Resident Self-Reported Change in Community Engagement and Well-Being in MAP Neighborhoods in NYC

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Executive Summary

The NYC Mayor's Action Plan (MAP) is a comprehensive neighborhood-based, strategy to increase safety through coordinated crime reduction efforts in 17 New York City Housing Authority (NYCHA) development communities across New York City (NYC). The general goal of this component of the MAP research is to link MAP intervention activities to resident changes in attitudes and beliefs about community engagement and community well-being. What distinguishes the MAP intervention from other place-based public safety interventions is the priority placed on motivating positive changes in community engagement (including positive changes in social cohesion, legitimacy, and collective action) and in community well-being (including public health, economic development, and education), in addition to changes in crime incidence and prevalence and the overall perception of public safety.

This paper is one of a series of reports describing research on the NYC Mayor's Action Plan (MAP). The MAP research is a partnership between the John Jay Research and Evaluation Center (JohnJayREC) at John Jay College of Criminal Justice, the Mayor's Office of Criminal Justice (MOCJ) in NYC, and NORC at the University of Chicago. MAP is an ambitious effort to integrate human service and violence reduction programs in communities managed by the New York City Housing Authority (NYCHA) with a history of higher than average violence and public safety challenges. The goal of this research is to better understand the effects of human services interventions designed to improve public safety in nontraditional ways—through increases in trust between government and residents, trust among residents, changes in readiness for collective action, and improvements in the perceived legitimacy of government actions. The three products from the MAP research are:

- **John Jay Research and Evaluation Center project updates**. The JohnJayREC research update series describes the evaluation plan, MAP implementation, and interim findings.
- ***Resident Self-Reported Change in Community Engagement and Well-Being in MAP Neighborhoods***. In this brief, NORC describes the results of a difference-in-differences analysis of changes in MAP and non-MAP residents' perceptions of social cohesion and collective efficacy over one year of MAP implementation.
- ***Measuring Change in Social Cohesion, Collective Efficacy, and Public Safety Outcomes during MAP Implementation in NYC***. In this brief, NORC describes the results of an analysis testing whether changes in MAP resident attitudes and beliefs affect reported crime.

The goal of the NORC survey research, as part of the larger MAP evaluation, was to measure changes in these three measures of resident outcomes (community engagement, community well-being, and perception of public safety), observed one year apart, using data from two cross-sectional survey data collections measuring resident attitudes and with administrative data on reported crime. In our companion research note, ***Measuring Change in Social Cohesion, Collective Efficacy, and Public Safety Outcomes during MAP Implementation in NYC***, we report the results of analyses examining the relationship between MAP participation and changes in resident attitudes about community engagement and community well-being, on observed changes in criminal incidence.

The central challenge of this research is that MAP implementation predates the baseline resident survey. As a result, the survey is not a traditional pre-post evaluation of the state of MAP neighborhoods before MAP implementation compared to the state of MAP neighborhoods after program implementation. MAP began in 2014 and was implemented in stages over the rest of the decade. Some important components of MAP were new during the period between the baseline and follow-up surveys, particularly the NeighborhoodStat (NStat) convenings designed to bring residents and government stakeholders together to discuss local problems. This was a departure from earlier NStat meetings where the convenings were held across neighborhoods. Other MAP elements were already implemented before the start of the survey in early 2019, but the program continued to evolve and iterate, to sharpen program goals and organization and service delivery.

The outcomes in this research, the dependent variables, are only indirect measures of program effects. The survey asked residents about their attitudes and beliefs across items comprising 10 scales. These scales are informed by decades of research on collective efficacy and informal neighborhood social controls. The intuition behind these measures is that they observe changes in residents' understanding of community well-being and willingness to work together and with the government to problem solve. Research suggests that changes in these attitudes can independently affect well-being and public safety, absent any government intervention.

While most prior research in this area suggests that long-term changes in resident attitudes toward community engagement can have positive effects for communities, what is not known is how quickly these attitudes can evolve. Some prior research finds that attitudes and beliefs are affected in relatively short periods by substantial changes in government-led interventions, while other research finds that these beliefs are deeply ingrained and only evolve slowly over long periods. NORC set out to understand whether the surveys could detect a change in a brief study period. Researchers hypothesized that a comprehensive place-based intervention such as MAP would likely have a limited impact on residents' attitudes and beliefs over one year. However, it was critical to understand resident perceptions and needs over time to inform both the MAP evaluation, as well as potentially inform MAP operations.

The limitation of this research derives from the distributed nature of the MAP program. MAP is not a discrete intervention, which is turned from off to on, but rather a comprehensive network of interventions and communication that builds from existing structures, both formal and informal. Thus, there is no current discrete measure of MAP dosage (although our companion report makes use of changes in resident knowledge about the MAP initiative to make inferences about dosage). Here, the research focuses instead on whether the MAP initiative, as implemented, changes residents' attitudes and beliefs as a test of how malleable those beliefs are to change.

While the main effects are constrained by the short study time frame and the distributed nature of the intervention, key differences in subgroup attitudes and beliefs were observed:

- Older respondents were significantly more likely to report higher social cohesion in their development but less likely to have knowledge of social support services than younger respondents. More positive perceptions of collective efficacy were associated with higher resident age.

- Relative to Black residents, Hispanic and multiracial residents rated social cohesion in their development significantly lower and were less likely to report knowing social support services. Hispanic residents also rated NYPD procedural justice significantly higher than Black residents.

Overall, the study finds little association between MAP implementation and changes in resident attitudes and beliefs. Each of the measures of change in the 10 scales is stable across the two waves of resident surveys. This supports the idea that resident beliefs are sticky over long periods, and even robust interventions may have difficulty changing those long-held beliefs in the very short-term, due to structural conditions of concentrated poverty intertwined with criminal justice factors. This suggests that the government faces a long road to gaining an improved level of trust. It is also worth noting, however, that our companion report *Measuring Change in Social Cohesion, Collective Efficacy, and Public Safety Outcomes during MAP Implementation in NYC* study finds an association between higher MAP awareness and reductions in officially reported crime. This finding suggests that attitudes and beliefs, at least with respect to the latent constructs measured in this study, may be relatively time-invariant. Future research can test whether those attitudes and beliefs change in the long-run, and, whether they mediate the effects of interventions like MAP on crime and safety.

The Project in Brief: The Mayor's Action Plan

Launched in 2014, the Mayor's Action Plan for Neighborhood Safety (MAP) is led by the New York City (NYC) Mayor's Office of Criminal Justice (MOCJ). MAP is a comprehensive neighborhood-based strategy to increase safety through coordinated crime reduction efforts at 15 NYCHA developments across New York City. The MAP approach enlists residents, City agencies, and community-based partners to help move beyond enforcement and address the factors underlying safety – providing opportunities for work and play, health and well-being, and youth development; promoting activated, well-maintained spaces through community and human-centered design; and improving trust between neighbors with a responsive and just government. The true mission of MAP is: "To improve community safety in places impacted by historic disinvestment by creating opportunities for residents to identify key issues underlying crime and participate in the decision-making to address these priorities." MAP outcomes focus on reductions in crime and victimization, as well as broad improvements in social determinants, including health, employment, youth programs, community-building, and social capital.

The John Jay College of Criminal Justice Research and Evaluation Center (JohnJayREC) is leading the independent evaluation of MAP. NORC at the University of Chicago (NORC) is partnering with JohnJayREC to administer and analyze a two-wave resident survey designed to inform JohnJayREC's evaluation of MAP. Beginning in late 2017, NORC and JohnJayREC developed the survey items and research design, using validated scales examining collective efficacy to perceptions of public safety. MAP outcomes are also measured by perceptions of improved neighborhood conditions and in perceptions of increased public safety. In addition, outcomes include improved perceptions of city agency legitimacy (i.e., procedural justice among New York City Police Department (NYPD) and NYCHA), social cohesion, neighborhood collective efficacy, and trust. The NORC research was conceptualized as a baseline survey and a follow-up survey administered one year apart to inform a difference-in-difference analysis, testing whether changes in outcomes were different in treated places as compared to untreated comparison places, controlling for general trends in crime, social determinants, and community building. The first survey was fielded in February and March of 2019 by web surveys and computer-assisted telephone interviews (CATI). The second survey was fielded in February and March of 2020, again, by web surveys and CATI.

The survey is designed to measure changes in resident attitudes and beliefs of a random sample of residents in the 17 MAP developments. To test the effect of MAP, a comparison group was constructed with a random sample of residents from 17 non-MAP matched comparison developments that also experience high rates of violence, which disproportionately impact historically disinvested communities.

The Survey Instrument and Data Collection

Survey Items and Measures

The foundations for the items used in the survey instrument were developed from social disorganization theory that posits that community social organization regulates and maintains effective informal social control. Effective strategies for preventing behavioral and health problems within a target community focus on the risk factors that lead to problems and on the protective factors that prevent them. While both risk and strength (protective factors) play a substantial role in determining community health, much more is known about risk factors—and thus, the survey focuses on measuring changes in risk that result from the MAP intervention.

To create items that address the breadth of the MAP program, key constructs in various literature were reviewed, including social capital: social cohesion, perceptions of safety, informal social control, and collective efficacy; perceptions of domestic violence; awareness of the availability of government services; procedural justice; and interactions with and trust in government figures and institutions. Literature and scales from various fields were reviewed, including the social sciences, criminal justice, and health and medicine. Internet searches were conducted using

Google©, Google Scholar©, and ProQuest© for the key constructs. Searches included the following keywords: social capital, safety and opportunity for community connectedness, perceptions of public safety, public agency legitimacy, perceptions of procedural justice, social cohesion, and trust; all with and without pairing the following words in the searches: survey, questionnaire, and scale.

Much of the survey research on the effect of social service-based interventions on public safety and social welfare in neighborhoods with concentrated disadvantage are derived directly from the Project on Human Development in Chicago Neighborhoods (PHDCN). PHDCN is a major interdisciplinary study aimed at deepening society's understanding of the causes and pathways of juvenile delinquency, adult crime, substance abuse, and violence (Earls, Brooks-Gunn, Raudenbush and Sampson, 1999). PHDCN measured social capital as collective efficacy by examining the causes and pathways of juvenile delinquency, adult crime, substance abuse, and violence, using surveys, interviews, observations, and administrative data. Combining two studies into a single, integrated design, the first study examined community, social, economic, organizational, political, and cultural structures, and the dynamic changes that take place within these systems. The second study followed randomly selected adolescents and young adults (N=7,000) and examined the changing circumstances of their lives as well as the personal characteristics that may lead them to choose or reject a variety of antisocial behaviors. PHDCN questions and scales inform many of the measures used in this instrument.

The survey instrument was assessed by two focus groups with 12 residents of MAP research sites (including residents of both treatment and comparison communities). Each participant was asked to complete the draft MAP Resident Survey and participate in a one-hour focus group regarding feedback on the survey questions. The objective of the focus group session was to assess the respondents' comprehension of the questionnaire items, including question intent and the meaning of specific words and phrases in the survey item. Residents also described issues with sensitive questions. The resident feedback from the focus groups was incorporated into the final survey instrument.

Sampling Frame

The survey was administered twice with an interval of one year between survey waves. The first survey was administered over a five-week period in the late winter of 2019 and the follow-up survey was administered over five weeks in the late winter of 2020. NORC implemented an address-based sampling, multimode approach to complete approximately 1,360 interviews in each of the two cross-sectional waves. The sample was drawn from residents living in NYCHA developments, both those receiving the intervention and those not receiving the intervention.[1]

[1] While 15 developments are receiving the intervention, three of the developments have two separately managed developments and are thus treated as independent observations in the survey data collection. These 17 developments are matched with 17 distinct non-treated developments in the control group.

For the baseline (and the follow-up wave), NORC sought to conduct about 680 interviews in the 17 NYCHA developments receiving the MAP intervention, and about 680 in 17 NYCHA developments that did not receive the MAP intervention.

The starting sample for each wave of the survey included 15,000 households. Adults (age 18 and over) residing in sampled households were randomly selected for the survey. The survey was conducted in English, Spanish, Cantonese, and Mandarin. Cognitive interviews verified that the survey was approximately 20 minutes in length. Randomly selected participants received a $2 pre-incentive in the mail along with an explanation of the survey goals and process, as well as directions to access the web-based instrument. Respondents received $15 upon completion of the survey with a $10 bonus for completing within two weeks of the beginning of the survey period. Sample response was continuously measured throughout the time the survey was in the field, as was data from survey respondents. Outbound telephone calls were scheduled to begin in Week 5 for survey nonrespondents, but were initiated after three weeks due to unexpectedly high production and only in the six communities where the neighborhood target had not been achieved.

Data Collection

Residents of the 34 selected developments received a letter inviting them to participate in the web survey via a mailing addressed to the resident with a request that an adult in the household access and complete the web survey. Data collection for the MAP Resident Survey began February 9, 2019, with the mailing of the web invitation letter. This letter provided a link to the web survey and the respondent's unique log-in credentials. During the first seven days of data collection, 1,429 completed surveys were received (by web), far exceeding expectations. NORC also received hundreds of calls to the project toll-free line. These calls were returned, and the survey was completed by telephone, as requested. An identical process was completed beginning February 7, 2020. For the baseline survey, data collection ended four weeks ahead of the planned 10-week period, with 1,941 completed surveys. For the follow-up survey, data collection again was completed four weeks ahead of schedule, with a total of 1,563 completed surveys.

Literature Review

This study uses repeated cross-sectional data from the two waves of the NYCHA MAP survey to investigate the relationship between changes in neighborhood crime and MAP interventions in NYCHA developments, controlling for development latent variables, such as collective efficacy and social cohesion. Prior research on collective efficacy and social cohesion was generally followed in this study in constructing both the latent variables and in developing the hypotheses to be tested.

A seminal article by Sampson et al. (1997), informed by data collected as part of the PHDCN, sparked interest in the relationship between collective efficacy and an array of outcomes that included crime, well-being, and education. One consistent finding emerging from these studies is that a neighborhood's collective efficacy is strongly tied to its socio-demographic characteristics. In particular, concentrated disadvantage evinces a strong negative relationship with collective efficacy. Nearly all prior studies of collective efficacy (and related latent constructs) rely on cross-sectional data, hence providing only a single snapshot in time.

Sampson et al. (1997) used individual-level responses to 10 questions from a PHDCN survey to construct two scales labeled "informal social control" and "social cohesion." These two scales were then combined at the neighborhood level using an item response model to produce a single measure labeled "collective efficacy." Collective efficacy is a measure that Sampson and colleagues posited captured the degree of linkage between a neighborhood's mutual trust and its willingness to intervene for the common good. The authors asserted that the "collective efficacy of residents is a critical means by which urban neighborhoods inhibit the occurrence of interpersonal violence."

The study employed three measures of violence, two of which were based on responses from the survey itself. The first was a scale based on five questions about respondents' perceptions of violence in their neighborhood in the past six months. The second was a (binary) measure based on whether they or anyone in their family had experienced violent victimization.

The authors also hypothesized that neighborhood-level collective efficacy reduces the direct effects of a neighborhood's social composition on violent behavior. In other words, collective efficacy mediates the effect of other neighborhood attributes on violence. The authors test this hypothesis by estimating models of the effect of concentrated disadvantage, immigrant concentration, and residential stability on violent behavior with and without a measure of collective efficacy. They found that community violence is less frequent where neighbors' willingness to intervene is higher. Subsequently, many other studies have provided evidence that collective efficacy can partially explain the variation in violence across neighborhoods.

Neighborhood-level social processes are not easy to study. A growing number of studies have turned to original survey-based approaches to assess neighborhood-level social ties and associations. Taylor et al. (1984) constructed block-level measures of the proportion of respondents in 63 Baltimore neighborhoods who belonged to a neighborhood organization and the proportion who felt responsible for what happened in the area surrounding their home. Both measures were significantly and negatively related to rates of violence, exclusive of other ecological factors. Simcha-Fagan and Schwartz (1986), who studied 553 residents in 12 NYC neighborhoods during the mid-1980s, found a significant negative relationship between the rate of self-reported delinquency and rates of organizational participation among residents. Using survey data from Great Britain, Sampson and Groves (1989) found the density of local friendship networks was associated with lower robbery rates.

An unresolved (theoretical and empirical) issue pertains to the timing, and causal order, of crime and latent scale measurement. One view is that changes in collective efficacy bring about changes in neighborhood crime rather quickly (i.e., in a matter of weeks or months) and that, conversely, changes in violence can translate in short order to changes in collective efficacy. An alternative view holds that movements in neighborhood collective efficacy are reflected in movements in crime with a more substantial lag, in terms of years instead of weeks or months.

Analytic Approach

This study investigates whether MAP was associated with a change in reported scale scores between the two survey waves. JJRECJohnJayREC developed the comparison sample by matching each MAP development with a similar non-MAP development via propensity score matching (PSM). The comparison sample was selected from among non-MAP developments in such a way as to ensure that non-MAP developments in the comparison group were similar to MAP developments on a range of observable characteristics. For a detailed description of JohnJayREC's PSM methods and results, please see the first MAP Evaluation Update.

To measure the effect of MAP on each of the 10 scale scores, a linear difference-in-difference estimator was applied.[2] The difference-in-difference estimator measures whether the change in MAP scale scores differed on average from the change in non-MAP scale scores across the two waves, holding constant other factors. This model assumes that changes in reported scale scores in MAP developments would be similar to those in the non-MAP comparison development, in the absence of any MAP intervention.[3] For a complete description of the survey items and scales, please see JohnJayREC's fourth MAP Evaluation Update.

If the variables in the propensity score matching procedure capture the important differences between MAP and non-MAP developments, then a simple difference-in-difference calculation will produce an unbiased estimator of MAP on changes in scale scores. However, MAP developments may still differ along unobservable dimensions from developments in the

[2] The model specification in this analysis defines the dependent variables as the summed scale scores across all items in the scale (for each scale) so that it resembles a roughly continuous variable. While the distribution of summed scale scores may still not be perfectly normal, we believe that our sample size is sufficiently large to rely on the asymptotic, or large-sample, properties of OLS (ordinary least squares). Based on the central limit theorem, normally distributed errors are not required for consistency. Scales with binary items, and hence a smaller range of possible values, are likely to deviate more from normality than scales with categorical items that have a larger range of possible values.

[3] This is the "parallel trends" assumption that underlies identification in difference-in-difference models. In the present context, this assumption states that, while MAP and non-MAP developments may have evinced different levels in their Wave 1 scale scores, we would have observed (approximately) that same level difference in Wave 2 *had there been no MAP intervention*.

comparison group. Since developments are not randomly assigned to receive MAP, a linear model may be biased. To address some dimensions of this possible confounding factor, we also specified fixed- and random-effects models, as described below.

Concerns about differential selection into MAP based on fixed development characteristics (e.g., time-invariant such as development population) can be addressed by a fixed-effects estimator, which controls for unobserved, time-invariant development heterogeneity. The fixed-effects approach restricts the variation used to identify the effect of MAP to variation in the scale scores and explanatory variables within developments over time (i.e., across the two survey waves), and ignores variation between developments. While this approach controls for time-invariant factors, unobserved factors that may distinguish MAP from non-MAP developments, it has the drawback of not being able to estimate any time-variant variables in our model, including the level effect of MAP treatment. In the fixed-effects specification, the MAP treatment variable drops out of the model, effectively constraining this level effect to be zero. This does not affect the estimation of the difference-in-difference effect, the coefficient on the interaction term (*MAPxPOST*). This term tests the full impact of MAP over the survey waves.

Finally, we estimate a random-effects, difference-in-differences estimator to account for development (unobserved) heterogeneity. The random effect, in this case, is a normally distributed random term that is added to the intercept. Each development has its own (random) intercept, which accounts for the correlation in outcomes within developments across survey waves. In contrast to the fixed-effects estimator, which relies solely on within-development variation, the random-effects estimator takes into account variation both between and within developments. The parameter estimates in the random-effects model are a weighted average of these two types of variation. Because it doesn't rely solely on variation within developments over time, time-constant effects (such as the level effect of MAP participation) can be estimated in the random-effects framework. Unlike the fixed-effects estimator, which allows independent variables to be freely correlated with the time-invariant portion of the error term, the unbiasedness of the random-effects estimator requires that the error term is uncorrelated with the independent (or explanatory) variables in the model. If the correlation between the error term and the independent variables is not a concern, the random-effects estimator may be more efficient than the fixed-effects estimator.[4]

[4] This is the "parallel trends" assumption that underlies identification in difference-in-difference models. In the present context, this assumption states that, while MAP and non-MAP developments may have evinced different levels in their Wave 1 scale scores, we would have observed (approximately) that same level difference in Wave 2 *had there been no MAP intervention*.

Data Analysis

A key concern with this survey data was the treatment of responses that are missing, which include a "don't know" response or a respondent declining to answer the question.[5] Some scales suffered from a high rate of missing values. If a respondent did not provide a valid response to any item in the scale, that respondent's scale score was considered missing ("don't know" and "prefer not to answer" were the two responses that did not correspond to a value on the item scale and were not considered valid responses). Four scales (social cohesion, collective efficacy, NYCHA, and NYPD procedural justice) included an "undecided" category, which corresponded to the middle value on a 5-point scale and was a valid response. Respondents may not have distinguished between "don't know" and "undecided" responses. Therefore, some of the survey and statistician experts from the JohnJayREC academic advisory panel recommended recoding "don't know" responses to "undecided." This recoding also had the benefit of reducing the number of missing cases in those four scales, without significantly altering the distribution of scale scores. For social cohesion, for example, recoding "don't know" responses to "undecided" resulted in 715 additional observations (from n=2,624 to n=3,339) while only raising the mean slightly from 36.55 to 36.74, and the standard deviation from 9.64 to 10.18.

In addition to this recoding approach, NORC tested two single-value imputation techniques: item-mean substitution and person-mean substitution.[6] Item-mean substitution uses the mean for the same question across all respondents to replace non-missing responses to that item. This approach has the benefit of being straightforward to implement, but it can distort the actual distribution of values. In particular, it reduces the variance of the scale scores. Item-mean imputation recovers roughly the same number of observations (n=3,502) and nearly the same distribution (mean of 36.47 and a standard deviation of 9.56) as recoding.

Person-mean substitution replaces items that were not completed on a given scale with the mean of a respondent's completed items within the same scale. The substitution patterns will differ by the respondent, person-mean imputation does not artificially reduce the measure's variability, as is the case with item-mean substitution. Thus, person-mean substitution was selected for imputation. Person-mean imputation led to an additional 161 observations in the sample (n=3,339 to n=3,500), producing a distribution of scale scores with a mean of 36.45 (and a standard deviation of 9.60).

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[5] NORC completed several steps to ensure data fidelity. For example, a small number of responses were excluded because the email for gift cards was the same, and follow-up with the respondent determined that more than one survey response was submitted.

[6] The academic advisory panel did not believe pursuing more complicated imputation techniques for missing scale items, as they would qualitatively change our results.

The standard diagnostic for item-mean and person-mean substitution is that they provide good estimates when the proportion of missing items within scales are around 20 percent or less (Downey and King, 1998; Bono et al., 2007). **Table 1** summarizes the change in valid responses after recoding "don't know" responses and after person-mean substitution. The last column in **Table 1** records the percent missing after "don't know" responses were recoded; of course, this recoding did not affect six of the 10 scales. The second collective efficacy scale comprised of binary items had by far the largest rate of missing observations of 64 percent, followed by government decision-making (54 percent) and government engagement (39 percent). Given these high rates of missing observations, results using these scales should be treated with some caution.

Table 1: Change in Valid Responses after Recoding "Don't Know" Responses and Person-Mean Substitution

Scale	Total Observations	#non-missing responses			% non-missing responses after recoding DKs
		Un-imputed	After recoding DKs	After person-mean substitution	
Social Cohesion	3,502	2,624	3,339	3,500	95%
Social Support	3,502	2,839	2,839	3,443	81%
Collective Efficacy	3,502	2,689	3,453	3,500	98%
Collective Efficacy (binary)	3,502	1,253	1,253	3,331	36%
Perception of Safety	3,502	3,192	3,192	3,488	91%
NYCHA ProcJust	3,502	3,042	3,411	3,483	97%
NYPD ProcJust	3,502	2,690	3,283	3,358	94%
Govt Decision-making	3,502	1,612	1,612	2,585	46%
Govt Engagement	3,502	2,121	2,121	3,353	61%
Domestic Violence	3,502	2,482	2,482	3,033	70%

Data describing the scale means for the treatment and comparison cohorts can be found in **Table 3** and **Table 4** in JohnJayREC's fourth MAP Evaluation Update.

Analysis

To evaluate the effect of MAP on the 10 scales measured in the MAP Resident Survey, we estimate the following ordinary least squares (OLS) difference-in-difference model:

$$SCALE_{ikt} = \beta_0 + \beta_1 POST_t + \beta_2 MAP_k + \beta_3 MAP_k x POST_t + \boldsymbol{\gamma} DEMO_{ikt} + \epsilon_{ikt} \quad \text{(Equation 1)}$$

In Equation 1 above, $SCALE_{ikt}$ captures the respective scale score reported by respondent i in development k at time t, $POST$ denotes a binary indicator variable for survey Wave 2, and MAP denotes a binary indicator for the participation of development k in MAP. The coefficient on the interaction term $MAPxPOST$, β_3, measures the difference-in-difference effect; namely, the difference between the average Wave 1 to Wave 2 change in scale scores among MAP developments relative to the change in average scale scores among non-MAP developments in the comparison group. The term $DEMO$ denotes a vector of respondent demographic characteristics. The normally distributed regression error term is captured by ϵ.

For each scale, we present results from two models: one that omits resident demographic variables—represented by the vector $DEMO$ in equation (1)—and another that includes them. Both models include the full interaction of a MAP participation indicator and a Wave 2 (i.e., post-intervention) indicator variable.[7] As discussed in the methodology section above, we also estimated a specification in Equation 2 below that incorporates development fixed effects:

$$SCALE_{ikt} = \beta_0 + \beta_1 POST_t + \beta_2 MAP_k + \beta_3 MAP_k xPOST_t + \boldsymbol{\gamma} DEMO_{ikt} + \alpha_k + \epsilon_{ikt}, \qquad \text{(Eq. 2)}$$

where the fixed effect for development k is captured by the term α_k. This term is equivalent to a dummy, or binary, variable that equals one for observations belonging to development k.[8]

Finally, as a further robustness check, we estimated a random-effects (or random-intercept) model, in which the development-specific term is no longer fixed, but rather modeled as a normally distributed development-specific intercept. Both the fixed-effects and the random-effects estimators produced broadly similar results to the simple difference-in-difference estimators that did not control for unobserved development heterogeneity.[9] Given no systemic differences in random-effects, fixed-effects, and OLS, we favor the simpler model. The random-effects model produced lower standard errors and did not appear to be more efficient.

Results

Table 2 and **Table 3** display the estimated coefficients from the interaction of MAP and Wave 2. In **Table 2** and **Table 3**, we observe no statistically significant effects of MAP—as measured by the coefficient on the interaction term *MAPxPOST*—for any of the 10 scales. For nearly every scale, the sign of the coefficient remained the same across estimators.[10] Results for the other

[7] The inclusion of demographic variables in the model results in a reduction in observations due to nonresponse to those items collecting demographic information from respondents.

[8] The addition of development fixed effects effectively restricts the variation used to identify $\beta_3$to variation *within* developments. Moreover, the time-invariant, fixed effects, α_k, can be arbitrarily correlated with the idiosyncratic error term, ϵ_{it}.

[9] In particular, Hausman tests confirmed no systematic difference between the fixed-effects and random-effects estimators.

[10] The only exception was for the “government engagement” scale where the positive (but small and statistically insignificant) OLS and random-effects estimates turned negative (but also small in magnitude and statistically insignificant) using the fixed-effects estimator.

demographic control variables in the model, discussed below, were also very similar across the three estimators, although the direction of their effects varied across some of the scale outcomes.

Table 2: OLS Difference-in-Difference Social Cohesion, Social Support, Collective Efficacy (Categorical and Binary), and Perception of Safety

	Social Cohesion		Social Support		Collective Efficacy (Categorical)		Collective Efficacy (Binary)		Perception of Safety	
MAP	-0.13 (0.56)	-0.37 (0.53)	0.21 (0.14)	-0.01 (0.12)	0.25 (0.20)	0.09 (0.23)	0.06 (0.13)	-0.05 (0.14)	-0.28 (0.37)	-0.50 (0.39)
POST	-0.48 (0.43)	-0.49 (0.46)	-0.13 (0.14)	-0.320* (0.13)	-0.14 (0.14)	-0.14 (0.15)	-0.10 (0.10)	-0.04 (0.10)	-0.24 (0.19)	-0.19 (0.25)
MAPxPOST	-0.07 (0.73)	-0.15 (0.72)	-0.01 (0.17)	0.26 (0.18)	-0.17 (0.22)	-0.24 (0.24)	0.03 (0.15)	0.00 (0.15)	-0.01 (0.32)	-0.01 (0.39)
Demographic Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Scale Min	12		0		4		0		6	
Scale Max	60		7		20		6		24	
Observations	3500	2498	3443	2476	3500	2498	3331	2405	3488	2496

Significance: *** p< 0.01, ** p<0.05, * p<0.10

Table 3: OLS Difference-in-Differences NYCHA Procedural Justice, NYPD Procedural Justice, Government Decision, Government Engagement, and Domestic Violence

	NYCHA Proc Justice		NYPD Proc Justice		Government Decision		Government Engagement		Domestic Violence	
MAP	-0.04 (0.53)	-0.11 (0.58)	-0.34 (0.39)	-0.40 (0.43)	0.00 (0.06)	-0.03 (0.07)	0.09 (0.06)	0.05 (0.07)	0.17 (0.15)	0.22 (0.17)
POST	0.26 (0.46)	0.43 0.48)	-0.26 (0.33)	-0.64 (0.49)	-0.04 (0.08)	-0.06 (0.09)	-0.03 (0.07)	-0.06 (0.08)	0.07 (0.17)	0.13 (0.16)
MAPxPOST	-0.36 (0.59)	-0.57 (0.67)	0.16 (0.54)	0.72 (0.64)	0.05 (0.11)	0.14 (0.13)	-0.04 (0.08)	0.01 0.11)	0.10 0.21)	0.05 (0.21)
Demographic Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Scale Min	8		8		0		0		3	
Scale Max	40		40		3		4		12	
Observations	3,483	2,486	3,358	2,422	2,585	1,958	3,353	2,425	3,033	2,230

Significance: *** p< 0.01, ** p<0.05, * p<0.10

Demographic Results

The demographic results reported here describe the average effects measured across all MAP and all non-MAP comparison developments—**Table 4** and **Table 5** show results on the demographic covariates in each model. The results describe the relationship between demographics and scale scores. However, some caution should be taken in interpreting the coefficients as these are not intended to be interpreted as causal relationships. The coefficients described here are the values from a regression test of the effect of MAP on scale scores, and the demographics are used as control variables. A complete test of the independent effect of each demographic on each scale would likely require a slightly different model specification. Nevertheless, the relationships described here are generally robust to model specification, meaning that the sign and the magnitude of the observed relationships are consistent binary (two-way) tests of the relationship.

For ease of interpretation, the 10 models are divided into two discussions of five models each.

Table 4: OLS All Variable Difference-in-Differences Social Cohesion. Social Support, Collective Efficacy (Categorical and Binary), and Perception of Safety

	Social Cohesion	Social Support	Collective Efficacy (Categorical)	Collective Efficacy (Binary)	Perception of Safety
AGE	-0.231** (0.07)	0.0852*** (0.02)	-0.0939*** (0.02)	-0.0508** (0.01)	-0.134*** (0.03)
AGE squared	3.256*** (0.81)	-0.862** (0.25)	1.026*** (0.28)	0.607*** (0.17)	1.281** (0.41)
Race: White	-2.954 (1.76)	-0.29 (0.30)	-0.651 (0.45)	-0.589 (0.36)	-0.742 (0.89)
Race: Other	0.958 (2.69)	0.0833 (0.58)	-0.487 (0.99)	-1.271* (0.60)	0.291 (1.14)
Race: Hispanic	-1.282** (0.42)	-0.553*** (0.10)	-0.857*** (0.16)	-0.558*** (0.11)	-1.269*** (0.24)
Race: Mixed	-1.391* (0.58)	-0.664*** (0.15)	-0.921*** (0.21)	-0.566** (0.16)	-0.669* (0.26)
Employed: Full-time	-0.363 (0.57)	-0.167 (0.13)	0.152 (0.19)	0.0302 (0.11)	0.23 (0.26)
Employed: Part-time	-1.331 (0.72)	-0.661*** (0.13)	-0.296 (0.25)	-0.127 (0.19)	-0.34 (0.34)
Retired	0.893 (0.67)	-0.152 (0.15)	0.0225 (0.27)	0.0406 (0.18)	0.151 (0.35)
Temporary Employment	-1.055 (0.75)	-0.594*** (0.15)	-0.278 (0.24)	0.0457 (0.15)	-0.127 (0.43)
Household Size: 1	0.587 (0.57)	-0.0115 (0.16)	0.232 (0.21)	-0.023 (0.14)	0.385 (0.25)
Household Size: 3	0.828 (0.56)	-0.117 (0.15)	0.126 (0.19)	0.0564 (0.13)	0.182 (0.30)
Household Size: 4	0.253 (0.59)	-0.348* (0.15)	0.275 (0.23)	0.263 (0.19)	0.0176 (0.25)
Household Size: 5+	0.743 (0.52)	-0.104 (0.16)	0.0517 (0.20)	0.201 (0.12)	0.00617 (0.33)
Education: Less than HS	0.147 (0.55)	0.0715 (0.10)	0.0932 (0.21)	0.0395 (0.14)	-0.0864 (0.38)

	Social Cohesion	Social Support	Collective Efficacy (Categorical)	Collective Efficacy (Binary)	Perception of Safety
Education: Some College	-2.204*** (0.53)	0.0715 (0.09)	-0.788*** (0.20)	-0.313** (0.09)	-0.549 (0.29)
Education: College Degree	-2.874*** (0.69)	-0.304* (0.14)	-1.092*** (0.26)	-0.442** (0.15)	-0.939*** (0.23)
Years in NYCHA: 3 to 5	0.828 (0.81)	-0.118 (0.16)	0.445 (0.29)	0.112 (0.20)	0.166 (0.45)
Years in NYCHA: 5 to 10	-1.682* (0.80)	-0.319 (0.17)	-0.629* (0.27)	-0.215 (0.20)	-1.062* (0.43)
Years in NYCHA: 10 to 20	-0.358 (0.70)	-0.569*** (0.13)	-0.291 (0.25)	-0.0196 (0.17)	-0.576 (0.31)
Years in NYCHA: More than 20	0.465 (0.78)	-0.484** (0.15)	0.085 (0.27)	0.286 (0.20)	-0.144 (0.34)
Scale Min	12	0	4	0	6
Scale Max	60	7	20	6	24
Observations	2498	2476	2498	2405	2496

Significance: *** p< 0.01, ** p<0.05, * p<0.10

Age

Older respondents were significantly more likely to report higher social cohesion in their development. The marginal effect of each additional year of age translated into a 3.3 point increase in the scale score for social cohesion (on a scale ranging from 12 to 60). Older respondents were less likely to have knowledge of social support services than younger respondents. More positive perceptions of collective efficacy were associated with higher resident age.

Age was not significantly associated with perceptions of NYPD procedural justice.

Race

Hispanic and multiracial residents rated social cohesion in their development significantly lower than Black respondents. Relative to Black residents, Hispanic and multiracial residents were less likely to report knowing social support services. Once again, Hispanic and multiracial residents rated collective efficacy in their development lower than Black residents; however, the magnitude of this effect was not large: for each group just under a point on average, on a scale ranging from 4 to 20. Residents who reported their race as one of the categories labeled "Other" rated NYPD procedural justice an average of 7.15 points (on a scale from 8 to 40) higher than Black residents. Hispanic residents also rated NYPD procedural justice significantly higher than Black residents.

Education

Higher levels of education, on the other hand, were associated with significantly lower scale scores for social cohesion. Respondents with some college education reported a scale score that was, on average, 2.2 points lower than respondents with a high school degree, and respondents with a college degree reported a cohesion scale score that was 2.9 points lower than those with just a high school degree. We do not observe a strong association between knowledge of social support services and education; having a college degree is negatively related to knowledge of social support services. More educated residents likely have less need for those services.

More educated residents—those with some college or college degree—also rated collective efficacy lower than those with just a high school degree.

Employment

Relative to unemployed residents, temporarily employed residents rated NYPD procedural justice nearly 2 points lower on average, holding other factors constant.

Length of NYCHA Residence

Residents with longer tenure in NYCHA (10 years or more) were less likely to report knowledge of social support services than newer residents (those living less than three years in a NYCHA development). The relationship between NYCHA tenure and procedural justice was negative, but this relationship was not statistically significant.

Table 5: OLS All Variables NYCHA Procedural Justice, NYPD Procedural Justice, Government Decision, Government Engagement, and Domestic Violence

	NYCHA Proc Justice	NYPD Proc Justice	Govt Decision	Govt Engagement	Domestic Violence
AGE	-0.209** (0.08)	-0.0543 (0.07)	-0.0495*** (0.01)	0.0449*** (0.01)	0.0767*** (0.02)
AGE squared	2.521** (0.89)	1.517 (0.79)	0.641*** (0.15)	-0.365** (0.11)	-0.922*** (0.21)
Race: White	-1.409 (1.50)	1.342 (1.45)	0.0649 (0.20)	-0.225 (0.15)	0.197 (0.44)
Race: Other	-0.421 (2.51)	7.150** (2.38)	-0.0444 (0.40)	0.495* (0.19)	0.85 (0.72)
Race: Hispanic	-0.992* (0.43)	1.511*** (0.39)	0.174* (0.08)	-0.0942 (0.05)	0.232 (0.12)
Race: Mixed	-0.59 (0.39)	0.636 (0.45)	-0.00835 (0.08)	-0.220*** (0.06)	-0.0725 (0.18)
Employed: Full-time	-0.561 (0.49)	-0.398 (0.54)	-0.0293 (0.09)	-0.0673 (0.06)	0.015 (0.11)
Employed: Part-time	-0.734 (0.45)	0.149 (0.53)	-0.06 (0.11)	-0.00382 (0.09)	0.0842 (0.18)
Retired	-0.367 (0.63)	-0.0924 (0.55)	-0.217 (0.14)	0.112 (0.08)	0.0744 (0.18)
Temporary Employment	-0.909 (0.74)	-1.896** (0.55)	-0.247* (0.09)	0.0273 (0.08)	0.26 (0.20)
Household Size: 1	0.781 (0.54)	-0.675 (0.56)	0.154 (0.11)	-0.138* (0.06)	-0.0404 (0.17)
Household Size: 3	0.848 (0.52)	0.82 (0.42)	0.157 (0.09)	-0.0247 (0.06)	-0.0532 (0.19)
Household Size: 4	0.388 (0.54)	0.593 (0.52)	0.220** (0.08)	-0.0308 (0.08)	-0.0749 (0.18)

	NYCHA Proc Justice	NYPD Proc Justice	Govt Decision	Govt Engagement	Domestic Violence
Household Size: 5+	0.493 (0.52)	0.951 (0.61)	0.143 (0.08)	0.0366 (0.09)	-0.104 (0.17)
Education: Less than HS	0.802 (0.42)	-0.0731 (0.42)	0.189* (0.09)	-0.0833 (0.05)	0.0477 (0.19)
Education: Some College	-1.186** (0.37)	-0.872* (0.36)	-0.361*** (0.08)	-0.0456 (0.06)	0.436** (0.14)
Education: College Degree	-2.053** (0.69)	-0.762 (0.45)	-0.463*** (0.08)	0.0556 (0.08)	0.697*** (0.14)
Years in NYCHA: 3 to 5	-0.272 (0.85)	0.96 (0.90)	0.00015 (0.14)	0.107 (0.13)	0.016 (0.29)
Years in NYCHA: 5 to 10	-2.428** (0.76)	-0.103 (0.78)	-0.125 (0.10)	-0.0842 (0.09)	0.278 (0.19)
Years in NYCHA: 10 to 20	-1.883* (0.76)	-0.156 (0.76)	-0.166 (0.12)	-0.0751 (0.09)	0.294 (0.19)
Years in NYCHA: More than 20	-1.917* (0.77)	-1.029 (0.76)	-0.301* (0.12)	-0.0799 (0.10)	0.400* (0.17)
Scale Min	8	8	0	0	3
Scale Max	40	40	3	4	12
Observations	2486	2422	1958	2425	2230

Significance: *** p< 0.01, ** p<0.05, * p<0.10

Conclusion

In this study, we test the effect of MAP on residents' self-reports on community engagement and well-being. We describe several approaches to resolve data that are missing from either within survey nonresponse (where some items in the study yield a valid response and other items within the same survey respondent do not yield a valid response). For the analysis, we choose person-mean substitution to resolve missing data, where the mean of a respondent's completed items within the same scale replaces items that were not completed on a given scale.

The paper then analyzes data from the two-wave survey of MAP residents and matched comparison sites. The analysis compares the before-MAP and after-MAP change in the mean response of the treated group to the mean before- and after-MAP changes in the comparison group on ten scales comprised of multiple items. For this analysis, to evaluate the effect of MAP on the 10 scales measured in the MAP Resident Survey we estimate an ordinary least squares (OLS) difference-in-difference model. We consider various solutions to the problem that responses in general, and the difference in differences in responses may reflect inherent differences in each of the developments where respondents are clustered rather than changes that are associated with participation in MAP or not. We find no empirical evidence that the development clustering contributes unobserved variation sufficient to confound the results of the simple OLS model. Thus, we present the results of the OLS models.

We observe no statistically significant effects of MAP for any of the 10 scales, including social cohesion, collective efficacy, and procedural justice (NYPD). For nearly every scale, the sign of the coefficient remained the same across estimators. We find some variation across demographic factors, including age, race, education and employment, and length of NYCHA residency. For the most part, these differences contribute to differences in baseline average scale response rather than differences in the effect of MAP on different subgroups of MAP residents.

There are two competing explanations for the findings in this report. One interpretation of these findings is that MAP was not associated with changes in residents' attitudes and beliefs. The other interpretation is that changes in residents' attitudes and beliefs are relatively sticky and difficult to change regardless of the efficacy of any given intervention, at least in the short-term (one year as measured here). Our interpretation of these results is affected by the findings in our companion report *Measuring Change in Social Cohesion, Collective Efficacy, and Public Safety Outcomes during MAP Implementation in NYC.* That study finds an association between higher MAP dosage and reductions in officially reported crime. This finding suggests that attitudes and beliefs, at least with respect to the latent constructs measured in this study, may be relatively time-invariant. The question remains and should be the subject of further study, whether those attitudes and beliefs change in the long-run, and, whether they mediate the effects of interventions like MAP on crime and safety.

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