

Costly Neighbors? Housing, Crime, and Support for Redistribution^{*}

Isabel Laterzo-Tingley[†]

Ayelén Vanegas[‡]

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Abstract

Public housing is often blamed for generating crime, though evidence is mixed and drawn mostly from Global North contexts. We ask whether public housing actually increases crime, and how such beliefs shape support for redistribution. Studying Brazil's *Minha Casa, Minha Vida*—one of the world's largest public housing programs—we combine multiple causal designs with an original survey. We find no evidence that public housing raises beneficiary, neighborhood, or city-wide crime: beneficiaries report high residential satisfaction; treated neighborhoods show no increase across crime indicators; and cities with exogenous increases in housing see homicide rates decline, even among vulnerable groups. A survey experiment shows that support for public housing depends less on whether it is linked to crime than on who is perceived to bear its costs. Support holds when costs are framed as confined to beneficiaries, but collapses when crime is framed as spilling beyond them.

Keywords: crime, redistribution, political economy, public housing, framing, Brazil

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[†]Assistant Professor, University of Texas at Austin, isabel.laterzo@austin.utexas.edu

[‡]Assistant Professor, Universidad Católica del Uruguay, avanegas@ucu.edu.uy

Introduction

In October 2025, Brazil's Minister of Cities declared that organized crime had taken over federally-funded public housing complexes, describing the situation as an “epidemic.”¹ The program in question was the government's own flagship initiative: *Minha Casa, Minha Vida* (MCMV), one of the world's most ambitious housing efforts. In less than a decade, the program delivered over three million homes, measurably improved beneficiaries' welfare (Menezes and Mourão, 2017; Belchior, Gonzaga and Ulyseia, 2023), and earned its architects electoral rewards (Bueno, Zucco and Nunes, 2023). In response to the minister's declaration, the government swiftly introduced programs to re-locate purportedly affected residents. Only months later, the minister declared that 2026 would be the “year of housing,” doubling down on investments in the program.² And strikingly, despite similar accusations throughout the program's history, MCMV continuously enjoyed remarkably high levels of public support, with approval even greater among non-beneficiaries than beneficiaries themselves (Bueno, Zucco and Nunes, 2023).

Does public housing *produce* crime – and if so, who bears the cost? And does the perceived bearer shape program support? The assertion that such programs generate crime is not new: across contexts, fears of rising insecurity fuel local opposition to housing, delay implementation, and shrink the scale of redistributive policy (Einstein, Glick and Palmer, 2020; de Benedictis-Kessner and Hankinson, 2019; Sahn, 2024). But these accounts center on *non-beneficiaries'* fears – spillover crime into surrounding neighborhoods – leaving the costs borne by beneficiaries themselves, and their implications, largely unexamined. Whether public housing generates crime at all remains contested (Griffiths and Tita, 2009; Freedman and Owens, 2011; Woo and Joh, 2015), although media and political discourse often present housing as criminogenic (Sheley and Ashkins, 1981). This can shape public opinion and program viability in ways that are perhaps disconnected from reality. We argue perception matters greatly: building on cost-benefit accounts of policy support (Holland, 2023), we argue that who is *believed* to bear security costs shapes coalitions more than whether crime actually occurs – explaining why support for public housing can

¹<https://www12.senado.leg.br/noticias/materias/2025/10/07/crime-organizado-em-condominios-populares-e-2018epidemia2019-diz-ministro>

²<https://www.camara.leg.br/noticias/1255116-ministro-das-cidades-diz-na-camara-que-2026-sera-o-ano-da-habitacao>

survive persistent crime narratives, so long as those costs are seen as falling on beneficiaries alone.

Leveraging MCMV, we present a series of studies on public housing's effects on crime and the political consequences of how that relationship is portrayed. We begin by establishing to what extent the association between public housing and crime is real. Existing evidence is mixed and context-dependent (Sandler, 2017; Aliprantis and Hartley, 2015; Freedman and Owens, 2011; Diamond and McQuade, 2019), and drawn almost entirely from the Global North. Existing theories offers three competing mechanisms for a housing-crime link: spatial concentration of poverty and relative deprivation (Merton, 1938; Runciman, 1966; Haberman, Groff and Taylor, 2013; Henderson et al., 2024), concentrated disadvantage and weakened social control (Griffiths and Tita, 2009; Ireland, Thornberry and Loeber, 2003; Tuttle, 2022), and territorial conquest by criminal organizations (Venkatesh, 2000; Magaloni, Franco-Vivanco and Melo, 2020; Daly and Barham, 2024). Each implicates a different victim (residents, neighbors, or the city) and distinct crime outcomes.

We test this relationship at three levels, tracing the outcomes predicted by each mechanism above. First, on residents: a national satisfaction survey of thousands of beneficiaries across 188 municipalities and a survey of MCMV's 2011 Rio de Janeiro lottery participants (Bueno, Nunes and Zucco, 2022) show few report wanting to leave due to violence, and an instrumental variable approach finds no effect of program entry on two measures of individual-level security. Second, on neighborhoods: in Rio de Janeiro - a hard case given the entrenched presence of organized crime (Magaloni, Franco-Vivanco and Melo, 2020) - a staggered difference-in-differences design finds no effect of MCMV construction on six crime indicators over time. Third, on cities: a natural experiment embedded in the program's rollout shows that an exogenous expansion of public housing does not increase city-wide crime. If anything, a surge in MCMV units *reduces* homicide rates, including among the demographic groups most exposed to organized criminal violence. Together, these findings challenge narratives that public housing systematically produces an increase in violence, for beneficiaries, neighborhoods, or cities alike. While cities and neighborhoods where public housing is built may be insecure to begin with – much like one of our analyzed cases, Rio de Janeiro – we find no evidence that public housing programs worsen violence, and find suggestive evidence they may improve it.

Nonetheless, media coverage and public statements sustain this association, even where the evidence may not bear it out. Because the support of relevant constituencies conditions whether redistributive programs can be implemented, expanded, and sustained (Holland and Schneider, 2017), we evaluate whether *perceptions* of crime, rather than crime itself, shape support for public housing. We evaluate how the bearer of these supposed costs – public housing residents vs. the broader community – may explain persistent support even in light of crime worries. Focusing on the organized crime mechanism (Venkatesh, 2000), given its dominance in media narratives and the Brazilian context, we field a pre-registered survey experiment among Brazilians in which respondents were randomly exposed to hypothetical news articles about public housing. These articles varied in whether they emphasized the program’s benefits alone or introduce crime-related externalities, distinguishing between city-wide crime and crime affecting only public housing residents. We block randomize our treatments across respondents who are eligible vs. ineligible for MCMV – those in theory likely to bear, or not bear, these costs – and investigate divergence in patterns of support.

Results demonstrate that support for public housing is strong but highly sensitive to framing, consistent with cost-benefit accounts of policy support (Holland, 2023; Vanegas, 2026). Framing that emphasizes the victimization of housing residents alone does not reduce support. This stands for respondents both eligible or ineligible to participate in housing programs. On the other hand, framing that extends crime to implicate the broader city does affect support. It even prompts housing ineligible respondents to retract support for local construction, a pattern absent when crime is confined to housing residents. This mirrors the Brazilian Minister of Cities’ own doubling-down on housing support despite victimization narratives, and echoes NIMBY and self-interest dynamics documented in the United States (Sahn, 2024; de Benedictis-Kessner and Hankinson, 2019). Citywide crime framing also increases support for policing relative to positive information alone, consistent with work on insecurity’s crowding-out of welfare demand (Altamirano, Berens and Ley, 2020, 2024; Laterzo, 2024) and on societal predispositions to govern low-income, race-class subjugated communities through coercive security institutions (Soss and Weaver, 2017).

Our analysis makes a handful of contributions. First, we not only extend analyses of the causal im-

pact of public housing on crime – which have largely been based on evidence from the U.S. (e.g., [Sandler, 2017](#); [Aliprantis and Hartley, 2015](#)) – to Latin America, but demonstrate a lack of systematic evidence for such a relationship. This is vital as such a perception can reinforce stigma, negative stereotypes of beneficiaries ([Motley and Perry, 2013](#)), and perceptions of deservingness ([Skigin, 2024](#)). These dynamics can, in turn, reduce support for these programs overall ([Layton, 2020](#)). Moreover, we demonstrate the widespread nature of mass media coverage that forwards this conclusion, and highlight how certain frames can undermine crucial support for these programs. This support is essential for policy survival.

Second, our analysis contributes to the broader literature on the social assistance-security tradeoff (e.g., [Altamirano, Berens and Ley, 2020, 2024](#); [Laterzo, 2024](#); [Berens and Karim, 2024](#); [Berens, López García and Maydom, 2024](#)). While previous work has highlighted how victimization and perceptions of insecurity within one's neighborhood can affect demand for public welfare provision, our analysis demonstrates how *who may be victimized* matters as well. While perceived victimization restricted to beneficiaries does little to shift preferences, if perceived insecurity spreads beyond them, support for these programs can suffer. More broadly, we speak to work on dynamic beneficiary preferences ([Cavaillé and Trump, 2015](#); [Vanegas, 2026](#)) and on how positive and negative externalities to beneficiaries and non-beneficiaries alike shape those preferences ([Dimick, Rueda and Stegmueller, 2017](#); [Holland, 2023](#)), as well as research on wealthy support for redistribution amid crime concerns ([Rueda and Stegmueller, 2016](#)). We show that redistributive coalitions can fracture even as material positions hold constant: what changes is not who bears costs, but who is *portrayed* as bearing them.

The article proceeds as follows: we present our theoretical framework and situate our contribution within the literature on public housing and preferences for social policies; provide contextual on Brazil's public housing programs; outline our research design and present each study's methods and results sequentially; and conclude.

Theoretical Framework

Housing insecurity poses a persistent and well-documented costs, including on education, mental health, and finances (Hock et al., 2024; Cutts et al., 2011). Contrary to this, housing security yields clear benefits, including wealth accumulation and social mobility, particularly for young households (Galiani et al., 2017; Sodini et al., 2023; Armentano et al., 2024). In turn, governments worldwide have invested in public housing with these benefits in mind.

As the case of MCMV illustrates, one typical motivation for public resistance to such programs is the supposed link between public housing developments and crime.³ In the United States, diverse demographic groups perceive dense and public housing developments as generating local crime, often tied to negative stereotypes about race and class (Tighe, 2012; Trounstein, 2023). While empirical evaluations of support for similar programs outside the U.S. and Western Europe are limited (Zavisca and Gerber, 2016), anecdotal evidence paints a similar picture.⁴ Yet, the empirical record is inconclusive: scholars disagree not only on whether the relationship exists, but, among those who find that it does, on the mechanisms and outcomes involved. This ambiguity does not diminish the perception's political weight: as citizens weigh housing programs' costs against benefits, persistent beliefs about crime shape the coalitions that support or oppose them.

In the following section, we first we examine this popularized relationship, the evidence behind it, and its weaknesses. We then consider how different framings of the housing-crime connection shape public support. Following this theoretical discussion, we turn to our case and empirical strategy.

The Housing-Crime Nexus

Does public housing lead to increases in crime? If so, how and for whom? For decades, scholars have studied these questions, but consensus has yet to be reached. On one hand, a series of studies associate public housing with elevated crime. Most prominently, analyses of public housing demolition in Chicago find

³<https://coloradosun.com/2023/03/13/boulder-group-home-nimby-property-value-crime-children-opinion/>

⁴In cities such as Buenos Aires, opposition to urban integration projects has invoked concerns about rising insecurity and the arrival of “delinquency” from informal settlements (Paiva, 2016).

that crime and other negative externalities declined once developments were razed (Sandler, 2017; Aliprantis and Hartley, 2015). On the other hand, various scholars have identified that public housing and related programs *reduce* crime. For example, Freedman and Owens (2011) find that low-income housing was associated with substantial decreases in violent crime, while others show that providing stable housing can reduce property crime by improving recipients' economic standing (Disney et al., 2023). Still others contend that any observed association is an artifact of site selection: developments are frequently constructed in already insecure urban areas, and thus neither generate nor attract crime, but rather enter into an existing insecure environment (Woo and Joh, 2015).

Among scholars who contend that public housing *does* generate crime, three dominant explanations have emerged: 1) spatial concentration of poverty, inequality, and relative deprivation, 2) concentrated disadvantage, isolation, and lack of social control, and 3) importation of violent entrepreneurs (organized crime). Table 1 summarizes each, including theorized crime outcomes. Importantly, each mechanism implicates distinct victims and predicts distinct crime outcomes. We leverage these predictions directly in Study I.

Table 1: Theoretical Frameworks, Mechanisms, and Predicted Outcomes for Public Housing and Crime

Theory	Mechanism	Affected Area	Predicted Crime Outcomes
Spatial Concentration of Poverty & Relative Deprivation	"Concentrating node" amplifying economic need and perceived inequality relative to surrounding areas	Surrounding neighborhoods	Increased property crime and non-violent crime in surrounding areas
Concentrated Disadvantage & Social Disorder	High-risk demographic composition combined with social isolation erodes internal social control	Residents	Increased intra-community victimization
Territorial Conquest by Criminal Organizations	Attractive spatial base for gangs and criminal organizations	Residents, surrounding neighborhoods, and city-wide	(1) Resident victimization; (2) local gang-associated crime (drug trafficking, possession, auto-theft); (3) city-level crime growth

The first of these explanations – spatial concentration of poverty, inequality, and relative deprivation

– suggests that public housing may be criminogenic because it serves as a “concentrating node” within an urban landscape of poverty (Haberman, Groff and Taylor, 2013). By concentrating disadvantaged populations, public housing is theorized to increase crime in surrounding areas through the non-mutually exclusive mechanisms of material need, strain, and relative deprivation. Drawing on strain theory (Merton, 1938), scholars argue that limited legitimate opportunities for economic advancement may increase economically motivated crime (Henderson et al., 2024). Relative deprivation theories similarly contend that proximity to more advantaged communities heightens awareness of inequality, generating resentment that can motivate criminal behavior (Runciman, 1966; Itskovich, 2025). Together, these perspectives predict increases in violence associated with economic motivations, particularly property crime. This expectation has been widely examined in studies of both public housing developments and high-poverty communities more broadly (e.g., Wilson and Hernstein, 1985; Sampson and Wilson, 2013; Henderson et al., 2024).

Others argue that any relationship between public housing and crime stems from concentrated disadvantage, social isolation, and weakened social control - concentrating crime (“hotbeds”) among residents themselves rather than generating spillover violence (Griffiths and Tita, 2009; DeKeseredy, 2003). Public housing can concentrate demographic groups disproportionately composed of individuals at crime-prone ages, including single-parent households and families with high adolescent-to-adult ratios (Ireland, Thornberry and Loeber, 2003). These dynamics may be compounded by physical environments characterized by extensive unmonitored public and semi-public spaces, which weaken mechanisms of social control and facilitate criminal behavior (Tuttle, 2022). Because offenders typically target those within their immediate vicinity, social isolation further increases the likelihood that residents are victimized by co-residents (Holzman, Hyatt and Kudrick, 2005; Haberman, Groff and Taylor, 2013), extending this dynamic beyond property crime to a broader range of offenses.

Finally, others suggest that public housing can become a spatial base for organized criminal groups, particularly given the concentration of individuals often targeted in recruitment efforts, such as adolescents and unemployed individuals (Barham and Daly, Working Paper; Levitt and Venkatesh, 2001; Ireland, Thornberry and Loeber, 2003). Sociologists highlight the role of public housing developments as

economic hubs for organized crime, including drug markets and auto-theft markets (Venkatesh, 2000; Aspholm, 2020). Because unclaimed territories are often contested, this expansion can generate local violence through turf wars (Magaloni, Franco-Vivanco and Melo, 2020), and gang presence can result in the victimization, intentional or unintentional (e.g., stray bullets), of residents (Popkin, 2000; Müller, 2019). Moreover, if public housing provides criminal organizations with new durable bases, it may strengthen their coercive capacity and contribute to broader increases in crime as groups spar, expand, and encroach on new territories (Daly and Barham, 2024). Overall, this mechanism points to three co-occurring outcomes: resident victimization, increases in neighborhood crime (e.g., drug trafficking and auto theft), and, if organizations are strengthened, increased crime at the city level.

Taken together, these literatures offer competing mechanisms but no settled answer as to whether public housing generates crime. This empirical ambiguity stands in sharp contrast to remarkably persistent public beliefs, revealing a potential gap between generalizable findings and public perception. These challenges are compounded by the fact that much of the existing evidence derives from major U.S. cities, limiting our understanding of such dynamics in other contexts.⁵

Crime, Cost-Benefit Analyses, and Policy Support

Regardless of the empirical reality, citizens often perceive public housing to be “crime ridden” (Motley and Perry, 2013). Such concerns are often fomented by media narratives, exposure to extreme violence, as well as public events such as Brazil’s Minister of Cities declaration discussed in this articles’ introduction (Pfeiffer, Windzio and Kleimann, 2005; Vétois et al., 2025). These beliefs have important political consequences, which can lead citizens to oppose affordable housing projects (Tighe, 2012). Indeed, groups of mobilized citizens have successfully thwarted or curtailed these projects in light of concerns about crime or other “undesirable” factors (Brouwer and Trounstein, 2024). Concerns about crime can motivate feelings of anxiety, fear, and anger (Ley, 2022), emotions that motivate citizens to engage in politics (Salamon and Van Evera, 1973; Albertson and Gadarian, 2015). This may be particularly salient in Latin American contexts, where citizens are disproportionately victims of crime and concerns about security

⁵With notable exceptions, such as Müller (2019).

run especially high ([AmericasBarometer, 2023](#)).

Thus, the (perceived) nature of the relationship between public housing and crime is of fundamental importance to the political viability of these programs. However, as our discussion of the housing-crime nexus highlights, who bears that cost may vary – and we theorize that this variation most implicates public support for housing programs. If public housing residents are victimized, backlash against the program may be more constrained; but if security concerns extend to the broad community and city, support for such programs may erode more considerably.

We consider these dynamics as part of broader cost-benefit calculations surrounding redistributive policies and their perceived externalities. Public housing is one such policy, reallocating public resources to expand access to shelter and reduce market-based inequalities in housing and neighborhood quality ([Rains, Krishna and Wibbels, 2019](#); [Xu, 2023](#)). Traditional theories of redistribution argue that support for these policies is concentrated among left-leaning and lower-income citizens, who stand to benefit most directly ([Huber and Stephens, 2012](#); [Pribble, 2013](#); [Feierherd et al., 2023](#); [Franko, 2016](#); [Rueda, 2007](#); [Rehm, 2009, 2011](#); [Cusack, Iversen and Rehm, 2005](#)). As a result, redistributive policies can struggle to generate the broad coalitions necessary for expansion ([Holland and Schneider, 2017](#)). Nevertheless, recent work has demonstrated that support for redistribution extends beyond expected beneficiaries, showing that “unlikely” groups, such as right-leaning citizens and the wealthy, can support welfare programs when they perceive broader social or political benefits ([De La O, 2015](#); [Fairfield and Garay, 2017](#); [Castiglioni, 2020](#); [Lupu and Pontusson, 2011](#); [Berens, 2020](#)).

Evolutions in scholarship have built on this consideration of “unlikely” supporters and turned to concepts of self-interest to understand these dynamics (e.g., [Cavaillé and Trump, 2015](#)). Inherent to these studies is a cost-benefit logic we theorize connects directly to the housing-crime debate: as [Holland \(2023\)](#) shows, citizens are willing to pay for policies as long as they perceive a benefit – even indirect – to themselves. [Rueda and Stegmüller \(2016\)](#) demonstrate the direct relevance to security, uncovering that the rich often support redistribution out of the belief it can combat crime. But, the converse is also true. If no benefit is perceived and only costs, or the benefit is perceived as inadequate, support is unlikely ([Holland, 2023](#); [Vanegas, 2026](#)).

These theories have important implications for public housing, particularly as the housing-crime nexus (Table 1) implies that security costs may be borne by different constituencies. Like other forms of redistribution, public housing generates direct benefits for beneficiaries (housing residents) and indirect benefits for non-beneficiaries (e.g., reductions in inequality); whether these programs retain public support, we argue, depends in part on who bears the associated security costs. As Table 2 shows, we map these coalitions (beneficiaries vs. non-beneficiaries) onto the predicted bearers of security costs in theoretically meaningful ways, generating cross-cutting pressures that distinctly shape support.

Table 2: Theoretical Expectations: Security Costs and Direct Benefits of Public Housing for Program Support

		Bearer of Security Costs	
		<i>Residents (Beneficiaries)</i>	<i>Neighborhood/City</i>
Housing Benefit Impact	<i>Direct (Beneficiaries)</i>	Support	Support
	<i>Indirect (Non-Beneficiaries)</i>	Support	Diminished Support

If theories predicting increased victimization among public housing residents prevail, beneficiaries bear those costs directly. Yet the material benefits of receiving housing are substantial, and many beneficiaries originate from neighborhoods where insecurity was already present. Consequently, beneficiaries may continue to support public housing programs despite potential security costs. In contrast, non-beneficiaries bear no direct costs when insecurity is confined to public housing residents while continuing to receive the indirect benefits of redistribution, suggesting that their support should also remain intact. The political calculus changes if crime extends beyond the boundaries of public housing developments, as theories of relative deprivation or organized crime expansion predict. In this case, non-beneficiaries may incur direct security costs, and in turn reduce their support for public housing programs.

These dynamics yield our first set of pre-registered expectations. Respondents exposed to the program's benefits should express greater support (H1). Among the eligible, this support should persist even when benefits are paired with security costs, regardless of who bears them (H2). Those unlikely to directly benefit because they are ineligible to participate should follow a different calculus: their support should remain elevated when benefits are paired with costs that fall only on beneficiaries (H3A), but should fall

below it when costs extend to a wider audience, including non-beneficiaries themselves (H₃B).

Beyond support for public housing programs directly, we also consider that security concerns as a “cost” of redistributive programs can also reshape broader evaluations of the state’s role and priorities. As [Altamirano, Berens and Ley \(2020\)](#) argue, heightened perceptions of insecurity are associated with a reduced demand for welfare provision, as they reflect a sense that the state is failing in its most basic function. When insecurity is seen as widespread, citizens prioritize the restoration of order over redistribution, reallocating their support toward security provision ([Altamirano, Berens and Ley, 2024](#); [Laterzo, 2024](#); [Berens and Karim, 2024](#)). In this sense, insecurity does not merely generate fear, but may also crowd out support for welfare policies by shifting expectations about what the state should be doing. This insight motivates a second observable implication of our framework: frames emphasizing widespread insecurity should not merely depress support for public housing, but may actively redirect preferences toward security provision.⁶

Setting: Public Housing in Brazil

We examine the public housing-crime debate in the context of Brazil, where one of the world’s largest public housing programs was launched in 2009. The *Minha Casa, Minha Vida* program (MCMV, My House, My Life), started under President Lula da Silva of the Workers’ Party (PT), combined subsidies and interest-rate incentives to improve access to housing for those within certain income brackets.⁷ The largest component of the program (*Faixa 1*) targeted the country’s lowest income citizens, providing subsidies of up to 95% of the cost of a home. To effectively supply these homes, the government constructed over 3 million housing units across the country in only seven years.⁸

In general, the program has been evaluated positively. Satisfaction among beneficiaries was reported

⁶We do not pre-register specific hypotheses related to security, but pre-register an exploratory analysis.

⁷MCMV largely maintained its original structure introduced by Lula until Jair Bolsonaro was elected to office in 2018, when he re-structured the program and renamed it. When Lula returned to office in 2023, MCMV was restored.

⁸<https://www.gov.br/cidades/pt-br/assuntos/noticias-1/conheca-o-programa-minha-casa-minha-vida>

to be high,⁹ and in the Northeast Region alone, the program decreased the housing deficit by 2.6%.¹⁰ Evaluations of the MCMV program show that beneficiaries reported a higher quality of life compared to non-recipients (Menezes and Mourão, 2017) and lower-income beneficiaries experienced both better formal employment outcomes and lower levels of welfare dependency (Belchior, Gonzaga and Ulysea, 2023). Some scholars have found, however, that incentives of developers pushed housing units towards the outskirts of the city, increasing segregation and urban sprawl (Biderman, Hiromoto and Ramos, 2018; Stiphany and Ward, 2019; Xu, 2023).

Beyond the popularity among beneficiaries, MCMV has enjoyed support across ideologically and income-diverse groups (Bueno, Nunes and Zucco, 2022). This popularity translated into important political dividends: both presidential and mayoral incumbents performed better in areas where MCMV was rolled out, compared to localities where it was not (Bueno, Zucco and Nunes, 2023). At the individual level, it is actually non-beneficiaries – who only benefit from the program indirectly, and for whom the details of implementation matter less – reward incumbents more for MCMV than beneficiaries (Bueno, Zucco and Nunes, 2023).

Salience of Crime Costs

Despite its high levels of support, shortly after MCMV launched, anecdotal and journalistic evidence circulated regarding a potential link between the program and crime. This association persisted for years. As shown in Figure 1, the media's crime coverage of the program increased steadily after its launch, over this period, peaking in 2021 before declining somewhat following the program's relaunch under Lula in 2023. This pattern is not idiosyncratic to a single outlet or period: in Appendix A.1, we corroborate it with an in-depth, article-level analysis of all MCMV coverage in *O Globo* (among the largest-circulation national newspapers) which reveals the same rising association between the program and crime.

One of the most dominant narratives was the takeover of MCMV complexes by organized crime.

⁹<https://www.brasildefato.com.br/2019/04/17/completando-10-anos-minha-casa-minha-vida-enfrenta-dificuldades/>

¹⁰<https://www.brasildefato.com.br/2019/04/17/completando-10-anos-minha-casa-minha-vida-enfrenta-dificuldades/>

Figure 1: Crime-Related Coverage of MCMV in Brazilian Media, 2009–2026

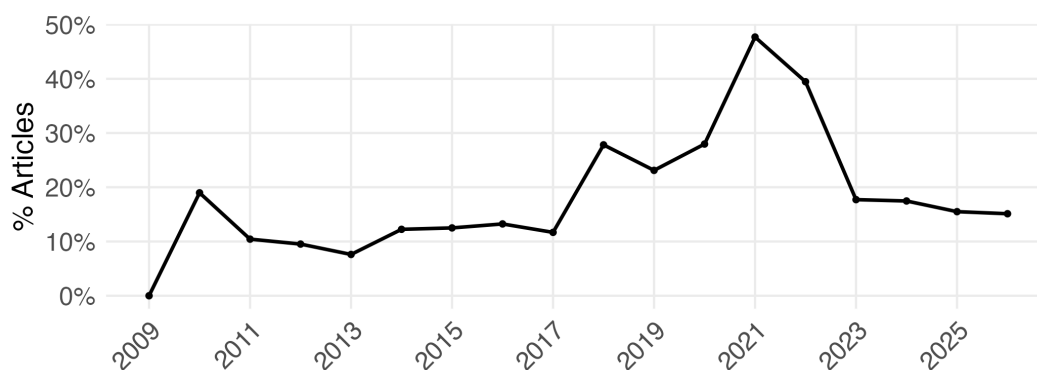


Figure Note: Data from Media Cloud. Figure reflects the percentage of MCMV-related articles that mention crime each year. Across all years, we identify 131,403 articles mentioning MCMV, of which 25,606 also mention crime (19.5%).

In 2015, one newspaper claimed that “all” MCMV housing complexes were targets of crime. The cited report noted that organized criminal groups had moved in and victimized residents, with one case of a group forcing out all 80 families to then distribute them to who they wished.¹¹ Indeed, reporting along these lines was particularly common in the city of Rio de Janeiro, although reporting in other large cities emerged noting similar dynamics as time progressed.¹² Other accounts detail gang-members victimizing individuals beyond the community’s boundaries, such stealing cars and demanding ransom payments from their owners or insurance companies.¹³ These concerns continue and have been elevated to the highest levels of government: as noted at the outset, by 2025 the country’s own Minister of Cities was describing the takeover of MCMV complexes by organized crime as an “epidemic.”

These narratives align most closely with organized crime takeover (Table 1), though they do not rule out other mechanisms. The organized crime account is distinctive in predicting all three outcomes: res-

¹¹<https://cbic.org.br/todos-os-condominios-do-minha-casa-minha-vida-no-rio-sao-alvos-do-crime-organizado/>

¹²<https://www.observatoriodasmetroles.net.br/retomada-do-programa-minha-casa-minha-vida-violencia-precarizacao-e-o-desafio-da-permanencia/>
Such as Feira de Santana, Bahia and Campinas, São Paulo, according to *O Globo* (2015).

¹³<https://oglobo.globo.com/rio/trafico-treinava-roubos-de-carro-em-conjunto-do-minha-casa-minha-vida-22969287>

ident victimization, increased crime in neighboring communities, and city-level growth. Any outcome in isolation (e.g., if only residents are victimized, this would be evidence of concentrated disadvantage and social disorder) would instead point to a rival mechanism; the absence of all three would suggest the association is not systematic.

Regardless, the scale and nature of this coverage, along with the government's response suggest a potentially concretized narrative that the country's largest public housing program has a crime problem. This pattern is especially consequential in light of evidence that negative news tends to attract greater attention than positive news (Robertson et al., 2023), as well as prior findings on the influence of crime-related coverage on public opinion in the region (Krause, 2014). At the same time, the persistence of the program's popularity amid such serious reporting is not straightforward to explain. It remains unclear whether incidents of violence in MCMV complexes are a consequence of the program's implementation, how widespread these concerns are, and whether they reflect a systematic relationship between public housing and crime. Addressing this gap is the central objective of our analysis.

Empirical Strategy & Results

Our study leverages the case of MCMV to investigate the public housing-crime nexus in depth. We have two empirical aims: 1) to evaluate *if* public housing generates crime, and if so via what mechanism; and 2) understand how the perceived public housing-crime linkage affects coalitions of public support. We employ a two-study approach to do so.

In Study I, we evaluate whether MCMV plausibly generates crime, testing the three prevailing theories summarized in Table 1, assessing (i) resident victimization, (ii) neighborhood crime, and (iii) city-wide effects. When relevant we also distinguish between different types of crime (e.g., property crime vs. lethal crime) to disentangle mechanisms. We do so via a multi-layered approach: surveys of beneficiaries and lottery participants, geo-located MCMV units in a city well-known for organized crime, and a national-level analysis exploiting a discontinuity in program design. Study II examines the political implications of the public housing-crime association via a pre-registered vignette experiment. By randomizing exposure

to positive program information and to crime-related externalities (resident vs. citywide framing), we test how citizens update their support for public housing and related policy priorities, namely security.

Study I: The Effects of Public Housing on Crime

Resident Insecurity

To identify if MCMV residents bear a security cost, we draw on two surveys of beneficiaries. These surveys offer us a complementary and robust picture of MCMV beneficiary experiences with violence, and allow us to leverage both descriptive and causal identification strategies, analyze beneficiaries at different points in the programs history, and across different geographies. Surveys provide important measures of both real and perceived security concerns, particularly in areas with low trust in the police and low levels of crime reporting.¹⁴ Moreover, the two surveys leverage different types of interview methods: face-to-face and phone-based. If face-to-face interviews raise concerns about under-reporting due to social desirability bias, fear of retribution, or coerced censoring, phone-based interview methods may guard against such concerns. For more details about both surveys, see Appendix B.

First, we analyze data from an independent phone-based survey of 2011 MCMV lottery participants in the city of Rio de Janeiro conducted by [Bueno, Nunes and Zucco \(2022\)](#).¹⁵ An additional benefit of this survey is that it was conducted in a context where much of the crime coverage regarding MCMV originated. This survey was conducted 8-9 years following the lotteries in question, and included both lottery winners and losers.¹⁶ It included two questions measuring respondents experiences related to violence: perception of security in their current neighborhood of residence, and claim making related to security.¹⁷ Given that the lotteries are randomly assigned, but not all lottery winners actually move into

¹⁴Surveys are useful tools even for measuring sensitive-to-report phenomena, such as organized crime presence(e.g., [Uribe et al., 2026](#)).

¹⁵See also [Bueno, Zucco and Nunes \(2023\)](#) and [Bueno, Nunes and Zucco \(2025\)](#).

¹⁶Because losers qualified for the program but did not enter it, the comparison holds eligibility constant. Both groups were surveyed in 2019–2020, 8–9 years after the 2011 lottery

¹⁷With respect to claim making, respondents were asked if they contacted the government, an NGO, or a local-level leader with a request related to security in the recent past. Thus, this should not be affected by low-propensity to report to the police, as Rio de Janeiro residents have other options for reporting, including community-based

MCMV units, we conduct an instrumental variable analysis where lottery victory serves as an instrument for relocation to an MCMV unit.¹⁸ This strategy allows us to estimate the causal effect (local average treatment effect, LATE) of relocation to an MCMV unit on perceptions of violence and security claim making for individuals who comply with the lottery assignment, while ensuring this effect is not biased by factors that would influence both move likelihood and our outcome variables (Angrist and Krueger, 2001). For the full estimating equation and procedures, see Appendix B.1.

Results from this analysis are presented in Table 3, with the second stage 2SLS estimates (columns 3 and 6) and their corresponding first stage F-statistics. We also present naive OLS models (columns 1 and 4) and the reduced form estimating intent-to-treat (ITT) effects of the lottery alone. As indicated by the F-statistics, the lottery is a strong predictor of one's eventual relocation to an MCMV unit. Across all model specifications, we find no evidence that relocating to an MCMV unit changes one's perception of neighborhood safety or likelihood of claim making related to security. Our results are robust to dropping covariates (Table B3).

This analysis compares insecurity between beneficiaries and would-be beneficiaries, isolating any change attributable to relocation itself. Because of this, a null result may imply different scenarios: a true lack of violence, or a lack of a difference in violence. Indeed, given residential segregation in urban Brazil, where many low-income households live in *favelas* with an established organized crime presence (Magaloni, Franco-Vivanco and Melo, 2020), middling violence in new MCMV communities need not represent an increase over what residents left behind.

To further probe this null, we analyze a second survey of beneficiaries. Via an access-to-information request, we obtain data from a face-to-face satisfaction survey ($n = > 7,000$) conducted in 2013 by the country's Ministry of Cities, covering 589 new public housing communities nationwide across 188 municipalities.¹⁹ This survey captures respondents' perceptions of insecurity as well as their reported access

organizations and crime hotlines (e.g., *Disque Denúncia*).

¹⁸ As Bueno, Nunes and Zucco (2022) note, some respondents secured MCMV units via other mechanisms, and not the 2011 lottery in question. We remove these individuals ($n = 32$) from the analysis as they are not randomly assigned to treatment. As a robustness check, in Table B4, we include them. Results remain the same.

¹⁹ Participants all belonged to Faixa 1. All respondents had lived in their new homes for at least 6 months before responding.

Table 3: MCMV Relocation, Perceptions of Violence, and Violence-Related Claim Making (Instrumental Variable Approach)

	<i>Dependent variable:</i>					
	<i>Satisfaction with Local Safety</i>			<i>Claim Making: Security</i>		
	OLS (1)	Reduced Form (2)	IV (3)	OLS (4)	Reduced Form (5)	IV (6)
Win Lottery + Relocate	-0.065 (0.242)		-0.201 (0.590)	-0.011 (0.009)		0.040 (0.029)
Win Lottery		-0.049 (0.143)			0.010 (0.007)	
Pre-treatment controls	✓	✓	✓	✓	✓	✓
First-stage F-statistic			383.61			390.13
Observations	2,073	2,073	2,073	2,082	2,082	2,082
R ²	0.005	0.005	0.005	0.002	0.003	-0.007

Note: *p<0.1; **p<0.05; ***p<0.01

Table Note: Data originally collected by [Bueno, Nunes and Zucco \(2022\)](#). Heteroskedasticity-robust standard errors in parentheses. Pre-treatment controls follow [Bueno, Nunes and Zucco \(2022\)](#): age, gender, race, CadÚnico registration, years in formal employment (RAIS, 2003–2010), and average logged formal income (2003–2010). IV specifications instrument compliance with lottery assignment. Satisfaction with local safety is a 1–10 scale (mean = 3.78), with higher values indicating greater satisfaction with local violence levels. Claim-making for security is binary, indicating whether the respondent contacted government, an NGO, or local leaders to request improved security (mean = 0.03). Further information in Appendix B.

to public services. Respondents were asked their satisfaction with their new home, their perceived violence, and if they plan to move in the next year. We descriptively map respondents’ overall perceptions of their homes, and analyze predictors of respondents’ desire to move, including perceptions of violence. For the latter step, we estimate via OLS (see Appendix B.3 for estimation details) the effect of perceived violence, along with a series of other covariates and controls, on how much respondents agreed with the phrase “we intend to move in the coming months.”²⁰ We include municipality-level fixed effects, and robust standard errors clustered at the municipality level.

Results are presented in Figure 2. Panel (a) plots the average level of agreement of respondents (normalized 0–1) with questions asked about the improvement in their lives as a result of MCMV, their like-

²⁰Controls: neighborhood and housing satisfaction, location satisfaction, nearby hospital and public transport, change in transport spending, whether the home is cheaper than the previous residence, reported life improvement, and sociodemographics (gender, age, race, higher education).

Figure 2: Resident Housing Perceptions and Move Intention

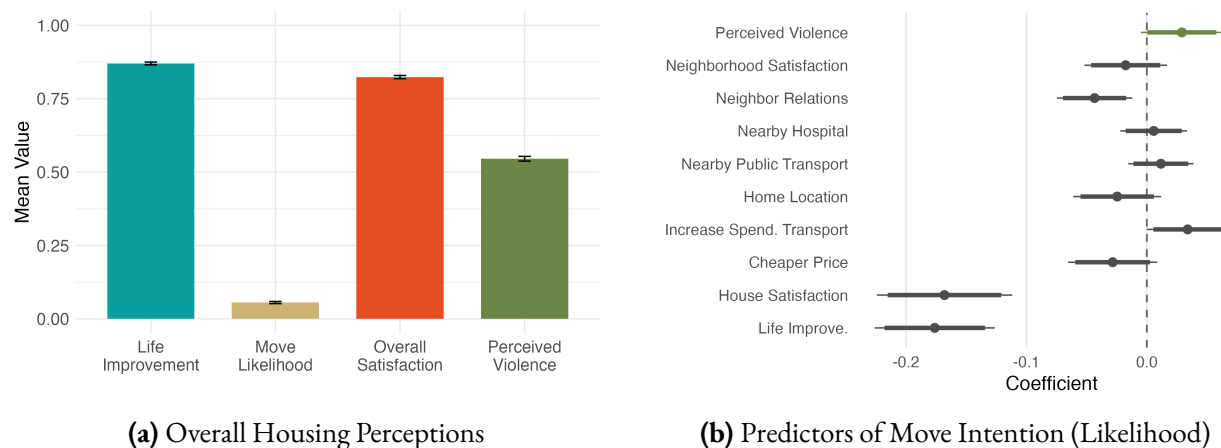


Figure Note: Survey conducted by Brazil's Ministry of Cities in 2013 ($n = 7,620$). Panel (A) corresponds to mean values across survey respondents. Panel (B) corresponds to coefficients from a linear model where reported move likelihood is the outcome variable; includes municipality-fixed effects and robust standard errors clustered at the municipality level, full results reported in Table B5. All question items were measured on 1–7 agreement scales (where high values are complete agreement) and normalized 0–1; full wording in Table B6.

likelihood to move, the overall satisfaction with their neighborhood, and if there is significant violence in their neighborhood. There is a strikingly high degree of satisfaction with one's neighborhood and the impression that their lives have improved. Move intention is close to zero.

Respondents on average report middling levels of agreement with the fact that their neighborhood suffers from violence. We probe this further by examining a second set of questions above move intentions, where respondents were asked if someone intends to move in the next year, and if so to list their primary motivations. Among those who report that someone in the home intends to move ($n = 365$, or 4.7% of respondents), only 15% of them report that the primary reason they wish to do so is related to violence and security ($n = 57$). In all, this represents less than 1% of respondents reporting that the primary reason a member of the household would leave their home in due to violence. Results in Figure 2 panel (b) demonstrate that violence is a marginally significant predictor of move intention ($p < 0.1$); moreover, the magnitude of this effect is small. In tandem with the overwhelming perception that beneficiary's lives have improved, this result suggests that violence is only a driving factor for a very small subset of beneficiaries.

In all, results provide interesting support for our expectations: even if violence is present, it does not

present a strongly salient “cost” to beneficiaries, given the large benefits. Indeed, given the landscape of social segregation in Brazil – particularly urban areas – this may not be surprising. Many low income individuals live in urban “slums” (*favelas* or *comunidades*), which in many cases may involve the presence of organized criminal groups (Magaloni, Franco-Vivanco and Melo, 2020). Thus, even if violence is present at middling areas in new MCMV communities, it may not be a relative increase if their previous neighborhood faced issues with violence. That is to say, MCMV could provide a substantial improvement in security for beneficiaries, particularly those coming from organized crime-dominated communities.

Neighborhood Effects

We evaluate potential effects of public housing on sub-city, neighborhood level crime in the city of Rio de Janeiro. This city provides a particularly hard case, as both perceived and real insecurity are high, and organized crime has had a persistent presence for decades. We draw on detailed, yearly crime data across the city’s 42 police precincts from the state’s Institute of Public Safety (ISP). We merge these data with detailed data of all *Faixa 1* MCMV units constructed, using their provided street address.²¹ In cases where street addresses were unavailable, we geo-locate them via Google Maps. Our unit of analysis is the precinct-year, and we identify 17 treated precincts. For descriptive statistics corresponding to these data, see Appendix C.

Our outcomes of interest are six types of registered crimes. First, intentional homicides, which are the most serious crime and most reliably recorded, as they pass through the coroners’ office; intentional homicide is also often used as an indicator of organized crime (Calderón et al., 2015; García-Ponce and Laterzo-Tingley, 2026). Second, vehicle theft, which is a comparatively well-reported crime, due to both the asset’s value and the need to receive a police report to file an insurance claim. Vehicle theft may be a symptom of both organized crime or property crime due to economic strain and relative deprivation. We also analyze four outcomes which may be subject to reporting irregularities, but provide important and complementary tests: robbery, assault, drug activity, and firearm seizures. We consider robbery to sup-

²¹Data from Brazil’s Ministry of Cities. We incorporate developments that were both exclusively *Faixa 1* and combined across levels (e.g., *Faixa 1.5 with 2*). Our results are robust to examining exclusively *Faixa 1*, as well.

plement vehicle theft as a test of property crime; assault, drug activity, and firearm seizures complement an analysis of symptoms of organized crime. All outcomes are log standardized rates per 100,000 precinct residents.

With these data, we rely on the Callaway and Sant'Anna (2021) dynamic difference-in-differences (DID) estimator. This strategy overcomes biases introduced by TWFE methods, and is designed to handle staggered treatments across time and space. We operationalize our treatment as a precinct's first treated year (first year when MCMV units arrive), and use never-treated precincts as our control group (see Figure C1). We incorporate two pre-treatment precinct-level covariates: population and the number of local health clinics. See Appendix C for a detailed discussion of our estimation procedure, including estimation equations.

Figure 3: Effect of MCMV Neighborhood-Level Rollout on Crime Indicators (Difference-in-Differences Approach)

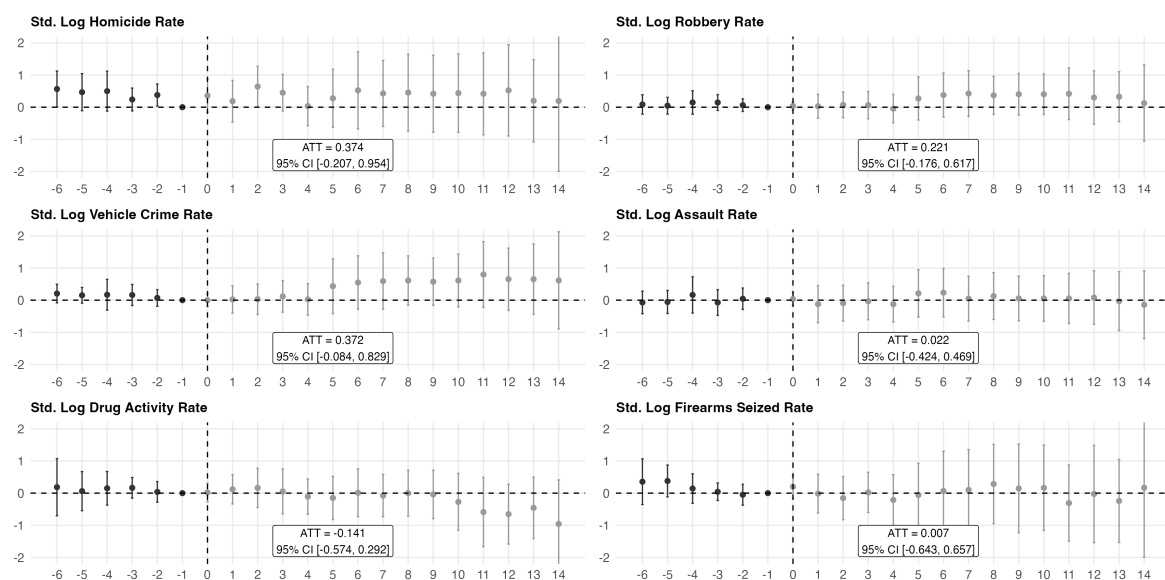


Figure Note: Outcome variables are homicide rate (logged) at the precinct level, and are standardized for interpretability. Black lines report pre-treatment estimates, and grey lines report post-treatment estimates with 95% CIs. Average treatment effects on the treated (ATTs) are included in boxes with 95% CIs reported.

Our main results are provided in Figure 3. Crucially, the parallel trends assumption, a necessary identification assumption for a DID design, is largely held across all tests.²² Pushing back on the proposed housing-crime linkage, we find null effects across all outcomes. First, the absence of increased property

²²Across all tests, homicide sees a violation in two periods (t -6 and t -2).

and non-violent crime in surrounding areas (e.g., vehicle theft, robbery) provides strong evidence against the spatial concentration and relative deprivation mechanism, which predicts these symptoms. Further, a lack of violent crime, and crimes often tied to organized criminal groups (e.g., homicide, vehicle crime, firearms seized, assault, and drug activity) once again provide necessary – but insufficient – evidence that the organized crime mechanism is not systematically at play.

These results provide strong evidence that public housing does not *generate* crime. However, that does not mean that neighborhoods where these developments are placed are free from insecurity. Indeed, in Table C2, we provide suggestive evidence that precincts where MCMV units were built were already more violent prior to MCMV arriving, consistent with Woo and Joh (2015)’s findings. This finding does not undermine our identification strategy, which relies on parallel pre-treatment trends rather than parallel levels. Rather, it suggests that the co-occurrence of MCMV developments and insecurity reflects pre-existing conditions – namely, that these units tend to be sited in areas that were already more vulnerable to violence – rather than a causal effect of housing on crime.

Our results are robust to re-specifying our control group as not-yet-treated (versus never treated, see Appendix C3). Further, given that one may consider our treatment operationalized as a continuous indicator measured by the number of units a precinct contains over time, we also analyze a dose-response relationship evaluating increases in the presence of MCMV units, among both never-treated and previously treated precincts. We rely on strategies proposed by De Chaisemartin and d’Haultfoeuille (2026) to estimate the effect of dosage increases (unit increases), rather than our more discrete measure of MCMV introduction (see Appendix C.4 for details). Results are consistent, and continue to show persistently null effects across all outcomes.

Citywide Effects

Finally, to evaluate *citywide crime*, we take advantage of a natural experiment in the design of the MCMV program. Beginning in June of 2011, the program introduced a differential price cap for supplier-side transactions related to MCMV unit costs in municipalities above and below 50,000 residents (R\$100,000

vs. R\$80,000, respectively).²³ That is to say, MCMV building developers were permitted to receive more from the government for their builds in municipalities just above the cutoff, incentivizing more construction in these communities. Others have demonstrated this discontinuity generated an increase in the MCMV housing supply (Gontijoi et al., 2018; Xu, 2023), which we confirm in Table D4. Thus, we use the 50,000 population threshold as an instrument to evaluate the impact of a surge in MCMV housing units on crime, as proxied by homicide rates.²⁴

To do so, we compile the number of housing units built via government subsidy across all municipalities from 2011-2019²⁵ from the Brazilian Ministry of Cities and unite this with municipal-level data on homicide rates drawn from Brazil's death records (DATASUS-SIM).²⁶ The 50,000 population threshold provides not only empirical leverage, but also captures cities that represent a typical case. Indeed, 94% of cities in the country are less than 100,000 residents.²⁷ Further, these cities are well distributed across the country, and even fall into its largest metropolitan zones.²⁸

With these data, we implement a fuzzy regression discontinuity design based on population data from Brazil's Institute of Geography and Statistics (IBGE), estimating the effect of the surge in MCMV housing units right above the threshold on homicide outcomes. On a yearly basis, crossing the population threshold corresponds to an increase in about 18 units, or about 199 units in cumulative stock. We examine if this surge results in changes in general homicide rates (logged) differentiating by race and age, as

²³These price caps changed slightly over time, but the population threshold remained. Prior to June 2011, the price cap for all municipalities below 250,000 individuals was R\$80,000 (Gontijoi et al., 2018). As noted by Gontijoi et al. (2018), developers had to meet similar requirements in terms of building parameters both above and below the threshold, including building sizes, number of rooms, and building material minimum quality standards.

²⁴While other pricing thresholds at other population cutoffs exist, we exclusively examine the 50k threshold for a handful of reasons. First, as of 2010, 4,839 out of 5,565 municipalities were under 50,000 residents, about 87% (Campolina Diniz and Vieira, 2016). The next most appropriate threshold would be that of 100k, however this price ceiling change was not introduced until later, limiting number of observations (Gontijoi et al., 2018).

²⁵We stop in 2019, as program changes were then made by President Bolsonaro

²⁶We focus on units corresponding exclusively to the lowest income bracket (*Faixa 1*) for the program. These types of units correspond to housing "blocks", or complexes, which were financed by the federal government from the outset.

²⁷As of 2012, there were 4952 cities of less than 50,000 residents and 324 between 50,000 and 100,000 residents.

²⁸In the metropolitan region of Rio de Janeiro city, there are multiple municipalities with less than 100,000 residents, including Rio Bonito, Cachoeiras de Macacu, and Japeri.

young and marginalized communities are typically most at risk of violent victimization (Calderón et al., 2015; Novaes, 2024), including by organized crime. We examine yearly rate changes for one and two year time windows. Following best-practices (Cattaneo, Idrobo and Titiunik, 2019), we rely on the optimal bandwidth that minimizes the mean-squared error (MSE), use a triangular kernel for observation weighting, and report robust standard errors. We cluster standard errors at the municipal level, and include fixed effects for year as well as mayoral incumbency and PT membership. In the Appendix, we confirm lack of sorting at the population threshold (Figure D2), the strength of the first stage of our identification (Table D4), and provide our full estimation procedure (Appendix D).

Figure 4: Effect of Surge of MCMV Units on Change in Homicide by Demographic Groups

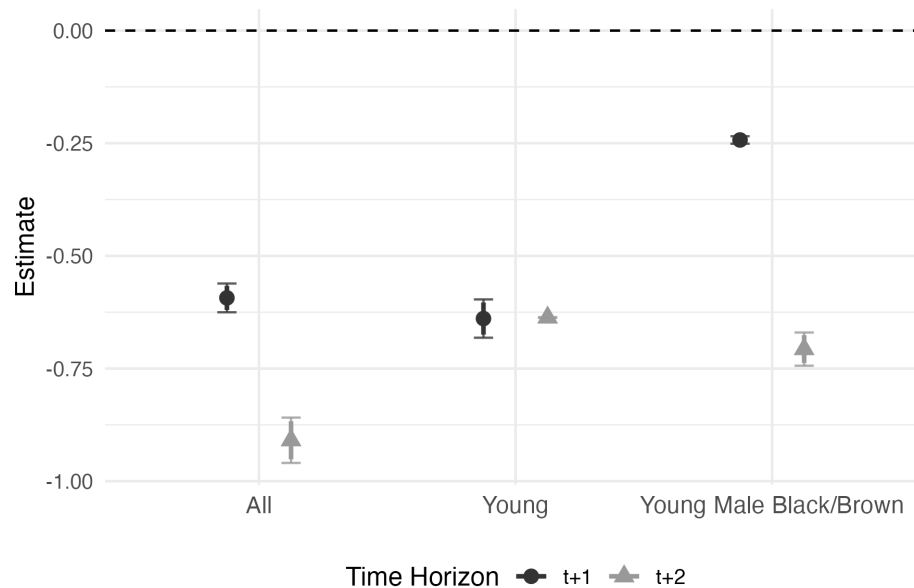


Figure Note: Outcome variables are standardized homicide rate changes (logged) from baseline to t+1 and t+2. Outcome is logged due to variable skewness, see Appendix D. For full estimates, first-stage F statistics, effective sample sizes, and bandwidths, see Appendix D.

Figure 4 reports coefficient estimates for our regression discontinuity design, including homicide outcomes differentiated by demographic group and time horizons (see also Table D5). These results suggest that a surge in MCMV units in fact depresses homicide rates, a result robust even among those most vulnerable to be victims of organized criminal violence. This pushes back against the supposed positive effect of housing on *citywide crime*, and particularly the organized crime mechanism. This result is robust to alternative bandwidths (Figure D3) and alternative outcome operationalizations (e.g., inverse hyperbolic

sine, Table D7; winsorized specification, Table D8).

In tandem with our findings of limited evidence of resident victimization and neighborhood-level crime, these results are inconsistent with a scenario in which public housing is systematically dominated by organized crime (Table 1). These accounts predict that increases in public housing stock should provide organized criminal groups with new territory to dominate, prompting expansion and conflict between rival groups. Instead, we find that increases in public housing are followed by reductions in homicide, not increases. Given the sweeping coverage of municipalities around our threshold across Brazil and its metropolitan areas, this result provides strong evidence against the idea that public housing fuels organized crime expansion and related conflict. That is not to say that organized crime presence in MCMV units is not possible; rather, our evidence indicates it is likely not systematic, and should not contribute to organized crime expansion.

Our findings instead align with a body of work showing that public housing and related programs reduce crime (e.g., Freedman and Owens, 2011; Disney et al., 2023). Our design, which exploits a discontinuity in program eligibility, is well suited to address this concern. However, it is less suited to adjudicate among the channels through which housing expansion reduces homicide. Some have theorized that public housing may decrease crime due to economic mechanisms, such as by ameliorating economic strain (Manea, Piraino and Viarengo, 2023) or raising the opportunity cost of crime (Belchior, Gonzaga and Ulyssea, 2023). In Table D9, we present evidence that the MCMV surge corresponds to an improvement in city's socioeconomic development, supporting this line of thinking. Disentangling these mechanisms - and assessing whether they extend beyond lethal violence - is an important direction for future research.

Study II: Perceptions and Implications for Public Support

Study I challenges the existence of the public housing crime nexus, even despite persistent media coverage to the contrary. However, perceptions are fundamental to support, often more so than reality. Building on the past study and theoretical mechanisms discussed, we now investigate how perceived public housing-induced security costs can affect support for these programs. Here, we operationalize the cost-benefit analysis logic by investigating if insecurity costs borne by public housing residents vs.

non-residents (e.g., citywide and neighborhood crime) differentially affects support among these groups. We delineate respondents based on housing eligibility criteria, leverage the means-tested income benchmarks of MCMV. We also consider support for policing initiatives in an exploratory analysis, as well as delineating individuals based on previous history with various public redistributive programs.

Methods

From February-March 2025, we conducted an online survey of 1,974 participants across Brazil’s urban and semi-urban territory, using quotas to meet demographic benchmarks.²⁹ Given our interest in program support based on individuals’ eligibility to participate in MCMV, we defined two groups of individuals (eligible vs. ineligible) based on the program’s income criteria.³⁰ Within eligibility blocks, respondents were randomly assigned to one of four experimental arms (control vs. three treatment conditions), such that our study employs a between-subjects, unpaired design.³¹ We provide descriptive statistics and balance tests in Appendix E.3.

Table 4: Vignette Experiment Design

Condition	Summary
Control	Increase in pet ownership (unrelated to housing)
Treatment 1	Positive impact of housing
Treatment 2	Positive impact of housing + beneficiary victimization
Treatment 3	Positive impact of housing + city-wide crime

The experiment varied information about public housing, all based on real news coverage. A sketch of treatment conditions are provided in Table 4, with full text in Table E1. Treatment 1 included an article

²⁹See Appendix A3.1 for a full description of our survey. We use Opinaia’s online panel.

³⁰Whether their family earns above or below 8,000 reais per month (about 1,500 USD), the approximate benchmark at the time of our survey. <https://agenciabrasil.ebc.com.br/economia/noticia/2025-04/entenda-ampliacao-do-programa-minha-casa-minha-vida>. Within our survey, we identify 1,263 who meet eligibility criteria, and 711 who do not.

³¹Given that our hypotheses are conditional on respondent income, we remove all participants who did not report their income. Within each block, respondents had a 0.25 probability of being assigned to the control or to any of the three treatment conditions.

discussing positive outcomes of a public housing program in Brazil. Treatments 2 & 3 both build on this article, but add a discussion of how the program resulted in victimization of program beneficiaries (T2) or crime that affected the broader city (T3). Following treatment assignment, respondents participated in a fixed budget allocation exercise designed to assess their support for public housing vs. investment in three other public services (financial assistance programs, security and policing, and education). Respondents were provided with ten coins which they allocated across these four areas. Budget experiments are useful to gauge support while mimicking real-life tradeoffs and minimizing “cheap talk” (Cafferata, Gingerich and Scartascini, 2023; Holland, 2023).³² We also include a series of questions following the experiment regarding respondents’ support for similar programs in their neighborhood or municipality via likert scales, as well as their interest in enrolling if they were able. Further, as discussed in our theoretical discussion, as a supplementary analysis, we also examine budget allocation for security. As a manipulation check, we also include a series of questions following the experiment regarding respondents’ emotional response to the vignette. Our main analysis involves estimating the effect of our treatments relative to the control condition on budget allocation for housing policy. To disentangle heterogeneous effects, we also estimate the difference between our security conditions (T2 & T3) relative to our positive information-only condition (T1), reported fully in Appendix E.10. We employ standard OLS models with heteroskedasticity-robust (HC2) standard errors, controlling for respondent gender, ideological self-placement, education level, and self-reported race (white).³³ We analyze the behavior of all respondents (pooled sample), as well as subsets of respondents based on program eligibility to examine heterogeneous effects across all treatment arms.

³²Full question wording in Appendix E.5.

³³Our tests in Appendix E.3 reveal balance across all characteristics, but some imbalance within blocks in terms of treatment assignment based on respondent race. We correct for this with controls.

Results

Main results for Study II are reported in Figure 5 (top panel).³⁴ As expected, respondents increase support for housing in light of positive information (T₁). Further, our analysis suggests that both MCMV eligible and ineligible respondents increase support for public housing when positive information is paired with victimization of housing beneficiaries (T₂). Substantively, the estimated treatment effect for all respondents in T₁ corresponds to an increase of 0.16 coins, or an approximately 8% increase in budget allocation from baseline.³⁵ These findings provide support for Hypotheses 1 & 2.

However, once security costs are framed as moving beyond residents to affect the broader city, this mobilizing effect disappears: relative to the control, allocations under T₃ are statistically indistinguishable from baseline, and relative to T₁ they decline significantly. The citywide frame, in other words, does not push support below where it started, but fully cancels the gains that positive information otherwise produces. And, contrary to our expectations, this pattern holds even among individuals eligible to benefit from MCMV, whom we expected to increase support in all cases regardless of crime costs.

We probe these results by analyzing an alternative measure of program support: rather than focusing on investment by the government generally, we ask respondents to report if they would support the building of public housing units in their local community.³⁶ Results in Figures E5-E6 paint an interesting picture. While baseline support is high among both groups, respondents ineligible to participate retract support – relative to all conditions – when crime is framed as city-wide. This pattern is not present for eligible respondents.³⁷ These results offer mixed support for H₃, suggesting heterogeneous effects may be present, but less in terms of *general* support for public housing, and more in terms of *localized* support.

³⁴We report a post-hoc analysis of minimum detectable effects in E.7. Our design detects effects of 0.19 coins pooled and 0.25–0.35 coins within eligibility subgroups at 80% power; null results should be read as bounding effects below these magnitudes.

³⁵In the control group, the mean coin allocation for housing is 1.98

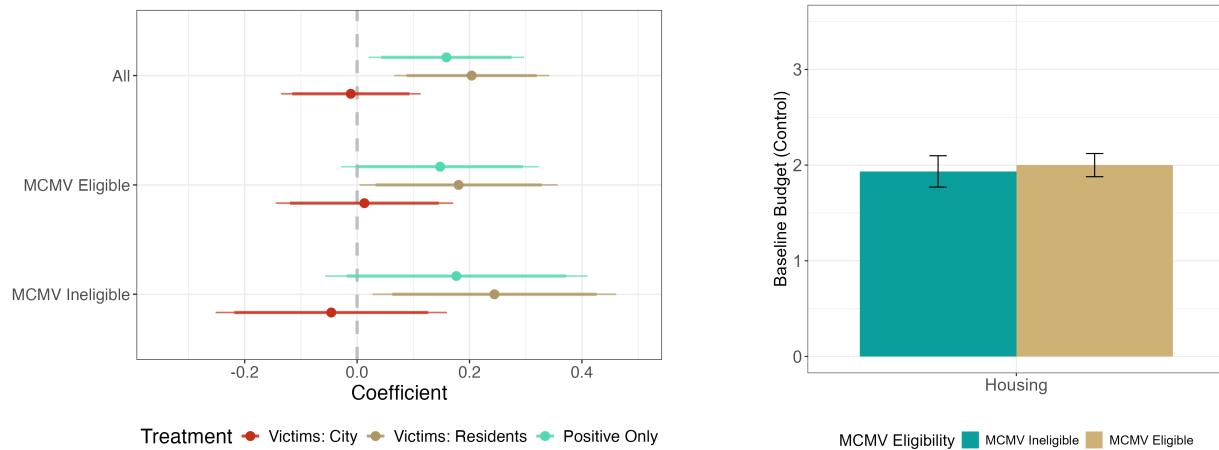
³⁶Specifically, we ask support both for building in their neighborhood and municipality.

³⁷We also examine if our budgetary results differ by previous beneficiary status (Appendix E.13.3) and more stringent measures of MCMV eligibility (Appendix E.13.2); results are largely consistent with our main budgetary results.

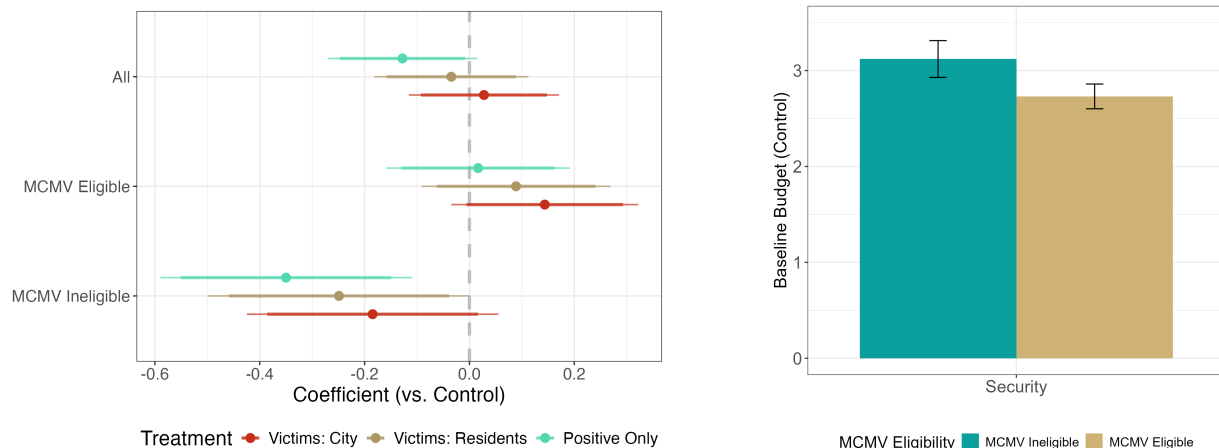
This is consistent with research on NIMBY dynamics and opposition to local housing initiatives.³⁸

Figure 5: Budget Allocation for Public Housing and Security

(a) Public Housing



(b) Security



This pattern implies two departures from our expectations. First, we find no evidence that eligible respondents' budgetary support is more insulated, though our design may not be able to detect very small differences between subgroups. Second, and consequently, eligibility appears to structure *where* support is withdrawn rather than *whether* it is. General budgetary support for housing responds to framing largely

³⁸We also explore another pre-registered outcome, which we were particularly interested in for eligible respondents: likelihood to express interest in enrolling in the program. We find that this is not influenced by any of our treatment conditions (Table E7). This finding, particularly among eligible individuals, is consistent with our expectations that support for the program should be resilient given the significant benefits it provides; indeed, expressed interest is high (about 70% of eligible individuals indicate they are interested, slightly lower for ineligible individuals).

irrespective of who stands to bear security costs, while support for construction in one's own neighborhood and municipality declines only among the ineligible. Therefore, the cost-benefit logic we propose is at play, but at the level of proximate housing siting rather than aggregate spending.

We also analyze effects of our experiment on security budget allocation (Figure 5, panel b).³⁹ Positive information regarding public housing alone – without discussion of security – depresses support for policing among respondents (pooled). This pattern is driven by those ineligible to participate in MCMV, who have higher baseline support for security. This suggests an innate association between public housing and crime among the MCMV ineligible group. Indeed, point estimates suggest this reduction attenuates as crime-related externalities are introduced (-0.25 under T2 and -0.19 under T3, versus -0.35 under T1), though we face power restrictions in precisely detecting this attenuation within the MCMV ineligible subsample.⁴⁰ We therefore treat this result as suggestive: those least likely to benefit from MCMV directly might revise their willingness to invest in policing when provided with information that social assistance is beneficial, or at minimum, not harmful to them. MCMV eligible respondents, on the other hand, show more stable support for policing. Combined with suggestive results in Appendix Table E4, where we find that city-wide crime increases spending on security among all respondents relative to the positive treatment, we consider these exploratory results as suggestive evidence of the second observable implication of our framework: frames particularly emphasizing widespread insecurity – that which affects individuals beyond the boundaries of public housing programs – may not merely depress support for public housing, but can also shift preferences toward security provision (Altamirano, Berens and Ley, 2020, 2024).⁴¹

Our estimates are robust to the exclusion of covariates (Appendix E.9). In Appendix E.13.3, we also

³⁹These results are exploratory. In our pre-analysis plan, we pre-registered our intention to analyze security budget allocation, but did not specify hypotheses.

⁴⁰Within the ineligible subsample we can detect effects only above roughly 0.35 coins at conventional power, See Appendix E.7.

⁴¹Relative to the control, the point estimate under the citywide frame is positive (0.14 coins) but falls below the 0.26-coin threshold our eligible subsample can reliably detect, so we treat a mobilizing response among eligible respondents as suggestive. Relative to positive treatment, respondents increase support for policing when crime is framed as citywide (0.15 coins, $p < 0.05$). The resident-victimization frame moves allocations in the same direction (0.09 coins), but this difference is smaller than our pooled detection threshold and is not statistically significant.

replicate our main analysis subsetting respondents by their previous experience with public assistance; results closely mirror our main findings. We also control for respondents' second-round vote choice, which does not alter treatment effects, despite partisanship strongly predicting baseline policy preferences (Appendix E.13.1). Finally, we analyze our data re-classifying MCMV eligibility more stringently, examining those only eligible for the program under more extreme income classifications (Appendix E.13.2).⁴² Treatment effects are substantively unchanged.

Discussion & Conclusion

We study whether public housing affects crime, and the consequences of perceptions of this relationship. Leveraging a natural experiment, a staggered difference-in-differences analysis, two beneficiary surveys, and an original survey experiment, we demonstrate that while increases in public housing stock do not increase crime, perceptions of this relationship, which are prominent and persistent across media narratives, can break down coalitions of support. We demonstrate these relationships in Brazil, investigating one of the world's largest public housing programs, *Minha Casa, Minha Vida*. But, it has also been widely criticized for fomenting crime in targeted communities.

Exploiting a population-based discontinuity in MCMV's pricing structure, we find that public housing expansion does not increase citywide crime. Rather, the estimated effect is negative, indicating reductions in homicide, and is robust across demographic subgroups, temporal windows, and model specifications. Beneficiary survey data reinforce this conclusion: program recipients report markedly improved living conditions, and insecurity ranks well below structural factors as a driver of relocation intentions. A staggered difference-in-differences analysis of Rio de Janeiro's police precincts (a hard case, given the entrenched presence of organized crime) likewise reveals no effect of MCMV construction on six crime indicators, including those most symptomatic of organized criminal activity (homicide, drug activity, and firearms seizures).

⁴²In terms of eligibility, we define a more stringent criteria of below 4,700 reais (Faixa 2 and lower) and below 2,640 reais (Faixa 1). In terms of existing beneficiaries, we examine those who report to have benefited from housing programs, unemployment insurance, or conditional cash transfer programs.

Interpreted in line with the three theoretical accounts summarized in Table 1, these null and negative results are inconsistent with three prominent theorized mechanisms linking public housing to crime increases. In all, we find consistent evidence that public housing does not *produce* crime. On the contrary, we find suggestive evidence that MCMV units were placed in areas that were already more violent, pre-treatment. This pushes back on narratives that public housing contributes to the spread of crime. That these findings diverge so sharply from prevailing public narratives is the central analytical puzzle the second half of this paper addresses.

Our experiment reveals that the perceived bearer of security costs – housing residents themselves or the wider city community – shapes dynamics of support. We show that support for public housing is not primarily structured by income or program eligibility, as standard distributional accounts predict, but by how the program’s externalities are framed. Positive information about public housing raises support among respondents both eligible and ineligible to participate in housing programs. This effect persists even when crime is described as victimizing residents of the complexes themselves. Only when insecurity is described as spilling into the wider city does this effect disappear: support in the citywide crime condition are statistically indistinguishable from the control, with the clearest declines appearing in support for housing in respondents’ own municipality among the MCMV-ineligible. The cross-class coalition, in other words, does not so much fracture as cease to be mobilizable.

Exploratory analyses of security spending point to a second shift: relative to the positive-information condition, respondents exposed to citywide crime reallocate toward policing. Notably, while housing preferences are largely unrelated to eligibility, security preferences are not. MCMV ineligible respondents drive both the reduction in policing support in light of positive housing information, as well as the maintenance of this support when crime concerns are invoked. These patterns suggest that the welfare–security tradeoff documented in prior work (Altamirano, Berens and Ley, 2020, 2024; Garland, 2017; Laterzo, 2024) may be channeled through the perceived *scope* of victimization rather than its actual occurrence. This distinction has direct implications for how public discourse regarding crime can shape mass preferences.

Our findings speak to a foundational tension in the political economy of redistribution: the condi-

tions under which cross-class coalitions form and endure. Existing accounts emphasize income position, ideology, and institutional design (Huber and Stephens, 2012; Rueda, 2007; Holland and Schneider, 2017; Berens, 2020), yet largely treat program externalities as fixed features of the policy environment. We argue that this assumption is consequential and often incorrect. The political salience of program costs is itself constructed – shaped by media narratives, discursive framing, and the perceived scope of victimization – and it is this construction, rather than the underlying reality, that governs coalition dynamics.

This suggests two revisions to our existing knowledge should be made. First, the fragility of redistributive coalitions is a function of framing, not solely of socioeconomic position. Coalitions are more durable than conventional accounts imply when externalities are localized, and more fragile than benefit-focused theories suggest when costs are portrayed as widespread. Second, insecurity narratives do not merely constrain redistributive policy, but rather shift mass preferences from redistribution toward security provision. In a context where security institutions have historically operated in coercive and racially targeted ways against low-income and race-class subjugated populations (Soss and Weaver, 2017), this potential substitution is not a neutral reallocation of preferences, but a reorientation of state priorities that compounds existing inequalities.

What this analysis establishes is that the political sustainability of redistributive programs cannot be understood by studying their effects alone. When narratives in public discourse associate housing provision with insecurity — irrespective of actual program effects — they may generate the political conditions under which redistribution loses and policing wins. But this is conditional: support for public housing survives crime narratives so long as the costs are understood to fall on beneficiaries themselves, and erodes only when insecurity is portrayed as reaching the wider public. This is not merely a problem of communication or agenda-setting. It reflects a structural feature of politics in unequal societies: the ease with which insecurity, once politicized, displaces demands for redistribution. Indeed, when even politicians within the government that initiate the program describe it as an epidemic of crime – as is the case in our opening vignette – the narrative may cease to require systematic evidence. Our findings suggest such claims perhaps rested on anecdotal, extreme occurrences rather than persistent, systematic, and generalizable effects.

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