DESTINATION EFFECTS: RESIDENTIAL MOBILITY AND TRAJECTORIES OF ADOLESCENT VIOLENCE IN A STRATIFIED METROPOLIS*

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Two landmark policy interventions to improve the lives of youth through neighborhood mobility—the Gautreaux program in Chicago and the Moving to Opportunity (MTO) experiments in five cities—have produced conflicting results and have created a puzzle with broad implications: Do residential moves between neighborhoods increase or decrease violence, or both? To address this question, we analyze data from a subsample of adolescents ages 9–12 years from the Project on Human Development in Chicago Neighborhoods, a longitudinal study of children and their families that began in Chicago—the site of the original Gautreaux program and one of the MTO experiments. We propose a dynamic modeling strategy to separate the effects of residential moving across three waves of the study from dimensions of neighborhood change and metropolitan location. The results reveal countervailing effects of mobility on trajectories of violence; whereas

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neighborhood moves within Chicago lead to an increased risk of violence, moves outside the city reduce violent offending and exposure to violence. The gap in violence between movers within and outside Chicago is explained not only by the racial and economic composition of the destination neighborhoods but also by the quality of school contexts, adolescents' perceived control over their new environment, and fear. These findings highlight the need to simultaneously consider residential mobility, mechanisms of neighborhood change, and the wider geography of structural opportunity.

In 1995, researchers conducted a baseline survey of caregivers who had volunteered for the Moving to Opportunity (MTO) program, a bold social experiment that randomly offered vouchers to public housing residents in five cities to move to low-poverty neighborhoods. When caregivers were asked about the most important reasons for moving, three out of four said that they wanted to move their children away from gangs and drugs (Kling, Liebman, and Katz, 2007). The potential for their children to become engulfed in the violence that surrounded them was, by a wide margin, the strongest force driving parents' desire to escape ghetto poverty (see also Popkin and Cove, 2007).

Years later, however, researchers were surprised to find that youth in experimental families who had moved to neighborhoods with lower poverty, overall, were no less entangled in the violence that characterized their origin neighborhoods than the control group. For example, adolescent boys and girls in the experimental group were not significantly different than the control group in reports of having been victimized or "jumped," seeing someone shot or stabbed, or taking part in violent activities themselves (Kling, Liebman, and Katz, 2007; Kling, Ludwig, and Katz, 2005). Moreover, although boys in the experimental group were less likely to be arrested for violent crime, they were *more* likely to be arrested for property crimes, to report a non–sport-related injury, to have a friend who used drugs, and to engage in risky behaviors themselves. On balance then, the experiment produced few consistent results related to youths' experiences with violence; for some outcomes, girls in the experimental group fared better than the controls, but these gains were counterbalanced by negative effects found for boys.

These results stand in striking contrast to those found in the now famous Gautreaux program, conducted in Chicago in the 1970s, which provided the evidentiary basis for federal investment in MTO decades later. Gautreaux was a court-ordered desegregation program that offered low-income families, most of whom were African American and receiving welfare, apartment units throughout the Chicago metropolitan area (Mendenhall, DeLuca, and Duncan, 2006; Rubinowitz and Rosenbaum, 2002).

Research from Gautreaux has shown that boys in families that moved to the suburbs were less likely to be arrested for drug, theft, or violent crimes, although these effects were not present for girls who moved to the suburbs and who had nonsignificant effects on arrests and higher rates of conviction (Keels, 2008). Other research from Gautreaux has shown positive effects of the program on youth mortality, stemming largely from lower levels of homicide victimization among youth in families that moved (Votruba and Kling, 2008).

A growing literature seeks to reconcile these somewhat puzzling findings with the main debate focusing on the strength of the neighborhood treatment and on potential violations of the assumptions that are necessary to make causal inferences in social experiments (Clampet-Lundquist and Massey, 2008; Ludwig et al., 2008; Sampson, 2008; Sobel, 2006). Experimental or quasi-experimental data from residential mobility programs like Gautreaux or MTO are appealing because they have the potential to produce exogenous variation in neighborhood environments, thereby counteracting "selection bias" when estimating neighborhood effects (Ludwig et al., 2008). Despite this advantage, however, residential mobility programs are not designed to assess the social processes or mechanisms that mediate the relationship among residential mobility, neighborhood change, and youth development. Indeed, most research from prominent programs such as MTO has focused on the first-order relationship between vouchers and social outcomes, leaving a gap in knowledge about what happens between the offer of a housing voucher and any given outcome of interest.

We believe that the "why" questions about mobility that the voucher experiments so clearly motivate are especially important to criminology. Does moving increase or decrease an adolescent's exposure to violence? Violent offending? Does neighborhood change matter and why? To answer these questions, we draw on and integrate two strands of research, the first focusing on the relationship among residential moves, the formation and dissolution of social capital, and youth development, and the second focusing on resources available to youth in different settings within highly stratified metropolitan areas. Integrating these two bodies of work suggests a revised perspective on residential mobility and violence, one that considers the distinct influences of moving itself, the larger geographic and structural changes resulting from a residential move, and the resulting change in resources and risks in the local neighborhood environment. The overall aim is to contribute to a theoretically grounded approach to the study of how residential mobility influences adolescents' developmental trajectories.

To do so, we draw on data from a longitudinal study of youth and their caregivers living in Chicago in the mid-1990s and followed wherever they

moved across three interview waves. These data are used to examine the effects of residential mobility, geographic destination, and neighborhood change on three distinct forms of violence—*individual violent behavior*, *exposure to violence*, and *violent victimization*. Chicago provides a theoretically strategic setting for the analysis because of its racial and ethnic diversity as well as the stark distinction between residential settings within and outside the city. Chicago is also one of the five MTO study sites, and the original Gautreaux mobility program was set in Chicago, with the city/suburban distinction being the most salient factor in predicting the outcomes of families taking part in the program, motivating our focus on the differential impact of moves within the city compared with suburban moves. Using methods to estimate the impact of time-varying treatments in the presence of time-varying confounders, we show that when adolescents move but remain within Chicago, they are more likely to exhibit high levels of violent behavior and be exposed to violence. The effects of moving outside the city generally show the exact opposite impacts, leading to lower levels of violent behavior and exposure to violence. The geographical and larger contexts of mobility thus matter beyond the socioeconomic dimensions of neighborhood change.

THE SETTING AND QUESTIONS

Much of the optimism regarding the potential impacts of residential mobility programs stems from the success of the Gautreaux program. The goal of Gautreaux was to place families in nonsegregated neighborhoods of less than 30 percent Black residents, although families also could be placed in neighborhoods that showed indications of strong economic development regardless of their racial composition. Although the program was not a true experimental design, the apartments offered to families that volunteered were determined largely by waitlist, and virtually all families (95 percent) accepted the first apartment offered to them (Mendenhall, DeLuca, and Duncan, 2006). Early research from Gautreaux argued that this process created an exogenous source of variation in the destination neighborhoods of participants, making it possible to compare the outcomes of families that ended up in different types of neighborhoods (Rubinowitz and Rosenbaum, 2002). Exploiting this variation in residential destinations, most research from Gautreaux has compared families that remained within the city and those that moved to Chicago's suburbs. For instance, the original studies from the program found that youth in families that moved to Chicago's suburbs were less likely to drop out of school, were more likely to enroll in college-track courses, and were more likely to be employed (Kaufman and Rosenbaum, 1992; Rosenbaum,

1995).[1]

One potential reason for the divergent results from Gautreaux and MTO is that the two programs had different designs. MTO is an experiment and, although not immune to violations of central assumptions necessary for causal inference (see Sampson, 2008; Sobel, 2006), a case can be made for considering the estimates derived from MTO as causal estimates of residential mobility to neighborhoods with relatively low poverty. Another perhaps more likely explanation is that the "treatments" in the two programs differ in fundamental ways. In most analyses of Gautreaux, the treatment is defined as moving to a neighborhood outside Chicago as compared with moving to a new neighborhood within the city (Rubinowitz and Rosenbaum, 2002). Because no true control group exists, the central variation in Gautreaux lies in the *geographic destinations* of families in the program, all of whom experience a residential move. By contrast, the treatment group in MTO comprises families offered vouchers that allow them to move to relatively low-poverty neighborhoods, whereas the control group comprises families not offered vouchers. This design allows for an unbiased estimate of a treatment effect that is very different from the treatment in Gautreaux. Instead of focusing solely on the effect of residential destination, the treatment effect in MTO actually combines two dimensions—first, the effect of a residential move and, second, the effect of a change in the economic composition of the neighborhood. Interpretation is complicated even more by the fact that MTO families in the treatment group were more likely to continue to make moves after their initial lease-up. In the MTO Interim Impacts Evaluation (Orr et al., 2003), which was conducted more than 4 years after families had entered the program, fully 31 percent of families in the treatment group reported living in their current housing for less than 6 months compared with just 9 percent of families in the control group.

In short, we argue that the effect of moving is theoretically and empirically distinct from the effect of an improvement or change in neighborhood conditions. The life-course literature on the effects of residential mobility on various developmental outcomes suggests that this distinction might be important for crime and thus deserves to be unraveled.

1. Recent research has challenged the claim that families' destination neighborhoods are exogenous, showing that characteristics of participating families' origin neighborhoods are associated with characteristics of their destination neighborhoods (Keels et al., 2005; Votruba and Kling, 2008). According to these critics, this evidence might suggest that families' preferences played some role in the assignment of families to apartments.

RESIDENTIAL MOBILITY IN THE LIVES OF CHILDREN AND ADOLESCENTS

In his foundational work on social capital in the lives of youth, James Coleman (1988) hypothesized that residential moves might lead to disruptions in the structure of intergenerational social networks linking parents with children, their children's peers, and other adults in the community. This type of intergenerational network closure is important in enabling parents to provide effective social controls for their children through interactions with their own children, as well as contact with the friends of their children and with the parents of these friends (Hagan, MacMillan, and Wheaton, 1996). When a family moves, the parent–child relationship typically remains intact, but the relationships that facilitate intergenerational closure are severed. It is these types of relationships that form the basis of social closure within a community, which is an essential element of collective efficacy available for children (Sampson, Morenoff, and Earls, 1999).

Empirical work at the community level has shown that the degree of residential mobility is linked with processes of social disorganization and with rates of violence (Sampson, Morenoff, and Gannon-Rowley, 2002). At the individual level, the connection among residential mobility, social capital, and development is supported in a series of studies that assess the influence of residential mobility on various developmental outcomes. Considerable evidence from this literature indicates that residential moves are associated with declines in academic performance and educational attainment as well as with increased levels of drug use, sexual activity, and other risky behaviors (Coleman, 1988; Hagan, MacMillan, and Wheaton, 1996; Pribesh and Downey, 1999). Particularly relevant from the perspective of the current analysis is a study demonstrating a strong, positive relationship between residential mobility and adolescent violent behavior using data from the National Longitudinal Study of Adolescent Health (Haynie and South, 2005). Moreover, recent research on victimization has demonstrated that when a dwelling unit turns over, its likelihood of being victimized is increased substantially (Xie and McDowell, 2008), which suggests a direct effect of mobility.

Although prior research has established a general association between residential mobility and numerous outcomes, it does not examine whether the influence of a residential move is contingent on the destination. Qualitative research from MTO designed to uncover the processes underlying the results found in the quantitative studies suggests that the local environment into which families move is crucial in influencing how youth respond (Clampet-Lundquist et al., 2006; Pettit, 2004). Exploring the gender differences that emerged earlier in the MTO study, Clampet-Lundquist et al. (2006) found that young men in the Chicago and Baltimore experimental

groups frequently considered their new neighborhoods, which typically were located within the city limits, to be little different from their original neighborhoods in terms of the threat of violence, the prevalence of drug markets and gang activity, or the presence and role of the police. Youth in families that moved also did not experience much change in their school environment, as parents lacked familiarity with ways to navigate the school system or were focused on more pressing problems than the quality of the child's school, such as financial issues or family legal problems. At the same time, boys in the experimental group were less equipped to navigate their new environments successfully to avoid trouble. Boys in the control groups that did not receive vouchers continued to live in violent neighborhoods but frequently mentioned various strategies they used to steer clear of potentially violent or dangerous situations. Along with the continued threat of violence in the neighborhood and the school, these differences in perceived ability to avoid violence emerged as a central hypothesis as to why young men in the experimental group in MTO showed the same or even increased levels of criminal or risky behaviors relative to boys in the control group.[2] It is a finding that is consistent with research focusing on adolescents' perspectives toward violence as an important predictor of the environments they create for themselves—what Sharkey (2006) refers to as "street efficacy."

Considering the experimental and observational literature on residential mobility as a whole, we are led to hypothesize a conditional relationship between residential mobility and neighborhood change in the explanation of adolescent development and violence. The MTO experiment was based on the theory that declines in neighborhood poverty would lead to improved developmental outcomes. In designing an experiment to provide a precise test of this theory, researchers tended to set aside literature showing that residential mobility itself has been found to influence development negatively, independent of any influence of the neighborhood environment. By contrast, the separate literature on the relationship between residential mobility and youth development generally has failed to consider the possibility that the impact of a residential move is contingent on the characteristics of the origin and destination neighborhood.

This article integrates these two strands of research to present what we believe is a more complex and yet realistic analysis of residential mobility as it usually unfolds in the lives of families within the highly stratified neighborhoods and metropolitan areas that characterize the United States.

2. These same patterns were not found among young women in the MTO experiment. Young women did not identify the same risks in their destination neighborhoods and were able to assimilate into these destinations more easily.

Rather than treating all moves as if they are part of the same causal process, we hypothesize that residential mobility brings youth into very different geographic and politically shaped environments (e.g., suburbs, school districts, and policing districts). The effect of moving on trajectories of adolescent violence, therefore, is argued to be dependent not just on neighborhood context but also on the larger social structure within which neighborhoods are embedded. After a description of the data, we offer an analytic formulation of a test of this overarching hypothesis.

DATA AND MEASURES

This study builds on a program of ongoing interdisciplinary research—the Project on Human Development in Chicago Neighborhoods (PHDCN). The overarching goal was to study developmental change in its changing neighborhood context. Although lacking the statistical advantages of an experimental design, unique features of the PHDCN study combine to offer analytic advantages that allow us to build on the research findings from experimental and quasi-experimental residential mobility programs to explore the underlying social processes at work. In particular, rather than starting with a sample in poverty or in public housing, the PHDCN reflects an ethnically diverse population representative of youth growing up in Chicago—the site of the Gautreaux program and one of five MTO study sites. The sampling frame thus allows for generalizations that extend beyond the population of poor public housing recipients targeted by residential mobility programs, and enables us to examine whether the same processes are at work across a representative sample of youth growing up in Chicago neighborhoods.

The sampling frame for the Longitudinal Cohort Study (LCS) is based on 1990 U.S. Census tract data for Chicago, which were used to identify 343 neighborhood clusters (NCs)—groups of 2–3 census tracts that contain approximately 8,000 people. Major geographic boundaries (e.g., railroad tracks, parks, and freeways), knowledge of Chicago's local neighborhoods, and cluster analyses of census data guided the construction of NCs so that they are relatively homogeneous with respect to racial/ethnic mixture, socioeconomic status (SES), housing density, and family structure. For the LCS, a two-stage sampling procedure was used that included selecting a random sample of 80 of 343 Chicago NCs stratified by racial/ethnic composition (seven categories) and SES (high, medium, and low). The aim was to have an equal number of NCs in each of the 21 strata that varied by racial/ethnic composition and SES. This objective was well approximated with only three exceptions—low-income White, high-income Latino, and high-income Latino/African American neighborhoods did not exist.

Within these 80 NCs, youth falling within seven age cohorts (ages 0, 3, 6, 9, 12, 15, and 18) were sampled from randomly selected households. This effort led to screening more than 40,000 households to obtain the desired sample. Dwelling units were selected systematically from a random start within enumerated blocks. Within dwelling units, all households were listed and age-eligible participants (household members within 12 months of age 0, 3, 6, 9, 12, 15, or 18) were selected with certainty. As a result, multiple siblings were interviewed within some households. Participants are representative of families living in a wide range of Chicago neighborhoods (16 percent European American, 35 percent African American, and 43 percent Latino) and evenly split by gender. Extensive in-home interviews and assessments were conducted with the sampled children and their primary caregivers at three points in time during a 7-year period at roughly 2-year intervals (wave 1 in 1995–1997, wave 2 in 1997–1999, and wave 3 in 1999–2002). Follow-up retention was relatively high for an urban sample—75 percent overall at wave 3 (Sampson, Sharkey, and Raudenbush, 2008).

We measure the following domains of adolescent violence: commission of violent behavior, exposure to serious violence, and violent victimization. The assessments of self-reported violence, exposure to violence, and victimization were given only to members of the older age cohorts (ages 9, 12, 15, and 18). Because of their status as young adults who were making independent residential choices, members of the 15- and 18-year age cohorts were excluded from the analysis, restricting our focus to older children and adolescents in age cohorts 9 and 12 ($N = 1{,}645$). *Violent behavior* is measured as the scale score from a Rasch model based on self-reported responses to 12 items asking whether subjects had committed a given violent act in the year prior to the interview (Raudenbush, Johnson, and Sampson, 2003). Previous research has shown that self-reported survey items are reliable indicators of criminal or violent behavior (Huizinga and Elliott, 1986). *Exposure to violence* is a dichotomous measure coded positively if the subject reported witnessing any of the following acts in the year prior to the interview: seeing someone attacked with a weapon, shot at or shot, sexually assaulted, or threatened with serious physical harm. *Violent victimization* is a dichotomous measure coded positively if the subject reported being victimized in any of the following ways in the year prior to the interview: being hit/punched outside the home, chased maliciously, attacked with a weapon, shot at or shot, or threatened with bodily injury. Similar measures of exposure to violence and victimization from the PHDCN have been validated elsewhere (Bingenheimer, Brennan, and Earls, 2005; Sharkey, 2006), and a measure of exposure to violence has been shown to predict subsequent neighborhood attainment in the PHDCN as well (Sampson and Sharkey, 2008).

We analyze two kinds of mobility, which for heuristic purposes, we conceptualize as the "treatment." Moving within Chicago is coded positively if the subject changes address between waves of the survey and enters a new census tract within Chicago; moving outside Chicago is coded positively if the subject moved from a tract within Chicago to a new tract outside Chicago's city limits.[3] In our sample, most moves outside the city did not take families far from the city—for instance, approximately 73 percent of subjects who moved out of Chicago at wave 2 remained in Illinois, and 59 percent who left Chicago at wave 3 remained in the state. Both treatments are defined at wave 2 (capturing moves made between the baseline or wave 1 interview and the wave 2 interview) and at wave 3 (capturing moves made between the wave 2 and wave 3 interviews). Descriptive statistics showing the prevalence of each treatment by race/ethnic group are shown in table 1. Moving within Chicago is more common among African Americans and Latinos compared with Whites, whereas exiting the city was most common among Whites. However, each treatment had sufficient representation of all race/ethnic groups, allowing us to estimate effects for the pooled sample as opposed to conditioning by subgroup.

Table 1. Percentages Moving Within and Outside Chicago by Race/Ethnicity: PHDCN Cohorts 9 and 12

Treatment Definition		Full Sample (N = 1,645)	African Americans (n = 587)	Whites (n = 227)	Latinos (n = 765)
Moved within Chicago	Wave 2	18%	22%	8%	19%
	Wave 3	19%	24%	5%	20%
Moved outside Chicago	Wave 2	7%	7%	12%	5%
	Wave 3	8%	7%	11%	7%

STABLE AND TIME-VARYING PREDICTORS OF MOBILITY

Pathways of mobility are modeled with a set of stable and time-invariant family- and subject-level covariates along with a set of time-varying covariates that had been validated in prior work. We begin with the *age* and *sex* of both subjects and caregivers along with the family's *length of residence* at the baseline address—an important predictor of moving. The

3. A limitation of the PHDCN is that it does not allow for the investigation of how mobility *into* Chicago might affect violence. This question requires a different study design.

caregiver's *race/ethnicity* is coded with several indicator variables denoting whether the caregiver is African American or Hispanic/Latino, with White and other racial or ethnic groups combined in the reference group. The subject's *immigrant generation* is measured with three dummy variables, indicating whether he/she is a first-generation (i.e., born outside the United States), a second-generation (i.e., at least one of the subject's birth parents was born outside the United States), or a third-generation or higher (the reference group) immigrant. We also included a *citizenship* variable (yes or no) indicating whether the caregiver is a U.S. citizen and a measure of *English language proficiency*, which is a self-reported indicator of whether the caregiver considers her/his English to be proficient. The caregiver's *educational attainment* is measured with four dummy variables indicating whether the caregiver has less than a high-school diploma, a high-school diploma or GED (the reference group), some college or professional school, or at least a college degree.

We measure several constructs that tap the capacity of caretakers to make residential choices. On the vulnerability side, we include problems with the criminal justice system, violence, and mental health that are known to compromise life-course outcomes. *Family criminality* represents the number of family members with a criminal record. *Domestic violence* represents the sum of dichotomous responses to nine survey items asking caregivers about violent or abusive interactions with any current or previous domestic partner. The measure of domestic violence is based on the Revised Conflicts Scale (CTS2) and has a reliability of .84. Evidence of the scale's validity is provided in Straus et al. (1996). On the support side, we examine a scale of *social support,* which for a long time has been considered a means by which parents collectively can manage parenting tasks and maintain informal controls over youth (Furstenburg, 1993). Building on this idea, we conceptualize the social support available to parents as a potentially important influence on the decision to move. The caregiver's perceived level of social support is captured by the mean of 15 survey items on the degree to which the caregiver can rely on friends and family for help or emotional support and on the degree of trust and respect between the caregiver and his/her family and friends. The reliability of the scale of social support is .77. Each measure of vulnerability/capacity significantly predicts neighborhood attainment in bivariate analyses and thus shows predictive validity (see also Sampson, Sharkey, and Raudenbush, 2008). Although these measures in principle can vary throughout time, they are available in only one wave of the survey, so we treat them as "stable" covariates in the specifications.

In addition to the set of stable covariates, we include a set of *time-varying* covariates that capture change in key aspects of individuals' lives occurring during the course of the survey. The first group relates to

employment and economic circumstances and includes the following measures: the *employment status* of the caregiver and the caregiver's spouse or partner (working or not working); the caregiver's total *household income*, which is measured with six dummy variables indicating whether total household income is less than $10,000 or between $10,000 and $19,999, $20,000 and $29,999, $30,000 and $39,999 (the reference group), $40,000 and $49,999, and $50,000 and higher; a measure of *occupational status*, which is based on the socioeconomic index (SEI) for caregivers (Nakao and Treas, 1994)[4]; and a dummy variable indicating whether the caregiver is *receiving welfare*. We also include time-varying measures of *home ownership*, *household size*, and the caregiver's *marital status*, which consists of dummy variables indicating whether the caregiver is single (the reference group), cohabiting, or married.

To capture changes in caregiver's mental health, we include a measure of caregiver *depression*, which is a dichotomous measure coded positively if the caregiver is classified as having experienced a period of major depression in the year prior to the interview. The measure of major depression is based on the *Composite International Diagnostic Interview Short Form* (Kessler and Mroczek, 1997), which yields a reliability of .93. Lastly, a measure of subject *peer delinquency* is constructed to tap into caregivers' concerns about their children's peer groups as a potential motivation to move. Peer delinquency also is consistently one of the strongest predictors of youth violence and delinquency, thereby simultaneously addressing selection concerns with respect to our outcomes. Our measure represents the mean value of responses to several survey items asking subjects about the prevalence of delinquent activities among their friends (Sharkey, 2006).

The descriptive statistics for all covariates used to model the selection into the two treatments of moving are shown in the first two columns of table 2.

4. If the caregiver is not employed and has a partner, then the partner's SEI score is used. If both the caregiver and a partner are employed, then the maximum score is used.

Table 2. Descriptive Statistics for Baseline and Time-Varying Predictors (Columns 1 and 2) and Logit Model Results for Selection into Moving Treatments at Waves 2 and 3 (Columns 3–6): PHDCN (N = 1,645)

	Descriptives		Model Results Outcome			
			Move Within Chicago		Move Outside Chicago	
	Mean	SD	Wave 2	Wave 3	Wave 2	Wave 3
Time-invariant and time-varying covariates measured at baseline						
Baseline outcomes						
Violent behavior	–2.14	1.45	1.06	.99	.93	1.10
Exposure to violence	.16	.36	1.06	.96	1.47	.71
Baseline neighborhood characteristics						
Percentage Black, 1990	.33	.38	2.13*	1.03	.38	1.55
Change in percentage Black 1990–1995	.02	.05	.27	.20	19.06	.06
Percentage Latino, 1990	.33	.31	.70	.52	.70	.61
Change in percentage Latino, 1990–1995	.03	.07	1.01	3.19	13.31	2.31
Log median income, 1990	10.44	.41	.72	.46**	.91	1.00
Change in log median income, 1990–1995	.04	.13	.37	.22	.21	2.41
Caregiver race/ethnicity (White/other is reference)						
African-American	.36	.48	1.03	1.36	1.18	.69
Hispanic/Latino	.47	.50	1.87**	2.11**	.59	.36**
Caregiver education (high-school diploma is reference)						
< High-school education	.45	.50	.94	.79	.85	1.48
Some college	.33	.47	1.41	1.48	.76	1.20
College degree	.09	.28	.86	3.91	.63	1.56

Income ($30–$40k is reference)						
Less than $10k	.23	.42	2.22**	1.83	.55	.24**
$10–$20k	.20	.40	1.40	1.53	.39**	.75
$20–$30k	.19	.39	1.67*	1.30	1.09	.45
$40–$50k	.09	.28	2.11**	.35*	1.07	1.66
More than $50k	.16	.36	.64	1.24	.91	.57
Caregiver marital status (single is reference)						
Married	.58	.49	.75	.81	.66	.39
Cohabits	.11	.31	.77	1.05	.52	.51
Immigrant status (Third+ generation is reference)						
First generation	.12	.32	1.22	.95	.82	4.46**
Second generation	.34	.47	1.16	.81	.68	2.06
Caregiver employed	.55	.50	.76	.93	.88	1.86
Caregiver's partner employed	.57	.49	1.16	.52*	1.41	3.78*
Occupational status	42.28	15.78	1.01	1.00	1.00	1.03**
Homeowner	.39	.49	.74	.97	1.01	1.07
Welfare receipt	.31	.46	1.26	.73	.89	.90
Household size	5.43	2.03	1.01	.84	.97	1.08
Caregiver is male	.10	.29	1.11	.94	.76	.87
Subject is male	.51	.50	1.02	.85	.94	1.29
Caregiver age	37.68	7.67	.99	.97**	.97	.94**
Subject age	10.65	1.53	1.04	1.03	.84**	1.13
Length of residence	6.06	6.67	.91***	.95***	.98	.98
U.S. citizen	.70	.46	1.61*	1.15	.59	1.49
English proficient (caregiver)	.59	.49	.85	1.07	1.73	.75
Family criminality	.49	.91	.82**	.90	1.04	1.12
Domestic violence	2.05	2.66	1.03	1.00	.99	1.01
Social support	2.60	.30	1.01	.72	.84	1.09
Caregiver depression	.09	.28	.88	1.01	.42	.67

Peer delinquency	1.38	.28	.82	1.29	.86	1.44
Time-varying covariates measured at time 2						
Prior treatment status						
Moved within Chicago	.18	.38		2.30***		.94
Moved outside Chicago	.07	.26		.55		n/a
Outcomes						
Violent behavior	–2.59	1.67		1.02		1.07
Exposure to violence	.31	.46		.81		.70
Victimization	.25	.43		1.25		2.23**
Income ($30–$40k is reference)						
Less than $10k	.17	.38		.81		.87
$10–$20k	.20	.40		.87		1.31
$20–$30k	.19	.39		1.49		1.00
$40–$50k	.10	.30		1.47		.46
More than $50k	.22	.41		1.51		1.12
Caregiver marital status (single is reference)						
Married	.60	.49		.83		.65
Cohabits	.09	.29		.97		.75
Caregiver employed	.65	.48		1.11		.66
Caregiver's partner employed	.59	.49		1.46		.63
Occupational status	49.72	23.85		.96		.97
Homeowner	.42	.49		.98		2.62**
Welfare receipt	.21	.41		1.08		.97
Household size	5.42	1.95		1.36		1.01
Caregiver depression	.22	.41		.71		.83
Peer delinquency	1.39	.32		1.49		.22**

NOTES: Figures represent odds ratios. Standard errors are not shown.
ABBREVIATION: n/a = not applicable.
*p < .05; **p <.01.

ANALYTIC MODELS

We are interested in the effect of two qualitatively distinct conditions—moving to a new neighborhood within Chicago versus outside Chicago—on three separate measures of violence in adolescents' lives. To aid our investigation, we draw from the language of randomized experiments and counterfactual approaches to conceptualize causality in terms of the effect of a definable and usually qualitatively distinct (or dichotomous) treatment on some outcome. Accordingly, we divide the sample population into a treatment group (e.g., families that moved outside Chicago) and a control group (families that did not move outside Chicago). Counterfactual methods force clarity in causal questions by taking a "potential outcomes" approach. Specifically, each individual has *two* potential outcomes, with the first that which an individual i demonstrates under the treatment condition, which we will call Y_{it}. The second is the outcome that the individual demonstrates under the control condition, which we will call Y_{ic}. For each individual, however, only one of these outcomes is observed; questions of causality thus can be cast as a "missing data problem," one that is solved in experimentation through randomization. Assuming that equivalence of controls and treatments permits the estimation of the average causal effect, $\bar{Y}_t - \bar{Y}_c$.

When dealing with a treatment at one point in time and observational data, propensity score matching often is used (Rosenbaum and Rubin, 1983). With this technique, one can model the propensity that each individual receives the treatment and then create two groups by matching those who did or did not receive the treatment on this propensity score. This strategy has been shown to yield consistent and unbiased estimates of causal effects as long as all potential confounding factors included in the model are used to create the propensity score. But propensity score matching as a method was not designed for dealing with time-varying treatments and outcomes. When later treatments are endogenous to intermediate outcomes of prior treatments, both linear adjustments and propensity score matching can produce biased estimates.

To address this problem, we employ inverse probability-of-treatment weighting (IPTW) methods for longitudinal data (Robins, 1986, 1999; Robins, Hernan, and Brumback, 2000). IPTW is motivated by a general problem that emerges in any scenario in which time-dependent covariates predict both the outcome of interest and the subsequent exposure to the treatment, and past exposure to the treatment predicts the time-varying confounder. Consider the example of one time-varying treatment (in this case, any residential move as the treatment) occurring either between baseline (time 1) and time 2 or between time 2 and time 3, one outcome at

time 3 (violence), and one time-varying confounder (employment) measured at baseline and at time 2. Assume we want to identify the causal effect of moving between time 2 and time 3 on the outcome at time 3. If we do not control for caregiver employment at time 2 using either traditional regression or propensity scoring, then we will bias our treatment effect estimate because a caregiver's employment status is likely to predict both whether s/he decides to move and also might impact his or her child's behavior. Yet we also have a problem if we control for employment at time 2 because employment status might be influenced by the earlier decision to move or to not move. Hence, the time 3 treatment is potentially endogenous to outcomes of prior treatments, and typical panel models that simply control for time-varying covariates might be biased.

Robins and colleagues (Hernán, Brumback, and Robins, 2000; Robins, Hernán, and Brumback, 2000) showed that bias and the inducement of artificial correlations between treatment and outcome can be addressed by fitting a model that weights each subject by a weight consisting of the inverse of the predicted probability that the subject received the treatment *that they actually received* at a given time point conditional on prior treatment history, time-varying covariate history, and baseline (time-invariant) covariates. One way to think about this issue is that the IPTW approach essentially borrows less information from subjects who are highly likely to be in a given treatment status and who are found in that treatment status; these subjects are "down-weighted." Subjects with a low probability of being observed in a given treatment status, and who are found in that treatment status, are "up-weighted" so that we are borrowing more information from this group (Sampson, Sharkey, and Raudenbush, 2008; Wimer, Sampson, and Laub, 2008).

We generate IPTWs for the current analysis by specifying a model that predicts the probability of receiving the treatment at time points 2 and 3, with the baseline (time 1) weight set at "1". We generate "stabilized" versions of the IPTWs to avoid weights with extremely high values (Robins, Hernán, and Brumback, 2000). Specifically, the stabilized baseline, time 2 weights (w_2), and time 3 weights (w_3) are represented in the following set of equations denoted as equation 1:

$$
\begin{aligned}
w_1 &= 1 \\
w_2(z_2) &= \frac{P(Z_2 = z_2)}{P(Z_2 = z_2 \mid X_1 = x_1, Y_1 = y_1)} \\
w_3(z_2, z_3) &= w_2(z_2) \times \frac{P(Z_3 = z_3 \mid Z_2 = z_2)}{P(Z_3 = z_3 \mid Z_2 = z_2, X_1 = x_1, Y_1 = y_1, X_2 = x_2, Y_2 = y_2)}
\end{aligned}
\tag{1}
$$

where Z_2 is an indicator for time 2 treatment status (e.g., equal to 1 if the subject moved outside Chicago before time 2); Z_3 is an indicator for time 3 treatment; X_1 is a set of baseline covariates, including the outcome at baseline, all fixed covariates, and all time-varying covariates measured at baseline; and X_2 is a set of time-varying covariates measured at time 2, including the measure of violence at time 2. In other words, the numerator at time 2 is simply the probability of receiving the treatment actually received, whereas the denominator is the same probability conditional on baseline covariates. Predicted probabilities are generated after estimating a logit model in which the dependent variable is an indicator of whether the subject received the treatment at time 2. At time 3, the numerator is the probability of receiving the treatment actually received, conditional on time 2 treatment status, and the denominator is the same probability conditional on time 2 treatment status, time 2 outcome, time-varying covariates measured at time 2, and all baseline covariates. The time 3 numerator and denominator then are multiplied by the time 2 numerator and denominator to generate the IPTWs at time 3.[5] Note that all time 1 (or baseline) measures are used to model selection into later treatments; a time 1 causal effect is not estimated.

In sum, rather than creating potential biases by including endogenous confounders as control variables or when creating a propensity score, IPTW methods weight each person-period by the inverse of the predicted probability of receiving the treatment status that they actually received in that period based on measured covariates. Analogous to survey weights, IPTW models create a "pseudo-population" of weighted replicates, allowing one to compare times when one does and does not experience a "treatment" without making distributional assumptions about counterfactuals. IPTW models thus provide a substantively motivated strategy to deal with potentially complex parametric causal pathways among time-varying treatments, time-varying covariates, and time-varying responses (Ko, Hogan, and Mayer, 2003; Wimer, Sampson, and Laub, 2008). However, this method still is based on observation and relies on measuring selection into treatments at each wave. Unmeasured covariates that predict treatment assignment after controlling the observed covariates still can introduce bias (Morgan and Winship, 2007). To address this concern, we present additional analyses using an instrumental variable (IV) approach. This strategy has its own limitations as described subsequently, but presenting evidence using an entirely distinct approach provides more

5. We created additional weights representing the inverse probability of attrition (results available upon request). These weights were multiplied by the IPTWs to create the final weights used in the analysis, thereby adjusting for nonrandom attrition.

confidence in the interpretive claims made from our findings than either approach on its own.

MEASURING VIOLENCE

We integrate the IPTW strategy with a cross-classified model to estimate trajectories of change in three forms of violence in the lives of adolescents during the course of the PHDCN study and across neighborhoods. The cross-classified model is necessary because of the complex nature of clustering in the data. At level 1 of the data, we have individual time points that represent the multiple survey points at which subjects were interviewed. Time points are nested within subjects; however, time points are also nested within neighborhoods, which change as subjects move during the course of the survey. Extending Sampson, Sharkey, and Raudenbush (2008: 847), we thus specify a cross-classified model with both subject-level random effects *and* neighborhood-level random effects as follows:

$$Y_{ti} = \gamma_1 + u_{1i} + \sum_{s=1}^{t}\sum_{j=1}^{J} D_{sij} v_j + \varepsilon_{ti} + I(t=2)(\gamma_2 + Z_{2i}\delta_2) + I(t=3)(\gamma_3 + Z_{3i}\delta_3) \quad (2)$$

Equation 2 might be regarded as a growth trajectory for each adolescent except that the trajectory is "deflected" by assignments to treatments and neighborhoods. Here, $I(t = 2)$ is an indicator taking on a value of unity at time $t = 2$ and a value of 0 at other times. Similarly, $I(t = 3)$ is an indicator taking on a value of unity at time $t = 3$ and a value of 0 at other times. The intercept of this trajectory has a fixed effect γ_1. The random effect u_{1i} is the adolescent-specific increment to the intercept. The average increase in the outcome between times 1 and 2 for an adolescent who does not experience the treatment at time 2 is γ_2. The average increase in the outcome between times 1 and 3 for an adolescent who does not experience the treatment at time 3 is γ_3. The predictor D_{sij} takes on a value of unity if adolescent i lives in neighborhood j at time s. Hence, unlike most previous research, even with panel data, our model allows neighborhood effects v_j, $j = 1, \ldots, J$ to cumulate throughout time (Raudenbush and Bryk, 2002: Chapter 12, example 2). Treatment effects come into the model at appropriate times through the definition of $I(t = 2)$, $I(t = 3)$. We assume the within-subject random effect is independent and normally distributed, $\varepsilon_{ti} \sim N(0,\sigma^2)$. We make the same assumptions for the neighborhood random effect ($v_j \sim N(0,\Psi^2)$) and the person-specific effects ($u_{1i} \sim N(0,\tau^2)$).[6]

6. We tested for lagged effects of moving within and outside the city at time 2 on

The specification in equation 2 allows for unique effects of mobility at each time point; however, the effect of residential mobility offers no theoretical reason to think that it should change from one interview wave to the next, controlling for time trends. Initial results from the specification in equation 2 support this notion, yielding estimated effects that were extremely similar across time points. To increase the precision of our estimates, we therefore report results for the effects of mobility within and outside Chicago pooled across time points. Although the results are essentially the same using the pooled versus the wave-specific estimates, we report the pooled results because they are simultaneously more precise and theoretically parsimonious.

An estimation of equation 2 would provide unbiased causal inferences if adolescents were assigned randomly to move within or outside Chicago at a given wave, but in an observational study, this pattern is not the case. However, if we assume sequentially ignorable treatment assignment and apply the results of Robins, Hernán, and Brumback (2000) and Hong and Raudenbush (2008), we can obtain consistent estimates of causal effects by applying the IPTW defined in equation set 1. We thus follow this procedure and then assess its robustness in an alternative instrumental variables strategy that makes very different assumptions. As noted, no one method—observational or experimental—is without limitations, which leads us to emphasize a triangulation of methods along with descriptive results and substantive theory.[7]

outcomes at time 3, but they were not significant, and all coefficients for the main treatment effects were unchanged. For the moving within Chicago treatment, we also tested for cumulative effects of moving at both time points, but it was not significant. Our definition of the moving outside Chicago treatment precludes the estimation of cumulative impacts, as it is impossible to move from Chicago to outside Chicago at each time point. Because lagged and interaction effects were trivial and materially did not change the pattern of results, we present the results for the simpler and more parsimonious model defined in equation 2.

7. It often is overlooked that experiments must make assumptions too, and in the neighborhood effects literature, these assumptions are sometimes just as heroic as those made in observational approaches (Sampson, 2008). To place undue burdens and expectations on a particular method or kind of data is therefore mistaken, in our view. So too is the common approach of conceptualizing selection bias as a purely individual-level property ("choice") best subjected to experimental randomization. This interpretation is profoundly misleading not only of the social world (Sampson and Sharkey, 2008) but also of the nature of human decision making (Heckman, 2005).

RESULTS

We begin by specifying a model for selection into the treatment status of mobility. We are aided in this effort by prior research on residential mobility reviewed previously, combined with research using the PHDCN data (Sampson and Sharkey, 2008; Sampson, Sharkey, and Raudenbush, 2008), which we draw on to specify a model of mobility within Chicago and of mobility outside Chicago, respectively. We construct time-varying IPTWs by modeling selection into the treatment as a function of all prior covariates, prior treatment history, and prior status on the outcome of interest. Specifically, time 2 treatment status is predicted by the full set of fixed and time-varying covariates measured at baseline, as well as baseline treatment status and the three measures of violence; time 3 treatment status is predicted by the full set of fixed covariates measured at baseline, time-varying covariates measured at baseline and at time 2, and treatment status and violence measures at both baseline and at time 2. The model results are shown in table 2.

Few consistent predictors are available of mobility within or outside Chicago at each time point, suggesting that confounding for residential mobility of this type is less severe than commonly assumed. Latinos are more likely to move within the city at time 2 and time 3 and are less likely to exit the city at time 3. Families that have lived at their baseline residence for long periods of time are less likely to move within the city at time 2 and time 3. Families that move within Chicago at time 2 are more likely to move within the city again at time 3, suggesting that a segment of families might be in relatively unstable residential circumstances and making frequent moves within Chicago. Otherwise, few covariates have strong influences on mobility across specifications. We should note, however, that the goal of these models is not to identify the independent or "direct" effects of each variable but to generate a complete model with as many observed predictors of treatment status as possible.

Hong (2007) showed that IPTW can produce biased inferences when treated and untreated cases are not similar, or "balanced," on covariates within propensity strata. To check that treatment and control group members are balanced on their propensity score, we used the results from the logit models predicting mobility within and outside Chicago at each wave to create propensity scores and then split the sample into equally sized deciles based on the estimated propensity score. Within each stratum and at each wave, we examined balance on the average propensity score, the standard deviation (SD) of the propensity score, and the predicted logit. Treatment and control group members were well balanced across deciles for both treatments and at all waves. Mean propensity scores were virtually identical within strata as were the SDs of the propensity score and the

predicted logits, suggesting that the selection models performed well for each treatment. Detailed results on the analysis of balance for each treatment at each wave are available from the authors upon request.

In table 3, we turn to the major results from the cross-classified IPTW models, beginning with the effect of moving within Chicago on trajectories of violent offending, exposure to serious violence, and violent victimization among 9- and 12-year-olds. The first two rows in table 3 describe the average trajectories of change in all three dimensions of violent environments. Violent behavior is measured at all three interview waves, allowing us to estimate growth trajectories across the full course of the PHDCN study. The estimated parameters show that individual violence declines, on average, from wave 1 to wave 3 of the study. Because the measures of exposure to serious violence and violent victimization are available only at waves 2 and 3 of the study, we can estimate change only from wave 2 to wave 3. The second and third columns of table 3 show that exposure to violence rises slightly, and no change is observed in victimization patterns for nonmovers.[8] The third row of the table displays the deflection from these average trajectories attributable to moving within Chicago at either wave. We find a strong and consistent pattern across the three outcomes; moving within Chicago leads to elevated levels of violent activity, exposure to violence, and victimization. Moving within the city leads to an approximately .13 SD increase in the measure of violent behavior and multiplies the odds of being exposed to violence by 1.56 and the odds of being victimized by 1.45.[9]

These results assume that the effects of moving are the same at each wave to improve the precision of the estimates. If we relax this assumption and allow for unique effects of mobility at each wave, then we continue to find that moving to a new neighborhood within Chicago increases violent behavior and exposure to violence by roughly the same magnitude at wave 2 and wave 3. The effects of mobility on victimization also are positive at each wave, but the effect of moving at wave 2 is not significant. Overall, our estimates suggest that when adolescents move out of their neighborhoods but remain within the social structure of Chicago, they are more likely to find themselves in violent social environments, to be victimized, and to be violent themselves.

8. This pattern is similar to change in violent behavior *from wave 2 to wave 3.* If we had measured change in violent behavior from wave 2 to wave 3, our estimated trajectories of change also would have shown a slight rise in violence during this time period.
9. Our discussion of odds ratios is included to make the results more interpretable, although odds ratios are biased slightly away from 1 in models of exposure to violence and victimization (Zhang and Yu, 1998). We therefore report the logit coefficients in tables 3 and 4.

Table 3. Estimated Effects of Moving Within Chicago: PHDCN Cohorts 9 and 12

	Treatment: Moved Within Chicago		
	Violent Behavior (n = 1,500) Coefficient (SE)	Exposure to Violence[a] (n = 1,350) Coefficient (SE)	Violent Victimization[a] (n = 1,350) Coefficient (SE)
Intercept	–2.21***	–.87***	–1.21***
	(.05)	(.08)	(.08)
Time trend[b]	–.18***	.22**	.10
	(.03)	(.11)	(.11)
Moved within Chicago[c]	.19**	.45***	.37**
	(.10)	(.16)	(.17)

NOTES: Data are IPT weighted and cross-classified with time points nested within subjects and time-varying neighborhoods.

[a]Coefficients in models of exposure to violence and victimization are from logistic regressions.

[b]For violent behavior, "time trend" is a linear trend of change from wave 1 to wave 3. For other outcomes, this term shows the change between waves 2 and 3, as these outcomes were not available at wave 1.

[c]Coefficient represents the effect of moving within Chicago relative to not moving within Chicago.

*$p < .10$; **$p < .05$; ***$p < .01$.

What about adolescents who leave Chicago? Table 4 shows the estimated effects of moving outside Chicago when analyzed as a distinct treatment. The trajectories of change for each outcome are similar to those found in table 3, showing declining levels of violent behavior across the three waves of the survey and rising levels of exposure to violence from wave 2 to wave 3. Although the effects of mobility outside Chicago are estimated with less precision, we do find that exiting Chicago has a strong and significant negative effect on violent behavior and exposure to violence. Moving outside Chicago reduces violent behavior by more than a third of a SD and reduces by half the odds of being exposed to violence. The effect on violent victimization is not significant. If we allow for unique effects of mobility outside Chicago at each wave, then we continue to find the same pattern of negative effects for violent behavior and exposure to violence, although the estimates are more imprecise and are not all significant. Although these results are not as consistent as the estimated effects of moving within the city, they do indicate that the two treatments under study—moving within Chicago and moving outside Chicago—are distinct and produce very different consequences for adolescent violence.

Table 4. Estimated Effects of Moving Outside Chicago: PHDCN Cohorts 9 and 12

	Treatment: Moved Outside Chicago		
	Violent Behavior (n = 1,500) Coefficient (SE)	Exposure to Violence[a] (n = 1,350) Coefficient (SE)	Violent Victimization[a] (n = 1,350) Coefficient (SE)
Intercept	–2.21***	–.73***	–1.09***
	(.04)	(.07)	(.07)
Time trend[b]	–.15***	.21**	.03
	(.03)	(.10)	(.11)
Moved outside Chicago[c]	–.51**	–.82**	.39
	(.21)	(.42)	(.35)

NOTES: Data are IPT weighted and cross-classified with time points nested within subjects and time-varying neighborhoods.
[a]Coefficients in models of exposure to violence and victimization are from logistic regressions.
[b]For violent behavior, "time trend" is a linear trend of change from wave 1 to wave 3. For other outcomes, this term shows the change between waves 2 and 3, as these outcomes were not available at wave 1.
[c]Coefficient represents the effect of moving outside Chicago relative to not moving outside Chicago.
*$p < .10$; **$p < .05$; ***$p < .01$.

FURTHER CHECKS AND ROBUSTNESS

Considering the differences in results found among boys versus girls in Gautreaux and MTO, we tested for gender interactions in all specifications. We found a marginally significant interaction in our estimate of moving within the city and violent behavior, with stronger positive effects of moving on violent behavior among males. In all other specifications, no significant or substantively meaningful gender interactions were observed. We conclude that, overall, no consistent or substantively important evidence exists of interactions of moving with gender.

As a falsification test, we estimated an additional set of analyses to test whether moving within or outside the city at a given wave had an association with outcomes related to violence measured in the *previous* survey wave or before the move occurred. If such an association existed where it should not, then it would suggest that our results are biased and might reflect unmeasured characteristics of adolescents that predict mobility as well as the youth's experiences with violence. Estimates from the same

specifications shown in tables 3 and 4 but using lagged outcomes measured before moves occurred showed no effects of mobility within or outside Chicago. All such estimates hovered around zero, and none were close to achieving statistical significance, providing support for the results reported in tables 3 and 4.[10]

Finally, to strengthen the evidence presented on the relationship between mobility and violence, we conducted an additional set of analyses using an IV approach. The details and results are included in appendix A. The first instrument is the presence of a grandparent who lives within the city, which is associated negatively with mobility out of Chicago and is associated positively with mobility within the city. The second instrument is residence near the border of Chicago, which is associated positively with mobility out of the city and is associated negatively with mobility within the city. We argue that neither of these factors has a direct influence on adolescent violence conditional on observed covariates. The results from the IV analyses are largely consistent with the pattern of findings from the IPTW models. In analyses that include a full set of control variables, we find that mobility within Chicago is associated positively with violence and that mobility outside the city is associated negatively with violence. Somewhat surprisingly, the pattern of results is virtually identical whether using the presence of a grandparent or residence on the border as the instrument for mobility. Because the IV method relies on very different assumptions than the IPTW approach, the convergence in results enhances our confidence in the underlying pattern that moving within and outside Chicago have opposite impacts on adolescent violence.

ELABORATING MECHANISMS OF MEDIATION

Like experiments in general, our counterfactual approach estimates the effect of a specific treatment, but it is not well suited to explicating the mechanisms underlying the relationship between a treatment and an outcome. As a supplement to the main analysis, we therefore provide theoretically motivated evidence on why we see such divergence among adolescents who move within or outside Chicago. Such exploratory analysis, combined with the formal counterfactual estimates, provides additional insight into causal processes (Morgan and Winship, 2007).

To generate evidence on the mechanisms connecting residential mobility to violence, we estimate the same cross-classified models for each measure of violence; however, we depart from the IPTW approach and weight the data only with a time-varying measure representing the inverse probability of attrition rather than a weight representing the product of

10. We thank the anonymous reviewers for suggesting the falsification and gender tests.

the inverse probability of attrition and the inverse probability of receiving the treatment. In each model, we include dichotomous indicators for any residential move at either wave as well as for a move outside Chicago. These indicators vary throughout time, so that a move between wave 1 and wave 2 enters the model at wave 2, and a move between wave 2 and wave 3 enters the model at wave 3. This specification allows for a direct comparison of the effect of moving within Chicago relative to staying in the same neighborhood and the effect of moving outside Chicago relative to moving within the city. In this way, the specification provides evidence on the fundamental contrasts that motivate the inquiry. We also include the set of wave 1 predictors that were used in the construction of the IPTWs described earlier; these variables are used as control variables in the current models. This analysis does not consider time-varying treatments and confounders, as in the earlier IPTW framework, and thus, the results are not appropriate for the types of causal claims we have made based on the results from tables 3 and 4. However, the analysis is better suited for providing suggestive evidence on the mechanisms driving our earlier results.

The first specification for each outcome includes indicators for any residential mobility and mobility outside the city along with the set of control variables. This specification establishes the relative association between moving within and outside the city on each dimension of violence. In the second specification, we include measures for a group of neighborhood-level characteristics that might represent mediating mechanisms. We consider the percentage of Black and Latino residents and the poverty rate in the adolescents' destination neighborhoods. One central critique of the MTO program is that the treatment under study was weak (Clampet-Lundquist and Massey, 2008); movers in the treatment group ended up in neighborhoods that had lower poverty rates but otherwise were similar to those they left behind in qualitative terms, most notably in terms of racial composition and violent crime (Sampson, 2008). Previous research using the PHDCN also found that the most substantial changes in families' neighborhood economic composition occur when families exit the rigid segregation found within the neighborhoods of Chicago (Sampson and Sharkey, 2008). These within-family changes in neighborhood racial and economic composition are not attributable to time-varying or stable characteristics of the family, leading to the hypothesis that one reason why moves within and outside Chicago might lead to such divergent outcomes is simply the very different residential environments found within Chicago as compared with the city's suburbs or even with other U.S. cities. The second specification directly tests this hypothesis.

In a third specification, we include measures capturing additional aspects of an adolescent's environment and his/her perspective on that environment. First, we include a subject-reported measure of the school

environment, which is based on a series of items asking the subject about the prevalence of fights in the school; alcohol, cigarette, and drug usage; students' engagement with classes and with academics in general; teachers' control over classes; and racial/ethnic conflict in the school. We also include a measure of the adolescent's own engagement in school, which is the mean of a series of items asking about how well the adolescent likes school and his/her teachers and how important grades and homework are. In the MTO experiment, youth in the treatment group experienced virtually no change in their school environments, despite the reductions in neighborhood poverty that accompanied their residential moves. Part of the reason this finding might be true is that youth in families that used vouchers typically remained in schools within the Chicago public school system, much of which, at the time of this study, was plagued by poorly performing schools where violence was a constant problem (Clampet-Lundquist et al., 2006). The quality of children's schooling was often a secondary concern for parents in the MTO treatment groups, who frequently mentioned more pressing financial or legal concerns.

In addition to the two measures of adolescents' perceptions of and engagement in school, we include two additional measures capturing perceptions of violence in the environment. The first is a measure of the adolescent's *street efficacy*, defined as his/her perceived ability to avoid violent confrontations while engaging in public life within the neighborhood (Sharkey, 2006). The second is a measure of the adolescent's self-reported fear of violence in his/her neighborhood and school. Previous research demonstrates that adolescents' confidence in their ability to engage in public life within the neighborhood while avoiding violence is an important predictor of the type of environment they construct for themselves and is associated strongly with subsequent violent behavior and peer delinquency. According to this perspective, social cognition acts as one mechanism by which aspects of the neighborhood environment influence the types of environments that youth carve out from what is available to them. This finding is broadly consistent with research from the Gautreaux program, which argued that one primary reason why adults who moved to the suburbs experienced success in the labor market was because of elevated levels of self-efficacy brought about by their residential move (Rosenbaum, Reynolds, and DeLuca, 2002). We thus include measures of street efficacy and fear of violence to test whether social cognition and personal responses to the potential for violence help to explain the divergent outcomes among movers within and outside Chicago.[11] We recognize that an

11. It would be desirable to examine neighborhood social process measures such as collective efficacy, disorder, and friend/kinship ties (Sampson, Morenoff, and Earls, 1999), but these data are unavailable outside Chicago. The purpose of this

adolescent's perceptions of violence in the school or neighborhood might be a result of the adolescent's involvement in violent activities. For example, it could be that students' involvement in violent activities leads them to perceive a more violent school environment. Because of this possibility, the results presented in this section should not be given strong causal interpretation but should be thought of as exploratory analyses of the possible mechanisms that explain the findings presented earlier.

The first three columns of results in table 5 display estimated coefficients from models of violent behavior. The first specification for violence contrasts the association between moving within Chicago and moving outside Chicago (model 1). The coefficient for the indicator labeled "moved between waves" can be interpreted as the effect on violence of moving within the city at the given wave *as compared with remaining in the original neighborhood*. Note that this comparison is different from the comparison made in table 3 in which all adolescents were a part of the control group (including those who moved outside Chicago). The coefficient for the indicator labeled "moved outside Chicago" can be interpreted as the effect on violence of moving outside Chicago at the given wave *as compared with moving within the city*. Again, this comparison is different from that made in table 4 in which movers out of the city were compared with the rest of the sample, including those who moved within Chicago. Our specification thus allows for a comparison of the two "treatments," moving within and outside Chicago, in a single model.

Despite the different specifications, we again find that moving within the city is associated with increases in violent behavior, whereas moving outside Chicago is associated with reductions in violence. Note that the coefficient for mobility out of the city is larger in absolute terms than the coefficient for mobility within the city; this result suggests that moving outside Chicago leads to positive effects over and above the negative consequences of moving within Chicago, which is consistent with table 4. In model 2 we find that the gap in violence between movers within and outside Chicago does not seem to be attributable to the composition of the neighborhoods into which the two groups of movers enter. By contrast, results from model 3 show that perceptions of the school environment and perceptions of violence in the adolescent's new environment have a strong association with individual violent behavior. Adolescents who perceive fewer problems in their school and who are engaged with school show less violent behavior. Adolescents with high levels of street efficacy are much less likely to be violent, and those who fear violence in their environment are also less likely to be violent themselves. Furthermore, including these

subanalysis is to assess the social mechanisms that account for the city/noncity divide.

Table 5. Cross-classified Regression Models of Mechanisms Linking Mobility with Violence: PHDCN Cohorts 9 and 12

	Violent Behavior			Exposure to Violence			Violent Victimization		
	Model 1 Coeff (SE)	Model 2 Coeff (SE)	Model 3 Coeff (SE)	Model 1 Coeff (SE)	Model 2 Coeff (SE)	Model 3 Coeff (SE)	Model 1 Coeff (SE)	Model 2 Coeff (SE)	Model 3 Coeff (SE)
Moved between waves	.20**	.20**	.12	.28**	.34***	.25*	.42***	.42***	.35***
	(.09)	(.09)	(.09)	(.13)	(.13)	(.12)	(.13)	(.13)	(.13)
Moved outside Chicago	−.37**	−.35**	−.28*	−.80***	−.65**	−.61**	−.12	−.11	−.02
	(.16)	(.16)	(.16)	(.27)	(.27)	(.28)	(.25)	(.26)	(.26)
Neighborhood % Black		.07	.05		.38*	.33		.18	.11
		(.15)	(.15)		(.22)	(.23)		(.23)	(.24)
Neighborhood % Latino		−.09	−.12		.23	.16		.13	.07
		(.16)	(.16)		(.24)	(.24)		(.25)	(.25)
Neighborhood % poor		.21	.13		1.86***	1.77***		−.12	−.39
		(.37)	(.36)		(.51)	(.52)		(.53)	(.54)
Perception of school environment[a]			−.52***			−.80***			−.56***
			(.07)			(.11)			(.12)
School engagement[b]			−.44***			−.20*			−.38***
			(.08)			(.12)			(.12)
Street efficacy[c]			−.32***			−.38***			−.38***
			(.06)			(.09)			(.10)
Fear of violence[d]			−.24***			.04			.23**
			(.07)			(.10)			(.11)

NOTES: Data are IPT weighted and cross-classified with time points nested within subjects and time-varying neighborhoods.

[a]School environment: Child-reported measure of school atmosphere, student engagement, drug/alcohol problems, violence and racial tension. Higher score means school has higher quality environment (i.e., fewer problems).

[b]School engagement: Child-reported measure of enjoyment and effort at school. Higher score means greater engagement in school.

[c]Street efficacy: Child-reported measure of confidence in own ability to find ways to avoid violence. Higher score means greater confidence.

[d]Fear of violence: Child-reported measure of how afraid s/he is in the neighborhood and school environment. Higher score means greater level of fear.

*$p < .10$; **$p < .05$; ***$p < .01$.

measures sharply reduces the association between residential mobility within the city and violence. Moving within Chicago has a small, nonsignificant association with violence in model 3, which suggests that adolescents' experiences in school and their perceptions of violence in their environment account for much of the positive association between mobility within Chicago and violent behavior. In the full specification, moving outside Chicago is associated with a reduction in violent behavior relative to moving within the city, but the difference in violent behavior by destination is only marginally significant.

Slightly different patterns describe the models of exposure to violence and victimization. A strong positive association is found between moving within the city and exposure to violence, and a significant contrast is found between moving within the city versus moving outside the city (model 1). Unlike violent behavior, both the percentage of Black residents and the poverty rate in adolescents' destination neighborhoods are associated positively with exposure to violence and account for a small portion of the relationship between geographic destination and exposure to violence (model 2). When we include the measures of the school environment and perceptions of violence in model 3, we find that adolescents' engagement in school, their school environment, and their own street efficacy are associated strongly with exposure to violence, whereas fear of violence is unrelated to exposure. When all these measures are included, the association between moving within the city and violence exposure is only marginally significant; moving within the city multiplies the odds of exposure to violence by 1.29 relative to not moving. It is interesting that a negative association remains between moving outside Chicago and exposure to violence that is not explained by destination neighborhood characteristics, the school environment, or street efficacy. Relative to moving within the city, moving outside Chicago reduces the odds of exposure to violence by half in the full specification.

The last set of results in table 5 reveals that the strongest contrast in violent victimization exists between movers within the city and nonmovers (model 1). Similar to previous analyses of victimization, we find no effect of moving outside Chicago as compared with moving within the city. The economic and racial composition of destination neighborhoods has no influence on victimization and does not explain any association between moving within the city and victimization (model 2). Yet the school context and the adolescent's perceptions of violence are associated strongly with victimization, partially explaining the role of mobility. In the full specification, moving within the city multiplies the odds of victimization by 1.42 relative to not moving. One interesting result in this final specification is that although street efficacy is associated negatively with violent victimization, adolescents who report higher levels of fear of violence are more

likely to report being victimized. This finding runs counter to that found in models of violent behavior, which showed that fear of violence is associated negatively with individual violent behavior. Although most predictors of violence and violent victimization run in the same direction, fear of violence seems to be one of the few domains in the lives of adolescents that operate differently.

Overall, these results do not provide a simple answer to the question of why we see such divergence in violence among adolescents depending on their geographic destination. Although our prior work suggested substantial differences between the neighborhood environments of movers within and outside the city might help explain the diverging trends among adolescents, neighborhood racial and economic composition are associated only with exposure to violence and have no relationship to violent behavior or violent victimization. Adolescents' perceptions of their school environments and their perceptions of violence play a more prominent role in mediating the relationship between residential mobility and violence. Although it is difficult to make strong conclusions about the causal ordering of these relationships, the findings are consistent with the idea that one major distinction between living within the city limits and outside the city relates to the administrative boundaries of key institutions in an adolescent's life, such as the school system. Moving to a new neighborhood within Chicago means that youth might experience a slightly more diverse group of neighbors and more economic opportunities, but they will continue to attend school in similar environments and face the same threat of violence. Our results suggest that escaping the institutional boundaries of Chicago, independent of any change in the residential environment, helps to explain why the destination of a residential move plays such a central role in adolescents' experiences with violence. It is not just about internal neighborhood effects, in other words, but the broader structure within which neighborhoods are embedded.

IMPLICATIONS

The impact of moving to a new neighborhood cannot be captured solely by examining a change in any single characteristic such as the poverty rate. Residential mobility, especially among adolescents, entails disruption to the social relationships formed in their neighborhood of origin and a forced introduction to a new social structure at destination. This disruption might be either "good" or "bad" depending on the outcome in question and the larger structural context of the move. For example, a move might mean a loss of social capital available to youth through the disruption of

intergenerational networks that serve to facilitate monitoring and supervision (Coleman, 1988; Hagan, MacMillan, and Wheaton, 1996) and the collective efficacy available for children (Sampson, Morenoff, and Earls, 1999). But disruption also can mean breaking away from a disadvantaged and violent environment, which may be beneficial if the new neighborhood is supportive and safe. This scenario seems to be what happened in the forced moves of some ex-offenders out of New Orleans after the destruction wrought by Hurricane Katrina (Kirk, 2009).

Our results support the idea that the process of moving itself plays an important role in shaping the trajectories of adolescent violence, apart from change in the economic or demographic characteristics of neighborhoods that result from a move. At the same time, however, we argue that a "contextually conditioned" perspective on the impact of residential mobility is necessary, one that avoids treating all moves equally. We found specifically that adolescents who move but remain within Chicago are more likely to exhibit violent behavior and to be exposed to violence, whereas those who exit the city show the opposite patterns. The pattern of divergent trajectories of movers within and outside Chicago is broadly consistent with the pattern of findings from the Gautreaux residential mobility program. Although the Gautreaux studies did not assess youth violence directly, the geographic location of families in the program was linked powerfully with educational, economic, and mortality outcomes. Youth in families assigned to apartments in suburban Chicago were more likely to be in school or employed and less likely to die when compared with their peers whose families were assigned an apartment within the city.

The salience of the city/suburban distinction in Gautreaux and in the present analysis leads to an intriguing question: How is it that the boundary separating Chicago from its suburbs seems to take on such importance in the lives of youth? We argue that to move toward an answer requires a conceptualization of Chicago as more than a spatially contiguous collection of neighborhoods. It is a highly stratified residential, political, and social structure as well. Seen in this way, the boundary distinguishing Chicago from its suburbs takes on added significance. Moving beyond this boundary means exiting the Chicago public school system, a system that faces some of the most severe challenges of any district in the nation. Leaving Chicago also means moving beyond the boundaries of the most intense gang activity of the city and the violence that structures social interactions in the most disadvantaged areas of Chicago's ghetto. Finally, leaving Chicago means exiting the rigid residential structure of segregation that characterizes its neighborhoods. Prior research on the sources of

neighborhood change among Chicago families demonstrates that substantial changes in the economic and racial composition of families' neighborhoods occur exclusively among families that exit the city (Sampson and Sharkey, 2008).

These perspectives on the city/suburb divide suggest several possible explanations for the divergent trajectories of adolescents who remain in the city and those who leave. The usual suspect is economic and racial segregation that exists within Chicago. But with the exception of exposure to violence, we found little support for the hypothesis that the divergence in violence trajectories can be explained by neighborhood characteristics of movers within and outside Chicago; controlling for the racial and economic composition of movers' destination neighborhoods did not explain any of the gap in violence or victimization between movers within and outside the city, although it did explain a small portion of the gap in exposure to violence. These results suggest that the importance of place is not encompassed solely in the economic or racial composition of neighborhood residents but also relates to the institutions that are organized along geographic boundaries (Briggs, 2005), such as schools. In other words, although neighbors certainly matter, administrative boundaries, institutions, and physical distance matter as well.

This conclusion provides a lens with which to view and interpret the mobility-related results from the Gautreaux program and the more recent MTO program. Although participants in Gautreaux were assigned apartments throughout the Chicago metropolitan area, members of the MTO treatment group selected their own apartments and typically moved to neighborhoods close in proximity to their origin neighborhoods. Data from MTO's Chicago site indicated that only a few families in the treatment group relocated outside the city limits (Sampson, 2008). And although the MTO treatment group families who moved within the city experienced declines in neighborhood poverty, the adolescents in these families nonetheless remained within the city's public school system, and they remained within the largely segregated, violent environment that exists within many of Chicago's neighborhoods (Clampet-Lundquist and Massey, 2008; Massey and Denton, 1993). It is telling that, in our sample, movers within the city experienced a similar slight improvement in neighborhood poverty; yet this improvement was overwhelmed by the negative effect of residential mobility within Chicago. If our results are any guide, then it is not surprising that youth in the treatment group in MTO, by and large, did not experience pronounced positive change in behavioral outcomes such as delinquency or risky behavior.

Considered in tandem with prior results from Gautreaux and MTO, the present study thus converges in suggesting a contextually conditioned and

theoretically supported set of relationships that distinguish among residential mobility, neighborhood change, and the structural geography of opportunity. The lesson is that multiple dimensions of a move must be considered simultaneously if one is to assess how mobility might impact an adolescent's developmental trajectory. In our case, destination matters—"getting out of town" or "knifing off" in the parlance of the life-course literature seems to have important consequences for reducing adolescent violence, a finding that is consistent with the quasi-experimental results from Gautreaux and with a more recent study of violence reduction sparked by the separation of ex-offenders from high-risk environments after Hurricane Katrina (Kirk, 2009). We view the combination of quasi-experimental research and observational studies, such as the present analysis, as crucial pieces of a larger scholarly effort to uncover the links between adolescents' social environments and their ongoing development.

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Appendix A

The goal of this instrumental variables analysis is to find an instrument that is unrelated to "potential outcomes" in the treatment and control state but that is associated with families' residential destinations (Morgan and Winship, 2007: Chapter 7). We use the following instruments: 1) the presence of grandparents who live within Chicago and 2) baseline residence in a census tract that borders the city limits.[12]

We argue that the presence of grandparents who live within Chicago is likely to influence whether a family decides to stay in the city, and yet it does not have a direct effect on adolescent violence through any other path. The first premise can be tested and is supported. Although 34 percent of the caregivers with a parent living in the city move within Chicago at some point during the course of the study, only 22 percent of caregivers *without* a parent in the city do so. Having a parent in Chicago also makes it less likely that caregivers will leave the city; among caregivers with a parent in Chicago, 11 percent move outside the city compared with 16 percent of caregivers without a parent in the city. One might challenge this instrument on the grounds that having a grandparent living within the city might impact directly an adolescent's experiences with violence. For instance, if the grandparent lives within the home, then he/she could serve as a direct source of social control over the adolescent. But previous research has investigated the role of extended kin living in the household and has found it has no relationship to youth violence (Sampson, Morenoff, and Raudenbush, 2005). We further address this concern by identifying and *excluding subjects who have a grandparent living in the same household*. Another challenge is that families with multiple generations of family members in Chicago might differ in various ways, observable and unobservable, from families that do not have members of the previous generation living in the city. To address observed heterogeneity, we estimate an additional specification, including a set of controls for basic demographics, neighborhood characteristics as of wave 1, and measures of family background.

12. A third instrument was suggested by a reviewer—proximity to a public transportation line that could link the family to a Chicago suburb with a nontrivial representation of residents from the same race/ethnic group. The idea is that families might be more willing to move to the suburbs if they feel they easily can access their origin neighborhood and thus maintain ties to friend and kin networks. We constructed such a measure but found that it was not correlated with mobility out of the city for most of the sample. A low correlation was found among Whites, but they were by far the smallest racial group in the study. The lack of a reasonable correlation between the IV and the treatment precluded us from using this additional instrumental variable.

The second IV is an indicator for living in a census tract that borders Chicago's city limits as of wave 1 of the survey. We expect residence in a border tract to be correlated with mobility outside or within the city simply because proximity to the city's border is likely to make moves outside the city limits more common because of familiarity with surrounding neighborhoods, schools, and housing markets; the ability to maintain ties with kin and friendship networks; and the lower costs associated with housing searches. In line with this assumption, we find that 20 percent of families in border tracts move outside Chicago at some point during the course of the study compared with 12 percent of families in nonborder tracts. Only 12 percent of families in border tracts move within the city compared with 31 percent of families in nonborder tracts. Again, one could argue that families in border tracts might differ from families in nonborder tracts in various ways, but we condition the estimates on the full set of controls, which include neighborhood characteristics. Of course, if any pathways are unobserved by which living in a border tract is directly associated with violence, then the assumptions for the IV estimator are violated.

We conduct a two-stage least-squares analysis using each of these measures as separate instruments for whether the family moved within or outside the city during the course of the study. One drawback of IV analysis is that it can produce imprecise estimates with large standard errors (Morgan and Winship, 2007). This issue is the case for our analysis, so we focus on the direction of the effects as opposed to on the magnitude of the effects. The first row in table A1a shows results estimating the effect of moving within the city at any wave on all three outcomes, using the presence of a grandparent within the city as an instrument for moving within the city. For this analysis, we exclude families with grandparents living within the household, although the results are no different when we include such families. The estimated effects from this specification are uniformly positive but are estimated imprecisely. Only the effects on exposure to violence and victimization are statistically significant. Based on these estimates, the predicted probability of being exposed to violence is estimated to be .73 for movers within the city compared with .26 for families that do not move within the city. Similarly, the predicted probability of victimization is .58 for movers within the city and is .17 for nonmovers. These estimates are extremely large, reflecting the imprecision of the estimates. Yet the results are remarkably similar when we use residence in a border neighborhood as the IV for moving within the city, although in this case, only the effect on victimization is significant. Although the IV approach clearly produces imprecise estimates, the results reinforce the general finding that moving within the city leads to increased levels of violence.

Table A1b shows the estimated effects of moving outside Chicago. Similar to the results for moving within the city, the results support the larger pattern of results in the article, which show negative effects of mobility outside the city on outcomes related to violence. However, once again, estimates are imprecise, and interpretations of the magnitude are difficult; only the estimated effects of moving outside the city on exposure and victimization are statistically significant. Based on these estimates, the predicted probability of being exposed to violence is estimated to be just .01 for movers outside Chicago and .50 for families that do not leave the city. The predicted probability of victimization is again .01 for movers outside the city and is .36 for nonmovers. Using residence in a border tract as the instrument, we find significant negative effects of moving outside Chicago on victimization and negative nonsignificant effects on violence and exposure to violence. In all cases, the estimates are imprecise; however, the coefficients for all measures of violence are substantively large, and the direction of the effect is consistent across all three outcomes.

Table A1. IV Estimates of the Effect of Moving Within and Outside Chicago on Violence: PHDCN Cohorts 9 and 12

a. Effect of Moving Within Chicago on Violence

	Violent Behavior Coefficient (SE)	Exposure to Violence Coefficient (SE)[a]	Violent Victimization Coefficient (SE)[a]
IV #1 Grandparent lives within the city (excludes families with grandparents in residence)			
Moved within Chicago[b]	.88	1.37***	1.27**
	(.96)	(.48)	(.59)
IV #2 Lives in border tracts as of wave 1			
Moved within Chicago[b]	1.15	.50	1.60**
	(1.46)	(1.06)	(.65)

b. Effect of Moving Outside Chicago on Violence

	Violent Behavior Coefficient (SE)	Exposure to Violence Coefficient (SE)[a]	Violent Victimization Coefficient (SE)[a]
IV #1 Grandparent lives within the city (excludes families with grandparents in residence)			
Moved outside Chicago[b]	–1.87	–2.56***	–2.28***
	(2.13)	(.69)	(.86)
IV #2 Lives in border tracts as of wave 1			
Moved outside Chicago[b]	–2.23	–1.02	–2.65***
	(2.97)	(2.21)	(.82)

[a]Coefficients in models of exposure to violence and victimization are from two-stage, least-squares probit regressions.
[b]Coefficient represents the effect of moving within Chicago relative to not moving within Chicago.
*$p < .10$; **$p < .05$; ***$p < .01$.