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Place of Work and Place of Residence: Informal Hiring Networks and Labor Market Outcomes

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Abstract

We use a novel dataset and research design to empirically detect the effect of social interactions among neighbors on labor market outcomes. Specifically, using Census data that characterize residential and employment locations down to the city block, we examine whether individuals residing in the same block are more likely to work together than individuals in nearby but not identical blocks. We find significant evidence of social interactions operating at the block level residing on the same versus nearby blocks increases the probability of working together by over 33 percent. The results also indicate that this referral effect is stronger when individuals are similar in sociodemographic characteristics (e.g., both have children of similar ages) and when at least one individual is well attached to the labor market. These findings are robust across various specifications intended to address concerns related to sorting and reverse causation. Further, having determined the characteristics of a pair of individuals that lead to an especially strong referral effect, we provide evidence that the increased availability of neighborhood referrals has a significant impact on a wide range of labor market outcomes including employment and wages.

Journal of Economic Literature Classification: J6, R2

Keywords: Social Interactions, Informal Hiring Networks, Employment, Neighborhood Effects

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1 INTRODUCTION

The relevance of social networks and local interactions for economic outcomes has been increasingly recognized by economists in a variety of contexts.[1] An important strand of this literature has focused on the detection and measurement of social interactions that operate at the level of the residential neighborhood.[2] The proper identification of such neighborhood effects is complicated, however, by the non-random sorting of households into neighborhoods and the likely presence of unobserved individual and neighborhood attributes.[3] The resulting correlation in unobservables among neighbors can lead to serious bias in the estimation of social interaction among neighbors in the absence of a research design capable of distinguishing social interactions from these alternative explanations.[4]

In this paper, we propose a new empirical approach designed to identify neighborhood effects in observational data by isolating block-level variation in the characteristics of neighbors within narrowly-defined neighborhoods. In particular, using Census data that detail the block on which each individual in the Boston metropolitan area resides, we compare outcomes for neighbors that reside on the same versus nearby blocks, where nearby blocks are defined to be those in the same Census block group.[5] The key identifying assumption underlying this design (testable on observable attributes) is that there is no block-level correlation in unobserved attributes within block groups.

We use this approach to study the impact of neighborhood referrals on labor market outcomes. Rather than focusing on more general forms of neighborhood effects, we exploit the fact that our restricted Census dataset characterizes the precise location of both an individual's place of residence and place of work to study the propensity of neighbors to work together. Specifically, we examine the propensity of a pair of individuals to work in the same location,

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[1] Some recent examples include crime (Glaeser et al. (1996), Bayer et. al. (2004)); welfare program participation (Bertrand et al. (2000)); the adoption of new technologies (Conley and Udry (2003), Bandiera and Rasul (2003), Burke et al. (2004)); peer effects in education (Hoxby (2000), Sacerdote (2001), Zimmerman (2003), Zax and Rees (2002)); knowledge spillovers and economies of agglomeration (Jaffe et al. (1993), Audretsch and Feldman (1996), Glaeser et al. (1992)). For a more extensive review of the literature, both theoretical and empirical, see Brock and Durlauf (2001).

[2] Case and Katz (1991) explore the role of neighborhood effects on several behavioral outcomes using a spatially auto-regressive model. Crane (1991) also looks at neighborhood influences on social pathologies, focusing on non-linearities and threshold effects. Jencks and Mayer (1990) present a survey of the older literature on neighborhood effects.

[3] See Manski (1993) and Moffitt (2001) for a general discussion of the identification of social interactions in the presence of correlated unobservables.

[4] The recent literature on neighborhood effects has focused on the development and use of research methodologies designed to distinguish among these potential explanations. We provide a detailed discussion in Section 2 below.

[5] Census block groups are defined by the Census Bureau and contain an average of ten contiguous city blocks in our sample.

comparing such propensities for pairs of individuals that reside on the same versus nearby blocks within a block group. We take the propensity to work in the same location as an indication that one member of the pair provided a referral (or more generally information) to the other member about jobs available in her place of work.

Our results indicate the existence of significant social interactions at the block level; residing on the same versus nearby blocks increases the probability of working together by over 33 percent. As a consequence, individuals are about 6.9 percentage points more likely to work with at least one person on their block than they would be in the absence of referrals. This result is robust to the introduction of detailed controls for the characteristics of the individuals in the pair as well as across various specifications intended to address the possibility of within block group sorting and reverse causation.

Our analysis also indicates that there is considerable variation in the likelihood of referrals across different pairs of neighbors. We estimate, for example, that a referral is significantly more likely among pairs of high school graduates, pairs of young adults, and pairs in which members have children of a similar age. More generally, our findings are broadly consistent with two common empirical findings in the existing literature on social networks and on informal hiring channels: (i) that there is strong assortative matching within social networks and (ii) that referrals can only occur when at least one member of the pair is well-attached to the labor market.

This analysis of heterogeneous referral effects serves a second purpose in our analysis. In particular, it allows us to develop an individual-specific measure of the availability of referral opportunities on each block in the metropolitan area. The resulting estimate of match quality provides a novel measure of neighborhood quality based on the specific match between an individual's characteristics and those of her neighbors. We include this measure in a series of standard regressions for labor force participation, employment, wages, and earnings (along with block group fixed effects and controls for both individual and block-level neighbor attributes). Given that many workers that receive a referral would likely find employment through some other search method in the absence of a referral, the results of these regressions provide a direct measure of the ultimate impact of neighborhood referrals on labor market outcomes. The results of this portion of our analysis reveal that neighborhood referral effects have a (statistically and economically) significant positive impact on all labor market outcomes under consideration; a one standard deviation increase in the match quality, for example, raises expected labor force participation for the average individual by 1.6 percentage points and earnings by 3.8 percent in our preferred specification.

In presenting the results related to neighborhood referrals and labor market outcomes, we also provide direct evidence on the key identifying assumption underlying of our research design. In particular, we present evidence that the within-block group correlation in observable neighbor characteristics does not contribute to the increased propensity of individuals on the same block to work together. In fact, the analysis implies that based on their observable characteristics (including education, sex, marital status, race, age, presence of children, immigration status), pairs on the same block are actually slightly *less* likely to work together than those on nearby blocks. Thus, in as much as it is testable on the observables, our research design is robust to within-block group sorting.

In this way, in addition to providing new evidence on the importance of neighborhood referrals for labor market outcomes, our analysis also demonstrates the potential strengths of the general research design that we introduce in this paper. In a manner that deals directly with the correlation of individual and neighbor characteristics (e.g., due to sorting), this design allows for the identification of neighborhood effects operating (i) through a specific mechanism, (ii) for a broad population and a wide variety of subsets of that population, and (iii) for individuals that have resided in a neighborhood for a variety of tenure lengths. The applicability of this design extends to the study of neighborhood effects in other contexts (e.g., other metro areas, specific types of neighborhoods), on specific populations (e.g., youths), and for alternative outcomes (e.g., education, teenage fertility, health, welfare participation), provided the researcher can demonstrate that the within-block group correlation in observable neighbor characteristics does not contribute significantly to outcomes, thereby ensuring that the key identifying assumption on unobserved characteristics is at least plausible.

The remainder of the paper is organized as follows. Section 2 sets the paper in the context of the existing literature. Section 3 describes the data set that we have assembled for the Boston metropolitan area. Sections 4 and 5 describe our research design and present evidence concerning the orthogonality of the block-level variation in individual and neighbor characteristics. In these sections, we also discuss several extensions of our methodology designed to deal with additional issues related to identification. We report our empirical findings in Section 6 and conclude in Section 7.

2 RELATED LITERATURE

In setting forth a new empirical design for detecting and measuring the importance of neighborhood referrals on labor market outcomes, this paper has two main goals. The first is to contribute a new methodology that can be used to identify neighborhood effects. The second is to contribute new results to the empirical literatures on social network effects in job search and social interactions more generally. In this short section, we describe briefly how our approach relates to each of these literatures.

The Identification of Neighborhood Effects. The study of the identification of neighborhood effects is a difficult problem without a completely general solution. An important line of recent research seeks to identify neighborhood effects by isolating a random component of neighborhood choice induced by special social experiments. Popkin et al. (1993) pioneered this approach using data from the Gautreaux Program conducted in Chicago in the late 1970's, which gave housing vouchers to eligible black families in public housing as part of a court-imposed public housing de-segregation effort. Similarly, Oreopolous (2003) and Jacob (2005) study the impact of re-locations arising from administrative assignment to public housing projects in Toronto and from the demolition of the public housing projects in Chicago, respectively. Most notably, Katz et. al. (2001) and Ludwig et al. (2001) have used the randomized housing voucher allocation associated with the Moving To Opportunity demonstration (MTO) to examine the impact of re-location to neighborhoods with much lower poverty rates on a very wide set of individual behavioral outcomes including health, labor market activity, crime, education, and more. Especially in the case of MTO, the advantages of this approach are clear – the randomization inherent in the program design ensures a clean comparison of treatment and proper control groups.

There are, however, important limitations in the extent to which the treatment effects identified through re-location are informative about the nature of general forms of neighborhood effects *per se*. First, individuals studied must be eligible for a re-location program in the first place; this typically implies that the resulting sample is special (i.e. so as to be a resident in public housing) and may not be as sensitive to neighborhood effects as other individuals. Second, the experimental design involves re-location to new neighborhoods that are, by design, very different from baseline neighborhoods; this implies that the identified treatment effect measures the impact of re-locating to a neighborhood where individuals initially have few social contacts and where the individuals studied may be very different than the average resident of the new neighborhood. In this way, the treatment effects identified with this design are necessarily a composite of several factors related to significant *changes* in neighborhoods that are not easily disentangled (see Moffitt (2001) for a detailed discussion).

A second broad approach seeks to deal with the difficulties induced by correlation in unobserved attributes at the neighborhood level by aggregating to a higher level of geography.

Evans, Oates, and Schwab (1992), Cutler and Glaeser (1997), Ross (1998), Weinberg (2000, 2004), Ross and Zenou (2004), and Card and Rothstein (2005) identify the effect of location on outcomes using cross-metropolitan variation. For example, Cutler and Glaeser (1997) analyze the impact of segregation within a metropolitan area on a variety of outcomes including education, labor market activity, and teenage fertility, and Evans, Oates and Schwab use metropolitan area poverty rates as an instrument for neighborhood level poverty. Again, the advantages of this approach are clear – aggregation certainly eliminates the problem of correlation in unobservables among neighbors (although potential correlation in unobservables at the metropolitan level becomes an issue). The effects identified through aggregation, however, include not only the average *neighborhood* effects operating in a metropolitan area but also any broader consequences of living in a segregated or high poverty metropolitan area.[6] Thus, the strict interpretation of the estimated effects as neighborhood effects requires the assumption that metropolitan segregation does not directly affect outcomes.[7]

An interesting way to view the research design developed in this paper is as the converse of designs based on across metropolitan area variation. That is, instead of aggregating to the metropolitan level, we disaggregate below the level of the neighborhood to isolate block-level variation in neighbor attributes. While the strict identification of neighborhood effects with the across metropolitan area design requires the assumptions of no metropolitan effects and no correlation in unobservables at the metropolitan level, strict identification with our design requires the assumptions that social interactions among neighbors are very local in nature – operating at the level of the block – and that there is no correlation in unobservables across blocks within block groups.[8] In this way, we view the current paper as offering a complementary approach to the existing literature that allows researchers to identify a wide range of causal neighborhood effects using an alternative set of assumptions (testable on the observables) than have been used in previous studies.

Job Information Networks and Social Interactions within Neighborhoods. A wide array of studies have documented the relationship between the neighborhood environment and employment outcomes. Some important examples include Ihlanfeldt and Sjoquist (1990) who

[6] More residentially segregated metropolitan areas might be associated, for example, with increased racial taste-based discrimination in the labor market, in the application of criminal justice, etc. due to decreased levels of regular inter-racial contact in residential neighborhoods.
[7] It is important to point out that Cutler and Glaeser (1997) do not claim that the effects identified in their analysis are strictly a neighborhood effects.
[8] To the extent that interactions occur among neighbors at greater distances, our estimates reflect only the increased intensity of interaction at the block level.

find that youth residing far from suburban areas where low skill jobs tend to be located had worse employment outcomes, Case and Katz (1991) who find a correlation between youth idleness and the idleness of neighbors, O'Regan and Quigley (1998) who find that youth are more likely to be high school dropouts and unemployed when they reside in high poverty neighborhoods, and Weinberg, Reagan and Yankow (2004) who find that people who move to neighborhoods with worse attributes have worse employment outcomes.

Many scholars have suggested job market referrals or information networks as an important factor behind such neighborhood effects.[9] Rees and Schultz (1970), Corcoran et al. (1980), Holzer (1988), Blau and Robbins (1990), Blau (1992), Granovetter (1995), Addison and Portugal (2001) and Wahba and Zenou (2003) all document the importance of referrals and other informal hiring channels in the labor market, using both U.S. and non-U.S. data. A number of these studies including Holzer (1988) and Blau and Robbins (1990) find that informal referrals are more productive than more formal methods in terms of job offer and acceptance probabilities. Additional studies including Datcher (1983), Devine and Kiefer (1991), Marmaros and Sacerdote (2002), and Loury (2004) find evidence that use of informal networks increases the quality of the match as captured by job tenure or earnings.[10]

Moreover, considerable evidence exists to suggest that the use of and impact of job information networks varies across demographic groups. According to Ioannides and Loury, the evidence on usage differences is mixed in general, but suggests that women and workers with higher education levels are less likely to use informal job networks. In terms of relative productivity, Bortnick and Ports (1992) found that these networks were slightly less productive for women as compared to men. Holzer (1987), Bortnick and Ports (1992), and Korenman and Turner (1996) found that such networks were substantially less productive for African-Americans. Ioannides and Loury (2004) provide a detailed survey of the literature on job information networks.

[9] The use of informal channels such as referrals by employers can be rationalized as a means to reduce the uncertainty regarding the quality of a prospective employee. Montgomery (1991) was the first to formally model a labor market in which both formal and informal hiring channels coexist. Focusing more closely on the information exchange among workers, Calvo-Armengol and Jackson (2002) analyze an explicit network model of job search in which agents receive random offers and decide whether to use them themselves or pass them on to their unemployed contacts depending on their own employment status and current wage.

[10] See Elliot (1999) and Loury (2003) for counter examples where the use of informal networks led to lower wages. Of course, the lower wages may be associated with increased match quality on desirable job attributes causing the individual to accept a lower wage as a compensating differential.

Only a small number of studies attempt to quantify the impact of specific social interactions or exposures on outcomes,[11] and these studies tend to be outside of the labor market context. Bertrand, Luttmer, and Mullainathan (2000) examine the relationship between an individual's own welfare participation and the welfare participation rate among those who speak the same language as this individual. They find a strong positive relationship suggesting the existence and impact of language specific social networks. Similarly, Aizer and Currie (2004) find evidence that the prenatal care use of pregnant women is most likely to be influenced by the behavior of new mothers in the same ethnic group as compared to mothers in different ethnic groups who reside in the same neighborhood. In the labor market context, Topa (2001) finds that employment in a given Census tract is positively affected by average employment in neighboring tracts when these tracts are located within a common, larger local community (as defined by their residents).

Our paper adds generally to the body of evidence suggesting that social networks have a substantial impact on labor market outcomes, and more specifically to this small, but very important literature, on the heterogeneous use of social contacts by individuals and how that use differs with respect to their observed characteristics. Our analysis indicates that there is considerable variation in the likelihood of successful referrals across different pairs of neighbors. Further, this heterogeneity in referral effects enables us to construct a proxy for match quality at the block level that we use in the second stage of our analysis to quantify the economic impact of referrals. Our research design and unique dataset allow us to focus very closely on a specific mechanism through which social interactions at the local level may operate, namely referrals and information about job opportunities, while still carefully addressing methodological concerns arising from sorting across neighborhoods.

3 DATA

The data for our analysis are drawn from a restricted version of the 1990 US Census of Population for the Boston metropolitan area. For the full (1-in-7) sample of individuals that filled out the long form of the Census, these data contain the complete set of variables that are available in the public-use version of the Census PUMS, but, in addition, detail each individual's

[11] Even if such analyses were conducted using referral data, the results would quite likely be based on self-reported networks that arise from individual choices. The studies cited below look at the impact of exposure to possible social networks, which are presumably less endogenous than the actual networks accessed by the individual. The key exception to this statement is Marmaros and Sacerdote (2002), who base their analysis on exposure to a randomly assigned roommate in a college dormitory. See Arcidiacono and Vigdor (2004) and Weinberg (2004) for recent studies that document sorting/assortive matching in the process of forming social networks using data on college and high school students, respectively.

residential and employment locations down to the Census block level. In addition to these geographic variables, the Census also provides a wide range of sociodemographic information: age, gender and marital status, education, race, family structure, and duration in the residence as well as information on labor market outcomes including labor force status, salary and wage income if employed, occupation, and industry.

With regard to the geographic structure of the data, Census blocks correspond roughly to actual city blocks; they are typically rectangular regions delimited by the four intersections that constitute the corners of the block.[12] Our sample consists of approximately 25,500 Census blocks arranged into 2,565 block groups, i.e., an average of 10 blocks per block group. The distribution of blocks per block group is depicted in Figure 1; the median number of blocks per block group is 8, and about 95 percent of all block groups have 20 blocks or fewer.

It is the precise geographical information for each individual in these restricted Census data that provides the backbone of our research design, permitting us to isolate the block-level variation in neighbor exposure by conditioning on block group fixed effects. The first stage of our analysis considers the propensity of a pair of individuals to work in the same location, comparing this propensity for a pair that live on the same versus nearby blocks. For this portion of our analysis, we construct a sample that contains of individuals that (i) are currently employed, (ii) are between 25 and 59 years of age, (iii) do not live and work in the same block, and (iv) for whom the Census data on place of work has not been imputed.[13] The total number of workers in the Census sample that meet these criteria is 129,175 (5.1 per block, 50 per block group). Figure 2 reports the corresponding histogram of workers meeting these criteria across blocks.[14]

In constructing a sample of pairs for our analysis, we apply two additional critieria, selecting all pairs that (i) reside in the same block group within the Boston metropolitan area and

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[12] Notice that this definition implies that Census blocks are not constituted as the set of buildings that face each other on the same street. To the extent that social interactions are also strong between residents on opposite sides of the same street, a comparison of interactions between individuals that reside on the same Census block versus other blocks in the same block group will tend to understate the increased effect of immediate neighbors as those on the opposite side of the same street will count in the control group. For some blocks, however, one may argue that the opposite holds: streets may effectively act as dividers of local communities, and interactions may be strongest in the alleys and courtyards connecting the rear sides of buildings on the same block. In either case, our research design should detect (although may understate) particularly local interactions provided that the block group contains a reasonable number of blocks.

[13] *Currently employed* refers to the reference week in the calendar year 1990 used by the Census. We focus on prime-age adults in this paper so as to avoid empirical issues related to the labor market participation versus continued schooling of youths and young adults. We drop all individuals for which place of work is imputed for obvious reasons. We also drop all individuals that work in the same block in which they reside to avoid any overstatement of referral effects due, for example, to the clustering of small businesses and other retail shops on commercial blocks within block groups.

[14] In the analysis below, we consider specifications that limit the analysis to blocks with five or more sample workers.

(ii) do not belong to the same household. Overall, the sample contains 2,037,600 pairs that meet all of the above criteria. The first column of Table 1 characterizes this sample of matched pairs, reporting the percentage of pairs that fit the description in the row heading: at least one member of roughly 72 percent of the pairs has children; about 15 percent of pairs match two single individuals.[15]

Examining the characteristics of the sample of pairs shown in Table 1 highlights three key dimensions of heterogeneity in which our study will be limited due to the small size of the corresponding sample in the Boston metro area. In particular, (i) only 0.53 percent of all pairs reflect a match between two high school dropouts, (ii) only 1.59 percent of all pairs reflect a match between two non-white workers, and (iii) only 1.92 percent of all pairs reflect a match between two immigrants. Given the nature of the sample, it is not surprising that the our analysis tends to be more precise in other dimensions of individual heterogeneity including age, the presence of children, education (aside from high school dropouts), gender, and marital status.

For the second stage of our analysis, which examines the impact of neighborhood characteristics on labor market outcomes including labor force participation and employment, we add those prime age (25 to 59) individuals that are not currently employed; this sample has 163,594 observations.[16] Table 2 reports summary statistics for this sample. The first column reports the sample frequencies for each individual characteristic, while the remaining five columns report labor market and commuting information: the fraction of individuals that are currently employed, average weeks worked in the previous year, average hours worked per week in the previous year, average earnings for the sample of individuals that were fully-employed in the previous year, and average commute for those that are currently employed.[17] College graduates, married males, and whites display the strongest attachment to the labor force, with respect to employment rates as well as hours and weeks worked. These groups also tend to work the farthest away from home. On the other hand, high school dropouts and married females tend to have weak labor force attachment and work close to home when employed.

4 EMPIRICAL DESIGN – DETECTING REFERRAL EFFECTS

[15] It should be noted that the sample contains only a small fraction of Asians and Hispanics and so these two groups are combined. Specifications where these groups are separated yield very similar results.

[16] We again limit the sample used in each labor market outcome equation to individuals for which the corresponding dependent variable has not been imputed.

[17] The Census provides information on current employment and labor force participation as well as the location of current workplace at the time of the survey in April 1990. Information on earnings, hours, and weeks are reported for the previous year. *Fully-employed* in 1989 refers to any individual who worked at least 40 weeks and at least 30 hours per week.

Given the structure of the dataset just described, it is straightforward to characterize our general research design. Our primary analysis explores the propensity for two individuals to work in the same location, comparing this propensity for a pair that lives in the same block with that of a pair that lives in the same block group but not the same block. The implementation of this design is straightforward and can be summarized in the following equation:

$$(1) \qquad W_{ij}^{b} = \rho_g + \alpha_0 R_{ij}^{b} + \varepsilon_{ij}$$

where i and j denote two individuals that reside in the same Census block group but not in the same household, W_{ij}^{b} is a dummy variable that is equal to one if i and j work in the same Census block, R_{ij}^{b} is a dummy variable that is equal to one if i and j reside in the same Census block, and ρ_g denotes the residential block group fixed effect – this is the baseline probability of working in the same block for individuals residing in the same block group. The statistical test of the null hypothesis that no local social interaction effect exists is simply a test of whether the estimated coefficient α_0 equals zero.

The inclusion of block group fixed effects in equation (1) is designed to control for any correlation in unobserved attributes among individuals residing in the same neighborhood. Such correlation can arise because of positive sorting into neighborhoods or because of unobserved factors present in those neighborhoods (e.g. similar access to the urban transportation network).[18]

In interpreting α_0 as a social interaction effect, therefore, we are implicitly making two key identification assumptions. First, that while individuals are able to choose their residential neighborhood (block group), there is no correlation in unobserved factors affecting work location among individuals residing on the same block within a block group. The plausibility of this assumption is motivated by two considerations. First, that the thinness of the housing market at such small geographic scales – the vast majority of block groups in our sample are less than 0.10 square miles in area – restricts an individual's ability to choose a specific block versus

[18] See Manski (1993) or Moffitt (2001) for a detailed discussion of these issues. It is also worth noting that due to the unique design of this analysis, the "reflection problem" studied by Manski (1993) does not have an obvious analogue for this portion of our analysis. Manski shows that it is generally impossible to distinguish the impact of group average outcomes from group average characteristics on individual outcomes because of the simultaneity in the determination of the individual outcomes. Because the dependent variable in our framework is a joint outcome for a pair of individuals and the identification of the existence of social effects is based on comparisons across different geographic scales rather than on correlations with group averages, the simultaneity issue does not arise in our context.

neighborhood.[19] Secondly, that it may be difficult for individuals to identify block-by-block variation in neighbor characteristics at the time of purchase or lease. That is, while an individual may have a reasonable sense of the socio-demographic structure of the neighborhood more generally, that variation across blocks within a neighborhood is less easily observed *a priori*.

The second key assumption is that interactions with neighbors are very local in nature – i.e., occur mostly among individuals on the same block. To the extent that individuals do have some interaction with neighbors on surrounding blocks, our design will provide only a lower bound on the overall strength of local interactions – measuring only the difference between these very local and broader effects. In this way, the design will allow us to detect interactions provided that they are significantly stronger at closer distances, but may still understate the strength of those interactions.

A Diagnostic Test of the Identifying Assumption. To examine whether our first key assumption – that there is no correlation in unobserved factors affecting work location among individuals residing on the same block within a block group – is reasonable, we analyze the correlation between observable individual and neighbor characteristics at the block level. While this kind of test does not prove anything with respect to the importance of potential correlation in unobserved factors, it certainly provides an indication of whether this assumption is at all reasonable.[20] In particular, for each block in the sample, a single prime age adult is selected and the characteristics of other individuals that reside in the same block but not the same household are used to construct a measure of average neighbor characteristics.[21] The first three columns of Table 3 report the

[19] In fact, only 11 percent of the blocks in our sample have an owner-occupied unit that changed owners in the 2 years prior to the Census. Given that the Census is a 1-in-7 sample and assuming a uniform probability for a house to be on the market in this two year period, this implies that the chances that *any* owner-occupied unit is available on a given block within a given 3 month period is only about 11 percent. Thus, it may be difficult for households searching in a given timeframe to select a house on a particular block. The comparable figure for renter-occupied units for blocks that contain at least one rental unit in our sample is 45 percent. This suggests that it is generally easier, although far from certain, for renters to find housing on a specific block within a particular search window.

[20] This is in the same vein of Altonji et al. (forthcoming): their approach to correct for selection bias suggests that selectivity in terms of unobserved heterogeneity is in some sense proportional to selectivity on observables.

[21] By sampling only one individual per block, we avoid inducing a mechanical negative correlation that would come about if all individuals were used in estimating the correlation between individual and average neighbor characteristics. This negative correlation arises because each individual is counted as a neighbor for all of the others in the same block, but not for herself. For estimates of the correlation that do not condition on block group fixed effects, this bias is inconsequential because an individual's own characteristics contribute very little to the average neighborhood characteristics of others in the full sample. For estimates that condition on block group fixed effects, however, this negative bias is quite large in magnitude because an individual's own characteristics contribute a significant amount to the average neighborhood characteristics of others within the same block group. By sampling only one individual per

average correlations for the full sample: the first column reports unconditional correlations, while the second conditions on block group fixed effects, and the third includes, in addition, specifically, whether the house is rented or owned and its corresponding rent or self-reported value, respectively.[22] In each case, both the individual and block measures are first regressed on the corresponding variables (e.g., block group fixed effects) and the correlation between the residuals is reported.

The results indicate a significant amount of sorting on the basis of education, race, age, and the presence of children across the neighborhoods of the metropolitan area as a whole. The correlation between whether an individual is a college graduate and the fraction of neighbors that are college graduates is 0.21, while that between whether an individual is black and the fraction of black neighbors is 0.56. The second and third columns provide an explicit test of our identification strategy, providing a measure of sorting on observables within block groups. As these successive columns clearly demonstrate, the correlation between observable individual and neighbor characteristics falls to near zero as only within-block group variation is isolated. The inclusion of block group fixed effects reduces the estimated correlations by 70-75 percent on average, with a remaining maximum correlation of 0.07 across all characteristics and 0.04 across all characteristics except race. The inclusion of housing characteristics, which is intended to control for the fact that some within-block group sorting would be expected if the housing stock differed significantly across blocks within a block group either in terms of prices or tenure of occupancy, drives these estimated correlations slightly closer to zero.

The second set of three columns in Table 3 reports average correlations for a sample of blocks with at least five sampled workers. We drop blocks with a small number of workers at various points throughout our analysis for two reasons. First, blocks with a small number of residents are largely non-residential and, consequently, interactions among neighbors may be limited on such blocks. Second, as we discuss in greater detail below, a measurement error arises related to the use of the 1-in-7 sample of individuals observed in the Census to estimate neighborhood effects. In this case, blocks with only a small number of workers may be particularly prone to measurement error.[23] This concern about the full sample is substantiated in

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block, we report an unbiased estimate of the correlation between individual and neighborhood characteristics at the block level.

[22] The housing controls include whether both individuals reside in owner-occupied housing, whether both individuals reside in rental housing, the average rent or house price for two households if both are owners or both renters, and the absolute value of the difference in rent or house price if both are owners or both renters.

[23] In particular, a bias is induced in the estimated correlations reported here as a result of the fact that the average block characteristics are constructed from a (1-in-7) sample of individuals rather than a complete

the unconditional correlation estimates, as these are significantly greater in a number of cases. The correlation estimates that condition on block group fixed effects, however, are generally of the same magnitude as those reported for the full sample. Moreover, the estimates that condition in addition on housing characteristics are in many cases even smaller than those reported for the full sample.

The magnitude of the remaining correlation between individual and neighbor attributes within block groups provides clear support for the notion that the amount of sorting within block groups on observables is less extensive than across the neighborhoods of the metropolitan area as a whole. This evidence is particularly compelling for our identification strategy because a number of these attributes, such as residents' race or the presence of families with children, would be the characteristics of one's immediate neighbors that might be most observable at the time of moving into a new residence. Thus, by controlling for these observables, it may be the case that within-block group sorting on other characteristics is even less extensive. While the correlation estimates reported in Table 3 are small, however, they are not identically zero. An obvious question, then, is whether the remaining block-by-block sorting on the basis of observables within block groups, small though it may be, is enough to significantly increase the propensity of pairs drawn from the same block within a block group to work together. We provide additional evidence on this question after first introducing a heterogeneous version of the model.

Heterogeneous Specification. The initial specification shown in equation can easily be extended to include a set of covariates X_{ij} that describe the pair of individuals (e.g., those summarized in Table 1) both in levels and interacted with R_{ij}^{b}:

(2) $$W_{ij}^{b} = \rho_g + \beta' X_{ij} + \left(\alpha_0 + \alpha_1' X_{ij}\right) R_{ij}^{b} + \varepsilon_{ij}$$

In this case, the estimated coefficients on the cross terms, α_1, allow us to investigate whether the social interaction effect is weaker or stronger for specific socio-demographic characteristics of the matched pair. There are two aspects to this: first, certain pairs are more likely to interact because of the assortative matching present in social networks: for instance, two individuals of similar

census of neighbors. This bias is present, however, in each specification reported in Table 3 and, importantly, should not generally be greater in the specification that conditions on block group fixed effects than in the unconditional specification. We confirmed this with Monte Carlo simulations. The results for the sample of blocks with five workers or more also is supportive of this notion, as measurement error should be substantially lower in this sample and yet the decrease in the estimated coefficients from the age, education, race, or with children of similar age.[24] Second, certain individuals may be more strongly attached to the labor market and may thus provide better referrals or information on jobs – for example, college graduates, married males or individuals with children. In this case, matches between pairs in which one individual is strongly attached to the labor market and the other generally more likely to need a referral should also lead to an increased number of referrals.

In equation (2), $\beta'X$ measures how the propensity to work together of two individuals that reside in the same block group but *not* the same block varies with the observable characteristics of the pair. Given an estimate of $\hat{\beta}$, this heterogeneous specification provides a way to test whether the remaining within-block group correlation between observable neighbor attributes would lead to a significantly higher predicted propensity for pairs on the same block to work together. Specifically, we compare the average $\hat{\beta}'X$ for those pairs that reside on the same block with those that reside on nearby (but not the same) blocks within the block group.[25] Given the $\hat{\beta}$ that we estimate below, the results of this test are as follows. The predicted propensity for pairs that reside on the same block is 0.343 percent; this is 0.01 percentage points *lower* than the observed (and predicted) propensity for pairs that reside in the same block group but not on the same block (0.355). Thus, the remaining block-level sorting on observables does not predict *any* increased propensity for individuals on the same block to work together. This evidence strongly favors the notion that our research design is credible in the face of the small amount of within-block sorting that exists in the data.

Another competing potential explanation for the finding of a greater propensity of pairs to work together at the block versus block group level is that this propensity is simply a declining function of the distance between any pair of individuals in the metropolitan area. While we do not address this possibility directly in the analysis, two aspects of the results that follow are important in ruling out this potential explanation. First, the magnitude of the social effect that we identify is large relative to the underlying propensity for two individuals in the same block group to work together. In this way, one would have to believe that slight differences (i.e., one- or two-block distances) in access to mass transportation stations or highways, for example, could cause a large increase in the propensity of individuals to work together at the block versus block group level.

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unconditional specification to the specification that conditions on block group fixed effects is greater in this sample.

[24] See Marsden (1987), (1988) for a discussion of the evidence from the General Social Survey on assortative matching in networks.

[25] This model is also rerun using the housing controls that were used our diagnostic test, the analysis of the correlation between individual and neighborhood attributed. As will be seen below, results are quite comparable across specifications.

A second way to gauge whether the increased proximity of individuals at the block level is a concern is to compare the coefficient estimates for the matched pair's covariates X_{ij}, in levels and as interactions with the block dummy R_{ij}^{b} (i.e., β and α_1, respectively). Assuming that the same factors that affect the propensity to work together at the neighborhood level are simply stronger at the block level, then one would expect to see a result at the block level (namely, in α_1) that is qualitatively similar and slightly larger (overall) in magnitude. As we discuss below, this is clearly not the case in our empirical analysis; in many cases β and α_1 have the opposite sign [26]

Additional Specifications and Robustness. As described above, our empirical design relies critically on the assumption that social interactions are especially strong at the block level, while households are only able to choose a block group at the time of the location decision, due perhaps to the thinness of the housing market. While the analysis of correlation between observable neighbor characteristics described above provides assurance that this assumption is reasonable, we also consider the robustness of our results to alternative samples designed to isolate those block groups that are most homogenous along a number of dimensions including: race, education, the presence of children in the household, and immigration status. In particular, in each case, we select the 50 percent of block groups that display the least amount of within-block group correlation between the corresponding individual and neighbor characteristics and re-estimate the baseline model for the restricted sample in order to see if our results are robust across samples.[27]

A separate confounding issue is the possibility that the estimated social interaction effect may be due to reverse causation: workers could receive tips and referrals about residential locations from their co-workers at a given firm. We address this issue in several ways. First, the empirical focus on the difference between block group- and block-level propensities again mitigates this problem because residential referrals are unlikely to result in people residing in exactly the same block, due to the thinness of the housing market at the block level.

We also tackle the potential for reverse causation directly by estimating equations (1) and (2) on a sub-sample of the data in which both respondents in a given matched pair have lived in that neighborhood for at least two years, but one of them was not employed for the full year in the previous year, defined as having worked less than 45 weeks in 1989. In this case, we can be fairly

[26] The limitation of this argument should also be clear. When there are several biases that work in different directions, the relative magnitudes of the biases may change as we shift the level of geography and as a result the sign of the bias might reverse. For example, at the block group level, most of the results may be driven by individual observable heterogeneity, but at the block level residential sorting on unobservable might become more important.

certain that if we see the same individuals working together in the current year then the referral was among residential neighbors rather than work colleagues. Unfortunately the Census does not contain any direct information on job search activity. Therefore, we use the "not employed for the full year in 1989" category as a proxy for the set of individuals who are most likely to have been actively searching for a job last year.[28] We also estimate an intermediate specification using the sub-sample of pairs whose members were both in residence at least two years, and adding controls for whether one and/or both individuals were not employed for the full year in 1989. The goal of this analysis is to examine whether evidence of referrals is present in this sub-sample. Importantly, because this sub-sample is (by construction) very different from the main sample, we do not expect the resulting model of social interactions to be identical to our baseline results. As a result, there is no reason to believe that the referral effect will be stronger for matches in this subsample or even to believe that the estimated parameters will be stable over this subsample. The strength, rate of utilization, and the form of the local referral network are likely to differ based on how long an individuals resides in a neighborhood.

Inference. Finally, a word about inference. The sampling scheme, which is based on drawing matched pairs of individuals who reside in the same block group, makes it very difficult to compute appropriate standard errors for our estimates. In particular, the observations in our sample -- pairs of individuals in the same block group -- do not constitute a random sample. In fact, suppose that individuals *a* and *b* work in the same block. Suppose further that individuals *b* and *c* work in the same block. Then, by transitivity, individuals *a* and *c* must also work in the same block. As a consequence, if we compute standard errors via the basic OLS formula, we may tend to understate their size because we are not taking into account this inherent correlation structure in the data. There is also the reasonable concern of heteroscedasticity across block groups that may bias standard errors in fixed effects analyses. In fact, the use of the linear probability model assures heteroscedastic errors. To address these issues, all standard errors in the match model are estimated based on pairwise bootstraps. It should be noted that some concerns have been raised concerning pairwise bootstrap in small samples (Horowitz, 2000). While our sample is quite large, we have a very small number of ones in our dependent variable, which may create similar problems. We verified the accuracy of the pairwise bootstraps by also estimating

[27] While the resulting analysis obviously changes the nature of the sample, the results described below do provide some re-assurance that our baseline results are not sensitive to sorting.

[28] Note that in estimating earnings and wage equations in Tables 6 and 7 we condition on a set of individuals that were *fully-employed* in the previous year defined as having worked at least 40 weeks and at

standard errors using a pairwise bootstrap with the HC_3 correction and also with a wild bootstrap (Mammen (1993); Flachaire (1999), (forthcoming)).[29]

5 EMPIRICAL DESIGN – LABOR MARKET OUTCOMES

Having analyzed the impact of local interactions on job referrals, the second portion of our analysis examines whether such referrals have an impact on labor market outcomes more generally. In particular, given the characterization of how the strength of social interactions related to job referrals (i.e., the propensity to work together) varies with the attributes of a pair of individuals identified in the first portion of our analysis, we explore whether an individual's labor market outcomes are related to the idiosyncratic quality of the strength of the potential networks available on her block. Specifically, we estimate a series of labor market outcome regressions that include a measure of match quality defined at the individual level along with controls for individual and average neighbor characteristics (measured at the block level) as well as block group fixed effects.

The goals of this portion of our analysis are two-fold. First, since we detect informal hiring effects indirectly, it serves as a check on the plausibility of the first portion of our analysis. Second, by focusing on outcomes we hope to be able to provide a better sense of the magnitude of our estimated network effects. It is certainly possible that referrals may be more likely among neighbors but may have little effect on labor market outcomes – i.e., that without the referral the individual would find a comparable job through another search method. In addition, our labor market models are less likely to understate the effect of referrals when compared to the referral effects model described in the previous section. In particular, with limited sorting within block groups, expected match quality for individual with others in the same block group is the same as their actual block match quality. Consequently, the block level index for match quality is likely to capture the effect of referrals both within the block and from neighboring blocks.

For this analysis, the unit of observation is an individual rather than a pair. For the employment and labor force participation outcomes, the econometric model is a linear probability

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least 30 hours per week. This definition is different than that for *not employed for the full year in 1989* used here, which is not at all based on hours.

[29] Pairwise bootstraps are estimated using a sample based on the pair of the predicted value and the predicted residual for each observation. The HC_3 correction scales the predicted residual for each observation by the estimated variance of the predicted residual for that observation while the wild bootstrap multiplies the predicted residual for each observation by a random number.

model.[30] For all other outcomes, such as weeks worked, hours-per-week worked, wages and earnings (in logs), we use a simple linear regression.

We then add – for each model specification – a 'network quality' proxy variable for each individual, which is constructed by examining that individual's matches with other adults in her block, using the coefficient estimates α_1 from the estimation of equation (2). Specifically, the average match quality for individual *i*, Q_i, is constructed using a sample of all possible pairings of individual *i* with other individuals who reside in the same block and do not belong to the same household. For each pair, a linear combination M_{ij} of the pair's covariates is created using the estimated parameters from the interaction of these variables with R_{ij}^b in equation (2): $M_{ij} = \hat{\alpha}_1^{'} X_{ij}$. Then, Q_i is computed as the mean value of M_{ij} over all matches for individual *i*:

(3) $$Q_i = \frac{1}{|N_i|} \sum_{j \in N_i} M_{ij}$$

where N_i is defined as the set of other individuals that reside on the same block but not in the same household as individual *i.*

We would generally expect individuals with good matches in their block – high value of Q_i – to have better labor force outcomes on average, after controlling for the direct effect of their attributes, the average attributes of their block, and block group fixed effects. We repeat the analysis for each of the various specifications described in Section 4 to address the sorting and reverse causation issues. In particular, by using a sub-sample of individuals that were not fully employed last year, we focus on the group that was most likely to have been looking for work in the past year. The effect of Q_i on labor market outcomes cannot be driven by residential referrals from coworkers if the sample and match quality model is conditioned (to the extent possible using census data) on a residential location match that arose before the employment location match. As mentioned previously, we have no a priori expectations concerning how the strength of the referral effect varies depending upon whether the employment referral occurred recently or sometime in the past. The specification used for this second stage of our analysis is given by:

(4) $$E_i = \theta_g + \delta_1^{'} X_i + \delta_2^{'} \overline{X}_i + \delta_3^{'} Q_i + u_i$$

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[30] We have also performed our analysis using a multinomial logit specification, with three discrete outcomes: out of the labor force, unemployed, and employed. The results are qualitatively very similar.

where θ_g are standard block group fixed effects, X_i is the vector of individual attributes that are the same set of attributes used in the workplace clustering specification, and $\overline{X}_i$ is the vector of block averages on the same attributes. The latter are included in order to control for overall or non-individual specific effects of neighborhood on employment.

It is useful to consider the reflection problem again in the context of the labor market outcome regressions in equation (4). As noted above, Manski shows that it is generally impossible to distinguish the impact of group average outcomes from group average characteristics on the outcome of interest. Ignoring the presence of block-level match quality Q_i in equation (4) for a moment, this implies that it is generally impossible to distinguish the effect of average neighborhood labor market outcomes from average neighborhood sociodemographic characteristics and, for this reason, we do not include a measure of average neighborhood labor market outcomes in equation (4). As Manski points out, δ_2 continues to provide a test for the presence of social interactions more generally but does not distinguish between these mechanisms.

In the presence of this general concern, the match quality variable constructed from our first stage analysis is intriguing because its basis on the propensity of individuals to work together implies that this effect comes about through labor market referrals. In this way, we argue that this effect is informative about a particular channel through which the employment of neighbors might affect an individual's outcomes. The magnitude of the impact of neighbor employment levels on outcomes, however, remains a function of the match between individual and neighbor characteristics (e.g., the likelihood that the two interact) and, consequently, it is important to keep in mind that this effect does not operate directly through a group average labor market outcome.

In principle, this model is identified with block fixed effects because Q_i varies across individuals in a block. In our opinion, however, it would not be appropriate to include block fixed effects in this model. The current specification with block group fixed effects is identified because similar individuals reside in different blocks within the same block group and therefore have different match quality. In other words, the conceptual experiment considered is to change the match quality for a generic individual with observables X_i by moving them from one block to another block in the same block group, which we believe is the appropriate comparison or exercise. A specification that included block fixed effects would be identified by a comparison of individuals with different match quality in the same block. But individuals with the same X_i have exactly the same Q_i if they are in the same block and, consequently, the associated, and in our opinion undesirable, conceptual experiment would involve changes in an individual's observable attributes. Clearly, the results of this second exercise would be very sensitive to parametric assumptions concerning how X_i enters labor market outcomes and, consequently, such an exercise is unlikely to provide reliable insights into the effect of match quality on labor market outcomes.

Finally, it is important to point out a limitation of this exercise. In particular, what is actually identified by the first-stage analysis are types of pairs that are more likely to work together due to the strength of the referral effect between the pair. As discussed above, we expect this effect to be large in two cases: (i) when a pair is more likely to interact within their residential neighborhood and (ii) when one person is well attached to the labor market and the other likely to need a referral. In this way, for a person that is not well attached to the labor market, the measure of match quality described here should do a good job of characterizing the quality of matches in a neighborhood. For a person better attached to the labor market, however, our match quality variable may actually measure neighborhoods in which such a person provides rather than receives referrals. In this way, to the extent that our estimated social interaction effects in the first stage of our analysis are driven by the asymmetry in labor market attachment rather than by the strength of neighborhood interactions, our analysis of the effect of match quality on labor market outcomes is likely to understate the benefits of improved matches.

Measurement Error. An important issue that arises in the estimation of equation (4) results because the Census contains only a 1-in-7 sample of households rather than the full set of households on each block. This means that the constructed average block neighbor attributes (including our constructed match quality variable) included in equation (4) are measured with error. Assuming that the Census sampling design ensures that the measurement error is uncorrelated with the true underlying average block attributes, this measurement error would not pose much of a problem for our analysis if average match quality Q_i were the only variable measured with error included in the analysis. In this case, letting σ_{Q*} represent the true variation in match quality and σ_Q the measured variation, the probability limit of the estimated coefficient would be equal to the true coefficient times the ratio of σ_{Q*} to σ_Q:

(5) $$plim\left(\hat{\beta}\right) = \beta \frac{\sigma_{Q*}}{\sigma_Q} \quad \Rightarrow \quad plim\left(\hat{\beta}\right)\sigma_Q = \beta\sigma_{Q*}$$

In this way, one can obtain a consistent estimate of the effect of a one standard deviation increase in the true measure of match quality on labor market outcomes by multiplying the estimated coefficient by the standard deviation of our constructed measure of average match quality. When multiple variables are measured with error, this result does not necessarily follow immediately because of the possibility of correlation across regressors. To address this concern, we also consider robustness to the omission of all block average attributes other than match quality in these labor market regressions. A finding of similar results for these alternative specifications provides some confidence that the results are not driven by measurement error.

6 RESULTS

Having described the research design for each portion of our analysis above, we now present the results. We begin by examining the propensity for two individuals to work together, first reporting some summary statistics and then the estimated coefficients of the baseline regression specifications given in equations (1) and (2). We then present results for the alternative specifications based on sub-samples drawn from the most homogeneous block groups along various sociodemographic dimensions. Having presented these estimates of the work match regressions, we proceed to discuss the corresponding labor market outcome regressions for each of these specifications. A final sub-section explores both employment location match and labor market outcome specifications that address the possibility of reverse causation, examining sub-samples that condition on residential tenure and on whether individuals were fully employed in the previous year.

Table 1 contains summary statistics for our matched pairs sample. As described above, the first column reports the fraction of pairs that fit the description in the row heading. The second column reports – for each category – the empirical frequency that two individuals that reside *in the same block group but not the same block* work together. The third column reports the probability that two individuals that reside *on the same block* work together. In this way, the first row indicates that the baseline probability of working together for two individuals that reside in the same block group but not the same block is 0.36 percent; this figure rises to 0.94 percent for two individuals that reside on the same block. As we will see below, much of this increased propensity for individuals residing on the same block to work together results from the fact that the sample of individuals that reside on the same block is disproportionately weighted to larger blocks – i.e., dense block groups. The inclusion of block group fixed effects in our main empirical specification ensures that our social referral effects are estimated purely on the basis of comparisons within the same block group.

The remaining rows of Table 1 reveal how these patterns vary with the characteristics of the pair of individuals. First, notice that individuals residing on the same versus nearby blocks show an increased propensity to work together across all of the types of pairs characterized in the table. This increased propensity to work together for individuals on the same block versus block group is especially strong for pairs of individuals in which (i) both have children and especially similar aged young children; (ii) both are married; (iii) both are young; (iv) both are high school graduates; and (v) both are recent immigrants. The propensity for recent immigrants to work together is not surprising given the importance of social networks for recent immigrants. Because immigrants in most visa classes are required by US law to be sponsored by a specific employer, it is very likely that many recent immigrants receive referrals for both residential location and employment from a social contact already in the US. Thus, importantly, we view the inclusion of immigration status in the subsequent analysis as a control for this possibility rather than as an attempt to identify the causal impact of neighborhood referrals for immigrants. It is also important to note that *all* of the results of the analysis presented below are robust to dropping immigrants from the sample.

Table 1 also makes clear that the propensity that two individuals residing in the same block work together is not a simple monotonic function of the baseline propensity for individuals residing in the same block group but not the same block. While pairs of all age combinations residing in the same block group but not the same block are about equally likely to work together, pairs of young workers residing on the same block are especially likely to work together. Similarly, while pairs of workers with children in nearby blocks are about as equally likely to work together as pairs without children, the corresponding propensity of pairs with children to work together is more than twice that of those without at the block level.

Baseline Specifications. While Table 1 provides suggestive evidence as to the presence and nature of a social interaction operating at the very local (block) level, two features of our regression specifications help clarify this evidence. First, the regressions include block group fixed effects. This ensures that the estimation of our social interaction effects is based exclusively on comparisons of block- versus block-group-level propensities to work together within the *same* block group. Second, by simultaneously including controls for education, race, age, children, marital status, and gender in the regression, these regressions isolate the marginal contribution of each characteristic. Given the strong correlation between marital status and the presence of children, for example, it is difficult to ascertain which of these is important from the analysis of Table 1 alone.

Table 4 reports the results of three specifications for both equations (1) and (2). The first row of each column reports the parameter estimate of the average social interaction effect, α_0, for specification (1), which includes block group fixed effects but no covariates X_{ij}. Column 1 reports results for the full sample; column 2 reports results for the sample that drops blocks with fewer than five workers in the sample; column 3 includes a series of controls that characterize the housing stock. These latter specifications relate directly back to the correlation analysis shown in Section 4. Given the results of that analysis, which show that the correlation between observable individual and neighbor characteristics falls to near zero with the dropping of blocks with small numbers of sampled workers and the inclusion of block group fixed effects, column 2 reports our preferred specification. While the inclusion of housing characteristics in that analysis moved the estimated correlations even closer to zero, the fact that house value and rent may in part capitalize some components of neighbor characteristics lead us to believe that this specification provides a lower bound on the interaction effects. As we will see, all three specifications yield quite similar results.

Starting with the results for the specifications without covariates summarized in the first row, the estimated social interaction effect is positive and statistically significant in each case, indicating a strong additional propensity for two workers living in the same block to also work in the same block, over and above the estimated propensity for matches in their block group. The magnitude is 0.12 percentage points for the full sample and the sample based on blocks with at least five workers, falling to 0.11 percentage points when housing controls are added. This effect is sizeable: it is roughly 33 percent the size of the baseline propensity to work together for two individuals that reside in the same block group but not the same block (0.355 percent).[31]

An increased propensity to work with a given neighbor implies a much larger propensity to work with at least one neighbor. For our preferred sample, which restricts the sample to blocks with at least five sampled workers, given the average of 80 individuals per block,[32] an estimated referral effect of 0.12 percentage points translates to approximately a 6.9 percentage point increase in the propensity that an individual works with at least one individual on the same block.[33] Thus, the referral effect estimated here is certainly economically meaningful.

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[31] As noted above that this effect is less than the mean difference reported in Table 1 suggests that a portion of the differences in mean between those residing in the same block versus those in the same block group but not the same block was driven by variation across block groups related to population density. See Section 4 for a discussion of this issue.

[32] While the average number of workers meeting our sample criteria for the match model is only 5.1 workers, the fact that the Census is a 1-in-7 sample and that many workers are excluded from our analysis due to the presence of imputed data accounts for the larger average number of actual prime-age workers per block.

[33] For computational ease, this calculation treats the likelihood of working with each neighbor as an independent event. The reported $0.069 = (1 - 0.00355)^{80} - (1 - (0.00355+0.0012))^{80}$, where 80 is the average number of adults on the same block, 0.00355 is the baseline propensity for individuals to work with someone in the same block group and 0.0012 is our estimated social interaction effect.

The remainder of Table 4 reports results for the specification in equation (2) that includes the full set of covariates shown in Table 1 in both levels and interacted with whether the pair resides on the same block. The rows are assembled by groups of variables, such as educational attainment or race/ethnicity of workers in the pair, where the parameter estimates for the level coefficients are listed for the entire set of variables followed by the parameter estimates for the variables when interacted with whether the two workers live on the same block, *bmatch*.

Focusing first on the results for the full sample, the *bmatch* interaction estimates are statistically significant for most of the included socio-demographic categories in X_{ij}.[34] The interaction effects vary by pair characteristics in a number of interesting ways. With respect to education, stronger interactions occur for matches where both individuals are high school graduates while the weakest interactions occur for matches between high school dropouts. Matches between individuals with children, and especially those with elementary or secondary school-aged children of the same age also result in strong referral effects. Similar evidence of assortative matching among neighbors can be seen in the age interactions, where the size of the referral effect is also largest for matches between the youngest adults in the sample.

Across gender and marital status categories, referral effects are weakest for matches between married females relative to all other combination, while matches where at least one of the members is a married male result in especially strong referral effects. [35] The results for high school dropouts and married females suggest that referrals happen less frequently in matches where both individuals share characteristics that are associated with particularly weak attachment to the labor force. In general, then, our findings are broadly consistent with two common empirical findings in the existing literature on social networks and on informal hiring channels: (i) that there is strong assortative matching within social networks and (ii) that referrals can only occur when at least one member of the pair is well-attached to the labor market.[36]

Four additional aspects of these heterogeneous results are worth mentioning. First, the results for race and immigration status show strong estimated coefficients among pairs where both members are recent immigrants and among pairs where both members are either Asian or Hispanic. This is not surprising given the propensities for recent immigrants residing on the same

[34] The negative intercept for the specification with covariates means that the effect is negative (but barely statistically significant) for the left out category: this is for matches between Asians/Hispanics and Blacks, where one person is a high-school graduate and the other is a college graduate, and one person is 25 years old while the other is 35, etc. Such a category is a very tiny portion of all pairs in the sample. The estimated social interaction effect is estimated to be positive for over 99 percent of pairs observed in the data for each specification shown in Table 4.

[35] Note, however, that the decreased referral effect for pairs of married females will be balanced by the increased effect for pairs with (especially similarly-aged) children.

block to work together reported in Table 1. Again, as noted there, because it is very likely that many recent immigrants simultaneously receive referrals for both residences and homes at the time of immigration, we do not interpret the resulting coefficient as a causal neighborhood effect but include immigration status only as a control. Again, all of the results reported in the paper are robust to dropping immigrants from the sample.[37]

Second, the results also reveal that social interaction effects are declining with population of a block (i.e., decreasing in density). That our estimated referral effects are driven by blocks with a smaller number of housing units is encouraging because the housing market for such blocks will naturally be thinner – hence with less scope for sorting within block groups.[38] Notice that this is another case where our estimated social effect has the opposite sign when compared to the baseline propensity for two individuals residing in the same block group to work together. That is, while individuals that reside in dense block groups are generally much more likely to work in the same location, we estimate that referrals from neighbors are less likely in dense places.

A third important aspect of the results presented in Table 4 is that there are significant differences between the level and the interaction coefficients associated with the X_{ij} covariates. For example, conditional on the other attributes in the model, pairs of married females within the same block group are each the *most* likely to work in the same block (as discussed above, perhaps because they tend to work close to home) and have the *weakest* referral effects among all gender and marital status categories. A similar pattern obtains for high school dropouts. As discussed in Section 4 above, such substantial differences between the estimated α_1 and β coefficients provide additional assurance that the estimated referral effects are not simply capturing additional sorting at the block level.

Finally, a comparison of the results across the three specifications reported in Table 4 reveals a very similar pattern as blocks with fewer than five sampled workers are dropped and housing characteristics for each pair are included as controls. Again, because these housing controls, which include price measures, might absorb out too much of the variation in the underlying effect that is actually attributable to neighbor characteristics (due to capitalization) we expect that this specification may understate the strength of the interaction for characteristics that

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[36] See, for example, Corcoran et al. (1980).

[37] Note that matches between pairs where both are non-US born individuals having immigrated in the past 8 years represent only 0.22 percent of the overall sample. Thus, the magnitude of this effect is not responsible for the overall average referral effect of 0.12. In fact, the estimated average effect falls by less than 0.02 percentage points when all immigrants are dropped from the sample.

[38] Alternatively, one could think that social interactions are weaker in larger blocks because it is more difficult to establish and maintain a social contact in such a block.

are most likely to be capitalized – such as college-educated neighbors. While there is some slight evidence of this, the same pattern generally holds for this specification. Given these complications, however, we treat the specification shown in Column 2 as our preferred specification. The correlation of predicted match quality across these specifications exceeds 0.95 in each case, so this choice has little impact on the second stage of our analysis.

Robustness – Sorting within Block Groups. While the correlation analysis presented in Section 4 and the results of the specifications reported in Table 4 provide a great deal of re-assurance regarding the robustness of our analysis to concerns about the sorting of households across blocks within block groups, we seek to provide additional evidence that such sorting is not fundamentally driving the results. To this end, as described in Section 4, Table 5 reports the results of estimates based on sub-samples based on the 50 percent of block groups that exhibit the least amount of block-by-block sorting in four dimensions: education, race, the presence of children in the household, and immigration status. It is important to note, of course, that these restrictions on the sample change the nature of the set of households for which social interaction effects are identified so that there is no reason to expect the results to be identical to the full specification. In our minds, then, this exercise serves mainly as a broad check regarding block-level sorting.[39]

The first row of the table again summarizes the results for specifications that do not include any covariates – either in the levels or interacted with *bmatch*. In each case, the results remain similar to the initial regression reported in Table 4, ranging from 0.09 to 0.14 percent. When covariates are included in the analysis, the main findings related to age, the presence of children, gender and marital status from our baseline specification are confirmed and, in some cases, strengthened. Matches between high school graduates continue to lead to strong referral effects relative to other categories.[40] Again, the match quality indices for these specifications have correlations with the match quality index from specification 2 in table 4 as well as with each other in excess of 0.90.

In sum, our estimated social interaction effects persist, even in areas that do not experience a significant degree of sorting below the block group level with respect to characteristics most likely to be observed at the time a household moves into a block. We believe

[39] It should also be noted that these estimates are run using the sample that drops small blocks, but does not include the housing variables since they had only a minor impact on the estimate correlations in Table 3.

[40] Again, the effects for race and immigration status are a bit difficult to evaluate across samples as by construction, these samples differ significantly in the number of immigrants and racial minorities included

that this set of results further validates our attempt to isolate referral effects from sorting via the general research design proposed in this paper.

Labor Market Outcome Regressions. We now turn to results of a series of labor market outcome regressions based on each of the specifications of the work match equation reported in Tables 4 and 5. As described in Section 5, each regression includes a set of individual and average neighbor characteristics for each socio-demographic characteristic included in the work match specification as well as a set of block group fixed effects. The three broad columns of Table 6 report the effect of a one standard deviation increase in match quality on labor market outcomes for specifications corresponding to the three columns of Table 4. In this table, we only report the coefficient estimates associated with match quality for the sake of expositional clarity.[41][42] Note also that the number of observations varies across specification due to the number of observations with imputed dependent variables in each case; we drop such observations from the analysis.

For the specifications based on the full sample, match quality has a positive and (statistically and economically) significant impact on all dependent variables under consideration. Our preferred specification, which drops blocks with fewer than five sampled workers, is reported in the second broad column. For this specification, a one standard deviation increase in match quality raises labor force participation by about 1.6 percentage points, average days worked per year by about 4 days, earnings by 3.8 percentage points and wages by 2.1 percentage points. In this way, our estimated referral effects are indeed associated with improved labor market outcomes especially as it concerns participation in the labor market and the intensity of that participation.[43] Similar results obtain when housing controls are included in the analysis.[44][45]

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in the analysis. The number of immigrants is lowest, for example, in the fourth specification that selects the block groups that are most homogeneous with respect to this characteristic.

[41] The estimation results for the full sets of individual and block-level covariates are quite standard and are available from the authors upon request.

[42] The first two dependent variables refer to labor market outcomes for the week preceding the census survey. The last four variables represent labor market outcomes for the preceding year. Earnings and wage regressions are run for the sample of individuals that were *fully-employed* in the previous year, defined as having worked at least 40 weeks and at least 30 hours per week.

[43] Recall from our discussion above that this analysis will tend to understate the benefits of improved match quality at the block level as the quality of local matches will typically be overstated for individuals who generally provide referrals.

[44] Standard errors are corrected for clustering at the block level in all labor market outcome regressions reported in the paper.

[45] It is also worth noting that the estimated coefficients on match quality are qualitatively similar when no additional controls are included for average neighbor characteristics at the block level. This provides some confidence that the estimated impact of match quality is robust to the possibility of correlation between the measurement error in these variables and the measurement error in match quality.

The magnitudes of the labor force participation and employment effects estimated in Table 6 are generally consistent with the increased propensity to work with at least one neighbor in the same block estimated using the match specification above. In particular, the estimated true standard deviation of match quality for our preferred sample (5+ workers on the block) is about 0.18 percentage points.[46] This change in the propensity to work with each neighbor raises the probability that an individual works with at least one neighbor by approximately 9.2 percent at the mean. Given that one person in a match is providing the referral, this in turns implies an increase in the propensity to find a job through a neighborhood referral of 4.6 percent. This number corresponds to the increased propensity to work with someone on exactly the same block and, therefore, provides a lower bound on the number of neighborhood referrals more generally.

When compared to the employment effect estimated for the corresponding sample (1.8 percent), this then suggests an upper bound of about 40 percent of referrals (1.8/ 4.6) that result in employment for an individual who would not be employed in the absence of the referral, while the other 60 percent of neighborhood referrals go to individuals who would find employment through another search method. Again, because the denominator in this calculation is expected to be understated while the numerator is not, the actual fraction of referrals that result in a non-infra-marginal employment is likely much less.[47]

Table 7 reports the coefficient on match quality for labor market outcome regressions corresponding to the work match regressions based on the block groups that exhibit the least block-by-block sorting reported in Table 5. In general, the results are qualitatively similar to the ones obtained using the full sample, thereby confirming the robustness of our analysis to block-level sorting. One interesting aspect of this analysis, however, is that the labor force participation and employment results are smaller for each of these sub-samples, while the wage results are larger suggesting that referrals may be useful largely for finding a good job rather than for finding any job.

Reverse Causation. Table 8 provides estimates of specifications designed to address the possibility that the estimated social interaction effect may be due to reverse causation, i.e.,

[46] As discussed in the last sub-section in Section 5, match quality is measured with error due to the 1-in-7 nature of the Census sample. As a result, the measured standard deviation is significantly greater than the true standard deviation, which we estimated through Monte Carlo simulations.

[47] Recall that we expect the labor market outcome regressions to provide an estimate of the ultimate impact of all actual referrals from the neighborhood including individuals in both the same and nearby blocks. In particular, with limited sorting within block groups, expected match quality for individual with others in the same block group is the same as their actual block match quality. Consequently, the block level index for match quality is likely to capture the effect of referrals both within the block and from neighboring blocks.

workers receiving tips and referrals about residential locations from their co-workers. These specifications examine pairs of individuals that have been in their current residence for at least two years and focus on the estimated interaction effects for individuals who were not employed for the full year in the previous year. As noted above, the goal of this analysis is to examine whether evidence of referrals is present in this sub-sample. Again, because this sub-sample is very different from the main sample, we do not expect the estimated social interactions to be identical to our baseline results.

For reference, the first panel in Table 8 reports results for the sample of pairs that have been in their current residence for at least two years, again restricting attention to the sample of blocks with at least five workers. The estimated coefficients in this case are broadly consistent with those reported for the full sample in the second column of Table 4; the correlation in the predicted measure of match quality from these specifications is 0.71. The estimated coefficients are qualitatively similar although generally smaller in magnitude to those in the baseline regression for education, age, the presence of children, gender and marital status, and immigration.

The middle panel of Table 8 adds controls in both levels and interactions with *bmatch* based on whether the workers in the pair were not employed for the full year in 1989, defined as having worked 45 weeks or less. While failing to rise to the level of statistical significance, social interactions are stronger for matches in which one of the individuals was not employed for the full previous year while the other individual was (0.02 percentage points greater), whereas interaction effects are dramatically weakened when both members of the pair were not employed for the full previous year (0.12 percentage points smaller) relative to pairs in which both were employed for the full previous year. Since these are workers who have resided in the same location for at least two years, these findings do not lend support to the reverse causation hypothesis (co-workers giving referrals about desirable residential locations to new employees).

The last set of columns in Table 8 focuses on the sub-sample of pairs with both individuals in residence at least two years, but with only one member employed for the full previous year. Again, this sampling scheme reduces the possibility of reverse causation, since we are considering workers who are more likely to have made a transition to full employment during the past year *and* whose residential tenure is longer than two years. At the same time, by looking at pairs in which one was employed for the full year while the other was not, we are focusing on instances in which it is most likely that a referral or information exchange actually took place.

As in the other specifications, the estimated social interaction effect is strongly positive and statistically significant for the version without covariates. When we introduce covariates, the estimation results become statistically weaker than in the larger samples, due in part to the smaller sample size. Qualitatively, however, our previous results are confirmed, especially with respect to the gender and marital status, immigration, age and education. Overall, these findings strongly support the job referral hypothesis and make the reverse causation argument unlikely.

Finally, we take a more detailed look at the effect of match quality on labor market outcomes in Table 9. The objective here is to focus on individuals who were more likely to be searching for a job and thus more likely to receive, rather than provide, referrals. In panel 1, we report estimates using the sub-sample of individuals that have been in residence at least two years, adding a dummy variable for whether the individual was not employed for the full previous year. We report the coefficient estimates both for our measure of match quality and for the interaction term of match quality with the 'not-employed-for-full-previous-year' dummy. In this case, the measure of match quality is based on the parameter estimates for the specification reported in the second set of columns in Table 8. The results are quite striking: match quality per se does not have a significant impact on any outcome for the individuals who were employed for the full previous year (presumably because they were unlikely to have been unemployed last year and did not need a referral), whereas it has strongly positive and significant effects for the individuals who were not employed for the full year, and thus more likely to benefit from referrals.

The second panel in Table 9 reports results of an analogous specification where the sample is limited to those in residence at least two years and not employed for the full previous year and match quality is based on the estimated coefficients of the specification reported in the third set of columns in Table 8. Despite the sharp reduction in the sample size, the results for labor force participation and employment correspond well with those reported for individuals not employed for the full previous year in the specification reported in the first panel. A one standard deviation increase in match quality is associated with a statistically significant increase in labor force participation of 2.4 percent and employment of 1.9 percent for those individuals not employed for the full previous year. In this way, the labor market outcome effects appear to be important for precisely the group that one would think was mostly likely to have received the referrals.

7 CONCLUSION

This paper aims at detecting and measuring the importance of neighborhood referrals on labor market outcomes by using a novel data set and identification strategy. Using Census data that detail the exact block of residence and workplace for a large sample of prime-age workers in the Boston metro area, we identify social interactions by comparing the propensity of individuals on the same versus nearby blocks to work together. We find significant evidence of social interactions: residing on the same block increases the probability of working together by over 33 percent. This finding is robust to the introduction of detailed controls for socio-demographic characteristics as well as across various specifications intended to address biases caused by sorting below the block group level and housing market referrals exchanged between people who work together. Furthermore, the relationship between socio-demographic characteristics and the strength of social interactions make sense. Social interactions tend to be stronger when the match involves individuals who are likely to interact because they are similar in terms of education, age, and presence of children, which is consistent with the notion of assortative matching in social networks. Interactions also appear to be stronger when they involve at least one type of individual who is strongly attached to the labor market leading to weaker interactions when both members of the pair are high school drop-outs or married females.

In the second half of our analysis we use our heterogeneous referral effect estimates to construct an individual-specific measure of the availability of referral opportunities on her block of residence. Even after controlling for individual attributes, observable block attributes, and block group fixed effects, this measure is a statistically significant determinant of all of the labor market outcomes considered across all of our specifications. In terms of economic magnitude, a one standard deviation increase in referral opportunities raises expected labor force participation by 1.0-1.6 percentage points and earnings by 2.7-3.8 percentage points.

More generally, this paper provides a new approach for examining the effect of social interactions based on variation in geographic scale. In presenting the results related to neighborhood referrals and labor market outcomes, we also provide direct evidence on the reasonableness of this new design by testing whether its key assumptions hold on observable characteristics. In particular, we demonstrate that based on their observable characteristics, pairs of individuals residing on the same block would actually be slightly *less* likely to work together than pairs in the same block group but not the same block. This provides strong evidence that our research design is likely to be robust to within-block group sorting.

This evidence also suggests that the research design proposed in the paper may be useful in a variety of contexts. For example, in the case of welfare participation, the block of residence is unlikely to greatly influence access to public service providers after controlling for the block group. More generally, this design might be extended to the study of neighborhood effects in specific contexts (e.g., specific types of neighborhoods), on specific populations (e.g., youths), and for alternative outcomes (e.g., education, teenage fertility, health, welfare participation), provided the researcher can demonstrate that the within-block group correlation in observable neighbor characteristics is zero, thereby ensuring that the key identifying assumption on unobserved characteristics is at least reasonable. In future work, we intend to extend this analysis to young adults for whom neighborhood contacts might be an especially important source of job referrals.

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TABLE 1: Sample of Pairs of Individuals Residing in Same Block Group

Variable Name	Code	Percentage of Sample	Percentage That Work in Same Location	
			Reside in Same Block Group but Not Same Block	Reside on Same Block
Full sample		100.00	0.36	0.94
Both high school drop out	hsd_hsd	0.53	1.03	ND
Both high school graduate	hsg_hsg	15.50	0.47	1.33
Both college graduate	clg_clg	36.41	0.34	0.98
HS drop out - HS grad	hsd_hsg	4.75	0.51	0.82
HS drop out – College grad	hsd_clg	4.95	0.29	ND
HS grad – College grad	hsg_clg	37.87	0.30	0.82
Both age 25-34	a25_25	14.70	0.36	1.72
Both age 35-44	a35_35	11.02	0.33	0.77
Both age 45-59	a45_45	9.55	0.42	0.65
Age 25-34 and age 45-59	a25_45	20.01	0.34	0.72
Age 35-44 and age 45-59	a35_45	19.86	0.38	0.63
Age 25-34 and age 35-44	a25_35	23.27	0.33	0.91
Both single male	sm_sm	3.01	0.36	0.77
Both single female	sf_sf	4.62	0.40	0.54
Single male–single female	sm_sf	7.17	0.33	0.43
Both married male	mm_mm	14.69	0.35	1.61
Both married female	mf_mf	8.07	0.52	1.78
Married male–married female	mm_mf	21.58	0.29	1.35
Single male-married female	sm_mf	7.87	0.33	0.54
Single male-married male	sm_mm	10.12	0.38	0.85
Single female-married female	sf_mf	10.03	0.41	0.64
Single female–married male	sf_mm	12.84	0.31	0.48
Both have children	child_m	26.98	0.36	1.58
Both have children age 0-5	c05_05	3.37	0.33	3.10
Both have children age 6-12	c612_612	4.12	0.37	2.50
Both have children age 13-17	c1317_1317	2.85	0.42	1.57
Both have children age 18-24	c1824_1824	3.01	0.48	ND
No children	nokid_m	27.71	0.37	0.72
Both White	wht_wht	86.51	0.35	0.77
White – Black	bl_wht	3.59	0.30	1.34
White – Asian/Hispanic	ashi_wht	8.31	0.39	1.69
Both Minority	other	0.51	0.47	2.38
Both born U.S.	bbus	78.75	0.34	0.84
One born U.S.	onbus	19.33	0.38	0.91
Both not born U.S.	bnbus	1.92	0.75	3.64
One not born U.S., in U.S. <= 8 yrs	oimm8	2.44	0.50	1.37
One not born U.S., in U.S. > 8 yrs	oimm9	7.13	0.34	0.56
Both not born U.S., both in U.S. <= 8 yrs	bimm8	0.22	ND	10.83
Both not born U.S. one <= eight one > 8	oimm89	0.73	0.73	2.86
Both not born U.S. and greater than eight	bimm9	0.97	0.66	ND
Both 2 years in residence or less	stay2_2	3.18	0.35	1.70
One <= 2 years months , one > 2 years in residence	stay2_3	25.60	0.35	1.24
Both > 2 years in residence	stay3_3	71.22	0.36	0.73
Both fully employed	employf_f	74.21	0.37	0.84
One not fully employed one fully employed	employf_u	23.81	0.31	1.17
Both not fully employed	employu_u	1.98	0.50	2.07
Both own house	ownocc	54.93	0.34	0.44
Both rent	renter	14.52	0.46	2.38
One rent one own	ownrent	30.54	0.35	0.42

Notes: The full sample includes 2,037,600 pairs of currently-employed, prime-age (25-59) adults that reside in the same block group but not the same household within the Boston metropolitan area in 1990. For the type of pair denoted in the row heading, the table describes the fraction of such pairs in the full sample, and the propensity of such pairs to work together (in same block) for individuals in the same block group but not the same block and those on the same block, respectively. All figures are expressed as percentages. ND indicates that a value was not disclosed because the number of individuals that work in the same block is less than 75.

Table 2: Sample of Prime-Age Adults in Boston Metropolitan Area

		Full Sample			Sample Fully Employed in 1989	Sample Currently Employed in 1990
Variable Name	**Percentage of Sample**	**Percent Currently Employed (1990)**	**Avg. Weeks Worked in 1989**	**Avg. Hours per Week in 1989**	**Avg. Earnings in 1989 in $1000's**	**Avg. Commute Distance**
Full sample	100.0	75.1	40.5	34.9	34.3	7.0
High school drop out	10.2	55.4	31.5	27.7	22.0	5.3
High school graduate	42.0	71.9	39.4	33.1	26.9	6.5
College graduate	47.8	82.0	43.4	37.9	42.1	7.6
Age 25-34	38.2	74.8	40.8	35.9	28.7	6.9
Age 35-44	31.6	77.0	41.1	35.0	37.3	7.3
Age 45-59	30.2	73.4	39.6	33.3	38.3	6.8
Single male	17.5	73.0	41.7	38.0	31.2	6.4
Single female	20.0	75.1	40.6	34.0	26.5	5.9
Married male	30.6	86.7	46.7	42.9	46.8	8.6
Married female	31.9	65.0	33.9	25.9	24.0	6.2
Has no children	48.1	77.3	42.2	36.9	32.5	6.7
Has children	51.9	73.0	39.0	33.0	36.2	7.3
Has children age 0-5	19.7	68.3	36.9	31.8	37.6	7.9
Has children age 6-12	20.5	71.6	37.6	31.6	37.6	7.2
Has children age 13-17	15.6	75.8	40.0	33.5	37.1	6.9
Has children age 18-24	17.0	74.7	40.4	33.8	33.3	6.7
Not Born U.S.	14.8	66.5	36.6	32.7	29.6	5.8
Not Born U.S. Resided <= 8 Years	4.9	59.2	31.9	30.1	25.0	5.1
Not Born U.S. Resided > 8 Years	9.8	70.2	38.9	34.0	31.4	6.1
White	87.9	76.7	41.2	35.3	35.3	7.2
Black	5.1	63.7	37.2	32.4	25.8	5.4
Asian/Hispanic	7.0	63.1	34.9	31.6	26.7	5.5
In residence <= 2 years	16.7	73.7	39.9	36.2	31.4	6.8
In residence > 2 years	83.3	75.3	40.7	34.6	34.9	7.0
Employed < 45 weeks in 1989	31.0	40.2	16.2	19.6	ND	5.8
Employed >= 45 weeks in 1989	69.0	90.8	51.5	41.7	34.3	7.2

Notes: The full sample includes 163,594 prime-age (25-59), adults that reside in the Boston metropolitan area in 1990. For the type of individual denoted in the row heading, the table describes the fraction of such individuals in the full sample, the fraction currently employed in 1990, average weeks worked in 1989, average hours per week in 1989, average earnings for those fully-employed in 1989, and average commute distance for those currently employed, respectively. For the purposes of examining earnings throughout the paper, *fully-employed in 1989* refers to any individual who worked at least 45 weeks and at least 30 hours per week; there are 113,575 such individuals for whom earnings are not imputed.

TABLE 3: Correlation Between Individual and Average Characteristics of Neighbors Residing on Same Block

	Full Sample			Blocks with 5+ Workers in Sample		
	Unconditional	**Conditional on Block Group**	**Adding Controls for Housing Characteristics**	**Unconditional**	**Conditional on Block Group**	**Adding Controls for Housing Characteristics**
HS Graduate	0.130	0.022	0.019	0.164	0.023	0.018
Col Graduate	0.214	0.036	0.030	0.268	0.051	0.044
Black	0.566	0.073	0.072	0.570	0.087	0.085
Asian or Hispanic	0.193	0.069	0.069	0.215	0.053	0.052
Age 45-59	0.052	0.015	0.010	0.070	0.013	0.007
Age 35-44	0.025	0.010	0.008	0.033	0.013	0.010
Age 25-34	0.087	0.027	0.013	0.128	0.041	0.023
Single Female	0.073	0.019	0.011	0.112	0.027	0.017
Single Male	0.069	0.022	0.018	0.072	0.007	0.000
Married Female	0.039	0.015	0.012	0.062	0.012	0.006
Married Male	0.076	0.021	0.013	0.098	0.027	0.018
Children	0.103	0.031	0.023	0.152	0.046	0.037
Children 0-5	0.033	0.012	0.012	0.030	0.003	0.004
Children 6-12	0.053	0.021	0.018	0.081	0.038	0.033
Children 13-17	0.039	0.010	0.005	0.053	0.011	0.007
Children 18-24	0.035	0.008	0.005	0.054	0.011	0.008
Not Born U.S.	0.164	0.045	0.045	0.210	0.054	0.054
Not Born U.S. Resided <= 8 Years	0.118	0.031	0.028	0.158	0.035	0.029
Not Born U.S. Resided > 8 Years	0.104	0.039	0.039	0.120	0.041	0.041
Block Group Fixed Effects:	No	Yes	Yes	No	Yes	Yes
Controls for Housing Characteristics:	No	No	Yes	No	No	Yes

Note: Table reports unbiased estimates of correlation between a series of individual characteristics and the corresponding average characteristics of other individuals residing on the same block but not in the same household. The first three columns reports correlations for the full sample; the final three columns drop blocks with fewer than five workers. For each sample, the first column reports unconditional correlation, the second conditions on block group fixed effects, and the third column adds three controls for housing characteristics (fraction renter-occupied, average rent, and average house value) in addition to including fixed effects.

TABLE 4: Estimates of Employment Location Match Regressions

		Full Sample		Blocks with 5+ Workers		Blocks with 5+ Workers; Adding Housing Controls	
Specification Without Covariates - Only Block Group Fixed Effects							
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside on Same Block	bmatch	0.0012	8.55	0.0012	7.67	0.0011	7.09
Sample Size:		2,037,600		1,234,494		1,234,494	
Includes Block Group Fixed Effects:		Yes		Yes		Yes	
Specification With Covariates and Block Group Fixed Effects							
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside in same block	bmatch	-0.0036	-2.49	-0.0031	-1.93	-0.0151	-5.58
Both high school drop out	hsd_hsd	0.0054	7.61	0.0036	3.62	0.0036	3.58
Both high school graduate	hsg_hsg	0.0013	8.41	0.0012	5.25	0.0011	5.15
Both college graduate	clg_clg	0.0004	3.01	0.0003	1.70	0.0003	1.71
HS drop out - HS grad	hsd_hsg	0.0011	4.47	0.0010	2.73	0.0009	2.62
HS drop out – College grad	hsd_clg	-0.0006	-2.51	-0.0008	-2.50	-0.0008	-2.53
	bmatch* hsd_hsd	-0.0028	-1.60	-0.0008	-0.40	-0.0012	-0.60
	bmatch* hsg_hsg	0.0017	4.28	0.0018	3.97	0.0016	3.61
	bmatch* clg_clg	0.0001	0.21	0.0000	0.13	0.0002	0.55
	bmatch* hsd_hsg	-0.0005	-0.73	-0.0006	-0.76	-0.0009	-1.15
	bmatch* hsd_clg	-0.0012	-1.97	-0.0010	-1.45	-0.0012	-1.66
Both White	wht_wht	0.0010	1.56	0.0007	0.87	0.0007	0.88
Both Black	bl_bl	0.0027	2.51	0.0023	1.64	0.0022	1.59
White – Black	bl_wht	0.0006	0.95	0.0001	0.16	0.0001	0.13
White – Asian/Hispanic	ashi_wht	0.0007	1.15	0.0007	0.87	0.0007	0.88
	bmatch* wht_wht	-0.0039	-3.22	-0.0038	-2.78	-0.0033	-2.45
	bmatch* bl_bl	-0.0045	-2.52	-0.0038	-1.85	-0.0039	-1.93
	bmatch* bl_wht	-0.0042	-3.18	-0.0039	-2.62	-0.0037	-2.51
	bmatch* ashi_wht	-0.0005	-0.38	-0.0005	-0.36	-0.0002	-0.15
	child_m	-0.0001	-0.78	-0.0001	-0.61	-0.0001	-0.45
Both have children age 0-5	c05_05	0.0001	0.19	-0.0003	-0.58	-0.0002	-0.49
Both have children age 6-12	c612_612	0.0004	1.52	0.0010	2.65	0.0011	2.66
Both have children age 13-17	c1317_1317	0.0003	0.90	0.0003	0.64	0.0003	0.61
Both have children age 18-24	c1824_1824	0.0006	1.98	0.0004	0.91	0.0004	0.87
	bmatch* child_m	0.0007	1.70	0.0008	1.71	0.0009	2.02
	bmatch* c05_05	0.0037	4.55	0.0038	4.11	0.0038	4.08
	bmatch* c612_612	0.0036	4.80	0.0033	3.87	0.0033	3.85
	bmatch* c1317_1317	0.0031	3.39	0.0033	3.26	0.0033	3.27
	bmatch* c1824_1824	-0.0011	-1.26	-0.0015	-1.49	-0.0016	-1.55
Both age 25-34	a25_25	0.0002	1.20	0.0002	0.74	0.0001	0.40
Both age 35-44	a35_35	0.0001	0.58	0.0000	0.12	0.0001	0.30
Both age 45-59	a45_45	0.0006	3.11	0.0007	2.17	0.0007	2.20
Age 25-34 and age 45-59	a25_45	0.0000	-0.15	0.0000	0.19	0.0000	0.18
Age 35-44 and age 45-59	a35_45	0.0005	3.25	0.0004	1.75	0.0004	1.89
	bmatch* a25_25	0.0019	4.83	0.0019	4.32	0.0018	3.89
	bmatch* a35_35	0.0000	-0.08	0.0000	0.02	0.0002	0.32
	bmatch* a45_45	0.0002	0.41	0.0003	0.54	0.0006	0.94
	bmatch* a25_45	0.0004	0.92	0.0002	0.53	0.0003	0.63
	bmatch* a35_45	-0.0001	-0.23	0.0000	0.03	0.0002	0.45
Both single male	sm_sm	-0.0012	-3.47	-0.0008	-1.64	-0.0009	-1.98
Both single female	sf_sf	-0.0006	-2.03	0.0000	-0.02	-0.0002	-0.45
Single male–single female	sm_sf	-0.0014	-5.08	-0.0008	-2.09	-0.0009	-2.52
Both married male	mm_mm	-0.0014	-6.45	-0.0008	-2.55	-0.0008	-2.56
Married male–married female	mm_mf	-0.0022	-10.83	-0.0015	-5.15	-0.0015	-5.17
Single male-married female	sm_mf	-0.0017	-6.96	-0.0010	-2.85	-0.0011	-3.02
Single male-married male	sm_mm	-0.0012	-4.92	-0.0008	-2.28	-0.0008	-2.47
Single female-married female	sf_mf	-0.0009	-3.69	-0.0005	-1.49	-0.0006	-1.69
Single female-married male	sf_mm	-0.0017	-7.72	-0.0011	-3.42	-0.0012	-3.64

	bmatch* sm_sm	0.0087	10.78	0.0087	9.66	0.0084	9.20
	bmatch* sf_sf	0.0069	9.39	0.0068	8.29	0.0064	7.68
	bmatch* sm_sf	0.0062	9.48	0.0063	8.51	0.0059	7.90
	bmatch* mm_mm	0.0125	20.84	0.0125	18.55	0.0126	18.63
	bmatch* mm_mf	0.0063	11.22	0.0060	9.54	0.0060	9.57
	bmatch* sm_mf	0.0060	8.94	0.0056	7.55	0.0055	7.32
	bmatch* sm_mm	0.0086	13.65	0.0088	12.33	0.0086	12.08
	bmatch* sf_mf	0.0073	11.51	0.0073	10.26	0.0071	9.89
	bmatch* sf_mm	0.0070	11.53	0.0070	10.30	0.0068	9.94
Both not born U.S.	bnbus	0.0034	5.17	0.0035	4.20	0.0035	4.17
One not born U.S.	onbus	0.0004	2.51	0.0004	1.60	0.0004	1.58
Both not born U.S., both in U.S <= 8 yrs	bimm8	0.0061	4.42	0.0074	4.40	0.0073	4.32
Both not born U.S., both in U.S. > 8 yrs	bimm9	-0.0006	-0.75	-0.0015	-1.43	-0.0014	-1.33
One not born U.S. and in U.S. > 8 yrs	oimm9	-0.0007	-3.16	-0.0010	-3.12	-0.0010	-2.98
	bmatch* bnbus	0.0013	0.93	0.0010	0.65	0.0009	0.59
	bmatch* onbus	-0.0014	-3.33	-0.0015	-3.04	-0.0015	-3.08
	bmatch* bimm8	0.0312	13.16	0.0302	11.44	0.0306	11.57
	bmatch* bimm9	-0.0035	-2.01	-0.0022	-1.15	-0.0022	-1.11
	bmatch* oimm9	-0.0004	-0.65	-0.0001	-0.16	0.0000	-0.05
Combined time in residence (/100)	lngth	0.0000	-0.16	0.0000	1.46	0.0000	1.94
Minimum time in residence (/100)	lngth_min	0.0100	3.08	0.0000	0.79	0.0000	0.77
Moved w/in 5 year of each other	lngth_win_5	0.0002	0.99	0.0007	2.87	0.0007	2.81
	bmatch* lngth	0.0000	0.45	0.0000	-0.50	0.0000	-0.10
	bmatch* lngth_min	0.0000	-0.07	0.0000	0.73	0.0000	0.61
	bmatch* lngth_win_5	0.0008	1.69	0.0003	0.50	0.0002	0.44
Block size (population/100)	blocksize	0.0000	-0.81	0.0000	0.77	0.0000	0.63
	bmatch* blocksize	0.0000	-1.87	0.0000	-2.05	0.0000	-2.08
Both owner-occupied	ownocc					-0.0001	-0.32
Both renter-occupied	renter					0.0019	1.70
Average rent	avgrent					0.0000	1.76
Difference in rent	diffrent					0.0000	-1.64
Renter status missing	rentmiss					0.0021	1.64
Average housing value	avghval					0.0000	-1.03
Difference in housing value	diffhval					0.0000	0.01
	bmatch* ownocc					0.0016	2.29
	bmatch* renter					0.0135	6.98
	bmatch* avgrent					0.0000	-0.81
	bmatch* diffrent					0.0000	-1.22
	bmatch* rentmiss					0.0112	5.03
	bmatch* avghval					0.0000	-2.91
	bmatch* diffhval					0.0000	1.20
Sample Size		2,037,600		1,234,494		1,234,494	
Includes Block Group Fixed Effects		Yes		Yes		Yes	

Notes: This table reports result for six specifications of a regression in which an observation is a pair of currently-employed, prime-age (25-59) adults that reside in the same block group but not the same household within the Boston metropolitan area in 1990. In each specification, the dependent variable equals one if both individuals work in the same location (Census block) and zero otherwise. The first column reports results for the full sample, which includes 2,037,600 pairs. The second column reports results for a sample that drops blocks with fewer than five workers. The third column adds additional controls for housing attributes. Block group fixed effects are included in all specifications. In the upper panel of the table, results are reported for a specification that includes only block group fixed effects and an indicator for whether the individuals reside on the same block. The lower panel reports results for specifications that include a full set of controls both in levels and interacted with the indicator for whether the individuals reside on the same block. Standard errors in all cases are estimated by pair-wise bootstraps and t-statistics are reported.

TABLE 5: Employment Location Match Regressions for Homogeneous Sub-Samples

		Block Groups Most Homogeneous w.r.t. Education		Block Groups Most Homogeneous w.r.t. Race		Block Groups Most Homogeneous w.r.t. Presence of Children		Block Groups Most Homogeneous w.r.t. Immigration	
Specification Without Covariates - Only Block Group Fixed Effects									
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside on Same Block	bmatch	0.0010	4.97	0.0014	6.36	0.0009	4.99	0.0013	6.07
Sample Size		801,461		723,524		824,821		713,015	
Includes Block Group Fixed Effects:		Yes		Yes		Yes		Yes	
Specification With Covariates and Block Group Fixed Effects									
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside in same block	bmatch	-0.0084	-4.46	-0.0087	-4.05	-0.0077	-4.10	-0.0094	-3.78
Both high school drop out	hsd_hsd	0.0007	0.58	0.0029	1.96	0.0015	1.31	0.0038	1.98
Both high school graduate	hsg_hsg	0.0010	3.79	0.0012	3.86	0.0009	3.46	0.0013	4.35
Both college graduate	clg_clg	0.0006	2.86	0.0004	1.60	0.0003	1.26	0.0003	1.47
HS drop out - HS grad	hsd_hsg	0.0007	1.56	0.0009	1.74	0.0006	1.46	0.0018	3.24
HS drop out – College grad	hsd_clg	-0.0009	-2.25	-0.0013	-2.81	-0.0010	-2.45	-0.0011	-2.22
	bmatch* hsd_hsd	0.0036	1.50	-0.0024	-0.82	0.0020	0.91	-0.0035	-1.07
	bmatch* hsg_hsg	0.0022	4.02	0.0024	3.81	0.0028	5.33	0.0021	3.51
	bmatch* clg_clg	-0.0008	-2.00	-0.0007	-1.45	-0.0003	-0.61	-0.0005	-1.24
	bmatch* hsd_hsg	-0.0004	-0.49	-0.0010	-0.98	0.0006	0.74	-0.0011	-1.00
	bmatch* hsd_clg	-0.0011	-1.34	-0.0009	-0.90	-0.0011	-1.29	-0.0004	-0.43
Both White	wht_wht	-0.0007	-0.78	0.0004	0.35	-0.0014	-1.52	-0.0020	-1.34
Both Black	bl_bl	0.0020	1.25	0.0066	2.86	-0.0006	-0.41	-0.0031	-1.00
White – Black	bl_wht	-0.0013	-1.31	-0.0004	-0.35	-0.0019	-1.96	-0.0024	-1.56
White – Asian/Hispanic	ashi_wht	-0.0006	-0.68	0.0008	0.70	-0.0011	-1.23	-0.0016	-1.09
	bmatch* wht_wht	0.0009	0.55	-0.0030	-1.73	0.0002	0.11	-0.0020	-0.90
	bmatch* bl_bl	-0.0003	-0.13	-0.0077	-2.34	0.0012	0.51	-0.0032	-0.77
	bmatch* bl_wht	0.0011	0.65	-0.0035	-1.84	0.0011	0.63	-0.0037	-1.55
	bmatch* ashi_wht	0.0042	2.65	0.0008	0.43	0.0036	2.25	0.0018	0.82
Both have children	child_m	-0.0002	-0.72	0.0001	0.21	0.0000	0.02	-0.0001	-0.20
Both have children age 0-5	c05_05	-0.0004	-0.70	-0.0004	-0.59	-0.0005	-0.87	0.0000	0.00
Both have children age 6-12	c612_612	0.0017	3.47	0.0019	3.28	0.0014	3.08	0.0013	2.53
Both have children age 13-17	c1317_1317	0.0006	1.00	0.0001	0.11	0.0004	0.76	0.0003	0.53
Both have children age 18-24	c1824_1824	-0.0002	-0.40	0.0001	0.15	0.0004	0.73	-0.0003	-0.56
	bmatch* child_m	0.0025	4.40	0.0030	4.38	0.0020	3.74	0.0025	4.05
	bmatch* c05_05	0.0036	3.21	0.0065	4.96	0.0030	2.80	0.0037	3.06
	bmatch* c612_612	0.0044	4.20	0.0056	4.46	0.0043	4.34	0.0064	5.72
	bmatch* c1317_1317	0.0023	1.79	0.0025	1.59	0.0034	2.83	0.0028	2.06
	bmatch* c1824_1824	-0.0018	-1.41	-0.0033	-2.12	-0.0021	-1.76	-0.0016	-1.23
Both age 25-34	a25_25	0.0003	0.91	0.0000	-0.14	0.0003	1.11	0.0003	0.95
Both age 35-44	a35_35	0.0000	-0.15	-0.0001	-0.41	0.0001	0.32	0.0003	0.87
Both age 45-59	a45_45	0.0003	0.91	0.0007	1.66	0.0004	1.04	0.0011	2.76
Age 25-34 and age 45-59	a25_45	0.0002	0.77	0.0003	0.85	0.0003	1.07	0.0004	1.40
Age 35-44 and age 45-59	a35_45	0.0005	1.64	0.0004	1.20	0.0003	1.24	0.0006	1.97
	bmatch* a25_25	0.0035	6.49	0.0027	4.62	0.0037	6.82	0.0025	4.20
	bmatch* a35_35	0.0009	1.31	-0.0010	-1.40	0.0003	0.45	-0.0008	-1.13
	bmatch* a45_45	0.0020	2.62	0.0002	0.24	0.0006	0.79	0.0007	0.89
	bmatch* a25_45	0.0003	0.64	-0.0001	-0.11	0.0005	0.88	-0.0003	-0.57
	bmatch* a35_45	0.0004	0.69	-0.0013	-2.03	0.0002	0.41	-0.0004	-0.57
Both single male	sm_sm	-0.0015	-2.55	-0.0008	-1.31	-0.0009	-1.52	-0.0010	-1.62
Both single female	sf_sf	0.0004	0.77	0.0003	0.47	0.0001	0.10	-0.0005	-0.85
Single male–single female	sm_sf	-0.0009	-1.97	-0.0003	-0.53	-0.0009	-2.00	-0.0013	-2.54
Both married male	mm_mm	-0.0007	-1.86	-0.0008	-1.70	-0.0007	-1.84	-0.0011	-2.62
Married male–married female	mm_mf	-0.0013	-3.60	-0.0010	-2.35	-0.0014	-3.95	-0.0020	-5.04
Single male-married female	sm_mf	-0.0010	-2.35	-0.0004	-0.84	-0.0008	-1.84	-0.0013	-2.75
Single male-married male	sm_mm	-0.0011	-2.67	-0.0003	-0.64	-0.0010	-2.46	-0.0014	-2.96
Single female-married female	sf_mf	-0.0002	-0.55	-0.0002	-0.51	-0.0003	-0.63	-0.0006	-1.27
Single female-married male	sf_mm	-0.0009	-2.15	-0.0005	-1.11	-0.0009	-2.27	-0.0013	-3.05

	bmatch* sm_sm	0.0099	9.22	0.0144	12.34	0.0092	8.35	0.0133	11.48
	bmatch* sf_sf	0.0072	7.27	0.0130	11.92	0.0065	6.59	0.0121	11.18
	bmatch* sm_sf	0.0081	9.05	0.0122	12.34	0.0066	7.39	0.0121	12.55
	bmatch* mm_mm	0.0113	13.50	0.0213	22.01	0.0112	14.04	0.0185	20.91
	bmatch* mm_mf	0.0074	9.55	0.0113	12.61	0.0070	9.44	0.0103	12.45
	bmatch* sm_mf	0.0085	9.35	0.0112	10.85	0.0074	8.27	0.0102	10.33
	bmatch* sm_mm	0.0085	9.84	0.0158	16.01	0.0081	9.65	0.0159	17.03
	bmatch* sf_mf	0.0084	9.74	0.0135	13.69	0.0075	8.91	0.0128	13.58
	bmatch* sf_mm	0.0082	9.90	0.0128	13.46	0.0074	9.16	0.0118	13.21
Both not born U.S.	bnbus	0.0020	2.00	0.0026	2.36	0.0024	2.55	0.0023	1.49
One not born U.S.	onbus	0.0003	1.12	0.0004	1.26	0.0003	0.98	-0.0002	-0.58
Both not born U.S., both in U.S <= 8 yrs	bimm8	0.0098	5.05	0.0122	5.64	0.0069	3.64	0.0146	4.47
Both not born U.S., both in U.S. > 8 yrs	bimm9	0.0002	0.18	-0.0008	-0.58	-0.0011	-0.95	-0.0020	-0.99
One not born U.S. and in U.S. > 8 yrs	oimm9	-0.0010	-2.55	-0.0008	-1.88	-0.0009	-2.34	-0.0001	-0.27
	bmatch* bnbus	-0.0022	-1.23	0.0025	1.32	-0.0031	-1.75	-0.0105	-4.22
	bmatch* onbus	-0.0022	-3.85	-0.0022	-3.46	-0.0027	-4.60	-0.0009	-1.29
	bmatch* bimm8	0.0009	0.28	0.0353	10.85	-0.0001	-0.03	0.0413	9.43
	bmatch* bimm9	0.0006	0.25	-0.0046	-1.81	0.0014	0.61	0.0076	2.34
	bmatch* oimm9	0.0003	0.33	-0.0006	-0.72	0.0012	1.53	-0.0021	-2.21
Combined time in residence (/100)	lngth	0.0000	1.67	0.0000	0.76	0.0000	2.24	0.0000	0.81
Minimum time in residence (/100)	lngth_min	0.0000	0.25	0.0000	1.04	0.0000	-0.15	0.0000	0.96
Moved w/in 5 year of each other	lngth_win_5	0.0008	2.54	0.0010	2.89	0.0010	3.36	0.0004	1.14
	bmatch* lngth	0.0000	-0.73	0.0000	-0.45	0.0000	-0.48	0.0000	-0.84
	bmatch* lngth_min	0.0000	0.07	0.0000	-0.13	0.0000	0.37	0.0100	0.94
	bmatch* lngth_win_5	0.0003	0.41	0.0002	0.29	0.0004	0.69	0.0000	0.05
Block size (population/100)	blocksize	0.0000	0.66	0.0000	1.89	0.0000	0.41	0.0000	-0.08
	bmatch* blocksize	0.0000	-2.46	0.0000	-3.10	0.0000	-2.32	0.0000	-1.18
Sample Size		1,042,153		1,196,738		1,032,769		1,032,769	
Block Group Fixed Effects		Yes		Yes		Yes		Yes	

Notes : This table reports result for six specifications of a regression in which an observation is a pair of currently-employed, prime-age (25-59) adults that reside in the same block group but not the same household within the Boston metropolitan area in 1990. In each specification, the dependent variable equals one if both individuals work in the same location (Census block) and zero otherwise. Each specification is based on the sample of pairs in blocks with at least five workers. The columns report results for samples of the most homogeneous block groups in terms of education, race, the presence of children, immigration status, and time in the household, respectively. Block group fixed effects are included in all specifications. In the upper panel of the table, results are reported for a specification that includes only block group fixed effects and an indicator for whether the individuals reside on the same block. The lower panel reports results for specifications that include a full set of controls both in levels and interacted with the indicator for whether the individuals reside on the same block. Standard errors in all cases are estimated by pair-wise bootstraps and t-statistics are reported.

TABLE 6: The Effect of Match Quality on Labor Market Outcomes

Effect of a One Standard Deviation Increase in Block-Level Match Quality

	Full Sample			**Blocks with 5+ Workers**			**Blocks with 5+ Workers; Adding Housing Controls**		
	coef	t-stat	N	coef	t-stat	N	coef	t-stat	N
Labor Force Participation	0.010	3.41	163594	0.016	3.98	128916	0.015	3.93	128916
Employed	0.013	4.22	163594	0.018	4.34	128916	0.017	4.28	128916
Weeks Worked Last Year	0.614	3.98	150485	0.763	3.77	118679	0.737	3.75	118679
Hours Worked Per Week	1.045	7.44	150567	1.287	6.63	118729	1.248	6.62	118729
Log(Earnings)	0.027	4.08	113575	0.038	4.77	89643	0.038	4.82	89643
Log(Wage)	0.015	2.85	94985	0.021	2.89	74915	0.021	2.96	74915

Notes: This table reports result for three specifications of six labor market outcome regressions. The labor market outcomes are labor force participation status in 1990, current employment in 1990, weeks worked in 1989, average hours worked per week in 1989, the log of 1989 earnings, and the log of 1989 hourly wage. For the first four of these outcome measures, respectively, the sample consists of all prime-age (25-59) adults that reside in the Boston metropolitan area in 1990. For the last two outcomes, the sample consists of all such individuals that were fully employed in 1989. In these earnings and wage regressions, *fully-employed* refers to individuals that worked at least 45 weeks and at least 30 hours per week. The first column reports results for the full sample, which includes 163,594 individuals. The second column reports results for a sample that drops blocks with fewer than five workers. The third column adds additional controls for housing attributes. In all cases any observations for which the Census imputed the dependent variable were dropped.

Block group fixed effects are included in all specifications along with controls for the full set of characteristics reported in Table 2 associated with race, education, age, sex, marital status, immigration status, time in residence, and presence of children. In each case, controls are included for the individual as well as the average for neighbors residing on the same block. The coefficients reported characterize the effect of a one standard deviation increase in match quality on the corresponding labor market outcome. For the three specifications reported match quality was constructed using the estimated coefficients from the corresponding regression in Table 4. Standard errors are corrected for clustering at the block level and t-statistics are reported.

TABLE 7: The Effect of Match Quality on Labor Market Outcomes - Homogeneous Sub-Samples

Effect of a One Standard Deviation Increase in Block-Level Match Quality

	Block Groups Most Homogeneous w.r.t. Education			Block Groups Most Homogeneous w.r.t. Race			Block Groups Most Homogeneous w.r.t. Presence of Children			Block Groups Most Homogeneous w.r.t. Immigration		
	coef	t-stat	N	coef	t-stat	N	coef	t-stat	N	coef	t-stat	N
Labor Force Participation	0.009	2.47	68555	0.011	1.72	57604	0.005	1.39	72452	0.010	2.00	67626
Employed	0.013	3.06	68555	0.014	2.20	57604	0.007	1.63	72452	0.006	1.28	67626
Weeks Worked Last Year	0.933	4.30	62988	1.071	3.54	52846	0.744	3.41	66658	0.621	2.61	62134
Hours Worked Per Week	1.123	5.96	63032	1.618	5.48	52873	1.063	5.57	66753	1.100	4.69	62135
Log(Earnings)	0.056	6.13	48008	0.031	2.56	40563	0.076	8.44	50770	0.042	4.13	47239
Log(Wage)	0.034	4.36	39962	0.024	2.22	33950	0.054	7.09	42222	0.026	2.80	39998

Notes: This table reports result for four specifications of six labor market outcome regressions. The labor market outcomes are labor force participation status in 1990, current employment in 1990, weeks worked in 1989, average hours worked per week in 1989, the log of 1989 earnings, and the log of 1989 hourly wage. For the first four of these outcome measures, respectively, the sample consists of all prime-age (25-59) adults that reside in the Boston metropolitan area in 1990. For the last two outcomes, the sample consists of all such individuals that were fully employed in 1989. In these earnings and wage regressions, *fully-employed* refers to individuals that worked at least 45 weeks and at least 30 hours per week. In all cases any observations for which the Census imputed the dependent variable were dropped.

Each specification is based on the sample of workers in blocks with at least five workers. Block group fixed effects are included in all specifications along with controls for the full set of characteristics reported in Table 2 associated with race, education, age, sex, marital status, immigration status, time in residence, and presence of children. In each case, controls are included for the individual as well as the average for neighbors residing on the same block. The coefficients reported characterize the effect of a one standard deviation increase in match quality on the corresponding labor market outcome. For the four specifications reported, match quality was constructed using the estimated coefficients from the corresponding regressions in Table 5 applied to the full set of neighbors observed in an individual's block. Standard errors are corrected for clustering at the block level and t-statistics are reported.

TABLE 8: Employment Location Match Regressions - Tenure-Based Sub-Samples

		Both in Residence at Least Two Years		Both in Residence at Least Two Years		Both in Residence at Least Two Years; One Not Employed for Full Year 1989	
Specification Without Covariates - Only Block Group Fixed Effects		**coef**	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside on Same Block	bmatch	0.0010	5.48	0.0010	5.48	0.0009	2.45
Sample Size:		846,061		846,061		196,167	
Includes Block Group Fixed Effects:		Yes		Yes		Yes	
Specification With Covariates and Block Group Fixed Effects		**coef**	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside in same block	bmatch	-0.0045	-2.10	-0.0045	-2.09	-0.0074	-1.85
Both high school drop out	hsd_hsd	0.0035	3.07	0.0035	3.08	0.0033	1.47
Both high school graduate	hsg_hsg	0.0010	4.14	0.0010	4.13	0.0014	2.93
Both college graduate	clg_clg	0.0001	0.45	0.0001	0.46	0.0000	-0.05
HS drop out - HS grad	hsd_hsg	0.0011	2.81	0.0011	2.81	0.0018	2.28
HS drop out – College grad	hsd_clg	-0.0007	-1.83	-0.0007	-1.82	-0.0019	-2.47
bmatch*	hsd_hsd	0.0002	0.11	0.0002	0.10	-0.0154	-3.61
bmatch*	hsg_hsg	0.0018	3.45	0.0018	3.45	0.0006	0.54
bmatch*	clg_clg	0.0005	1.16	0.0005	1.16	-0.0011	-1.34
bmatch*	hsd_hsg	-0.0008	-0.93	-0.0008	-0.93	-0.0033	-1.99
bmatch*	hsd_clg	-0.0017	-2.00	-0.0017	-1.99	-0.0029	-1.69
Both White	wht_wht	0.0003	0.28	0.0003	0.28	0.0033	1.63
Both Black	bl_bl	0.0038	2.20	0.0038	2.20	0.0015	0.44
White – Black	bl_wht	0.0001	0.08	0.0001	0.09	0.0026	1.22
White – Asian/Hispanic	ashi_wht	0.0004	0.37	0.0004	0.37	0.0030	1.52
bmatch*	wht_wht	0.0025	1.33	0.0025	1.33	0.0062	1.80
bmatch*	bl_bl	0.0025	0.95	0.0025	0.95	0.0034	0.71
bmatch*	bl_wht	0.0000	0.00	0.0000	-0.01	0.0029	0.79
bmatch*	ashi_wht	0.0050	2.65	0.0050	2.65	0.0138	4.01
	child_m	-0.0002	-0.74	-0.0002	-0.65	0.0001	0.27
Both have children age 0-5	c05_05	-0.0004	-0.86	-0.0004	-0.85	-0.0002	-0.25
Both have children age 6-12	c612_612	0.0012	2.94	0.0013	2.97	0.0001	0.14
Both have children age 13-17	c1317_1317	0.0003	0.66	0.0003	0.64	0.0017	1.88
Both have children age 18-24	c1824_1824	0.0004	0.78	0.0003	0.72	0.0002	0.23
bmatch*	child_m	0.0004	0.87	0.0004	0.86	0.0027	2.65
bmatch*	c05_05	0.0016	1.49	0.0016	1.49	-0.0002	-0.10
bmatch*	c612_612	0.0010	1.04	0.0010	1.04	0.0046	2.66
bmatch*	c1317_1317	0.0039	3.67	0.0039	3.68	-0.0012	-0.61
bmatch*	c1824_1824	-0.0011	-1.06	-0.0011	-1.04	-0.0003	-0.13
Both age 25-34	a25_25	-0.0001	-0.26	-0.0001	-0.26	0.0003	0.42
Both age 35-44	a35_35	0.0003	1.20	0.0003	1.19	0.0009	1.50
Both age 45-59	a45_45	0.0008	2.60	0.0008	2.58	0.0008	1.19
Age 25-34 and age 45-59	a25_45	0.0002	0.80	0.0002	0.80	0.0001	0.27
Age 35-44 and age 45-59	a35_45	0.0005	2.00	0.0005	1.97	0.0009	1.86
bmatch*	a25_25	0.0013	2.10	0.0013	2.08	-0.0004	-0.29
bmatch*	a35_35	0.0004	0.67	0.0004	0.68	-0.0018	-1.40
bmatch*	a45_45	-0.0001	-0.21	-0.0001	-0.21	-0.0021	-1.51
bmatch*	a25_45	0.0001	0.19	0.0001	0.18	-0.0028	-2.61
bmatch*	a35_45	0.0005	0.95	0.0005	0.97	-0.0017	-1.62

Both single male	sm_sm	-0.0014	-2.37	-0.0015	-2.45	-0.0002	-0.21
Both single female	sf_sf	-0.0002	-0.33	-0.0002	-0.42	0.0001	0.07
Single male–single female	sm_sf	-0.0017	-3.79	-0.0017	-3.89	-0.0018	-2.15
Both married male	mm_mm	-0.0012	-3.57	-0.0013	-3.90	-0.0007	-0.90
Married male–married female	mm_mf	-0.0018	-5.86	-0.0019	-5.87	-0.0017	-3.21
Single male-married female	sm_mf	-0.0015	-3.64	-0.0015	-3.66	-0.0010	-1.46
Single male-married male	sm_mm	-0.0013	-3.51	-0.0014	-3.68	-0.0009	-1.15
Single female-married female	sf_mf	-0.0008	-2.16	-0.0008	-2.19	-0.0017	-2.60
Single female-married male	sf_mm	-0.0014	-4.02	-0.0015	-4.19	-0.0016	-2.38
bmatch*	sm_sm	0.0054	4.62	0.0053	4.59	0.0083	3.53
bmatch*	sf_sf	0.0008	0.85	0.0008	0.85	0.0008	0.43
bmatch*	sm_sf	0.0016	1.76	0.0016	1.74	0.0051	3.03
bmatch*	mm_mm	0.0046	6.15	0.0046	6.07	0.0076	4.73
bmatch*	mm_mf	0.0004	0.57	0.0004	0.54	0.0022	1.87
bmatch*	sm_mf	-0.0007	-0.76	-0.0007	-0.78	0.0013	0.86
bmatch*	sm_mm	0.0030	3.62	0.0030	3.58	0.0067	4.00
bmatch*	sf_mf	0.0019	2.37	0.0019	2.34	0.0034	2.41
bmatch*	sf_mm	0.0009	1.16	0.0009	1.14	0.0018	1.23
Both not born U.S.	bnbus	0.0050	4.16	0.0050	4.18	0.0041	1.94
One not born U.S.	onbus	0.0000	0.13	0.0000	0.14	-0.0001	-0.10
Both not born U.S., both in U.S < 8 yrs	bimm8	0.0050	1.69	0.0050	1.69	0.0165	3.55
Both not born U.S., both in U.S. > 8 yrs	bimm9	-0.0029	-2.10	-0.0030	-2.13	0.0006	0.24
One not born U.S. and in U.S. > 8 yrs	oimm9	-0.0009	-2.46	-0.0010	-2.49	-0.0007	-0.92
bmatch*	bnbus	0.0081	3.63	0.0081	3.62	0.0106	2.70
bmatch*	onbus	-0.0009	-1.39	-0.0008	-1.39	-0.0020	-1.67
bmatch*	bimm8	0.0483	10.26	0.0483	10.27	0.0324	4.44
bmatch*	bimm9	-0.0063	-2.47	-0.0063	-2.47	-0.0093	-1.97
bmatch*	oimm9	0.0011	1.29	0.0011	1.29	0.0017	1.02
Combined time in residence (/100)	lngth	0.0000	1.28	0.0000	1.30	0.0000	0.98
Minimum time in residence (/100)	lngth_min	0.0000	0.59	0.0000	0.62	0.0000	-0.33
Moved w/in 5 year of each other	lngth_win_5	0.0008	2.85	0.0008	2.82	0.0009	1.51
bmatch*	lngth	0.0000	0.02	0.0000	0.01	0.0000	-0.41
bmatch*	lngth_min	0.0100	0.76	0.0100	0.76	0.0200	0.91
bmatch*	lngth_win_5	0.0005	0.85	0.0005	0.84	-0.0002	-0.14
Block size (population/100)	blocksize	0.0000	-0.37	0.0000	-0.37	0.0000	1.08
bmatch*	blocksize	0.0000	-1.51	0.0000	-1.51	0.0000	-1.09
One not employed full year 1989	one_nfe			-0.0009	-4.75		
Both not employed full year 1989	both_nfe			0.0018	3.09		
bmatch*	one_nfe			0.0002	0.57		
bmatch*	both_nfe			-0.0012	-0.95		
Sample Size:		846,061		846,061		196,167	
Includes Block Group Fixed Effects:		Yes		Yes		Yes	

Notes: This table reports result for six specifications of a regression in which an observation is a pair of currently-employed, prime-age (25-59) adults that reside in the same block group but not the same household within the Boston metropolitan area in 1990. In each specification, the dependent variable equals one if both individuals work in the same location (Census block) and zero otherwise. Each specification is based on the sample of pairs in blocks with at least five workers. The first two columns report results for a sample that includes only those individuals that have lived in their current residence for at least two years. The second column adds controls that indicate whether one or both members of the pair were not employed for the full year in 1989, which is defined as employed for 45 weeks or less. The third column restricts the samples to pairs in which at least one member was not employed for the full year in 1989.

Block group fixed effects are included in all specifications. In the upper panel of the table, results are reported for a specification that includes only block group fixed effects and an indicator for whether the individuals reside on the same block. The lower panel reports results for specifications that include a full set of controls both in levels and interacted with the indicator for whether the individuals reside on the same block. Standard errors are estimated by pair-wise bootstraps and t-stats are reported.

TABLE 9: The Effect of Match Quality on Labor Market Outcomes - Tenure-Based Sub-Samples

Effect of a One Standard Deviation Increase in Block-Level Match Quality

	In Residence at Least Two Years					In Residence at Least Two Years; Not Employed for Full Year 1989		
	Match Quality		Match Quality * Not Employed for Full Year 1989			Match Quality		
	coef	t-stat	coef	t-stat	N	coef	t-stat	N
Labor Force Participation	-0.0025	-1.0100	0.0169	4.72	106183	0.0242	2.93	32,126
Employed	0.0039	1.5200	0.0067	2.18	106183	0.0189	2.31	32,126

Notes: This table reports results for two current labor force participation and current employment regressions. Specifications are based on a sample of those prime-age (25-59) adults that reside in the Boston metropolitan area in 1990 that have lived at their current residence for at least two years. The first specification reports results for block-level match quality and interactions of block-level match quality with an indicator for whether the individual was *not employed for the full year* in 1989, which refers to individuals that worked less than 45 weeks in 1989. The second specification reports results for only the sample of individuals that was not employed for the full year in 1989.

The coefficients reported characterize the effect of a one standard deviation increase in block-level match quality on the corresponding labor market outcome. For the two specifications reported, match quality was constructed using the estimated coefficients from the corresponding regressions shown in the second and third main columns of Table 8, respectively. Block group fixed effects are included in all specifications along with controls for the full set of characteristics reported in Table 2 associated with race, education, age, sex, marital status, immigration status, and presence of children. In each case, controls are included for the individual as well as the average for neighbors residing on the same block. Standard errors are corrected for clustering at the block level and t-statistics are reported.

Figure 1: Distribution of Blocks per Block Group

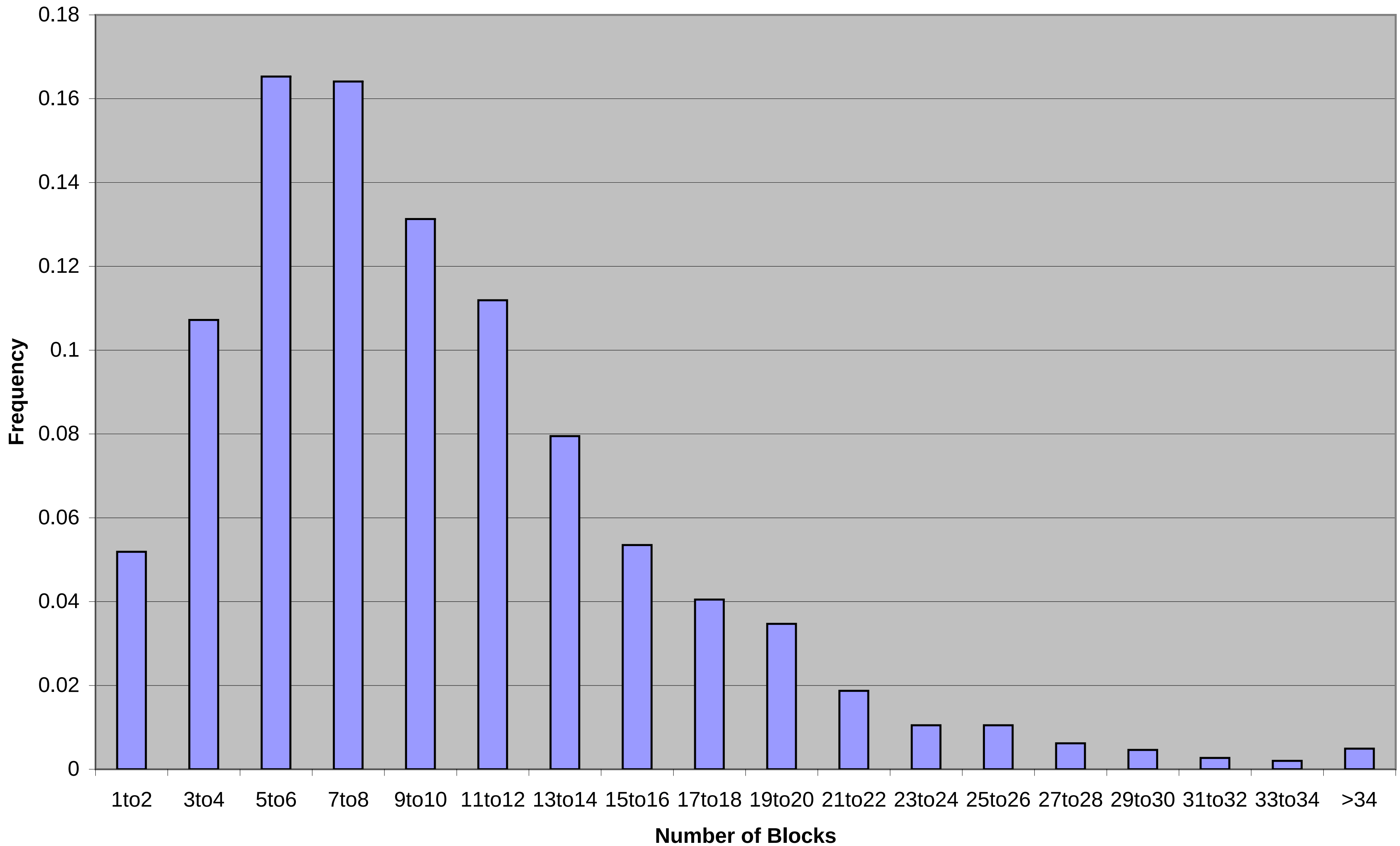

Figure 2: Distribution of Sampled Workers per Block