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Migration responses to Hurricane Katrina

by

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Abstract:

This paper uses data from the 2006 American Community Survey to consider migration responses and outcomes of those displaced by Hurricane Katrina. I first consider the distances individuals moved and the types of new neighborhoods they moved to. I then match— by likelihood of moving post-Katrina (based on pre-Katrina characteristics)— each treated individual (previously living in New Orleans and moved during 2005-2006), with a control individual (previously living in any one of ten control cities and unaffected by the hurricane). Through propensity score matching, I estimate the causal effect of moving on the short-term outcomes of individuals who are exogenously induced to move.

I find the following main results. First: when individuals are induced to move by such a shock, they are more likely to make long moves than short moves and do *not* necessarily end up in ostensibly “better” (i.e. higher socioeconomic status) neighborhoods. Second: non-white individuals *are* likely to move to less-white neighborhoods. Third: for adults and young adults, long movers face greater disruption than short movers (and vice-versa for children). Fourth, regardless of the type (across different definitions of “better” and “worse”) of new neighborhood non-white individuals move to, they face significant disruption. And fourth: non-white individuals experience the greatest disruption when they move to whiter neighborhoods.

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Introduction:

Within the past decades, a concerted interest has arisen regarding how neighborhoods can shape the outcomes of the individuals living within them. Prior research has motivated the idea that helping families gain access and move to “better” neighborhoods (i.e. higher median income, higher average educational attainment, etc.) can lead to more positive outcomes. At the same time, moving may also disrupt social networks, educational attainment, and job opportunities. Isolating the causal effect of moving on these outcomes is difficult because moves are typically the result of household choice and may be linked to both observable and unobservable characteristics of the household.

This study observes families displaced by Hurricane Katrina and asks the following questions: 1.) where are families moving when faced with an unanticipated exogenous shock, 2.) what are the impacts of these moves, 3.) how do these impacts differ depending on the distance of the move, and 4.) how do these impacts differ depending on the type of new neighborhood they move to? In my analysis I focus on the employment outcomes of adults and young adults (i.e. probability of being unemployed, and probability of losing employment) as well as the educational outcomes of children and young adults (i.e. probability of being an overage learner, probability of being enrolled in school at vulnerable ages, and probability of being a high school drop out).

These research questions are important because they encourage us to understand where individuals move under non-experimental conditions and how they fare in their new neighborhoods. It has historically been the case that when experimentally moved to “better” neighborhoods, households remain in the short-term, but end up moving again when given the chance. This suggests that some aspect of the new neighborhood or of the move itself induces them to move again. But what? The “typical” mover has usually determined that the benefits of moving to a specific location outweigh the costs; they are then inspired by this calculus to choose to move. Since this is the case, we usually only observe positive effects to moving. In the case of Hurricane Katrina, I am able to isolate the negative short-term outcomes of moving when there is no pull factor (i.e. a job opportunity, a positive income shock, etc.) and can thereby observe the disruptive part of the move that is usually counteracted and masked by these positive pull factors. Ultimately, a better understanding of the short-term disruption of a move could inspire change to current US housing policy—change that better equips families for success and stability after a move.

This paper draws from the American Community Survey (ACS), which contains information regarding a household's migration status over the course of the past one-year period. I start with observations from the 2006 survey and restrict on age (eliminating anyone below the age of 4 and above the age of 65) as well as on migration status within the past year (eliminating anyone who has moved from abroad, as the study focuses on internal US migration). Following precedent set by a 2018 paper by Deryugina, Kawano, and Levitt, I then designate the treatment group to be all individuals previously living in New Orleans who moved post-Katrina and create a control group of individuals from the ten US cities most similar (by measures of median income, population growth rate, and percentage of population which is black) to New Orleans. I then utilize propensity score matching to pair every individual from the treatment group with a similar individual from the control group on the basis of race, age, gender, educational attainment, and family information and in this way estimate the average treatment effects of moving post-Katrina on disruption outcomes.

Ultimately, through a descriptive analysis, I find that when forcibly displaced by a large-scale shock, individuals are more likely to make long moves than short moves. Additionally, non-endogenous movers do not necessarily choose to move to neighborhoods with higher median income and/or higher average educational level (i.e. ostensibly "better" neighborhoods). However, non-white individuals who move long distances *are* likely to move to areas with higher concentrations of non-whites. While these individuals may be disproportionately constrained in their choice of neighborhood because of income and/or discrimination, it could also be the case that non-white individuals have unique neighborhood preferences.

In my empirical work, I find that for adults and young adults, long movers are more likely than short movers to face employment disruption. In contrast, for children, short movers are more likely than long movers to face disruption in educational attainment. Lastly, I find that for adults, no matter the type of new neighborhood (i.e. better or worse on the dimensions of socioeconomic status, racial concentration and multigenerational household status), long movers and short movers both experience disruption. Specifically, moves to whiter neighborhoods by non-white individuals appear to be especially disruptive.

Prior research:

This study follows a recent body of literature which details the long-term benefits of moving (Chetty, Hendren, and Katz 2016, Chetty and Hendren 2016). A prominently featured instance of migration in this research is HUD's Moving to Opportunity Program. In the 1990s, HUD randomly selected low-income households who already qualified for affordable housing and enabled them (through vouchers and coaching) to move from areas of high poverty to areas of low poverty. Several authors have found and confirmed positive long-term outcomes; for example, one major result is that children who made the MTO move to "better" neighborhoods before the age of 13 received annual incomes 31% higher than those who did not (Chetty, Hendren, and Katz 2016).

Many questions arise from the MTO experiment. For one, do the long-term benefits of moving apply only to individuals who move to "better" neighborhoods? Additionally, do individuals choose to move to these types of neighborhoods on their own (outside of an experimental setting)? Several papers attempt to answer this question, exploiting instances of unanticipated natural disasters as quasi-random shocks to mobility. In a 2017 paper, Nakamura, Sigurdsson and Steinsson consider the 1973 eruption of a dormant volcano off the coast of Iceland. Using the destruction of houses as an instrument for moving away from the Westman Islands (the affected area), the authors estimated an increase in long-run labor earnings by $27,000 per year (Nakamura, Sigurdsson and Steinsson 2017). The authors also found that movers from their study moved "away from opportunity, at least from the perspective of average income" (i.e. all movers are leaving a high-income location), suggesting that when unguided, individuals do not necessarily move to "better" neighborhoods (as defined by HUD for the MTO experiment). The authors further explain, however, that individuals are taking the shock as an opportunity to move to towns where their skills and education are more well-matched with the industries and jobs available. This suggests that individuals perceive "better" neighborhoods in a way that is different from what HUD outlined for the MTO experiment.

More applicable to the US setting is research conducted on the effects of migration post-Hurricane Katrina. After making landfall on August 29, 2005, Hurricane Katrina (has been estimated to have) contributed to the deaths of over 1,000 individuals and the displacement of over 700,000 individuals (Gabe et al. 2005). Prior to 2006, Hurricane Andrew in 1992 was the largest natural disaster in the US and led to the evacuation of 350,000 residents (and the displacement away from affected areas of 40,000); in contrast, Katrina led to the evacuation of all 455,000 inhabitants (and the displacement away from New Orleans of over 200,000) (Smith and McCarty 1996, Sastry and Gregory 2014). In short: New Orleans was especially hard hit and residents of New Orleans were especially vulnerable to the storm because of "the eroding coastline along the Gulf of Mexico, the city's fragile levee system, and the social and economic characteristics of its inhabitants" (Cutter and Emrich 2006).

There are several existing papers which report the long-term positive impacts of moving from New Orleans post-Katrina. The most recent comes from Deryugina, Kawano, and Levitt, who use propensity score weighting and individual tax returns data to show that starting in 2008, the average hurricane victim (from New Orleans) actually had higher incomes than control households (Deryugina, Kawano, and Levitt 2018). The authors posit that this is because "when forced by an exogenous shock to migrate, people are able to choose from a wide range of possible locations, and they seem to choose places that offer them better economic opportunities" (Deryugina, Kawano, Levitt 2018). They justify this hypothesis by observing that "the increase in labor income was highest for those who left and never returned to New Orleans" (Deryugina, Kawano, Levitt 2018). However, the authors do not comment on how individuals identify neighborhoods as offering better economic opportunities, nor do they complete a descriptive analysis to actually evaluate what types of new neighborhoods individuals move to. This is one contribution of this study.

In all of the aforementioned instances of migration, authors have speculated about how individuals evaluate neighborhoods before choosing to move and also in choosing where to move. Many authors agree that what individuals are looking for during this decision-making process are superior economic opportunities and public goods/amenities, which are signaled to them by the average income and educational attainment of the new neighborhood. This paper hypothesizes that income and educational level are perhaps not so easily observed and are furthermore not the most straightforward indicators of perceived future social and economic success in a new neighborhood; indeed "individuals may be less than fully informed about the alternatives in their choice set, particularly regarding the degree to which their choice of location influences varying outcomes related to their own employment, health, or children's well-being" (Vigdor 2007).

Instead, especially for individuals of minority identity, more realistic signals of success relate to the amount of support that would be available to them; "cultural constraints shape migration patterns for...ethnic groups due to the group's needs for social support networks, kinship ties, and access to informal employment opportunities that tend to be available in areas that house large concentrations of co-ethnics" (Frey and Liaw 2005). If this is the case, then non-white individuals (and perhaps white individuals as well) might prefer new neighborhoods with higher concentrations of their race/ethnicity (which they perceive as an indication of future success in the new neighborhood). Other observable indicators of "social support network, kinship ties, and access to informal employment opportunities" include whether or not the new neighborhood is in the individual's birth state and whether or not the new household is a multigenerational one. On the first, previous migration literature suggests that people are drawn back toward their birthplace, especially following major life-course events, such as retirement or divorce, which are likely to have similar disruptive effects on people's lives as experiencing a disaster like Hurricane Katrina (Sastry and Gregory 2014). Similarly, the choice for a household to be multigenerational one is often to compensate for fewer financial and social capital, a condition that many households share post-Katrina (Muennig et al. 2017). This paper contributes to existing literature by exploring how new neighborhoods can further be characterized (beyond socioeconomic status) and also considers the possibility that different individuals have different preferences for new neighborhoods.

At the same time, it is important to acknowledge that where individuals move is not entirely determined by preference and choice. Oftentimes, friction exists in the decision-making process. At an institutional level, "zoning levels may prevent households from consuming their most desired bundle of housing services, amenities, and local public goods" (Vigdor 2007) Furthermore, housing discrimination "may lead individuals to locate in neighborhoods they would not otherwise select" (Vigdor 2007). Lastly, the individuals choosing to move do not have perfect information regarding all of the alternatives available to them and the extent to which these alternatives would affect their short and long-term outcomes. With this caveat in mind, this paper is able to summarize and present a case for what types of neighborhoods individuals move to after a catastrophic exogenous shock.

Regardless of what type of neighborhoods individuals are moving to in the wake of the hurricane, previous literature suggests that they face educational and employment disruption in the short-run and unexpected gains in both areas (i.e. better outcomes than individuals who were unaffected by Katrina and did not move) in the long-run (Vigdor 2007, Sacerdote 2008, Deryugina, Kawano, and Levitt 2018). These results are somewhat complicated by an observation: while these new neighborhoods were supposedly “better”, a large percentage of individuals returned back to New Orleans (perhaps before they were able to experience the long-term benefits of moving). In fact, 58% had returned to the city by December 2006 (Fussell et al. 2010). A possible hypothesis is that there are significant short-term disruptions to moving; these disruptions are so large that people not willing to undertake them even if doing so is likely to result in positive outcomes in the long-term.

There is also perhaps a link between the distance of a move and the likelihood of making future moves. In the case of the MTO experiment, even though the new neighborhoods households were moving to were “better” (in terms of socioeconomic status), by the time of the interim evaluation of the program (as early as four years after the move), two-thirds of the MTO families moved again, oftentimes back to high poverty areas that were closer in distance to their original neighborhoods (Orr et al. 2003). Again, perhaps something is happening in the short-term that differs as a result of short versus long-distance moves.

This paper’s second contribution to existing work is a consideration of the effect of the distance of a move on how disruptive the move is. A central hypothesis is that moves of longer distances are more disruptive and this is what causes individuals to want to move again, closer to their original locations. On one hand, this could be because the type of individual who moves longer distances are, on average, more likely to face worse educational and employment outcomes. For example: Frey et al., using ACS and IRS data, found that in 2006, “black and low-income residents who were displaced by Katrina were more likely to be living in distant locations, but whites and higher-income movers were more likely to have been displaced to nearby locations” (Frey et al. 2007, Sastry and Gregory 2014). The authors hypothesize that individuals who moved farther are perhaps those who face greater barriers to returning to New Orleans (ex: a flood-damaged dwelling) (Sastry and Gregory 2014). In short, there are observable as well as unobservable characteristics that dictate who makes short versus long moves and these characteristics could drive differences in disruption for short and long movers. On the other hand, the fact that long distance movers face greater disruption could be because long moves are inherently more disruptive than short moves. More specifically, since the strength of social relationships decreases with geographical distance, it could be the case that many short-term outcomes are adversely affected by distance from pre-existing social networks (Patacchini, Picard, and Zenou 2015). For example, a child or young adult's educational success is dependent on both the relationships the child is able to make, as well as the relationships the parents is able to make with neighbors, teachers, and other parents. Also, a young adult or adult's ability to find a job in a new area is often dependent on the connections they already have or otherwise are able to make.

In short: this paper fills in the gaps from previous literature by exploring *where* individuals are moving (considering distance and broadening the characterization of new neighborhoods) and *how* distance and type of new neighborhood play a role in their short-term outcomes. While migration post-Katrina is a unique circumstance, this paper still sheds light on all moves that lack a pull-factor and therefore can isolate the extent of short-term disruption from the usually observed, positive outcomes.

Data:

This paper uses data from the American Community Survey (ACS), which contains information regarding a household's migration status over the course of the past one-year period. I begin with observations from the 2000, 2005, and 2006 surveys (my main analysis is conducted with just the 2006 data) and restrict on age (ages 4-65) as well as on internal (within the US) migration status within the past year. I further restrict my sample to a treatment and control group, which I then pull from in my empirical analysis to perform propensity score matching. To create treatment and control groups, I draw directly from Deryugina, Kawano, and Levitt's 2018 paper.

First, I designate the treatment city to be New Orleans. This choice falls in line with prior Hurricane Katrina-related literature as well as the observations that 1.) half of the damage from the hurricane was concentrated in this one city and 2.) New Orleans has a disproportionate amount of low-income individuals, meaning their residences are most likely to be disrupted and arguably, they are more incentivized to do any sort of move (Sastry and Gregory 2014). Since ACS data itself does not provide a specification of whether or not the individual is moving because of Katrina, it becomes important to select a treatment city where it is most likely the case that the choice to move was the direct effect of the "random shock" of Katrina; New Orleans is such a choice. Even so, I designate the treatment group to only include individuals who previously lived in New Orleans and during the 2005-2006 period *did* move.

To construct the control group, Deryugina, Kawano, and Levitt start with all US cities with populations greater than 100,000. From there, they compute differences between New Orleans' median earnings, population growth rate, and percent of population that is black and each potential control city's values for each of these three outcomes. These specific dimensions are chosen because of New Orleans' unique economic (which the first two dimensions seek to capture) and racial (which the final dimension addresses) environment. The differences obtained above are then normalized by standard deviation, squared, and then summed across the different outcomes and across years (from 2000 to 2005).

The cities with the smallest resulting sums were deemed to be the most similar to New Orleans. The final selected cities are Baltimore, MD; Birmingham, AL; Detroit, MI; Gary, IN; Jackson, MI; Memphis, TN; Newark, NJ; Portsmouth, VA; Richmond, VA; and St. Louis, MO. For my analysis, I designate the control group to be all individuals residing in these control cities pre-Katrina (regardless of whether or not they moved sometime during the 2005-2006 period). Ultimately, the idea is that both movers and non-movers from the control cities are a good representation of what would have happened to individuals who lived in New Orleans and were displaced by Hurricane Katrina *if* Hurricane Katrina had not occurred.

To corroborate Deryugina, Kawano, and Levitt's process, I compare neighborhood characteristics of the treatment and control groups in 2000. In their own analysis, Deryugina et al. caveat that New Orleans is very unique, making it difficult to find control cities that match its "high reliance on tourism, low income levels and employment rates, and a high percentage of black residents" (Deryugina, Kawano, Levitt 2018). The results in Table 1 indeed suggest that median income and % of population which is white are both statistically different between the treatment and control groups.

Table 1: Pre-Katrina (2000) average neighborhood comparison

Current Neighborhood Quality (2000)	For those living in New Orleans	For those living in control cities
Median Income	32,000***	40,600
% 25+ with at least high school degree	0.485	0.488
% white people	0.247***	0.338
% in multigenerational homes	0.346	0.338
N	*16,495*	*154,341*

Asterisks refer to whether the treatment group mean is statistically different from the control group.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

However, when I evaluate summary statistics from 2000 regarding the disruption outcome variables of interest (discussed in further detail in the empirical strategy section of the paper) I find support for my designation of treatment and control groups. In Table 2, I observe that within races, outcomes are not statistically different between the treatment and control groups for all adult outcomes. This suggests the selected control group is an apt comparison to the treatment group. However, all outcomes for young adults (enrolled, idle, unemployed, work disruption) are statistically different between the groups; furthermore, enrollment and dropout rate are statistically different between the groups for children. Ultimately, I find upon closer observation that treatment group individuals are, pre-Katrina, statistically more likely than control group individuals to experience positive outcomes (such as being enrolled) and statistically less likely than control group individuals to experience negative outcomes (such as being a high school drop out, being idle, being unemployed, and facing work disruption). This consistent trend suggests that on average, individuals from New Orleans experience better outcomes than individuals from the control city, which actually strengthens the argument for my proposed results. If I do find disruption effects from moving post-Katrina (when comparing treatment individuals to control individuals), it must be that disruption was so great that it counteracted the previous gains residents of New Orleans had over residents of the control cities.

Table 2: Pre-Katrina (2000) outcome comparison

Disruption outcome	Treatment	Control	Treatment (white)	Control (white)	Treatment (non-white)	Control (non-white)
Adults (25-64)						
Unemployed	0.049	0.049	0.023	0.023	0.061	0.065
Work Disruption	0.029	0.032	0.016	0.017	0.036	0.042
N	9,999	95,730	3,219	37,753	6,780	57,977
Young adults (18-24)						
Enrolled	0.462	0.364	0.621***	0.436	0.418**	0.334
Idle	0.077	0.096	0.015***	0.040	0.095***	0.120
Unemployed	0.115	0.127	0.035**	0.055	0.136**	0.136
Work Disruption	0.063	0.081	0.029	0.040	0.072***	0.098
N	2,001	17,337	459	5,299	1,542	12,038
Children						
Overage learner (6-16)	0.028	0.027	0.019	0.022	0.029	0.028
N	3,266	29,984	432	7,585	2,834	22,399
Enrolled (4-5)	0.866	0.766	0.955***	0.755	0.853***	0.769
N	596	5,716	80	1,457	516	4,259
Dropout (15-18)	0.050	0.065	0.027**	0.058	0.053*	0.067
N	1,258	10,916	170	2,964	1,088	7,952

Asterisks refer to whether the treatment group (within a race) is statistically different from the control group. For a description of all of the featured outcome variables see Table 11.

Lastly, in support of the validity of my treatment and control groups, I create five versions of treatment and control groups, non-inclusive of the primary definition (Table 3). My empirical findings are robust to each of these designations (Tables 21-25).

Table 3: Different treatment and control group designations

Treatment group	Control group
*NOLA movers	*Control cities (everyone)
NOLA movers	NOLA non-movers
NOLA movers	Control cities non-movers
NOLA movers	NOLA non-movers and control cities
Control cities movers	Control cities non-movers
All movers	All non-movers

*Refers to the primary treatment and control groups

Preliminary result: Katrina as an exogenous shock to moving

Table 6 and Figure 1 illustrate that the likelihood of moving is higher for those who lived in New Orleans than for those who previously (2005) lived in a control city, suggesting that Hurricane Katrina is indeed correlated with greater migration. Still, it is unclear whether or not there is a causal story to be told; perhaps it is the case that New Orleans and the control cities are inherently different from one another with respect to migration frequency. However, by comparing those from New Orleans to those from control cities in 2005, I find that the frequencies of moving are much closer to each other in magnitude (Table 6). Still, these frequencies are significantly different from each other at the 99% confidence level. This is perhaps because the 2005 data is contaminated: Katrina hit August of 2005 and because ACS data does not report when (i.e. what month) an individual was interviewed— it is possible that the data captures individuals who had already moved because of Katrina. ACS data is furthermore restricted in the variables available in years prior to 2005 and therefore, I am unable to see how an individual moved in years prior to 2005.

However, in defense of previous literature that Katrina is random exogenous shock which causes migration, I can make the observation that the likelihood of moving is almost 50% higher for the NOLA group in 2006 than in 2005 and likelihood of moving is less than 1% higher for the control group in 2006 than in 2005. Indeed, in conducting a straightforward difference-in-differences analysis with 2005 and 2006 data (Table 4), I find that across age groups, likelihood of moving post-Katrina increases by 14-22%.

Table 4: Difference-in-differences analysis of probability of moving

	(1)	(2)	(1)	(2)	(2)
	Did Move (6-16)	Did Move (4-5)	Did Move (15-18)	Did Move (18-24)	Did Move (25-64)
Difference-in-differences	0.204^{***}	0.219^{***}	0.144^{***}	0.164^{***}	0.183^{***}
	(0.0339)	(0.0854)	(0.0504)	(0.0459)	(0.0157)
N	11623	2226	4348	7043	43447

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Each column represents an age group of interest (tied to the different outcome variables displayed in Table 11). Across all age groups, probability of moving increases by a statistically significant amount post-Katrina.

Descriptive Results: where do individuals post-Katrina?
Location:

Having confirmed Katrina as an exogenous, positive shock to migration, I can conduct some descriptive work to understand where individuals moved locationally post-Katrina. Interestingly, movement within the state of Louisiana constituted 46% of all moves from New Orleans post-Katrina, with movement within the city itself accounting for 18%. Below, Table 5 considers the top 5 state destinations for evacuees.

Table 5: Top post-Katrina destinations for those who previously lived in New Orleans and moved during 2005-2006

Top destinations	% out of NOLA movers	% out of all movers
Louisiana	0.456	0.077
Within New Orleans	*0.136*	*0.022*
Texas	0.297	0.058
Houston	*0.147*	***0.147***
Georgia	0.073	0.023
Florida	0.026	0.018
California	0.019	0.010
% of movers captured in top 5 state destinations	0.872	

Distance of move:

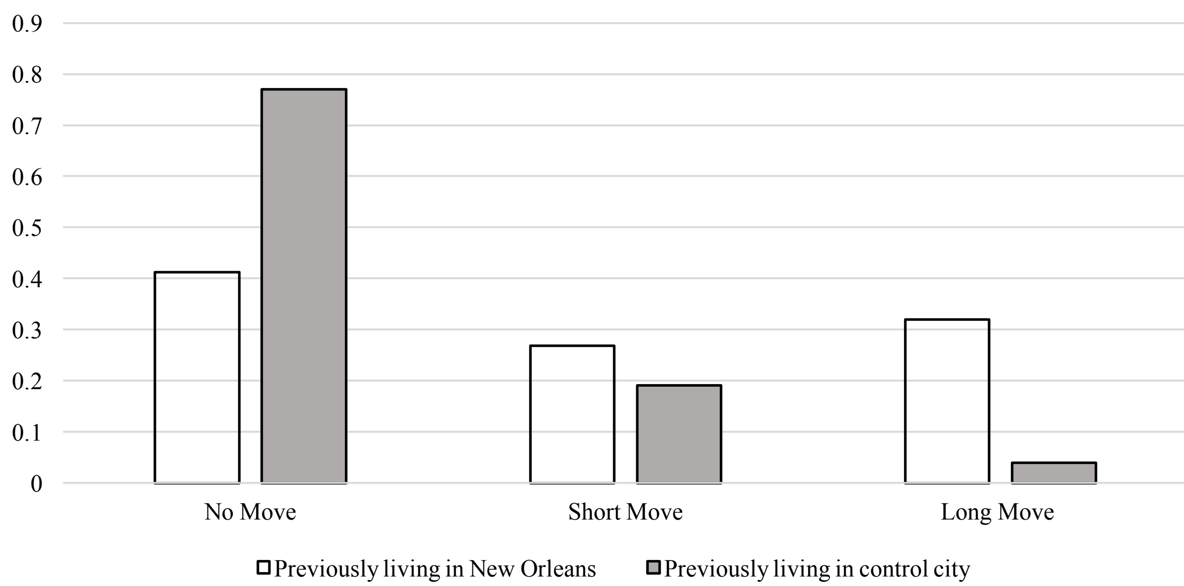
Beyond location, I am able to characterize the move of an individual in several other ways. Foremost, I consider the distance traveled in a move (Table 6). For the rest of this analysis, I designate short moves to be moves within a state (within PUMAs and between PUMAs) and long moves to be moves between states (between contiguous states and between non-contiguous states).

Table 6: Percentage of individuals who made each type of move during 2005-2006

Type of Move	Pre-Katrina lived in New Orleans	Pre-Katrina lived in control city
No Move	0.412	0.770
Short Move	0.268	0.191
Long Move	0.320	0.039

Results cover the 2005-2006 period. "Control cities" refer to Baltimore, MD; Birmingham, AL; Detroit, MI; Gary, IN; Jackson, MI; Memphis, TN; Newark, NJ; Portsmouth, VA; Richmond, VA; and St. Louis, MO (this distinction will be elaborated upon later in the paper). Short moves are those within a state (within PUMAs and between PUMAs); long moves are those between states (between contiguous states and between non-contiguous states).

Figure 1: Percentage of individuals who made each type of move during 2005-2006



See notes for Table 6.

Table 7: Percentage of individuals who made each type of move during 2004-2005

Type of Move	Pre-Katrina lived in New Orleans	Pre-Katrina lived in control city
No Move	0.706	0.764
Short Move	0.155	0.198
Long Move	0.139	0.038

See notes for Table 6.

Table 6 reveals that from 2005-2006, previous residents from the control group were more likely to move short distances than long distances. Similarly, from 2004-2005 (Table 7), those treatment and control group individuals were both more likely to move short distances than long distances. The outlier becomes treatment group individuals in the 2005-2006 period (i.e. individuals who moved because of Katrina), who, in contrast, were more likely to move long distances than short distances. These results suggest that endogenous moves are more likely to be short moves than long ones. One explanation might be that moves of longer distances are perceived to be costlier and/or more disruptive, and therefore are more likely to be made out of necessity than by choice. Nevertheless, even involuntary long moves might land people in neighborhoods that are "better" on some observable dimensions

Type of new neighborhood:

Tables 8 and 9 characterize the types of neighborhoods that individuals (long movers and short movers, respectively) moved to. For the purposes of this descriptive analysis, I equate "neighborhoods" with PUMAs; while PUMAs are slightly larger than the classic designation of a neighborhood, they are the smallest geographical unit available via the ACS. I focus on five characteristics of a neighborhood: median income, average educational attainment (takes the highest level of educational attainment in each household and then finds the PUMA-level average), % population that is the individual's race, whether or not the new neighborhood is in the individual's birth state, and whether or not the individual is moving into a multigenerational home (multigenerational homes includes households with 2 adjacent adult generations, 2 non-adjacent generations, and 3+ generations).

Some important notes regarding these characteristics: first, for birth state, I do not link a child to their parent's birth state (therefore, estimates for frequency of moving to one's birth state are likely skewed downwards; a child cannot move to their birth state if they are currently still living in it, but could/would to a parent's birth state); and second, for moving into multigenerational homes, I do not distinguish between households that already where multigenerational and those that become multigenerational post-move (therefore, estimates are perhaps skewed upwards—regardless, I am interested in households that either become *or* otherwise remain as multigenerational homes).

Table 8: Type of new neighborhood for long movers

Type of new neighborhood	Treatment (white)	Control (white)	Treatment (non-white)	Control (non-white)
Higher median income	0.539	0.764	0.326	0.657
Higher educational attainment	0.558	0.675	0.330	0.517
Higher concentration of own race	0.996	0.961	0.634	0.684
In birth state	0.235	0.219	0.095	0.250
In multigenerational home	0.216	0.147	0.314	0.235
N	*166*	*687*	*591*	*524*

Asterisk indicates a restricted sample such that the summary statistic is calculated only for individuals who were not born in NOLA. Before this restriction, statistics were artificially low for the treatment group because so many of these individuals were born in Louisiana and therefore could not be counted as moving *to* Louisiana. For clarification: higher educational attainment is determined by finding the average of the highest educational attainment in each household; multigenerational homes are 2+ generations within one household (2 generation child-adult HHs do not count but 2 generation adult-adult HHs do).

Table 9: Type of new neighborhood for short movers

Type of new neighborhood	Treatment (white)	Control (white)	Treatment (non-white)	Control (non-white)
Higher median income	0.754	0.926	0.561	0.906
Higher educational attainment	0.356	0.674	0.139	0.636
Higher concentration of own race	0.996	0.854	0.199	0.436
In multigenerational home	0.282	0.190	0.371	0.270
N	*172*	*2,517*	*463*	*3,417*

A.) Differences between treatment (exogenously motivated) and control (endogenously motivated) movers

A primary result from this analysis is that for both long and short movers, individuals from the control cities are likely to move to "better" (in line with the MTO definition: higher median income and higher educational attainment) neighborhoods. For example, 76% of white and 66% of non-white individuals who made long moves relocated to neighborhoods with a higher median income than their original neighborhood; similarly, 93% of white and 91% non-white individuals who made short moves are relocating to neighborhoods with a higher median income. In other words, many voluntary moves appear to be to places that are better on observable dimensions.

However, in the case of Katrina movers (i.e. individuals from New Orleans), this appears less likely to be the case. For example, only 54% of white and 33% of non-white individuals who made long moves also moved to neighborhoods with a higher median income. Similarly, only 36% of white and 14% of non-white individuals make short moves moved to neighborhoods with higher educational attainment. These results are consistent with the idea that an unplanned, involuntary move is likely to be different from a voluntary move. Ultimately, these results are interesting because if voluntary moves are often made to better neighborhoods (i.e. better opportunities, financial gains, etc.), it is possible that the disruption of the move is completely overshadowed by the positive effects of moving to opportunity. By observing the outcomes of Katrina movers (later during the empirical analysis component of the paper, I am able to shed some light on the extent of the disruption experienced by individuals who make unplanned, involuntary moves, most often *not* to opportunity ("opportunity" as defined by previous literature, i.e. neighborhoods of higher socioeconomic status).

For the remaining three neighborhood characteristics, there are three notable observations. First, long movers from the treatment and control groups are both likely to move to neighborhoods with higher concentration of their own race; there is no consistent trend relating treatment and control groups for short movers. Second, treatment and control group individuals are both not likely to move to a new neighborhood in their birth state. Lastly, all individuals are not likely to move to multigenerational homes. In comparing the magnitude of these statistics between treatment and control groups, there is no consistent trend. These results suggest that endogenously-motivated and exogenously-motivated movers are perhaps not particularly different in their preferences for or restrictions from neighborhood characteristics related to social networks.

An alternative way of comparing how the treatment and control groups vary in their neighborhood types can be observed in Table 10.

Table 10: Average neighborhood quality post-Katrina for treatment and control group individuals

New Neighborhood Quality (2006)	For those previously living in New Orleans	For those previously living in control cities
Median Income	30,350	42,000
% 25+ with at least high school degree	0.510	0.477
% white people	0.190	0.376
% in multigenerational homes	0.324	0.234
N	*1,392*	*7,145*

This table look at the new neighborhoods individuals have moved to by the time of the 2006 survey. The left-hand column summarizes the new neighborhood characteristics of those who moved from New Orleans during 2005-2006; the right-hand column is likewise for control cities.

Ultimately, more can be said by observing differences within the treatment group (and also within the control group) between long and short movers and also between non-white and white movers.

B.) Differences between long movers and short movers

Results from this descriptive analysis suggest that there are differences between long and short movers. I hypothesize that longer moves are less desirable because they are more disruptive. At a fundamental level, long moves require more resources (they are more expensive by nature of distance) and therefore are perhaps more disruptive via this channel. Long moves are also potentially disruptive because of the distance from one's original social networks. Therefore, the question becomes: are individuals are compensating longer moves with moves to "better" neighborhoods. Alternatively, it could also be the case that those who move longer distances are even more constrained by income and discrimination and simply end up in certain types of neighborhoods.

Ultimately, I find that within the treatment group, long movers are more likely than short movers to move to new neighborhoods with higher educational attainment and with higher concentration of their race, but less likely to move to new neighborhoods with higher median income and into multigenerational households. One striking result is that long-distance nonwhite treatment individuals are over two times more likely to move to new neighborhoods with a higher concentration of non-white individuals than their short-move counterparts. However, this trend is not consistent across all characteristics of new neighborhoods, nor is it consistent within the two main categorizations of new neighborhoods (higher socioeconomic status and potential for social networks). Taken as a whole, these results somewhat rule out the definitive claim that those who are moving longer distances are also consciously choosing to move to "better" (in terms of socioeconomic status) neighborhoods. The final result, especially, is evidence in favor of the the idea that non-white individuals may try to compensate for the long move by choosing neighborhoods where they believe they can quicker and better build new networks. It could alternatively be the case that these neighborhoods are least resistant to their migration in terms of housing discrimination, etc.

C.) Differences between non-white and white movers

As alluded to, differences in where individuals move also exist between races. One explanation for this might be that non-white individuals are especially constrained in choice and disproportionately affected by, for example, transaction costs of moving, zoning laws, and housing discrimination. One result is that within treatment and control groups there are disparities between races (white versus non-white) in the likelihood of moving to neighborhoods of higher socioeconomic status than their previous neighborhoods; this disparity is especially large for Katrina-moves. For example, in the sample of long movers, within the control group, white individuals are 16% more likely than non-white individuals to move to new neighborhoods with a higher median income; within the treatment group, white individuals are 65% more likely than non-white individuals to move to new neighborhoods with a higher median income. This disparity is also present when looking at short movers and also when looking at the neighborhood characteristic of educational attainment. While one explanation is that non-white individuals are more constrained in their choice, it could also be the fact that non-white individuals are interested in different observable characteristics of a neighborhood. As mentioned in the earlier literature review, this is because this group is more reliant on social support networks, kinship ties, and access to informal employment opportunities than white individuals are, especially post a natural disaster (Sastry and Gregory 2014).

It is also noteworthy that counterintuitively, while there are disparities between races in the likelihood of moving to neighborhoods with high potential for social networks, this disparity is *not* larger for Katrina-movers (versus control movers). I would expect that that in the wake of a large shock/natural disaster, non-white individuals, who are adversely affected (depletion of assets, etc.), would be especially interested in neighborhoods with greater potential for building networks, but this is not the case. For example, within long movers from the treatment group, non-whites are 45% more likely to move into multigenerational households than whites; within long movers from the control group, nonwhites are 60% more likely to move into multigenerational households than whites. Within short movers from the treatment group, non-whites are 34% more likely to move into multigenerational households than whites; within short movers from the control group, nonwhites are 42% more likely to move into multigenerational households than whites.

A final noteworthy observation: long distance, non-white, treatment movers are actually the most likely (when compared to white control, white treatment, and non-white treatment individuals) to move to their birth state. It could be the case that non-white individuals are especially resource constrained post-Katrina. Results are again consistent with the observation that non-white individuals are more likely to face discrimination when trying to find housing elsewhere and also that non-white individuals are perhaps especially interested in neighborhoods with greater potential for social networks.

Empirical Strategy for estimating effect of moving post-Katrina:

In an ideal world, this paper would be based on an analysis which isolated the effects of type of move (distance of move and type of new neighborhood) on the disruption caused by the move. It proved difficult, however, to find an event that randomly caused some individuals to move short distances and others to move long distances; similarly, it it proved difficult to find an instrumental variable identification strategy that otherwise would have sufficed (same with moves to neighborhoods of different types). Ultimately, while this paper cannot directly compare individuals who make different types of moves, it does compare each type of mover (i.e. long, short, to neighborhoods of higher socioeconomic status, etc.) with an equivalent counterfactual individual via propensity score matching.

In short: this matching process, based on age, gender, race, educational attainment, and family information (the process will be discussed in greater detail below), compares each individual who previously lived in New Orleans and moved post-Katrina with an individual who previously lived in a control city and had an equal likelihood of moving post-Katrina (based on the aforementioned pre-Katrina characteristics). Ultimately, the idea is that the matched control individual is a good representation of what would have happened to the individual who lived in New Orleans and was displaced by Hurricane Katrina *if* Hurricane Katrina had not occurred.

Outcome variables of interest

Prior to matching all individuals from the treatment group to an individual from the control group, I need to establish disruption variables of interest, as they inform the covariates I choose to match individuals on as well as the restrictions and specifications I make on matches. In general, I focus on disruption outcomes that are visible in the short-term and also can extend to have effects on individuals in the long-term.

For my adult sample (those between 18 and 24, inclusive), I am interested in the outcome variables of unemployment and work disruption. I count an individual as unemployed if they are, at the time of the survey, in the labor force but not working. However, this is somewhat of an imperfect measure: it could be the case that an individual who is currently unemployed was also unemployed prior to the move; i.e. perhaps it is the case that observed instances of unemployment post-Katrina are not reflective of the disruption of a move, but rather reflective of a certain type of individual. I therefore also run regressions with work disruption as an outcome variable. I count an individual as facing a work disruption if they were employed one-year ago and no longer employed at the time of the survey (post-Katrina). This nuanced version of unemployed is important because it enables me to narrow in on the true effect of the move on the outcome of the individual.

I also evaluate the disruption outcomes of young adults and children. For young adults (those between 15 and 18, inclusive), I look at several overlapping outcomes. As a sort of all-encompassing outcome, I consider whether or not an individual is “idle” post-Katrina; this is when an individual is unemployed (implying that there are in the labor force) and also has not, at any point in the past three months, attended school or college. I then separate the components of this variable into whether the individual is currently enrolled in school or college, unemployed, and/or facing a work disruption. In this way, I am able to isolate the driver of disruption for the young adult.

Lastly, for my main child sample (those between 6 and 16, inclusive), I am predominantly interested in whether or not an individual is an overage learner. This outcome observes those who are “more than one year older than the median age for [a] grade” (Barrat 2013) and captures instances where an individual voluntarily or involuntarily took time off from school or was otherwise held back. This measure is both useful and important because it is observable in the short-term and also associated with the future long-term outcomes of an individual (ex: lower likelihood of high-school graduation) (Haveman, Wolfe, and Spaulding 1991). It is worth caveating that the ACS only reports whether or not an individual is attending any grade in the categories 1-4, 5-8, or 9-12. I am therefore only able to capture individuals that would be considered overage if they were currently enrolled in the 4th, 8th, or 12th grade. However, this leaves me with an underestimate— ultimately, any significant effect that I find on this outcome is likely to be larger in reality. To capture the same sort of educational disruption in preschool children and children approaching college age, I observe enrollment status (if the individual has at any point within the past 3 months attended school) and high school dropout status (if the individual has begun to attend high school, is no longer enrolled in school, but does not have a high school degree or its equivalent), respectively.

A summary of age groups of interest and their corresponding disruption outcomes of interest can be found in Table 11.

Table 11: Disruption outcomes of interest separated by age

Age	**Outcome**	**Definition**
Children		
6-16	Overage learner	If the individual is “more than one year older than the median age for [a] grade” (Barrat 2013); for example, the median age for an individual in kindergarten is 5, therefore anyone who is older than 6 but still in kindergarten is overage (etc.)
4-5	Enrollment	If the individual has, at any point in the past 3 months, attended school or college
15-18	High school dropout	If the individual's highest level of schooling completed is ≥ 9 but <12 and the individual has not, at any point in the past 3 months, attended school or college
Young adults		
18-24	Idle	If the individual is unemployed (in the labor force) and also has not, at any point in the past 3 months, attended school or college
	Enrollment	If the individual has, at any point in the past 3 months, attended school or college
	Unemployment	If the individual is in the labor force but currently not working
	Work disruption	If the individual was employed 1 year ago, currently in the labor force, but not currently working
Adults		

25-64	Unemployment	If the individual is in the labor force but currently not working
	Work disruption	If the individual was employed 1 year ago, currently in the labor force, but not currently working

Propensity score matching process

The first step in the propensity score matching process is pairing individuals from the treatment and control groups by the probability they would move post-Katrina (based on pre-Katrina characteristics). A limitation of the 2006 ACS data is that individuals are surveyed at any point during the the 2005-2006 period. Hurricane Katrina was August 23, 2005 – August 31, 2005, so I am observing individual's post-Katrina characteristics. Therefore, covariates used to predict each individual's propensity score (probability of moving after Katrina based on their pre-Katrina characteristics), are limited to those exogenous to Katrina (i.e. consistent pre and post-Katrina).

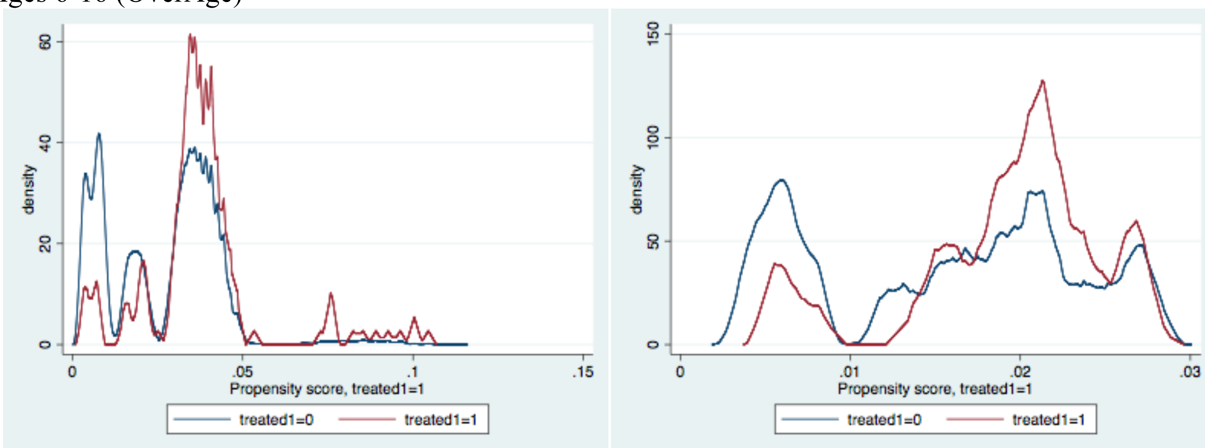
The chosen covariates vary across age groups (as a result of data restrictions as well as endogeneity issues regarding the disruption outcome variables that are relevant to each age group). For adults 25-62 (inclusive), I predict propensity scores for moving either a short or long distance based on age, gender, race (white non-Hispanic, and non-white), educational attainment (highest degree received), and an interaction term between the number of children below the age of five in the household and the gender of the individual. Regarding the final covariate: ideally, I would match on a more straightforward indication of family life and dynamics; however, it is the case that both marriage and number of siblings (and family size in general) are subject to change post-Katrina and therefore cannot be used as a predictor (Deryugina, Kawano, Levitt 2014).

For individuals 15-18, 16-20, and 18-24 (inclusive), I predict propensity scores based on age, gender, and race. For all three of these age groupings, I face the data constraint that individuals who do not live in the same household as their parents cannot be attached to the educational attainments of their parents. Because the majority of these individuals do not live at home, matching and conducting analysis on parental educational attainment ends up dropping these individuals for analysis, biasing results. Arguably, an individual's own educational attainment could be used for those 15-18 and 16-20, but given that I am interested in educational attainment as a result, I cannot also use it as a predictor. Lastly, for individuals 6-16 (inclusive), I match on the basis of age, gender, race, and parental educational attainment.

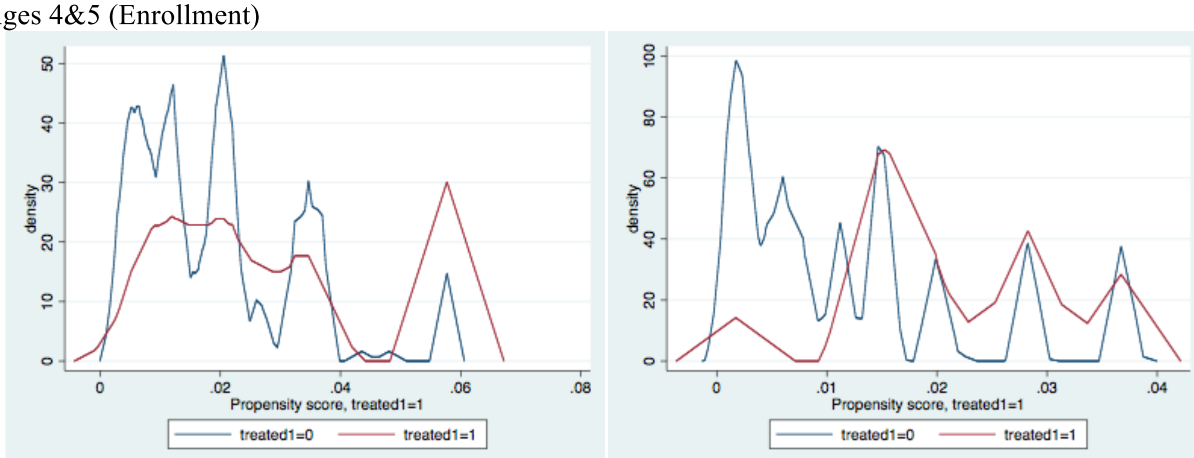
When conducting propensity score matching, evidence of the common support condition (i.e. evidence in the final sample there is at least one control group member within the propensity score range of each treatment group member) is imperative and ensures that any combination of characteristics observed in the treatment group can also be observed among the control group (Bryson, Dorsett, and Purdon, 2002). The following graphs are evidence of the requirement and indicate the success of my matching process. Note that the distribution of individuals in the control group overlaps with the distribution of individuals in the treatment group (i.e. for each propensity score predicted for the treatment group, there, for the most part, exists someone in the distribution of the propensity scores of the control group with the same propensity score). This means that across age groups (and distance specifications), individuals from the treatment are likely to be well-matched with an individual from the control group. The overlap graphs are most convincing for my adult population and somewhat less convincing for the young adult and child populations; however, I am able to rule out that different quintiles of the propensity score are unbalanced across the predictive covariates. Instead, it must be the case that the sample sizes of these age groups are quite small. In every case, however, there is at least one control group member within a 0.05% probability of moving relative to each treatment group member, as discussed below.

Figure 2: Overlap graphs to validate the effectiveness of the matching process (Order: Long movers, short movers)

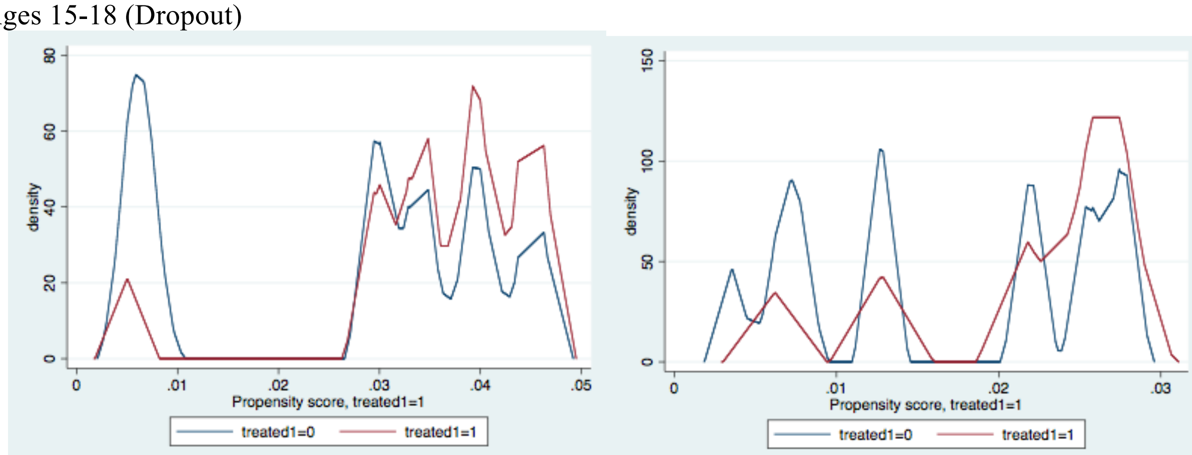
Ages 6-16 (OverAge)



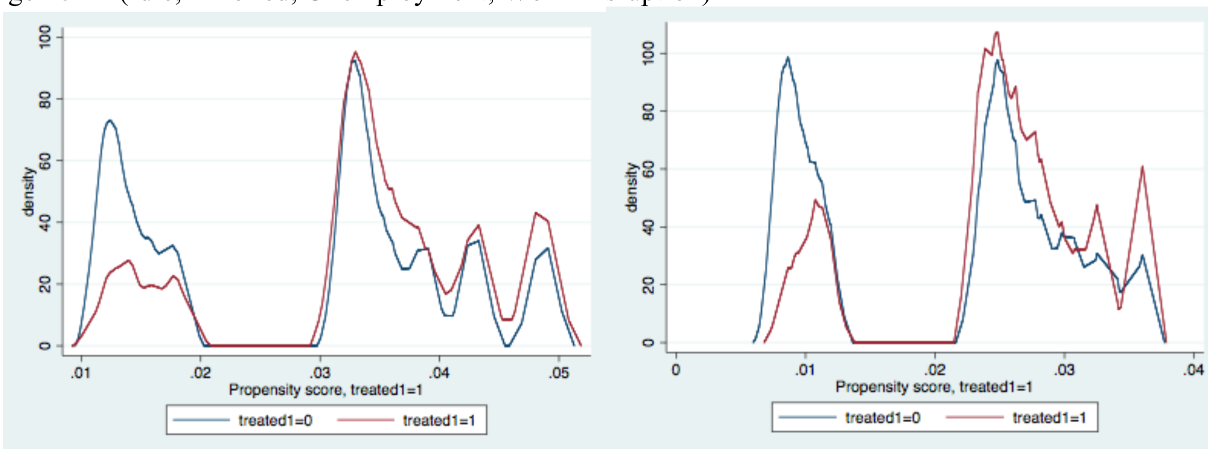
Ages 4&5 (Enrollment)



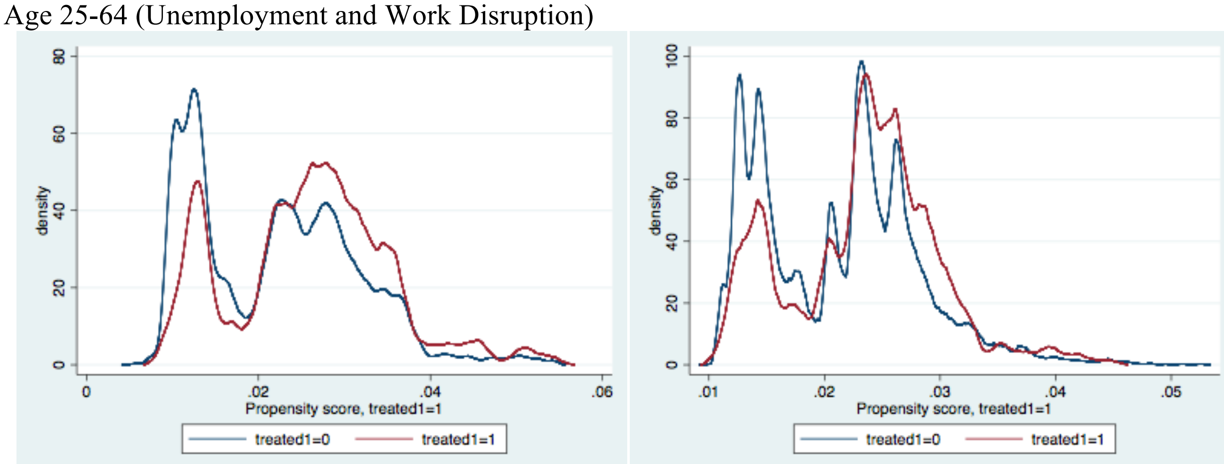
Ages 15-18 (Dropout)



Age 18-24 (Idle, Enrolled, Unemployment, Work Disruption)



Age 25-64 (Unemployment and Work Disruption)



From the calculation of propensity scores, I then move on to estimating treatment effects. My analysis matches only on treated subjects and therefore the causal effect I arrive at can be interpreted as the average treatment effect for individuals in the treatment group (i.e. those who moved because of Katrina). Before arriving at this point however, I apply two additional constraints. First, I specify for standard errors to be robust to heteroskedasticity. Second, I restrict how similar a control group individual must be to the treated individual in order to be matched with them. Initially, I followed standard protocol referenced by Garrdio et al. 2014 and Austin 2011 and restrict propensity score matches to an "optimal" caliper of 0.2 of the standard deviation of the logit of the propensity score. However, across age groups and between specifications, I find that this strategy often leads to a caliper that is too small (i.e. some treatment individuals end up with no match). To minimize caliper while still ensuring that all treatment individuals are matched, I specify ATET (estimating the expected causal effect of the treatment for treatment individuals, i.e. matching on treatment individuals) versus ATE (estimating the expected causal effect of the treatment across all individuals in the population, matching on both treatment and control individuals). I also use the 0.2 rule as a starting point, and for each specification, move to the smallest caliper (that is a multiple of 0.005) that still allows matches for all individuals in the treatment group.

Table 12: Breakdown of calipers used per age group for (main) propensity score matching

	Long movers		Short movers	
Age group	Optimal caliper	Adjusted caliper	Optimal caliper	Adjusted caliper
6-16 (Over age)	0.003	0.015	0.002	0.005
4-5 (Enrolled)	0.003	0.010	0.002	0.015
15-18 (Dropout)	0.003	0.005	0.002	0.005
18-24 (All YA)	0.002	0.005	0.002	0.005
25-64 (All adult)	0.002	0.005*	0.001	0.010

*Adjusted caliper was not standard across all outcomes for long moving adults: for unemployment as an outcome when individuals were furthermore matched by employment status last year, I had to use a higher caliper of 0.02.

A final area of consideration for my empirical strategy is how I pool individuals for analysis. My main pooling separates long movers from short movers. The “long sample” includes just those in the treatment group who made a long move (from the ACS, I designate long movers as those who moved to a contiguous or non-contiguous state, i.e. moved to a different state) and also all individuals from the control cities. The “short sample” includes just those in the treatment group who made a short move and then everyone in the control group cities (from the ACS, I designate short movers as those who moved within or between PUMAs, i.e. moved within the same state). This decision comes from previous descriptive work which observes that individuals with certain characteristics are more likely to move farther distances. Frey et al., using ACS and IRS data, found that in 2006, black and low-income residents who were displaced by Katrina were more likely to be living in distant locations, but whites and higher-income movers were more likely to have been displaced to nearby locations (Frey et al. 2007, Sastry and Gregory 2014). Indeed, my own descriptive analysis corroborates that who moves different distances is not randomly assigned. For example, race plays a role (Table 13). Therefore, it becomes imperative for me to separate long movers from short movers.

Table 13: Type of 2005-2006 move broken down by race

	No move		Short movers		Long movers	
	NOLA	Control	NOLA	Control	NOLA	Control
% white	0.635	0.407	0.271	0.424	0.219	0.576
Total	975	23964	635	5934	757	1211

No move frequencies are calculated out of all individuals who previously lived in NOLA and the control cities, respectively. Short and long move frequencies are calculated out of those individuals from NOLA and the control cities who moved.

I also considered running my analysis separately for whites and non-whites. However, I found that white and non-white individuals are equally dispersed in each quintile of propensity scores (probabilities of moving). In other words: the treatment and control groups had the same percentage of non-white people in each quintile of the propensity score distribution and therefore individuals of the same race are being fairly being paired and compared. It is not the case that I am matching, for example, a treatment individual who is highly educated and black with a control individual who has less than a high-school degree and is white. Results of this process can be found in Table 14.

Table 14: Concentration of non-white individuals in each quintile of the propensity score for the adult population

	Propensity score quintile	% white in treatment group	% white in control group
For long movers	1st	1	0.999
	2nd	0.985	0.979
	3rd	0.322	0.289
	4th	0	0
	5th	0	0
Total		*458*	*20,281*
For short movers	1st	*1*	*1*
	2nd	*1*	*0.996*
	3rd	*0.253*	*0.215*
	4th	*0.083*	*0.053*
	5th	*0.009*	*0.016*
Total		*443*	*20,281*

Total in “For long movers” (top) table refers to total number of adults who made long moves in the treatment group and total number of adults who made long moves in the control group; total in “For short movers” (bottom) table refers to total number of adults who made short moves in the treatment group and total number of adults who made short moves in the treatment group.

Perhaps it would still be preferable to split the population by race as a robustness check, but especially when it comes to long movers, there already too few individuals to further split the sample. Splitting the sample would likely lead to less accurate matches between treatment group and control group individuals.

Propensity score matching results: what is the effect of moving different distances?

Adult disruption outcomes

In Table 15, I observe the disruption outcomes for adults (those between the ages of 25 and 64, inclusive). Making a long-move as a result of Katrina results in a 13.9% increase in likelihood of being unemployed and a 12.2% increase in the likelihood of experiencing a work disruption; making a short-move as a result of Katrina results in a 5.0% increase in the likelihood of being unemployed and 3.7% increase in the likelihood of experiencing a work disruption. Notably, for both long and short moves, the magnitude of the coefficient for unemployment is larger than that for work disruption. This makes intuitive sense: the unemployed outcome takes into account all instances of unemployment, whereas the work disruption outcome is only instances where an individual was previously employed, but no longer. The 12.2% increase in likelihood of facing work disruption (given a long move) is a statistically significant and large result, therefore increasing the probability of the explanation that the majority of unemployment is indeed a direct result of the move itself (i.e. the individuals experiencing unemployment are *not* just more likely to be unemployed in general). The results in columns 2 and 5 support this conclusion.

Unfortunately, the coefficients from the long sample are not directly comparable to those from the short sample, as the pools of people being matched for these analyses are different. For example, long movers are more likely to be black and low income; this is partly corroborated by the descriptive work I perform in Table 12 (Frey et al. 2007). Despite this, it is worth noting that the estimated impacts of moving are larger for the long sample;13.8% vs. 5.0% (unemployment likelihood) and 12.2% vs. 3.7% (work disruption likelihood). While this could be because the types of people who are treated in the long sample are different, these results are also consistent with the notion that long moves cause more disruption. Lastly, noting that long-moves are more likely to be made by non-white individuals, I can also hypothesize that that moving (regardless of distance) has a more detrimental effect for non-whites.

Table 15: Adult disruptions:

	Long movers			Short movers		
	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployed	Unemployed	Work Disruption	Unemployed	Unemployed	Work Disruption
ATET	0.139***	0.144***	0.122***	0.0496***	0.0426***	0.0367***
	(0.0186)	(0.0188)	(0.0172)	(0.0142)	(0.0153)	(0.0128)
N	*20739*	*20739*	*20739*	*20724*	*20724*	*20724*

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Columns 2 and 5 use the same dependent variable (unemployed) as columns 1 and 4, but additionally match on the individual's work status last year.

Young adult disruption outcomes

Secondary results from my analysis can be found in Tables 16 and 17 which indicate the treatment effects on children and young adults, respectively. These results are included because of relevance to my research questions, but with the caveat that the sample size becomes quite small for all age groups outside of the adult sample. For children, only the overage variable for both long and short moves is statistically significant. Making a long-move as a result of Katrina increases the likelihood of a child becoming an overage learner by 3.4%; likelihood increases by 5.5% for short movers. These increases are significant as in 2000, only 2.8% of children from New Orleans are overage learners. Furthermore, 2.7% of children from the control cities are overage learners. However, post-Katrina in 2006, 14.3% of children who were from New Orleans and made short moves were overage learners. This is in contrast to the 4% of children who were from control cities and made short moves and were overage learners. These results are furthermore impressive because they are underestimates. A data constraint of the ACS in the years of interest is that it only reports if an individual is attending any grade in the categories 1-4, 5-8, and 9-12. Therefore, the overage variable being used in this analysis only captures individuals who would be considered overage if they were enrolled in 4th, 8th, or 12th grade.

Again, I cannot directly compare the effects of moving between the long and short samples, but can observe that the magnitude of disruption is larger for the short sample. This is somewhat counterintuitive and contrasts the result that the adult long sample was more likely to experience disruption than the adult short sample. A final observation regarding the children sub-sample is that moving causes no significant decrease in the likelihood of enrollment for those who are 4-5 and no significant increase in the likelihood of dropout for those 15-18. Therefore, it is likely that those who are 6-14 are especially vulnerable to disruption.

Table 16: Young adult disruptions

	Long movers				Short movers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Idle	Enrolled	Unemployed	Work Disruption	Idle	Enrolled	Unemployed	Work Disruption
ATET	0.159***	-0.062	0.201***	0.145***	-0.0099	0.0032	0.0614	0.0597
	(0.0439)	(0.0429)	(0.0476)	(0.0444)	(0.0344)	(0.0610)	(0.0470)	(0.0426)
N	*3388*	*3388*	*3388*	*3388*	*3361*	*3361*	*3361*	*3361*

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Child disruption outcomes

Lastly, in Table 17, I can observe the likelihood of disruption for young adults. Making a long move because of Katrina significantly increases the likelihood of a young adult being idle, unemployed, and facing a work disruption by 15.9%, 20.1%, and 14.5%, respectively. Making a short move because of Katrina does not significantly increase the likelihood of any of the aforementioned outcomes. Interestingly, for both samples, enrollment is not significantly affected by the displacement. This suggests that the significance of the idle coefficient is not predominantly driven by young adults deferring from going to college, but rather from their inability to find work. Notably, the magnitude of disruption is higher than that of adults, suggesting that young adults are perhaps particularly vulnerable (but not when making short moves). This could also be because a young adult's well-being does not have to solely depend on their own income (i.e. parents are presumably also working and can choose to support their children). Therefore, young adults can voluntarily opt to be unemployed, leading to greater observed instances of unemployment than in the case of adults.

Table 17: Childhood disruptions

	Long movers			Short movers		
	(1)	(2)	(3)	(4)	(5)	(6)
	Overage (6-16)	Enrolled (4-5)	Dropout (15-18)	Overage (6-16)	Enrolled (4-5)	Dropout (15-18)
ATET	0.0344*	-0.150	0.0136	0.0552**	0.0921	0.0744
	(0.0198)	(0.0950)	(0.0335)	(0.0274)	(0.111)	(0.0583)
N	*5584*	*1074*	*2141*	*5519*	*1066*	*2118*

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Robustness check results:

In further of my counterfactual and main results, I provide two additional propensity score matching analyses. In the first, I keep my treatment and control group the same but use pre-Katrina, 2000 data. In the second, I use 2006 data but vary designations of treatment and control groups.

2000 Propensity score matching

This process should further support the assumption that, pre-Katrina, individuals the treatment and control groups do indeed look the same. In this specification of propensity score matching, treatment individuals are matched to control individuals on the likelihood of being in New Orleans in the pre-period, based on the same pre-period characteristics that I utilize in my primary regression. If the designated control city individuals are a valid counterfactual, I should see insignificant and/or zero effects of being in the treatment group versus in the control group; this should hold across age groups and across outcomes.

Indeed, for adults (the primary age group of interest), the estimated effect of being in New Orleans prior to Katrina has an insignificant effect on outcomes (Table 18); this suggests that treatment and control group adults are similar. For young adults and children (Table 19 and 20), the results are somewhat complicated. In the case of children, it appears that being in New Orleans increases one's likelihood of being enrolled and decreases one's likelihood of being a high-school dropout. Additionally, there are insignificant differences between the treatment and control groups in terms of the overage outcome. In the case of young adults, moving decreases one's likelihood of being idle, unemployed, and facing a work disruption and increases one's likelihood of being enrolled. Generally, individuals in the treatment group are less likely than individuals in the control group to experience disruption (i.e. negative outcomes) in the pre-Katrina period. These results for young adults and children work in favor of the narrative that moving post-Katrina was indeed disruptive.

In Table 16, I observe that for young adult long movers, likelihood of being idle, unemployed, and facing work disruption all increase post-move (i.e., in comparison to control group individuals, long moving young adults from the treatment group are more likely to experience these negative outcomes). This is in direct contrast to the difference between the treatment and control groups during the pre-Katrina period, where the treatment group is less likely than the control group to experience the negative outcomes of interest). Similarly, in Table 16, I observe that long moving and short moving children see increased likelihood of being overage learners and furthermore face a non-statistically significant different likelihood (vs. control group individuals) of being enrolled or being a high school dropout. Ultimately, the 2000 PSM results in Tables 19 and 20 amplify the claim (from Tables 16 and 17) that moving from New Orleans post-Katrina is disruptive, i.e. moving is so disruptive that it negates the previous gains (or previous indifferences) New Orleans individuals had over control city individuals across outcomes.

Table 18: 2000 PSM adult outcomes

	(1)	(2)
	Unemployed	Work Disruption
ATET	0.0000618	-0.00289
	(0.00225)	(0.00177)
N	105734	105734

Standard errors in parentheses
* $p < 0.1$, $^{**}p < 0.05$, *** $p < 0.01$

Table 19: 2000 PSM young adult outcomes

	(1)	(2)	(3)	(4)
	Idle	Enrolled	Unemployed	Work Disruption
ATET	-0.0260***	0.104***	-0.0227***	-0.0198***
	(0.00627)	(0.0110)	(0.00746)	(0.00574)
N	19344	19344	19344	19344

Standard errors in parentheses
* $p < 0.1$, $^{**}p < 0.05$, *** $p < 0.01$

Table 20: 2000 PSM childhood outcomes

	(1)	(2)	(3)
	Overage	Enrolled	Dropout
ATET	-0.00131	0.0911***	-0.0177***
	(0.00287)	(0.0147)	(0.00647)
N	33251	6312	12174

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

2006 PSM with different designations of treatment and control groups

In this iteration of my main analysis, I consider five additional configurations of treatment and control groups (I hold my treatment and control cities constant, but pick which specific individuals, e.g. movers from the treatment city as the treatment group and non-movers from the control cities as the control group, should be used in propensity score matching). In this way, I am able to consider several slight variations of my primary counterfactual (comparing individuals from New Orleans who moved post-Katrina with all individuals from the control cities, regardless of whether or not they moved). I focus only on the adult age group and find that across variations, my results are robust.

First is the comparison between individuals from New Orleans who moved (treatment) and individuals from New Orleans who did not move (control). In Table 21, I observe that coefficients in this iteration are smaller in magnitude than those from my primary analysis (Table 16); furthermore, the coefficients are only statistically significant for long movers. This can be explained by the fact that non-movers in NOLA are also negatively affected by the storm independent of moving; therefore, the difference is disruption between treatment and control individuals appears to be smaller. Importantly, the difference between the magnitude of the coefficients and lack of significance for short movers suggests that moving is inherently disruptive and this is why I observe disruption at such a large scale when comparing NOLA movers to all control city individuals.

The next two designations are somewhat similar and are fairly straightforward robustness checks. The first of the two compares NOLA movers to control city non-movers. For some, this may seem like the preferable designation, as I am comparing movers with non-movers as opposed to comparing movers with a mix of movers and non-movers. Regardless, in Table 22, I arrive at coefficients that are similar in magnitude and significance to the original treatment group and counterfactual analysis. The second variation of these two compares NOLA movers with all individuals from the control cities as well as NOLA non-movers. In my original designation, I completely exclude non-movers from NOLA from the sample. After adding these individuals back into the sample, and specifically to the control group, I find in Table 23 that results that are almost entirely identical with my main results (which makes sense, especially because NOLA non-movers is such a small population in comparison to the rest of the control group).

The fourth alternative I run compares all movers with all non-movers (regardless of the original city that they live in). I observe significant coefficients across the two outcomes for both long movers and short movers, but additionally find that these coefficients are much smaller in magnitude (than for my original results) (Table 24). This falls in line with an argument I present several times in the paper: when individuals move endogenously, they are often inspired to move by a positive pull-factor. Individuals are not incentivized to move unless the pull-factor is so great that it overshadows the cost of moving. Because this is the case, we often only observe positive outcomes from endogenous moves. This would explain why the difference in likelihood of experiencing a disruption between treatment and control groups is smaller in this iteration. In other words: because the treatment group in this version includes both NOLA-movers (endogenously motivated to move) and control city movers (endogenously motivated to move), the true effect of exogenously moving is underestimated.

The final alternative I run compares control city movers with control city non-movers. While I would expect insignificant or otherwise zero results (because the move is completely endogenous and therefore individuals are choosing when and where to move), I instead find (Table 25) positive and statistically significant coefficients. This indicates that even when individuals are choosing to move (including when and where they move), there are still disruptions in outcomes. Notably, however, these disruptions are smaller than in the case of individuals who are displaced by Katrina. Therefore, I still find evidence for the argument that exogenous moves are more disruptive than endogenous moves.

Table 21: 2006 PSM using NOLA movers vs. NOLA non-movers

	Long movers		Short movers	
	Unemployed	Work Disruption	Unemployed	Work Disruption
ATET	0.117^{***}	0.0973^{***}	0.0193	0.00312
	(0.0265)	(0.0249)	(0.0303)	(0.0281)
N	1171	1171	1156	1156

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Main regression results for adults can be found in Table 15. In summary, individuals who make long moves increase their likelihood of unemployment by 13.9% and likelihood of experiencing a work disruption by 5.0%; individuals who make short moves increase their likelihood of unemployment by 12.2% and likelihood of facing a work disruption by 3.7% (all of these results are statistically significant at the 99% confidence level.

Table 22: 2006 PSM results using NOLA movers vs. control cities non-movers

	Long movers		Short movers	
	Unemployed	Work Disruption	Unemployed	Work Disruption
ATET	0.145^{***}	0.128^{***}	0.0535^{***}	0.0415^{***}
	(0.0186)	(0.0171)	(0.0150)	(0.0130)
N	16573	16573	16558	16558

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
See notes for Table 21.

Table 23: 2006 PSM results using NOLA movers vs. control cities (all) and NOLA non-movers

	Long movers		Short movers	
	Unemployed	Work Disruption	Unemployed	Work Disruption
ATET	0.139^{***}	0.122^{***}	0.0483^{***}	0.0356^{***}
	(0.0183)	(0.0170)	(0.0145)	(0.0127)
N	21452	21452	21437	21437

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
See notes for Table 21.

Table 24: 2006 PSM results using all movers vs. all non-movers

	Long movers		Short movers	
	Unemployed	Work Disruption	Unemployed	Work Disruption
ATET	0.0782***	0.0697***	0.0177***	0.0159***
	(0.00962)	(0.00881)	(0.00475)	(0.00414)
N	18055	18055	20668	20668

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
See notes for Table 21.

Table 25: 2006 PSM results using control cities movers vs. control cities non-movers

	Long movers		Short movers	
	Unemployed	Work Disruption	Unemployed	Work Disruption
ATET	0.0393***	0.0350***	0.0129***	0.0130***
	(0.0102)	(0.00924)	(0.00489)	(0.00426)
N	16884	16884	19512	19512

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
See notes for Table 21.

Propensity score matching results: what is the effect of moving to different types of neighborhoods?

In this part of my analysis, I seek to contextualize my aforementioned descriptive and empirical results by estimating the effect of moving to specific types of new neighborhoods on long and short movers, respectively. It is worth caveating that I am limited in the analysis I can conduct because of sample size. Ideally, I would like to observe how individuals fare by moving to each type of neighborhood that I acknowledge in my descriptive analysis (i.e. higher median income, lower median income, higher mean educational attainment, lower mean educational attainment, higher concentration of own race, lower concentration of own race, in birth state, not in birth state, into a multigenerational home, and not into a multigenerational home). However, there are still not enough individuals (*especially* when I pool by race and *even* when I only look at outcomes for the largest population in my sample, adults) in each category to do so. This remains the case when I combine neighborhoods with either higher median income or higher mean educational attainment to count as a joint neighborhood type called "higher socioeconomic status". In Table 26, I summarize the number of treatment long movers and short movers who move to each type of new neighborhood. Ultimately, for non-white individuals, I evaluate outcomes for those who move to neighborhoods of higher socioeconomic status, lower socioeconomic status, higher non-white population, lower non-white population, into multigenerational homes, and not in multigenerational homes. For white individuals, I am unable to evaluate any specific type of move.

Table 26: Number of treatment long movers and short movers who move to each type of new neighborhood

For non-white individuals in treatment group	Long Move	Short Move
Higher SES	276	188
Lower SES	315	149
Higher white concentration	382	87
Lower white concentration	209	250
To birth state	51	--
Not to birth state	540	--
Into multigenerational home	181	186
Not into multigenerational home	410	277
Total	*591*	*463*

For white individuals in treatment group	Long Move	Short Move
Higher SES	129	90
Lower SES	37	18
Higher non-white concentration	165	107
Lower non-white concentration	1	1
To birth state	43	--
Not to birth state	123	--
Into multigenerational home	34	41
Not into multigenerational home	132	133
Total	*166*	*172*

SES (Socioeconomic status) combines two previously independent characteristics of new neighborhoods: a new neighborhood has higher SES if it has either higher median income than the previous neighborhood or higher mean educational attainment; a new neighborhood has lower SES if it has neither higher median income nor higher mean educational attainment. See notes for Table & for a description of each of these characteristics.

One striking result from this analysis is that for non-white individuals, regardless of the characteristics of their new neighborhood, moving increases likelihood of disruption. More specifically, I find (in Tables 27, 28, 29, and 30) significant and positive estimates (indicating increased likelihood of disruption) for individuals making long and short moves to neighborhoods that are both better and worse in terms of socioeconomic status, racial composition, and multigenerational support within the home. Additionally, these results hold across my two main adult outcomes of unemployment and work disruption.

A second result from this analysis is that even though I am observing different groups of movers when looking at different categories of new neighborhoods (i.e. these effects are not directly comparable— similar to long movers versus short movers), the point estimates are consistent with the argument that there is the greatest disruption for non-white long movers who move to whiter neighborhoods. In general, when comparing the estimates between the same distance of move but to neighborhoods that are better and worse along the same characteristic (whether it be socioeconomic status, racial composition, or multigenerational support within the home), it is worth observing that in general, the standard errors on these estimates are large enough that the confidence intervals of the coefficients would likely overlap. In other words, while I observe that disruption estimates may seem consistently larger for individuals moving to "worse" neighborhoods versus the estimates for individuals moving to "better" neighborhoods, it is likely that these coefficients are not statistically different from one another (although this is hard to test directly in a propensity score matching setting).

In summary: 1.) moving increases likelihood for all types of movers and regardless of the type of new neighborhood the individual is moving to and 2.) non-white long movers who move to white neighborhoods seem to experience the greatest amount of disruption. These results ultimately suggest that there is much work to be done in considering even more types of neighborhoods in order to better understand the mechanisms of disruption.

Table 27: Unemployment outcomes of non-white individuals who make long moves to different types of neighborhoods

	(1)	(2)	(3)	(4)	(5)	(6)
	Higher socioeconomic status	Lower socioeconomic status	Higher non-white population	Lower non-white population	Into multi-generational HH	Not into multi-generational HH
ATET	0.170***	0.136***	0.127***	0.201***	0.152***	0.154***
	(0.0322)	(0.0332)	(0.0266)	(0.0405)	(0.0396)	(0.0271)
N	11160	11144	11199	11105	11081	11223

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Socioeconomic status combines two previously independent characteristics of new neighborhoods: a new neighborhood has higher SES if it has either higher median income than the previous neighborhood or higher mean educational attainment; a new neighborhood has lower SES if it has neither higher median income nor higher mean educational attainment. Unemployment refers to the basic definition; I do not control for employment status last year in this part of my analysis.

Table 28: Work disruption outcomes of non-white individuals who make long moves to different types of neighborhoods

	(1)	(2)	(3)	(4)	(5)	(6)
	Higher socioeconomic status	Lower socioeconomic status	Higher non-white population	Lower non-white population	Into multi-generational HH	Not into multi-generational HH
ATET	0.157***	0.115***	0.120***	0.168***	0.130***	0.140***
	(0.0301)	(0.0299)	(0.0251)	(0.0368)	(0.0366)	(0.0253)
N	11160	11144	11199	11105	11081	11223

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 29: Unemployment outcomes of non-white individuals who make short moves to different types of neighborhoods

	(1)	(2)	(3)	(4)	(5)	(6)
	Higher socioeconomic status	Lower socioeconomic status	Higher non-white population	Lower non-white population	Into multi-generational HH	Not into multi-generational HH
ATET	0.0769***	0.181***	0.125**	0.116***	0.0955***	0.0614**
	(0.0287)	(0.0473)	(0.0527)	(0.0301)	(0.0326)	(0.0249)
N	11114	11072	11039	11147	11108	11164

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 30: Work disruption outcomes of non-white individuals who make short moves to different types of neighborhoods

	(1)	(2)	(3)	(4)	(5)	(6)
	Higher socioeconomic status	Lower socioeconomic status	Higher non-white population	Lower non-white population	Into multi-generational HH	Not into multi-generational HH
ATET	0.0473**	0.151***	0.0815*	0.0915***	0.0594**	0.0503**
	(0.0239)	(0.0420)	(0.0422)	(0.0268)	(0.0264)	(0.0230)
N	11114	11072	11039	11147	11108	11164

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Conclusion:

In this paper I have examined not only where different types of individuals moved post-Katrina, but also how their outcomes varied depending on how far they moved and what type of new neighborhoods they moved to. I find that Katrina-movers were more likely to move long distances than short distances and were *not* necessarily likely to move to neighborhoods of higher socioeconomic status. However, non-white individuals were likely to move to neighborhoods of higher non-white concentration. I also find that adults, young adults, and children all experience varying extents of educational and employment disruption because of moving. Notably, long moving adults and young adults are more likely (than their short moving counterparts) to experience disruption, while short moving children are more likely (than their long moving counterparts) to experience disruption. Additionally, I find that regardless of the type of neighborhood non-white individuals move to, they experience significant disruption. Notably, non-white individuals appear to face greatest disruption (in comparison both white and non-white individuals making all other types of moves) when moving to whiter neighborhoods. Broadly: I find evidence for the claim that endogenously motivated moves are different from exogenously motivated moves. I also find evidence that different types of individuals end up in different types of neighborhoods (either by choice or by some sort of constraint) and face varying levels of disruption because of it.

These results suggest several policy implications. First and foremost, perhaps all relocation housing programs should be more critically evaluated. As evidenced: even when individuals are being moved to neighborhoods of higher socioeconomic status or neighborhoods of higher perceived potential for social networks, they experience disruption. It is not necessarily that I am rejecting Deryugina, Kawano, and Levitt's conclusion that there are long-term positive effects of moving post-Katrina. Rather, I am suggesting that there is a substantial short run transition cost to moving (that individuals are not willing to endure) that perhaps explains why individuals move again, even if staying might be good for their long-run outcomes. A second policy implication is that non-white individuals should not necessarily be moved to neighborhoods of higher socioeconomic status and higher educational attainment (like in the case of the Moving to Opportunity Program) – especially if they are making long moves— because these neighborhoods are likely to also be more white. My results suggest that these types of movers face the greatest disruption. Ultimately, there is a definite trade-off between short term disruption and long term benefits that needs to be further investigated.

More specifically, the results of this study suggest that there needs to be a more thorough consideration of the mechanisms behind disruption. While I provide some evidence for the argument that individuals who are able to quicker and better build social networks in their new neighborhoods face less disruption (i.e. non-white individuals moving to less white neighborhoods), I do not directly measure or test the varying strengths of social networks. Furthermore, there are many additional characteristics of neighborhoods that I have yet to explore (severity of housing discrimination and racial bias, school quality, availability and affordability of public transportation, etc.). Further analysis in this area becomes increasingly important as the US continues to consider and operationalize programs based on the assumed success of moving individuals to “better” neighborhoods.

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