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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 those in nearby but not identical blocks. We find significant evidence of social interactions: residing on the same versus nearby blocks increases the probability of working together by over 50 percent. We also provide evidence as to which types of matches between individuals result in greater levels of referrals. These findings are robust across various specifications intended to address concerns related to sorting and reverse causation. Further, our estimated match effects have a significant impact on a wide range of labor market outcomes more generally including employment and wages.

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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 'neighborhood effects', broadly defined as any mechanism through which the local neighborhood affects an individual's outcomes.[2] In many of these studies, conditioning on neighborhood attributes gives rise to serious measurement issues as people are not likely to be randomly assigned to neighborhoods but instead typically select into them on the basis of individual and neighborhood characteristics.

To address such concerns, recent research has followed two broad approaches, each designed to identify neighborhood effects by isolating a (quasi-) random component of neighborhood choice. The first approach bases estimates of neighborhood effects on the random component of neighborhood choice induced by special social experiments. Most notably, Katz et. al. (2001) have used the randomized housing voucher allocation associated with the Moving To Opportunity demonstration (MTO) to examine neighborhood effects on a very wide set of individual behavioral outcomes including health, labor market activity, crime, education, and more.[3] The second broad approach seeks to deal with neighborhood-level sorting by aggregating to a higher level of geography. Cutler and Glaeser (1997) use such a design to analyze the impact of segregation on a variety of outcomes including education, labor market activity, and

[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) or Conley and Topa (forthcoming).

[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. Aaronson (1998) exploits data on siblings growing up in different neighborhoods to address any selection bias arising from families sorting into neighborhoods. Weinberg et al. (forthcoming) find significant neighborhood effects in hours worked using detailed panel data from the NLSY. Jencks and Mayer (1990) present a survey of some of the older empirical work on neighborhood effects.

[3] In Chicago in the late 1970's, the Gautreaux Program -- as part of a court-imposed public housing de-segregation effort -- gave housing vouchers to eligible black families in public housing to move to white or racially mixed neighborhoods. Popkin et al. (1993) find notable improvements in labor outcomes resulting from the relocation. With regard to the MTO experiment, Ludwig et al. (2001) study the Baltimore site and find a significant reduction in juvenile crime following the relocation.

teenage fertility, studying the impact of segregation measured at the metropolitan area rather than neighborhood level.[4]

The advantages of these approaches are clear and real, allowing much stronger causal conclusions to be drawn than the vast majority of the previous literature. However, disadvantages with each broad approach remain. In datasets like the one associated with the MTO, the sample of individuals is generally special having been selected into the initial sample on the basis of past outcomes (e.g., as a resident in public housing). Moreover, because the experimental design involves re-location, the initial effects that are identified describe the impact of neighborhood for individuals who have only just moved into a new residence. Aggregating to higher levels of geography also has its disadvantages; most notably, researchers who have used this approach have typically had to remain somewhat agnostic as to the actual mechanisms linking neighborhoods to individual outcomes.[5] Thus, while these two broad approaches have given researchers a new set of tools for drawing inferences about neighborhood effects, alternative strategies, especially those that allow causal inferences to be drawn about particular channels and for broader populations, have the potential to increase our understanding of the impact of neighborhoods on individual outcomes.

In this paper, we propose a new empirical design based on quasi-random variation in the characteristics of one's immediate neighbors to identify neighborhood effects in observational data. In particular, using Census data that detail the block on which each individual in the Boston metropolitan area resides, we first present evidence that the block-level variation in neighbor characteristics within each Census block group (a collection of approximately ten contiguous city blocks) is uncorrelated with an individual's own characteristics, due perhaps to the thinness of the housing market at

[4] Cutler and Glaeser and other similar studies, see for example Evans, Oates, and Schwab (1992), Ross (1998), Weinberg (2000, 2004), and Ross and Zenou (2004), identify the effect of location on outcomes using cross-metropolitan variation, which will be insulated against the biases caused by sorting as long as assignment to a metropolitan area is random. Another approach is to estimate the parameters of structural models of local interactions, in which sorting and social effects are separately identified: see, for instance, Brock and Durlauf (2001), Glaeser et al. (1996), Topa (2001). Here, however, the issue of possibly correlated unobservables is not fully resolved.

[5] The impact of segregation at the metropolitan level, for example, generally combines the effect of living in racially isolated neighborhoods with the associated effects of increased exposure to poverty, of attending predominantly minority schools, of reduced access to suburban employment opportunities, etc.

such a small scale. Given this finding, we propose a research design that identifies the effect of neighbors on outcomes using only this quasi-random block-level variation. This approach detects the presence of social interactions as long as such interactions are significantly stronger for individuals that reside on the same versus nearby blocks and generally represents a lower bound on the magnitude of neighborhood effects, quantifying only the differential effect that results from residing on exactly the same versus proximate blocks.

As an application of this design, we study the effects of neighborhood on labor market outcomes. Rather than focusing on only a broad neighborhood effect, we instead 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 a particular mechanism through which an individual's neighbors affect labor market outcomes, namely informal hiring networks.[6] Following the general research design described above, we examine the propensity of a pair of individuals to work in the same location, comparing the propensities for pairs of individuals that reside on the same versus nearby blocks within a block group. In addition to measuring a mean effect, we also consider heterogeneity in this referral effect by examining the types of pairs for which this effect is largest.

For this portion of our analysis, our results indicate the existence of significant social interactions at the block level, increasing the propensity that two individuals in the same block group work together by approximately 50 percent. This result is robust to the introduction of detailed controls for the sociodemographic 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. The results also indicate that this referral effect is stronger when individuals are similar in sociodemographic characteristics (e.g., both have children under five) and when one individual is well

[6] The use of informal methods in job search can be rationalized as a means to reduce the two-sided uncertainty regarding the quality of a prospective employer-employee match. Montgomery (1991) models the employer's side of the problem. Calvo-Armengol and Jackson (2004) explicitly model the information exchange process within workers' networks. Rees and Schultz (1970), Corcoran et al. (1980), 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.

attached to the labor market and the other more likely to need a referral (e.g., when one individual was fully employed in the previous year and the other was not).

Having determined the characteristics of a pair of individuals that lead to an especially strong referral effect, we then examine the impact of including a measure of average block-level match quality for individual workers into standard regressions for labor force participation, employment, wages, and earnings; these regressions also include controls for individual and block sociodemographic characteristics as well as block group fixed effects and controls for housing characteristics. The results of this portion of our analysis reveal that referral effects have a (statistically and economically) significant positive impact on all labor market outcomes under consideration; a one standard deviation increase in match quality raises expected labor force participation by 1.4 percentage points and earnings by 3.3 percentage points in our preferred specification.

In addition to providing new evidence concerning the importance of informal hiring networks for labor market outcomes, this application 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 well beyond our specific application to the study of neighborhood effects in other contexts (e.g., other metro areas, specific types of neighborhoods), on specific populations (e.g., youths, immigrants), and for alternative outcomes (e.g., education, teenage fertility, health, and more), provided interactions are especially strong at the block level and the researcher can demonstrate that the block level variation in individual and neighbor characteristics is uncorrelated for the relevant sample.

The remainder of the paper is organized as follows. Section 2 describes the data set that we have assembled for the Boston metropolitan area. Sections 3 and 4 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 5 and conclude in Section 6.

2 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 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.[7] 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

[7] 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.

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 all pairs of individuals that (i) reside in the same block group within the Boston metropolitan area, (ii) do not belong to the same household, (iii) are each currently employed, (iv) are each U.S. born and (v) are each between 25 and 59 years of age.[8] Overall, the sample contains 4,032,109 pairs, constructed from a set of approximately 110,000 employed individuals. The average number of workers per block is 4.7 (47 workers per block group). Figure 2 reports the corresponding histogram: the median number of workers per block is about three, and 95 percent of all blocks contain 13 workers or less.[9]

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 three quarters of the pairs has children; about 15 percent of pairs match two single individuals.[10] Notice also that restricting the sample to U.S. born adults in the Boston metro area results in a sample in which less than ten percent of all pairs involve at least one high school dropout and most pairs – 94 percent – contain two individuals that are white. This limits, to some extent, our ability to speak to the heterogeneity of neighborhood effects by race and for high school dropouts.

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 U.S. born, prime age (25 to 59) individuals that are not currently employed; this sample has 151,572 observations. 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

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[8] *Currently employed* refers to the reference week in the calendar year 1990 used by the Census. In future work we intend to extend this analysis to youths and immigrants. We focus on U.S. born prime-age results in this paper so as to avoid certain considerations and empirical issues unique to each of these alternative groups.

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

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

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.[11] 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 in the labor market.

3 EMPIRICAL DESIGN – DETECTING REFERRAL EFFECTS

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 essential identification assumptions are two. First, that while individuals are able to choose their residential neighborhood (block group), the exact block that they choose within this block group is as good as randomly assigned with respect to the characteristics of their neighbors. 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 neighborhood. 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*.

[11] 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.

The second assumption is that interactions with neighbors are very local in nature – i.e., occur mostly among individuals on the same block. As alluded to above, 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.

Baseline Specifications. The implementation of our basic 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.

This initial framework 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}:

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

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 age, education, race, or with children of similar age.[12] 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 social interaction effect.

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 when a researcher tries to infer whether average behavior in a group affects individual behavior, it is generally impossible to distinguish the impact of group average outcomes from group average characteristics on individual outcomes. In our context, however, an observation is a pair of individuals and the dependent variable indicates a joint outcome for that pair. Consequently, the reflection problem, which arises because of the simultaneity in the determination of the individual outcomes in Manski's framework, is not a concern here since the dependent variable is a joint outcome for the pair. In our framework, the identification of social effects does not rely on the use of group averages, but rather exploits different geographic scales.

Manski (1993) further argues that the identification of social interactions effects is made very problematic – if not impossible – by the likely presence of unobserved attributes that are similar across individual members of a given group. The presence of such "correlated unobservables" typically arises because of positive sorting into groups and locations, or because of similar institutional settings. This is a much thornier issue than the reflection problem discussed above, since it is nearly impossible to rule out the possibility that any observed comovement in outcomes is not generated by social interactions, but rather arises because of correlation in unobservable attributes.

It is this issue, which permeates the entire literature on social interactions, that our proposed research design addresses by focusing on what we argue is quasi-random variation at the block level, for a given Census block group. In particular, our first identifying assumption above (that agents may sort into block groups but are not able to

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

choose a specific block within that block group) is crucial to address the potential presence of correlated unobservables. Therefore, while a variety of additional issues related to our design are important (we discuss these below), in what follows we focus first on providing evidence concerning this identification assumption.

Correlation Analysis. Table 3 reports estimates of the correlation between observable individual and neighbor characteristics at the block level. In particular, for each block in the sample, a single U.S. born 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.[13]

The first three columns of Table 3 reports the 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. 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 0.23, while that between whether an individual is black and the fraction of black neighbors is 0.60. 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

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

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 75 percent on average, with a maximum of 0.09 across all characteristics and 0.05 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 even closer to zero. In this case, they average 0.03 across all categories and 0.02 across all non-race categories.

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.[14]

This concern about the full sample is substantiated in 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 (race, in particular) even smaller than those reported for the full sample. The estimated correlation coefficients reported in this last column again average only 0.03 with a maximum of 0.06 across all categories including race.

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

The broad lack of correlation in observable characteristics demonstrated in the conditional specifications reported in Table 3 provides strong evidence that block-by-block sorting on the basis of observables within block groups is not substantial. This provides particularly compelling evidence 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.

Alternative Explanations for Clustering in Work Locations and Our Research Design. As described above, the inclusion of block group fixed effects in our baseline specification (1) ensures that α_0 is identified by only block-level variation in neighbor interactions and, therefore, avoids endogeneity problems associated with sorting across neighborhoods (block groups). Including the baseline probability of an employment match for individuals living in the same block group, ρ_g, as well as the pair's covariates in levels, $\beta' X_{ij}$ also naturally addresses a number of alternative explanations for clustering in work locations among neighbors. In this sub-section, we discuss a number of such explanations as well as how our research design ensures that our estimated interaction effect, $\alpha_0+\alpha_1' X_{ij}$, indeed describes informal job referrals among neighbors rather than these other possibilities.

One alternative explanation for the clustering of work locations among neighbors is the possibility that a variety of observed and unobserved factors may collectively influence the employment location choices of individuals residing in the same neighborhood. For example, features of the urban transportation network (both observed and unobserved) might induce clustering in the segments that connect work and residential locations. In other words, people who live physically near each other may have very similar access to transportation networks and/or employment clusters. The inclusion of block group fixed effects ensures that the identification of the social interaction effect is not affected by such considerations provided that they operate at the neighborhood rather than block level, which seems reasonable.

Further, worker characteristics (again, both observed -- such as race/ethnicity, education, occupation -- and unobserved -- religion, cultural traits, etc.) might be correlated *both* with their residential location preferences *and* with the likelihood to work in a given location, if firm locations tend to cluster along these same attributes. For instance, members of certain demographic groups may be more likely to live together on the one hand, and choose jobs near central transportation nodes or in specific industrial clusters on the other: as a result, these groups will be more likely to work in the same location. This potential problem is directly addressed by the inclusion of demographic controls in levels, $\beta' X_{ij}$. These controls absorb the general propensity of certain types of individuals who live in the same block group to work together, allowing the comparable parameters for individuals who reside on the same block, α_1, to identify the strength of the social interaction for these individuals.

Temporal issues might also complicate the analysis. Suppose current residents of a given block group moved in at similar times because the neighborhood was developed at that time. Since employment and residential changes often move together (temporally), it is possible that many residents of that neighborhood may have found jobs in similar locations, i.e. where employment growth was occurring at the time. This source of bias is addressed in the same way as the ones above: in this case, the inclusion of level controls for age and tenure in residence are especially noteworthy because one provides information on when the individual most likely entered the labor market and the other contains controls for when the individual moved to this particular neighborhood.

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 correlation analysis described above provides assurance that this assumption is reasonable – the correlation between individual and neighbor characteristics falls to near zero with the inclusion of block group fixed effects and the dropping of blocks with very few individuals – 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, and the presence of children in the household. 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.[15]

An additional way to determine whether sorting at the block level is indeed 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 effects measured at the neighborhood level, which are captured by the level coefficients, are driven by factors that would generally bias our analysis, then β describe the empirical correlations that arise from these biases. If the biases at the block group level are similar to those at the block level and only the geographic scale has changed, then one would expect to see a qualitatively similar result at the block level (namely, in α_1). This does not seem to be the case in our empirical analysis.[16]

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. Further, we tackle the reverse causation problem 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 40 weeks in 1989. In this case, we can be fairly

[15] 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.
[16] 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 that 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.[17] 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.

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 heteroscedatisticy 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 standard errors using a pairwise bootstrap with the

[17] 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 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.

HC_3 correction and also with a wild bootstrap (Mammen (1993); Flachaire (1999), (forthcoming)).[18]

4 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 measured at the block level along with controls for individual and average neighbor characteristics (also 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. Most of the existing literature on informal search methods does not analyze their impact on aggregate labor market outcomes.[19]

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

[18] 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.

[19] Notable exceptions include Holzer (1988) and Datcher Loury (2004). The former uses NLSY data to study the choice of search method in a sample of unemployed young males, and finds that informal referrals are the most productive method in terms of job offer and acceptance probabilities. The latter studies the impact of informal referrals on earnings.

probability model.[20] The likelihood of falling into one of these discrete categories is specified as a linear function of household, individual, and neighborhood variables. 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:

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

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 3 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. We expect the effect of Q_i on labor market outcomes to be more strongly positive if the individual was working less than full time in the previous year, as we would be more likely to detect an actual instance of using one's referral network during an active job search.

The specification used for this second stage of our analysis is given by:

[20] 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.

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

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.

5 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 then present 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 0.44 percent; this figure rises to 0.93 percent for two individuals that reside on the same block.

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) one individual in the pair is black; and (v) both are high school graduates.

Table 1 also makes clear that the propensity that two individuals that reside in the same block group work together varies across types of pairs. While college graduates in the same block group but not the same block only work together 0.34 percent of the time, pairs with one high school dropout and one high school graduate are twice as likely to work together. Notice, however, 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. For example, while pairs of workers age 45-59 residing in nearby but not the same blocks have a higher propensity to work together than any other age group (0.53 percent), the increased propensity of pairs involving older individuals that reside on the same block to work together is smaller than that for younger workers. Similarly, within education, while the combination of a high school dropout and a high school graduate has the highest propensity to work together should they reside on nearby blocks, the increase in this propensity moving to the block level is much greater for pairs of high school graduates. It is this contrast between the propensity of individuals to work together on nearby versus the same block that identifies our social interaction term and, consequently, it is reassuring that the propensities to work together at the block group level do not simply scale up to those 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; the comparisons of Table 1 do not ensure that the difference is driven by only within-block group variation. 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 3. 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 0.24 percentage points for the full sample, falling to 0.23 percentage points for the other specifications. Given the near zero correlation between observable individual and neighbor characteristics associated with each of the designs for each of the latter columns, this estimate of 0.23 percentage points is robust – we argue – to household sorting. This effect is sizeable: it is roughly half the size of the baseline propensity for two individuals that reside in the same block group but not the same block to work together (0.44 percent).[21]

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 a pair of individuals reside on the same block. As discussed in Section 3, including covariates in the levels accounts for variation in the propensity of certain types of pairs of individuals residing in the same neighborhood to work together as the result, for example, of the fact that certain types of individuals are more likely to work close to home or use certain transportation modes. 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

[21] That this effect is less than the mean difference reported in Table 1 suggests that a portion of the initial mean was driven by variation across block groups related to population density. See Section 3 for a discussion of this issue.

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}.[22] The interaction effects vary by group in interesting ways. With respect to education, stronger interactions occur for matches where both individuals are high school graduates or (less so) college graduates. This is consistent with two common empirical findings in the existing literature on social networks and on informal hiring channels: first, that there is strong assortative matching within social networks and, second, that informal referrals are more prevalent for relatively less educated workers.[23] The results on race and ethnicity are statistically insignificant due to the small number of native born minorities in the Boston metropolitan area, but the magnitude of the effect of a match between blacks is similar to the effect found for a match between high school graduates.

We also find significant referral effects for matches between households with children, and especially where both households have pre-school age or teenage children, and between workers of similar ages. Again, these results seem highly consistent with the existing empirical consensus on positive sorting in social networks.[24] Further, we find very strong interaction effects for all gender and marital status categories relative to matches between married females. Matches where at least one of the members is a married male are especially strong, which is consistent with the notion that married males have a particularly strong attachment to the labor force and therefore may be better sources of referrals. Finally, social interactions are slightly stronger for smaller blocks: this is encouraging since such areas typically have fewer housing units and represent thinner housing markets -- hence with less scope for sorting within block groups.[25]

[22] 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.
[23] See, for example, Corcoran et al. (1980).
[24] Also, older workers tend to experience larger referral effects: this is consistent with the empirical evidence reported in Granovetter (1995).
[25] 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.

Finally, there are significant differences between the level and the interaction coefficients associated with the X_{ij} covariates. For example, pairs of married females are the most likely to work in the same block (perhaps because they tend to work close to home), but also have the weakest referral effects among all gender and marital status categories, which is consistent with their relatively low labor force attachment. Similarly, high school dropouts are more likely to work together (again because they tend to work close to home), but do not exhibit stronger referral effects than other education categories. These differences between the estimated α_1 and β coefficients are reassuring in light of our discussion with regard to sorting in Section 3.

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.[26] 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 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 3 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 3, Table 5 reports the results of estimates based on sub-samples based on the 50

[26] 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.

percent of block groups that exhibit the least amount of block-by-block sorting in three dimensions: education, race, and the presence of children in the household. 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. 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.

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 are almost perfectly identical to the initial regression reported in Table 4. When covariates are included in the analysis, the main findings of our baseline specification are confirmed and, in some cases, strengthened. Most socio-demographic categories included in the analysis experience statistically significant referral effects. The results for age, presence of children, and gender/marital status are very similar qualitatively to our baseline. The education results confirm that the education categories involving matches between individuals with the same educational attainment (especially for High School graduates) are characterized by stronger social interactions. The presence of children, especially of pre-school or high school age, leads to stronger social interactions. For age, strong benefits from social interactions primarily arise between individuals who are both similar in age (thus likely to interact) and older (thus likely to provide referrals). Finally, the effects for race are strengthened sufficiently to rise to the level of statistical significance. Specifically, after controlling for sorting attributable to race or educational attainment, pairs containing two blacks or at least one white worker exhibit considerably larger positive effects of social interactions than the omitted category that includes matches between workers in the Hispanic or Asian group and any minority group (black, Hispanic, or Asian). 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 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 4, 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.[27][28]

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 1.4 percentage points, average days worked per year by about 4 days, earnings by 3.3 percentage points and wages by 2.8 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.[29]

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

[28] 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.

[29] 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.

Similar results obtain when housing controls are included in the analysis.[30] Also, 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.

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. The results are qualitatively and quantitatively similar to the ones obtained using the full sample, confirming the robustness of our analysis to block-level sorting.

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., 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, a subset of individuals that may be particularly likely to have benefited from a referral in the past year.

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 0.53. The estimated coefficients are qualitatively similar to those in the baseline regression with the exception of the gender/marital status and the race/ethnicity categories. In terms of racial/ethnic groupings, the interactions are particularly strong for matches in which at least one member is white where the omitted category is a match between individuals belonging to the Hispanic or Asian group and any minority group.

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

Again, this is consistent with a referral interpretation, since whites tend to be in the labor force more consistently than other groups and can therefore be expected to provide referrals on a more regular basis.

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 40 weeks or less. The key result in this specification is that social interactions are stronger for matches in which one of the individuals was not employed for the full previous year (0.06 percentage points greater) while the other individual was, whereas interaction effects are dramatically weakened when both members of the pair were not employed for the full previous year (0.30 percentage points smaller) relative to pairs in which both were employed for the full previous year. This is entirely consistent with our job referral hypothesis, as one would expect referral effects to be the most prominent for the former type of matches, and the least important for the latter. In addition, 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: if anything, the size of the estimated α_0 is about 50 percent larger than using the full sample (0.0037 vs. 0.0024). 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 fertility and age characteristics of the match. 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 columns 1-5 of each panel, 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. Results are reported for three specifications: the full sample, dropping those blocks with 5+ workers, and adding housing controls, respectively. 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, 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 final set of columns in Table 9 reports results of three analogous specifications 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. While the sample size is much smaller, thus precluding sharp statistical inference, the results correspond roughly to those reported for individuals not employed for the full previous year in the specification reported in the first five columns.

6 CONCLUSION

This paper aims at detecting the presence of informal referral effects in the labor market by using a novel data set and identification strategy. We find significant evidence of social interactions: residing on the same versus nearby blocks increases the probability of working together by over 50 percent. These findings are robust to the introduction of detailed controls for socio-demographic characteristics and block group fixed effects, 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, young, or married females.

Furthermore, our estimated referral effects have a positive impact on labor market outcomes. Even after controlling for individual attributes, observable block attributes, and unobservable block group attributes using fixed effects, an individual's match quality is a statistically significant determinant of most 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 one percentage point, weeks worked by about four fifths of one week, and earnings by about three percentage points.

This paper provides a new approach for examining the effect of social interactions based on variation in geographic scale, and this approach might 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, and in the case of intellectual spillovers it seems likely that a firm's access to local suppliers or the regional labor market (the other two major sources of agglomeration economies) would vary much less within individual block groups. In future work on social interactions on employment, we plan to extend this analysis to two groups of individuals for whom we expect informal hiring networks to be especially important: namely, young adults and recent immigrants.

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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	0.44	0.93
Both high school drop out	hsd_hsd	0.38	ND	ND
Both high school graduate	hsg_hsg	18.63	0.59	1.41
Both college graduate	clg_clg	32.36	0.34	0.74
HS drop out - HS grad	hsd_hsg	4.68	0.68	1.05
HS drop out – College grad	hsd_clg	4.25	0.48	0.77
HS grad – College grad	hsg_clg	39.70	0.41	0.89
Both White	wht_wht	94.03	0.44	0.86
Both Black	bl_bl	0.55	0.42	ND
White – Black	bl_wht	2.81	0.33	2.09
White – Asian/Hispanic	ashi_wht	2.38	0.40	1.33
Other Pairs	other	0.23	ND	ND
Both have children	child_m	28.80	0.53	1.58
Both have children age 0-5	c05_05	3.55	0.40	2.64
Both have children age 6-12	c612_612	4.53	0.63	2.28
Both have children age 13-17	c1317_1317	3.00	0.67	1.76
Both have children age 18-24	c1824_1824	3.28	0.62	0.87
No children	nokid_m	25.74	0.37	0.59
Both age 25-34	a25_25	14.22	0.39	1.25
Both age 35-44	a35_35	11.34	0.44	1.01
Both age 45-59	a45_45	9.36	0.53	0.87
Age 25-34 and age 45-59	a25_45	20.14	0.43	0.71
Age 35-44 and age 45-59	a35_45	19.95	0.48	0.86
Age 25-34 and age 35-44	a25_35	23.44	0.41	0.93
Both single male	sm_sm	3.22	0.35	0.54
Both single female	sf_sf	4.30	0.42	0.66
Single male–single female	sm_sf	7.21	0.39	0.49
Both married male	mm_mm	13.69	0.36	1.09
Married male–married female	mm_mf	21.99	0.42	1.37
Single male-married female	sm_mf	8.52	0.51	0.68
Single male-married male	sm_mm	10.37	0.37	0.61
Single female-married female	sf_mf	9.84	0.52	0.80
Single female-married male	sf_mm	11.97	0.35	0.55
Both married female	mf_mf	8.90	0.73	2.06

Notes: The full sample includes 4,032,109 pairs of currently-employed, prime-age (25-59), US-born 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, US-Born 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	76.9	41.2	35.2	36.80	6.9
High school drop out	7.6	53.0	30.6	26.6	24.50	5.3
High school graduate	43.0	73.1	39.8	33.3	29.00	6.4
College graduate	49.4	83.8	44.0	38.2	43.90	7.5
Age 25-34	37.9	76.8	41.6	36.4	30.40	7.0
Age 35-44	31.8	78.7	41.6	35.2	40.30	7.1
Age 45-59	30.3	75.0	40.1	33.6	41.40	6.7
Single male	17.7	74.7	42.3	38.5	32.00	6.4
Single female	20.5	76.9	41.2	34.5	27.80	5.9
Married male	30.2	88.2	47.4	43.4	48.00	8.8
Married female	31.6	67.1	34.6	26.0	28.20	6.0
Has no children	49.0	79.0	42.8	37.30	33.80	6.6
Has children	51.0	74.8	39.6	33.20	40.20	7.2
Has children age 0-5	19.3	70.4	37.5	32.0	42.32	7.8
Has children age 6-12	19.9	73.4	38.1	31.7	42.78	7.1
Has children age 13-17	15.1	77.7	40.6	33.7	41.47	6.9
Has children age 18-24	16.8	76.3	41.0	34.1	36.56	6.7
White	93.9	77.9	41.6	35.5	37.30	7.0
Black	4.0	62.2	36.4	31.8	27.90	5.2
Asian/Hispanic	2.2	58.9	33.2	29.7	29.20	5.8
In residence for < 2 years	15.0	77.1	41.60	37.40	33.30	6.90
In residence for >= 2 years	85.0	76.8	41.10	34.80	37.40	6.90
Employed <40 weeks in 1989	24.6	34.2	11.7	16.8	NA	6.1
Employed 40+ weeks in 1989	75.4	90.8	50.8	41.2	36.80	7.0

Notes: The full sample includes 151,572 prime-age (25-59), US-born 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 40 weeks and at least 30 hours per week; there are 93,053 such individuals in the sample.

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	0.14	0.03	0.02	0.18	0.02	0.02
Col Graduate	0.23	0.05	0.04	0.28	0.04	0.04
Black	0.60	0.09	0.09	0.59	0.09	0.06
Asian or Hispanic	0.26	0.09	0.07	0.26	0.09	0.03
Age 45-59	0.06	0.02	0.01	0.06	0.03	0.02
Age 35-44	0.03	0.01	0.01	0.04	0.03	0.03
Age 25-34	0.08	0.03	0.03	0.11	0.04	0.03
Single Female	0.09	0.02	0.01	0.12	0.02	0.00
Single Male	0.07	0.02	0.02	0.11	0.04	0.03
Married Female	0.07	0.02	0.01	0.10	0.01	0.00
Married Male	0.08	0.03	0.02	0.11	0.03	0.02
Children	0.12	0.04	0.04	0.16	0.06	0.05
Children 0-5	0.04	0.02	0.02	0.05	0.03	0.03
Children 6-12	0.07	0.03	0.02	0.08	0.03	0.04
Children 13-17	0.04	0.01	0.01	0.06	0.02	0.02
Children 18-24	0.05	0.02	0.02	0.06	0.03	0.03
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

Specification Without Covariates - Only Block Group Fixed Effects

		Full Sample		Blocks with 5+ Workers		Blocks with 5+ Workers; Adding Housing Controls	
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside on Same Block	bmatch	0.0024	22.10	0.0023	11.74	0.0023	11.71
Sample Size:		4,032,109		2,632,897		2,632,897	
Includes Block Group Fixed Effects:		Yes		Yes		Yes	

Specification With Covariates and Block Group Fixed Effects

		Full Sample		Blocks with 5+ Workers		Blocks with 5+ Workers; Adding Housing Controls	
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside in same block	bmatch	-0.0036	-1.94	-0.0034	-1.22	-0.0323	-6.07
Both high school drop out	hsd_hsd	0.0028	4.39	0.0032	4.25	0.0029	3.07
Both high school graduate	hsg_hsg	0.0013	12.35	0.0014	12.78	0.0012	8.76
Both college graduate	clg_clg	-0.0003	-3.51	-0.0003	-3.99	-0.0002	-2.09
HS drop out - HS grad	hsd_hsg	0.0022	11.83	0.0023	10.16	0.0021	7.53
HS drop out – College grad	hsd_clg	0.0006	2.98	0.0007	3.96	0.0006	2.58
bmatch*	hsd_hsd	0.0006	0.34	0.0005	0.24	0.0010	0.46
bmatch*	hsg_hsg	0.0016	5.32	0.0015	3.58	0.0017	3.79
bmatch*	clg_clg	0.0008	3.17	0.0007	2.30	0.0004	1.39
bmatch*	hsd_hsg	0.0003	0.58	0.0003	0.37	0.0006	0.79
bmatch*	hsd_clg	0.0000	-0.08	-0.0003	-0.44	-0.0002	-0.26
Both White	wht_wht	-0.0014	-1.56	-0.0014	-1.50	-0.0011	-1.14
Both Black	bl_bl	0.0014	1.25	0.0016	1.59	0.0017	1.22
White – Black	bl_wht	-0.0019	-2.01	-0.0018	-1.97	-0.0017	-1.69
White – Asian/Hispanic	ashi_wht	-0.0014	-1.53	-0.0013	-1.34	-0.0011	-1.09
bmatch*	wht_wht	0.0012	0.70	0.0007	0.27	0.0003	0.09
bmatch*	bl_bl	0.0021	0.99	0.0022	0.71	0.0018	0.48
bmatch*	bl_wht	0.0010	0.55	0.0006	0.22	0.0003	0.09
bmatch*	ashi_wht	0.0011	0.61	0.0008	0.28	0.0003	0.08
	child_m	0.0008	7.00	0.0010	9.25	0.0010	7.59
Both have children age 0-5	c05_05	-0.0004	-1.67	-0.0007	-3.29	-0.0006	-2.55
Both have children age 6-12	c612_612	0.0014	6.98	0.0012	5.53	0.0011	4.11
Both have children age 13-17	c1317_1317	0.0008	3.66	0.0006	2.17	0.0005	1.62
Both have children age 18-24	c1824_1824	0.0001	0.46	-0.0001	-0.55	-0.0002	-0.53
bmatch*	child_m	0.0008	2.45	0.0005	1.09	0.0004	0.90

	bmatch* c05_05	0.0024	3.82	0.0030	2.63	0.0028	2.34
	bmatch* c612_612	-0.0006	-1.14	-0.0006	-0.60	-0.0008	-0.74
	bmatch* c1317_1317	0.0043	6.21	0.0050	4.33	0.0049	3.99
	bmatch* c1824_1824	-0.0009	-1.26	-0.0006	-0.64	-0.0005	-0.58
Both age 25-34	a25_25	0.0001	0.88	0.0001	0.69	0.0000	-0.09
Both age 35-44	a35_35	-0.0003	-2.07	-0.0004	-2.96	-0.0003	-1.74
Both age 45-59	a45_45	0.0006	3.75	0.0007	4.89	0.0008	3.84
Age 25-34 and age 45-59	a25_45	0.0001	0.50	0.0001	1.41	0.0001	1.01
Age 35-44 and age 45-59	a35_45	0.0002	1.97	0.0002	2.13	0.0003	2.39
	bmatch* a25_25	0.0015	4.59	0.0016	3.59	0.0016	3.46
	bmatch* a35_35	0.0019	4.92	0.0019	3.84	0.0019	3.50
	bmatch* a45_45	0.0020	4.60	0.0018	3.42	0.0015	2.43
	bmatch* a25_45	0.0005	1.67	0.0004	1.09	0.0003	0.93
	bmatch* a35_45	0.0017	5.28	0.0017	4.18	0.0015	3.36
Both single male	sm_sm	-0.0027	-10.47	-0.0025	-10.62	-0.0028	-9.33
Both single female	sf_sf	-0.0018	-7.57	-0.0017	-6.99	-0.0020	-7.04
Single male–single female	sm_sf	-0.0023	-11.32	-0.0021	-10.20	-0.0024	-9.72
Both married male	mm_mm	-0.0036	-22.26	-0.0035	-22.51	-0.0035	-16.94
Married male–married female	mm_mf	-0.0030	-19.71	-0.0028	-17.81	-0.0028	-14.24
Single male-married female	sm_mf	-0.0016	-8.57	-0.0016	-7.96	-0.0017	-7.01
Single male-married male	sm_mm	-0.0029	-16.56	-0.0028	-16.55	-0.0030	-12.77
Single female-married female	sf_mf	-0.0014	-7.99	-0.0013	-7.09	-0.0015	-5.94
Single female-married male	sf_mm	-0.0030	-18.11	-0.0029	-17.21	-0.0031	-14.14
	bmatch* sm_sm	0.0037	5.85	0.0040	5.03	0.0042	5.12
	bmatch* sf_sf	0.0037	6.33	0.0042	5.01	0.0047	5.53
	bmatch* sm_sf	0.0026	5.15	0.0031	4.21	0.0034	4.67
	bmatch* mm_mm	0.0055	11.67	0.0059	7.27	0.0058	7.42
	bmatch* mm_mf	0.0039	9.03	0.0044	5.72	0.0043	5.89
	bmatch* sm_mf	0.0036	6.91	0.0040	5.25	0.0041	5.39
	bmatch* sm_mm	0.0042	8.50	0.0046	6.19	0.0047	6.43
	bmatch* sf_mf	0.0042	8.54	0.0046	5.79	0.0048	6.27
	bmatch* sf_mm	0.0035	7.36	0.0040	5.61	0.0041	6.01
Combined time in residence (/100)	lngth	0.0046	7.91	0.0037	4.97	0.0049	4.92
Minimum time in residence (/100)	lngth_min	0.0010	0.57	0.0026	1.70	0.0035	1.82
Moved w/in 5 year of each other	lngth_win 5	0.0248	1.92	0.0227	1.91	0.0192	1.21
	bmatch* lngth	0.0001	0.05	0.0007	0.27	0.0009	0.32
	bmatch* lngth_min	-0.0037	-0.80	-0.0048	-0.89	-0.0043	-0.77
	bmatch* lngth_win 5	0.0013	0.04	0.0029	0.07	0.0041	0.09
Block size (population/100)	blocksize	0.0004	1.73	0.0004	1.59	0.0002	0.76

	bmatch* blocksize	-0.0011	-4.39	-0.0013	-4.89	-0.0012	-4.00
Both owner-occupied	ownocc					-0.0012	-4.78
Both renter-occupied	renter					0.0038	3.90
Average rent	avgrent					0.0000	0.24
Difference in rent	diffrent					0.0000	-1.54
Renter status missing	rentmiss					0.0031	2.87
Average housing value	avghval					0.0000	-0.15
Difference in housing value	diffhval					0.0000	0.35
	bmatch* ownocc					-0.0031	-5.43
	bmatch* renter					0.0272	7.09
	bmatch* avgrent					0.0000	1.74
	bmatch* diffrent					0.0000	-1.36
	bmatch* rentmiss					0.0291	7.02
	bmatch* avghval					0.0000	5.33
	bmatch* diffhval					0.0000	1.46
Sample Size		4,032,109		2,632,897		2,632,897	
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), US-born 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 4,032,109 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 rported 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

Specification Without Covariates - Only Block Group Fixed Effects

		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	
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside on Same Block	bmatch	0.0024	9.32	0.0023	9.36	0.0024	8.90
Sample Size		1,042,153		1,196,738		1,032,769	
Includes Block Group Fixed Effects:		Yes		Yes		Yes	

Specification With Covariates and Block Group Fixed Effects

		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	
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside in same block	bmatch	-0.0131	-2.75	-0.0083	-1.35	-0.0075	-1.55
Both high school drop out	hsd_hsd	0.0063	3.32	0.0016	1.15	0.0031	2.14
Both high school graduate	hsg_hsg	0.0015	6.52	0.0013	6.32	0.0012	5.38
Both college graduate	clg_clg	-0.0003	-1.77	-0.0005	-3.17	-0.0003	-1.98
HS drop out - HS grad	hsd_hsg	0.0020	4.26	0.0021	5.13	0.0014	3.76
HS drop out – College grad	hsd_clg	0.0006	1.69	0.0002	0.46	0.0007	2.04
bmatch*	hsd_hsd	-0.0006	-0.13	0.0010	0.28	-0.0003	-0.07
bmatch*	hsg_hsg	0.0014	1.96	0.0017	2.85	0.0018	2.50
bmatch*	clg_clg	0.0008	1.75	0.0006	1.57	0.0004	0.82
bmatch*	hsd_hsg	0.0010	0.82	0.0005	0.40	0.0007	0.66
bmatch*	hsd_clg	0.0002	0.23	0.0004	0.42	-0.0013	-1.25
Both White	wht_wht	-0.0032	-1.63	-0.0028	-1.09	-0.0030	-1.73
Both Black	bl_bl	0.0001	0.04	0.0006	0.21	-0.0020	-0.96
White – Black	bl_wht	-0.0029	-1.47	-0.0028	-1.11	-0.0031	-1.75
White – Asian/Hispanic	ashi_wht	-0.0035	-1.78	-0.0024	-0.92	-0.0021	-1.21
bmatch*	wht_wht	0.0088	1.94	0.0057	0.94	0.0026	0.55
bmatch*	bl_bl	0.0130	2.34	0.0103	1.44	0.0032	0.61
bmatch*	bl_wht	0.0086	1.84	0.0037	0.60	0.0039	0.82
bmatch*	ashi_wht	0.0100	2.08	0.0073	1.16	0.0017	0.36
Both have children	child_m	0.0008	3.73	0.0012	6.61	0.0009	4.12
Both have children age 0-5	c05_05	-0.0008	-1.98	-0.0011	-3.00	-0.0009	-2.34
Both have children age 6-12	c612_612	0.0010	2.57	0.0017	3.94	0.0012	2.84
Both have children age 13-17	c1317_1317	-0.0005	-0.99	0.0007	1.58	0.0009	1.84
Both have children age 18-24	c1824_1824	-0.0002	-0.40	-0.0004	-1.01	0.0004	0.80
bmatch*	child_m	0.0022	2.81	0.0010	1.50	0.0010	1.33

	bmatch* c05_05	0.0016	0.88	0.0052	3.04	0.0034	1.62
	bmatch* c612_612	-0.0032	-1.86	-0.0015	-0.95	-0.0025	-1.43
	bmatch* c1317_1317	0.0052	2.80	0.0037	2.29	0.0045	2.31
	bmatch* c1824_1824	-0.0045	-3.61	-0.0005	-0.36	-0.0022	-1.51
Both age 25-34	a25_25	0.0001	0.58	0.0000	-0.03	-0.0001	-0.48
Both age 35-44	a35_35	-0.0005	-1.89	-0.0007	-2.95	-0.0005	-1.96
Both age 45-59	a45_45	0.0006	2.02	0.0008	2.86	0.0003	1.08
Age 25-34 and age 45-59	a25_45	0.0003	1.40	-0.0001	-0.78	-0.0003	-1.84
Age 35-44 and age 45-59	a35_45	-0.0002	-0.83	0.0000	-0.20	-0.0002	-0.88
	bmatch* a25_25	0.0018	2.75	0.0020	3.14	0.0032	4.89
	bmatch* a35_35	0.0024	3.00	0.0028	3.82	0.0032	3.98
	bmatch* a45_45	0.0021	2.48	0.0020	2.43	0.0014	1.59
	bmatch* a25_45	-0.0002	-0.25	0.0012	2.10	0.0012	2.01
	bmatch* a35_45	0.0025	3.62	0.0020	3.41	0.0025	3.72
Both single male	sm_sm	-0.0025	-5.60	-0.0021	-4.60	-0.0024	-5.35
Both single female	sf_sf	-0.0014	-3.29	-0.0014	-3.44	-0.0017	-3.67
Single male–single female	sm_sf	-0.0020	-5.13	-0.0019	-5.17	-0.0022	-6.25
Both married male	mm_mm	-0.0034	-11.19	-0.0035	-11.43	-0.0035	-10.62
Married male–married female	mm_mf	-0.0028	-9.69	-0.0030	-9.45	-0.0031	-9.99
Single male-married female	sm_mf	-0.0016	-4.50	-0.0012	-3.40	-0.0021	-5.63
Single male-married male	sm_mm	-0.0031	-9.40	-0.0028	-8.58	-0.0029	-8.48
Single female-married female	sf_mf	-0.0013	-3.45	-0.0012	-3.54	-0.0011	-2.93
Single female-married male	sf_mm	-0.0029	-9.21	-0.0027	-8.62	-0.0029	-8.57
	bmatch* sm_sm	0.0053	4.35	0.0047	3.94	0.0054	3.95
	bmatch* sf_sf	0.0055	4.13	0.0053	4.54	0.0061	4.51
	bmatch* sm_sf	0.0041	3.57	0.0043	4.05	0.0044	3.85
	bmatch* mm_mm	0.0075	5.82	0.0068	6.19	0.0085	6.62
	bmatch* mm_mf	0.0064	5.01	0.0051	4.49	0.0059	4.92
	bmatch* sm_mf	0.0062	5.07	0.0050	4.79	0.0064	5.24
	bmatch* sm_mm	0.0068	5.72	0.0059	5.57	0.0056	4.82
	bmatch* sf_mf	0.0069	5.34	0.0061	5.98	0.0052	4.35
	bmatch* sf_mm	0.0052	4.41	0.0055	5.45	0.0056	4.60
Combined time in residence (/100)	lngth	0.0041	2.91	0.0031	2.21	0.0038	2.71
Minimum time in residence (/100)	lngth_min	0.0016	0.54	0.0036	1.29	0.0010	0.36
Moved w/in 5 year of each other	lngth_win 5	0.0004	1.82	0.0004	1.79	0.0003	1.47
	bmatch* lngth	0.0017	0.44	-0.0060	-1.53	0.0021	0.51
	bmatch* lngth_min	-0.0121	-1.44	0.0069	0.82	-0.0060	-0.71
	bmatch* lngth_win 5	0.0001	0.20	-0.0015	-2.31	-0.0002	-0.24
Block size (population/100)	blocksize	0.0001	0.20	0.0006	1.28	0.0000	-0.11

bmatch* blocksize	-0.0012	-2.61	-0.0015	-3.60	-0.0007	-1.60
Sample Size	1,042,153		1,196,738		1,032,769	
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), US-born 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, and the presence of children 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	0.011	5.75	151,572	0.014	5.88	113,405	0.014	5.60	113,405
Employed	0.009	3.72	151,572	0.011	3.96	113,405	0.011	3.70	113,405
Weeks Worked Last Year	0.590	5.90	151,572	0.787	6.28	113,405	0.759	5.94	113,405
Hours Worked Per Week	0.911	10.51	151,572	1.220	11.34	113,405	1.202	10.96	113,405
Log(Earnings)	0.020	5.13	93,053	0.033	6.63	70,005	0.034	6.59	70,005
Log(Wage)	0.014	3.71	93,053	0.028	5.47	70,005	0.028	5.46	70,005

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), US-born 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 40 weeks and at least 30 hours per week. The first column reports results for the full sample, which includes 151,572 individuals. The second column reports results for a sample that drops blocks with fewer than three workers. The third column adds additional controls for housing attributes.

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, and the 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		
	coef	t-stat	N	coef	t-stat	N	coef	t-stat	N
Labor Force Participation	0.015	6.07	113,405	0.016	6.53	113,405	0.012	5.25	113,405
Employed	0.013	4.72	113,405	0.015	5.36	113,405	0.012	4.26	113,405
Weeks Worked Last Year	0.792	6.77	113,405	0.787	6.53	113,405	0.754	6.05	113,405
Hours Worked Per Week	1.007	10.10	113,405	0.997	9.71	113,405	1.136	10.61	113,405
Log(Earnings)	0.027	5.90	70,005	0.021	4.29	70,005	0.035	7.14	70,005
Log(Wage)	0.023	5.14	70,005	0.017	3.49	70,005	0.030	6.08	70,005

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), US-born 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 40 weeks and at least 30 hours per week.

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, and the 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 regressions in Table 5 while the full sample is used in each case. 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

Specification Without Covariates - Only Block Group Fixed Effects

		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	
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside on Same Block	bmatch	0.0024	10.91	0.0024	10.91	0.0037	7.75
Sample Size:		1,907,051		1,907,051		368,797	
Includes Block Group Fixed Effects:		Yes		Yes		Yes	

Specification With Covariates and Block Group Fixed Effects

		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	
		coef	**t-stat**	**coef**	**t-stat**	**coef**	**t-stat**
Reside in same block	bmatch	-0.0040	-1.34	-0.0041	-1.30	0.0016	0.26
Both high school drop out	hsd_hsd	0.0037	3.23	0.0036	3.22	0.0006	0.41
Both high school graduate	hsg_hsg	0.0012	8.27	0.0012	7.51	0.0019	6.68
Both college graduate	clg_clg	-0.0004	-3.63	-0.0004	-3.58	-0.0004	-1.84
HS drop out - HS grad	hsd_hsg	0.0023	7.83	0.0023	7.19	0.0029	5.54
HS drop out – College grad	hsd_clg	0.0007	2.50	0.0007	2.58	0.0005	1.14
bmatch*	hsd_hsd	-0.0001	-0.04	-0.0001	-0.05	0.0003	0.07
bmatch*	hsg_hsg	0.0009	1.90	0.0009	1.91	0.0005	0.48
bmatch*	clg_clg	0.0008	2.48	0.0008	2.32	0.0010	1.08
bmatch*	hsd_hsg	0.0004	0.43	0.0004	0.47	-0.0019	-1.25
bmatch*	hsd_clg	-0.0001	-0.13	-0.0001	-0.13	-0.0014	-0.83
Both White	wht_wht	0.0004	0.31	0.0004	0.37	0.0025	1.67
Both Black	bl_bl	0.0008	0.52	0.0009	0.55	0.0049	2.47
White – Black	bl_wht	-0.0005	-0.40	-0.0004	-0.38	0.0016	1.10
White – Asian/Hispanic	ashi_wht	0.0002	0.18	0.0003	0.23	0.0015	0.97
bmatch*	wht_wht	0.0059	2.03	0.0060	2.01	0.0031	0.56
bmatch*	bl_bl	0.0032	0.98	0.0032	0.98	-0.0035	-0.60
bmatch*	bl_wht	0.0028	0.91	0.0028	0.90	-0.0005	-0.08
bmatch*	ashi_wht	0.0052	1.67	0.0052	1.66	-0.0020	-0.33
	child_m	0.0009	5.81	0.0009	5.88	0.0016	5.55
Both have children age 0-5	c05_05	-0.0010	-3.42	-0.0010	-3.43	-0.0017	-3.39
Both have children age 6-12	c612_612	0.0016	5.27	0.0016	4.88	0.0019	3.76
Both have children age 13-17	c1317_1317	0.0004	1.23	0.0004	1.23	0.0007	1.17
Both have children age 18-24	c1824_1824	-0.0002	-0.80	-0.0002	-0.67	0.0006	0.91

	bmatch* child_m	-0.0003	-0.74	-0.0003	-0.77	-0.0013	-1.19
	bmatch* c05_05	0.0020	1.64	0.0020	1.64	0.0049	1.75
	bmatch* c612_612	-0.0013	-1.23	-0.0013	-1.37	-0.0015	-0.76
	bmatch* c1317_1317	0.0047	3.90	0.0046	3.91	0.0046	2.01
	bmatch* c1824_1824	0.0001	0.09	0.0001	0.09	-0.0004	-0.18
Both age 25-34	a25_25	0.0001	0.68	0.0001	0.70	-0.0008	-2.56
Both age 35-44	a35_35	-0.0005	-2.72	-0.0005	-2.67	-0.0004	-1.25
Both age 45-59	a45_45	0.0008	3.53	0.0008	3.82	0.0009	2.39
Age 25-34 and age 45-59	a25_45	0.0002	1.15	0.0002	1.48	-0.0004	-1.91
Age 35-44 and age 45-59	a35_45	0.0002	1.03	0.0002	1.10	0.0002	0.64
	bmatch* a25_25	0.0003	0.44	0.0003	0.46	0.0004	0.30
	bmatch* a35_35	0.0017	3.30	0.0017	3.23	0.0024	1.74
	bmatch* a45_45	0.0011	1.86	0.0011	1.68	0.0036	2.37
	bmatch* a25_45	-0.0001	-0.13	-0.0001	-0.12	0.0012	1.18
	bmatch* a35_45	0.0012	2.71	0.0012	2.56	0.0024	2.17
Both single male	sm_sm	-0.0032	-8.64	-0.0032	-9.25	-0.0022	-3.45
Both single female	sf_sf	-0.0022	-6.47	-0.0021	-6.34	-0.0028	-4.40
Single male–single female	sm_sf	-0.0028	-9.55	-0.0027	-8.90	-0.0021	-4.29
Both married male	mm_mm	-0.0041	-16.63	-0.0039	-15.82	-0.0043	-9.69
Married male–married female	mm_mf	-0.0032	-13.28	-0.0031	-12.42	-0.0034	-9.78
Single male-married female	sm_mf	-0.0018	-6.34	-0.0018	-6.21	-0.0016	-4.13
Single male-married male	sm_mm	-0.0034	-12.84	-0.0033	-13.46	-0.0027	-6.65
Single female-married female	sf_mf	-0.0017	-5.76	-0.0016	-5.45	-0.0019	-4.64
Single female-married male	sf_mm	-0.0035	-13.88	-0.0034	-13.80	-0.0038	-9.38
	bmatch* sm_sm	0.0014	1.39	0.0014	1.39	0.0002	0.11
	bmatch* sf_sf	0.0001	0.06	0.0001	0.07	0.0000	0.01
	bmatch* sm_sf	-0.0003	-0.32	-0.0002	-0.29	0.0000	0.00
	bmatch* mm_mm	0.0008	0.97	0.0008	1.04	0.0002	0.13
	bmatch* mm_mf	0.0005	0.65	0.0005	0.63	0.0017	1.13
	bmatch* sm_mf	0.0003	0.33	0.0003	0.34	0.0029	1.69
	bmatch* sm_mm	0.0010	1.27	0.0010	1.22	0.0019	1.14
	bmatch* sf_mf	0.0005	0.65	0.0005	0.63	0.0021	1.34
	bmatch* sf_mm	0.0000	0.02	0.0000	0.03	0.0011	0.66
Combined time in residence (/100)	lngth	0.0039	3.31	0.0038	3.03	0.0087	4.10
Minimum time in residence (/100)	lngth_min	0.0031	1.34	0.0030	1.23	-0.0009	-0.23
Moved w/in 5 year of each other	lngth_win 5	0.0002	0.96	0.0002	0.98	0.0003	1.01
	bmatch* lngth	0.0004	0.12	0.0004	0.12	-0.0064	-0.76
	bmatch* lngth_min	-0.0019	-0.31	-0.0020	-0.29	-0.0042	-0.25
	bmatch* lngth_win 5	0.0001	0.15	0.0001	0.17	-0.0009	-0.67

Block size (population/100)	blocksize	0.0006	1.70	0.0006	1.68	0.0020	3.30
	bmatch* blocksize	-0.0016	-4.96	-0.0016	-4.40	-0.0025	-3.60
One not employed full year 1989	one_nfe			0.0005	3.65		
Both not employed full year 1989	both_nfe			0.0024	3.83		
	bmatch* one_nfe			0.0006	1.47		
	bmatch* both_nfe			-0.0030	-1.73		
Sample Size:		1,907,051		1,907,051		368,797	
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), US-born 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 40 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
Full Sample								
Labor Force Participation	0.0018	0.92	0.0127	7.35	128,797	0.0130	1.52	31,778
Employed	-0.0005	-0.21	0.0084	3.83	128,797	0.0052	0.62	31,778
Blocks with 5+ Workers								
Labor Force Participation	-0.0075	-4.65	0.0269	7.97	95,441	0.0078	0.74	23,422
Employed	-0.0057	-2.46	0.0135	3.99	95,441	0.0108	1.09	23,422
Blocks with 5+ Workers; Additional Controls for Housing								
Labor Force Participation	-0.0055	-3.28	0.0180	5.36	95,441	0.0036	0.39	23,422
Employed	-0.0040	-1.66	0.0059	1.76	95,441	0.0084	0.98	23,422

Notes: This table reports results for a series of current labor force participation and current employment regressions. The upper-most panel reports specifications based on a sample of those prime-age (25-59), US-born adults that reside in the Boston metropolitan area in 1990 that have lived at their current residence for at least two years. The middle panel drops those individuals in this sample that reside on blocks with fewer than five workers. The lower-most panel includes additional controls for individual and average neighborhood housing characteristics to the specification reported in the middle panel. In each panel, two specifications are reported. The first 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 40 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 in the upper-most panel, match quality was constructed using the estimated coefficients from the corresponding regressions shown in the second and third main columns of Table 8. For the other specifications, match quality measures were based on specifications analogous to those reported in Table 8 but with the corresponding sample restrictions. 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, and the 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

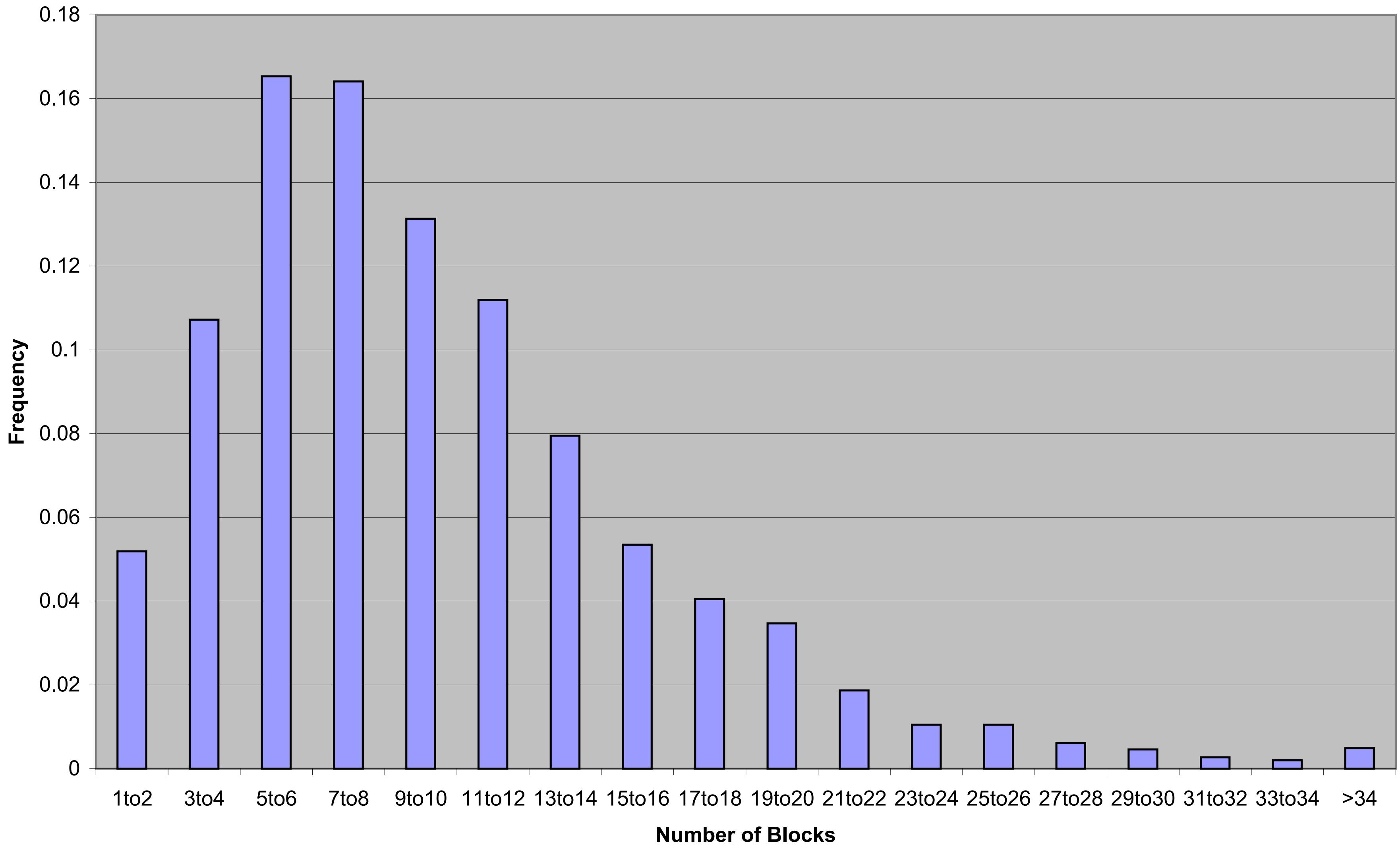

Figure 2: Distribution of Workers per Block