



University of
Connecticut

Department of Economics Working Paper Series

**Social Interactions within Cities: Neighborhood Environments
and Peer Relationships**

Stephen L. Ross
University of Connecticut

Working Paper 2009-31

September 2009

341 Mansfield Road, Unit 1063
Storrs, CT 06269-1063
Phone: (860) 486-3022
Fax: (860) 486-4463
<http://www.econ.uconn.edu/>

This working paper is indexed on RePEc, <http://repec.org/>

Abstract

Cities and their surrounding suburbs provide the homes, workplaces, and social and educational environments for most individuals and families in developed nations, but these urban areas are typically characterized by substantial stratification across racial, ethnic, and economic groups and associated with substantial levels of inequality. This chapter will examine our knowledge concerning the impact such stratification has on individual outcomes especially through its influence on the social interactions that occur within neighborhoods, schools, workplaces, and other institutions. The largest challenge faced in understanding the causal impact of social interactions arises from the fact that stratification is not an outside event, but rather is the result of individuals making choices that involve segregating themselves from others that differ in some way. The extent to which an individual makes segregating choices is invariably related to that individual's specific opportunities and therefore highly correlated with unobservables that drive that individual's success and life outcomes. Accordingly, the chapter will focus heavily on approaches for obtaining causal estimates of the effect of social interactions and evidence that arises from studies that have a convincing strategy for identifying these causal effects.

Journal of Economic Literature Classification: I2, J1, J6, R2

Keywords: Neighborhood Effects, Peer Effects, Social Interactions, Friendship Networks

Social Interactions within Cities: Neighborhood Environments and Peer Relationships

Stephen L Ross

University of Connecticut

Economists have long been interested in whether the outcomes of individuals are shaped by their interactions with those around them. This question is especially important in urban areas where production, education, and residential neighborhoods are all in close proximity to other activities and the composition of activities is determined individual choices. Further, the impact of racial and ethnic segregation or concentrated poverty on urban residents and the metropolitan environment is intimately related to how neighborhood environments and peers influence individuals and their communities as a whole.

Attempts to estimate the relationship between an individual's social interactions and that individual's outcomes face a variety of complex and interrelated challenges. Social networks are purposely created by choice of neighborhood and school and by direct decisions of individuals to interact either socially or professionally. Further, regardless of how group membership was established, individuals who belong to the same group are likely exposed to similar environmental factors that may lead to similar outcomes even if social interactions have no influence on outcomes. Finally, even when researchers successfully identify the causal effect of social interactions, it is often very difficult to untangle the mechanisms behind such effects. In most cases, researchers cannot separate the impact of one individual's behavior on another's behavior from the impact of the individual's attributes on the behavior, cannot identify which members of a group are influencing others within the group, or whether the effects are driven directly by social interactions or through behavior of third parties, such as teachers or police officers (Manski, 1993; Moffitt, 2001; Brock and Durlauf 2001).

This chapter begins by providing a detailed discussion of the various strategies that have been used to identify a causal relationship between peers, neighborhoods, or social contacts and an individual's behaviors or outcomes. Section 2 discusses the empirical literature on neighborhood effects starting with the important literature on racial segregation and outcomes of African-Americans, followed by discussions of the effect of neighborhoods on labor market outcomes and then more general effects on families and children. Section 3 discusses the literature on peer effects focusing primarily on peer effects in education including potential mechanisms and the role of friendship networks, but also discusses peer effects in other settings, especially the workplace. The paper concludes by discussing the implications for planning.

I. Isolating the Causal Impact of Social Interactions

At their most basic, studies of social interactions, neighborhood effects, or peer effects strive to estimate the impact of a specific environment on the outcomes of individuals, families, or groups exposed to that environment. Estimates from simple regression models will be biased because key aspects of environment, like neighborhood poverty rates or school demographic composition, will be correlated with individual or place unobservables that influence outcomes because individuals sort into environments based on their unobserved attributes and based on unobserved aspects of a location that both influence the environment factor and affect outcomes.

These biases arise in large part because social science and policy researchers are forced by necessity to use surveys of individual outcomes in the real world. The most natural and straightforward solution is to use an experiment to create random variation in a group environment and then compare differences in outcomes by assigned environment. The best known examples of such studies are the mobility programs for public housing residents, Gatreaux (Popkin et al. 1993) and Moving to Opportunity (Katz, Liebman, and Katz 2007, and

studies of college students based on random assignment of students to dorm rooms or floors, e.g. Sacerdote (2001) or Foster (2006). Such studies generate random variation in the treatment experienced allowing researchers to estimate the causal impact of such treatments.

However, there are several important limitations to such designs. First, such studies are limited to subpopulations where random assignment is practical or already occurs in practice. So the mobility studies focus on public housing residents, who are quite poor and often single parents, and roommate studies usually focus on elite private and public colleges and so have little to say about other educational environments. The response of such groups may not represent the population as a whole. Second, the experimental treatment in social experiments must be a feasible policy action, which means that the treatment is usually complex such as moving someone to a new neighborhood which requires a physical move disrupting existing relationships, and changing the neighborhood environment, schools, and possibly workplace. .

The alternative to experimental variation is to design a strategy for isolating exogenous variation in a sample of individuals whose outcomes are observed in a non-controlled environment. Probably, the best known strategy for avoiding bias from sorting into neighborhoods or schools is to look at a higher level of aggregation where there is less opportunity for sorting. For example, Cutler and Glaeser (1997), Card and Rothstein (2007), and Ananat (2007) examine the effect of across metropolitan area differences in racial segregation, and Ross (1998) and Weinberg (2000, 2004) examine the effect of access to employment or the spatial mismatch hypothesis. Most notably, Cutler and Glaeser (1997) and Ananat (2007) isolate the causal impact of segregation by instrumenting for segregation with historical measures of metropolitan structure, number of jurisdictions and railroad networks, In the literature on student peer effects,

While aggregation mitigates concerns about selection into neighborhoods, these approaches face a number of limitations. First and foremost, aggregation does not guarantee exogeneity. While mobility across metropolitan areas is low relative to mobility across neighborhoods, migration between metropolitan areas over time is an important demographic force in society and unobserved metropolitan attributes that affect outcomes also likely influence migration decisions. For example, Ananat (2007) finds lower housing prices in segregated metropolitan areas suggesting that they are less attractive to migrants. Second, any effects identified captures both the average influence of neighborhood or peers, but also the influence of the regional variable itself on outcomes. For example, the negative effect of segregation on African-Americans may arise because African-Americans are concentrated into more segregated and lower quality neighborhoods or because no matter where they live African-Americans do worse in segregated metropolitan areas due to metropolitan wide differences in educational or labor market opportunities.

Often researchers tend to search out situations that likely mimic random assignment due to constraints on individual's ability to freely sort or observe the necessary information to sort systematically. For example, Oreopolous (2003) and Jacob (2005) study the impact of relocations arising from administrative assignment to public housing projects in Toronto and from the demolition of the public housing projects in Chicago arguing that long waiting lists for public housing assure fairly random allocation of households across those projects. Alternatively, Bayer et al. (2008) examine the impact of an individual's immediate neighbors on their labor market outcomes or automobile consumption, respectively, after conditioning on a broader set of neighbor assuming that while individuals systematically sort into broad neighborhoods those individuals cannot choose the precise location of their residence. Similarly, many peer effect

studies have identified the effect of peers on student outcomes using cohort variation within schools, e.g Hoxby (2000) and Lavy and Schlosser (2007). While parents can select a specific school based on the school's quality, they cannot easily observe the attributes of a particular cohort of students and so differences in the demographic composition across years should be exogenous to student unobservables.

These strategies face similar limitations. For example, the forced public housing relocations suffer from exactly the same limitations of the Gatreux and Moving to Opportunity Programs. On the other hand, studies that focus on within neighborhood or within school variation suffer from exactly the opposite problem of aggregation studies. While aggregation studies mix neighborhood and broader metropolitan effects, research designs based on within group variation eliminate any neighborhood or peer effects that operate at the scale of the overall neighborhood or school. Bayer et al. (2008) condition away any spillovers that occur beyond an individual's immediate neighborhoods, and so the effect of more distant and weaker, but possibly broader and more useful (Granovetter 1995), social networks are missed in their studies. Similarly, cohort studies only capture peer effects that operate through short run mechanisms. For example, these studies will not capture the effect of peers that operate through teacher expectations that are shaped over many years of classroom experience or through school environment and culture.

All studies that do not exploit random assignment impose assumptions in order to identify causal effects. Increasingly, however, researchers are realizing that these assumptions are often testable, and the use of such diagnostics dramatically increases the quality of the empirical evidence. For example, in both cohort and immediate neighbor studies, the maintained assumption is that after selecting into a school or neighborhood the parent or individual has little

control over and no ability to sort in response to within school or neighborhood variation. Specifically, Lavy and Schlosser (2007) tests whether the within cohort variation of peer composition can explain a pre-determined student attributes, and Bayer et al. (2008) test whether an individual's attributes correlate with the attributes of their immediate neighbors after controlling for the average composition of the neighborhood. If parents or individuals could sort based on the information being used to identify neighborhood or peer effects, such sorting would likely create a correlation between individual and group attributes.

In addition, information on the timing of events and outcomes can often be exploited to validate an identification strategy. For example, Ananat (2007) identifies the effect of racial segregation of African-American on outcomes using railroads as a source of exogenous variation in racial segregation. In order to test whether her findings mistakenly capture the effect of segregation on the development of railroad networks, she tests for a relationship between the railroad network and the metropolitan attributes that pre-date the great migration of African-Americans. Similarly, Bayer et al. (2008) examine whether two individuals who are immediate neighbors are more likely to work at the same location presumably because one neighbor provided a labor market referral to the other. To assure that their results are not driven by workplace housing referrals, Bayer et al. (2008) re-estimate their model for a sample of individuals who were not fully employed last year, but had been in the same residential location.

These strategies for validating instruments are closely related to estimation techniques that bound bias from selection. Specifically, Altonji, Elder, and Tabor (2005) argue that selection bias from observables is likely more severe than bias from unobservables so that the bias on observables can be used to place bounds on the causal effect. The logic behind this approach is that researchers have put large amounts of effort into collecting the information that informed

individuals believe is very important, and so any bias that remains after controlling for these “key observables” is likely small relative to bias caused by omission of those observables. The diagnostics discussed earlier test whether there is any selection on observables arguing that if people are not sorting on observables then it is likely that they are not sorting on unobservables as well. In my opinion, the assumptions required for the Altonji, Elder, and Tabor approach are stronger than the assumptions required to rely on such diagnostics are weaker than assumptions for bounding because bounding approaches are often implemented when some evidence of sorting has been identified in the sample.

A key limitation of most causal evidence of peer or neighborhood effects is that such studies are implicitly or explicitly reduced form. A causal effect has been associated with being exposed to peers or neighbors of a certain type, such as that share who are in poverty or the share who are African-American. The existence of a causal effect of exposure to high poverty rates on outcomes does not imply that poverty itself is the mechanism behind this effect. In practice, a researcher must choose a limited number of attributes or variables to describe the randomly assigned groups, and these attributes are almost certainly correlated with many other observed and unobserved group attributes. It is impossible to know whether the group attribute, such a poverty rate, actually drives the effect or whether the mechanism arises from other factors that correlate with poverty. Even random assignment cannot solve this problem because in practice it is not possible to randomly assign the attributes of the peers who are being randomly assigned.

Considerable progress has been made in providing credible evidence that neighborhoods and school peers have a causal impact on outcomes. However, every identification strategy described above imposes considerable limitations on generalizability either by focusing on very idiosyncratic samples or by restricting the questions that can be answered. To move beyond these

limitations, researchers must rely on more theoretical structure usually imposing much stronger behavioral assumptions in order to replace the restrictions that were used to obtain identification through random or quasi-random variation. In the peer effects literature, such theoretically anchored work has coalesced around a common framework, which we will refer to as social networks. The study of social networks began in sociology in the 1920's and 30's with case studies that provided very detailed descriptions of social groups specifying and analyzing the links between group members. Social networks can be characterized by the number of links separating group members, amount of clustering among members, and whether there are dominant members in the network with a disproportionate number of links (Jackson 2008). Several recent papers, e.g. Bearman et al. 2004 and Calvó-Armengol et al. (In Press), examine the impact of social networks between school peers and student outcomes and behaviors.

Other than social network theory in peer effect models, the economics literature does not draw on a single theoretical structure for empirical investigation of social interactions. For example, Brock and Durlauf (2001) develop equilibrium model for considering social interactions involving discrete choices, such as incarceration, smoking, or the selection of one from a set of possible occupations. With discrete behaviors, they are able to separately identify the effect of peer's behaviors from the effect of peer's attributes (reflection problem). Weinberg (2006) also examines the reflection problem. His specification generates the standard empirical model where an individual's behavior depends linearly on the average behavior of peers, but the model implies a very non-linear equilibrium relationship group composition and group behavior, and he is able to test several of the hypotheses implied by the model. As a final example, Bayer and Ross (2008) develop a model of neighborhood effects where group membership is endogenous and this choice is affected by unobservables associated with both the individual and

the group. They propose the following solution: if membership in the group is priced in some way, such as housing prices in a neighborhood, price will reflect the overall quality of the group capturing both observed and unobserved group attributes so that the researcher need only address sorting based on individual unobservables.¹

II. Neighborhood Effects on Individual Outcomes

Racial Segregation and Outcomes of African-Americans

Many studies on the importance of location address the experiences of African-Americans. For example, Wilson (1987) argued that African-American outcomes are in part explained by their concentration in increasingly poor and distressed central city neighborhoods while Kain (1968) focused on the increasingly poor job access of African-Americans as jobs decentralized from central cities to the suburbs (spatial mismatch hypothesis). African-Americans face much higher levels of residential segregation and centralization than other minority groups in the U.S. (Massey and Denton 1993), and adverse changes in U.S. central cities over the last few decades may have disproportionately affected African-Americans.

Several studies exploiting cross-metropolitan variation find that African-Americans who reside in highly segregated metropolitan areas have worse outcomes than whites. This empirical relationship has been established for educational attainment, labor market outcomes, and single parenthood by Cutler and Glaeser (1997) and Ananat (2007), and standardized test scores by Card and Rothstein (2007). Cutler and Glaeser (1997) raise the concern of reverse causality where if the African-American population has worse relative outcomes whites will respond by avoiding integrated neighborhoods. Cutler and Glaeser (1997) and Ananat (2007) address this by using jurisdictional fragmentation and railroad tracks as instruments, respectively.

¹ Durlauf (2004) provides a more detailed survey of theoretical work on neighborhood effect models.

These studies find evidence that the mechanism behind the negative impact of segregation is related to the neighborhood environment rather than school segregation. Cutler and Glaeser (1997) find that exposure to the college educated can explain almost half of the impact of residential segregation. Card and Rothstein (2007) find no impact of school segregation after controlling for residential segregation and find that residential segregation operates primarily through neighbor's incomes. Similarly, Cutler, Glaeser, and Vigdor (2007) find that residential segregation is harmful for immigrant groups with low levels of human capital. On the other, Bifulco, Furtado, and Ross (2009) find that the educational environment in segregated metropolitan areas can explain much of the relationship between residential segregation and African-American outcomes mostly due to African-American youth exposure to substantially poorer student populations in segregated metropolitan areas.

However, some studies point towards a selective migration explanation for these findings. Vigdor (2002) finds evidence that the demographic attributes of the previous generation of migrants can explain part of the relationship between segregation and black outcomes. Similarly, Bifulco, Fletcher, and Ross (2009) find that residential segregation and racial differences in neighborhood exposure to education in the current metropolitan area explain educational attainment of 28-33 year olds whose education was likely determined prior to migration. Finally, Ananat (2008) finds that housing prices are lower in segregated metropolitan areas suggesting that such areas are less attractive to migrants on average.

In a related literature, Borjas (1995) examines the human determinants of ethnic groups in the United states. He finds a strong influence of the past generation's human capital on current levels of education. This effect appears to operate both indirectly through the human capital present in the neighborhood and directly through ethnic group human capital. Further, Cutler,

Glaeser, and Vigdor (2008) find that the benefits of immigrant segregation depend upon the human capital levels of the group. In Sweden, Gronqvist (2006) finds that residing in a municipality with a large ethnic enclave reduces the educational attainment of second generation immigrants.²

Finally, a huge literature exists on the spatial mismatch hypothesis as described by Kain (1968). Many studies exploit across metropolitan area variation.³ Weinberg (2000) finds that across area differences in job access explain differences in the labor market outcomes. Ross (1998) posits that mismatch involves constraints on mobility, and finds that blacks are less likely to engage in a work related residential move in metropolitan areas where blacks have relatively poor access to employment. Alternatively, Ross and Zenou (2008) exploit across metropolitan variation in expected exposure to neighborhood environments, and find no evidence of spatial mismatch. Hellerstein, Neumark, and McInerney (2008a) find that black unemployment is associated with the distribution of black employment rather than job access overall.

Neighborhoods and Labor Market Outcomes

As discussed earlier, the Gautreaux produced early evidence that relocation out of high poverty neighborhoods could improve labor market outcomes. The program moved black public housing residents to private market housing in Chicago and its suburbs. Placement counselors determined housing options, and 95 percent of candidates accept the first housing shown. In fact, there are no differences between program participants in urban and suburban neighborhoods on like educational attainment and family structure. Nonetheless, suburban movers were much more

² Card and Schmidt (2003) discuss a series of papers with similar findings for second generation immigrants in Germany, The Netherlands, Denmark, and Sweden in a special issue of the Journal of Population Economics.

³ Many other studies exploit within metropolitan area variation in job access, but these studies may be biased by unobserved differences across neighborhoods. For example, O'Regan and Quigley (1998) find that neighborhood quality has much larger labor market effects than employment access. See Ihlanfeldt and Sjoquist (1998) and Kain (1992) for detailed surveys.

likely than urban movers to be employed after the move even after controlling for employment status prior to the move (Popkin et al. 1993). Further, long-run follow-ups showed that the suburban movers were more likely to remain in low poverty, suburban neighborhoods (Keels et al., 2005), and had lower welfare usage and higher employment (Mendenhall, DeLuca, and Duncan, 2006).

However, much recent evidence has been unable to replicate these results. Oreopolous (2003) and Jacob (2005) study the impact of re-locations arising from administrative assignment to public housing projects in Toronto and Chicago, and neither of those studies find evidence of an employment effect arising from improved neighborhood environment. Finally, the Moving To Opportunity (MTO) randomly selected public housing residents in five metropolitan areas to provide vouchers with a requirement to move to a low poverty neighborhood. Focusing on the labor market, Kling, Leibman, and Katz, (2007) do not find any impact of the voucher on employment, labor market earnings, or welfare participation.

One major difference between the Gatreux experiment and the Moving to Opportunity Study is that the Gatreux experiment compared people who moved to the suburbs to movers who remained in the city while MTO compares people who receive a random mobility treatment with those who do not. The advantage of MTO is clear in that assignment to treatment is random while in Gatreux even with a 95% acceptance rate for first housing offer individuals may directly influence the location of the first offer made or the housing counselor may systematically sort candidates across locations (Votruba and Kling, 2004). On the other hand, MTO combines two treatments since voucher families both move and change neighborhood while Gatreux compares urban and suburban movers.

Further, several findings suggest that the MTO findings might be attributable to mobility. The MTO had large, long-run impacts on measures of adult mental health associated with distress, depression, anxiety, calmness, and sleep (Kling, Leibman, and Katz, 2007).⁴ Further, the MTO sample was comprised predominantly of single mother, AFDC recipients, and Meara and Frank (2006) study welfare recipients facing the same changes in federal programs, and they find that women with poor mental health are much less likely to transition off welfare and had much lower earnings than other welfare recipients. This raises a question “Did the mental health benefits of MTO have no positive impact on economic success or could there be some alternative factors that offsets the impact of mental health gains?”

Bayer and Ross (2008) provide some direct evidence on this question. They examine the labor market outcomes of individuals using an instrumental variables approach where the demographic composition of an individual's neighborhood is predicted based on the neighborhood exposure of individuals who are observationally equivalent. They find that exposure to poverty and low overall neighborhood quality both lead to worse labor market outcomes, but that exposure to a low fraction of college educated individuals improves labor market outcomes. Further, this effect appears associated with large neighborhood effects for women.

One potential explanation for this effect is that referrals and job networks are used less intensively by highly educated individuals (Ioannides and Loury, 2004). The disruption of social networks under MTO could be an important explanation for why the program showed no improvements in labor market outcomes.⁵ Several recent studies provide evidence on influence

⁴ The early evidence from MTO found an impact on the emotional well-being of mothers, and so significant efforts were made to accurately capture the mental health status in the five year follow-up.

⁵ Ioannides and Loury (2004) provide a detailed review of the extensive literature on labor market referrals and networks documenting several important stylized facts. Also see Granovetter (1995).

of social networks. Bayer, Ross, and Topa (2008) find that individuals who reside on the same block or more likely to work together especially when both individuals have a high school degree, have children who are similar in age, are young adults, and are married males. Per-Anders, Fredriksson, and Aslund (2003) examine recent immigrants to Sweden who were exogenously placed in locations as part of a national refuge settlement program. They find that being located in an ethnic enclave improves the labor market outcomes of lesser educated migrants and improves the outcomes for high education migrants for ethnic enclave of high income groups.⁶ Lalive (2003) examines the effect of unemployment benefit extensions in Mexico and finds that increased unemployment among covered workers leads to increases in unemployment for uncovered individuals as well. Munshi (2003) using annual rainfall in home region of Mexico as an instrument for changes in the immigrant network size and network size improves the employment of Mexican migrants. Finally, Furtado and Theodoropoulos (2009) shows that the relationship between employment of immigrants and whether an immigrant marries a native is stronger when networks should be most beneficial i.e. when immigrant group employment rates are low, the immigrant are highly educated, and group population is small.

Some very recent papers exploit employee-employer linked data to examine labor market networks. Hellerstein, McInerney, and Neumark (2009) find that employees at the same firm are more likely to come from the same neighborhood relative to two employees who work in the same location, but at different firms. This effect primarily works within racial and ethnic groups and is stronger for blacks and Hispanics. Dustman, Glitz, and Schonberg (2009) find that minority workers in Germany are much more likely to work in locations where other minorities work, and this effect persists when identified off arguably random variation over time in share minority workers at the firm. Consistent with referrals providing information about workers, they

⁶ Damm (2006) has similar findings for a refuge resettlement program in Denmark.

find that workers at firms with a high share of their minority group earn higher initial wages due to the revealed information, but have slower wage growth over time presumably since firms learn less about workers over time. Finally, Kramarz and Skans (2008) find that children's first jobs after high school or college are disproportionately likely to be at the same employer as their parent even after controlling for the school-graduating class fixed effects. Interestingly, unlike Dustman et al. (2009) who find that the referrals provide information, Kramarz and Skans (2008) find that parental referrals are lead to lower wage placements for their least able children and during bad economic times suggesting some kind of insurance role for parental referrals.

Neighborhoods and Outcomes for Families and Children

As discussed earlier, the experimental evidence for mothers is mixed with Gautreaux mothers having lasting improvements in earnings reductions in welfare usage (Mendenhall, DeLuca, and Duncan, 2006) and MTO yielding substantially better mental health outcomes, having no consistent impact on employment or welfare usage (Kling, Liebman, and Katz, 2007). Several recent papers examine the role of neighborhood on program participation. Bertrand, Luttmer, and Mullainathan (2000) find evidence that residing near individuals who speak the same language as ones self raises welfare usage for members of language groups that have a high rate of welfare participation. Cohen-Cole and Zanella (2008) identify the effect of local welfare usage on individual welfare usage using the presence of individuals who experienced a temporary mental, physical, or emotional shock as an instrument for welfare usage of neighbors. and find an effect of local welfare usage from the welfare usage by members of an individual's own racial or ethnic group. Ashlund and Fredriksson (2008) examine data from the same Swedish refuge placement policy discussed earlier, and find that being placed in a welfare dependent community increases welfare usage. Aizer and Currie (2004) find that publically

funded pre-natal care is concentrated within groups and neighborhoods. They distinguish between first and second birth to identify women who have automatically been informed about public funding and find the same clustering of the use of pre-natal care for both first and second births. They suggest that this effect is not driven by information on availability and appears to be driven by mother's clustering by group and neighborhood into particular hospitals.

The Gautreaux follow-up also examined the long-run differences between the children of urban and suburban movers. Keels (2008) finds reduced arrests and convictions for suburban boys, but increased convictions for girls. Votruba and Kling (2004) find that placement in neighborhoods with greater human capital endowments and employment among residents reduces the mortality rates of Gautreaux children. Keels (In Press) finds that Gautreaux children placed in suburban neighborhoods are more likely to reside in low poverty, suburban neighborhoods as adults. Turning to MTO, the mobility treatment had large, but mixed, impacts on children. Kling, Leibman, and Katz (2007) find strong positive benefits for girls related to mental health, and moderate benefits in terms of educational outcomes and reduced likelihood of engaging in risky behaviors. For Male youth, they found large negative effects associated with injury and substance abuse with implications for the physical health of those youth.

The mixed results for MTO boys and girls, in contrast with the generally positive effects for children in Gautreaux, suggests that residential moves might affect children's outcomes. Pribesh and Downey (1999) and Hanushek, Kain, and Rivkin (2004) use longitudinal data to show that student performance worsens after a residential move. Pettit and McLanahan (2003) and Pribesh and Downey (1999) find that residential mobility reduces the quality of children's social networks. Hanushek, Kain, and Rivkin (2004) find larger effects for low income and minority students. Parente and Mahoney (2009) find that residential mobility is associated with

aggression in boys, but not in girls, potentially consistent with the negative impact of MTO treatment on boy's outcomes.

Beyond experimental evidence, a huge literature has studied the impact of neighborhood on families and children.⁷ While MTO documents mental health effects of neighborhood among mothers and their female children, little credible evidence exists of a causal connection between neighborhood and mental or physical health using non-experimental data.⁸ Several recent articles, however, provide compelling evidence of the impact of neighborhood on children's academic outcomes. Aaronson (1998) exploits the variation created by residential moves and finds that children facing more exposure to neighborhood poverty had lower rates of high school and college completion. Currie and Yelowitz (2000) exploit the gender rules in public housing assignment by examining families with two children, some of which have opposite gender children and so are eligible for an extra bedroom, and they find that children in public housing reside in less dense residential settings and in turn are less likely to be held back in school. Lalive (2003) finds that cash grants to support school attendance that in randomly chosen villages increased attendance by all children including those who did not receive the subsidy.

A considerable literature examines the link between neighborhood and delinquency or criminal behavior. Exploiting the experimental variation in MTO, Ludwig and Kling (2007) find no evidence that neighborhood crime rates explain the criminal activities of movers, but rather that crime is related to neighborhood disadvantage especially racial isolation. Similarly, Jacob, Lefgren, and Moretti (2007) exploit the short-run relationship between crime and weather as a source of exogenous variation. They find that the strong persistence in criminal activity in locations over time that is often viewed as suggestive of social interactions is reversed, and the

⁷ Much of this literature has been reviewed in a series of articles beginning with Jencks & Mayer (1990) and including more recently Ellen and Turner (1997) and Durlauf (2004).

⁸ See Ellen and Turner (1997) who mention the lack of studies on this topic.

reduced crime from a weather shock has no effect on crime in the near future. Several recent papers examine the impact of neighborhood on crime controlling for neighborhood fixed, but the inclusion of neighborhood fixed effects is likely insufficient to identify a causal effect because the neighborhood attributes considered are outcomes as well and may evolve simultaneously with crime. Ihlanfeldt (2007) addresses this concern by including time trends in order to limit the possibility that results are driven by common neighborhood trends and finds that employment access reduce crime. Similarly, Garmaise, Moskowitz, and Tobias (2006) find that bank mergers lead to increases in crime, but find no correlation between crime and contemporaneous or future bank changes.

Homeownership and Neighborhood Outcomes

Most studies of the effect of homeownership offer little evidence on causality beyond conditioning on standard demographic and neighborhood variables.⁹ Some notable exceptions include DiPasquale and Glaeser (1999) who document a strong relationship between homeownership and a variety of citizenship variables even after instrumenting for homeownership using demographic group homeownership rates; and Aaronson (2000), Green and White (1997) and Haurin, Parcel, and Haurin (2001) who find effects of homeownership on children's outcomes in longitudinal samples with good controls for family background.

Homeownership also might influence the surrounding neighborhood and the quality of social networks. However, very few studies examine the correlation between neighborhood owner-occupancy rates and the behaviors and outcomes of residents. Boyle (2002) provides an exception where she examines the correlation between child outcomes and both family owner-

⁹ Rohe, Van Zandt, and McCarthy (2002) and Haurin and Dietz (2003) provide survey the literature mostly from the 1970's and 80's that documents the relationship between homeownership and a wide array behaviors and outcomes including social relationships, participation in community organizations, neighborhood commitment, expenditures on home maintenance, life satisfaction, household mobility, children's behaviors and outcomes, labor market outcomes, and financial success

occupancy and neighborhood owner-occupancy rates. While she finds a relationship for whether the family resides in owner-occupied housing, she finds no correlation between children's outcomes and neighborhood rates of owner-occupancy.

The impact of homeownership on neighborhood attractiveness might be reflected in housing prices or neighborhood stability. Lee, Culhane, and Wachter (1999), Cummings, DiPasquale and Kahn (2002), and Ellen et al. (2002) use a differencing in differences approach by comparing changes in housing prices near a public sponsored development of owner-occupied housing to changes in housing prices in comparable locations. Two of the three studies find positive spillover effects. Next, Rohe and Stewart (1996) finds that the 1980 tract share owner-occupied explains share of households residing 5 or more years in 1990, and Rosenthal (2008) finds that a high owner-occupancy rate substantially slows the rate at which neighborhoods filter through the income distribution as they age.

III. Peer Effects

Unlike with neighborhood effects, peer effects has been studied in a laboratory setting. Some recent examples include Falk and Fischbacher (2002) who find that subjects are less likely to steal when the overall level of stealing in their environment falls, Falk, Fischbacher, and Gächter (2004) who find that a subject's donation to a public good is influenced by the amount donated by a "neighbor", and Falk and Ichino (2006) find that an individual's effort increases with the observable effort of other subjects.¹⁰

Peer Effects in Education

When examining peer effects in the field, however, opportunities for random assignment are more limited. The largest literature using random assignment has arisen in higher education

¹⁰ Social Psychologist experimentally document that peers affect individual's perceptions (Aronson 1999).

where individuals are randomly assigned to a specific residence. The findings are mixed, e.g. Sacerdote (2001) and Marmaros and Sacerdote (2002) find that randomly assigned roommate affects grade point average, the joining of social groups, and employment outcomes of graduating seniors, but Stinebrickner and Stinebrickner (2006) and Foster (2006) find no effect of peers on roommate or floormate GPA, respectively.

Carrell, Fullerton and West (2008) argue that researchers may be understating or missing peer effects in higher education because roommate does not capture a substantial portion of an individual's peer group. They examine randomly assigned groups at the Air Force Academy where these groups are required to interact intensively, and find much larger evidence of the effect of peer. Of course, peer effects may in fact be smaller in other higher education circumstances where peer groups are not forced by rule to involve a small, intensively interacting group. Sacerdote and Marmorose (2006) do find that the estimated peer effects on the likelihood of joining a fraternity are somewhat larger, when roommate as peer is replaced by a proxy based on email contacts, but much smaller effects than Carrel et al., and Foster (2006) finds no evidence that the closeness of a social group leads to larger peer effects on GPA.

In primary and secondary education, the opportunity for random assignment is almost non-existent. The only example in the literatures in the U.S. involves data from the Tennessee STAR's program where students were assigned randomly to different class sizes. In addition to the well know positive effects of class size reductions, Graham (2007) finds that being assigned to classes with higher average test scores raises a student's test scores, and Whitmore (2005) finds that student perform better in classrooms with more girls even after controlling for girls higher test scores.

Opportunities sometimes exist in developing countries for experimental studies of peer effects. For example, in Kenya, 120 schools split their first grade into two classes where randomly allocated between assigning students randomly assign to classes and tracking students based on ability. Duflo, Dupas and Kremer (2008) find that tracking benefits students at all levels of the ability distribution presumably due to increased homogeneity of ability in the classroom. The study also finds that in non-tracked classes students benefits from high ability peers, but finds no evidence that the ability of peers affects student outcomes in tracked schools. Possibly, higher ability peers improve outcomes in very heterogeneous environments where teachers are forced to teach at many different levels, but when teachers can focus on a smaller number of ability levels average peer ability does not appear to matter. In China, Ding and Lehrer (2006) examine student administrative where assignment is based entirely on observable factors that can be included as controls. Duflo et al., Ding and Lehrer (2006) find that student performance is lower when student heterogeneity is high, but they find strong evidence of peer effects even though Chinese schools are highly tracked. Both studies compared nearly identical ability students on either side of admission thresholds to identify peer effects with tracking.

Using data from an Italian University, DeGiorgi, Pellizzari, and Redaelli (2009) and DeGiorgi, Pellizzari, and Woolston (2009) examine the effects of random assignment to sections of compulsory classes. They define a student's peer group as all students who were assigned to one of the same classes. They find a large effect of peers on choice of major between economics and business, but that peer driven choices are associated with lower academic performance, wages, and job satisfaction. They also that reductions in classroom heterogeneity leads to increased academic performance in later classes. However, at an English university, Martins and

Walker (2006) find no effects of peers on examination scores when exploiting the alphabetic assignment of students to class sections.

Most efforts at analyzing peer effects have used school administrative data. In non-random assignment settings, the most convincing evidence of a causal effect of peers on outcomes arises from exploiting variation in student composition across cohorts within schools since parents are unlikely to be able observe to observe composition of a student's potential grade and must select school based on the overall composition (Hoxby 2000). Most studies of this type find little or no correlation between cohort composition deviations and predetermined student attributes. Hoxby (2000) finds lower test scores for students in a cohort with a lower fraction of female students or with more African-Americans. Gould, Lavy, and Passerman (2004), Lavy and Schlosser (2007), Lavy, Passerman, and Schlosser (2008), and Friesen and Krauth (2008) find that presence of disadvantaged immigrant groups, boys, low ability peers, and non-native speakers, respectively lowers performance on standardized test scores. Unlike the other studies, Bifulco, Fletcher, and Ross (2009) use a longitudinal sample where they can observe later life outcomes and find a positive effect of the presence of peer's with college educated mothers on high school graduation, college attendance and substance abuse, but no effect of racial composition on student outcomes after high school.

Hoxby and Weingarth (2005) and Hastings and Weinstein (2009) both exploit variation created by redistricting finding that an increase in average test scores of peers improves student test scores. Hastings and Weinstein (2009) find that the effect of peer ability in their estimates for movers based on a redistricting are driven almost entirely by female students. They also find no evidence of peer effects associated with parental income or minority students after controlling for peer achievement. Hastings and Weinstein (2009) also control for neighborhood fixed effects,

which essentially identifies the effect of peers by comparing individual who are on either side of the new attendance zone boundary. When comparing students in the same neighborhood, Hastings and Weinstein (2009) find no correlation between whether a student was redistricted and student pre-determined attributes; similar to diagnostics run for cohort studies.¹¹

A number of studies have attempted to look at peer effects within classrooms. The attraction of such studies is that students that share a classroom almost certainly interact with each other and form a more natural peer group than either cohorts or schools. The associated cost, however, is that within classroom variation is much more likely to be influenced by parental or teacher involvement in the assignment of students to classrooms. Vigdor and Nechyba (2004) and Atkinson et al. (2008) both identify schools for which classroom assignment is apparently random by examining the distribution of classroom characteristics within schools and find positive effects of peers on academic performance.

Mechanisms for Peer Effects in Education

Some cohort studies have uncovered evidence concerning the mechanisms underlying peer effects. Lavy and Schlosser (2007) and Lavy, Passerman, and Schlosser (2008) find that classroom gender and ability composition, respectively, influence the student-teacher relationships, teacher's practices, and classroom disruption and violence, and they suggest these behavioral responses as mechanisms by which gender and ability composition affect outcomes. On the other hand, Bifulco, Fletcher, and Ross (2009) find no evidence that parental education of peers affects school environment. Rather, their findings suggest a contagion effect because children of college educated parents are more likely to graduate from high school and attend

¹¹ A few studies have used movers to identify the effect of peers on student outcomes, but unlike the within school variation of cohort based studies or the redistricting study with neighborhood fixed effects student mobility is likely associated with unique events that create differences between the movers and non-movers in the same neighborhood or school grade.

college and all students have higher rates of these activities when there are substantial numbers of children with college educated parents.¹²

A number of other studies document a relationship between disruptive behavior and peer effects. Aizer (2008) estimates the impact of having classmates with Attention Deficit Disorder before and after diagnosis finding that diagnosis improves peer performance. Similarly, Hoekstra and Carrell (In Press) find that students from families with domestic violence reports exhibit more disruptive behavior, and peers of students exposed to domestic violence have worse behavior and academic when compared to the peer's siblings who did not have such exposure. . Further, as with Aizer, these effects disappear after an intervention, the reporting of the violence to the court (Hoekstra and Carrell, 2009). The fact that the peer effect is ameliorated by actions expected to mitigate disruptive behavior makes these studies especially convincing.

Several other papers provide substantial evidence of a role for student behavior and peer effects. Neidell and Waldfogel (2008) find that presence of classroom peers who have been to preschool affects cognitive achievement in kindergarten and the effects operate through the disruptive behavior of students with the most severe behavior problems. Figlio (2007) finds that boys with names that are most commonly given to girls are more prone to misbehavior as they get older and that the presence of boys with such names leads to lower test scores and more behavior problems among other students. Finally, MacCoon et al. (2008) find that sixth grade students attending middle schools rather than grade school, and so are exposed to cohorts of older and likely more delinquent peers, are more likely to be cited for discipline problems

¹² Bifulco et al. find, however that racial composition (share of black and Hispanic students) has a negative impact on the school environment and student behaviors even though racial composition has no impact on educational attainment or behaviors after leaving high school.

Peer Effects on Student Substance Abuse and Health

Typically, studies of peer effects on substance abuse and health behavior have been forced to rely on cross-sectional and longitudinal surveys, rather than the detailed school administrative data that have been used to develop convincing evidence of peer effects on academic outcomes. Therefore, the evidence of a causal relationship between peers and substance abuse and health behaviors is typically quite weak. A few notable exceptions do exist. Argys and Rees (2008) use the month of a child's birth combined with across state differences in the age requirements for starting school and find that females with older peers are more likely to start using marijuana, alcohol, and tobacco earlier in life. Bifulco, Fletcher, and Ross (2008) apply the cohort approach to longitudinal data collected and find that share minority students contributes to marijuana use in high school and share students whose mothers do not have a college degree contributes to post high school marijuana use. Finally, following the work of Altonji, Elder, and Tabor (2005), Krauth (2005) uses selection on observables to bound the effect of peers on smoking and continues to find evidence that peers matter.

Several studies have also documented a potential role of peers in obesity. Cohen-Cole and Fletcher (2008a) replicate earlier findings using longitudinal data and find that the estimate peer effects on obesity are not robust to standard controls for heterogeneity across schools. In addition, Cohen-Cole and Fletcher (2008b) examine the impact of friends or peers outcomes, such as height, acne, and headaches, for which a causal peer effect is typically thought to be implausible, and using standard approaches they find positive and statistically significance evidence of peer effects that are quite likely spurious.

Peer Effects and Friendship Networks

Many studies define peers as individuals who self-identify as friends or social contacts as opposed to simply being members of the same classroom, school, or neighborhood. Halliday and Kwak (2008b) directly argue that friendships are a better indicators of peers than other indirect connections. They estimate models of peer effects using defining peers either based on school and grade or based on friendship nominations and find much larger peer effects using friendship nominations. Empirical research on social networks has been growing rapidly in recent years and in part, this growth has been fueled by the National Survey of Adolescent Health (AddHealth) because the initial survey conducted a virtual census of students in a sample of schools and asking those students to identify up to five each of their male and female friends.

Several studies have used this unique data to test whether key features of the friendship network and/or student's position within the network can explain outcomes. In their study of teenage suicide, Bearman and Moody (2004) find that both network isolation and intransitivity, which captures being friends with people that do not form a cohesive group of friends, are associated with higher rates of suicidal thoughts (females only) and suicide attempts. In a study of delinquency, Haynie (2001) finds that delinquency falls with both the centrality of a student's position within the friendship network and the density of their network. Crosnoe and Needham (2004) finds that network centrality is an important factor in the formation of student clusters and that those clusters explain delinquency even after controlling directly for the behavior of peers. Calvó-Armengol, A., Patacchini, E. and Y. Zenou (In Press) find that being more centrally located in the network increases student performance. Most notably, they find no correlation between pre-determined variables and peer attributes after controlling for network fixed effects suggesting no systematic sorting of students into friendship pairs within networks. In a

companion paper, Patacchini, E. and Y. Zenou (2008) finds that links to students involved in crime increase one's own criminal activities relative to other in the same network. Babcock (2008) explicitly uses a cohort approach and find that being part of a more connected grade raises the likelihood of high school graduation and college attendance. He also finds that the benefit of increased connectivity exists even if individuals are connected to low performing students. Also following a cohort approach, Nathan finds that racial heterogeneity of friendships leads to higher academic performance especially among whites.

In related work, Warr (1996) examines data on youth delinquency using the 1967 National Longitudinal Survey of Youth (NSY), which surveyed respondents on criminal activity including number of companions present during particular delinquent acts and their age, sex, closeness to the respondent, and whether they initially suggested committing the delinquent act. Warr finds that offenders commonly belong to more than one delinquent group, delinquent groups are more specialized in acts than the individual offenders, most groups have an identifiable older instigator, offenders evolve in their role with experience, and delinquent groups tend to sort by gender. These findings suggest the importance of more complex relationships in friendship networks than have been captured by the existing literature. In addition, Jacob and Lefgren (2003) examine daily criminal activity using plausibly exogenous variation in teacher in-service days and find that school attendance increases violent crimes suggesting a role for social interactions in school in propagating criminal acts.

Peer Effects in Other Settings

Several papers examine the impact of peers on either absenteeism or productivity. On absenteeism, Ichino and Maggi (2000) focus on movers between the branches of an Italian bank finding that workers adapt to the absenteeism norm of the new location. Similarly, Bokenblom

and Ekblod (2007) find that work group absentee rates affect individual absenteeism controlling for workplace fixed effects using data from a large municipal government. They also find that effects occur within age and gender groups. Lindbeck, Palme, and Persson (2007) examine the impact of neighborhood absenteeism rate and again find that individual movers adjust to neighborhood absenteeism norms. Most convincingly, De Paola (2008) finds peer effects on absenteeism in small work groups at a large public Italian institutions where positions are awarded via national competition and assignment to work groups is random.

On productivity, Falk and Ichino (2006) provide experimental evidence in a laboratory setting that an individual's effort increases with the observable effort of other subjects. Bandiera, Barankay and Rasul (2005) find that a shift from relative compensation to piece rate increases the productivity of fruit pickers and the effects were largest when the workers were friends and could observe each other consistent with workers internalizing the negative effect of high productivity on others under the relative compensation plan. Mas and Moretti (2006) examine scanner data for grocery checkers and find positive spillovers from the presence of high productivity workers especially among low productivity workers.

Finally, Nanda and Sorenson (2008) uses an employee-employer panel from Denmark allowing them to control for prior career experiences and time varying peer entrepreneurial experience. They find positive effects of peers' previous entrepreneurial experiences and peers experience diversity on self-employment. These effects are smaller for people whose parents were self-employed suggesting that exposure to peers mitigates the impact of a lack of own experience with entrepreneurial activity.

In other contexts, Nair, Manchanda, and Bhatia (2006) examined detailed individual prescription data along with self-reported information by physicians of the other doctors on

whose opinions they rely using the release of new guidelines about a specific drug as a exogenous shock to peer prescription behavior. They find strong evidence that peers effect the prescription decisions of physicians. In a pair of papers, Duflo and Saez (2002, 2003) examine the impact of peers at work on participation in retirement plans at a large university using wage and tenure structure of departments as an instrument for enrollment levels and examine the results of an experiment where departments are selected at random and random individuals within selected departments were given a financial incentive to attend a information fair. They find strong evidence that peers enrollment affect individual enrollment decisions. Bayer, Hjalmarsson, and Pozen (In Press) find that individuals incarcerated with people who have committed a crime in the past are more likely to commit that same crime in the future. **V.**

Implications of Research on Social Interactions for Urban Planning

Based on my survey, the planning literature has very few studies neighborhood and peer effects. An important area of overlap, however, is the literature on the redevelopment of public housing. For example, recent studies like Clampet-Lundquist (2004) and Boston (2005) document that households displaced by Hope VI public housing redevelopment projects end up residing in better neighborhoods. On the other hand, Goetz (2002) and Keeting (2000) criticize Hope VI decriing the loss of public housing units and the breaking up of established, supportive communities within existing public housing projects. For example, Gibson and Toulan (2007) document the strong social ties and physical safety found in a public housing complex that was redeveloped under Hope VI, while Kleit (2004) questions whether value of the mixed income neighborhoodl created by Hope VI finding that most social interactions in a project occur along ethnic and economic lines.

Planners and policymakers have long standing interests in housing dispersal policies for the poor (Goetz, 2003), and the neighborhood effects literature is often central in this debate. Are the current social networks in place for public housing residents a valuable resource, or will displacement to a lower density, lower poverty neighborhood contribute to the resident well-being? The negative findings on employment and the mixed findings for children in MTO clearly raises questions about the benefits of Hope VI for current public housing residents point to substantial mobility costs from relocation.

Further, both research on ethnic clusters and friendship networks have implications for the potential benefits of mixed income communities, a goal of the “New Urbanism” movement (Talen, 2002). While racial segregation has negative effects for African-Americans, the general literature suggests that residing in ethnic clusters can be beneficial. Many of the benefits of integration with a high skill majority are unlikely to accrue without meaningful social interactions between the different groups residing in the same neighborhood, and as shown in the peer and neighborhood effect literature such social interactions tend to take place along socio-economic and demographic lines.

More generally, research on neighborhood effects relates to the general question of social capital. Social capital is typically thought of as the quality of norms, trust and networks among people that contribute to successful community activities and individuals outcomes. The recent popularity of the term social capital can be traced back to articles on the subject by Putnum (1993, 1995). Soon afterwards, an entire issue of *Housing Policy Debate* was dedicated to applying the concept of social capital to community development (Lang and Hornberg, 1998). More recently, a symposium in the *Journal of the American Planning Association* (Hutchinson,

2004) defines, measures, and provides prescriptions for the development of social capital in order to further community development goals.

Similarly, planner's interest in sprawl is motivated in part by the implications of sprawl for social capital and social interactions between neighbors. Ever since Putnum (1995) coined the term "bowling alone," planners have increasingly asked whether spatial form of America's metropolitan areas lead to an isolation of households from their same neighbors and considered the impacts of such isolation. Burchell et al. (1998) and Ewing (1997) both identify loss of connection between neighbors and the accompanying values and responsibilities as a major cost of sprawl. This view stands in contrast to historical views of high density urban areas and the associated anonymity as isolating individuals and breaking down community bonds (Jacobs and Appleyard, 1987; Churchman, 1999).

Both the recent interest in social capital and sprawl expand set of relevant issues for designing successful strategies for economic development and urban redevelopment. There is an important social element to revitalizing impoverished neighborhoods and declining central business districts, as opposed to simply addressing the flight of capital. The neighborhood effects literature suggests that social networks can be very productive in generating successful labor market outcomes, but the literature also suggests substantial barriers to establishing and maintaining networks across diverse groups. In fact, Peters and Fisher (2004) argue that economic development policy needs to be radically transformed with more attention focused on improving worker employability and community development efforts, and Bendick and Eagan (1993) suggest coordinating economic and community development efforts. In fact, understanding neighborhood effects is central to the longstanding competition between people and place based initiatives for addressing urban problems because spillovers between

neighborhood residents can magnify both types of interventions and yet those interventions may have very different impacts on the social networks that drive such spillovers.

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