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

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Abstract

There is growing evidence that where a child grows up has long-run, economic mobility consequences. We explore the role of neighbors in this place-based transmission. Using linked census records for over 6 million boys and 4 million girls and historic census sheet microgeography, we 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 further down the 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, and ethnic, race, and child age homophily strengthens transmission, consistent with information and exposure channels. Childhood neighbors have 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.

Keywords: Neighborhood networks, peer effects, occupational transmission
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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 high quality neighborhoods matter? One probable mechanism explored in the literature is that they provide access to amenities such as better education, health, and safety infrastructure (Laliberté, 2021). In this paper, we focus on another potentially important mechanism: neighborhoods provide positive human capital spillovers from mentors or 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.

Why might nearby adult neighbors influence children's later career choices? While some neighborhood effects operate at different geographic scales, many are highly localized (Aliprantis, 2017; Billings et al., 2022; Redding and Sturm, 2024), and there is evidence of strong local, neighborhood employment networks (Bayer et al., 2008; Hellerstein et al., 2011; Tan, 2022). A growing literature finds that 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). Studies on 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. By estimating the effect of nearby neighbors, we remove these last two channels and investigate the importance of information and exposure spillovers.

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 described 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 regressions that estimate exposure effects for all children who lived near doctors (and many other occupations) in the 1910 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 careers. 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 street) is as-good-as-randomly assigned. We assess this assumption using a variety of tests in our data.

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 intergenerational transmission of occupations from parents to children (Lentz and Laband, 1989;

Polyakova et al., 2020; Ventura, n.d.). 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, including, as mentioned above, by restricting attention to sheets with only one doctor and to using sheets with transcribed street addresses as an alternative measure of proximity. When using street address-based measure of proximity as an instrument for the census sheet measure of proximity to correct for measurement error in proximity, we find that next door neighbors are even more important, increasing the probability that a child grows up to be a doctor by 143% relative to other children on the same sheet.

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. 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 his 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. Using all 50 occupations, an individual in the child's own household is about an order of magnitude more predictive than the next door neighbor. 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 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 or high education occupation. We also explore how heterogeneity across neighborhood 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 explore how homophily between neighbors may affect the transmission of occupations. Boys are more likely to adopt their neighbor's occupation when they share a birthplace, are of the same race, when adjacent household heads have similar occupational incomes or educations, and when they have the same last name.[1]

Childhood neighbors have real economic implications. Boys growing up next door to a high-income, high-skill worker, such as a doctor or lawyer, have significantly higher education and earnings as adults relative to other boys on the same census sheet. Whereas growing up next door to a porter, truck driver, or laborer actually results in lower income for the children 30 years later. These effects are partially driven by the direct transmission of occupation from adults to neighbor children, but there are spillovers to other, similar occupations as well, suggesting a broader information mechanism, where living next to someone of particular occupations exposes children to more general information about potential occupations. For example, growing up next to a doctor increases the probability that a boy chooses a different

[1] While next door neighbors have a larger effect on children's occupation when they have the same last name, we also show that next door households with different last names have similar effects to our baseline estimates, so our main results are not driven by extended families living next door to one another.

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.

Overall, these results suggest that children are influenced in their career choices by their interactions with people in their social networks. It appears that this influence is highly correlated with physical proximity. 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.

Transmission of careers within the neighborhood is stronger for high-income occupations. 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.

This work 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. The measure of residential segregation developed by Logan and Parman (2017a) has subsequently been used to 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). Eriksson and Ward (2019) used the same technique to construct measures of residential segregation by immigrant groups. Others have used a similar neighbor design to understand racial sorting in a modern context (Bayer et al., 2022). Rather than measuring local segregation, we innovate on the ability to use neighborhood microgeography to capture differences in children's neighborhood exposure.[2]

[2]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

We advance the literature identifying neighbors in historical censuses 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. We propose several modifications to these methods to minimize these errors. We also use census street addresses when available as an alternative measure of neighborhood microgeography, as well as using the street address measure of proximity as an instrument for the census sheet measure of proximity to correct for measurement error.[3] To show the plausibility of our identification assumption using our census sheet measure of microgeographic proximity, we use the Logan and Parman (2017a) technique to construct a measure of residential segregation by occupation; to the best of our knowledge, we are the first to conduct this analysis. We find that occupations are much less residentially segregated than either race or immigrant groups.

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. Finally, Section 7 briefly concludes.

2 Data

Existing work has relied on occupation case studies (e.g., congressional legislatures (Dal Bó et al., 2009)) 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

that workers' industry composition is more concentrated within census sheets.

[3]Census street addresses have been used to identify the owners of particular residences in, e.g., Akbar et al. (2019); Quincy (2022).

the propensity of sons to follow the occupation choice 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 we must simultaneously be able to observe the occupations of all of their 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 including 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. Using the 1910 census enumeration district, sheet, and line number we can approximately reconstruct neighborhoods and create a proxy for the geographic proximity

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

between individuals. We use our proximity measure to identify the occupations of each household's neighbors. Using the 1940 census we can observe the child's own occupational choice as an adult, 30 years later. In some analyses we also use 1920 and 1930 full count census micro data, to explore the dynamics of occupation transmission over time. We merge the de-identified full count census data to IPUMS records using historical IDs (HISTID) to work with cleaned, categorized measures.

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 is 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. We then use all Census Tree links between 1910 and 1940 to identify all of the 1910 children that we can observe in 1940. 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] In 1910 only 17.6 percent of women between 30 and 64 were employed and by 1940 it was only 17.9 percent. Because the employment and occupation decisions were so vastly different for each gender during this time period, we focus on outcomes for boys and girls separately. We link 48.3 percent of boys and 32 percent of girls between 1910 to 1940. As seen in Table 1 our linked sample comes from household settings that are representative of the full population, but our linked sample is more white than the full population, which is perhaps unsurprising given the difficulty linking Black individuals during this time period.

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, and wage income.

Because women historically change their last name upon marriage, traditional linking methods perform poorly when linking women. For this reason researchers will often exclude women or focus on subsamples of women that are easier to follow over time. As such our ability to evaluate outcomes of girls is a relative innovation. However, the patterns and opportunities for employment and education were quite different for boys and girls in 1910. 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

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

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.

2.3 Measuring Neighbors in the Full Count Census

Our empirical approach relies on identifying geographically proximate households. We rely on the enumerators' record keeping to identify potential neighbors and adults the focal children might observe or interact with. 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)." As specified in the 1910 enumerator instruction guide, the intent was to record families on a census sheet *neighbor-by-neighbor*. This allows us to use the census collection information, such as enumeration district, sheet (page) number, line number, and in some cases address to identify likely neighbors with fine geographic precision.

We will estimate occupational transmission for many occupations, but for clarity we will describe our data creation process just looking at doctors. First, we flag all households across all 1910 census sheets that include a focal sample child 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 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 that equal one if 1) there is a doctor in one's own household, 2) there is a doctor on the same sheet and one household above or one household below one's own household ("next door"), and 3) there is a doctor on the same sheet and 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 5. We repeat this process for each target occupation separately. We will 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.

Given census instructions for enumeration, sheet ordering should allow us to accurately identify neighbors and geographic proximity. Focusing on the subset of households where street address has 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 with sheet ordering and the gap between house numbers (e.g., 32 Mulberry Street vs 34 Mulberry Street) increases as the sheet ordering distance increases between households. Although this is true in general, it is not always the case. If no one was home when the enumerator stopped, that household is 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. Unless having a doctor (or member of a different focal occupation) in your household is correlated with not being available when the enumerator visits, this will produce classical measurement error and attenuate our estimates.

Unfortunately, for most of the census we cannot know if the sheet ordering leads us to mis-assign next door and further-away neighbors, either the census does not have address information recorded or we do not have street maps from 1910 to verify that addresses are listed in order. However, using the Sanborn Fire Insurance Maps (Sanborn Map Company, Various Years) we can quantify the level of measurement error for a subset of observations.

Overall, we find evidence that sheet order is a relatively accurate proxy for street address proximity. In the results section we further show that our sheet order measures of treatment are robust to excluding individuals on the last 1, 2, or 5 sheets in each enumeration district (to eliminate supplemental sheets where the sheet ordering does not reflect neighbor proximity), as well as using street ordering next door neighbor status, where address is available, to instrument for sheet ordering next door neighbor status, to eliminate measurement error bias.

3 Identification Strategy

Intuitively, we exploit the idea that, while households undoubtedly sort into neighborhoods, including selecting the street they want to live on 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. We rely on this quasi-random assignment of neighbors' occupations within a localized neighborhood to identify the effect of adult neighbors on children's future occupation choice. To see this more formally, consider a simple linear model:[6]

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

where Occ1940_{is} is an indicator equal to one if a child i that we observe in 1910 has a particular occupation in 1940. AdultOcc1910_{id} is equal to one if the adult living distance d from household i in 1910 is in that occupation. If distance proxies for exposure and interaction, and exposure to adults matter for children's later occupational choices, then β_d should be positive and declining in d. But if households sort into where they want to live, then children may be more likely to go into the same occupation as their more proximate neighbors than the average child for reasons unrelated to exposure effects. 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. As a less extreme case,

[6] We estimate a modified version of this, which we describe in detail, in Section 4 below.

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 exposure effects were zero, since high income parents have the resources and familial social capital to encourage their children to enter high income occupations. In these cases,

$$\text{Cov}(\text{AdultOcc1910}_{id}, \epsilon_i) > 0.$$

The problem with the examples given above is that we are comparing outcomes for children who live far apart from one another, and so the coefficient on distance to adults with a particular occupation is picking up not only potential exposure effects but also other characteristics of the neighborhood the child's family has sorted into. Our estimates would still be causal if we could control for all of these other neighborhood characteristics. But neighborhoods differ in a multitude of dimensions, most of which are unobservable to the econometrician. One path forward is to compare only children who live in the same neighborhood. But neighborhoods can be large and have substantial heterogeneity within them. Because we can reconstruct the microgeography of a neighborhood at the house-to-house level from the historical censuses, we instead make comparison between children who reside on the same census manuscript sheet, which is usually only a portion of a typical street block.Within the area covered by a single census manuscript sheet, nearly all relevant neighborhood characteristics are held constant. Moreover, at the level of a single manuscript sheet, households typically cannot choose how close to be to another household with particular characteristics. So,

$$\text{Cov}(\text{AdultOcc1910}_{id}, \epsilon_i | s) = 0,$$

where s is the census manuscript sheet. In our preferred regressions specifications below, we include census sheet fixed effects. In many cases, we focus specifically on the influence of an adult living next door to a focal child. Hence all that is required is that, conditional on being on the same census manuscript page, individuals cannot choose their immediate next door neighbor. Then, conditional on appearing on the same census manuscript sheet, the occupation of a focal child's next door neighbor is as good as randomly assigned. Similar identification has been used in housing transaction data in a modern context (Bayer et al., 2022).

3.1 Plausibility of the Identification Assumption

Our identification strategy would fail if households can sort based on their neighbors occupation *on the same census manuscript sheet*. We think this is unlikely for several reasons.

We find no evidence that adults having the same occupation are able to sort into adjacent houses. In many cases, a particular occupation occurs only once on a census sheet. 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. For rare occupations like doctors, which we use as an illustrative example in Section 4.1, an even larger fraction of sheets that have a doctor have only one doctor. 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.

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:

$$\eta_j = \frac{E(\bar{x_j}) - x_j}{E(\bar{x_j}) - E(\underline{x_j})}, \tag{2}$$

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.[7] $\eta_j = 0$ 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 sorting.

4 Results

4.1 Doctors

We begin the empirical analysis with an illustrative example, focusing narrowly on boys in 1910 and one particular occupation: Doctors. In the next section, we broaden the analysis

[7] 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.

to many occupations and both girls and boys.

Here, we presents 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 error term that we cluster 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.[8]

[FIGURE 2 ABOUT HERE]

[8] 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. We also believe that focusing on next door neighbors is a more 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 operative farther than next door, this will bias against estimating a positive effect of next door neighbors on occupational choice.

Next we restrict our attention to the own household doctor effect and the immediate next door neighbor effect and test the sensitivity of our estimates to several alternative specifications. We estimate:

$$\text{Doctor1940}_{is} = \alpha + \beta_0 \text{OwnDoc1910}_i + \beta_1 \text{NextDoorDoc1910}_{is} + \text{Age}_i + \text{LocationFEs} + \epsilon_{is}, \tag{4}$$

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.[9] The neighbors further down the street are now included in the omitted category. LocationFEs is a fixed effect for household i's location. We use different 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 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, 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

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

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.

[TABLE 2 ABOUT HERE]

In the Appendix, we present results using additional sets of fixed effects. 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. In Appendix Table A1, we minimize concerns raised in Section 2 that our results our contaminated by the inclusion of census manuscript sheets with unordered households on supplemental pages included at the end of each enumeration district by repeating the baseline results in Column 4 of Table 2 but omitting the last several pages from each enumeration district.

In Section 2, we discuss possible reasons why the order in which individuals are listed on a census manuscript sheet may fail to reflect actual geographic proximity. We suggest that individuals' street addresses, when recorded in the census manuscripts, may provide an alternative measure of geographic proximity. While both methods likely are measured with error, we argue that the sources of error for each are likely to be orthogonal to one another. We exploit this fact in Table 3. In Column 1, we estimate our preferred version of Equation 4 (including census sheet fixed effects) when restricting attention 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 4 of Table 2, although when we restrict attention to the sheets with street addresses the baseline probability that children will grow up to become doctors is substantially larger as well. Having a doctor in one's own household makes a child about 20.8 times more likely to become a doctor and having a doctor live next door makes a child 59% more likely to become a doctor. In Column 2, 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.[10] 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. Consistent with the instrument resolving classical measurement error in household proximity, 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 times larger than in our estimates using only the street address in Column 3.

[TABLE 3 ABOUT HERE]

In our baseline results, we link children in the 1910 census to their adult occupations in 1940. In Table 4, we repeat this exercise using our preferred specification 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

[10]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.

capital. By 1920, and to a lesser extent by 1930, a large fraction of children in 1910 would not yet have completed their training to become doctors.

[TABLE 4 ABOUT HERE]

4.2 Other Occupations for Boys and Girls

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. Here we present results for several occupations one at a time, and then in the next section we will use a stacked regression approach that incorporates information from multiple occupations in the same regression.

We replace the indicators Doctor1940$_{is}$, OwnDoc1910$_i$, and NextDoorDoc1910$_{is}$ with analogous indicators for an arbitrary profession. 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.[11]

Figure 3a presents the coefficients from separate occupations graphically for boys, and Figure 3b does the same for girls. The left-hand panel plots the own household coefficients, and the right-hand panel shows the next door coefficients. 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 percentage changes relative to the untreated mean.[12] Both are separately sorted by effect size and solid filled markers indicate that the coefficient was statistically significant at the the 5% level. We

[11] We exclude farmers from this set of occupations for three reasons. First, farmers often live in communities where most households have the same occupation; it is likely that farmers that live on a street with no other farmers, for instance a lone urban farmer in a neighborhood, are very different from the average farmer. Second, the nature of farming was changing substantially between 1910 and 1940. Third, 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 using farmers in Appendix A.

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

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 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 driven by the rapidly evolving female labor force size and composition between 1910 and 1940, leading to fewer daughters working in the same occupation as their mothers.

In the right-hand panel, 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, the lack of statistical precision at the individual occupation level is one reason that we focus parts of the heterogeneity analysis below on boys only.

These plots make it clear that some occupations are more likely to transmit to neighboring children. However, from these sets of coefficients, it is difficult to draw conclusions about the channels of occupation transmission by inspecting individual cases, so we attempt to study these channels more carefully in Section 5 below.

[FIGURE 3 ABOUT HERE]

4.3 Stacked Regressions

To explore potential causal mechanisms that could explain occupational transmission from neighbors we use data from all 50 occupations for boys in one regression and all 25 occupations for girls in another regression. We estimate

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

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 location are interacted with occupation to make the same within-occupation comparisons that we made in Figure 3.

We present estimates from the stacked regression in Table 5a for boys, and Table 5b for girls. Columns use the same location fixed effects 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, essentially accounting for locational sorting up to the sub-street level. 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.

[TABLE 5 ABOUT HERE]

In Appendix A, we perform the same set of robustness tests as we do for the doctors. In all of the alternative specifications, we again find similar results to those in Table 5. We also again use street addresses as an instrument for proximity on the census manuscript sheet and find that, after correcting for measurement error, estimated effect sizes are even larger. We also find larger effect sizes when we include farm owners and laborers among our occupations in the stacked regressions.

As suggested by Figure 3, these average effects may mask substantial heterogeneity. In the following section, we use versions of the stacked regression model to test for heterogeneity along several dimensions.

In Table 6 Panel A, we explore the dynamics of the own household and next door effects as we do in Table 4, estimating coefficients for boys linked to the 1920, 1930, and 1940 censuses respectively. Here we see a different pattern than we do when we examine only doctors: the coefficients for both own household and next door are smallest in 1920 but then peak in 1930, before declining in 1940. This pattern is likely capturing two forces. First, it takes time for individuals to accumulate human capital necessary to enter into particular occupations; this force likely dominates for high human capital occupations like doctors, which is why effects were largest in 1940 when using only doctors. At the same time, once individuals experience an occupation for themselves, they may decide it is not the best fit for their skills or interests, and so we may observe attrition over time leading to smaller estimates. For the average occupation in the stacked regression, these two forces combine to produce a hump-shaped pattern, with the largest coefficients occurring two decades after we observe childhood exposure. We observe a virtually identical pattern in Panel B, where we restrict attention to the subsample of boys who are observed in all three of the 1920, 1930, and 1940 censuses in Panel B. In Table 7 we report the same results for girls. Here we see a consistently falling effect for both own household and next door neighbors. This likely is driven by the common employment pattern for girls at the time, who typically detached from the labor force at the time of family formation.

[TABLE 6 AND TABLE 7 ABOUT HERE]

5 Heterogeneity

The previous sections showed that exposure effects vary across different occupations. In this section we study this heterogeneity systematically, and show that different characteristics of occupations are associated with larger or smaller exposure effects. We also explore heterogeneity across neighborhood characteristics and across the characteristics of individual neighbors. 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.

5.1 By Occupation Characteristics

To estimate heterogeneity by occupation characteristics, in Table 8 we re-estimate the stacked regression from Equation 5 with census sheet fixed effects but restrict the sample to only include occupations that have particular characteristics. In Column 2, we restrict the sample to include occupations with an above-median occupational income score.[13] Exposure effects are larger for high income occupations, both when the individual with a high income is in the same household and when they live next door. Having an individual with a high income occupation in the same household increases the probability that a boy enters that high income occupation by 5.37 percentage points, a 158% increase. Having an individual with a high income occupation next door increases the probability that the boy enters that occupation by 0.4 percentage points, or 11.8%. This estimate is between our estimates for doctors, a particularly high income, prestigious occupation, and for all of the largest occupations in the stacked regression.

In Column 3, we limit the sample to occupations with an above median occupational education score.[14] Estimated magnitudes for both own household and next door coefficients lie between those for high income occupations and for all of the largest occupations in the stacked regression. It might be especially important to live next door to a high education occupation as a child if children need to make human capital investments early in their lives

[13]Individual-level income data is unavailable in the censuses prior to 1940. Occupational income scores are based on the median total incomes of individuals in each occupation in the 1950 census. Note that this measure is at the occupation level, rather than the individual level. A doctor earning a very low income would be included in this sample, whereas a plumber earning a very high income would not be. For more on the advantages and drawbacks of using occupational income scores, see Feigenbaum (2018).

[14]The occupational education score is constructed similarly to the occupational income score, and has the same strengths and drawbacks. Namely, it is the percent of workers in the 1950 census that have at least one year of college.

to enter into these occupations. On the other hand, children may be interested in the earning potential of a job rather than its level of occupation per se, and the larger coefficients for the high education occupations may be reflecting the fact that many occupations, such as doctors and lawyers, are both high income and high education. In Appendix Table A5, we further split occupations into those with both above median income and above median education, those with above median income and below median education, those with below median income and above median education, and those with below median income and education. The next door neighbor effects are especially large for high income low education occupations, which provide high earnings for less human capital investment, and are especially small for low income high education occupations, which are the opposite.

One thing children might learn from adults are what skills are required to go into business for oneself. In Column 4, we show that the next door neighbor coefficient is larger than the baseline coefficient, both in magnitude and as a percent increase, for occupations with an above median level of individuals that are self-employed. The own household coefficient is much larger, at 5.7 percentage points or a 206.9% larger than the base level. This much larger increase for the own household is consistent with parents passing on family firms to their children (Lentz and Laband, 1990).

[TABLE 8 ABOUT HERE]

5.2 By Neighborhood Characteristics

The characteristics of a household's neighborhood may also affect the ability of children to learn about occupations from others. In this section, we explore heterogeneity of the own household and next door neighbor exposure effects by neighborhood characteristics. We again estimate the stacked regression from Equation 5 with census sheet fixed effects but now restrict the sample to only include neighborhoods that have particular characteristics.

In Table 9 Columns 2 and 3, we show how the effects are different in urban and rural neighborhoods, respectively. It is plausible that exposure effects may be larger in either urban or rural areas. For instance, 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). We find more evidence for the latter view. In urban neighborhoods, boys are 0.28 percentage points (8.8%) more likely to go into the same occupation as their next door neighbor than are boys farther away on the same street. In rural neighborhoods, boys are 0.42 percentage points (11.8%) more likely to go into the occupation of their next door neighbor.

We next investigate whether next door neighbors are more predictive of children's occupations in counties with high shares of immigrants in Columns 4 and 5. The theory is once again agnostic on where exposure effects should be more important. On one hand, households may interact more in immigrant enclaves (Damm, 2009).[15] On the other hand, if immigrant communities provide alternative networks, next door neighbors may be relatively less important for the transmission of information about occupations. We again find more evidence for the latter view. In neighborhoods in counties with above median share of immigrants, a boy is 0.29 percentage points (9%) more likely to adopt the occupation of their next door neighbor than are other children farther away on the same street. In neighborhoods in counties with below median share of immigrants, a boy is 0.48 percentage points (13%) more likely to adopt the occupation of their next door neighbor.

Another reason why next door neighbors may be less predictive of children's future occupations in high immigrant neighborhoods is that people in those neighborhoods may be more transient, hindering the development of social capital and limiting the exposure of a child to any particular neighbor. Consistent with this, in Columns 6 and 7, we show that next door neighbors are less predictive in neighborhoods with an above median share of

[15]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.

household heads that were born out of state, regardless of whether those household heads are international immigrants or not.

In Appendix Table A6, 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.

[TABLE 9 ABOUT HERE]

5.3 By Individual Characteristics

While the previous section examined differences across neighborhoods, here we investigate heterogeneity among neighbors themselves. In particular, we test whether homophily among neighbors makes exposure effects stronger. To do this, we estimate

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

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 who is different along a characteristic.

In Table 10 Panel A 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. In Column 3, we perform a similar test but using country of birth instead of birthplace. Exposure effects are smaller when the next door neighbor was born in a different country.

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. However, this negative effect must be interpreted relative to the appropriate counterfactual group. Inherently we are comparing 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 counterfactual kids are more likely to be of the same race as the person in the target occupation. For example, this suggests that a non-White child is less likely to become a doctor, even if he grew up next door to a doctor, relative to other, likely White children who grew up on the same street. In Panel B of Table 10 which compares within race the different characteristic estimates are no longer negative.

In Column 5, we check how the exposure effect varies if the next door neighbor has the same last name as the focal child. Boys are 2.2 percentage points more likely to go into the occupation of next door neighbors with the same last name. This large coefficient is unsurprising, since having the same last name makes it very likely that the focal child shares a cultural and ethnic background with the next door neighbor, and may well be directly related; hence, in these cases the interactions between the focal child and the next door neighbor are likely much more frequent. Boys are 0.3 percentage points more likely to go into the occupation of a neighbor with a different last name than are children farther away on the same street, similar to our baseline estimate of 0.34 percentage points. This estimate gives us confidence that our baseline results are largely not driven by families sorting to live next door to one another. In Appendix Table A7, we provide further evidence that family connections are not driving our results by restricting our sample to focal boys with last names that appear in only one household on their street.

In Column 6, 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 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 by labor economists have indicated 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).

In Column 7, we test whether exposure effects differ depending on whether the boy's household head has a similar income to the household head of the next door neighbor. As in the results for Table 8, we split household heads into those with an above median occupational income score and those with a below median occupational income score. $\text{SameCharacteristic}_{ij}$ is equal to one if both households have an above median income score or if both households have a below median income score. Boys are 0.59 percentage points more likely to go into the same occupation as their next door neighbor relative to other children on the same street if their household heads have similar incomes. If their household heads have different incomes, then children are 0.12 percentage points *less* likely to go into the occupation of their next door neighbor. In Column 8, we use occupational education score instead of income and observe a similar pattern, although magnitudes are even larger. Boys are 0.76 percentage points more likely to go into the occupation of their next door neighbor if they have similar education levels, and 0.38 percentage points less likely to go into the occupation of their next door neighbor if they have different education levels. Once again, this is likely because the children in the counterfactual group, further down the street, are more likely come from households that are similar to the target occupation's characteristics.

The results in Table 10 Panel A test for the importance of homophily, but they cannot tell us if, for instance, children in high income households are more influenced by their high income next door neighbors than children in low incomes households are by their low income next door neighbors. We perform this exercise in Table 10 Panel B for race, occupational income, and occupational education. Columns 2 and 3 use the sample of White and non-White (largely Black) children, respectively, and test the influence of next door neighbors of the same race. For both White and non-White boys, the coefficients for same race and different race neighbors are statistically indistinguishable from one another.[16]

In Column 4, we show that when a boy in a high income household lives next door to an individual with a high income, they are 0.18 percentage points more likely to enter into that high income occupation than are other boys living on the same street. When a boy in a high income household lives next door to an individual with a low income, however, they are 0.31 percentage points more likely to enter into that occupation. Boys from low income households are roughly equally as likely to go into the occupation of their next door neighbor relative to other boys on the same street regardless of whether the neighbor is high or low income. The results for occupational education are similar to those for income. Boys growing up in high education households are 0.18 percentage points more likely to enter the occupation of high education next door neighbor than are other boys on the street, but 0.29 percentage points more likely to enter the occupation of a low education neighbor. Boys in low education households are about equally likely to enter the occupation of next door neighbors relative to other boys on the street regardless of the neighbors education level. The asymmetry in the importance of homophily—or in this case heterophily—between children in high income and education households on the one hand and children in low income and education households on the other suggests that perhaps children growing up in high income and education households have little exposure to lower socioeconomic occupations through

[16] In the case of non-White children, we cannot reject the null of zero exposure effects; that is, non-White children living next door to an individual in a particular occupation are no more likely to enter into that occupation than are other children on the same street, regardless of the race of the next door neighbor. We stress that, as shown in Table 1, we are able to link a smaller share of non-White children to future censuses than we are for White children.

their family and peer networks. Closely observing a neighbor working in those occupations is thus especially influential in these cases.

[TABLE 10 ABOUT HERE]

6 Exposure Effects on Economic Outcomes

As discussed in Section 4, 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 transmissiblity, (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. Here we additionally consider income and education effects for girls, whose economic outcomes 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.[17] As seen in Figure 4a, 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

[17] 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 occuaption.

in the same neighborhood in 1910, and thus accounting for socioeconomic neighborhood sorting.

[FIGURE 4a ABOUT HERE]

There are other high-income occupations such as manager and locomotive engineers 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.

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. Similar to boys, living next to laborers or private laundresses actually led to lower annual income relative to other girls from the same neighborhood.

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. For this reason we next explore how childhood neighbors' occupations affect children's educational attainment.

6.2 Effects on Educational Attainment

In Figure 5, we find that living next door to someone of a particular occupation has a similarly heterogeneous by occupation 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. 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 5b shows similarly large effects for many professional occupations with educational requirements. 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. Girls who had the potential to enter the growing professional occupations of the early 1900s greatly benefited from living next to a professional and educated neighbor.

[FIGURE 5 ABOUT HERE]

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 Table 11 we re-estimate 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 or manager in 1940 relative to other boys on the 1910 census sheet, but it does not increase the probability of being a 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. In levels, growing up next door to a doctor led to the largest increase in becoming a manager, official, or proprietor (by 0.011 percentage points). However, this is a very common occupation in 1940, with 14.5 percent of children on the same sheet as a doctor in 1910 entering that occupation. In a relative sense, growing up next door to a doctor had the largest percent effect on the boy becoming a doctor (41 percent), then a lawyer (16 percent), followed by managers, officials, and proprietors (8 percent). 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.

[TABLE 11 ABOUT HERE]

7 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 characteristics, neighborhood characteristics, and individual characteristics. In general, high-income and high-education occupations tend to be more transmissible. This is consistent with both information and exposure channels. Living next door to and interacting with someone in a high-income or high-education occupation can provide information and make the returns of that occupation salient. Similarly, knowing someone in a high-income, high-education occupation can 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.

The heterogeneity by neighborhood characteristics also seem to suggest an information or exposure mechanism at play. Transmission was largest among children in rural areas, with fewer immigrants, and more stable populations. These patterns are all consistent with stronger neighbor-to-neighbor relationships. As are the differences by individual characteristics. In general we observe patterns of homophily. Children are significantly more likely to enter the occupation of their childhood neighbor if the child's family is similar to the neighbor's family in terms of ethnicity, race, age of children, and socioeconomic status. These patterns are consistent with stronger social ties, which convey more information about or exposure to a particular occupation.

Regardless of the mechanism, childhood neighbors have significant, long-lasting impacts on children's economic outcomes. If a person's childhood neighbors were in particular occupations (such as doctor, lawyer, teacher, or clerical worker), the child observed higher income and educational attainment as an adult, relative to other children that grew up on the same street. Meanwhile, 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.

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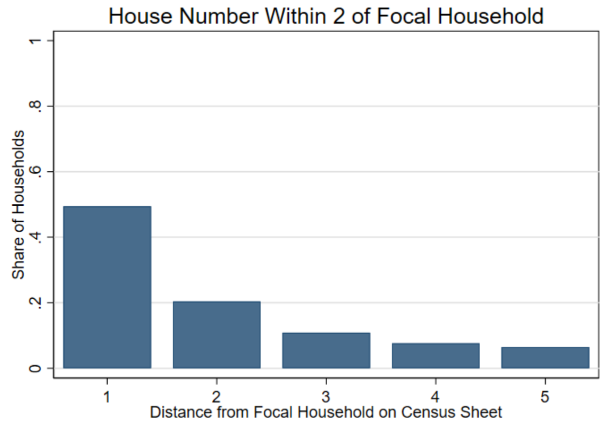
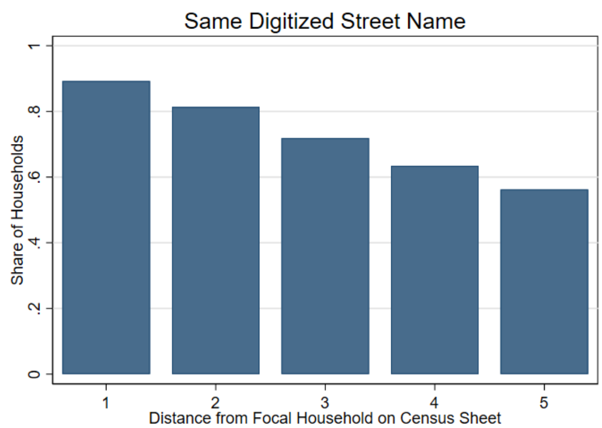
Tables and Figures

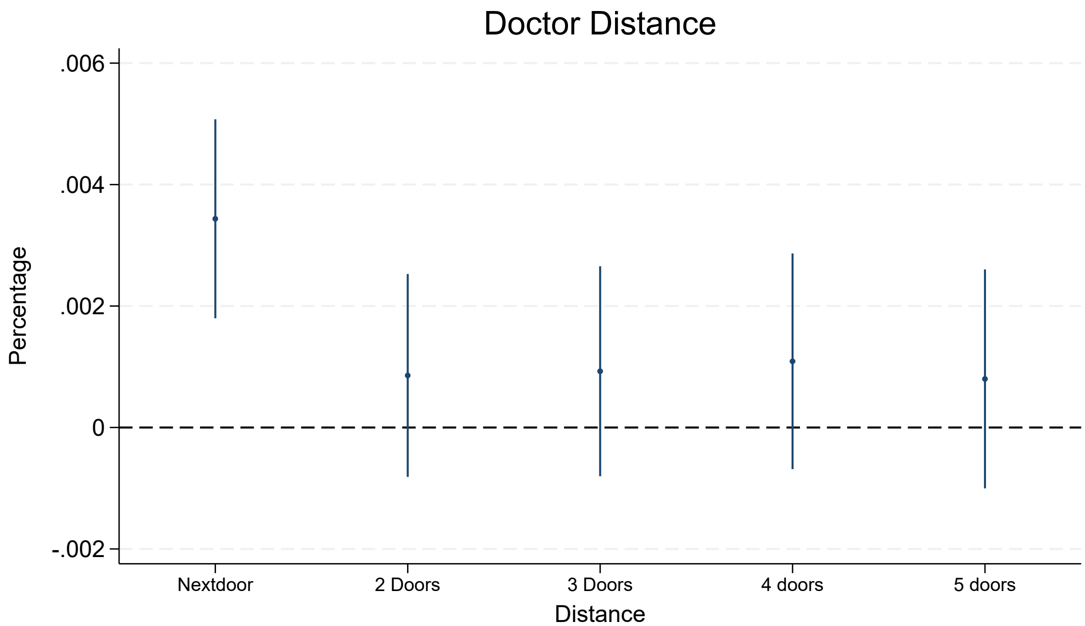
Figure 1: Sheet Proximity and Address Measures





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.

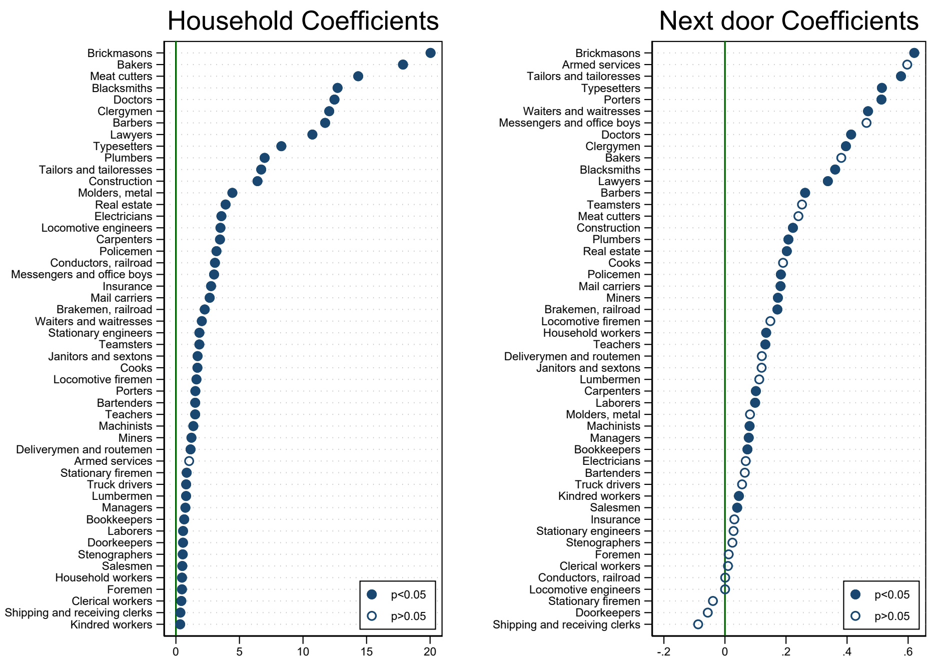
Figure 2: NeighborDoc1910 Coefficients



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

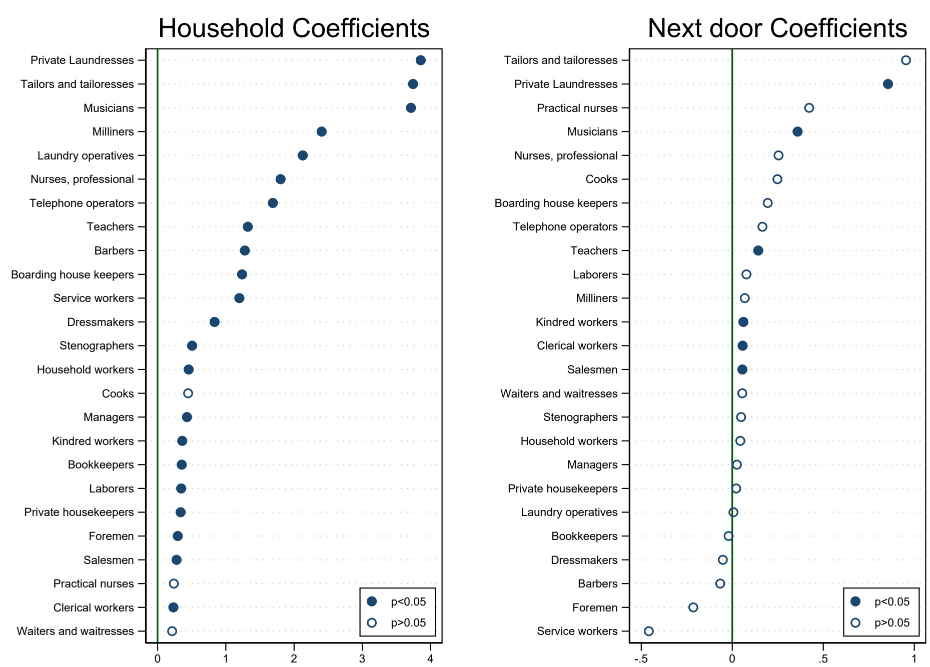
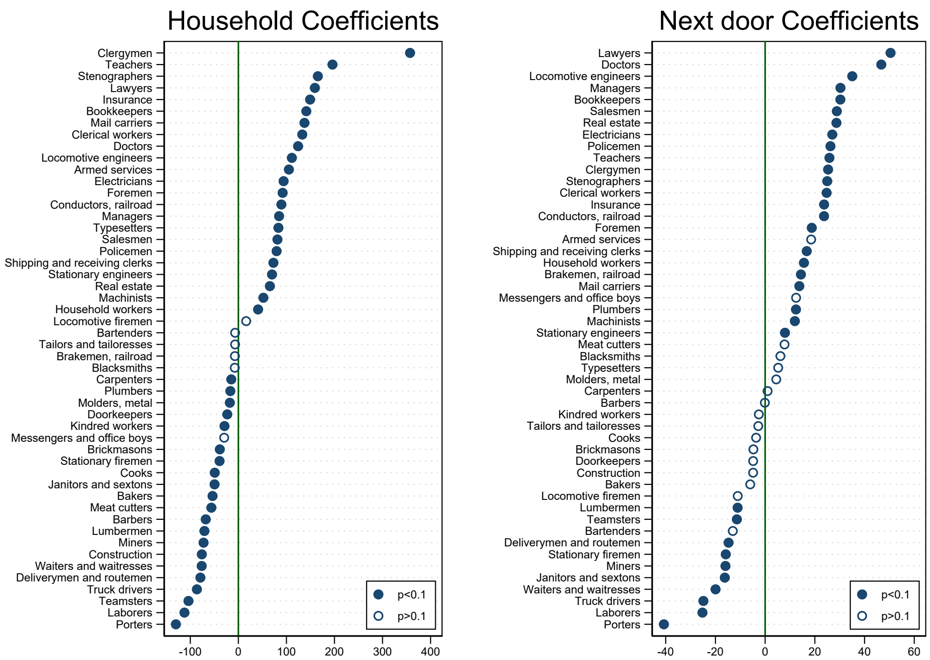


Figure 4: Household and Next Door Coefficients on Income

(a) 50 Largest Occupations for Men



(b) 25 Largest Occupations for Women

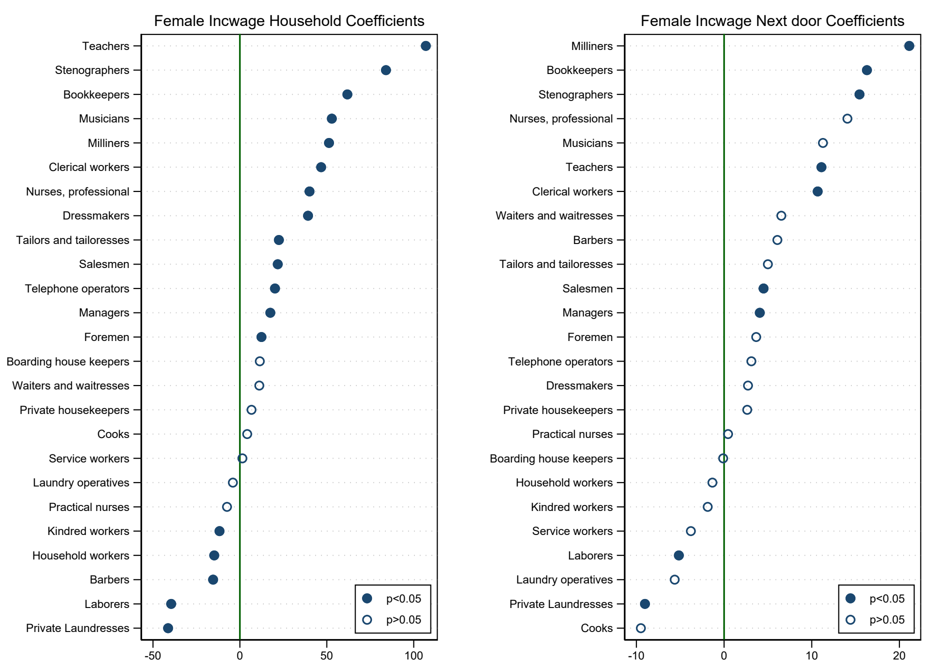
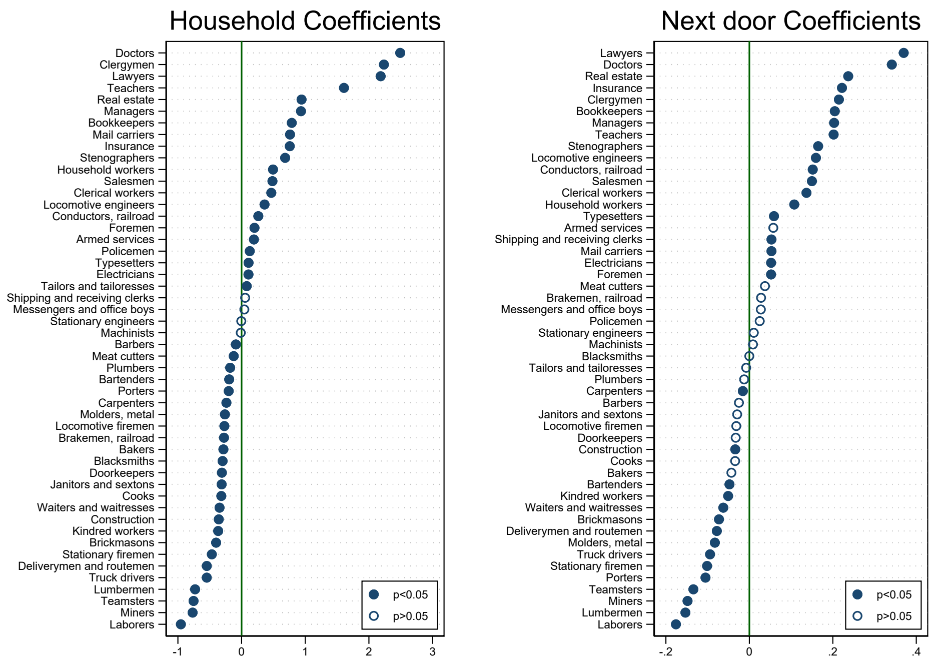


Figure 5: Household and Next Door Coefficients on Education

(a) 50 Largest Occupations for Men



(b) 25 Largest Occupations for Women

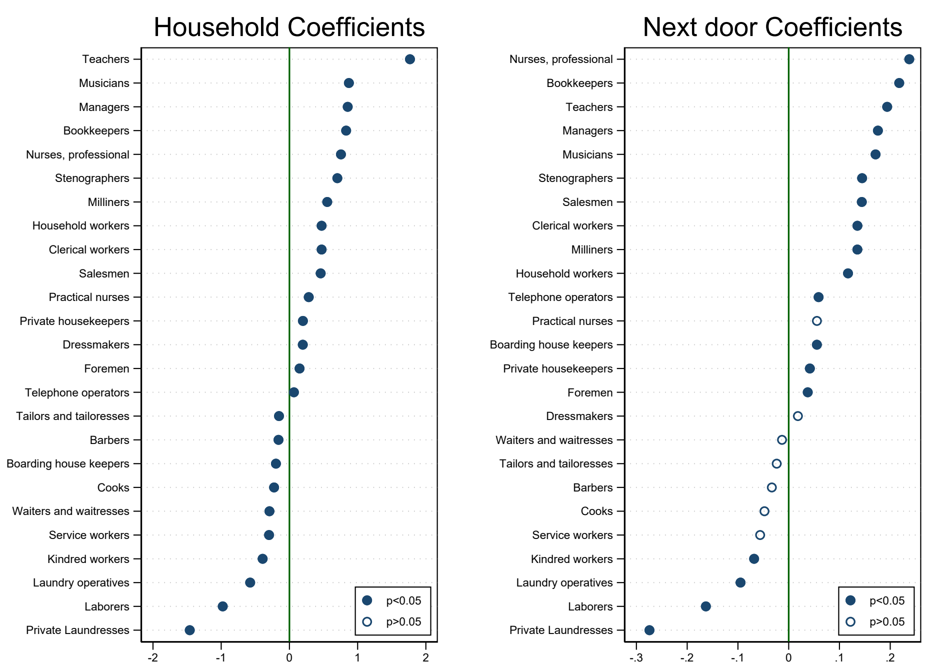


Table 1: Summary Statistics

	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
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

Dependent variable: Doctor occupation in 1940	All sheets	At least one doctor occupation per sheet: 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: Doctor Results Using Street Addresses

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 Results Linked to Various Censuses

Dependent variable: Doctor in…	1920 (1)	1930 (2)	1940 (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 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: 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 7: 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 8: Heterogeneity by Occupation Characteristics

Dependent variable: Target occupation in 1940	Baseline	High income	High education	High self-employment
	(1)	(2)	(3)	(4)
Own household	0.0381***	0.0537***	0.0439***	0.0571***
	0.0002	0.0003	0.0003	0.0003
Next door neighbor	0.0034***	0.0040***	0.0037***	0.0036***
	0.0001	0.0002	0.0002	0.0002
R-squared	0.323	0.337	0.323	0.339
Untreated mean	0.0331	0.0339	0.0421	0.0276
Observations	26,649,908	8,481,845	10,662,704	10,839,995

Notes: Column 1 includes all occupations. Columns 2-4 include occupations with income, education, and self-employment score above median respectively.

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

Table 9: Heterogeneity by Neighborhood Characteristics

Dependent variable: Target occupation in 1940	Baseline	Urban	Rural	High immigrant share	Low immigrant share	High out-of-state share	Low out-of-state share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own household	0.0381***	0.0360***	0.0415***	0.0366***	0.0437***	0.0356***	0.0434***
	0.0002	0.0002	0.0003	0.0002	0.0004	0.0002	0.0003
Next door neighbor	0.0034***	0.0028***	0.0042***	0.0029***	0.0048***	0.0028***	0.0046***
	0.0001	0.0002	0.0002	0.0001	0.0003	0.0001	0.0002
R-squared	0.323	0.331	0.311	0.322	0.328	0.324	0.322
Untreated mean	0.0331	0.0317	0.0356	0.0322	0.0370	0.0319	0.0360
Observations	26,649,908	16,639,195	10,009,934	21,176,906	5,472,864	18,313,568	8,336,069

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

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

Table 10: Heterogeneity by Neighbor Similarity

(a)

Dependent variable: Target occupation in 1940	Baseline	Birthplace	Birth country	Race	Last name	Same age	Income score	Education score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own household	0.0381***	0.0381***	0.0381***	0.0381***	0.0381***	0.0382***	0.0376***	0.0375***
	0.0002	0.0002	0.0002	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***	0.0220***	0.0035***	0.0059***	0.0076***
		0.0002	0.0002	0.0001	0.0010	0.0003	0.0002	0.0002
Treatment 1 average		*0.0984*	*0.1595*	*0.2284*	*0.0041*	*0.0311*	*0.1300*	*0.1334*
Treatment 2: Different characteristic		0.0023***	0.0019***	-0.0083***	0.0030***	0.0031***	-0.0012***	-0.0038***
		0.0002	0.0002	0.0006	0.0001	0.0001	0.0002	0.0002
Treatment 2 average		*0.1453*	*0.0829*	*0.0086*	*0.2328*	*0.1639*	*0.0848*	*0.0814*
F-test		100.28	66.90	404.93	325.74	1.60	849.42	2223.73
Two-tailed p-value		0.0000	0.0000	0.0000	0.0000	0.2058	0.0000	0.0000
R-squared	0.323	0.323	0.323	0.323	0.323	0.323	0.323	0.323
Untreated mean	0.0331	0.0331	0.0331	0.0331	0.0331	0.0338	0.0338	0.0338
Observations	26,649,908	26,649,908	26,649,908	26,649,908	26,649,128	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 6, same age is defined as any child in the target occupation house being within one year of the focal child. In columns 7-8 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 5 is slightly less due to the fact that individuals whose last name is missing and whose neigbhors' last names are also missing are dropped from the regression.

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

(b)

Dependent variable: Target occupation in 1940	Baseline	White	Non-white	High income	Low income	High education	Low education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Own household	0.0381***	0.0373***	0.0198***	0.0437***	0.0323***	0.0395***	0.0325***
	0.0002	0.0002	0.0015	0.0004	0.0003	0.0005	0.0002
Next door neighbor	0.0034***						
	0.0001						
Treatment 1: Same characteristic		0.0029***	0.0008	0.0018***	0.0032***	0.0018***	0.0028***
		0.0001	0.0013	0.0005	0.0002	0.0005	0.0002
Treatment 1 average		*0.2298*	*0.2166*	*0.0855*	*0.1806*	*0.1202*	*0.1671*
Treatment 2: Different characteristic		0.0029***	0.0010	0.0031***	0.0028***	0.0029***	0.0032***
		0.0006	0.0023	0.0003	0.0003	0.0003	0.0003
Treatment 2 average		*0.0064*	*0.0483*	*0.1494*	*0.0573*	*0.1155*	*0.0708*
F-test		0.00	0.01	4.44	1.12	3.90	1.64
Two-tailed p-value		0.9948	0.9277	0.0352	0.2902	0.0483	0.2006
R-squared	0.323	0.321	0.484	0.428	0.367	0.432	0.358
Untreated mean	0.0331	0.0328	0.0461	0.0303	0.0345	0.0303	0.0343
Observations	26,649,908	25,682,014	661,760	6,935,351	14,073,164	6,427,565	14,802,656

Notes: In columns 2-7, treatments compare head of household of the focal child to the target occupation holder next door.

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

Table 11: Effects of Doctors on Entering the Top 5 Occupations by Income

Dependent variable: Target occupation in 1940	Doctors	Lawyers	Managers, Officials, or Proprietors	Foremen	Compositors and Typesetters	Any top five occupation
	(1)	(2)	(3)	(4)	(5)	(6)
Own household	0.0970***	0.0105***	-0.0021	-0.0061***	-0.0022***	0.0970***
	0.0018	0.0011	0.0027	0.0009	0.0005	0.0032
Next door neighbor	0.0032***	0.0021***	0.0118***	-0.0008	0.0006	0.0169***
	0.0007	0.0007	0.0021	0.0008	0.0004	0.0024
R-squared	0.327	0.310	0.309	0.266	0.286	0.315
Untreated mean	0.0078	0.0132	0.1454	0.0201	0.0058	0.1922
Observations	305,059	305,059	305,059	305,059	305,059	305,059

Notes: Estimates for the effect of having a doctor living next door in 1910 on being in the target occupation in 1940.
*p<0.1, **p<0.05, ***p<0.01.

A Additional Tables and Figures (Online Appendix)

Table A1: 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.

Table A2: 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.

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

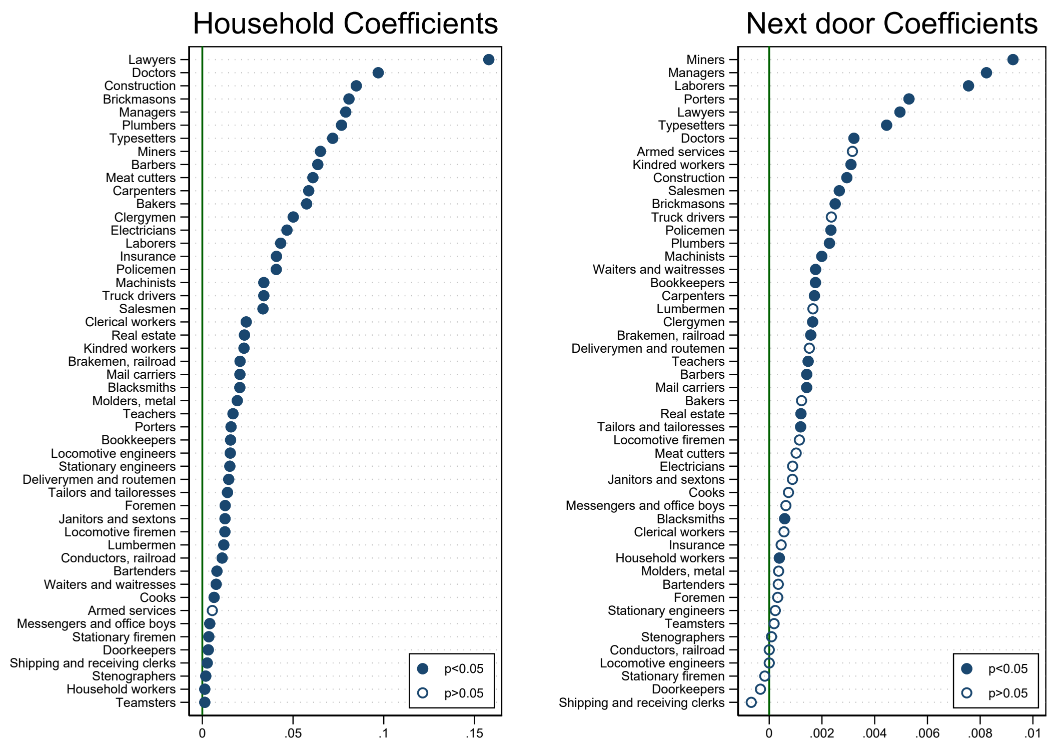


Table A3: Stacked Regression Results Using Street Addresses

	Sheet Ordering			Instrument	
Dependent variable:	Baseline	Street sample	Street ordering	First stage	Second stage
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.

Table A4: 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.

Table A5: Heterogeneity by Income and Education

	High income		Low income	
Dependent variable:	High education	Low education	High education	Low education
Target occupation in 1940	(1)	(2)	(3)	(4)
Own household	0.0609***	0.0396***	0.0242***	0.0334***
	0.0004	0.0004	0.0004	0.0002
Next door neighbor	0.0053***	0.0014***	0.0017***	0.0036***
	0.0003	0.0002	0.0003	0.0002
R-squared	0.334	0.290	0.301	0.322
Untreated mean	0.0485	0.0103	0.0358	0.0314
Observations	5,356,209	3,125,636	5,306,495	12,861,567

Notes: Each column reports results within four 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 A6: 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 A7: Heterogeneity by Neighbor Similarity for Last Names that Are Unique to Each Sheet

Dependent variable: Target occupation in 1940	Baseline	Birthplace	Birth country	Race	Last name	Same age	Income score	Education score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own household	0.0374***	0.0374***	0.0374***	0.0374***	0.0374***	0.0375***	0.0369***	0.0368***
	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002
Next door neighbor	0.0030***							
	0.0001							
Treatment 1: Same characteristic		0.0042***	0.0036***	0.0034***	0.0187***	0.0033***	0.0056***	0.0072***
		0.0002	0.0002	0.0001	0.0048	0.0004	0.0002	0.0002
Treatment 1 average		*0.0908*	*0.1541*	*0.2335*	*0.0002*	*0.0320*	*0.1274*	*0.1335*
Treatment 2: Different characteristic		0.0023***	0.0018***	-0.0074***	0.0030***	0.0027***	-0.0013***	-0.0039***
		0.0002	0.0002	0.0007	0.0001	0.0002	0.0002	0.0002
Treatment 2 average		*0.1580*	*0.0937*	*0.0084*	*0.2410*	*0.1661*	*0.0906*	*0.0845*
F-test		43.01	42.79	230.11	10.66	1.99	571.95	1515.42
Two-tailed p-value		0.0000	0.0000	0.0000	0.0011	0.1583	0.0000	0.0000
R-squared	0.3260	0.3260	0.3260	0.3260	0.3260	0.3260	0.3260	0.3261
Untreated mean	0.0323	0.0323	0.0323	0.0323	0.0323	0.0329	0.0330	0.0330
Observations	18,558,482	18,558,482	18,558,482	18,558,482	18,558,404	18,558,482	18,558,482	18,558,482

Notes: This table restricts to individuals with unique last names. In columns 2-5, treatments compare head of household of the focal child to the target occupation holder next door. In column 6, same age is defined as any child in the target occupation house being within one year of the focal child. In columns 7-8 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 5 is slightly less due to the fact that individuals whose last name is missing and whose neigbhors' last names are also missing are dropped from the regression.

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