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Original Contribution

Reducing Violence by Transforming Neighborhoods: A Natural Experiment in Medellín, Colombia

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Neighborhood-level interventions provide an opportunity to better understand the impact that neighborhoods have on health. In 2004, municipal authorities in Medellín, Colombia, built a public transit system to connect isolated low-income neighborhoods to the city's urban center. Transit-oriented development was accompanied by municipal investment in neighborhood infrastructure. In this study, the authors examined the effects of this exogenous change in the built environment on violence. Neighborhood conditions and violence were assessed in intervention neighborhoods ($n = 25$) and comparable control neighborhoods ($n = 23$) before (2003) and after (2008) completion of the transit project, using a longitudinal sample of 466 residents and homicide records from the Office of the Public Prosecutor. Baseline differences between these groups were of the same magnitude as random assignment of neighborhoods would have generated, and differences that remained after propensity score matching closely resembled imbalances produced by paired randomization. Permutation tests were used to estimate differential change in the outcomes of interest in intervention neighborhoods versus control neighborhoods. The decline in the homicide rate was 66% greater in intervention neighborhoods than in control neighborhoods (rate ratio = 0.33, 95% confidence interval: 0.18, 0.61), and resident reports of violence decreased 75% more in intervention neighborhoods (odds ratio = 0.25, 95% confidence interval 0.11, 0.67). These results show that interventions in neighborhood physical infrastructure can reduce violence.

causality; economic development; environment; neighborhood; residence characteristics; violence

Abbreviations: CI, confidence interval; HGLM, hierarchical generalized linear model; PREVIVA, Prevención de Violencia en el Valle de Aburrá.

Violence is a major cause of death and disability worldwide (1). According to the World Health Organization, 1,424 people die in acts of homicide on an average day—1 homicide per minute, on average—and many more are victims of nonfatal violence (1). Thus, violence contributes significantly to the global burden of injury and disability and the cost of health and welfare services (1, 2).

There is a long line of research linking the prevalence of violence to neighborhood factors such as concentrated disadvantage, residential instability, and the absence of "collective efficacy" (3–10). Moreover, many antiviolence initiatives target neighborhoods rather than individuals—for example, efforts to improve public spaces and infrastructure, have police engage more with the community, clean up debris, and reduce social disorder—in the hopes that changing environments will trigger widespread and long-lasting reductions in violence (11–16).

Still, many critics dismiss the evidence for "neighborhood effects" as resting on observational studies in which neighborhood treatment conditions are not randomly assigned and are probably confounded by unobserved factors that determine both the outcome (e.g., violence) and where a person lives (17, 18). One of the few attempts to study the effects of randomly assigned neighborhood conditions was the Moving to Opportunity experiment, conducted among publicly housed families from poor urban neighborhoods in 5 US cities (19), where the treatment was an opportunity to move away from poor neighborhoods via a housing voucher (20–22). The results have been mixed. Although male and female youth from families that received vouchers to relocate to lower-poverty areas experienced declining involvement in violent crimes, male youth also experienced more conduct disorder, other problem behaviors, and property crime arrests (23). A key limitation of Moving to Opportunity and other studies of voucher programs is that the neighborhood effects they identify are moves to new neighborhoods rather than changes in current neighborhoods. Moreover, such studies cannot disentangle the potentially disruptive effects of a residential move from the potentially beneficial effects of living in a less impoverished neighborhood.

One way to address the limitations of observational studies and voucher-based experimental studies of neighborhood effects is to conduct randomized interventions that change neighborhoods rather than move individuals to new neighborhoods. Such a project would require a staggering commitment of resources and political capital in order to 1) implement a sufficiently effective intervention, 2) deny the intervention to a randomly selected control group of neighborhoods, and 3) collect data on large samples of neighborhoods and individuals, before and after the intervention. Although community trials are common in epidemiologic research, such studies tend to be based on a small number of neighborhoods, and often the intervention is not randomly assigned (24–26).

In cases where large-scale social experiments are prohibited for practical or ethical reasons, researchers often look for ‘‘natural experiments’’—usually changes resulting from policies, interventions, or acts of nature—that exogenously assign treatment conditions, but we are aware of few prior attempts to test neighborhood-effect hypotheses using a natural experiment (27). The current study fills this void by capitalizing on a large-scale natural experiment resulting from a public works project in a subset of neighborhoods from a major Latin American city: Medellín, Colombia.

Using data contemporaneously collected on Medellín neighborhoods that were targeted by the public works project and a matched set that were not, we assessed whether neighborhoods that received the intervention experienced significantly greater 1) declines in violence and 2) corresponding improvements in neighborhood conditions that might contribute to reductions in violence, including collective efficacy, citizens' trust in the criminal justice system and reliance on police, and the availability of neighborhood amenities such as parks and cultural activities.

MATERIALS AND METHODS

The intervention

Medellín is Colombia's second-largest city, with a population of over 3 million in the metropolitan area (28). Fueled by drug-related conflict involving the Medellín drug cartel and various paramilitary gangs, it became one of the most violent cities in the world during the 1980s and 1990s. Homicide has been the leading cause of death in Medellín since 1986 (29). In 2002, before the intervention, Medellín's homicide rate was 185 per 100,000 population, accounting for 28% of deaths in the city (30). By comparison, the highest homicide rate in a US city in 2002 was 53 per 100,000 population, in New Orleans, Louisiana (31).

In 1999, the municipal government of Medellín enacted a territorial plan to promote urban and rural development, which included a cable-propelled transit system (gondola) known as Metrocable (32). The first line (K) opened in 2004, connecting an elevated train system in the city center to the impoverished Santo Domingo neighborhood in the mountainous periphery, with 4 stops covering a distance of 2,072 m and reaching an elevation of 399 m (33). The municipal government made other improvements to neighborhoods serviced by the gondola, including additional lighting for public spaces; new pedestrian bridges and street paths; ‘‘library parks’’; buildings for schools, recreational centers, and centers to promote microenterprises; more police patrols; and a family police station next to a gondola station. The locations were selected by a panel of municipal authorities on the basis of topographic and geographic feasibility.

Study design

Most of our preintervention data came from a 2003 household survey on neighborhoods and violence known as Prevención de Violencia en el Valle de Aburrá (PREVIVA), or Violence Prevention in the Valley of Aburrá, conducted in a representative sample ($n = 2{,}500$) of Medellín's noninstitutionalized population aged 12–60 years. A sample of 212 blocks was selected from each of the city's 16 districts, with probability proportionate to the size of the district's population aged 12–60 years, and 12 residents were selected per block, with probability inversely proportionate to size, so that the sample would be self-weighting (34). The response rate was 90.3%. In 2008, we conducted follow-up interviews with 466 of the 599 PREVIVA respondents (78%) who at baseline had resided in 1) one of the 25 study neighborhoods where the first gondola system was installed (City Districts 1 and 2) or 2) one of 23 study neighborhoods located in comparable city districts (4 and 8) that were *not* serviced by the gondola system. We refer to the first set of neighborhoods as the ‘‘intervention group’’ and the second as the ‘‘control group.’’

To choose neighborhoods for the control group, we performed an agglomerative cluster analysis (35) of all 16 city districts and selected PREVIVA neighborhoods that clustered with City Districts 1 and 2. The variables used in this analysis included indicators of service infrastructure (number of schools, sports sites, health centers), a classification of ‘‘socioeconomic stratification’’ based on official evaluations of city blocks used for property tax assessments, the proportion of residents eligible for welfare (from the Municipal Office of Planning), and average levels of antisocial behavior and collective efficacy (from the baseline PREVIVA survey). Web Figure 1 (available on the *Journal*'s website (http://aje.oxfordjournals.org/)) shows the locations of intervention and control neighborhoods.

The resulting sample contained 225 respondents from intervention neighborhoods and 241 respondents from control neighborhoods. Intervention- versus control-group status was based on where respondents resided during the baseline interview, so our estimates represent the ‘‘intent-to-treat’’ effect and may underestimate the treatment's effects on persons who were fully exposed to it. At the time of the follow-up interview, most respondents ($n = 484$; 81%) were still living in their baseline neighborhood, and of the 115 respondents who left, only 45 (7.5%) moved from an intervention-group neighborhood to a control-group neighborhood or vice versa.

Outcomes

We constructed neighborhood-level outcome measures of violence and the local social/institutional climate (Table 1) from the multilevel measurement models described below. The violence measures included 1) survey reports of violent events occurring in the neighborhood during the past 6 months and 2) annual neighborhood-level homicide event counts in 2003–2008 from the Office of the Public Prosecutor. The other outcome measures included survey-based scales of 1) collective efficacy (modified version of the scale developed by Sampson et al. (6), including questions about the likelihood of neighbors taking action in situations that threaten the social order and about their willingness to help one another), 2) trust in the criminal justice system (including the police, the family welfare system, judges, and prosecutors), 3) reliance on police to help solve neighborhood problems, and 4) the presence of neighborhood amenities (including parks, recreational centers, and cultural activities).

Analysis

We first tested the preintervention balance between the intervention and control groups with regard to covariates and baseline measures of outcomes, to determine whether differences were large enough to warrant further adjustment via matching. We employed an omnibus balance test that compares the Mahalanobis distance between groups on characteristics (Table 2) with a probability distribution generated by randomly assigning intervention and control labels to neighborhoods across all permutations (36). This approach avoids the problems of performing multiple tests and is robust in small samples ($n = 48$ neighborhoods) (37). Finding matching to be warranted, we used a Bayesian logistic regression model to estimate each neighborhood's propensity of receiving the intervention, conditional on covariates listed in Table 2 (38), and matched neighborhoods on their propensity scores using the *optmatch* tool in R (37). We summarized the matched, preintervention differences using a similar omnibus balance statistic, assessing it against the matched permutation distribution of such statistics, the distribution induced by independently randomizing intervention and control labels within each matched set.

To measure change over time, we fitted hierarchical generalized linear models (HGLMs) for each outcome on pooled pre- and postintervention data. For the survey-based outcomes, we used a logit link function to regress responses to the binary items in a given scale on 1) item dummy variables, 2) a postintervention versus preintervention dummy variable, and 3) individual- and neighborhood-level random effects for the intercept and postintervention dummy variable. We fitted another HGLM to neighborhood homicide rates, using a Poisson distribution, a log link function, and random effects for neighborhoods only (see Web Appendix for details). Interpreting the neighborhood-level random component of the post- versus preintervention terms as change scores, we tested for the presence of treatment effects by comparing mean differences of intervention and control neighborhoods' change scores with the distribution of means of such differences obtained by permuting intervention and control labels within matched sets. This layering of permutation tests over parametric measurement models excludes the possibility of findings that are falsely significant because of "interference" across neighborhoods or misspecification of the measurement model (39–41).

To produce confidence intervals and point estimates, we iteratively tested a series of null hypotheses, each positing a uniform treatment effect of a given size t. Hypotheses with $t = 0$ correspond to the tests for the presence of a treatment effect just described; to test each hypothesis positing a nonzero t, we estimated an HGLM with an offset term for the intervention group of size t, so that if the group differences in change were equal to t, the change scores for the two groups would be approximately equal. The Hodges-Lehmann estimate (42) of the treatment effect is then the value of t giving rise to an estimated HGLM in which matched differences between intervention and control neighborhoods' change scores most nearly average to zero. For hypothesis tests, neighborhood change scores were extracted from each model and the mean of their matched differences was compared with its permutation distribution, yielding a 2-sided test with a significance level of $\alpha = 0.05$.

If the null hypothesis was sustained, the hypothesized treatment effect was included in the confidence interval (i.e., t was considered one of the plausible values of the actual treatment effect). We iterated this estimation and testing procedure using progressively larger values of t to determine the upper limit of the 95% confidence interval, and we also tested negative values of t when necessary to determine the confidence interval's lower limit.

We subjected all significant findings to sensitivity analysis to determine how high the correlation between an unobserved confounder and the intervention variable would have to be to reduce the treatment effect to nonsignificance (42). Confounding with the intervention variable was summarized using Rosenbaum's gamma, which indicates the proportionate amount by which a neighborhood's odds of receiving the intervention could be increased/decreased by the unobserved confounder (see Web Appendix for details).

RESULTS

Table 1 presents group-level means for each time period and the corresponding percentage changes for the outcome scales and their component items. Because these results serve descriptive purposes, no hypothesis tests are presented in Table 1. Levels of homicide and perceived violence declined in both groups, but the drop was steeper in the intervention group, where the homicide rate dropped by 84% as compared with 60% in the control group, and the proportion of respondents endorsing the average item (i.e., responding "yes" to a prototypical item) in the violence scale dropped

Table 1. Characteristics (%[a]) of Intervention and Control Neighborhoods Before and After the 2004 Introduction of a Public Transit System and Infrastructure Improvements, Medellín, Colombia, 2003–2008

Outcome	Intervention Neighborhoods			Control Neighborhoods		
	2003	2008	% Change[b]	2003	2008	% Change[b]
Mean homicide rate per 100,000 population	188.02	30.46	−0.84	104.28	42.19	−0.60
Perceived violence[c]						
Fights with weapon(s)	0.44	0.04	−0.91	0.20	0.05	−0.75
Gang fights	0.56	0.05	−0.91	0.27	0.05	−0.81
Other assaults						
Robbery	0.22	0.03	−0.86	0.21	0.09	−0.57
Rape/sexual abuse	0.06	0.01	−0.83	0.04	0.00	−1.00
Average item[d]	0.32	0.03	−0.90	0.18	0.05	−0.74
Collective efficacy[e]						
Residents work on local committees	0.53	0.59	0.11	0.59	0.57	−0.03
Neighbors help one another	0.67	0.73	0.09	0.67	0.72	0.07
Residents help watch neighborhood	0.59	0.61	0.03	0.63	0.64	0.02
Residents care for each other's children	0.59	0.64	0.08	0.64	0.65	0.02
Residents interfere in child fights	0.65	0.81	0.25	0.71	0.72	0.01
Residents interfere if child disrespectful to adult	0.65	0.78	0.20	0.61	0.64	0.05
Residents contact police if parent hits child	0.53	0.71	0.34	0.52	0.72	0.38
Residents intervene to prevent graffiti	0.53	0.81	0.53	0.55	0.71	0.29
Average item	0.59	0.75	0.27	0.61	0.69	0.08
Trust in criminal justice system[f]						
Police for domestic violence	0.54	0.69	0.28	0.53	0.60	0.13
Welfare system	0.49	0.69	0.41	0.51	0.62	0.22
Police officers	0.69	0.64	−0.07	0.68	0.57	−0.16
Human rights office	0.61	0.75	0.23	0.56	0.62	0.11
Ombudsman	0.56	0.72	0.29	0.57	0.62	0.09
Federal prosecutor	0.57	0.72	0.26	0.52	0.62	0.19
Attorney general	0.65	0.69	0.06	0.60	0.61	0.02
Judge/tribunal	0.68	0.69	0.01	0.62	0.62	0.00
Justice in general	0.62	0.71	0.15	0.62	0.65	0.05
Average item	0.60	0.70	0.16	0.58	0.61	0.06
Reliance on the police[g]						
For suspicious activity	0.48	0.85	0.77	0.71	0.81	0.14
For street fights	0.40	0.79	0.98	0.54	0.78	0.44
For delinquent acts	0.24	0.32	0.33	0.28	0.36	0.29
Average item	0.37	0.65	0.75	0.51	0.65	0.27
Neighborhood amenities[h]						
Parks/playgrounds	0.68	0.67	−0.01	0.83	0.80	−0.04
Recreational areas	0.62	0.64	0.03	0.75	0.76	0.01
Music/theater venues	0.35	0.51	0.46	0.53	0.60	0.13
Educational activities	0.42	0.59	0.40	0.59	0.67	0.14
Average item	0.52	0.60	0.16	0.68	0.71	0.05

[a] All data are percentages except homicide rates.
[b] Postintervention versus preintervention.
[c] Percentage who answered "sometimes" or "often" rather than "seldom" or "never."
[d] Proportion of respondents endorsing the average item (i.e., responding "yes" to a prototypical item).
[e] Percentage who answered "likely" or "very likely" rather than "somewhat likely" or "unlikely."
[f] Percentage who answered "a lot" rather than "a little" or "nothing."
[g] Percentage who answered "likely" or "very likely" rather than "somewhat likely" or "unlikely."
[h] Percentage who answered "yes" rather than "no."

Table 2. Baseline Characteristics of Intervention and Control Neighborhoods Before Propensity Score Matching, Medellín, Colombia, 2003

Preintervention Covariate	Intervention Neighborhoods		Control Neighborhoods		Standardized Difference[a]
	No., Rate, or Mean	%	No., Rate, or Mean	%	
Socioeconomic characteristics					
Neighborhood social class[b]					
Class 1 (low-low class)		28.0		13.0	0.37
Class 2 (low class)		72.0		30.0	0.9**
Class 3 (mid-low class)		0.0		57.0	−1.61***
Welfare receipt[c]		64.0		44.0	0.75*
Newly registered businesses per 100,000 population (2003)[d]	22.74		44.1		−0.34
Demographic characteristics					
Population density (no. of people/km^2)[e]	368.08		322.98		0.37
Male sex[f]		33.0		33.0	−0.01
Age group[f]					
Youth (ages 12–20 years)		41.0		28.0	0.58
Middle-aged adults (ages 36–61 years)		26.0		27.0	−0.11
Marital status[f]					
Married		42.0		42.0	0.03
Separated/divorced		5.0		12.0	−0.66
Home ownership[f]					
Own home		56.0		64.0	−0.3
Rent home		35.0		28.0	0.29
Employed[f]		27.0		35.0	−0.39
Education[f]					
More than high school		7.0		16.0	−0.56
High school		58.0		56.0	0.1
Physical and social environment[f] (mean scale score)					
Recreational and cultural amenities	0.64		0.67		−0.23
Collective efficacy	0.56		0.56		−0.03
Attitudes toward government and law enforcement[f] (mean scale score)					
Trust in criminal justice system	0.26		0.2		0.53
Reliance on police for problem-solving	0.48		0.7		−0.91
Crime and public safety					
Natural log of homicide rate, 2002[g]	3.06		2.34		0.96
Perceptions of violence[f]	0.26		0.25		0.07

* $P < 0.05$; ** $P < 0.01$; *** $P < 0.0001$.

[a] Difference between intervention and control neighborhoods' mean values or percentages in multiples of the across-neighborhood standard deviation, as calculated separately in each group and then pooled (using the conventional pooled variance formula).

[b] Social class is determined by the Colombian government for each neighborhood block face, based upon housing conditions (façade, type of door, size of front lawn, type of garage), urban surroundings (type of roadways and streets), and zone where the block is located. Since this study focused on a low-income population, in this sample, class values ranged from 1 (low-low class) to 3 (mid-low class). Across the city of Medellín, class values range from 1 (low-low) to 6 (high). Source: National Department of Statistics, Colombia, 2003 (unpublished data).

[c] Source: National Department of Statistics, Colombia, 2001 (unpublished data).

[d] Source: Medellín Chamber of Commerce, 2000–2003 (unpublished data).

[e] Source: National Department of Statistics, Colombia, 2002 (unpublished data).

[f] Source: Prevención de Violencia en el Valle de Aburrá survey, 2003 (unpublished data).

[g] Source: Office of the Public Prosecutor, 2002 (unpublished data).

by 90% as compared with 74% in the control group. Collective efficacy increased in the intervention group but remained stable in the control group. For example, the proportion of intervention-group respondents reporting that their neighbors would intervene to break up a fight among children increased from 65% to 81%, while the corresponding proportion in the

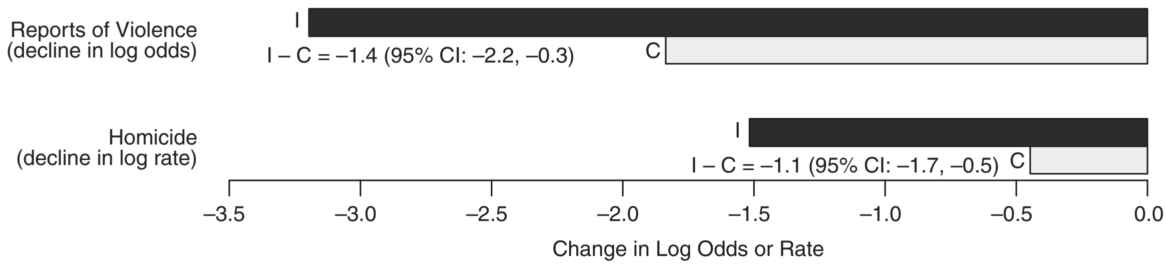

Figure 1. Diminishing violence in 25 Metrocable intervention (I) neighborhoods and 23 matched control (C) neighborhoods, Medellín, Colombia, 2003–2008. Violence decreased in both groups of neighborhoods, but intervention-group neighborhoods enjoyed a greater decrease than their matched comparison neighborhoods. The bars plot estimated changes in the log homicide rate and the log odds of affirmative responses to the survey-based measure of perceived violence. (Since violence and homicide declined over time, the bars report negative numbers b, with $1 - \exp(b)$ interpretable as the percent reduction in the outcome between 2003 and 2008.) Estimates of intervention effects appear to the left of the bars, along with corresponding 95% confidence intervals (CI).

control group was 71%–72% at both time points. Trust in the criminal justice system and reliance on the police for help also increased more dramatically in the intervention group. For example, the proportion of respondents who said they would call the police if they saw suspicious activity increased by 77% over time in the intervention group as compared with a 14% increase in the control group. The reported prevalence of parks and recreational areas did not change over time in either group, but reports of local opportunities to participate in cultural and educational activities increased in the intervention group.

Selection of neighborhoods for the intervention appears to have been driven by geographic and topographic factors, which might potentially have been correlated with relevant baseline variables. Table 2 compares intervention and control groups with regard to 23 such variables, mostly finding differences no larger than would be expected if all neighborhoods had precisely the same propensity to be selected for the intervention. Combining these baseline differences into an omnibus test, the hypothesis of simple randomization of neighborhoods was narrowly sustained ($\chi^2 = 30.5$ (21 degrees of freedom (df)); $P = 0.08$). Still, we propensity-score-matched intervention and control neighborhoods to ensure that the degree of balance in our analytic sample would be at or above the second quartile of the reference distribution produced by simple random assignment. This yielded 21 matched pairs and 2 triplets, pairs of higher-propensity treatment neighborhoods matched to a shared control. A subsequent balance test placed the combined baseline difference between matched intervention and control neighborhoods within the second quartile of a reference distribution corresponding to matched random assignment ($\chi^2 = 21.3$ (21 df); $P = 0.44$). The outcome analyses that followed would assume this paired assignment model.

Figure 1 shows the estimated change in each outcome for the matched intervention and control groups along with estimated group differences in change ("difference-in-difference" estimates) and confidence intervals. The group differences for change in both homicide and perceived violence were significant at the 5% level under 2-sided tests. Compared with the control group, the intervention group experienced a 66% ($0.66 = 1 - \exp[-1.1]$) greater drop in the homicide rate (95% confidence interval (CI): −1.7, −0.5) and a 75% ($0.75 = 1 - \exp[-1.4]$) greater drop in the odds of reported violence (95% CI: −2.2, −0.4). Sensitivity analysis revealed that these effects were robust, as an unobserved variable would need to increase the odds of treatment by at least 80% ($\gamma = 1.8$) in the case of homicide and by 20% ($\gamma = 1.2$) for perceived violence in order to confound the estimated treatment effects.

None of the other difference estimates were significant at the 5% level in 2-sided tests (Figure 2). However, reliance on the police increased 2.5 times more (exp[0.9]; 95% CI: −0.2, 1.8) in the intervention group; this difference would have been significant in a 1-sided test reflecting our a priori hypotheses ($P = 0.043$), and it is significant at the 10% level ($P = 0.086$) in a 2-sided test. In addition, collective efficacy grew 1.5 times more (exp[0.4]; 95% CI: −0.2, 1.1) in the intervention group than in the control group, a difference that would be marginally significant ($P < 0.075$) in an appropriate 1-sided test.

DISCUSSION

Despite the increasing popularity of place-based interventions and the considerable body of evidence from observational studies linking neighborhood conditions to the prevalence of violence and other health outcomes, skepticism remains about the credibility of the findings and the wisdom of using them to inform interventions (18, 43–45). Many critics and proponents of these studies agree that in the absence of a neighborhood randomized controlled trial, researchers should seek out natural experiments in which the assignment of a treatment condition closely approximates randomization. A large place-based intervention provides conditions for a natural experiment as well as the opportunity to assess the benefits to public health of policies that target neighborhood environments, even if those policies are not explicitly designed to improve health.

The fortunate coincidence in Medellín of a preintervention household survey (PREVIVA) and a large-scale public

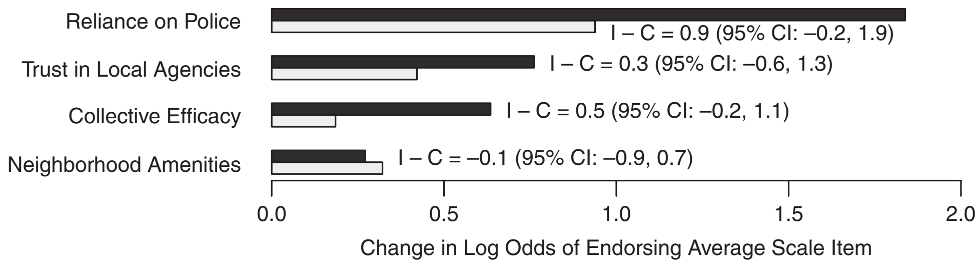

Figure 2. Improvement in community resources in 25 Metrocable intervention (I) neighborhoods (black bars) and 23 matched control (C) neighborhoods (gray bars), Medellín, Colombia, 2003–2008. Conditions improved in both groups of neighborhoods, but intervention-group neighborhoods enjoyed greater improvements than their matched comparison neighborhoods. The bars plot estimated changes in the log odds of endorsing the average scale item for each survey-based outcome. (The outcomes increased, so their bars report positive numbers b, with $\exp(b) - 1$ interpretable as percent increase over time.) Estimates of intervention effects appear at the right of the bars, along with corresponding 95% confidence intervals (CI). Intervention neighborhoods experienced greater improvement on all measures except neighborhood amenities.

works project (Metrocable), wherein treatment was plausibly assigned to neighborhoods on the basis of exogenous factors, provided an unprecedented opportunity to study a natural experiment on neighborhood effects. We took several steps to ensure that we had the strongest case for causal inference. First, we took advantage of a naturally occurring exogenous source of neighborhood change to address the concern of self-selection of individuals into neighborhoods. Second, we used cluster analysis to select comparable control neighborhoods and then matched neighborhoods on their propensity scores to ensure that the groups were as comparable as would be expected under randomization. Third, we used randomization-based inference to conduct hypothesis tests that remained valid even in the presence of potential threats to causal inference, such as treatment "spillover."

The intervention was associated with significant declines in neighborhood violence: The drop in homicide between 2003 and 2008 was 66% times higher in intervention neighborhoods than in control neighborhoods, while the corresponding drop in reports of violent events was 74% higher in intervention neighborhoods. Residents of intervention neighborhoods also experienced more growth in willingness to rely on the police and perceptions of collective efficacy, although these effects were marginally significant (the latter only in a 1-sided test). We found little evidence of an intervention effect on the presence of neighborhood amenities, but our ability to detect treatment effects on amenities was constrained by the broad nature of preintervention survey questions (which were replicated in follow-up interviews) that asked only about the presence of amenities and not about aspects of their condition that may have been improved by the intervention.

Although our study was not designed to investigate the mechanisms that produced the observed drops in violence, one account that is consistent with the pattern of changes we observed is that by improving public spaces and creating new institutions, the intervention provided more opportunities for neighbors to interact, develop trust, and become willing to intervene when the social order was threatened (5, 6). It also appears that relations between citizens and police improved in intervention neighborhoods, which could increase the efficacy of law enforcement in fighting violence and further deter would-be violent offenders (46).

Our study illustrates how the benefits of place-based interventions can diffuse beyond their intended areas of impact. The government's principal motivation for bringing effective public transportation to remote areas of Medellín was to improve residents' access to jobs and attract new businesses to impoverished neighborhoods. Reducing levels of violence, generating more collective efficacy, and increasing reliance on police appear to be downstream benefits of the dynamics set in place by the investment in public works. Another recent study provides a similar example by showing how the installation of a light rail transit system in North Carolina was associated with declining obesity and increasing physical activity in the affected areas (27).

There were several limitations of the study. First, we could not rule out the possibility that unobserved factors confounded our estimated treatment effects. This concern is allayed, however, by the exogenous nature of the intervention, the extensive set of covariates used in propensity score matching, and a series of sensitivity analyses assessing the effect of the intervention under assumptions of comparison group mismatch. Second, our design was not equipped to explain the mechanisms responsible for the decline in violence. Such an investigation would require additional sources of exogenous variation and a factorial design to assess how different combinations of treatment assignment and factors representing potential treatment mechanisms affected violence. However, the documented change in neighborhood conditions that accompanied the gondola intervention provides preliminary evidence on the mechanisms that explained the change in risk behaviors over time. Third, it is difficult to generalize from the results of this wide-ranging public works intervention to draw lessons for specific types of interventions. Our intent in this study was to examine whether large-scale investments in neighborhood infrastructure can be linked to measurable changes in violent activity, not to identify what types of investments are likely to have the most payoff.

The evaluation of Metrocable represents one of the first natural experiments on neighborhoods and violence, and the first, to our knowledge, in a developing country. Our findings have potential policy implications for urban settings beyond Medellín and indicate that it is possible, even in low- to middle-income countries, to harness municipal resources to implement structural interventions that will have an important impact on risk behaviors that place a significant burden on the health of populations.

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