorting we would expect similar transmission, even if the neighbor in the focal occupation died before the child could be "exposed" to the career. As we show in the results section, the effects for these children are smaller.

4 Results: Doctor Case Study

To introduce our empirical analysis, we begin with an illustrative example, focusing on boys in 1910 and on one particular occupation: doctors. In the next section, we broaden the analysis to many occupations and both girls and boys.

Here, we present results based on this regression model:

$$\text{Doctor1940}_{is} = \alpha + \beta_0 \text{OwnDoc1910}_i + \sum_{d=1}^{5} \beta_d \text{NeighborDoc1910}_{isd} + \text{Age}_i + \gamma_s + \epsilon_{is}, \quad (3)$$

where Doctor1940_{is} is an indicator for the focal child i on census sheet s listing doctor as their occupation in 1940, OwnDoc1910_i is an indicator for the focal child living with an adult in their household in 1910 that has listed doctor as their occupation, and the indicators $\text{NeighborDoc1910}_{id}$ are equal to one if at least one of the households d steps away from the focal child in the 1910 census sheet has an adult that lists doctor as their occupation. For example, if $\text{NeighborDoc1910}_{is1} = 1$, then the focal child has at least one doctor that lives one house away (that is, lives next door) in either direction on their side of the street. Age_i is a fixed effect for the focal child's age in the 1910 census. Finally, γ_s is a census sheet fixed effect, and ϵ_{is} is an idiosyncratic error term that includes all unobservable local amenities and individual- and household-level characteristics. We cluster errors at the household level.

We first want to assess whether neighbors have a larger influence on childrens' occupation choices when those neighbors are geographically closer relative to neighbors who live further down the street. Figure 2 shows this relationship (the coefficient for OwnDoc1910_{is} is estimated but not reported for ease of interpretation). For doctors, we find that the immediate next door neighbor is the only neighbor that has a significant positive influence. For this

reason, and to simplify the exposition, for the remainder of the paper we focus only on the immediate next door neighbors. Focusing on next door neighbors only may be a conservative test, since households only two doors away from a doctor are now included as part of the untreated group. To the extent exposure effects are operating farther than next door, this will bias against estimating a positive effect of next door neighbors on occupational choice.[10]

We estimate the own household doctor effect and the immediate next door neighbor effect as follows

$$\text{Doctor1940}_{is} = \alpha + \beta_0 \text{OwnDoc1910}_i + \beta_1 \text{NextDoorDoc1910}_{is} + \text{Age}_i + \gamma_s + \epsilon_{is}, \quad (4)$$

where $\text{NextDoorDoc1910}_{is}$ is an indicator equal to one if one of the households next to i has an adult who is a doctor.[11] The neighbors further down the street are now included in the omitted category. The inclusion of census sheet fixed effects (γ_s) is key to our identification strategy, allowing us to compare households in the same narrow geographic location that have different next-door neighbor occupation exposure. We explore various levels of location fixed effects in each column of Table 2 to help assess our identification assumptions. Column 1 does not include any location fixed effects and includes all households with a child in the 1910 census. Column 2 also does not include any location fixed effects, but to include a consistent sample in which there is identifying variation in more restrictive specifications, we restrict attention to census sheets with a child in the 1910 census and for which there is at least one household with a doctor. Column 3 adds broad geographic fixed effects for state-city-enumeration district, and Column 4 adds our preferred, more narrow sheet-level fixed effects. Across all four specifications, we see a very consistent positive effect of

[10]To estimate Equation 3 for households five doors away from the doctor or less, we require census sheets with at least six households (the household with the doctor and five neighbors). Sheets with several larger households may have fewer than six households on the sheet, and so looking at the effects on children farther away from a doctor mechanically reduces the sample size. Also, if we examine effects up to five doors down, households at the top and bottom of the census sheet are treated differently than households in the middle of the census sheet, since a household at the edge of a sheet only has neighbors in one direction. This is another reason to focus only on next door neighbors.

[11]In other words, NextDoorDoc1910_i is equivalent to $\text{NeighborDoc1910}_{is1}$ from Equation 3.

own household transmission. Boys that grow up with a doctor in the home are 9.54-9.83 percentage points more likely to become a doctor than other boys on the street. We report the mean probability of becoming a doctor for the counterfactual group – boys with no doctor in the home or next door. Those with doctors in the home are at least 10 times more likely to become a doctor than their peers on the same sheet, a result that matches other estimates of the intra-household transmission rate in other contexts. Our key coefficient of interest is the effect of living next door to a doctor in 1910. For this coefficient, the level of fixed effects is relevant for our identification assumption. The effect with no fixed effects includes not only the causal effect of exposure, but also a selection effect where kids that live in the same neighborhood as other doctors may have been more likely to become doctors even absent exposure. We see that the coefficient in column 1 of 0.0072 falls to 0.0036 in Column 2 when considering only sheets with doctors, and falls slightly farther to 0.0033 in Column 3 with enumeration district fixed effects. In Column 4, with sheet-level fixed effects, the estimate is 0.0032. This estimate removes the street and neighborhood-level sorting, and we interpret the remaining coefficient as the causal exposure effect. Boys that live next door to a doctor are 0.32 percentage points more likely to become a doctor, or 41% more likely than the boys who live further down the street.[12]

4.1 Robustness

We provide three sets of robustness checks, which we briefly summarize here and provide further analysis in the Appendix. First, we probe our main results using more restrictive regression specifications and alternative sample definitions. Second, we consider an alternative method for constructing neighbor proximity using street addresses. Third, we conduct a placebo test focusing on cases where doctors died shortly after the census enumeration.

[12]In Appendix Table A1, we repeat this exercise but link children to the 1920, 1930, and 1940 censuses. For both own-household and next door coefficients, effect sizes are smallest in 1920, of intermediate value in 1930, and largest in 1940. This is consistent with doctors being an occupation that requires substantial investments in human capital that take time. However, if we scale by the untreated mean the relative effects are similar across the three decades, suggesting that we do not lose out on addition information by focusing on 1940 outcomes.

4.1.1 Alternative Specifications and Samples

In the appendix, we show results for a variety of alternative sample restrictions and regression specifications to bolster the credibility of the main results. In Appendix Table A2, we restrict our sample to census sheets on which only a single doctor resides. This both simplifies the source of treatment variation and ensures that doctors are not sorting to live next to one another.

A related concern is that access to unobservable amenities may vary even within census sheets. For instance, houses close to the end of a block may be larger or have easier access to resources on a cross street, and so doctors and those with a predilection to become doctors may be more likely to sort to live close to the ends of a block. To minimize these concerns, in Appendix Table A2 we include a fixed effect for each household's position on the census sheet (e.g., a control for households at the top of the page, one house down from the top of the page, etc.).

To further assess robustness to endogenous sorting, in Appendix Table A3 we present results using additional sets of household controls and fixed effects. First, we control directly for household head's age, education, race, and nativity. Then we include fixed effects for the household head's occupation, allowing for the comparison of children on the same street with same parental occupations but different neighbors. Finally, we restrict the sample and the scope of the fixed effects to only include children that live at most three houses from a doctor.

In all cases, we find similar results: having a doctor in one's own household increases the likelihood that a boy becomes a doctor in 1940 by about ten percentage points, and living next door to a doctor also significantly increases the probability that a boy becomes a doctor in 1940 by between .25 and .4 percentage points.

4.1.2 Alternative Proximity Measure: Street Ordering

As described in Section 2, while we believe the census sheet order usually accurately captures the microgeography of neighborhoods, in some cases it may be measured with error. One possible issue is that enumerators skip houses if they initially can't find the family at home and place them instead on a supplemental sheet at the end of their stack of sheets. These skips could attenuate our estimates as households appear closer together on the sheet than they actually are physically on the street. In Appendix Table A12, we re-run the specification in Column 4 of Table 2 but omit the last several pages from each enumeration district to ensure that results aren't biased by supplemental sheets. We find nearly identical results when dropping the last 1-5 sheets from each enumerator's stack of pages, minimizing concerns about attenuation bias.

A more careful approach to dealing with bias from enumerator misordering is to recalculate our proximity measures using only the recorded street addresses (where available) and ignore the ordering on the enumeration sheets altogether.[13] We consider a household to be next door to a doctor if it has the closest house number (either larger or smaller) on the same street as the doctor. In Table A14 we use this street address-based measure for next door neighbors and find results that are similar to our baseline estimates. We find an even larger point estimate when we use the street address-based measure of proximity as an instrument for the sheet order-based measure of proximity and correct for non-classical measurement error as suggested by Pischke (2007). See Appendix D for details.

4.1.3 Placebo Check: Doctor Deaths

Thus far, we have operated under the assumption that within a street the placement of doctors relative to children is as-good-as-random. Despite the robustness evidence on sorting that we have presented, lingering identification concerns could arise about selection into

[13] Akbar et al. (2019) and Quincy (2022) also use street addresses in historical censuses to determine the proximity of houses.

immediate proximity to doctors. The ideal experiment to identify the causal effect of occupation exposure would eliminate doctors next to particular children on a street at random, to determine if children that grow up next to doctors are selected differently than other children on the street. In this section, we analyze a natural experiment that is similar in spirit to randomized exposure, exploiting doctor deaths. Using the Database of Human Lifespan (Price, 2024), we tag doctors with documented deaths between 1910 and 1915, the five-year window following the 1910 census where we measure occupation exposure. Then we compare outcomes for boys that live next to a doctor in 1910 who dies before 1915 (6% of the sample) to boys that live next to a doctor who survived past 1915 (note that 31% of the sample live next to a doctor with a missing death date, and are therefore excluded from the analysis). This quasi-random shift in the duration of exposure to a doctor neighbor can help solidify the causal link between exposure and outcomes. In both cases, the boy's family similarly selected into homes next to doctors, but we can determine if boys with less exposure experience similar treatment effects. Due to the cross-sectional nature of the census snapshot, we can't measure the exact duration of doctor exposure for children of different ages, but we can assert that a doctor death shortens the exposure period, likely during very formative ages.

Table 4 shows regression results for the specification in Equation 4, but with additional regressors indicating that the child lived in the same household or next door to a doctor that died by 1915. We find that doctor death sharply decreases the probability that a child becomes a doctor both for the doctor's own children as well as the next door neighbor. Children of surviving doctors have an 11 percentage point higher probability of becoming a doctor compared to the untreated mean compared to only 4 percentage points for children of doctors that die. The effect of next door neighbors similarly falls from 0.3 percentage points down to -0.1 percentage points, a coefficient that is not statistically distinguishable to zero. We also restrict to doctors that died before the age of 60 (perhaps more likely to be

an unexpected death) and find similar results.[14] These findings support the causal influence of exposure on occupation choice and helps to rule out the possibility that effects are biased by selection or sorting in street ordering.

5 Results: Heterogeneity by Occupations

Now that we have established a significant next door neighbor transmission effect in the case of doctors, we can consider a broader set of occupations, including some of the growing professions available to women in the time period. We estimate effects occupation-by-occupation, but also implement a stacked regression approach to understand how occupational characteristics influence exposure effects.

For each occupation we replace the indicators Doctor1940_{is}, OwnDoc1910_{i}, and $\text{NextDoorDoc1910}_{is}$ with analogous indicators for that particular occupation. To simplify the analysis and to ensure that we have a sufficient number of individuals in each occupation, we focus on the 50 non-farmer occupations that have the largest representation among men in the 1910 census, and the 25 largest non-farm occupations for women.

These coefficients are provided for boys and girls for each occupation separately in Figure 3. Both within household and next door neighbor effects are presented. In both cases, we scale the coefficients by the mean among kids with no individuals in the occupation in their household or next door so that the plotted coefficients can be interpreted as percent changes relative to the untreated mean.[15] Both are separately sorted by effect size and solid filled markers indicate that the coefficient was statistically significant at the the 5% level. We find that, for all 50 of the largest occupations for men, having an individual in the same household significantly predicts that the child is more likely to enter that occupation. There is, however, substantial heterogeneity in effect size across occupations. For own-household transmission for boys, doctors are indeed ranked highly, with the fifth largest estimate in

[14] Appendix Table A4 reproduces the results in separate regressions by death status and missing status and finds similar results.

[15] We plot the un-transformed coefficients in Appendix Figure A1.

percentage terms. But other occupations of various type, such as brickmasons, bakers, meat cutters, blacksmiths, clergymen, barbers, and lawyers also predict that kids are more than ten times more likely to go into the occupation if an adult in their own household is in that occupation. Own household transmission for girls is positive and statistically significant for most occupations, but the effect sizes are much lower than for boys. Girls are likely to choose the occupation of a parent one to four times as often as an untreated peer, with a range of occupations affected, including tailors, nurses, teachers, and musicians among others. The muted transmission effect for girls may be due to lower overall employment among women (in both 1910 and 1940) or expanding occupational opportunities that were available to women in 1940 but not 1910.

Focusing on the next door coefficients, we see that most occupations show positive transmission effects from next door neighbors. In contrast to the own-household coefficients, however, not all are statistically different from zero and three (stationary firemen, doorkeepers, and shipping and receiving clerks) are negative, although not statistically significantly so. The most predictive occupation is for brickmason, the same as for the own-household coefficients, although the next several largest differ between the two lists. Doctors once again rank highly, in eighth place. Girls show more mixed evidence on neighbor transmission. While the majority of the top-25 occupations have positive transmission, only five are statistically significant. So although the effects for girls are suggestive, we lack the same level of statistical precision for women.

These plots make it clear that some occupations are more likely to transmit to neighboring children. However, it is difficult to draw conclusions about the channels of occupation transmission by inspecting individual cases, so we introduce a stacked regression approach to systematically explore patterns of heterogeneity and causal mechanisms. To do this, we take each occupation-specific regression sample and stack them, so that all 50 occupations for boys appear in one regression sample and all 25 occupations for girls appear in another

regression sample. We estimate

$$\begin{aligned}\text{Occ1940}_{ijs} =& \alpha + \beta_0 \text{OwnOcc1910}_{ij} + \beta_1 \text{NextDoorOcc1910}_{ij} + \text{Occ}_j \times \text{Age}_i \\ &+ \text{Occ}_j \times \gamma_s + \epsilon_{ijs}, \end{aligned} \tag{5}$$

where j indexes each occupation. In this specification, each child i is included in the regression multiple times, one for each occupation that occurs on the child's street. Since the same household is used multiple times, in this specification we two-way cluster standard errors at the household level and at the individual i level. Our fixed effects for age and census sheet are interacted with occupation to make the same within-occupation comparisons that we made in Figure 3.

We present average effects across occupations from the stacked regression in Table 5a for boys, and Table 5b for girls. Columns use the same location fixed effects strategy as in Table 2. In Columns 1 and 2, we have no location fixed effects (although we still include a fixed effect for each occupation j). Recall that Column 1 includes all census manuscript sheets, while Column 2 includes only sheets that have at least one individual of each occupation j.

In contrast to the results when examining only doctors, in the stacked regression coefficients for both own-household effects and next door neighbor effects become substantially smaller as we include geographically smaller location fixed effects. This is consistent with substantial locational sorting for the average occupation. However, as we include finer geographic fixed effects, and compare children living next door to someone in the target occupation to children that are more and more geographically proximate, the estimated effect of $\text{NextDoorOcc1910}_{ij}$ falls.

Our preferred specification in Column 4 includes a fixed effect for each census manuscript sheet interacted with a fixed effect for each occupation. In this specification, growing up with an adult in the same household in an average occupation increased the probability that the boy enters that occupation by 3.81 percentage points, or about 115%. Growing up next door

to an individual with an average occupation increases the probability that the boy enters that occupation by 0.34 percentage points, or about 10.3%. For both the own-household and next door coefficients, the estimates in Table 5 are smaller than those for doctors in Table 2, which is consistent with our findings in Figure 3 that exposure effects for doctors are larger than for an average occupation. For girls, shown in Table 5b the effects are also positive and significant, but smaller in percentage point change. Girls are 0.85 percentage points, or about 54%, more likely to choose their own parent's occupation, and 0.1 percentage points, or about 6.4%, more likely to choose their neighbor's occupation. The relative increase is about half that for boys off of a much lower base. This smaller effect size likely reflects the changing nature of female employment during this time period, and the lower overall propensity to participate in the labor force at all compared to men. In the Appendix, we perform a similar set of robustness tests as we do for the doctors and find that the results are robust to alternative specifications (Table A5), using street addresses to account for measurement error (Table A15),and examining earlier outcomes in 1920 and 1930 (Table A6).[16]

5.1 What Occupation Characteristics Matter?

To estimate heterogeneity by occupation characteristics, we re-estimate the stacked regression from Equation 5 with census sheet fixed effects but restrict the sample to only include stacked panels for occupations that have particular characteristics. Because of the more limited employment opportunities and lower labor force participation for women in the time period, we focus this section on our sample of boys and associated occupations. We focus on variation in occupational income levels and education requirements, as captured by occupation income scores and education scores. We plot the next door neighbor effects, scaled by

[16] As described above, our baseline analysis does not consider farmer owners or farmer laborers. We find larger effect sizes when we include farm owners and laborers among our occupations in the stacked regressions (Appendix Table A9).

the untreated mean, in Figure 4.[17] Exposure effects for high income occupations are slightly larger on average, around 12 percent, while effects for high education occupations are slightly lower, at 9 percent. However, there are stark difference across the four mutually exclusive groups when interacting income and education level. Exposure effects are larger for high income occupations that have higher and lower education requirements, although the point estimate is larger for high income occupations with low educational attainment. Effects are also larger for low-income, low education occupation. However the exposure effects for low income jobs that require high educational attainment (like teacher or clergyman) are significantly lower, at less than 5 percent.[18]

6 Exposure Effects on Economic Outcomes

Growing up next to someone in a particular occupation changes a child's likelihood of working in that occupation, but does this change the child's long-run outcomes? Some high-income occupations, such as doctor and lawyer have higher exposure effects, (see Figure 3), but several low-income occupations, such as brickmason, tailor, and waiter are also highly transmissible. It is possible that the type of neighbor you grow up next to could affect a child's earnings and educational attainment as an adult. Alternatively, growing up next door to a neighbor in a particular occupation may have little effect on a child's economic outcomes if, absent exposure to the neighbor's occupation, the child would have gone into a different occupation that had similar economic characteristics. For instance, growing up next door to a doctor may induce children who would otherwise become lawyers to become doctors instead, leading to only modest changes to the child's income and educational attainment. We consider income and education effects for both boys and girls, whose economic outcomes

[17] Unscaled estimates, along with estimates for having someone of a particular occupation in your own household are provided in Appendix Table A8.

[18] We explore other occupational characteristics, such as heterogeneity by occupational self-employment rates, occupations with apprenticeship structures, or whether occupations are growing or in decline but find few meaningful differences, suggesting our main neighbor spillover finding is likely not driven by direct transmission of business, human capital, or structural labor market changes (see Table A10.

may have been even more elastic to network effects than boys at the time.

6.1 Effects on Adult Income

Using the same approach as in Equation 4 but for each of the top 50 male occupations, we estimate the effect of living next door to someone of a particular occupation in 1910 on wage income in 1940.[19] As seen in Figure 5, many neighbors' occupations have a significant impact on boy's adult income and there is substantial heterogeneity. Living next to a lawyer or doctor during childhood is associated with a $45-50 (1940$) increase in annual earnings in 1940, relative to other children on the census sheet. Relative to average annual income of $1,071 in 1940 among similarly-aged (30-50) working men, this represents a 4.2-4.7 percent increase in annual income. Importantly, this is relative to other children who were living in the same neighborhood in 1910, and thus accounting for socioeconomic neighborhood sorting.

There are other high-income occupations such as manager and locomotive engineer that also lead to increases in the neighbor child's adult income, but there are also low- or middle-income occupations such as teacher, clergyman, and clerical worker that lead to significant increases in neighbor children's adult income. Once again, since these estimates are relative to other children living the same neighborhood, this likely speaks to the occupation choice counterfactual. Teachers in general did not live in the most affluent neighborhoods, but living next door to a teacher exposed a child to opportunities that were relatively better than their peers.

There are also occupations that significantly *reduced* next door children's wage income in 1940 relative to other neighbor children. Living next to a porter, a truck driver, or a laborer—all of which are low-income occupations—led to annual income reductions at least half as large as the gain from living next to a lawyer or doctor.

[19]The 1940 census only collected information on wage income. Many workers in high skill occupations such as doctors, lawyers, managers, and real estate agents report no wage income, but report that they had non-wage income (but not the amount). If anything, this under-reporting attenuates the results for high skill occupations.

Girls also have a few professions whose influence increases income. Living next to a milliner, bookkeeper, or stenographer during childhood increases annual earnings by $15-20, a 7-9 percent increase relative to average annual wage income ($204) and a 2-3 percent increase relative to average annual wage income among employed women. These results likely differ in magnitude from men because of differences between men and women in the extensive margin decision to participate in the labor force during this time period, which is influenced by neighbors' occupations, as seen in Figure 3b. Similar to boys, living next to laborers or private laundresses actually led to lower annual income relative to other girls on the same sheet.

Although it is clear that living next to someone in a particular occupation as a child has an impact on future earnings, the channels through which this operates is not clear. It could all be driven by an occupation match effect if, for example, growing up next to a doctor increased a child's likelihood of becoming a doctor, but had no other effects. However, it is also possible that having a doctor for a neighbor increases exposure to information about high-paying jobs in general, or the human capital requirements necessary to qualify for a high-paying job. As noted above, there are some low income, high education occupations, such as teacher or clergyman, that are not very transmittable to neighbor children (see Figure 3) but do lead to income gains (see Figure 5). For this reason, we next explore how childhood neighbors' occupations affect children's educational attainment.

6.2 Effects on Educational Attainment

In Figure 6, we find that living next door to someone of a particular occupation has a similarly heterogeneous impact on boy's educational attainment. Living next to a doctor or lawyer in 1910 increased average years of schooling by nearly 0.4 relative to other children on the census sheet. But even many of the low income, high education occupations that led to income gains lead to increases in educational attainment as well. The occupations that have the largest effects on neighbor children's educational attainment are occupations

that require formal education, while living next to a neighbor in some trade occupations, such as lumberman, miner, teamster, or laborer, actually reduced children's educational attainment. Figure 6b shows similarly large effects for many professional occupations with educational requirements for girls. Girls living next to nurses, bookkeepers, and teachers attain about 0.2 more years of education than the other girls on the street. Living next to some occupations decreases educational attainment, including laundresses, laborers, and kindred workers. These effects speak to the network and mentoring channel of occupation exposure.

Since neighborhood exposure to many occupations changes a child's ultimate educational attainment, it seems plausible that growing up next door to a doctor changes more than just the child's probability of becoming a doctor. In Figure 7 we plot the scaled next door coefficient from Equation 4 for boys, but change the outcome to be a binary measure for being in each of the top five highest paying, large occupations, including lawyer, manager/official/proprietor, foreman, or compositor/typesetter. We find that living next to a doctor in 1910 significantly increases the probability of being a lawyer (16 percent) or manager (8 percent) in 1940 relative to other boys on the 1910 census sheet, but it does not increase the probability of being in a high paying, low education occupation such as foreman or compositor/typesetter. Living next door to a doctor as a child significantly increased the probability that the child entered a high-paying, high-skilled occupation that requires educational training, but not high-paying manual or trade occupations. This is consistent with the effect on increased educational attainment. The benefit of growing up next to someone in a high-income occupation, like doctor, seems both general, directing children to occupations that require more education and training, but also specific, having the largest impact on children's decisions to become doctors. The implications are similar when looking at teachers, a low income, high education occupation. In Figure 8 we examine how growing up next to a teacher affects a boy's probability of being in one of the five occupations that have the highest educational attainment (doctor, lawyer, clergyman, insurance agent, and

teacher). Consistent with these low wage, high education jobs inducing neighbors to attain more education, we see that growing up next to a teacher increases the probability of being a doctor as much or more than being a teacher, but also significantly increases the probability of being a lawyer or insurance agent.

7 Why Neighbors Matter: Social Ties and Homophily

Given the strong transmission of occupation, income, and education from childhood neighbors, we next explore what makes neighbors matter. To do this, we build on the stacked regression analysis discussed above but explore heterogeneity by neighborhood characteristics and the characteristics of the actual neighbor to understand if measures correlated with network strength and the level of interaction matter.

7.1 Neighborhood Characteristics

The interconnectedness of neighborhoods might affect the magnitude of exposure effects. We examine three measures that potentially capture dimensions of neighborhood interaction and connectedness: county-level urbanicity, immigrant share, and the out-of-state share. Neighborhoods that are less transient and have more permanent residents are likely to have stronger social ties. For this reason, we explore heterogeneity by the share of the population that was not born in that state, or the out-of-state share. If network strength matters, we expect to see neighbor exposure effects to be stronger in counties with fewer out-of-state residents. For urbanicity, the theoretical implications are more agnostic. In urban areas, residences are likely to be geographically close together, and so individuals may have more frequent interactions with their neighbors. At the same time, because of this proximity, children may be able to interact with more distant neighbors more easily in urban areas. It is also possible that urban life is more anonymous, and so interactions are more formative in rural areas (Dunkelman, 2017; Wirth, 1938). This is similar for immigrant shares. Households may interact more in immigrant enclaves (Damm, 2009), but immigrant communities

could also provide alternative networks, making next door neighbors relatively less important for the transmission of information about occupations.[20]

We present the scaled next door neighbor coefficients split by county-level urbanicity, immigrant share, and the out-of-state share in Figure 9. We find that exposure effects are significantly larger in neighborhoods that are more rural, have lower immigrant shares, and that have more residents who were born in the state (low out-of-state share). This patterns are consistent with more homogenous places with stronger connectedness facilitating occupational transmission.[21]

7.2 Individual Characteristics

Given the heterogeneity across neighborhoods, it is plausible that there is heterogeneity across characteristics of the neighbors themselves. In particular, we test whether homophily among neighbors makes exposure effects stronger. To do this, we modify equation (5) but allow the effects to vary if the focal child and neighbor have the same characteristics, as follows

$$\begin{aligned}\text{Occ1940}_{ijs} =& \alpha + \beta_0 \text{OwnOcc1910}_{ij} + \beta_1 \text{NextDoorOcc1910}_{ij} \times \text{SameCharacteristic}_{ij} \\ &+ \beta_2 \text{NextDoorOcc1910}_{ij} \times \text{DifferentCharacteristic}_{ij} + \text{Occ}_j \times \text{Age}_i \\ &+ \text{Occ}_j \times \gamma_s + \epsilon_{ijs}, \end{aligned} \tag{6}$$

where $\text{SameCharacteristic}_{ij}$ is set equal to one if i has a next door neighbor in occupation j who is the same as i along various demographic and economic characteristics. Similarly, $\text{DifferentCharacteristic}_{ij}$ equals one if i has a next door neighbor in occupation j

[20] Living in an area with a large share of immigrants might be particularly important if the focal individual themselves comes from an immigrant family or shares an ethnicity with their neighbors. We explore the importance of the similarity between two neighbors in the next section.

[21] In Appendix Table A10, we explore heterogeneity among other neighborhood characteristics, including the share of non-Whites, whether or not there is an institution of higher education in the same county, and region of the U.S. In all specifications, we find positive average transmission effects, suggesting that most types of neighborhoods in this time period were facilitating occupational spillovers.

who is different along a characteristic. For example, when considering racial homophily, a Black boy living next to a White doctor would have $\text{SameCharacteristic}_{ij} = 0$ and $\text{DifferentCharacteristic}_{ij} = 1$.

In Table 6 Column 2, we estimate exposure effects for next door neighbors who are and are not from the same birthplace, using the state of birth variable in the census. Boys are more likely to enter the occupation of their next door neighbor when the neighbor is born in the same state, 0.47 percentage points more likely for neighbors from the same state versus 0.23 percentage points more likely for neighbors from different states. We present F-test statistics showing that these two coefficients are strongly significantly different from one another. We see similar patterns in Column 3 when we examine differences by birth country (with all native-born grouped together).

In Column 4, we examine whether the race of the next door neighbor affects the magnitude of the exposure effect. Children with same-race next door neighbors are 0.38 percentage points more likely to enter into the occupation of that neighbor. Children are 0.83 percentage points *less* likely to go into the occupation of their different-race neighbors. Importantly, this negative effect must be interpreted relative to the appropriate counterfactual group. These coefficients compare the occupational choice of a different-race child relative to the choices of children who live further away, but still on the same street. Racial segregation at the time is high, so these comparison children are more likely to share the same race as the person in the target occupation. For example, a non-White child living next to a White doctor is less likely to become a doctor than other (mostly White) children who grew up on the same street. [22] In Appendix Table A11 we also show differences by household income, household education, and last name homophily.

In Column 5 we test whether the presence of a child with the same age in the next door household affects the magnitude of the exposure effect. If children are more likely

[22]In unreported analysis, we restrict to sub-samples of children of the same race, and find that non-White children have statistically zero next-door effects when living next to a White adult of the target occupation. Because of strong racial segregation in 1910, there are very few cases of mixed race neighborhoods, making it difficult to estimate cross-race effects with much statistical power.

to interact with other children of the same age on their streets, then they are also more likely to be exposed to the occupations of the parents of these similar-age children. Recent studies indicate that the parents of children's peers are important in determining educational outcomes in modern settings (Chung, 2020; Fruehwirth and Gagete-Miranda, 2019). We do find that next door neighbors are more predictive of children's future occupations when they have a child of the same age in the household relative to next door neighbors without a same age child, although the differences in magnitude are modest (0.35 percentage points versus 0.31 percentage points).

We also explore dimensions of gender homophily. To this point we have estimated the effect of a neighbor's occupation regardless of their gender. We now re-estimate a version of equation (5), but in place of the next door indicator include two mutually exclusive indicators that equal one if the neighbor is in the target occupation and either male or female. We provide these results for our sample of boys and girls in Table 7. For both boys and girls we find significant exposure effects from both male and female neighbors. However, the exposure effects for boys are three times as large if the neighbor is male relative to female, while the exposure effects for girls are twice as large if the neighbor is female, relative to male. Consistent with recent evidence of gender homophily in friendships, we find that exposure effects are larger when gender's match. Taken together, the heterogeneity by neighborhood and neighbor characteristics are consistent with exposure effects being larger when social ties are potentially stronger.

8 Conclusion

Where an individual spends their childhood has important implications for their future economic success (Chetty et al., 2018). Using neighborhood microgeography reconstructed from historic, door-to-door census enumeration, we show that part of this can be explained by the composition of neighbors a child grew up next to. Among boys in 1910, living next door to someone in a particular occupation, increased the likelihood that they worked in

that occupation in 1940 by 10 percent, relative to other boys who were living on the same street. Girls similarly had positive transmission of occupations, albeit concentrated among different occupations and at slightly lower rates.

This neighbor-to-neighbor transmission of occupation varies across occupation, with larger exposure effect from neighbors in high income, high education occupations. The occupations of childhood neighbors also have long-term economic impacts on children. If a person's childhood neighbors were in particular occupations (such as doctor, lawyer, teacher, or clerical worker), the child experienced higher income and educational attainment as an adult, relative to other children that grew up on the same street. Part of this comes through the direct effect of occupation matching, but growing-up next door to neighbors in more educated occupations also increases educational attainment and the likelihood of working in a high-education occupation. At the same time, some occupations such as truck driver and laborer actually led to reductions in income and educational attainment for neighboring children. Positive transmission effects on education are striking for girls, who in this time period were beginning to enter professional occupations at higher rates and perhaps benefited uniquely by networking with older professional women in their neighborhood.

Overall, our results suggest that childhood neighbors matter, and who you grew up with can help explain some of the effect of place on children's long-run economic mobility. We find that neighbor exposure effects are larger in neighborhoods that are plausibly more connected and among neighbors who share common characteristics such as similar-aged children, place of origin, and race. These patterns emphasize the role of social ties and connectedness and are consistent with both information and exposure channels. Interacting with someone in a high-income or high-education occupation can make the returns of that occupation salient or remove information barriers that keep people from being eligible to work in those occupations. However, these effects could also be simply driven by exposure. Children might not know that a particular occupation exists in their choice set unless they know someone in that occupation. Although social connectedness between physical neighbors might be weaker

now than in the past, these patterns suggest that the composition of socially connected adults in the neighborhood and community influence children's long-term outcomes.

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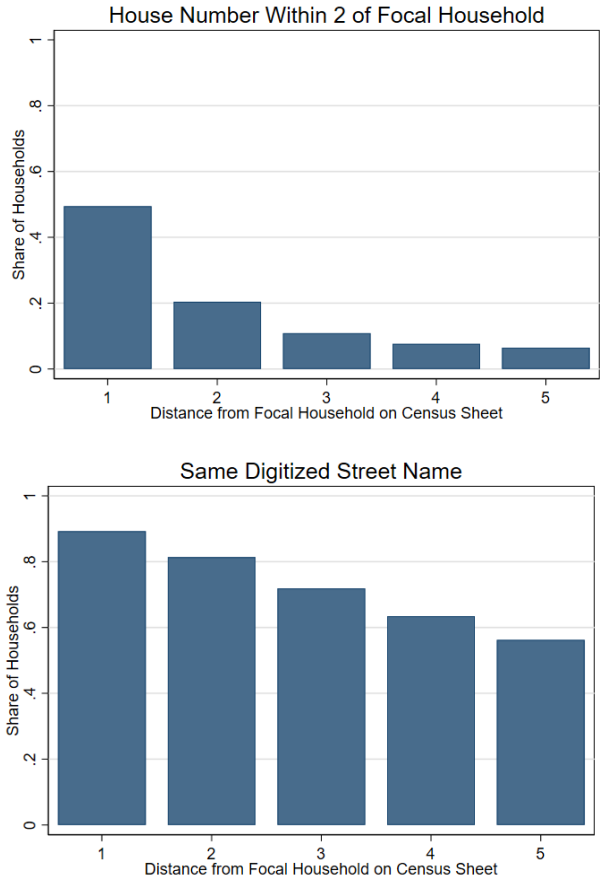
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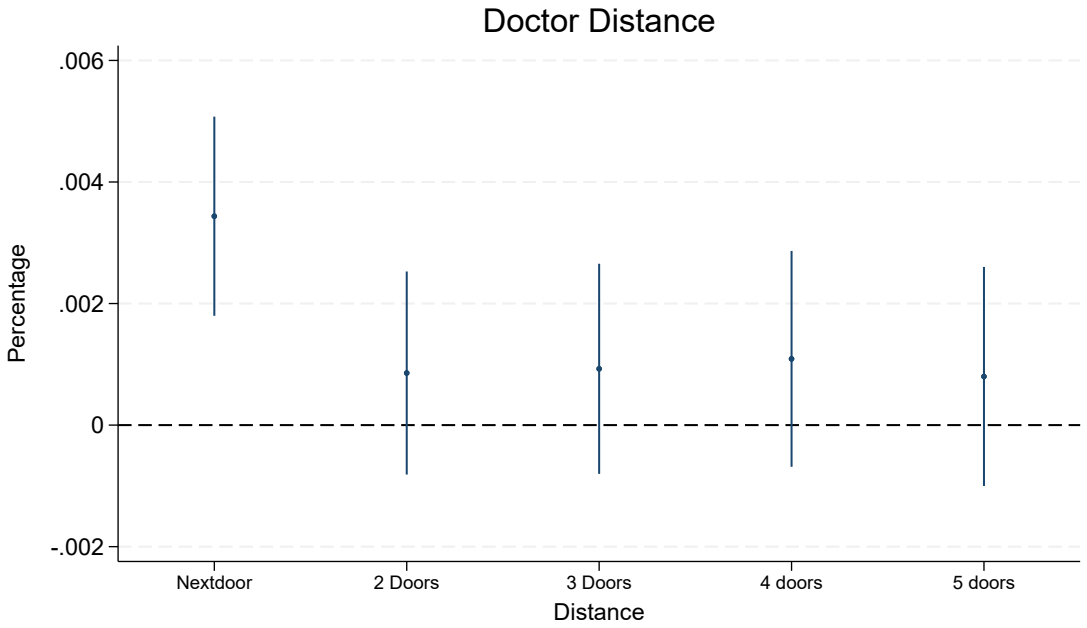
Tables and Figures

Figure 1: Sheet Proximity and Address Measures



Notes: Observation at the household level from the 1910 full count census. Sample restricted to enumeration districts where 80 percent of households have digitized address information. In the top panel, for each household, the share of households where the house number is within 2 digits of the house number is plotted by the number of households apart based on the sheet ordering definition. In the bottom panel, for each household, the share of households that are on the same street is plotted by the number of households apart based on the sheet ordering definition.

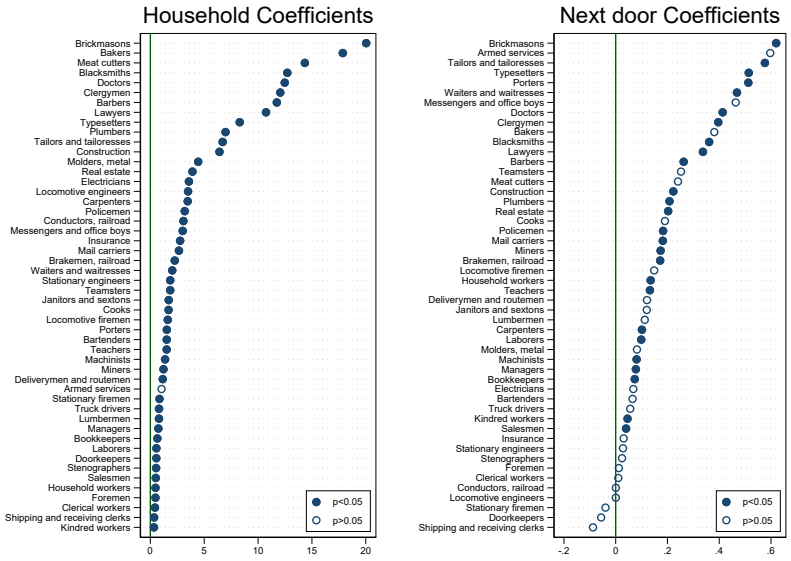
Figure 2: NeighborDoc1910 Coefficients



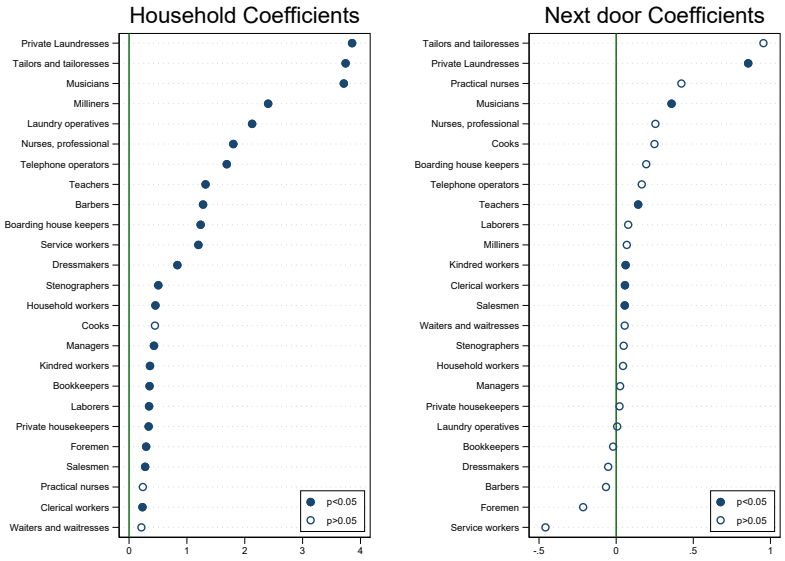
Notes: Sample restricted to boys between the ages of 5 and 18 in 1910 that can be linked to the 1940 census observation using the Census Tree links. The outcome is a binary measure that equals one if the boy is a doctor in 1940. This is regressed on binary measures that equal one if the boy's family lived next door, 2 doors away, three doors away, 4 doors away, or 5 doors away from a doctor in 1910, respectively, as well as a binary measure that equals one if the child had a doctor in their own household. These coefficients are plotted with 95 percent confidence intervals. The coefficient on having a doctor in their own household is not plotted. Census sheet fixed effects are also included, making this a comparison between boys who lived in the same local area that lived close to a doctor versus slightly further away. Standard errors are corrected for clustering at the household level.

Figure 3: Household and Next Door Coefficients, Individual Occupations

(a) 50 Largest Occupations for Men



(b) 25 Largest Occupations for Women



Notes: These figures report the results of next door regressions for each occupation run separately. The outcome is a binary measure that equals one if the child reports working in that occupation in 1940. The independent variable of interest is an indicator for living next door to someone with that occupation in 1910. Each marker represents the next door coefficient scaled by the untreated mean of the outcome. Markers are filled if the coefficient is statistically significant at the 5% level. All regressions also include own household indicators (not reported) and age fixed effects.

Figure 4: Heterogeneity by Occupation Type

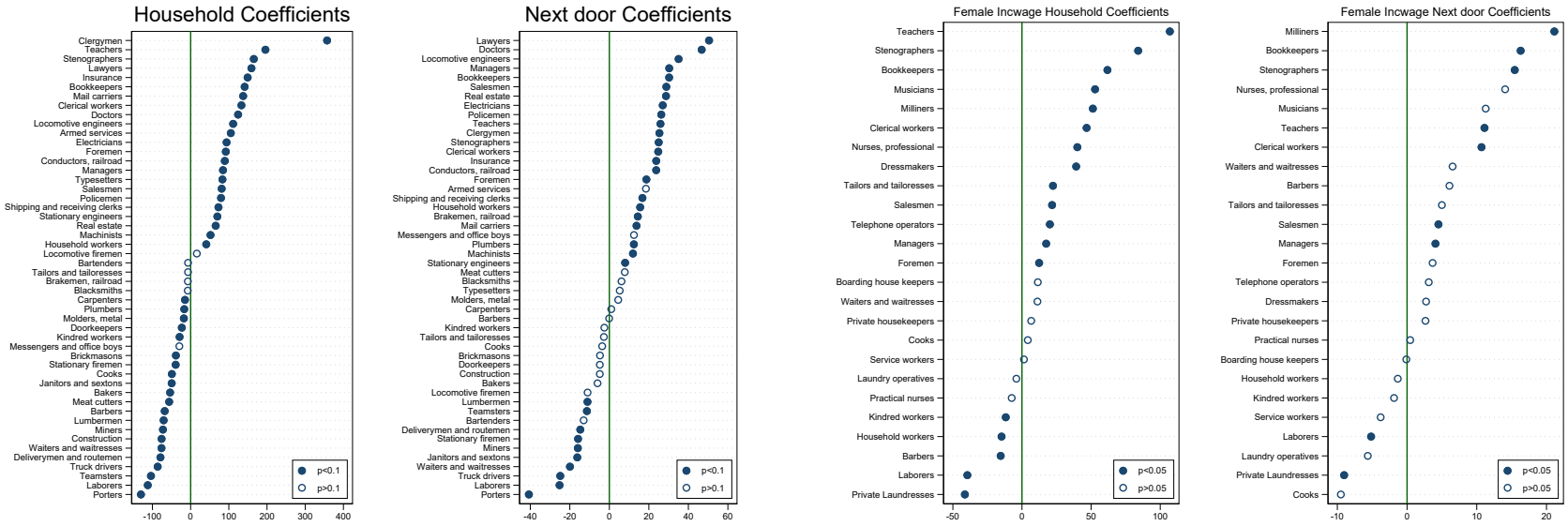


Notes: Each bar reports the next door coefficient scaled by the untreated mean from the stacked regression specification described in equation 5. Each regression sample is restricted to the subset of occupations in the stack denoted on the y-axis. Sample of interest is boys 5-18 years old in 1910 that are linked to the 1940 census. Error bars represent the 95% confidence interval (scaled using the the delta method). The occupation groups are divided using the median income and education occupation scores for each occupation from IPUMS.

Figure 5: Household and Next Door Coefficients on Income

(a) 50 Largest Occupations for Men

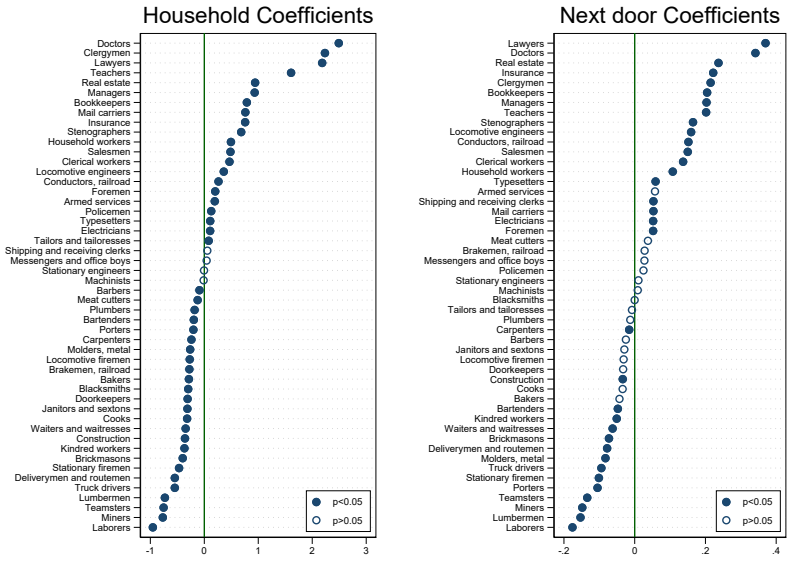
(b) 25 Largest Occupations for Women



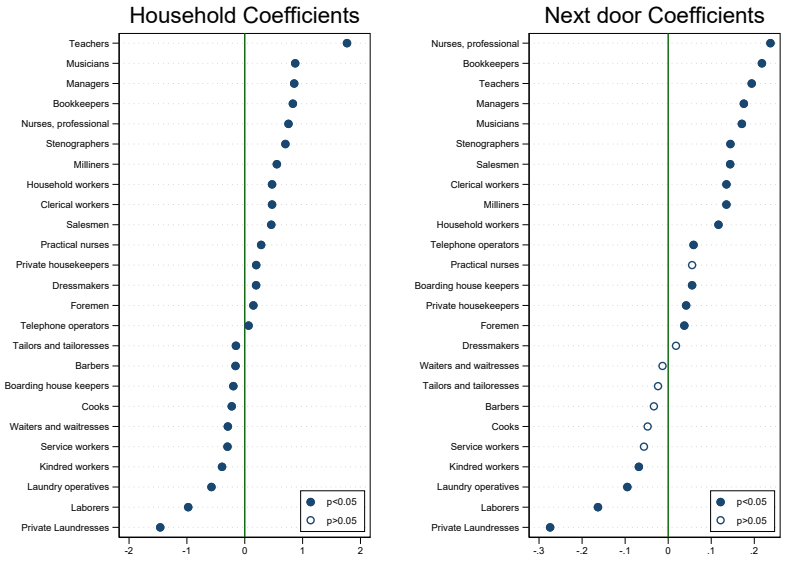
Notes: These figures report the results of income on next door regressions run for each occupation separately. The outcome is reported income in the 1940 census. The independent variable of interest is an indicator for living next door to someone with that occupation in 1910. Each marker represents the next door coefficient scaled by the untreated mean of the outcome. Markers are filled if the coefficient is statistically significant at the 5% level. All regressions also include own household indicators (not reported) and age fixed effects.

Figure 6: Household and Next Door Coefficients on Education

(a) 50 Largest Occupations for Men

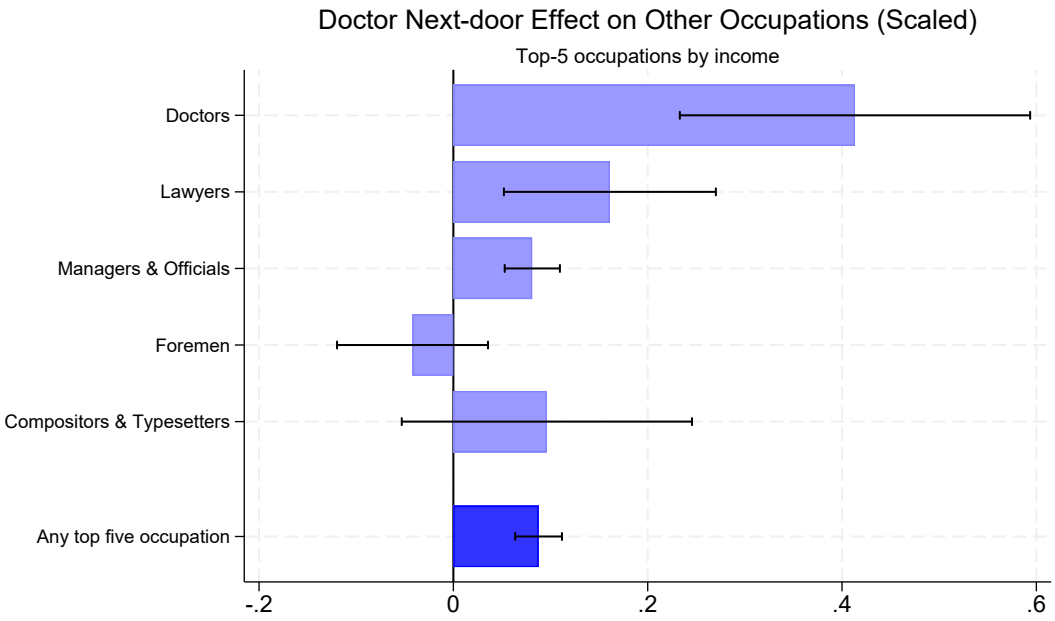


(b) 25 Largest Occupations for Women



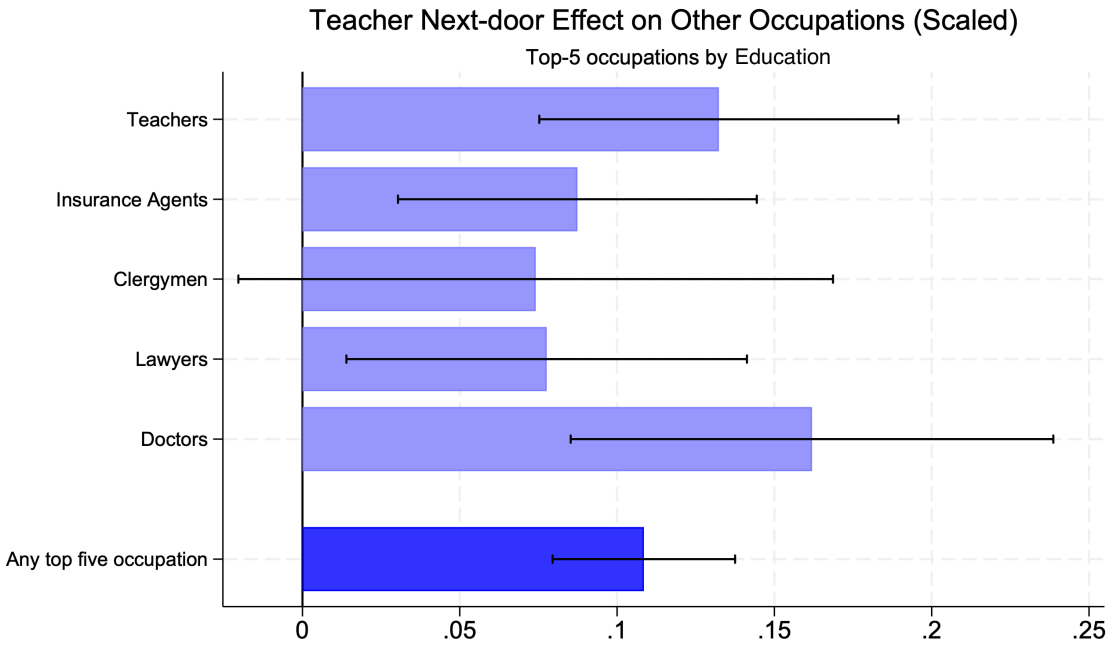
Notes: These figures report the results of education on next door regressions run for each occupation separately. The outcome is years of educational attainment reported in the 1940 census. The independent variable of interest is an indicator for living next door to someone with that occupation in 1910. Each marker represents the next door coefficient scaled by the untreated mean of the outcome. Markers are filled if the coefficient is statistically significant at the 5% level. All regressions also include own household indicators (not reported) and age fixed effects.

Figure 7: Alternative Career Choices of Boys Near a Doctor



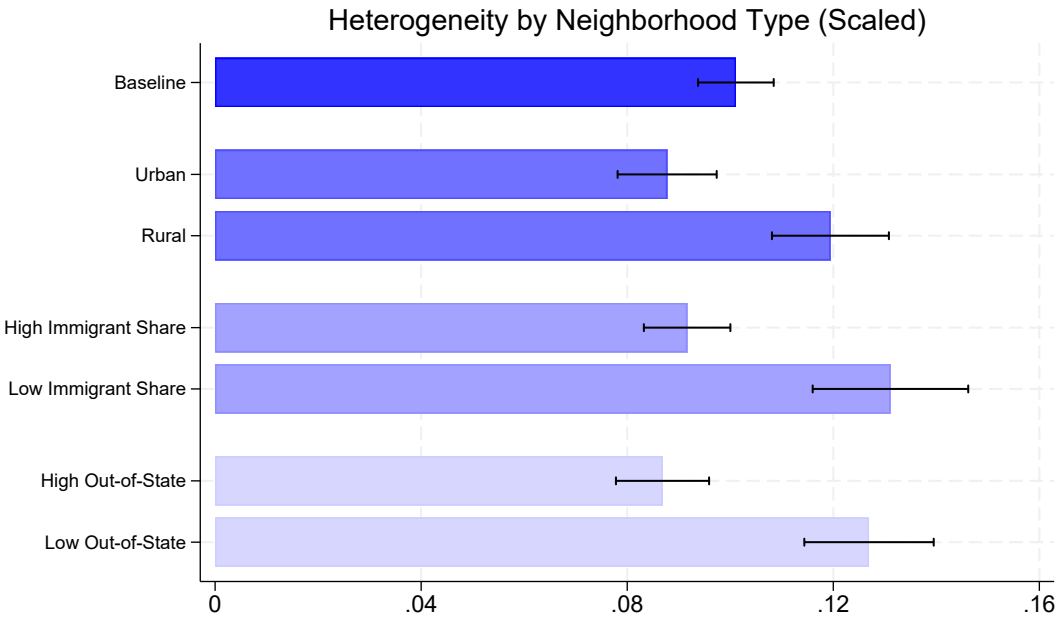
Notes: Each bar represents the next door coefficient scaled by the untreated mean outcome from a variant of equation 4. For each regression, the doctor outcome is replaced by alternate occupation choices. The independent variable of interest is living next door to a doctor in 1910. Occupations are selected as the five occupations with the highest occupation scores in the 1950 census from IPUMS. Sample is boys 5-18 years old in 1910 that are linked to the 1940 census. Error bars represent the 95% confidence interval (scaled using the the delta method).

Figure 8: Alternative Career Choices of Boys Near a Teacher



Notes: Each bar represents the next door coefficient scaled by the untreated mean outcome from a variant of equation 4. For each regression, the doctor outcome is replaced by alternate occupation choices. The independent variable of interest is living next door to a teacher in 1910. Occupations are selected as the five occupations with the highest education scores in the 1950 census from IPUMS. Sample is boys 5-18 years old in 1910 that are linked to the 1940 census. Error bars represent the 95% confidence interval (scaled using the the delta method).

Figure 9: Neighborhood Characteristics: Connectedness



Notes: Each bar reports the next door coefficient scaled by the untreated mean from the stacked regression specification described in equation 5. Each regression sample is restricted to groups of sheets that fit the description on the y-axis. Sample of interest is boys 5-18 years old in 1910 that are linked to the 1940 census. Error bars represent the 95% confidence interval (scaled using the the delta method).

Table 1: Summary Statistics

	All 1910 children	1910 - 1940 Linked children	1910 Boys	1910 - 1940 Linked boys	1910 Girls	1910 - 1940 Linked girls
	(1)	(2)	(3)	(4)	(5)	(6)
Age in 1910	11.380	11.244	11.366	11.228	11.395	11.268
Male	0.503	0.603	1.000	1.000	0.000	0.000
Nonwhite	0.130	0.047	0.128	0.060	0.132	0.027
Rural	0.603	0.646	0.611	0.634	0.595	0.666
Northeast	0.252	0.226	0.250	0.240	0.253	0.204
Midwest	0.315	0.378	0.316	0.368	0.314	0.395
South	0.371	0.324	0.371	0.323	0.371	0.325
West	0.063	0.072	0.063	0.070	0.062	0.076
Head of household						
Married	0.884	0.914	0.883	0.907	0.885	0.923
Foreign born	0.311	0.277	0.311	0.292	0.310	0.255
Income score	21.975	22.107	21.899	22.170	22.051	22.011
Education score	12.342	12.867	12.124	12.712	12.563	13.103
Total	26,161,014	10,529,180	13,146,449	6,346,719	13,014,565	4,182,461

Notes: This table shows summary statistics for the sample data. Column 1 describes all children age 5-18 in the 1910 census. Column 2 describes the children linked from the 1910 census to the 1940 census. Columns 3-4 further restricts the sample to boys only, while columns 5-6 restrict to girls only.

Table 2: Baseline Doctor Results

		At least one doctor occupation per sheet		
Dependent variable: Doctor occupation in 1940	All sheets	No geographic FE	City - enumeration district FE	Sheet FE
	(1)	(2)	(3)	(4)
Own household	0.0983***	0.0954***	0.0963***	0.0970***
	0.0018	0.0019	0.0018	0.0018
Next door neighbor	0.0072***	0.0036***	0.0033***	0.0032***
	0.0005	0.0006	0.0006	0.0007
R-squared	0.011	0.046	0.138	0.327
Untreated mean	0.0038	0.0078	0.0078	0.0078
Observations	6,335,660	305,059	305,059	305,059

Notes: Column 1 includes all sheets in 1910 census, while columns 2-4 include only sheets with at least one adult in the target occupation. All columns include age fixed effects.
*p<0.1, **p<0.05, ***p<0.01.

Table 3: Street-level Sorting: Doctor Results

Dependent variable: Doctor in 1940	Sheet ordering: Baseline (1)	Sheet ordering: Street sample (2)	Street ordering (3)	Street order IV (4)
Own household	0.0970***	0.1269***	0.1176***	0.1278***
	0.0018	0.0045	0.0040	0.0045
Next door neighbor	0.0032***	0.0036*	0.0028**	0.0087**
	0.0007	0.0019	0.0014	0.0043
R-squared	0.283	0.369	0.200	0.012
Untreated mean	0.0038	0.0061	0.0061	0.0061
Observations	6,044,153	1,053,150	1,053,150	1,053,150

Notes: Columns 1-2 use the ordering of census sheets to define neighborhood proximity. Columns 3 uses street ordering. Column 4 is an instrumental variables regression with street order variables instrumenting for sheet order variables.

*p<0.1, **p<0.05, ***p<0.01.

Table 4: Doctor Deaths Placebo Check

Dependent variable: Doctor occupation in 1940	One Doctor Per Sheet (1)	One or More Doctors Per Sheet (2)	One Doctor Per Sheet Less Than 60 (3)
Own household: doctor died	0.0439***	0.0480***	0.0479***
	0.0058	0.0059	0.0070
Next door neighbor: doctor died	-0.0004	0.0025	-0.0020
	0.0024	0.0026	0.0033
Own household: doctor *did not* die	0.1047***	0.1091***	0.1076***
	0.0023	0.0022	0.0024
Next door neighbor: doctor *did not* die	0.0031***	0.0033***	0.0032***
	0.0009	0.0009	0.0010
Share of children living on a sheet with a doctor that died	0.0564	0.0600	0.0390
R-squared	0.322	0.326	0.324
Untreated mean	0.0071	0.0075	0.0072
Observations	123,833	141,794	111,446

Notes: Regressions use the baseline doctor sample but exclude doctors that have a missing death date. Column 3 further restricts to doctors less than age 60 in 1910. All columns include age and sheet fixed effects.

*p<0.1, **p<0.05, ***p<0.01.

Table 5: Baseline Stacked Regression Results

(a) Men

Dependent variable: Target occupation in 1940	All sheets	At least one person in focal occupation per sheet		
		No geographic FE	City - enumeration district FE	Sheet FE
	(1)	(2)	(3)	(4)
Own household	0.0548***	0.0467***	0.0424***	0.0381***
	0.0001	0.0002	0.0002	0.0002
Next door neighbor	0.0198***	0.0112***	0.0070***	0.0034***
	0.0001	0.0001	0.0001	0.0001
R-squared	0.038	0.048	0.087	0.323
Untreated mean	0.0100	0.0331	0.0331	0.0331
Observations	316,783,008	26,649,908	26,649,908	26,649,908

Notes: Column 1 includes all sheets in 1910 census, while columns 2-4 include only sheets with at least one adult in the target occupation. All columns include age fixed effects.
*p <0.1, **p <0.05, ***p <0.01.

(b) Women

Dependent variable: Target occupation in 1940	All sheets	At least one person in focal occupation per sheet		
		No geographic FE	City - enumeration district FE	Sheet FE
	(1)	(2)	(3)	(4)
Own household	0.0158***	0.0116***	0.0095***	0.0085***
	0.0001	0.0001	0.0001	0.0002
Next door neighbor	0.0087***	0.0041***	0.0020***	0.0010***
	0.0001	0.0001	0.0001	0.0001
R-squared	0.011	0.018	0.083	0.356
Untreated mean	0.0072	0.0156	0.0156	0.0156
Observations	104,389,272	11,135,893	11,135,893	11,135,893

Notes: Column 1 includes all sheets in 1910 census, while columns 2-4 include only sheets with at least one adult in the target occupation. All columns include age fixed effects.
*p <0.1, **p <0.05, ***p <0.01.

Table 6: Heterogeneity by Neighbor Similarity

Dependent variable: Target occupation in 1940	Baseline	Birthplace	Birth country	Race	Same age
	(1)	(2)	(3)	(4)	(5)
Own household	0.0381***	0.0381***	0.0381***	0.0381***	0.0382***
	0.0002	0.0002	0.0002	0.0002	0.0002
Next door neighbor	0.0034***				
	0.0001				
Treatment 1: Same characteristic		0.0047***	0.0040***	0.0038***	0.0035***
		0.0002	0.0002	0.0001	0.0003
Treatment 1 average		*0.0984*	*0.1595*	*0.2284*	*0.0311*
Treatment 2: Different characteristic		0.0023***	0.0019***	-0.0083***	0.0031***
		0.0002	0.0002	0.0006	0.0001
Treatment 2 average		*0.1453*	*0.0829*	*0.0086*	*0.1639*
F-test		100.28	66.90	404.93	1.60
Two-tailed p-value		0.0000	0.0000	0.0000	0.2058
R-squared	0.323	0.323	0.323	0.323	0.323
Untreated mean	0.0331	0.0331	0.0331	0.0331	0.0338
Observations	26,649,908	26,649,908	26,649,908	26,649,908	26,649,908

Notes: In columns 2-5, treatments compare head of household of the focal child to the target occupation holder next door. In column 5, same age is defined as any child in the target occupation house being within one year of the focal child.
*p<0.1, **p<0.05, ***p<0.01.

Table 7: Heterogeneity by Gender Homophily

Dependent variable: Target occupation in 1940	Boys Baseline	Boys Homophily	Girls Baseline	Girls Homophily
	(1)	(2)	(3)	(4)
Own household	0.0381***		0.0085***	
	0.0002		0.0002	
Next door neighbor	0.0034***		0.0010***	
	0.0001		0.0001	
Male own household		0.0431***		0.0051***
		0.0002		0.0002
Male next door neighbor		0.0037***		0.0008***
		0.0001		0.0002
Female own household		0.0095***		0.0149***
		0.0003		0.0003
Female next door neighbor		0.0012***		0.0014***
		0.0003		0.0002
R-squared	0.323	0.323	0.356	0.356
Untreated mean	0.0331	0.0331	0.0156	0.0156
Observations	26,649,908	26,649,908	11,135,893	11,135,893

Notes : This analysis uses the stacked regression framework. Columns 2 and 4 separate the treatment variables by the gender of the adult in the target occupation. All columns include age fixed effects.
*p <0.1, **p <0.05, ***p <0.01.

A Additional Tables and Figures (Online Appendix)

Figure A1: Household and Next Door Coefficients for 50 Largest Occupations for Men, Without Scaling by Mean of Untreated Kids

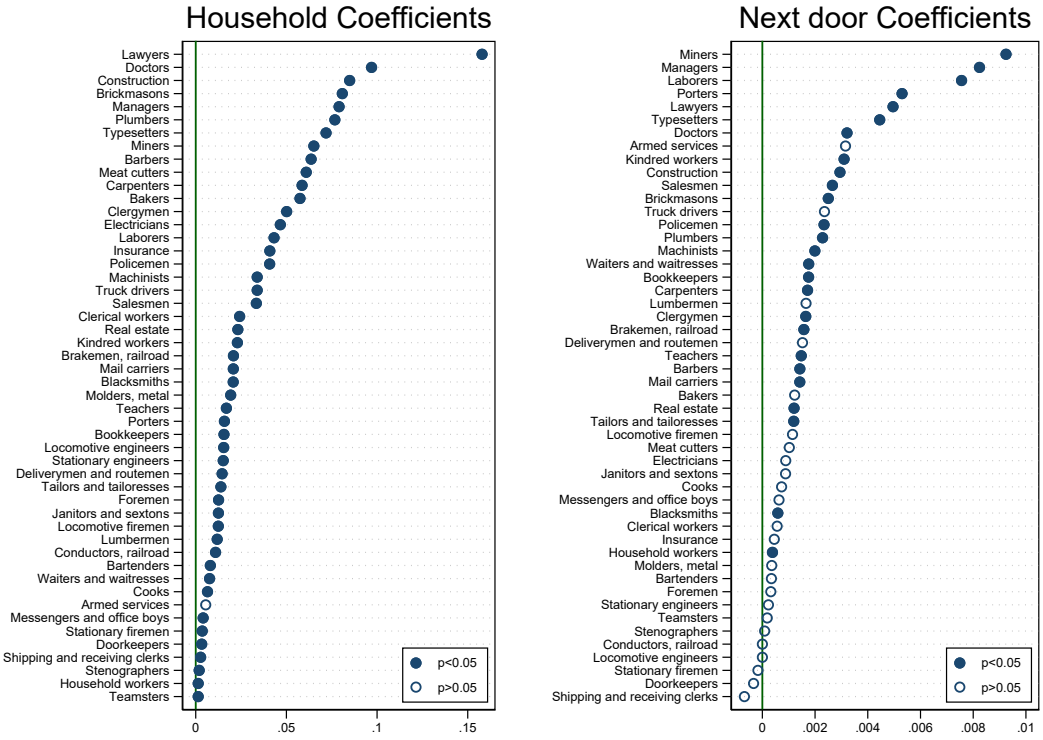


Table A1: Doctor Results Linked to Various Censuses

Dependent variable: Doctor in…	1920	1930	1940
	(1)	(2)	(3)
Own household	0.0164***	0.0852***	0.0970***
	0.0007	0.0016	0.0018
Next door neighbor	0.0008***	0.0025***	0.0032***
	0.0003	0.0006	0.0007
R-squared	0.266	0.321	0.327
Untreated mean	0.0014	0.0065	0.0078
Observations	410,385	323,151	305,059

Notes: Columns 1-3 uses the outcome of being a doctor in 1920-1940. Sample excludes sheets with no doctors in 1910.
*p<0.1, **p<0.05, ***p<0.01.

Table A2: Doctor Results: Robustness

Dependent variable: Doctor occupation in 1940	Baseline	One doctor per sheet	Drop group quarters	Include sheet position fixed effects	Drop last sheet	Drop last two sheets	Drop last five sheets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own household	0.0970***	0.0912***	0.0979***	0.0970***	0.0969***	0.0964***	0.0960***
	0.0018	0.0020	0.0018	0.0018	0.0018	0.0018	0.0019
Next door neighbor	0.0032***	0.0024***	0.0031***	0.0032***	0.0029***	0.0028***	0.0029***
	0.0007	0.0007	0.0007	0.0007	0.0007	0.0007	0.0008
R-squared	0.327	0.319	0.327	0.283	0.282	0.282	0.282
Untreated mean	0.008	0.008	0.008	0.004	0.0038	0.0038	0.0039
Observations	305,059	268,500	300,416	6,044,153	5,882,132	5,683,285	5,030,195

Notes: Column 1 reports the baseline doctor regression for children with at least one adult doctor on the sheet, and including sheet fixed effects. Column 2 restricts to children on a sheet with a single adult doctor. Column 3 drops children living in "group quarters". Column 4 adds a fixed effect for the line the household appears on. Columns 5-6 sequentially drop the last sheets of the enumeration district which were often labeled as "supplementary" and included households that were missed during the initial enumeration and are therefore out of order. All columns include age fixed effects.
*p<0.1, **p<0.05, ***p<0.01.

Table A3: Doctor Results: Various Household Controls

Dependent variable: Doctor occupation in 1940	Baseline	Head of Household controls	Head Occupation FE	Within 3 doors of doctor	Within 3 + Head Occupation FE
	(1)	(2)	(3)	(4)	(5)
Own household	0.0970***	0.0709***	0.0284***	0.0984***	0.0300***
	0.0018	0.0021	0.0026	0.0022	0.0029
Next door neighbor	0.0032***	0.0025***	0.0027***	0.0040***	0.0031***
	0.0007	0.0007	0.0006	0.0011	0.0007
Head occscore		0.0001**			
		0.0000			
Head ed score		0.0003***			
		0.0000			
Head age		-0.0002***	-0.0002***		-0.0003***
		0.0000	0.0000		0.0000
White		-0.0039*	-0.0001		-0.0016
		0.0021	0.0011		0.0022
US Native		-0.0028***	-0.0047***		-0.0067***
		0.0010	0.0007		0.0012
education flag		0.0100***			
		0.0013			
R-squared	0.327	0.329	0.055	0.406	0.062
Untreated mean	0.008	0.008	0.008	0.008	0.008
Observations	305,059	305,047	305,036	143,141	143,133

Notes: Column 2 includes sheet fixed effects and adds controls for observable characteristics of the head of household. Column 3 inludes head of household occupation fixed effects as well as the head of household controls. Columns 4-5 restrict the sample to only individuals who live within three doors of a doctor. All columns include age fixed effects.

*p<0.1, **p<0.05, ***p<0.01.

Table A4: Doctor Deaths Placebo Check - Separate Regressions

Dependent variable: Doctor occupation in 1940	Doctor Did Not Die by 1915	Doctor Died by 1915	Missing Doctor Death Date
	(1)	(2)	(3)
Own household	0.1047***	0.0435***	0.0453***
	0.0023	0.0059	0.0037
Next door neighbor	0.0031***	-0.0003	0.0014
	0.0009	0.0024	0.0012
R-squared	0.323	0.289	0.290
Untreated mean	0.0070	0.0083	0.0079
Observations	114,086	9,747	41,839

Notes: All columns include only sheets with one doctor per sheet and all columns include age and sheet fixed effects.

*p<0.1, **p<0.05, ***p<0.01.

Table A5: Stacked Regression Results: Robustness

Dependent variable: Target occupation in 1940	Baseline	One target occ. per street	Drop Group Quarters	Sheet Position Fixed Effects	Excluding Occupations Likely to be on the Last Two Sheets
	(1)	(2)	(3)	(4)	(5)
Own household	0.0381***	0.0325***	0.0385***	0.0381***	0.0389***
	0.0002	0.0002	0.0002	0.0002	0.0002
Next door neighbor	0.0034***	0.0018***	0.0033***	0.0033***	0.0028***
	0.0001	0.0001	0.0001	0.0001	0.0001
Interaction					
R-squared	0.323	0.301	0.323	0.323	0.327
Untreated mean	0.0331	0.0264	0.0332	0.0331	0.0304
Observations	26,649,908	17,838,972	26,328,032	26,649,908	21,829,224

Notes: Column 1 shows baseline stack regression including sheet by occupation fixed effects. Column 2 includes only sheets that have one worker in the target occupation. Column 3 drops children living in group quarters. Column 4 includes position on the sheet fixed effects. Column 5 drops occupations that are most likely to appear on last two sheets, such as laborers.

*p <0.1, **p <0.05, ***p <0.01.

Table A6: Stacked Regression Results Linked to Various Censuses - Boys

(a) All Links

Dependent variable: Target occupation in…	1920	1930	1940
	(1)	(2)	(3)
Own household	0.0365***	0.0450***	0.0381***
	0.0001	0.0002	0.0002
Next door neighbor	0.0032***	0.0037***	0.0034***
	0.0001	0.0001	0.0001
R-squared	0.318	0.324	0.323
Untreated mean	0.0262	0.0329	0.0331
Observations	37,437,872	28,531,044	26,649,908

Notes: Columns 1-3 uses the outcome of being the target occupation in 1920-1940. Sample excludes sheets with no one of the target occupation in 1910.

*p<0.1, **p<0.05, ***p<0.01.

(b) Boys Found in All Censuses

Dependent variable: Target occupation in…	1920	1930	1940
	(1)	(2)	(3)
Own household	0.0375***	0.0471***	0.0410***
	0.0002	0.0002	0.0002
Next door neighbor	0.0032***	0.0036***	0.0034***
	0.0002	0.0002	0.0002
R-squared	0.405	0.385	0.377
Untreated mean	0.0257	0.0327	0.0330
Observations	15,473,431	15,473,431	15,473,431

Notes: Columns 1-3 uses the outcome of being the target occupation in 1920-1940. Sample excludes sheets with no one of the target occupation in 1910. Observations are children in 1910 that are linked to 1920, 1930, and 1940 censuses.
*p<0.1, **p<0.05, ***p<0.01.

Table A7: Stacked Regression Results Linked to Various Censuses - Girls

(a) All Links

Dependent variable: Target occupation in…	1920	1930	1940
	(1)	(2)	(3)
Own household	0.0168***	0.0107***	0.0085***
	0.0001	0.0002	0.0002
Next door neighbor	0.0016***	0.0011***	0.0010***
	0.0001	0.0001	0.0001
R-squared	0.338	0.351	0.356
Untreated mean	0.0255	0.0180	0.0157
Observations	23,530,340	13,458,635	11,165,884

Notes: Columns 1-3 uses the outcome of being the target occupation in 1920-1940. Sample excludes sheets with no one of the target occupation in 1910.

*p<0.1, **p<0.05, ***p<0.01.

(b) Girls Found in All Censuses

Dependent variable: Target occupation in…	1920	1930	1940
	(1)	(2)	(3)
Own household	0.0140***	0.0090***	0.0080***
	0.0002	0.0002	0.0002
Next door neighbor	0.0011***	0.0011***	0.0009***
	0.0002	0.0002	0.0002
R-squared	0.426	0.395	0.393
Untreated mean	0.0200	0.0146	0.0149
Observations	6,885,595	6,885,595	6,885,595

Notes: Columns 1-3 uses the outcome of being the target occupation in 1920-1940. Sample excludes sheets with no one of the target occupation in 1910.

*p<0.1, **p<0.05, ***p<0.01.

Table A8: Heterogeneity by Occupation Income and Education

Dependent variable: Target occupation in 1940	Baseline	High Income	High Education	High income: High education	High income: Low education	Low income: High education	Low income: Low education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own household	0.0381***	0.0537***	0.0439***	0.0609***	0.0396***	0.0242***	0.0334***
	0.0002	0.0003	0.0003	0.0004	0.0004	0.0004	0.0002
Next door neighbor	0.0034***	0.0040***	0.0037***	0.0053***	0.0014***	0.0017***	0.0036***
	0.0001	0.0002	0.0002	0.0003	0.0002	0.0003	0.0002
R-squared	0.323	0.337	0.323	0.334	0.290	0.301	0.322
Untreated mean	0.0331	0.0339	0.0421	0.0485	0.0103	0.0358	0.0314
Observations	26,649,908	8,481,845	10,662,704	5,356,209	3,125,636	5,306,495	12,861,567

Notes: Each column reports results within groups of occupations based on whether the occupation is above or below the median income and education score.

*p<0.1, **p<0.05, ***p<0.01.

Table A9: Stacked Regression Results while Handling Farmers in Different Ways

Dependent variable: Target occupation in 1940	Baseline occupations (no farm owners or farm laborers)	Baseline occupations plus farm owners & laborers	No farm owners	No farm laborers	Only farm owners	Only farm laborers	Only farm owners & farm laborers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own household	0.0381***	0.0408***	0.0341***	0.0454***	0.1028***	0.0105***	0.0498***
	0.0002	0.0002	0.0002	0.0002	0.0007	0.0004	0.0004
Next door neighbor	0.0034***	0.0042***	0.0031***	0.0045***	0.0112***	0.0013***	0.0073***
	0.0001	0.0001	0.0001	0.0001	0.0008	0.0003	0.0003
R-squared	0.323	0.368	0.315	0.376	0.335	0.250	0.376
Untreated mean	0.0331	0.0346	0.0336	0.0342	0.0690	0.0402	0.0484
Observations	26,649,908	33,117,306	29,644,924	30,122,288	3,472,382	2,995,017	6,467,399

Notes: Column 1 excludes farm owners and laborers, column 3 excludes farm owners, column 4 excludes laborers, columns 5-6 uses only farm owner and/or farm laborers.

*p<0.1, **p<0.05, ***p<0.01.

Table A10: Heterogeneity by Additional Neighborhood Characteristics

Dependent variable: Target occupation in 1940	High non-white share	Low non-white share	University in county	No university in county	Northeast	Midwest	South	West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own household	0.0396***	0.0370***	0.0360***	0.0402***	0.0361***	0.0385***	0.0425***	0.0338***
	0.0003	0.0002	0.0002	0.0002	0.0003	0.0003	0.0004	0.0006
Next door neighbor	0.0038***	0.0030***	0.0029***	0.0038***	0.0027***	0.0032***	0.0049***	0.0025***
	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0003	0.0004
R-squared	0.336	0.312	0.328	0.319	0.330	0.308	0.337	0.315
Untreated mean	0.0338	0.0327	0.0324	0.0340	0.0335	0.0318	0.0365	0.0296
Observations	11,676,940	14,972,663	13,604,749	13,044,706	9,198,224	9,743,871	5,464,572	2,243,240

Notes: Each column reports results within different subsets of the census sample based on neighborhood or characteristics or region.

*p<0.1, **p<0.05, ***p<0.01.

Table A11: Heterogeneity by Neighbor Similarity for Last Names that Are Unique to Each Sheet

Dependent variable: Target occupation in 1940	Baseline	Last name	Income score	Education score
	(1)	(2)	(3)	(4)
Own household	0.0381***	0.0381***	0.0376***	0.0375***
	0.0002	0.0002	0.0002	0.0002
Next door neighbor	0.0034***			
	0.0001			
Treatment 1: Same characteristic		0.0220***	0.0059***	0.0076***
		0.0010	0.0002	0.0002
Treatment 1 average		*0.0041*	*0.1300*	*0.1334*
Treatment 2: Different characteristic		0.0030***	-0.0012***	-0.0038***
		0.0001	0.0002	0.0002
Treatment 2 average		*0.2328*	*0.0848*	*0.0814*
F-test		325.74	849.42	2223.73
Two-tailed p-value		0.0000	0.0000	0.0000
R-squared	0.323	0.323	0.323	0.323
Untreated mean	0.0331	0.0331	0.0338	0.0338
Observations	26,649,908	26,649,128	26,649,908	26,649,908

Notes: In columns 2-5, treatments compare head of household of the focal child to the target occupation holder next door. In columns 3-4 the treatments evaluate if both head of household of focal kid and target occupation next door are above/below median in occscore and edscore. The number of observations in column 2 is slightly less due to the fact that individuals whose last name is missing and whose neigbhors' *p <0.1, **p <0.05, ***p <0.01.

B Census Neighborhood Reconstruction and Measurement Error

Figure A2: 1910 Census Enumerator Instructions

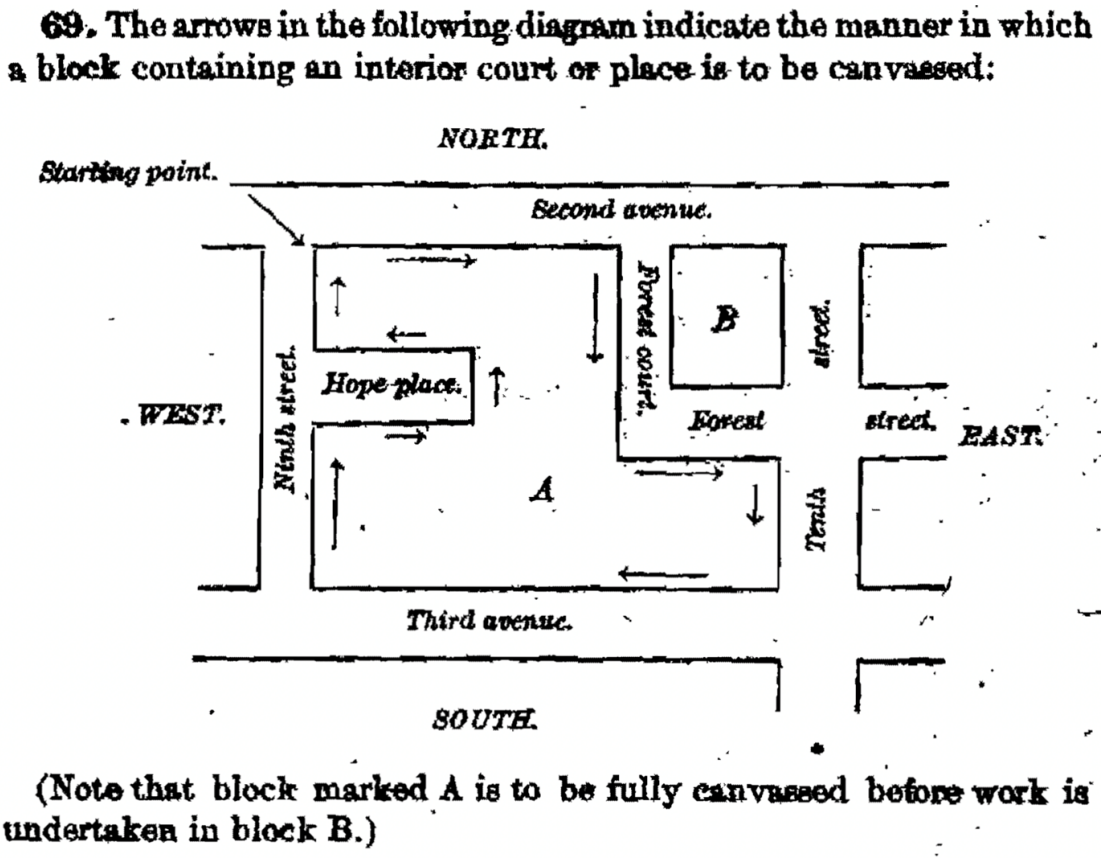


Table A12: Doctor Results while Dropping Possible Supplemental Sheets

Dependent variable: Target occupation in 1940	Baseline	Number of sheets dropped: Last sheet	Last two sheets	Last five sheets
	(1)	(2)	(3)	(4)
Own household	0.0970***	0.0969***	0.0965***	0.0956***
	0.0018	0.0018	0.0019	0.0020
Next door neighbor	0.0032***	0.0030***	0.0029***	0.0032***
	0.0007	0.0007	0.0007	0.0008
R-squared	0.327	0.326	0.325	0.323
Untreated mean	0.0078	0.0078	0.0079	0.0080
Observations	305,059	298,801	288,966	250,270

Notes: Columns 1-4 include only sheets with at least one adult in the target occupation. All columns include age fixed effects. *$p<0.1$, **$p<0.05$, ***$p<0.01$.

Figure A3: Example 1910 Census Manuscript Page, Grace St., Chicago

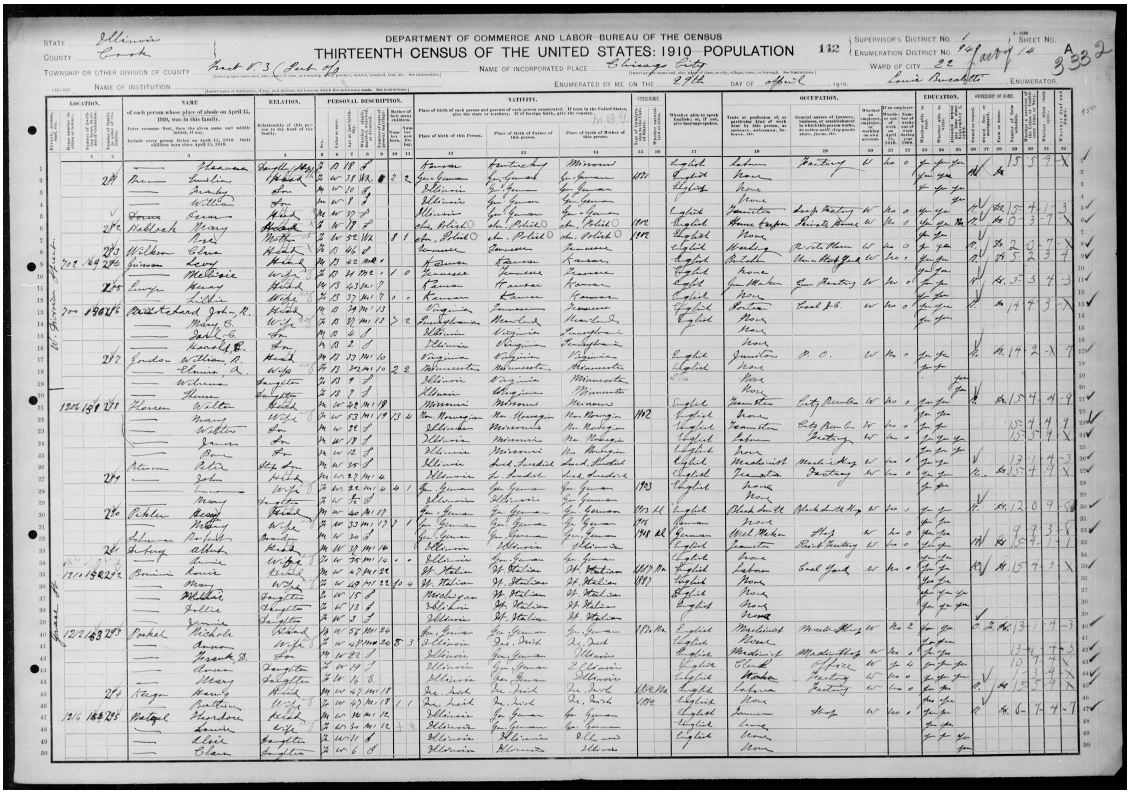
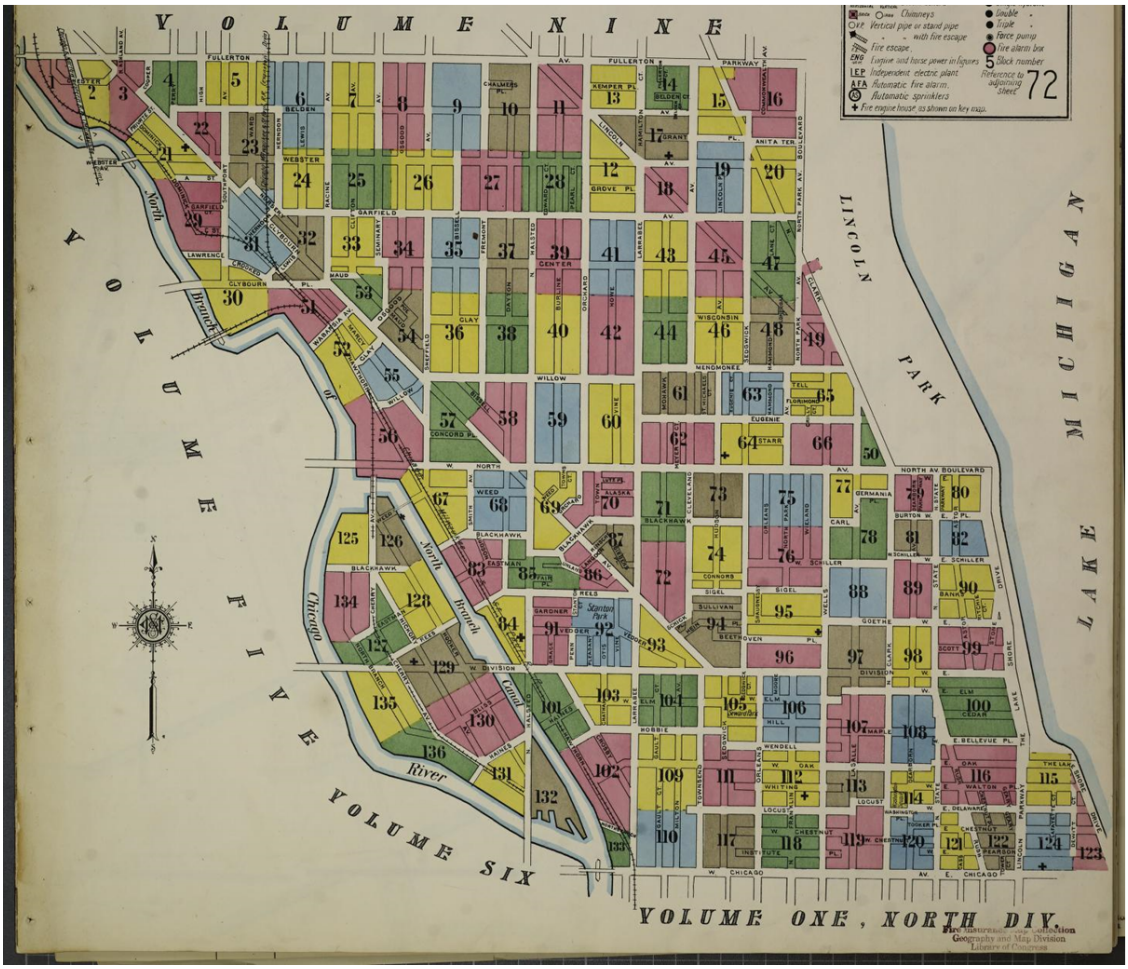


Table A13: Stacked Regression Results while Dropping Possible Supplemental Sheets

Dependent variable: Target occupation in 1940	Baseline	Number of sheets dropped		
		Last sheet	Last two sheets	Last five sheets
	(1)	(2)	(3)	(4)
Own household	0.0408***	0.0408***	0.0408***	0.0407***
	0.0002	0.0002	0.0002	0.0002
Next door neighbor	0.0042***	0.0041***	0.0041***	0.0041***
	0.0001	0.0001	0.0001	0.0001
R-squared	0.368	0.367	0.367	0.365
Untreated mean	0.0346	0.0345	0.0345	0.0344
Observations	33,117,306	32,372,452	31,235,482	26,389,976

Notes: Columns 1-4 include only sheets with at least one adult in the target occupation. All columns include age fixed effects.
*p<0.1, **p<0.05, ***p<0.01.

Figure A4: Example of Sanborn Fire Insurance Map



C Within-Neighborhood Sorting in the 1910 Census

Another way to show that adults with the same occupation do not cluster adjacent to one another is to use a measure of within-neighborhood residential segregation, such as that proposed by Logan and Parman (2017a). We adopt their measure, which calculates how likely an adult in one occupation is to live next to an adult in a different occupation relative to what would be expected when randomly allocating occupations across households. For a given occupation j, this is given by:

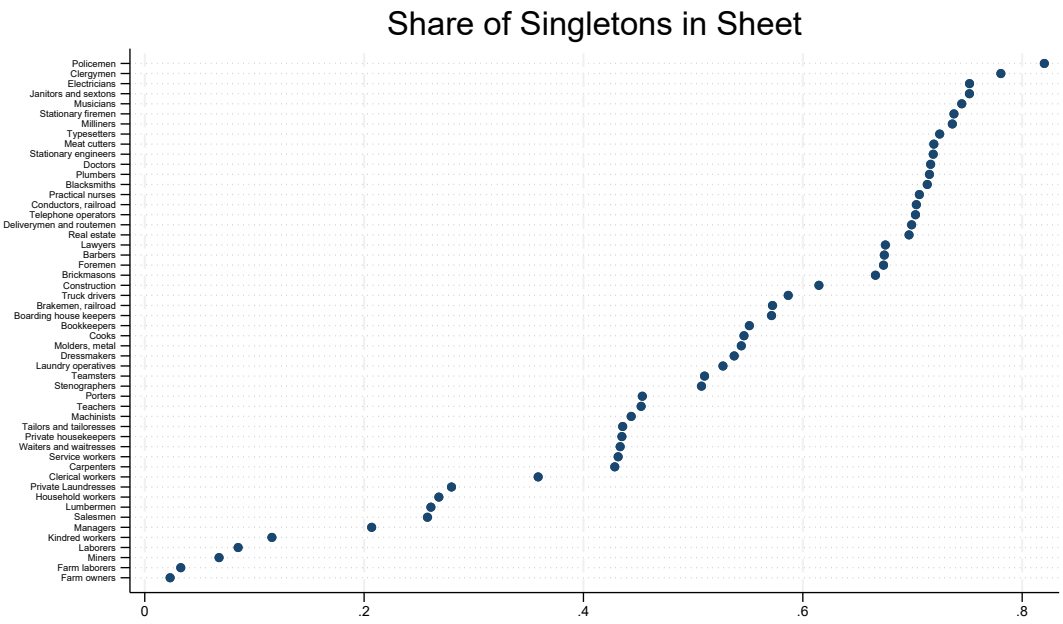
$$\eta_j = \frac{E(\bar{x_j}) - x_j}{E(\bar{x_j}) - E(\underline{x_j})}, \tag{7}$$

where x_j is the observed number of pairs of adjacent households in which one household has occupation j and the other does not, $E(\bar{x_j})$ is the expected x_j if households sorted randomly given the total number of j in the population, and $E(\underline{x_j})$ is the expected x_j if households were perfectly segregated by occupation.[23] $\eta_j = 0$ therefore corresponds to no residential segregation for occupation j, while $\eta_j = 1$ corresponds to perfect segregation. In all cases, η_j is close to zero. To put these segregation measures into perspective, we compare them to segregation measures by race and ethnicity (country of origin) for the 5 largest foreign born groups: German, Italian, Irish, Russian, and Canadian. Every occupation that we study is far less segregated than are race and ethnicity. Comparing across occupations, η_j tends to be larger for occupations related to agriculture or natural resource extraction (farm laborers, miners), where we expect most households on a census sheet to have the same occupation, although even in these cases segregation is much less than by race or ethnicity. In our baseline results, we exclude farmers and farm laborers, in part because of this potential for

[23] In the simplest case of an enumeration district, $E(\underline{x_j}) = 2$, since all households of occupation j would be clustered together on one part of the sheet and hence only one occupation j household in the first row of the sheet containing j occupations and one occupation j household at the last row containing occupation j households would be adjacent to a household with a different occupation. However, if this enumeration district was split into multiple, smaller sheets it is possible that $E(\underline{x_j}) = 0$, if every household has someone in the same occupation j.

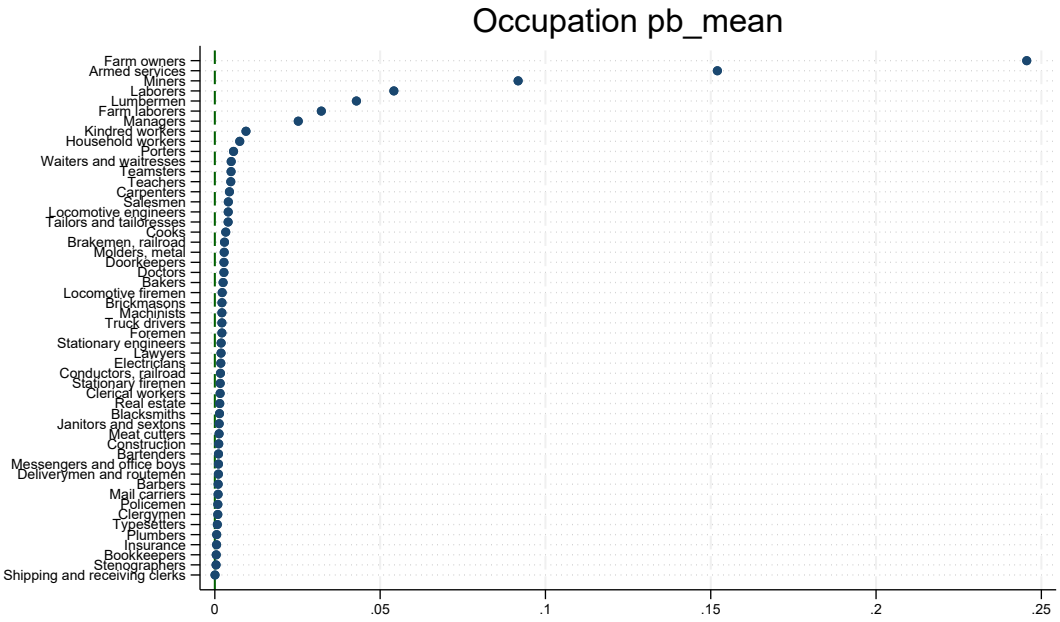
sorting.

Figure A5: Share of Singleton Occupations



Among the census manuscript pages with at least one occurrence of each occupation, the share of pages in which the occupation appears exactly once.

Figure A6: Logan-Parman (2017) Measure of Segregation



D Results Using Street Addresses

As discussed in Section 2, our indicator for living next door to a doctor may be subject to misclassification error in some cases, potentially biasing the OLS estimates. Misclassification could be due to skips or other inconsistencies in the enumeration process. Unlike continuous regressors, misclassification of binary regressors is necessarily non-classical. If the dummy is one (zero), measurement error can only be negative (positive), meaning that the error is mechanically negatively correlated with the true dependent variable. However, as Pischke (2007) notes, it is possible to recover the magnitude of bias by using the extent of misclassification.

Following the notation in Pischke (2007), consider the estimated $\hat{\beta}$ coefficient associated with some treatment (d_i) such as living next door to a doctor. If we only observe a version of the treatment ($\tilde{d}_i$) that misclassifies some observations then

$$plim\hat{\beta} = \beta \left[P(d_i = 1|\tilde{d}_i = 1) - P(d_i = 1|\tilde{d}_i = 0) \right]$$

We are able to estimate these probabilities using the subsample of doctors that we hand checked against the Sanborn Fire Insurance maps. In this sample, we find that among the people we classify as neighbors of doctors ($\tilde{d}_i = 1$) 59.3 percent actually live next door to doctors based on the address and map information. Among people we classify as living more than one door away from doctors ($\tilde{d}_i = 0$), 2.12 percent actually live next door to doctors. Based on these measures, our estimates of the effect of living next door to a doctor are biased downward by 57.2 percent. The causal effect is about 1.75 times larger than our estimate for doctors.[24]

One approach to dealing with misspecification error is to instrument our indicator for being next door to a doctor on the census sheet with an indicator for being next door using

[24] Across each of the different occupations, as long as $P(d_i = 1|\tilde{d}_i = 1) > P(d_i = 1|\tilde{d}_i = 0)$ our point estimates will be biased downward.

the street addresses. But because our treatment of interest is binary and the associated measurement error is non-classical, this commonly used IV approach to correct classical measurement error does not apply in our setting. The IV estimate will, however, still be useful to compute in a bounding exercise, as recommended by Pischke (2007) and Aigner (1973). For a subset of census sheets, enumerators list street names and numbers. As such, we can define a measure of proximity using the street-ordering on pages. Next door indicators defined under this definition are likely also measured with error, but for reasons that we argue are likely to be orthogonal to the error in the sheet-ordered dummies (e.g. transcription errors of house numbers and street names). We exploit this fact in Table A14. In Column 1, we report the baseline estimates from Column 4 in Table 2 and compare them to Column 2, where we restrict to households that also have recorded street addresses. Both the coefficients for having a doctor in the focal child's own household and next door are slightly larger than the baseline estimates. In Column 3, we use the street addresses to measure proximity. We consider a household to be next door to the focal household if it has the closest house number (either larger or smaller) on the same street.[25] Instead of sheet-based fixed effects, we include street name-by-enumeration district fixed effects.

We find results that are qualitatively similar to our results in Column 1, although coefficients for both own household and next door are slightly smaller in magnitude. When using street addresses, growing up next door to a doctor makes a child 0.28 percentage points, or about 46%, more likely to become a doctor. Since the measurement error in both of our measures of household's proximity are believed to be orthogonal, in Column 4 we use the measure of NextDoorDoc1910$_i$ using recorded street addresses as an instrument for NextDoorDoc1910$_i$ using census sheet order. The coefficient on NextDoorDoc1910$_i$ is 2.4 times larger than in our estimates using only the census sheet order in Column 2 and 3.1

[25]One of the reasons we use the sheet ordering as our preferred specification is because addresses are not recorded for about half of the census observations. To ensure that we are capturing neighbors with the street address ordering, we restrict the sample to enumeration districts where at least 80 percent of households have address recorded. This allows us to focus on neighborhoods where the enumerator actually recorded addresses and missing values are due to transcription or digitization error.

Table A14: Doctor Results Using Street Addresses

	Sheet ordering			
Dependent variable:	Baseline	Street sample	Street ordering	Street order IV
Doctor in 1940	(1)	(2)	(3)	(4)
Own household	0.0970***	0.1269***	0.1176***	0.1278***
	0.0018	0.0045	0.0040	0.0045
Next door neighbor	0.0032***	0.0036*	0.0028**	0.0087**
	0.0007	0.0019	0.0014	0.0043
R-squared	0.283	0.369	0.200	0.012
Untreated mean	0.0038	0.0061	0.0061	0.0061
Observations	6,044,153	1,053,150	1,053,150	1,053,150

Notes: Columns 1-2 use the ordering of census sheets to define neighborhood proximity. Columns 3 uses street ordering. Column 4 is an instrumental variables regression with street order variables instrumenting for sheet order variables.

*p<0.1, **p<0.05, ***p<0.01.

times larger than in our estimates using only the street address in Column 3. The OLS and IV estimates bound our estimated effect of growing up next to a doctor on the probability of becoming a doctor between 0.32 percentage points and 0.87 percentage points. Our measurement error corrections place the estimate squarely between these estimates (0.56 percentage points).

Table A15: Stacked Regression Results Using Street Addresses

	Sheet Ordering			Instrument	
	Baseline	Street sample	Street ordering	First stage	Second stage
Dependent variable:					
Target occupation in 1940	(1)	(2)	(3)	(4)	(5)
Own household	0.0410***	0.0372***	0.0371***	-0.1207***	0.0379***
	0.0002	0.0003	0.0003	0.0007	0.0003
Next door neighbor	0.0042***	0.0026***	0.0026***	0.4067***	0.0066***
	0.0001	0.0002	0.0002	0.0007	0.0005
R-squared	0.332	0.371	0.269	0.663	0.371
Untreated mean	0.0104	0.0110	0.0109	0.0155	0.0121
Observations	314,295,968	56,042,752	56,042,752	56,042,752	56,042,752

Notes: Columns 1-2 use the ordering of census sheets to define neighborhood proximity. Column 3 uses street ordering. Column 4 and 5 report the first and second stage of an instrumental variables regression with street order variables instrumenting for sheet order variables.
*p <0.1, **p <0.05, ***p <0.01.