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Julia Latash Date

The Impact of Public Housing Closures on Atlanta Mothers' Residential Trajectories and Health Behaviors during Pregnancy

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Epidemiology

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The Impact of Public Housing Closures on Atlanta Mothers' Residential Trajectories and Health Behaviors during Pregnancy

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Abstract

The Impact of Public Housing Closures on Atlanta Mothers' Residential Trajectories and Health Behaviors during Pregnancy

By Julia Latash

Introduction: Housing is an important determinant of health. Because traditional public housing projects contributed to residential segregation and poverty concentration within neighborhoods, recent U.S. housing policies have encouraged project demolition and shifted residents primarily to portable housing vouchers. There are mixed findings on the health impacts these policy-induced moves had on former project residents.

Methods: We constructed a retrospective cohort of 2,564 women residing in Atlanta, Georgia public housing from 1994 to 2007 who had a birth in traditional public housing, and follow-up birth while residing in either a project or the private market using deterministically-linked birth records of siblings with the same mother. Generalized estimating equations were used to assess the impact of project closure on women's moves to new neighborhoods and important health behaviors including their likelihood to smoke or utilize prenatal care in their second pregnancies. Mediation analyses were conducted to determine whether health behaviors in follow-up pregnancies operated through the magnitude of inter-pregnancy change in neighborhood poverty and deprivation. All analyses were repeated in a propensity score (PS)-matched cohort of 560 women.

Results: Conditional on demographic and other factors, women who experienced project closure moved to neighborhoods with higher percentages of residents in poverty and greater material deprivation. Policy-induced moves were also associated with slight, non-significant protective effects against smoking (adjusted odds ratio (aOR): 0.90, 95% confidence interval (CI): 0.62, 1.31), receiving inadequate prenatal care (aOR: 0.95, 95% CI: 0.76, 1.18), and receiving less than adequate prenatal care (aOR: 0.86, 95% CI: 0.71, 1.04). Health behaviors were not mediated by neighborhood changes. The PS-matched cohort achieved covariate balance, with analyses yielding similar effect estimates.

Discussion: Demolition of housing projects was associated with women's moves to neighborhoods having greater poverty and more deprivation. Policy-induced moves were unassociated with or modestly protective against harmful health behaviors in subsequent pregnancy, but did not operate through changes in neighborhood circumstances. More research on the impact of housing policy changes on health behaviors during pregnancy is warranted.

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Table of Contents

Background

Housing and Health

Housing has long been acknowledged as a key determinant of health. In the 19th Century, urban and sanitary reformers noted that improvements in housing could not only improve the health of individual residents, but of the community as a whole (1). Friedrich Engels, in describing housing in 1840s England, wrote, "The dwellings are bad and filthy, often so much so that they give rise to disease" (2). Crowded and poorly-maintained housing during the Industrial Revolution gave rise to a number of infectious disease, including cholera, tuberculosis, and whooping cough, among others (3). Issues related to health and housing have been brought to the fore at numerous points in the 20th Century as well, from the creation of public housing during the Great Depression, to racial disparities and discrimination in housing in the Civil Rights era and beyond (4).

Recognizing this historic link, much of the evidence base relating housing and health is concerned with the direct ways in which housing impacts human health. Dampness, cold, and mold, have been associated with respiratory health problems, including asthma (5–7), as have household pests such as cockroaches (7,8). Exposure to indoor air pollutants and irritants, including radon, nitrogen dioxide, and tobacco smoke also impact lung function (3,4), and lead pipes and paint have been shown to impair neurological development (9,10). Acknowledging these and other environmental factors linking housing and public health, the National Center for Healthy Housing and American Public Health Association created the National Healthy Housing Standard outlining concrete ways to improve health in homes (11). While the data linking the

physical conditions of homes with health is well-established, a growing body of literature is also concerned with the indirect ways in which housing and health are associated.

Housing, and home ownership in particular, have been found to bestow a sense of privacy and safety, which has important implications for health (12,13). Homes can be seen as "a reflection of self identity and pride, a place of refuge, a site for the exercise of control, [and] a source of social status," with individuals who are unhappy at home or worried about being forced to move reporting poorer physical and mental health than those who felt secure (14). Homeowners also tend to report better mental and physical health (15). In contrast, foreclosure has been associated with high levels of stress, as well as depressive symptoms and overall poor health (16). Moreover, homeless populations have especially poor health, though some health outcomes, such as mental health problems, may precede homelessness, while others, including those due to exposure to the elements, may result from it (3).

Neighborhood Effects and Health

Housing also indirectly impacts health in ways beyond the idea of home and the security it provides. Where housing is located has profound effects on individual and population health. Neighborhood characteristics can impact a wide variety of health outcomes across the socio-economic gradient (17). These include injury, infectious disease, chronic disease, physical activity, mental health outcomes, and birth outcomes, with poorer health outcomes occurring in neighborhoods with lower socio-economic status (SES) (4,18–20). This is partly due to the fact that affordable housing tends to be located "near noise, pollution, and noxious social conditions," and that those with greater

resources can "use those resources to garner health advantages" (21). Moreover, neighborhood-level social deprivation and poor resource allocation contribute to difficulties in accessing necessary health care (22,23). Lower-income individuals experience a number of barriers in accessing medical care, including obtaining insurance coverage, finding providers, accessing providers, the cost of care, and stigma (24,25). Poorer neighborhoods, particularly in urban areas, have also become progressively more isolated from employment, educational, and other opportunities, resulting in what sociologist William Julius Williams called "concentration effects—the effects of concentrated neighborhood poverty on individual residents"(26). In recognition of the complex factors that impact a neighborhood's effects on health or other outcomes, composite indices have been developed to better account for the various social factors affecting neighborhood characteristics.

One such index is the Neighborhood Deprivation Index (NDI), developed by Lynne Messer and others. Acknowledging the breadth of demographic and social domains used to characterize neighborhoods, particularly in the absence of easily-measurable "status" indicators, this index uses Census tract-level data, as tracts are designed to be small and relatively homogenous with respect to living conditions and other features (27). The Census variables incorporated into the NDI include items that have been associated with health outcomes, including poverty, income level, employment status, and crowded housing, among others (27), to reflect the entangled nature of many of these social factors. By operationalizing material deprivation, the NDI not only manages to "more accurately reflect the multidimensional character of community socioeconomic position", it also allows for more generalizable comparisons across

neighborhoods to identify areas of particular deprivation, even in the presence of varying geographic and demographic characteristics (27).

Housing Policy and Neighborhood Effects

This variation in neighborhood deprivation and poverty concentration has been partially attributed to previous housing policies enacted by the U.S. Department of Housing and Urban Development (HUD). Beginning in the 1930s, federal housing policy centered on the development of publicly-owned and -subsidized apartments (28). By the 1980s and 1990s, however, concerns about the concentration of inner-city poverty and residential segregation shifted the focus of housing policy to "help through individual assistance rather than the construction of either project-based housing or scattered site housing" (29). Chief among these newer efforts was the Housing Choice Voucher (HCV) Program (previously, Section 8) in which families lived in housing of their choice, with the voucher paying the difference between their contribution toward rent and actual rent (30). By virtue of their portability, Section 8 vouchers were seen as "enabling low-income families to move from high- to low-poverty neighborhoods [and having] the potential to reduce the levels of income segregation and, as a corollary, the degree of racial separation" at the neighborhood level (29).

In addition, housing mobility policies, whether litigation-driven, such as the Gautreaux Assisted Housing Program in Chicago, or HUD-initiated, such as the Moving to Opportunity (MTO) for Fair Housing Demonstration Program, provided vouchers to residents for relocation to lower-poverty neighborhoods with greater socio-economic diversity (26, 31). In response to continuing concerns about the state of public housing,

and the National Commission on Severely Distressed Public Housing's 1992 report, which noted that public housing could improve residents' environments, Congress passed the Urban Revitalization Demonstration Program (HOPE VI) (26). HOPE VI focused on redeveloping distressed public housing nationwide, primarily through the demolition of housing projects and construction of new units more in line with the local community. In addition to encouraging self-sufficiency for residents, HOPE VI included the development of a social service provision infrastructure for residents, including job training, education, and case management. Moreover, HOPE VI aimed to attract higher-income families to newly-constructed mixed-income housing, allowing lower-income families and families paying market-rate rents to live side-by-side (26). While these policy changes aimed to mitigate and correct the extreme poverty concentration that resulted from past public housing efforts and residential segregation, results are mixed regarding the extent to which relocation to new neighborhoods resulted in quality-of-life improvements for low-income residents.

The Moving to Opportunity program, by randomly selecting families to receive vouchers for moves to lower-poverty neighborhoods, created opportunities to examine neighborhood differences among similarly-situated families, as well as the downstream effects of neighborhood on health. This experimental program allowed for the evaluation of causal questions related to residential and social mobility's effects on a number of outcomes (32), typically a difficult undertaking. Previous research on MTO families has not found broad improvements in physical health among adults who moved to neighborhoods with less poverty, though these moves were associated with improvements in adults' mental health (33), possibly due to reduced stress and lessened

fears of violence, as well as higher rates of self-reported well-being (34). Among children, moves to neighborhoods with less poverty were associated with less risky behavior and improved mental and physical health outcomes for female youth but adverse outcomes among male youth, and with improved outcomes among children who moved before age 13, compared with youth who moved when they were older (32,33). Moreover, while the HOPE VI program facilitated traditional public housing residents' moves to neighborhoods with less poverty and violent crime, the destruction of housing projects and residents' subsequent relocation may also have contributed to the loss of supportive social networks, stigmatization from new neighbors (and its accompanying stress), less access to affordable foods or other items, and declines in access to health care providers, including doctors that accept Medicaid or safety-net providers such as Federally Qualified Health Centers (35,36).

Research Question

The observed mixed effects of low-income families' moves to improved neighborhoods underscore the need to better understand residents' experiences with social and residential mobility and, in particular, whether policy efforts to deconcentrate poverty are having the intended downstream health effects on residents. Understanding the mechanism by which these ensuing health outcomes occur has not been fully explored in the extant housing mobility literature. Moreover, previous findings that such moves may benefit younger children more than older children suggest that a life course perspective is warranted when determining the effect that housing mobility has on health outcomes.

To do so, this analysis will consider a retrospective, administratively-defined cohort of women residing in Atlanta, Georgia public housing who had at least one birth occurring between 1994 and 2007. This time period encompasses a period during which housing projects owned and run by the Atlanta Housing Authority (AHA) were demolished and transitioned to mixed-income housing, with most residents shifted to Housing Choice Vouchers. Because of the nature of project closure, this study can be viewed as a natural experiment in which some residents were "randomized" to move to a new neighborhood, whereas others were unaffected by policy and could choose to remain in public housing or voluntarily leave. The study cohort was created with maternal longitudinally-linked live-birth data, and cohort eligibility determined by a birth while residing in traditional AHA-administered housing projects. Women were subsequently followed for successive births and residences at the time of those births. Using this cohort, exposed women were defined as those with a birth in public housing within one year before the project's closure date, and follow-up birth while residing in another AHA property or in private housing. Unexposed women, in contrast, had a birth in public housing, with a subsequent birth in either the same public housing project or in a different location, but with a transition out of public housing that was not temporally connected to that project's demolition. This analysis aims to address the following questions:

1. Did women who left traditional public housing between pregnancies as a result of housing policy changes move to areas with less poverty and less material deprivation compared with women unaffected by housing policy changes who either voluntarily left public housing or who remained in public housing?

2. Were women's moves due to these housing policy changes associated with differences in adequacy of prenatal care utilization or their proclivities to smoke during subsequent pregnancies?

3. To what degree were (possible) changes in smoking rates or prenatal care adequacy related to, and mediated by, changes in neighborhood poverty and deprivation?

Answering these questions will add to the body of research examining associations between housing and health outcomes of low-income individuals, and the effects of place on health. Moreover, determining whether the poverty deconcentration goals of housing project demolition were met, and whether these changes resulted in subsequent improvements in health, could have implications for the future successes or the continuation of such policies.

Methods

Data

Atlanta women who had at least one live birth while residing in traditional public housing from 1994 to 2007 were followed for subsequent births. This cohort was constructed via deterministically-linked birth records, from the Georgia Department of Public Health, of siblings with the same mother. Using this maternally-linked birth data, a mother's residential history could be created across her successive deliveries, allowing each maternal residence listed on a birth certificate to be geocoded. Geocoded addresses were then compared with maps of Atlanta Housing Authority-administered public housing projects in order to determine which births occurred in public housing. Additional information regarding spatial and record linkage may be found elsewhere (37).

Women's eligibility for the cohort was established with their first births in AHA-administered housing projects and their next measured births and residences (*i.e.*, a consecutive two-birth sequence, where the baseline birth was in public housing). Women with inconsistently-coded races across multiple births were excluded, as were women with improbably-timed births (births occurring less than six months apart), and women with implausibly recorded parity. Because 98% of the women were black, the cohort was further restricted to black women, and only those births for which maternal residential address could be reliably geocoded to the street address or Census block group-level were included. In addition, because so few follow-up births occurred in mixed-income developments, only follow-up births occurring in traditional public housing or in private-market residences were considered (Figure 1).

Exposure and Outcomes

To determine whether moves to new neighborhoods in response to the demolition of traditional public housing affected subsequent health behaviors during pregnancy, a woman was considered exposed to a policy-induced move if she gave birth within one year before a project's closure and had her follow-up birth in private housing or in another AHA-administered project. Alternatively women could remain in AHA public housing projects for both pregnancies, or could have moved out of AHA public housing projects at a time unrelated to the policy-induced closure and demolition.

Health behaviors during women's follow-up (second) pregnancies were the outcomes of interest, and included smoking during pregnancy (yes, no) and adequacy of prenatal care, as measured by the Kotelchuck Index. The Kotelchuck Index, which categorizes prenatal care adequacy into four levels (inadequate, intermediate, adequate, adequate-plus) is based on the combination of a woman's month of entry into prenatal care, as well as the number of prenatal care visits she attended compared with the expected number of visits as recommended by the American College of Obstetrics and Gynecology (38).

Mediators and Covariates

To further understand the extent to which possible differences in health behaviors might be explained by change in contextual neighborhood environments resulting from women's housing transitions, between-pregnancy changes in Neighborhood Deprivation Index and Census tract-level percentages of residents living in poverty were considered possible intermediaries along the hypothesized causal path from housing project closure

to health behaviors (Figure 2). NDI was standardized so a value of 0 represented the average Census tract deprivation in 2000 in the tracts in which women were observed, with each unit representing a one-standard deviation change. Negative NDI values corresponded to Census tracts of lower deprivation, while positive NDI values corresponded to Census tracts with higher deprivation. Changes in NDI and poverty were calculated by subtracting baseline-birth values for tract-level NDI and poverty percentage from the second-birth values. A positive change in either NDI or poverty meant that women moved to areas of greater deprivation or poverty, while a negative change in NDI or poverty indicated that women moved to areas will less deprivation or poverty.

Moreover, to account for secular changes in policies, neighborhood characteristics, and health outcomes, year of birth, centered on 2000, was also included in all regression models. Other considered covariates included type of housing transition (public-public, public-private), as well as maternal age in years, educational attainment (less than high school graduate/GED, high school graduate/GED, some college), marital status (married, unmarried), inter-pregnancy interval (greater than 6 months, less than or equal to 6 months), parity (2-3 children, 4 or more children), and payer for delivery (Medicaid, other payer).

Analytic Methods

A challenge of neighborhood-effects research is that the "selection" of people into neighborhoods (*i.e.*, the individual characteristics of residents that make them more likely to live in a certain neighborhood) might be so particular to a given area that finding

comparable individuals in other areas becomes increasingly difficult. Termed "structural confounding," this phenomenon violates the assumptions of positivity and exchangeability between exposed and unexposed populations that are necessary for causal inference (39). Because the closure of traditional public housing projects in Atlanta was somewhat randomized, however, in this study, women's "forced" moves out of public housing are less likely to have been affected by these selection and confounding concerns, allowing for the consideration of causal questions.

Causal mediation analysis was used to help clarify the extent to which women's forced housing transitions were associated with their health behaviors during subsequent pregnancies. Decomposing the total effect of involuntary housing mobility on smoking behaviors and adequacy of prenatal care into direct and indirect effects not only allowed for greater consideration of the experiences of mothers who moved because of policy changes, but also posits that neighborhood change could be a mechanism through which these potential health changes occur.

To that end, linear regression models were used to determine the effect of project closure on changes in tract-level poverty and NDI, a relationship constituting one component of the indirect effect. A series of generalized estimating equations (GEE), which accounted for Census tract clustering, were used to estimate the direct effect of forced mobility on pregnancy health behaviors, and results of these analyses were compared with the results from fitting generalized linear models (GLM) that did not account for this clustering. Lastly, using the mediation analysis methods outlined by Linda Valeri and Tyler VanderWeele, which allow for non-linear outcomes and

mediators in addition to possible exposure-mediator interaction (40), the indirect effect characterizing how housing closure and health behaviors relate to neighborhood characteristics and poverty was estimated (Figure 2). In addition to conducting these analyses with the entire cohort of women, analyses were repeated with a 1-to-1 nearest-neighbor propensity score-matched cohort. Women were matched based on the log-odds of their probability of exposure using a "greedy match" algorithm, which involved "round[ing propensity scores] to 5 significant figures and randomly selecting pairs that match exactly on this score. For the unmatched subjects, the score is then rounded to 4 significant figures and exact matches selected, with the process continuing until subjects are matched to 1 significant figure" (41).

All analyses were conducted using SAS version 9.4 (SAS Institute, Cary, NC). PROC FREQ, PROC MEANS, and PROC UNIVARIATE were used to calculate descriptive statistics, while PROC GENMOD was used for linear regressions and logistic and log-binomial regressions (both GEE and GLM models). Calculation of indirect effects used the SAS mediation macro developed by Valeri and VanderWeele (40), and propensity-score matching was achieved with a SAS macro developed by Lori Parsons (42).

Results

Descriptive statistics

The original maternally-linked dataset contained 14,275 births to mothers residing in Atlanta, GA for at least one birth in the 1994-2007 period. 11,423 births were excluded in order to identify women's second births after their first births (baseline/qualifying births) in Atlanta Housing Authority-administered housing projects. Of the remaining 2,852 births, an additional 288 births were excluded based on housing transition type (*i.e.*, moves from public housing to mixed-income housing between subsequent births), maternal race, quality of residential geocode, and implausible recorded parities. A 560-birth subset of the 2,564 births in the analysis cohort was created using 5-to-1 "greedy" propensity-score matching (Figure 1).

Characteristics of women in the analysis cohort may be found in Table 1. Women exposed to project closure were, on average, two years older than those unexposed to project closure (age 26 vs. age 24) and were much more likely to be living in private-market housing (82% vs. 49%) than in other AHA-administered housing projects for their second deliveries. Women who experienced policy-induced moves were also more likely to have graduated from high school compared with women unaffected by housing policy changes (48% vs. 41%) and more likely to have some college education (13% vs. 8%). While marital status and parity were comparable across both groups of women, those who were not exposed to project closures were more likely to have had inter-pregnancy intervals of less than 6 months (14% vs. 2%) and were more likely to have Medicaid as the payer for their deliveries than exposed women (66% vs. 50%). While the covariate balance was different between exposed and unexposed women in the analysis cohort,

balance was achieved after propensity-score matching (Table 2, Figure 3), as indicated by standardized differences of less than 0.1 between groups (43). The standardized difference is used in propensity-score matching to compare means and prevalences of covariates between treatment groups (43).

Research Question 1: Project Closure and Neighborhood Change

The distributions of change in Neighborhood Deprivation Index and percent poverty at the Census-tract level in the analysis and propensity score-matched cohort may be found in Tables 3 and 4. In unadjusted linear regressions, policy-induced moves were associated with moves to Census tracts with less poverty; on average, exposure to a forced move was associated with moves to tracts in which the percentage of residents living in poverty was nearly 5 percentage points lower than women's original tracts (*i.e.*, those in baseline housing project residences). Forced moves were also associated, on average, with second-birth residences in tracts with less material deprivation than baseline-birth tracts (0.77 standard deviations from average 2000 tract-level NDI lower than at women's cohort-qualifying births).

However, after adjusting for the type of housing transition, maternal age, marital status, educational attainment, delivery payer, parity, inter-pregnancy interval, and year of delivery, women who were exposed to housing project closures ended up moving to neighborhoods with greater poverty and deprivation. This change in direction of the association was primarily driven by conditioning on the type of housing transition women experienced. Forced moves were associated with women's subsequent residences in tracts where the percentage of residents living in poverty was, on average, 4.34

percentage points higher than in their housing-project tract, and neighborhood deprivation was 1.41 standard deviations from 2000 average deprivation higher than their original tracts. Backwards elimination was then used to find most parsimonious model, by successively dropping covariates until the resulting magnitude of the effect of project closure on change in neighborhood poverty or NDI was no longer within 10% of the magnitude of the changes in the fully-adjusted models. Following backwards elimination, project closure was associated with an increase in neighborhood poverty of 4.00 percentage points and an increase in neighborhood deprivation of 1.38 standard deviations from the 2000 average. Similar effects were found in analyses of the propensity score (PS)-matched cohort—an increase in neighborhood poverty of 4.81 percentage points, and increase in neighborhood deprivation of 1.26 standard deviations from the 2000 average. In all models, housing closure was a significant predictor of changes in neighborhood (p-values ≤ 0.01) (Table 5).

Research Question 2: Project Closure and Pregnancy Health Behaviors

The distributions of smoking status and adequacy of prenatal care (the four Kotelchuck Index categories) for the analysis and propensity score-matched cohorts may be found in Tables 6 and 7. Because the prevalence of smoking during pregnancy was relatively rare, ranging from 10%-12% among women in both cohorts, logistic regression was used to model the association between traditional public housing project closure and smoking. To assess the effect of policy-induced moves on prenatal care, the Kotelchuck Index was dichotomized in a number of ways. First, the association between forced moves and inadequate prenatal care (*i.e.*, inadequate vs. the combination of intermediate, adequate, and adequate-plus) was examined. Second, project closure's association with

less than adequate care (*i.e.*, inadequate and intermediate vs. adequate and adequate plus) was modeled. Lastly, because more intensive prenatal care services are associated with higher-risk pregnancies (38), the association between forced moves and extensive prenatal care (*i.e.*, adequate-plus vs. adequate) was also explored. For each of these comparisons, because the prevalence of the prenatal care services of interest exceeded 20%, log-binomial models regressions were used. For all health-outcome analyses, generalized estimating equations (GEE) were used to account for clustering at the Census-tract of the cohort-defining birth. Analyses were later repeated without accounting for this clustering using generalized linear models (GLM) to determine whether clustering affected estimates of the risk ratios (RRs) and odds ratios (ORs), along with their respective 95% confidence intervals (CIs) comparing women exposed to project closure and those unexposed to closure.

In unadjusted GEE analyses, housing closure was very slightly protective (OR: 0.93, 95% CI: 0.66, 1.32) against women's propensity to smoke during pregnancy. In the fully-adjusted GEE model, the protective effect of closure was amplified (OR: 0.90, 95% CI: 0.62, 1.31), and in the most parsimonious model, achieved through backward elimination of covariates, policy-induced moves were even more strongly protective (OR: 0.86, 95% CI: 0.60, 1.24). In analysis of the propensity score-matched model, the association between project closure and smoking during subsequent pregnancies was essentially null (OR: 0.97, 95% CI: 0.60, 1.56). Identical effect estimates, with slightly wider confidence intervals, were found using GLM analyses. In all instances, effect estimates were fairly small with wide confidence intervals, and exposure to project

closure was not significantly associated with smoking at the 5% significance level (Table 8).

The association between forced moves and receiving inadequate prenatal care was protective in unadjusted GEE analyses (RR: 0.87, 95% CI: 0.70, 1.08), though the protective effect largely disappeared after conditioning on covariates completely (RR: 0.95, 95% CI: 0.76, 1.18) and in the most parsimonious model (RR: 0.95, 95% CI: 0.76, 1.18). Similar null findings were found in analyses of the PS-matched cohort (RR: 0.95, 95% CI: 0.70, 1.28). As with the smoking analyses, GLM regressions found identical effect estimates with slightly wider confidence intervals, and across all models, exposure to project closure was not a significant predictor of receiving inadequate prenatal care (Table 9).

Women's residential moves resulting from the demolition of AHA-administered projects were protective against receiving less than adequate (*i.e.*, inadequate or intermediate) prenatal care in unadjusted GEE analyses (RR: 0.82, 95% CI: 0.67, 0.99). However, after adjusting for confounding, this protective effect was slightly attenuated in both the fully-adjusted (RR: 0.86, 95% CI: 0.71, 1.04) and most parsimonious (RR: 0.86, 95% CI: 0.70, 1.04) models, with a similar finding in the propensity score-matched cohort (RR: 0.87, 95% CI: 0.68, 1.12). GLM models not accounting for Census-tract clustering once again found similar effect measures and similarly wide confidence intervals (Table 10).

There was no association between policy-induced moves and receiving extensive (adequate-plus) prenatal care services, regardless of whether GEE analyses were

unadjusted (RR: 0.98, 95% CI: 0.83, 1.16), fully-adjusted (RR: 0.97, 95% CI: 0.83, 1.15), most parsimonious (RR: 0.97, 95% CI: 0.82, 1.14), or conducted with the PS-matched cohort (RR: 1.04, 95% CI: 0.84, 1.29). These findings also held in GLM analyses (Table 11).

Research Question 3: Effect Decomposition

Because similar effect estimates and confidence intervals were found in project closure-pregnancy health behavior analyses, regardless of whether regression models accounted for Census-tract clustering, the Valeri and VanderWeele mediation SAS macro (40) was used to determine the extent to which these associations were mediated by changes in neighborhood poverty and deprivation, without further adjustment for clustering. Moreover, because for all analyses marital status was not a confounder in the data, it was not included as a confounder when determining indirect effects.

In both the analysis and propensity score-matched cohorts, the effect of policy-induced moves on smoking in pregnancy or adequacy of prenatal care did not appear to be mediated by neighborhood changes in either Census-tract percent poverty or Neighborhood Deprivation Index. The percentages of the total effect of project closure on these health behaviors during pregnancy mediated by changing neighborhood effects ranged from 0% to 6% (Table 12).

Discussion

Among black women who gave birth while living in Atlanta Housing Authority-administered housing projects from 1994 to 2007, those who were forced to move out between their first births in traditional public housing and their subsequent deliveries (due to housing project demolition) were more likely to move to neighborhoods with more material deprivation and a higher percentage of residents living in poverty, conditional on maternal age, educational attainment, marital status, parity, second-birth payer, inter-pregnancy interval, type of housing transition, and year of child's birth, compared with black women who either remained in public housing or chose to move to the private market for reasons unrelated to housing policy changes. Moreover, while there appeared to be no effect of policy-induced moves on women's tendency to receive extensive prenatal care, and only modest protective effects against receiving inadequate prenatal care, there were more pronounced, though still statistically insignificant, protective effects against smoking during pregnancy and receiving less than adequate prenatal care. Because forced moves were more protective against receiving inadequate or intermediate care than against receiving inadequate care alone, this may suggest that this greater effect was driven by fewer women receiving intermediate prenatal care if they moved as a result of project demolition. The associations between forced moves and women's health behaviors during their subsequent pregnancies did not appear to be related to changes in the degree of material deprivation or percentage of residents living in poverty from their project-birth neighborhood to their subsequent-birth neighborhood.

These seemingly contradictory findings—that women both moved to poorer or more deprived neighborhoods, and that their moves were modestly protective against

certain harmful pregnancy behaviors—may be explained in a number of ways. It is possible that safety-net health care providers were more likely to be located in neighborhoods with more deprivation, and that women's moves to these neighborhoods subsequently allowed for them to access prenatal care more easily. In addition, it may be that in this population, neighborhood context or poverty was unrelated to smoking, consistent with previous findings that, while neighborhood poverty level impacted men's propensities to smoke, the same could not be said of women (44). It is also possible that, through use of Housing Choice Vouchers, women were able to move closer to family members, enhancing their degree of social support, or that women were able to escape "draining" or unsupportive social relationships in housing projects, reducing stress (45) and possibly, by extension, their likelihood to smoke. Alternately, the covariate imbalance in the analysis cohort may explain these differing results. The finding that women moved to worse neighborhoods was primarily driven by conditioning on housing transition type. While half of the unexposed women moved from a public housing project to another housing project, fewer than 20 percent of exposed women moved to another project (Table 1).

Strengths and Limitations

This study has a number of strengths. Deterministic record-linkage allowed for the creation of women's partial residential trajectories, and provided richer, more complete information about women's pregnancies over time than standalone birth records could have. Additionally, the "natural experiment" of traditional public housing closure in Atlanta allowed for a pseudo-randomized exposure, which partially mitigated many of the concerns about well-defined interventions, exchangeability of populations, and

population positivity that often plague neighborhood-effects studies. In combination, these two facets of this study contributed to one of its greatest strengths—the ability to approximate causal inference related to the effect of policy-induced residential moves on health behaviors during pregnancy without concerns about reverse causation or structural confounding.

Moreover, the use of propensity-score matching in addition to traditional regression and confounding assessment allowed for consideration of violation of assumptions for causal inference including assurance of positivity between exposed and unexposed groups, and providing an additional means for control of confounding. Causal effects may be identified from observational data only when the conditions of exchangeability, positivity, and a well-defined intervention are met (46). These criteria are closest to being satisfied when considering the propensity score-matched population. Housing closures were not differently applied to residents who were subsequently forced to move, satisfying the well-defined intervention criterion. After propensity score-matching occurred, women exposed and unexposed to project closure were balanced on the confounding factors of interest, as indicated by standardized differences of less than 0.1 for nearly all variables (Table 2), and thus represented exchangeable populations on all measured covariates. Moreover, the nearly identical distributions of propensity scores among women exposed and unexposed to housing demolition in the PS-matched population (Figure 3) demonstrate that the positivity assumption held. Because model selection is subject to a certain degree of subjectivity, the fact that analyses of the propensity score-matched cohort yielded similar findings to those in the larger analysis

cohort lends support both to the findings from the traditional regressions and to the causal inferences made based on those findings.

Even with these strengths, a number of limitations remain. Chief among these are the data quality concerns inherent in all vital records-based research. The health behavior outcomes in this study, smoking and utilization of prenatal care, are items captured on birth certificates that are particularly vulnerable to misclassification and other validity concerns (47). In addition, while the deterministic linkage method used to create women's residential trajectories was highly specific, it is still possible that records were incorrectly linked. The linkage process, therefore, may have created additional opportunities for misclassification (48). Also, the last AHA-administered housing projects were demolished in 2010, yet the vital records used in these analyses were current only through 2007. If the experiences of women living in and giving birth in traditional public housing during the last three years of their operation—as well as women's corresponding health behaviors during their pregnancies subsequent to those in 2008-2010—were meaningfully different from the experiences of women in the analysis cohort, selection bias may be of concern. Moreover, because the women in the analysis cohort were "captured" during a time period that included the transition from the 1989 birth certificate revision to the 2003 revision, potential control variables for analyses were restricted to those that existed across revisions. Furthermore, factors such as women's underlying health status or insurance status during pregnancy may represent unmeasured confounders of the exposure-outcome or mediator-outcome relationships.

In addition, because there was no way to identify women's exact move dates from birth records, there may be exposure misclassification. While births within one year of closure were used to define women who were forced to move (exposed women), some women may have voluntarily moved before closure, and were therefore unexposed. Alternately, women who gave birth more than one year from closure, but who remained in projects until the closure date would be classified as unexposed women, when in fact they did experience forced moves. Furthermore, while exposed women were considered those who moved due to project demolition, unexposed women included both those who did not move, and those who moved for reasons unrelated to policy changes. While both types of unexposed women made voluntary housing transitions (*i.e.*, they were either non-mobile or were not forced to leave), the women who stayed in public housing may have been different from those who left public housing. There were inadequate numbers of women in each of these groups to explore possible differences in their experiences, though these differences might have been meaningful.

Aside from the possible sources of bias outlined above, because it is unclear how much time would be required for a new neighborhood to affect individuals' health, it is also possible that the time between housing project closure and women's subsequent births was insufficient to impact behavior changes. If this were the case, the observed (small) protective effects against smoking or less than adequate prenatal care utilization would not be due to project closure, but rather due to other factors, including possibly chance.

Assuming that the time lag between the closure of traditional public housing projects and women's next pregnancies was sufficient to influence health behaviors in pregnancy, and conditional on the demographic factors and secular trends outlined earlier, black women who gave birth while living in Atlanta housing projects tended to move to neighborhoods with higher levels of poverty and greater material deprivation if they moved as a result of project demolition, compared with those who did not experience forced housing closures. These policy-induced moves out of housing projects were also marginally, though not significantly, protective against certain harmful health behaviors in subsequent pregnancy, but the effects of forced moves on these health behaviors were not mediated by changes in neighborhood characteristics. Future analyses should consider additional years of data during which women may have been impacted by project closures, as well as possible linkage to other data sources, including hospital discharge or other medical records to gain a fuller understanding of factors that might also impact pregnant women's propensities to receive adequate prenatal care or smoke. In addition, comparing the experiences of Atlanta mothers with mothers in other cities that demolished housing projects could aid in understanding the extent to which these findings are generalizable to the broader population of former public housing residents.

Public Health Importance

Recognizing that people's health is affected by "the resources and supports available in our homes, neighborhoods, and communities," the U.S. Department of Health and Human Services (HHS) included the social determinants of health as one of the 42 topic areas comprising *Healthy People 2020* (49). Yet even before its official incorporation into HHS's most recent 10-year plan outlining benchmarks for population health improvement, housing was understood as an important factor impacting well-being. Acknowledging that neighborhood circumstances and affordable housing as well as physical conditions inside homes influence health outcomes, recent U.S. housing policy has shifted from the construction of projects—which contributed to racial segregation in housing and neighborhood-level disparities in accessing opportunity—to individual assistance, usually in the form of portable housing vouchers. Evaluation of these efforts, including the Moving to Opportunity Demonstration Program (MTO) and the Urban Revitalization Demonstration Program (HOPE VI), looked not only at the types of neighborhoods residents moved to using vouchers, but also at the downstream health impacts moves had on those receiving federal housing support.

The mixed findings on the health impacts of these moves out of housing projects and into (most often) the private rental market is what motivated this study. Following passage of legislation authorizing HOPE VI, the Atlanta Housing Authority began demolishing housing projects and transitioning residents to mixed-income housing and vouchers (37). Examining the health outcomes Atlanta residents living in AHA-administered properties experienced subsequent to the initiation of project closure can

therefore add to the current literature examining the impact of these housing policies and enhance understanding of the residential and health trajectories residents experienced.

For the black mothers followed in this study, the destruction of traditional public housing projects did not appear to result in their moves to neighborhoods with lower poverty and less material deprivation, in contrast to the policy's ostensible aims. Though women's forced moves from housing projects resulted in small protective effects against smoking and receiving less than adequate care during pregnancy, these changes did not appear to operate through changes in neighborhood composition. These findings add to and underscore the previous mixed results of other housing policy shift evaluations, and highlight the fact that the ensuing implications of these changes for residents defy simple characterization or narratives.

Future research on this cohort of women should consider all pregnancies during the period of project closure in Atlanta, to fully capture the impact that HOPE VI had on women's health behaviors. In addition, the experiences of similarly-situated mothers in other HOPE VI or MTO cities should be studied to determine whether the above findings are consistent across demonstration sites, or whether variation exists in the effect of project closures on pregnant women's health behaviors in pregnancies following their forced moves. This work would both provide insight into the health of economically disadvantaged populations, and may help guide future policies aimed at improving the health of low-income populations through intervention on one of the social determinants of health.

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Figure 1. Flowchart of analytic sample selection; Atlanta, GA (1994-2007).

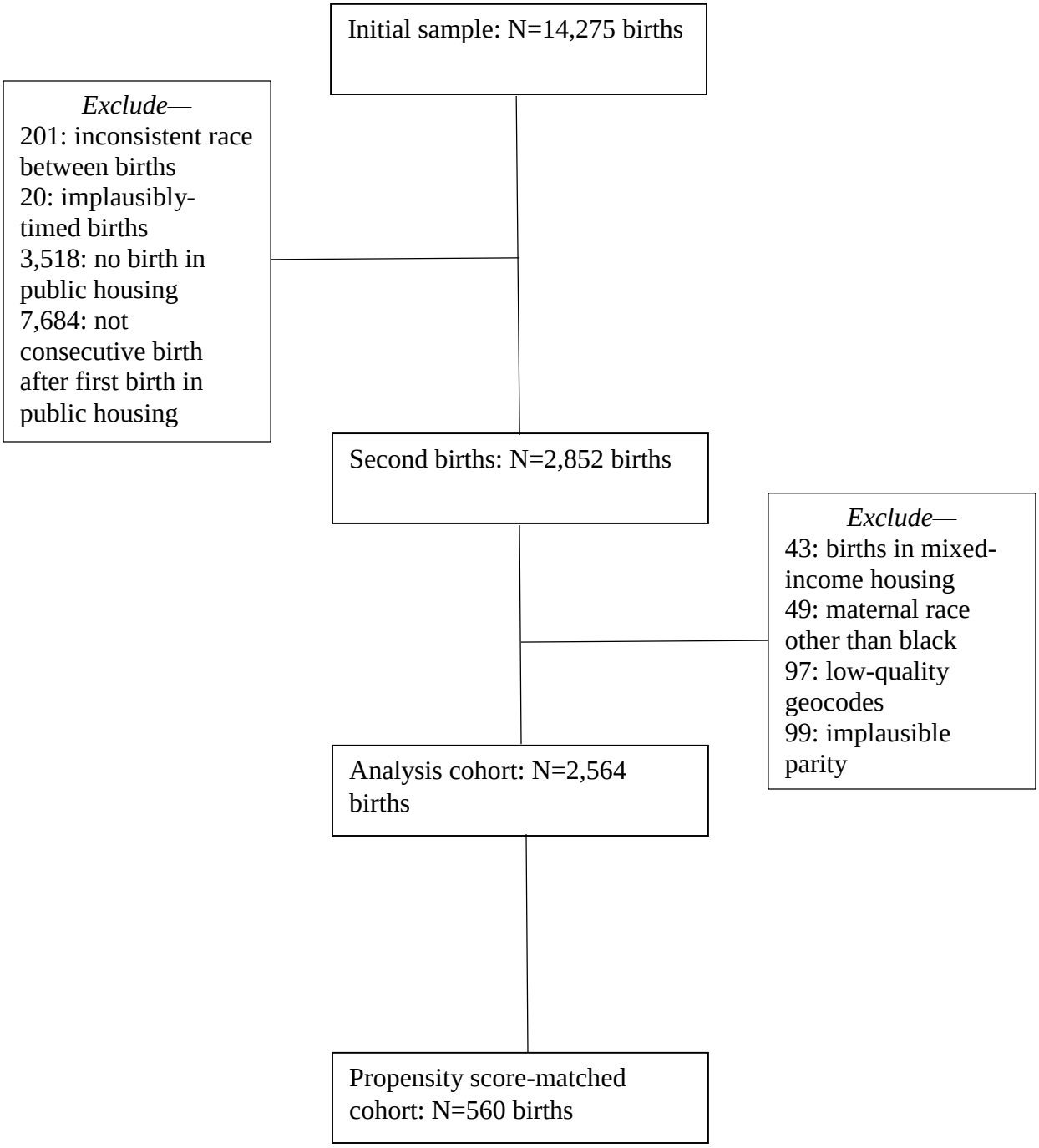


Figure 2. Hypothesized directed acyclic graph (DAG) for the association between public housing project closure and pregnancy behaviors, mediated by changes in neighborhood characteristics; Atlanta, GA (1994-2007).

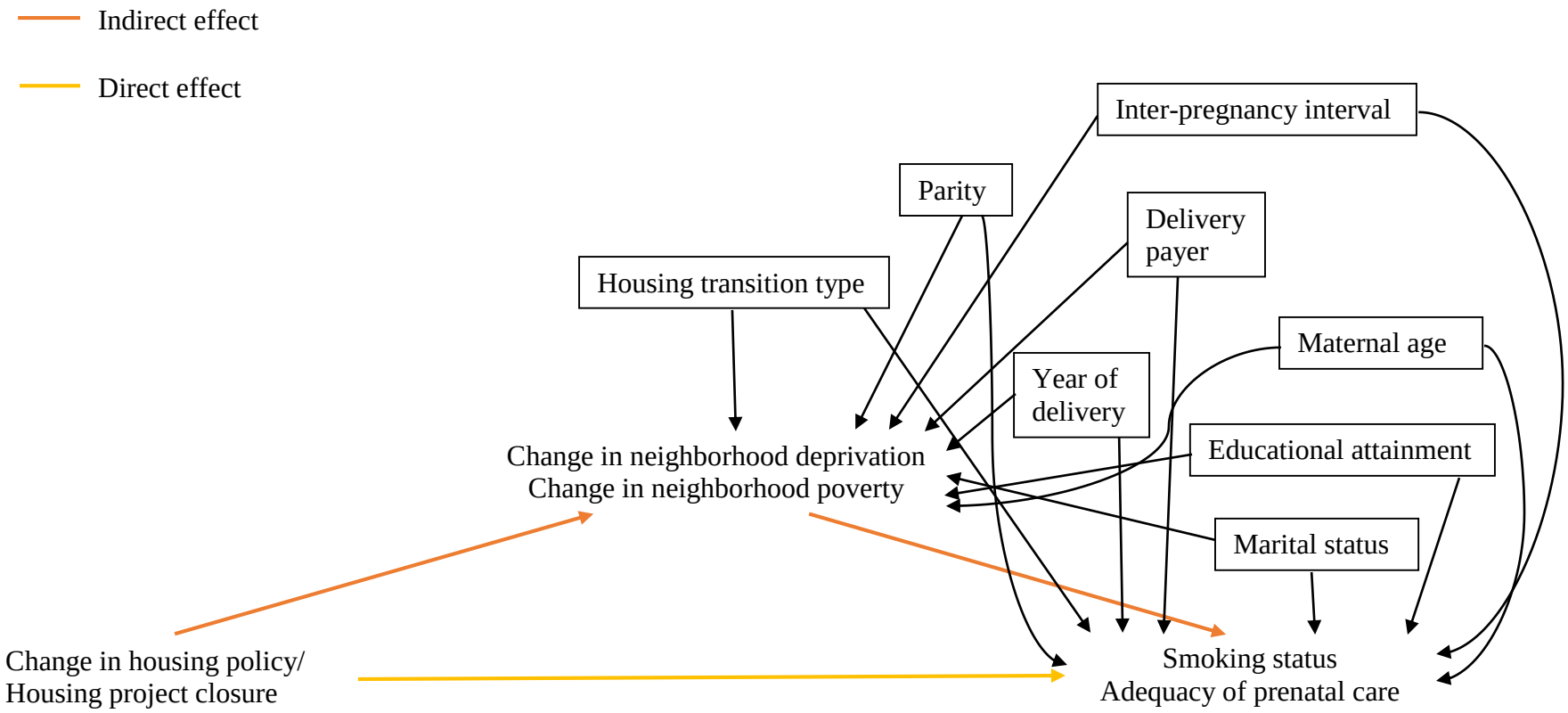


Table 1. Characteristics of women exposed and unexposed to public housing project closure in analysis cohort; Atlanta, GA (1994-2007).

Characteristic	Unexposed to Project Closure (N = 2,268)	Exposed to Project Closure (N = 296)	Standardized Difference[1]
Age – mean ± SD	23.68 ± 4.75	25.96 ± 4.80	0.48
Housing transition type – n (%)			
Public - public	1,152 (50.79)	54 (18.24)	0.73
Public - private	1,116 (49.21)	242 (81.76)	0.73
Educational attainment level – n (%)			
Less than high school/GED	1,153 (51.36)	115 (39.38)	0.24
High school/GED	917 (40.85)	139 (47.60)	0.14
At least some college	175 (7.79)	38 (13.02)	0.18
Missing	23	4	
Marital status – n (%)			
Married	106 (4.68)	14 (4.73)	0.00
Unmarried	2,161 (95.32)	282 (95.27)	0.00
Missing	1	0	
Child's birth year – n (%)			
1994	4 (0.18)	0 (0.00)	0.06
1995	132 (5.82)	0 (0.00)	0.35
1996	192 (8.47)	12 (4.05)	0.16
1997	213 (9.39)	20 (6.76)	0.08
1998	250 (11.02)	19 (6.42)	0.14
1999	210 (9.26)	25 (8.45)	0.02
2000	205 (9.04)	30 (10.14)	0.03
2001	217 (9.57)	25 (8.45)	0.03
2002	167 (7.36)	19 (6.42)	0.03
2003	155 (6.83)	18 (6.08)	0.03
2004	144 (6.35)	14 (4.73)	0.06

2005	151 (6.66)	16 (5.41)	0.04
2006	128 (5.64)	51 (17.23)	0.28
2007	100 (4.41)	47 (15.88)	0.29
Parity – n (%)			
2-3 children	1,397 (62.14)	194 (65.99)	0.08
4 or more children	851 (37.86)	100 (34.01)	0.08
Missing	20	2	
Inter-pregnancy interval – n (%)			
6 months or less	322 (14.20)	7 (2.36)	0.44
Greater than 6 months	1,946 (85.80)	289 (97.64)	0.44
Payer for delivery – n (%)			
Medicaid	1,504 (66.31)	148 (50.00)	0.34
Non-Medicaid	764 (33.69)	148 (50.00)	0.34

1. For continuous covariates, the standardized difference is:

$$d = \frac{(\overline{x}_{treatment} - \overline{x}_{control})}{\sqrt{\frac{s^2_{treatment} + s^2_{control}}{2}}}$$

For categorical covariates, the standardized difference is:

$$d = \frac{(\hat{p}_{treatment} - \hat{p}_{control})}{\sqrt{\frac{\hat{p}_{treatment}(1 - \hat{p}_{treatment}) + \hat{p}_{control}(1 - \hat{p}_{control})}{2}}}$$

Differences of less than 0.1 indicate a negligible difference in mean or prevalence

(Austin PC. An Introduction to Propensity Score Methods for Reducing the Effects of Confounding in Observational Studies. *Multivariate Behav. Res.* 2011;46(3):399–424)

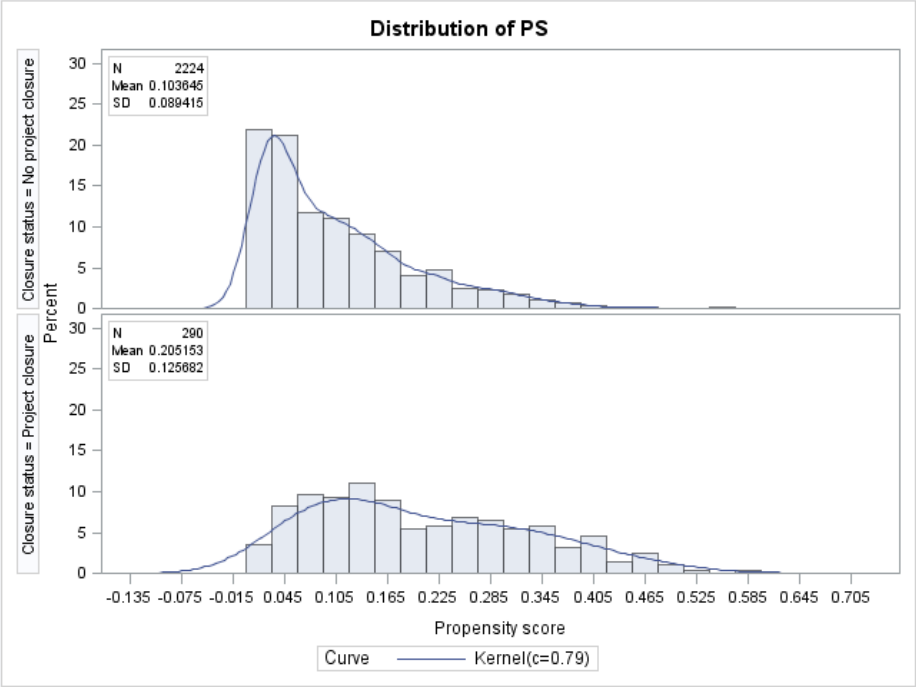
Table 2. Characteristics of women exposed and unexposed to public housing project closure in propensity score-matched cohort; Atlanta, GA (1994-2007).

Characteristic	**Unexposed to Project Closure** (N = 280)	**Exposed to Project Closure** (N = 280)	**Standardized Difference**
Age – mean ± SD	25.86 ± 5.42	25.66 ± 4.68	0.04
Housing transition type – n (%)			
Public - public	52 (18.57)	53 (18.93)	0.01
Public - private	228 (81.43)	227 (81.07)	0.01
Educational attainment level – n (%)			
Less than high school/GED	116 (41.43)	113 (40.36)	0.02
High school/GED	126 (45.00)	133 (47.50)	0.04
At least some college	38 (13.57)	34 (12.14)	0.04
Marital status – n (%)			
Married	21 (7.50)	14 (5.00)	0.09
Unmarried	259 (92.50)	266 (95.00)	0.09
Child's birth year – n (%)			
1994	0 (0.00)	0 (0.00)	0.00
1995	4 (1.43)	0 (0.00)	0.17
1996	9 (3.21)	11 (3.93)	0.03
1997	19 (6.79)	20 (7.14)	0.01
1998	22 (7.86)	19 (6.79)	0.03
1999	24 (8.57)	25 (8.93)	0.01
2000	25 (8.93)	29 (10.36)	0.04
2001	22 (7.86)	25 (8.93)	0.03
2002	22 (7.86)	19 (6.79)	0.03
2003	19 (6.79)	17 (6.07)	0.02
2004	27 (9.64)	12 (4.29)	0.18
2005	32 (11.43)	13 (4.64)	0.22
2006	33 (11.79)	46 (16.43)	0.11
2007	22 (7.86)	44 (15.71)	0.19

Parity – n (%)			
2-3 children	191 (68.21)	182 (65.00)	0.06
4 or more children	89 (31.79)	98 (35.00)	0.06
Inter-pregnancy interval – n (%)			
6 months or less	6 (2.14)	7 (2.50)	0.02
Greater than 6 months	274 (97.86)	273 (97.50)	0.02
Payer for delivery – n (%)			
Medicaid	145 (51.79)	144 (51.43)	0.01
Non-Medicaid	135 (48.21)	136 (48.57)	0.01

Figure 3. Propensity score (PS) balance in analysis and propensity-score matched cohorts; model includes year of delivery, maternal age, housing transition type, educational attainment level, marital status, parity, inter-pregnancy interval, and delivery payer.

A. Analysis cohort; Atlanta, GA (1994-2007).



B. Propensity score-matched cohort; Atlanta, GA (1994-2007).

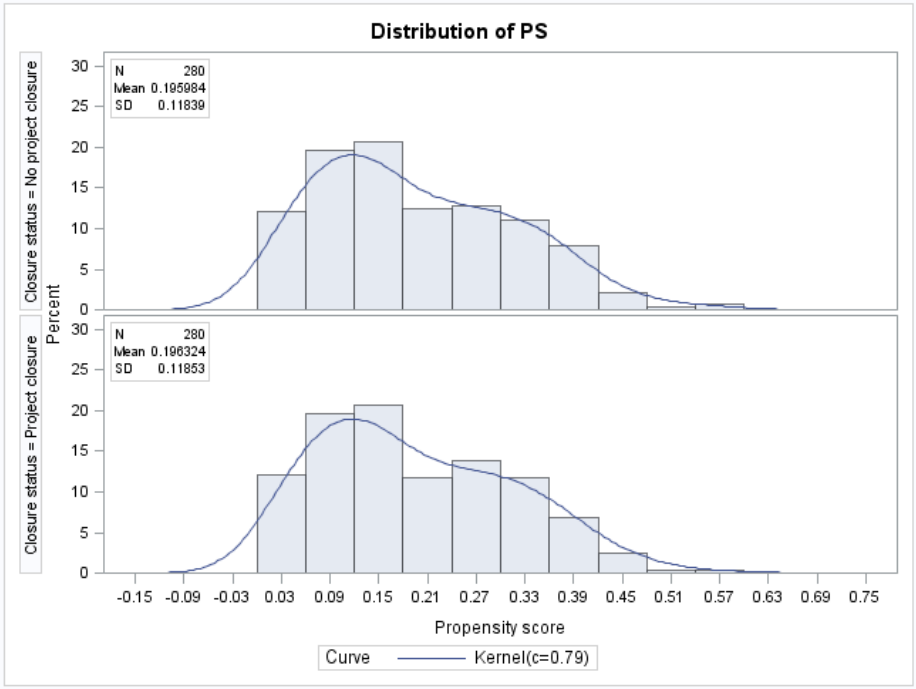


Table 3. Distributions of measures of neighborhood change among women exposed and unexposed to housing project closure in analysis cohort; Atlanta, GA (1994-2007).

Outcome	Unexposed to Housing Closure (N = 2,268)	Exposed to Housing Closure (N = 296)
Change in % neighborhood poverty between births – mean ± SD	-0.20 ± 0.23	-0.25 ± 0.23
Change in % neighborhood poverty between births – five-number summary		
Minimum	-0.90	-0.73
Q1	-0.36	-0.42
Median	-0.13	-0.26
Q3	-0.03	-0.10
Maximum	0.63	0.35
Change in Neighborhood Deprivation Index between births (normalized to average 2000 NDI) – mean ± SD	-3.18 ± 3.98	-3.95 ± 4.85
Change in Neighborhood Deprivation Index between births (normalized to average 2000 NDI) – five-number summary		
Minimum	-16.09	-14.54
Q1	-5.80	-7.29
Median	-1.79	-4.27
Q3	-0.31	-0.77
Maximum	11.02	9.19

Table 4. Distributions of measures of neighborhood change among women exposed and unexposed to housing project closure in propensity score-matched cohort; Atlanta, GA (1994-2007).

Outcome	**Unexposed to Housing Closure** (N = 280)	**Exposed to Housing Closure** (N = 280)
Change in % neighborhood poverty between births – mean ± SD	-0.28 ± 0.23	-0.24 ± 0.23
Change in % neighborhood poverty between births – five-number summary		
Minimum	-0.88	-0.68
Q1	-0.45	-0.41
Median	-0.28	-0.24
Q3	-0.10	-0.07
Maximum	0.36	0.35
Change in Neighborhood Deprivation Index between births (normalized to average 2000 NDI) – mean ± SD	-5.02 ± 4.08	-3.75 ± 4.83
Change in Neighborhood Deprivation Index between births (normalized to average 2000 NDI) – five-number summary		
Minimum	-16.09	-14.54
Q1	-7.89	-7.14
Median	-4.92	-4.10
Q3	-1.57	-0.67
Maximum	7.35	9.19

Table 5. Estimated changes in neighborhood poverty and deprivation from baseline birth in public housing to subsequent birth, comparing women exposed and unexposed to public housing project closures, across models; Atlanta, GA (1994-2007).

Neighborhood change	**Unadjusted Model** Estimate (p-value for housing closure)	**Fully-Adjusted Model**[1] Estimate (p-value for housing closure)	**Most Parsimonious Model** Estimate (p-value for housing closure)	**PS Model** Estimate (p-value for housing closure)
Percent poverty	-4.95% (0.0005)	4.34% (0.0003)	4.00% (0.0008)[2]	4.81% (0.01)
NDI standard deviations	-0.77 (0.0022)	1.41 (<0.0001)	1.38 (<0.0001)[3]	1.26 (0.0008)

1. Adjusted for housing transition type, year of delivery, maternal age, educational attainment, marital status, inter-pregnancy interval, delivery payer, and parity
2. Adjusted for housing transition type, year of delivery, maternal age, and inter-pregnancy interval
3. Adjusted for housing transition type, year of delivery, and maternal age

Table 6. Distributions of prenatal care adequacy and smoking status during pregnancy among women exposed and unexposed to housing project closure in analysis cohort; Atlanta, GA (1994-2007).

Outcome	**Unexposed to Housing Closure** (N = 2,268)	**Exposed to Housing Closure** (N = 296)
Smoking status – n (%)		
Non-smoker	1,978 (88.58)	249 (89.25)
Smoker	255 (11.42)	30 (10.75)
Kotelchuck Index – n (%)		
Inadequate	592 (28.03)	63 (23.77)
Intermediate	234 (11.08)	20 (7.55)
Adequate	631 (29.88)	91 (34.34)
Adequate-plus	655 (31.01)	91 (34.34)

Table 7. Distributions of prenatal care adequacy and smoking status during pregnancy among women exposed and unexposed to housing project closure in propensity score-matched cohort; Atlanta, GA (1994-2007).

Outcome	**Unexposed to Housing Closure** (N = 280)	**Exposed to Housing Closure** (N = 280)
Smoking status – n (%)		
Non-smoker	243 (88.69)	234 (88.97)
Smoker	31 (11.31)	29 (11.03)
Kotelchuck Index – n (%)		
Inadequate	64 (25.20)	60 (23.90)
Intermediate	29 (11.42)	20 (7.97)
Adequate	84 (33.07)	86 (34.26)
Adequate-plus	77 (30.31)	85 (33.86)

Table 8. Estimated odds ratios (ORs) and 95% confidence intervals (CIs) for smoking during pregnancy, comparing women exposed and unexposed to public housing project closures across models and model types; Atlanta, GA (1994-2007).

Model Type	**Unadjusted Model** OR (95% CI)	**Fully-Adjusted Model[1]** OR (95% CI)	**Most Parsimonious Model[2]** OR (95% CI)	**PS Model** OR (95% CI)
GEE	0.93 (0.66, 1.32)	0.90 (0.62, 1.31)	0.86 (0.60, 1.24)	0.97 (0.60, 1.56)
GLM	0.93 (0.63, 1.40)	0.90 (0.58, 1.36)	0.86 (0.56, 1.32)	0.97 (0.57, 1.66)

Table 9. Estimated risk ratios (RRs) and 95% confidence intervals (CIs) for inadequate prenatal care, comparing women exposed and unexposed to public housing project closures across models and model types; Atlanta, GA (1994-2007).

Model Type	**Unadjusted Model** RR (95% CI)	**Fully-Adjusted Model[1]** RR (95% CI)	**Most Parsimonious Model[3]** RR (95% CI)	**PS Model** RR (95% CI)
GEE	0.87 (0.70, 1.08)	0.95 (0.76, 1.18)	0.95 (0.76, 1.18)	0.95 (0.70, 1.28)
GLM	0.87 (0.69, 1.09)	0.95 (0.75, 1.19)	0.95 (0.75, 1.19)	0.95 (0.70, 1.29)

1. Adjusted for housing transition type, year of delivery, maternal age, educational attainment, marital status, inter-pregnancy interval, delivery payer, and parity
2. Adjusted for year of delivery and maternal age
3. Adjusted for inter-pregnancy interval

Table 10. Estimated risk ratios (RRs) and 95% confidence intervals (CIs) for less than adequate prenatal care, comparing women exposed and unexposed to public housing project closures across models and model types; Atlanta, GA (1994-2007).

Model Type	**Unadjusted Model** RR (95% CI)	**Fully-Adjusted Model[1]** RR (95% CI)	**Most Parsimonious Model[2]** RR (95% CI)	**PS Model** RR (95% CI)
GEE	0.82 (0.67, 0.99)	0.86 (0.71, 1.04)	0.86 (0.70, 1.04)	0.87 (0.68, 1.12)
GLM	0.82 (0.68, 0.98)	0.86 (0.71, 1.04)	0.86 (0.71, 1.03)	0.87 (0.68, 1.11)

Table 11. Estimated risk ratios (RRs) and 95% confidence intervals (CIs) for extensive prenatal care, comparing women exposed and unexposed to public housing project closures across models and model types; Atlanta, GA (1994-2007).

Model Type	**Unadjusted Model** RR (95% CI)	**Fully-Adjusted Model[1]** RR (95% CI)	**Most Parsimonious Model[3]** RR (95% CI)	**PS Model** RR (95% CI)
GEE	0.98 (0.83, 1.16)	0.97 (0.83, 1.15)	0.97 (0.82, 1.14)	1.04 (0.84, 1.29)
GLM	0.98 (0.84, 1.15)	0.97 (0.83, 1.15)	0.97 (0.83, 1.13)	1.04 (0.83, 1.30)

1. Adjusted for housing transition type, year of delivery, maternal age, educational attainment, marital status, inter-pregnancy interval, delivery payer, and parity
2. Adjusted for inter-pregnancy interval
3. Adjusted for delivery payer

Table 12. Estimated natural indirect effects (odds ratios or risk ratios, and 95% confidence intervals) for the effect of project closure on health behaviors during pregnancy, by neighborhood change; Atlanta, GA (1994-2007).

Health Behavior	**Change in Poverty**		**Change in Neighborhood Deprivation Index**	
	Analysis Cohort OR/RR (95% CI)	***PS-Matched Cohort*** OR/RR (95% CI)	***Analysis Cohort*** OR/RR (95% CI)	***PS-Matched Cohort*** OR/RR (95% CI)
Smoking	1.00 (0.97, 1.03)	1.02 (0.96, 1.08)	1.02 (0.96, 1.08)	1.06 (0.97, 1.15)
Inadequate prenatal care	1.02 (1.00, 1.03)	1.02 (0.98, 1.05)	1.01 (0.98, 1.04)	1.02 (0.97, 1.07)
Less than adequate prenatal care	1.02 (1.00, 1.04)	1.02 (0.99, 1.06)	1.03 (1.01, 1.05)	1.04 (1.00, 1.08)
Extensive prenatal care	1.01 (1.00, 1.02)	1.02 (0.99, 1.05)	1.03 (1.00, 1.05)	1.02 (0.99, 1.05)