***American Journal of Epidemiology* Submitted Manuscript**

**Title:** A matched analysis of the association between federally-mandated smoke-free housing policies and health outcomes among Medicaid-enrolled children in subsidized housing, 2015-2019, New York City

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***Editor's note:*** *An invited commentary on this article is forthcoming.*

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**Funding:** This work was supported by the National Cancer Institute, National Institutes of Health (grant R01 CA220591). Andrea R. Titus was supported by an Agency for Healthcare Research and Quality Training Grant (T32HS026120).

**Data Availability Statement:** The data set is not publicly available.

**Thanks:** N/A

**Conference presentation:** N/A

**Preprint Information:** N/A

**Disclaimer:** The views and opinions expressed in this article are those of the author(s) and do not necessarily reflect the official policy or position of the New York State Department of Health. Examples of analysis performed within this article are only examples. They should not be utilized in real-world analytic products.

**Conflict of Interest:** The authors report no conflicts.

**Running Head:** Smoke-free housing and pediatric health outcomes

**Key words:** smoking; secondhand smoke; asthma; respiratory infections; housing; public policy





## Abstract

Smoke-free housing policies are intended to reduce the deleterious health effects of secondhand smoke (SHS) exposure, but there is limited evidence regarding their health impacts. We examined associations between implementation of a federal smoke-free housing rule by the New York City Housing Authority (NYCHA) and pediatric Medicaid claims for asthma, lower respiratory infections (LRIs), and upper respiratory infections (URIs) in the early post-policy period. We used geocoded address data to match children living in tax lots with NYCHA buildings (exposed to policy) to children living in lots with other subsidized housing (unexposed to policy). We constructed longitudinal difference-in-differences models to assess relative changes in monthly rates of claims between November 1, 2015 and December 31, 2019 (policy introduction was July 30, 2018). We also examined effect modification by baseline age group (0-2, 3-6, 7-15). In NYC, introduction of a smoke-free policy was not associated with lower rates of Medicaid claims for any outcomes in the early post-policy period. Exposure to the smoke-free policy was associated with slightly higher than expected rates of outpatient URI claims (IRR=1.05, 95% CI=1.01, 1.08), a result most pronounced among children ages 3-6. Ongoing monitoring is essential to understanding long-term health impacts of smoke-free housing policies.

Exposure to secondhand smoke (SHS) is associated with a range of pediatric health outcomes, including respiratory infections, middle ear disease, cough, phlegm, asthma, and wheeze-related illnesses.[1] Despite substantial declines in the prevalence of SHS exposure in the U.S. in recent decades, levels of SHS exposure among children remain high. According to biomarker data from the National Health and Nutrition Examination Survey (NHANES), the prevalence of SHS exposure among children ages 3-11 was nearly 40% in 2017-2018.[2] Moreover, children and adults from lower-income households are considerably more likely to be exposed to SHS than individuals in higher-income households.[2,3] Prior research has established that housing environments are critical determinants of SHS exposure prevalence and disparities. Youth who live with two or more smokers are more than three times as likely to be exposed to SHS, compared to youth who do not live with a smoker.[4] In addition, adults and children in multifamily housing are at increased risk of SHS exposure compared to those in single-family homes, in part due to transfer of SHS between smoking and nonsmoking apartments.[5–8]

Given the central role of home environments in shaping SHS exposure patterns, many municipalities and housing developments across the U.S. have introduced smoke-free housing policies. In 2016, the Department of Housing and Urban Development (HUD) issued a nationwide rule that required all public housing agencies – organizations that receive federal financing to manage rental housing developments for low-income families[9] – to implement smoke-free policies in housing developments by July 30, 2018.[10] The potential impact of the HUD rule is substantial, given that nearly one million households in the U.S. reside in public housing developments.[9] Yet, evidence to date on the effectiveness of smoke-free policies in multifamily housing is mixed. Single-city evaluations using self-reported data have generally found that smoke-free policies in multifamily housing are associated with decreases in SHS exposure, including in nonsmoking households.[11–13] However, a survey-based study of six U.S. communities found that smoke-free policies were associated with reduced SHS incursions among residents in market-rate housing but not in subsidized housing.[14] Evaluations using objective measures of SHS exposure (e.g., airborne nicotine and fine particulate matter, or $PM_{2.5}$)

are also inconsistent. Within Philadelphia public housing developments, a smoke-free policy was associated with a significant decrease in airborne nicotine in public areas in the year following policy implementation,[15] while a smoke-free policy in Norfolk, VA public housing developments was associated with increased $PM_{2.5}$ and airborne nicotine one year after the policy was introduced.[16] Two studies in Boston Housing Authority (BHA) developments found that smoke-free policies were associated with lower levels of $PM_{2.5}$ in households[6] and common areas.[17] However, a third evaluation of BHA smoke-free policies found that airborne nicotine in apartments without resident smokers declined at a similar rate in BHA and comparison developments after policy implementation.[18] In New York City, changes in airborne nicotine in common areas, stairwells, and nonsmoking apartments one year following the implementation of a smoke-free policy were not significantly different in NYC Housing Authority (NYCHA) buildings, compared to control areas in another type of subsidized housing (the section 8 voucher program).[19]

While evidence regarding smoke-free housing and air quality is somewhat equivocal, even less is known about associations between smoke-free policies in public housing developments and SHS-attributable health outcomes. We are aware of only one health outcome-focused evaluation of a smoke-free public housing policy within the U.S. A survey-based analysis conducted among Colorado public housing residents before and after the implementation of a smoke-free policy found that there was a significant decrease in self-reported breathing problems after the policy was introduced.[20] Observed changes in other health outcomes did not reach statistical significance.[20] Studies of other policy interventions targeting SHS exposure (e.g., state and local smoke-free laws) suggest that these restrictions may have significant impacts on pediatric health outcomes. For example, a meta-analysis focused on smoke-free laws in workplaces and public areas found that these policies were associated with a 10% reduction in pediatric hospitalizations for asthma.[21] Rigorous studies to further our understanding of the associations between smoke-free housing policies and health outcomes are critically needed,[22] as these policies have the potential to impact longstanding disparities in respiratory health morbidity across housing environments.[23,24]

In this study, we sought to evaluate the impact of the HUD rule in NYC by examining associations between the introduction of a HUD-compliant smoke-free policy in NYCHA developments and subsequent changes in pediatric health outcomes for SHS-sensitive conditions (asthma, lower respiratory infections (LRIs) and upper respiratory infections (URIs)) using Medicaid claims data. The NYCHA policy offers a unique opportunity to employ a quasi-experimental evaluation approach, given the sizeable population of individuals residing in NYCHA buildings (subject to the HUD rule) and also in other types of subsidized housing in NYC (not subject to the HUD rule). In evaluating the NYCHA smoke-free policy, we contribute to the nascent literature on the potential impact of smoke-free housing policies in public housing on downstream pediatric health outcomes.

## METHODS

**Sample.** We linked NYC Medicaid claims data to borough-block-lot parcels (BBLs),[25] which represent unique tax lot identifiers. Linkages were based on residential addresses of Medicaid recipients in Medicaid eligibility data. Addresses are updated when enrollees in the state's social welfare programs move, as well as annually or semi-annually upon enrollment renewal. We constructed a cohort of children enrolled in Medicaid as of November 1, 2015 who either 1) lived in BBLs that contained NYCHA developments or 2) lived in BBLs that contained other

HUD-financed programs (e.g., the section 8 housing voucher program), property tax incentive programs tied to the provision of low-income housing, zoning initiatives, or other city and state housing subsidization programs. We excluded BBLs with subsidized housing associated with the Mitchell-Lama program, as it targets moderate and middle-income families rather than lower-income families.[26] BBLs containing any type of subsidized housing were identified using information from the NYU Furman Center's CoreData.nyc, updated in 2019.[27] We excluded addresses linked to BBLs that were not classified as residential (e.g., commercial and office buildings), as well as BBLs that were comprised of one- and two-family buildings, cooperatives, luxury types, and buildings that were constructed after the year 2000, according to NYC Primary Land Use Tax Lot Output (PLUTO) data.[28] We restricted the cohort to children ages 15 or under at baseline, so that all children in the cohort would remain age-eligible for Children's Medicaid at the time of the policy intervention (July 2018). Finally, we excluded children dually eligible for Medicare and Medicaid, given the small number of children (n<100) in this group. We followed the cohort through December 31, 2019.

**Outcomes.** Outcomes were monthly individual-level counts of health care encounters in the emergency department (ED) and non-ED outpatient settings with any diagnosis code associated with asthma, LRIs, or URIs. We selected these outcome conditions based on their established associations with SHS exposure.[1] Conditions were defined using ICD-10 code maps from the Global Burden of Disease Study (Web Table 1).[29]

**Smoke-free housing policy.** The smoke-free housing policy was implemented by NYCHA on July 30, 2018. Per HUD regulations, the policy prohibited the smoking of specified tobacco products in all housing developments, including residential units, indoor common areas, administrative offices, and within a 25-foot perimeter of buildings.[10] Consistent with an intention-to-treat approach, all children with a residential address linked to a NYCHA development were considered exposed to the smoke-free housing policy as of August 1, 2018, irrespective of actual policy implementation experience. Children in other types of subsidized housing were considered unexposed to the policy. Given the policy's implementation date, the study period included nearly 3 years (33 months) of pre-policy data and nearly 1.5 years (17 months) of post-policy data.

**Matching and covariates.** Children in tax lots comprised of NYCHA developments were matched to children living in tax lots with other types of subsidized housing at baseline. We conducted the matching in three stages. In the first stage, we conducted 5:1 nearest neighbor Mahalanobis distance (MD) matching with replacement, within groups exactly matched on age category (0-2, 3-6, 7-15), race/ethnicity (non-Hispanic White, non-Hispanic Black, non-Hispanic Asian, Hispanic, other non-Hispanic, unknown), sex (male, female), and disability/blindness status. Within these matched groups, MD matching was conducted using the following characteristics measured at baseline: age (continuous), presence of any chronic disease diagnoses, presence of a building with at least seven floors in the BBL, and several characteristics of the population in the census block group (CBG), including percent of households with poverty income ratios (PIRs) of less than 1.00 and between 1.00 and 1.24; percent Black, Hispanic; percent ages 0-17, 18-49, and 50+; and population density. We also matched on the number of Medicaid enrollment months over the entire pre-policy intervention period and on an estimated propensity to live in a NYCHA development derived from a model using the above variables, with the exception of age group. Individual-level variables were obtained from Medicaid data, while BBL and CBG measures were obtained from 2018 PLUTO data and 5-year (2014-2018) estimates from the American Community Survey. We chose not to match on measures of health care utilization in the pre-policy intervention period given concerns about differential regression to the mean in matched difference-in-differences (DiD) analyses.[30] Nearest neighbor matches were restricted to the area of common support. In order to improve matching within age subgroups, we used a LASSO procedure to identify important interactions between the age subgroup and the above variables included in the MD match.[31] In the final stage, we re-estimated the propensity score model with the matched data, identified interaction terms, and calculated overlap weights to further balance covariate distributions across groups.[31] To assess the effectiveness of the matching approach we compared standardized differences of overlap-weighted means. As expected (since overlap weights are known to achieve near-perfect balance),[31] absolute values of all standardized mean differences were less than 0.04 for the full sample and less than 0.08 within age groups.

**Statistical analysis.** We conducted a DiD analysis using generalized estimating equations (GEE) models with a negative binomial distribution and incorporating a first-order autoregressive correlation structure, robust standard errors, and overlap weights. DiD modeling relies on the assumption that outcome trends in the post-policy period in the comparison group are a valid counterfactual for outcome trends in the treatment group in the absence of the policy.[32] This assumption is not empirically testable, but we visually examined the extent to which outcome trends across the two groups in the pre-policy period appeared to be parallel. Regression models included indicators for treatment status (NYCHA vs. comparison), time (pre- vs. post-policy intervention), and their interaction. We examined the statistical significance of the interaction term at the 0.05 level to determine whether outcome trends in the NYCHA group were statistically different from outcome trends in the comparison group in the post-policy period. Models also included all covariates used in the propensity score estimation and matching, with the exception of total months of Medicaid enrollment. In addition to estimating models for the full sample, we examined the potential for effect modification by age, stratifying the sample by age category at baseline (ages 0-2, 3-6, and 7-15). This effect modification analysis was specified a priori, given evidence of the potential for differential drivers of asthma outcomes across age ranges among children.[1]

We conducted several sensitivity analyses. First, to address the possibility that associations were in part due to differential Medicaid disenrollment and/or churn between the groups, we estimated models excluding child-months when a child was not enrolled in Medicaid. We constructed two additional matched cohorts to include only: 1) children who were continuously enrolled in Medicaid over the course of the study period (with an allowable one month gap), and 2) children who remained in the same BBL throughout the study period. We also assessed whether our results differed if we defined outcomes based on primary diagnosis code, rather than all diagnosis codes. We estimated models excluding data from a 3-month "washout" period following policy implementation, which entailed using post-intervention data from November 1, 2018 onwards, instead of August 1, 2018 onwards. Finally, we conducted two sensitivity analyses to assess the robustness of our results to unmeasured, potentially confounding characteristics of the housing environment (e.g., general aging and deterioration of buildings). First, we performed a negative control outcome anlaysis,[33] where we examined associations with a housing-sensitive condition (injuries)[34] that we would not expect to be impacted by the smoke-free policy. Second, we conducted a “placebo test”, in which we specified the post-policy intervention period as August 2017-July 2018 (one year before the actual policy introduction date). Significant associations with outcomes in the placebo year would suggest that our main analysis results could be due to residual confounding.

Data processing was conducted in SAS version 9.4 (SAS Institute, Inc., Cary, North Carolina). All analyses were conducted in Stata version 16.0 (StataCorp LP, College Station, Texas). This study was reviewed and approved by the NYU Langone Health Institutional Review Board.

**RESULTS**

Descriptive statistics for the unmatched and matched samples are included in Table 1, and descriptive statistics by age subgroup are included in Web Tables 2-4. Prior to matching, children in NYCHA buildings tended to be older and were more likely to have a disability or to be blind, compared to children in other types of subsidized housing. Children in NYCHA buildings were also more likely to be non-Hispanic Black and less likely to be non-Hispanic White or to have unknown race/ethnicity status. Finally, the groups differed with regard to area-level characteristics. Children in the NYCHA sample were more likely to live in BBLs with a high-rise building (7+ floors), and to live in CBGs with a lower mean proportion of individuals ages 18-49, a higher proportion of individuals ages 50+, a higher proportion of Black individuals, and a higher proportion of households with PIRs <1.0 or 1.00-1.24. Population density was slightly higher in CBGs with other subsidized housing, compared to CBGs with NYCHA developments. These differences likely reflect that the “other subsidized housing” comparison group captures a relatively broad range of housing programs and a more heterogeneous group of households, compared to the sample in NYCHA developments.

Table 2 includes monthly mean visits per 1000 children associated with each outcome condition in the pre- and post-policy periods. Children in NYCHA developments tended to experience slightly higher encounter rates, with the most pronounced differences for asthma visits. In the pre-policy period, children in NYCHA developments experienced an average of 2.8 additional asthma-associated outpatient visits and 1.7 additional asthma-associated ED visits per 1,000 children each month, relative to children in the comparison group. Web Table 5 includes weighted counts of encounters for each condition before and after the policy was introduced. Rates for all outcomes in the pre-policy period (aggregated by quarter), which we used to assess the extent to which outcome trends were parallel between the groups, are plotted in Web Figures 1-3. Because this analysis was a longitudinal assessment of a fixed cohort, enrollment in Medicaid declined over time within our sample, and consequently visit rates tended to decline over time. Approximately 70% of the original cohort was enrolled in Medicaid at the end of the study period (Web Figure 4).

Table 3 includes DiD coefficient estimates associated with the treatment*time interaction for each outcome in each care setting in fully adjusted models. We observed a statistically significant coefficient with regard to URIs in the outpatient setting (incidence rate ratio (IRR)=1.05, 95% confidence interval (CI)=1.01,1.08), which suggests that rates in the post-policy intervention period among children in NYCHA developments were slightly higher than what would have been expected, given trends in the comparison group. We did not observe statistically significant associations for any other outcome.

Table 4 includes DiD coefficient estimates for each outcome, stratified by age group. For outpatient URI visits, the point estimate was most pronounced among children ages 3-6 at baseline (IRR=1.08, 95% CI=1.02,1.15), although the estimate was also elevated among children ages 7-15 (IRR=1.05, 95% CI=0.99,1.11). Aside from outpatient URI visits among children ages 3-6, there were no other statistically significant associations between the policy and any of the other outcomes within any age stratum.

In addition to the main analysis, we assessed changes in rates of outcomes when restricting the analysis to child-months where a child was enrolled in Medicaid. Average monthly rates in the pre- and post-policy intervention periods under this scenario are included in Web Table 6. DiD regression results were identical to the main analysis in sign and significance (Web Table 7). We also restricted the sample to children who were continuously enrolled in Medicaid throughout the study period (NYCHA N=43,695, comparison N=22,817), and re-matched on all variables previously described. DiD point estimates for the restricted cohort were directionally similar to the main analysis, however, no estimates reached statistical significance (Web Table 8). We estimated models using primary diagnosis code to define outcomes, instead of any diagnosis code (Web Table 9). There was a significant positive association between the policy and rates of ED asthma visits (IRR=1.13, 95% CI=1.03,1.25). We created a matched sample of children who did not move between BBLs during the study period (NYCHA N=50,061, comparison N=29,146), and re-ran analyses with this restricted sample (Web Table 10). Under this specification, the significant association between the policy and URI visits in the outpatient setting became marginally significant (IRR=1.04, 95% CI=1.00,1.08; p=0.067). Results from models incorporating a 3-month washout period following policy implementation were similar to the main model specification with regard to directionality and significance (Web

Table 11). We did not find evidence of a significant association between the smoke-free policy and encounters for injuries (a negative control outcome) in either care setting (Web Table 12). When we specified August 2017-July 2018 as a placebo treatment year, estimates were directionally similar to the main analysis (with the exception of outpatient LRI visits), though point estimates were attenuated (Web Table 13). None of the associations in the pseudo-treatment year were statistically significant.

**DISCUSSION**

Within a large matched sample of children in New York City, we did not find evidence that implementation of a smoke-free public housing policy was associated with a decrease in health care encounters for SHS-sensitive conditions within the first 17 months post-policy. While a growing body of evidence has evaluated smoke-free housing policies with regard to air quality, this analysis is among the first to examine associations with health outcomes. Our findings are consistent with a prior study of air quality in NYC, which suggested that the smoke-free housing policy in NYCHA developments was not significantly associated with reductions in airborne nicotine after one year.[19]

Our findings may be explained, in part, by challenges associated with implementation of the smoke-free policy during the early roll-out period. A qualitative analysis of barriers to successful implementation of the NYCHA policy noted perceptions among residents and staff that the policy overreached by prohibiting smoking in individual apartments, in addition to public areas. Other barriers to implementation included safety concerns with regard to challenging others' smoking behavior, as well as structural barriers to compliance (e.g., lacking a comfortable area to smoke outdoors), and a lack of smoking cessation resources.[35] Many residents felt that the policy was unfairly singling out smokers in the context of widespread marijuana use as well as other pressing housing quality issues.[35] The complexities associated with maintaining smoke-free policies in multifamily housing suggest the need for ongoing engagement between residents and housing managers to develop sustainable implementation strategies.

Rates of outpatient URI visits declined for both groups in the post-policy period, but were slightly higher among NYCHA residents than would be expected, given observed rates within the comparison group. Likewise, rates of ED asthma visits among children in NYCHA developments were elevated in the post-policy period compared to the control group when we defined outcomes using primary diagnosis codes. We interpret these unexpected findings with caution for several reasons. First, the possibility of artifactual influences of residual confounding or selection bias cannot be ruled out. Second, IRRs were small in magnitude. For example, with regard to outpatient URIs, the change in the NYCHA group pre- and post-policy was an average of -8.3 visits per 1,000 children per month, versus a change in the comparison group of -9.1 visits per 1,000 children per month. Consequently, the difference between the groups (comparing the pre- and post-policy periods) was a relatively modest 0.8 visits per 1,000 children per month.

Nevertheless, although differences between the groups were small, the potential for adverse policy effects should be probed in future research. A prior study of public housing in Norfolk, Virginia found that airborne nicotine and $PM_{2.5}$ declined immediately following the introduction of a smoke-free policy but increased beyond pre-policy levels 12 months following policy implementation.[16] There is also limited evidence that other types of smoking restrictions – such as smoke-free laws in workplaces, restaurants, and bars – could be associated with displacing smoking from regulated environments into home environments,[36,37] though most studies have not observed such an association.[37] It is possible that smoke-free housing policies could, in some cases, displace smoking behavior from common areas (where the policy is more likely to be enforced) into private apartments, despite the ban. This could lead to increased SHS exposure for children who live with residents who smoke.

Strengths of this study include leveraging the large population of residents in NYCHA and other subsidized housing developments in NYC to conduct a well-powered DiD analysis. We integrated hypothesized individual- and area-level confounding variables, incorporating demographic characteristics, aspects of the built environment, and CBG-level demographic and economic factors. We explored a number of model specifications to test the robustness of our findings to potential forms of confounding (e.g., examining a negative control outcome and pseudo-treatment year) and differences in sample selection approaches (e.g., restricting the sample to continuously enrolled children).

This study also had a number of limitations. Although average enrollment within groups was very similar over time, it is possible that the drivers of attrition differed between groups, which could have impacted estimates. While we visually inspected outcome trends in the pre-policy period, the counterfactual assumption underlying the DiD analysis is not empirically testable. Even though other types of subsidized housing developments were not required by the HUD rule to implement smoke-free policies, it is possible that some children in our comparison sample lived in developments that had independently instituted smoking restrictions. These policies could dilute observed associations between the NYCHA policy and health outcomes, particularly if they were implemented during the study period. Furthermore, while we selected children for the comparison group from BBLs that contained other types of subsidized housing, comparison BBLs were heterogeneous and often contained mixed income housing. It is possible that children in the comparison group differed from children in NYCHA developments on the basis of both individual factors (e.g., aspects of socioeconomic status) and built environment factors (e.g., building age and maintenance history) that were not addressed in this study. In the context of a DiD analysis, these factors would also have to be associated with outcome trends (rather than outcome levels) to result in confounding.[30] Finally, we had limited post-intervention data to examine time-varying associations. This is an area for future research, given prior evidence of differential impacts of smoke-free policies on air quality over time.[16]

Addressing the high levels of SHS exposure in multifamily housing remains a priority, as a substantial body of literature has highlighted disparities in respiratory health outcomes across housing environments.[23,24,38] In this large, quasi-experimental study, we did not find evidence that smoke-free policies introduced in NYC public housing developments were associated with attributable decreases in health care encounters for SHS-sensitive conditions among children during the first 17 months after policy implementation. We found suggestive evidence that the policy was instead associated with relatively higher levels of health care encounters than would be expected, although these findings were not consistent across model specifications. Ongoing monitoring of smoke-free housing policies over time will be essential to elucidating the potential impact of these policies on patterns of SHS exposure and attributable health outcomes. Most importantly, additional efforts to support policy enforcement, provide cessation services, and raise awareness of the health benefits of reduced SHS exposure in home settings are needed. Strategies for improving SFH policy effectiveness may include providing more support for smokers to quit, further engaging residents in implementation, and investing to improve environmental and structural health of buildings as part of a wider healthy-homes policy agenda.

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Table 1. Descriptive characteristics of unmatched and matched samples of Medicaid-enrolled children living in NYCHA and other subsidized housing at baseline (November 1, 2015), New York City[a]

| Characteristic | Unmatched | | | Matched | | |
|---|---|---|---|---|---|---|
| | NYCHA (*n* = 72,072 | Comparison (*n* = 108,780 | Std. Diff. | NYCHA (*n* = 71,114 | Comparison (*n* = 47,174 | Std. Diff. |
| Mean age (years) [b] | 8.1 | 7.6 | 0.11 | 7.8 | 7.8 | 0.00 |
| Age category (%) | | | | | | |
| 0-2 | 18.2 | 21.8 | -0.09 | 20.3 | 20.5 | -0.01 |
| 3-6 | 24.9 | 25.9 | -0.02 | 25.7 | 25.5 | 0.01 |
| 7-15 | 56.9 | 52.2 | 0.09 | 54.0 | 54.0 | 0.00 |
| Female (%) | 49.0 | 48.8 | 0.00 | 49.0 | 49.0 | 0.00 |
| Race/ethnicity (%) | | | | | | |
| Non-Hispanic White | 4.2 | 8.0 | -0.16 | 4.6 | 5.3 | -0.03 |
| Non-Hispanic Black | 38.2 | 26.9 | 0.25 | 37.0 | 37.0 | 0.00 |
| Hispanic | 27.5 | 26.3 | 0.03 | 26.1 | 26.1 | 0.00 |
| Non-Hispanic Asian | 3.4 | 5.7 | -0.11 | 3.5 | 3.5 | 0.00 |
| Other | 3.7 | 5.0 | -0.06 | 4.4 | 4.4 | 0.00 |
| Unknown | 23.1 | 28.2 | -0.12 | 24.4 | 23.7 | 0.02 |
| Blind/Disabled (%) | 9.6 | 5.0 | 0.18 | 8.9 | 8.9 | 0.00 |
| Chronic medical condition (%) | 72.2 | 68.6 | 0.08 | 69.3 | 69.3 | 0.00 |
| High-rise building in BBL (%) | 72.3 | 14.0 | 1.21 | 45.5 | 45.5 | 0.00 |
| Block group characteristics | | | | | | |
| % Ages 0-17 | 25.8 | 24.9 | 0.10 | 25.4 | 25.4 | 0.00 |
| % Ages 18-49 | 42.8 | 46.8 | -0.47 | 44.0 | 44.0 | 0.00 |
| % Ages 50+ | 31.4 | 28.3 | 0.31 | 30.6 | 30.6 | 0.00 |
| % Black | 48.5 | 35.4 | 0.54 | 47.1 | 47.1 | 0.00 |
| % Hispanic | 47.7 | 47.7 | 0.00 | 46.0 | 46.0 | 0.00 |
| % <1.00 PIR | 43.8 | 31.1 | 0.77 | 38.3 | 38.3 | 0.00 |
| % 1.00-1.24 PIR | 8.8 | 7.6 | 0.18 | 8.8 | 8.8 | 0.00 |
| Population/square meter | 0.04 | 0.04 | -0.12 | 0.03 | 0.03 | 0.00 |
| Number of months enrolled in pre-policy period | 29.3 | 28.6 | 0.08 | 28.7 | 28.7 | 0.00 |

Abbreviations: NYCHA, New York City Housing Authority; Std. Diff., standardized difference; BBL, borough-block-lot; PIR, poverty income ratio

a) All characteristics measured at baseline with the exception of number of months enrolled in pre-policy period. Matched sample incorporates overlap weights.
b) Values are expressed as mean.

Table 2. Monthly mean visits per 1000 children with 95% confidence intervals for outcome conditions in pre-policy and post-policy periods, matched sample, New York City, 2015-2019

| Outcome | Pre-policy | | | | Post-policy | | | |
|---|---|---|---|---|---|---|---|---|
| | NYCHA | | Comparison | | NYCHA | | Comparison | |
| | Mean | 95% CI | Mean | 95% CI | Mean | 95% CI | Mean | 95% CI |
| Outpatient visits | | | | | | | | |
| Asthma | 27.5 | 27.2, 27.8 | 24.7 | 24.4, 25.0 | 23.9 | 23.5, 24.2 | 21.7 | 21.3, 22.1 |
| LRIs | 3.8 | 3.7, 3.9 | 3.6 | 3.5, 3.7 | 2.3 | 2.1, 2.4 | 2.3 | 2.2, 2.4 |
| URIs | 27.9 | 27.6, 28.2 | 27.7 | 27.3, 28.0 | 19.6 | 19.3, 20.0 | 18.6 | 18.3, 19.0 |
| ED visits | | | | | | | | |
| Asthma | 7.1 | 6.9, 7.2 | 5.4 | 5.2, 5.5 | 7.9 | 7.7, 8.1 | 5.7 | 5.5, 5.9 |
| LRIs | 1.4 | 1.4, 1.5 | 1.2 | 1.1, 1.3 | 1.3 | 1.2, 1.4 | 1.0 | 0.9, 1.1 |
| URIs | 7.8 | 7.7, 7.9 | 6.5 | 6.4, 6.7 | 6.5 | 6.3, 6.7 | 5.2 | 5.0, 5.3 |

Abbreviations: NYCHA, New York City Housing Authority; CI, confidence interval; ED, emergency department; LRIs, lower respiratory infections; URIs, upper respiratory infections

Table 3. Difference-in-differences coefficient estimates comparing outcome rates between children in NYCHA housing and children in other subsidized housing, matched sample, New York City, 2015-2019[a]

| Outcome | IRR | 95% CI | p-value |
|---|---|---|---|
| Outpatient visits | | | |
| Asthma | 0.99 | 0.94, 1.04 | 0.614 |
| LRIs | 0.94 | 0.85, 1.03 | 0.195 |
| URIs | 1.05 | 1.01, 1.08 | 0.007 |
| ED visits | | | |
| Asthma | 1.05 | 0.98, 1.13 | 0.143 |
| LRIs | 1.12 | 0.98, 1.27 | 0.099 |
| URIs | 1.05 | 0.99, 1.11 | 0.122 |

Abbreviations: NYCHA, New York City Housing Authority; IRR, incidence rate ratio; CI, confidence interval; ED, emergency department; LRIs, lower respiratory infections; URIs, upper respiratory infections; BBL, borough-block-lot; CBG, census block group

[a]IRR estimates and p-values correspond to coefficient associated with treatment time interaction in negative binomial DiD regression model. Models include matching weights and control for age (continuous), age category, sex, race/ethnicity, blindness/disability status, chronic conditions, presence of high-rise in BBL, season, and CBG-level characteristics listed in Table 1.

Table 4. Difference-in-differences coefficient estimates comparing outcome rates between children in NYCHA housing and children in other subsidized housing, matched sample, stratified by baseline age group, New York City, 2015-2019[a]

| Outcome | Baseline age 0-2 (*n* = 23,524) | | | Baseline age 3-6 (*n* = 30,187) | | | Baseline age 7-15 (*n* = 64,577) | | |
|---|---|---|---|---|---|---|---|---|---|
| | IRR | 95% CI | P value | IRR | 95% CI | P value | IRR | 95% CI | P value |
| Outpatient visits | | | | | | | | | |
| Asthma | 0.96 | 0.83, 1.12 | 0.637 | 1.01 | 0.93, 1.09 | 0.882 | 0.99 | 0.93, 1.06 | 0.796 |
| LRIs | 0.95 | 0.81, 1.11 | 0.520 | 0.89 | 0.75, 1.05 | 0.156 | 0.95 | 0.80, 1.14 | 0.591 |
| URIs | 1.02 | 0.95, 1.08 | 0.619 | 1.08 | 1.02, 1.15 | 0.010 | 1.05 | 0.99, 1.11 | 0.080 |
| ED visits | | | | | | | | | |
| Asthma | 1.12 | 0.98 ,1.29 | 0.087 | 1.06 | 0.94, 1.20 | 0.338 | 1.01 | 0.91, 1.12 | 0.863 |
| LRIs | 1.09 | 0.89, 1.33 | 0.412 | 1.11 | 0.88, 1.41 | 0.370 | 1.14 | 0.89, 1.45 | 0.311 |
| URIs | 1.07 | 0.97, 1.19 | 0.174 | 1.05 | 0.94, 1.17 | 0.378 | 1.01 | 0.91, 1.11 | 0.878 |

Abbreviations: NYCHA, New York City Housing Authority; IRR, incidence rate ratio; CI, confidence interval; ED, emergency department; LRIs, lower respiratory infections; URIs, upper respiratory infections; BBL, borough-block-lot; CBG, census block group

[a]IRR estimates and p-values correspond to coefficient associated with treatment*time interaction in negative binomial DiD regression model. Models include matching weights and control for age (continuous), sex, race/ethnicity, blindness/disability status, chronic conditions, presence of high-rise in BBL, season, and CBG-level characteristics listed in Table 1.