ROCKWOOL Foundation Berlin
Institute for the Economy and the Future of Work (RFBerlin)

DISCUSSION PAPER SERIES 105/26

The Effect of Housing Access on Intimate Partner Violence: Evidence from the Family Options Study

K. Pun Winichakul, Ning Zhang

www.rfberlin.com APRIL 2026

The Effect of Housing Access on Intimate Partner Violence: Evidence from the Family Options Study

Authors

K. Pun Winichakul, Ning Zhang

Reference

JEL Codes: J61, J64, R23

Keywords: Housing assistance; Intimate partner violence; Independent housing; Economic stress; Spillover on children

Recommended Citation: K. Pun Winichakul, Ning Zhang (2026): The Effect of Housing Access on Intimate Partner Violence: Evidence from the Family Options Study. RFBerlin Discussion Paper No. 105/26

Imprint

RFBerlin
ROCKWOOL Foundation Berlin –
Institute for the Economy
and the Future of Work

Gormannstrasse 22, 10119 Berlin
Tel: +49 (0) 151 143 444 67
E-mail: info@rfberlin.com
Web: www.rfberlin.com

The Effect of Housing Access on Intimate Partner Violence: Evidence from the Family Options Study*

K. Pun Winichakul†
Ning Zhang‡

March 27, 2026

Abstract

Intimate partner violence (IPV) affects individuals globally: one-third of women worldwide and 41% of women in the US report experiencing some form of violence from their intimate partners (WHO, 2021; Leemis *et al.*, 2022). Yet, we know little about the effect of living situations on intimate partner violence and its underlying mechanisms. This paper uses a novel panel dataset of US homeless families to study the effect of housing access on the incidence of IPV. We find that housing access reduces IPV incidence by 5.6 percentage points, a 62% reduction relative to the control group mean. This effect is most precisely estimated for a housing program that offers a potentially indefinite subsidy and greater individual choice over living options, akin to a housing voucher. We identify higher rates of living in independent housing and reductions in financial stress as key mechanisms behind the decrease in IPV, while labor market improvements are not supported and partnership effects are more nuanced. We also find suggestive evidence that housing access affects children's outcomes through a reduction in IPV. Our paper has important policy implications for how housing access could reduce IPV incidence and contribute to improved gender equality.

JEL Codes: D10, D13, J16, J22, J31, K36

*We are very thankful for helpful and constructive comments from Abi Adams, Anna Aizer, Julius Ilciukas, Helmut Rainer, Uta Schoenberg, and seminar participants at ifo Institute, CUHK-IESR joint workshop, and International Symposium on Contemporary Labor Economics. We would like to thank Arnold Ventures (Grant ID Number 23-09969) and the Hong Kong Research Grants Council (ECS Grant Number 24606324) for their generous funding. We would also like to thank Sari Kerr and Shital Sharma at the Boston FSRDC for their support with this project. The Census disclosure clearance numbers for the estimates reported in this paper are 12603, 12718, 12783, 12900, and 12924. The authors declare no conflicts of interest.

†Smith College, kwinichakul@smith.edu
‡The Chinese University of Hong Kong, ning.zhang@cuhk.edu.hk

Disclaimer

Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3100. (CBDRB-FY25-P3100-R12603; CBDRB-FY25-P3100-R12718; CBDRB-FY26-P3100-R12783; CBDRB-FY26-P3100-R12900; CBDRB-FY26-P3100-R12924)

1 Introduction

In the United States, 41% of women are estimated to have experienced sexual violence, physical violence, and/or stalking by an intimate partner (Leemis *et al.*, 2022). The repercussions of intimate partner violence (IPV) extend beyond personal safety and have significant long-term consequences for the overall well-being of women and their families (Adams *et al.*, 2024; Bindler and Ketel, 2022). The existing economics literature on IPV has primarily examined how labor market outside options influence the incidence of domestic violence (Aizer, 2010; Anderberg *et al.*, 2016; Bhalotra *et al.*, 2025; Erten and Keskin, 2021; Sanin, 2021a). Most of this work finds that improved (reduced) labor market opportunities for women reduce (increase) the likelihood of experiencing physical violence. However, employment opportunities are only one form of outside option available to people facing IPV (Adams, 2025). Non-labor-market factors—such as housing access—are another category of outside options that could provide safety and economic security for women.

Housing access may be particularly important because it addresses barriers to exiting relationships that labor market opportunities cannot overcome. Unlike employment, which takes time to secure and generates periodic income, housing access can provide immediate physical safety when women must leave dangerous situations quickly. Survey data from IPV survivors identifies lack of housing as primary reason for remaining in or returning to abusive relationships. According to the National Domestic Violence Hotline, 84.5% of domestic violence survivors had unmet housing requests in 2022, and 46% of homeless women reported staying in abusive relationships because they had nowhere to turn (National Domestic Violence Hotline, 2025). Even when labor markets are strong, abusers can use economic control tactics, such as sabotaging employment, controlling finances, or creating debt, to prevent partners from accessing employment-based outside options (Adams *et al.*, 2008; Stylianou, 2018).

Abusers also frequently use housing control as a coercive tactic, threatening eviction to trap women in abusive relationships (Sharp, 2008). Housing programs that do not require an abusive partner's cooperation (e.g., public housing lotteries, individually-awarded vouchers) can provide exit options that bypass this control. This is especially critical for women with children, who

face higher costs of leaving and may struggle to convert even stable employment into adequate family housing in expensive housing markets. Despite the potentially distinct role of housing access in reducing IPV, there is limited evidence on whether these forms of non-labor-market outside options have the potential to reduce IPV incidence.

In this paper, we study the effect of housing access on IPV incidence. We also examine the underlying mechanisms through which housing access shapes IPV prevalence. We distinguish between direct and indirect channels. First, access to housing could directly decrease IPV incidence by providing independent, safe, and stable shelter where victims can live apart from their abusers. This is the core resource that housing programs are designed to deliver. Second, housing access may indirectly reduce IPV through several mediating pathways: (1) lowering financial and psychological stress that accompanies unstable housing situations, which could decrease victims' reliance on abusive partners for economic support or reduce financial tensions that inflame aggression[1]; (2) affecting partnership status and relationship dynamics; and (3) offering complementary resources that may introduce new labor market opportunities that empower victims to achieve self-sufficiency by increasing labor market attachment and reducing economic dependence on abusive partners. Distinguishing between direct and indirect channels has important policy implications. If the direct shelter channel dominates, expanding housing supply should be the priority. If indirect channels are substantial, bundling complementary services such as financial stress relief or labor market programs with housing solutions could be more cost-effective in reducing IPV. Finally, we extend our analysis beyond adult IPV to explore potential spillover effects on children. Specifically, we consider whether reductions in household IPV incidence due to housing access improve children's outcomes.

To explore our question, we use restricted-use data from the Family Options Study, a randomized controlled trial conducted by the US Department of Housing and Urban Development (HUD) that assigned homeless families to receive different housing assistance resources. Families were tracked and surveyed over multiple years; in a baseline survey at the time of random assignment,

[1]Bhalotra *et al.* (2025), Arenas-Arroyo *et al.* (2021), and Schwab-Reese *et al.* (2016) examine how economic stress such as job loss and COVID-19 affect IPV.

a 20-month follow-up survey, and a 37-month follow-up survey.[2] The result of this endeavor is a novel panel dataset of US homeless families, some of whom received greater housing support and services relative to others. To the best of our knowledge, this dataset represents the first panel dataset that directly links the random assignment of housing support to the incidence of IPV.

The Family Options Study (FOS) dataset is an ideal resource for our research question due to several key factors. First, and as noted above, the FOS is a randomized control trial. This study design gives us an ideal setting to test for the causal effect of housing access on the incidence of IPV. Second, much of the existing research on IPV relies on cross-sectional data. In contrast, the panel structure of the FOS and the rich information collected in each survey wave about respondents, nearly all of whom were women (92%), enables us to examine dynamic channels through which housing assistance affects IPV. These channels are often overlooked or cannot be explored in the current literature due to data limitations. Third, the FOS includes information on both respondents and their children. This allows us to link adult respondent experiences with IPV to a range of children's outcomes. That is, the dataset allows us to examine the effect of housing access on both potential IPV reduction and its downstream impact on children's outcomes.

Families who participated in the FOS were randomized into one of four conditions. There were three forms of housing assistance that families could receive priority access to: (1) SUB, which provided a potentially permanent housing subsidy, such as a housing voucher; (2) CBRR, which provided short-term community-based rapid re-housing; and (3) PBTH, which provided short-term, project-based transitional housing and additional non-housing support services. The fourth condition, "usual care" (UC), serves as the control group, as families were offered standard homeless and housing support services available in a shelter but did not receive priority access to any specific assistance program. Our main analysis exploits the random assignment of households into different housing support programs (treatment arms) to analyze the pooled treatment effect of any ("ANY") housing assistance program and the individual effect of different forms of assistance on IPV incidence, compared to usual care.

We find that enrollment in ANY type of housing assistance program reduces the incidence

[2] As of February 2026, there is currently a 120-month follow-up survey data collection in progress.

of IPV. Our local average treatment effect estimate indicates that participation in ANY housing assistance program (SUB, CBRR, or PBTH) reduces the incidence of IPV in the 6 months prior to the respective 20- or 37-month follow-up surveys by 5.6 percentage points (p.p.). This effect is equivalent to a 62% reduction relative to the control group mean. When examining the effect of each type of housing assistance, we find that participation in SUB, which is designed to provide long-term housing resources, reduces the incidence of IPV by 6.6 p.p., a result that is statistically significant at the 1% level. We also find decreases in IPV among those who receive CBRR and PBTH, which provide shorter-term housing support, ranging from a 3.1 p.p. reduction to an 8.0 p.p. reduction; however, these estimates are not statistically significant. In comparing the separate effects of each housing program, the more precisely estimated effect of SUB suggests that longer-term housing assistance may be more effective in reducing the incidence of IPV. Further, we develop bounding estimators to address possible attrition bias. These bounds are similar to our main estimates, suggesting that our results are robust to the potential effects of survey attrition.

We not only evaluate whether housing access reduces IPV but also analyze *why* the incidence of IPV falls. We first examine the direct channel of how housing access may reduce IPV; specifically, by offering independent housing for women. Second, we examine three indirect channels: (1) potential reductions in respondent financial and psychological stress; (2) the partnership status of respondents; and (3) potential improvements in labor market outcomes. Because our most precisely estimated effect is found for SUB, we focus our mechanism analysis on the pooled effect of ANY housing program takeup and the SUB-specific program effect. We approach this analysis in two ways. Our first approach uses the same empirical strategy used for our principal results, but replaces the IPV outcome with variables reflecting the direct and indirect mechanisms described above as the outcomes of interest. Our second approach quantifies the effects of the indirect channels using the causal mediation analysis framework described in Bhalotra *et al.* (2022), which takes advantage of the panel data structure of the FOS and the sequential timing of different events. We believe the causal mediation estimates provide useful insights into the economic significance of the indirect channels. However, because the mediator variables are only observed in a discrete survey wave rather than continuously, these estimates likely represent a

specific trajectory of each indirect channel.

First, we find evidence that housing access substantially increases independent housing. This effect is both large and persistent, as we see sharp increases in independent housing in the months immediately after random assignment that continue over three years later (39% and 25% increase relative to the control group mean in the 20th and 37th month, respectively). This result supports the direct mechanism for how housing access contributes to IPV reduction by providing safe, stable, and independent housing.

Second, we find that housing access indirectly affects IPV by lowering economic stress. We estimate that housing assistance reduces economic stress by 0.22 standard deviations and can explain at least 10% of the overall effect of housing assistance programs on IPV. We also document complementary evidence that housing access reduces psychological distress and substance abuse.

Third, we find that housing access increases the likelihood of being single. However, this increase in single status does not appear to be consistent with the explanation that respondents break free from long-term abusive relationships. Instead, we document that reductions in IPV are concentrated among those who were single at the time of the baseline survey. A more likely explanation is that housing access helps respondents who are not in existing long-term relationships avoid unstable and abusive short-term encounters.

Fourth, in our setting, we do not find that housing access reduces IPV by strengthening victims' labor market outcomes. If anything, we find that housing assistance programs reduce respondents' labor market attachment and working hours. Therefore, our evidence collectively shows that compared to labor market outside options (the focus of most existing literature), non-labor market outside options such as housing are an important independent factor affecting IPV.

Finally, we explore the impact of household experiences with intimate partner violence on children's outcomes, complementing past work on this question (Bhuller *et al.*, 2024; Carrell *et al.*, 2018; Moss *et al.*, 2023). We specifically focus on the downstream spillover effects of decreases in IPV due to housing. In complement to previous work exploring the effects of housing on children's outcomes, our results suggest that housing access can have a direct positive impact on children, particularly on their socioemotional and behavioral development (Jacob *et al.*, 2015;

Ludwig *et al.*, 2013). When using the causal mediation analysis framework described above, we estimate an effect that is directionally consistent with improvements in children's outcomes being partly attributable to the reduction in the incidence of IPV in the household; however, this estimate is not statistically significant.

Our primary contribution is to the literature on the effect of women's outside options on the incidence of domestic violence. Most existing studies focus on the availability of labor market outside options, and examine the impact of gender-specific labor market shocks on the occurrence of physical violence (Aizer, 2010; Anderberg *et al.*, 2016; Bhalotra *et al.*, 2025; Erten and Keskin, 2021; Sanin, 2021b). Most of studies have demonstrated that greater (weaker) labor market opportunities for women reduce (increase) the likelihood of physical violence, although a few find evidence of "male backlash" in response to stronger women's economic standing. In contrast to this work, we focus on the more immediate outside option of providing women with safe and independent housing and examine its impact on domestic violence.

Second, we contribute to a complementary literature that explores the broader determinants of domestic violence. Past research identifies factors such as educational attainment (Erten and Keskin, 2018), cultural dynamics (Tur-Prats, 2019, 2021), the tax and transfer system (Bobonis *et al.*, 2013), health innovations (Papageorge *et al.*, 2021), gender ratios (Amaral and Bhalotra, 2017), cold weather (Bollman *et al.*, 2026), pandemic-related lockdowns (Leslie and Wilson, 2020; Bullinger *et al.*, 2021; Bhalotra *et al.*, 2021a), and homeownership (Lagomarsino and Rossi, 2024).[3] Most papers on both labor market outside options and other determinants of domestic violence use cross-sectional data and study short-run effects. Our paper uses a novel US household panel dataset to provide the first evidence on whether housing assistance affects IPV and to identify the underlying mechanisms through which housing support influences the incidence domestic violence.

Third, we add to the literature on policies that empower women to leave or avoid abusers and its related effect on the incidence of IPV. These studies show that IPV is closely related to social

[3]Lagomarsino and Rossi (2024) takes homeownership as an example of an illiquid asset to explore how jointly-owned and single-owned illiquid assets may differentially affect IPV. Our paper instead focuses on the principal effects of housing access on IPV, where access is provided as different forms of housing assistance and where we can identify multiple underlying mechanisms.

norms and institutions that determine whether women can leave their partners easily (Bhalotra *et al.*, 2021b; Alesina *et al.*, 2021). Specifically, researchers note that the introduction of unilateral divorce in the US, Spain, and Egypt can decrease domestic violence (Stevenson and Wolfers, 2006; Brassiolo, 2016; Corradini and Buccione, 2023). In contrast, García-Ramos (2021) finds that the introduction of unilateral and no-fault divorce in Mexico increases IPV in the long run because IPV is used more often to prevent women from leaving marriages. In our paper, we show that housing assistance policies can empower women to live in independent and safe shelter, avoid abusive partners, and thus reduce the incidence of IPV.

Fourth, we connect to the literature that studies the repercussions of domestic violence (Bindler and Ketel, 2022; Adams *et al.*, 2024; Chang *et al.*, 2023). For example, Bindler and Ketel (2022) assess both the short- and long-term earnings costs of reporting domestic violence, while Adams *et al.* (2024) analyze the dynamic labor market costs of cohabiting with an abusive partner. Within this limited body of literature, a subset of studies focus on the dynamic impact of domestic violence and its repercussions on women's outcomes through the termination of relationships with abusers (Anderberg *et al.*, 2023; Adams *et al.*, 2024). Because our study identifies housing opportunities as a channel that can reduce the incidence of IPV, we present an opportunity to reduce the long-term costs and negative repercussions associated with domestic violence.

Finally, our paper contributes to understanding the effect of housing access on individual socioeconomic outcomes and inequality. Existing research focuses on the effect of housing on, for example, labor market outcomes, neighborhood quality, and child and youth outcomes (Jacob and Ludwig, 2012; Zhang, 2025; Chetty *et al.*, 2016; Jacob *et al.*, 2015; Ludwig *et al.*, 2013). This work either uses variations of the *Moving to Opportunity* program or the random assignment of housing assistance in specific geographic areas to study their outcomes of interest, focusing on families who are either in public or private housing at the time they receive housing support. In complement to this work, we use novel panel data from homeless families across the US to explore how the random allocation of housing assistance to families affects IPV and its implications for gender inequality.

Our paper is organized as follows. In Section 2, we provide context on the prevalence of

intimate partner violence and institutional details about housing in the United States. Section 3 describes the FOS dataset and reports summary statistics. Section 4 outlines our empirical strategy. Sections 5 and 6 report the main results of our analysis of the impact of housing access on the incidence of IPV and the underlying mechanisms. Section 7 analyzes the contribution of reduced IPV due to housing improvements on children's outcomes. Finally, in Section 8, we discuss the policy implications of our results and conclude.

2 Institutional Background

Intimate partner violence (IPV) is highly prevalent in the United States, particularly among low socioeconomic households. According to the National Intimate Partner Violence Survey (NIPV, 2010-2012), 53% of high school dropouts reported having experienced IPV. This is similar to the incidence of IPV reported by our sample of homeless families at the beginning of the FOS study period (50%, see Table 1 below).

2.1 Housing Access as a Critical Barrier to Leaving Abusive Relationships

Access to independent, safe, and stable housing represents one of the most significant barriers preventing women from leaving abusive partners. Housing instability is both a cause and a consequence of IPV: women experiencing housing insecurity are more vulnerable to abuse, while those fleeing abuse often face homelessness (Wilson *et al.*, 2021; Baker *et al.*, 2003). Past qualitative research has identified lack of housing as a primary reason for remaining in or returning to abusive relationships. For example, when Jaffe *et al.* (2002) asked survivors of domestic violence (who were also mothers) to talk about the reasons why they had not left their violent partner earlier, common themes were concerns about safe and affordable housing and access to money. Evidence from domestic violence shelter providers further highlights the critical role of housing. A study of 3,400 shelter residents across eight states found that 84% identified, "finding housing I can afford" as a need at shelter entry, second only to safety (Lyon *et al.*, 2011).

Housing access can affect an IPV victim's ability to escape abuse through the direct channel of providing independent housing. For one, independent housing provides physical separation

from abusers, which is essential for safety. Studies show that the period immediately following separation is often the most dangerous for IPV victims, with increased risk of severe violence and homicide (Campbell *et al.*, 2007). Without access to safe housing alternatives, women may not be able to achieve this critical physical separation. Housing instability also creates dependencies that abusers exploit. Qualitative research with IPV survivors reveals that abusers frequently use housing control as a coercive tactic, threatening eviction or sabotaging rental applications (Sharp, 2008). When women lack independent housing options, these tactics effectively trap them in abusive relationships. Finally, lack of stable independent housing forces some women into a cycle of temporary living arrangements with partners, acquaintances, or in shelters, environments that may expose them to new or recurring abusive encounters (Baker *et al.*, 2010).

At the same time, the US housing market has seen a significant increase in real estate prices over the past few decades. Between 2000 and 2020, residential housing prices in the US increased by 42% in real terms, ranking among the highest globally over this period. This trend has contributed to an increase in homelessness rates and heightened susceptibility to homelessness (Meyer *et al.*, 2021). Even households that are not categorized as homeless often allocate a significant portion of their income toward housing expenses. In 2020, 46% of American renters directed 30% or more of their income toward housing costs, with 23% allocating at least 50% of their income (US Census American Housing Survey, 2020). Those who pay a higher proportion of their income toward housing expenses are often low-income households that live in neighborhoods characterized by high poverty rates and limited employment prospects (HUD, 2019). The intersection of rising housing costs and IPV vulnerability is particularly acute for women with children. Single mothers comprise a substantial proportion of homeless families (Meyer *et al.*, 2021). To avoid homelessness or cope with housing cost constraints, victims may be compelled to cohabit with abusers and depend on them for shelter. Issues such as financial stress, substance abuse, and alcohol-related problems exacerbated by housing instability may further contribute to domestic violence. Greater financial fragility and growing homelessness risk in the United States over the past couple decades has thus likely increased individuals' susceptibility to remaining in or entering situations involving IPV.

The primary target of US housing program initiatives are exactly these populations, who may now be experiencing heightened risk of becoming a victim of IPV due to US housing market trends. Therefore, our study could have meaningful policy implications. It has the potential to identify the extent to which housing access through assistance programs could affect the incidence of IPV, while focusing on a population where the economic and social returns to reducing the costs of experiencing domestic violence may be particularly large.

3 Data and Descriptive Statistics

3.1 Data

We use restricted-use data from the US Department of Housing and Urban Development's Family Options Study. Much of the data description that follows is summarized from material available on the HUD website about the FOS (HUD, 2025).

The FOS is a multi-site randomized control trial designed to investigate the effects of different housing and service interventions for homeless families. Between September 2010 and January 2012, 2,282 families (including more than 5,000 children) were enrolled in the study from emergency shelters in 12 communities nationwide (see Online Appendix Figure A.1 for locations).[4] Families were randomly assigned to one of three treatment interventions or a control condition:[5]

1. Subsidy-only (SUB): families were offered a potentially permanent housing subsidy, often delivered through a Housing Choice Voucher (HCV). Beyond the housing subsidy, families did not receive additional non-housing support services.

2. Community-based rapid re-housing (CBRR): families were offered temporary rental assistance for up to 18 months. However, the length of assistance provided through the CBRR intervention was expected to be roughly equivalent to project-based transitional housing (PBTH) described below. Unlike PBTH, families assigned to CBRR could use assistance on

[4]We provide further background on the FOS in Online Appendix B.
[5]The random assignment process is presented in Online Appendix Figure A.2

conventional, private-market housing. Families assigned to this intervention also received limited amounts of other housing-focused support services.[6]

3. Project-based transitional housing (PBTH): families were offered temporary housing assistance for up to 24 months (with an average expected length of stay between 6-12 months) in transitional housing facilities. The transitional housing offer was combined with non-housing support services, which included an assessment of family needs, social worker case management, and the provision of (or referral to) services to meet identified needs. Example services or resources that were provided or referred included those related to employment, child care, transportation, entitlements, medical access, behavioral health, trauma, safety, children's emotional, cognitive, and developmental needs, and child welfare and family preservation and reunification.

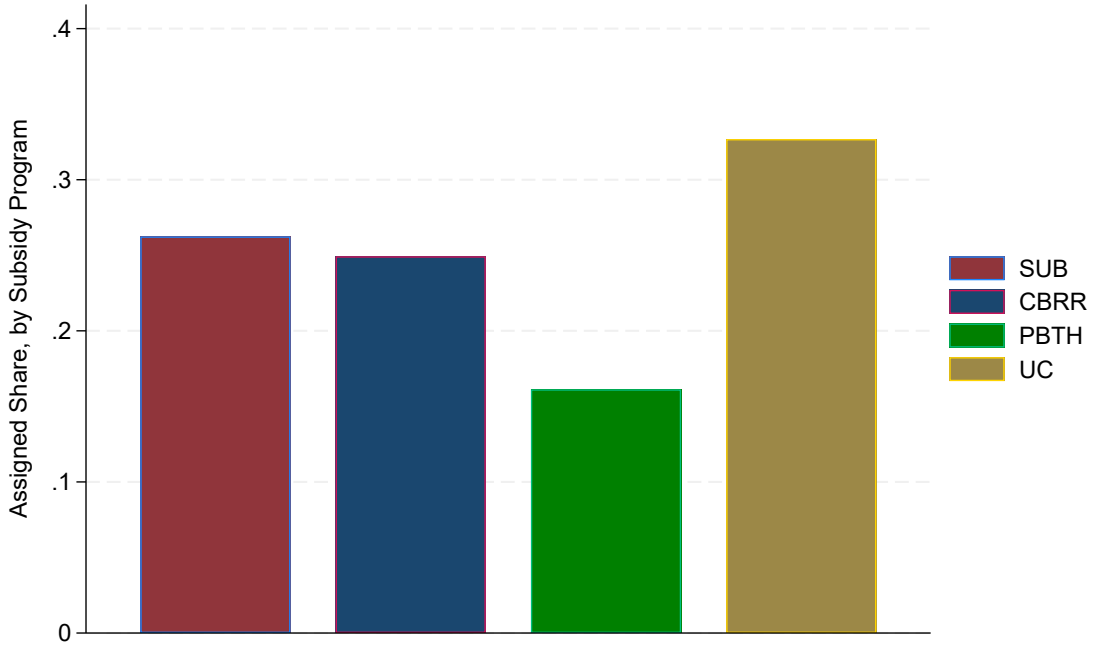
4. Usual care (UC): families could receive assistance that they would normally access from a shelter in the absence of these other interventions. The type of usual care that families could access varied by shelter. One example of continued support provided by some emergency shelters is a shallow rental subsidy funded through the Homelessness Prevention and Rapid Re-housing Program (HPRP). These rental subsidies are of the same general class of assistance offered in the CBRR intervention; however, the CBRR intervention was further standardized to mirror the level and length of assistance offered in PBTH, while HPRP subsidies generally varied more in both their levels and length of rental assistance. Further, these rental subsidies also varied with respect to whether other non-housing support services were provided.

To summarize, SUB and CBRR provided rental subsidies for private market apartments or single-family homes, whereas PBTH provided temporary housing in project-based facilities. The expected length of housing assistance differed across SUB, CBRR, and PBTH interventions. SUB assistance was indefinite as long as tenants abided by the terms of the assistance; for example, they had to cooperate with annual recertification of income used to determine the tenant's share

[6]For example, other housing-focused services provided additional support toward the housing placement process.

of the rent and continue to live in standard quality housing. The median expected length of assistance for families referred to CBRR and PBTH was intended to be similar, around 6 months. In contrast, UC often provided very short-term assistance; the median expected length of stay for families for those receiving usual care in shelters was 30-90 days. Finally, in terms of support services, SUB and CBRR provided limited housing-focused services. In contrast, PBTH provided comprehensive non-housing services, while those assigned to UC could receive varied support, which includes rental subsidies and housing and non-housing services, but depended on what programs had capacity and were available in a particular shelter. The share of families assigned to each of the programs is shown in Figure 1.

Figure 1: Share of Random Assignment by Housing Assistance Type



Notes: Figure depicts the share of the 2,282 families assigned to the different housing assistance programs. Data is from the Family Options Study public-use micro-level data.

3.2 Intervention takeup

Not all families who were assigned to a housing assistance program ultimately received the housing and non-housing services associated with the intervention. Families assigned to SUB, CBRR, or PBTH had to contact the housing assistance program provider to formally enroll in the program

while staying in the shelter. Then, for families who enrolled in CBRR and most who enrolled in SUB, their offer of rental assistance required them to find a housing unit with a willing landlord to lease the housing. For families assigned to PBTH and for a few families for whom the SUB offer was public housing or project-based assisted housing, enrolling in the program was close to synonymous with a guaranteed move into new housing. Not all families took these further steps; therefore, "takeup" of housing assistance interventions is lower than 100 percent. This feature motivates our use of two-stage least squares (2SLS) as our preferred empirical method, which is further detailed in Section 4 below.

3.3 Survey timing

Families participating in the FOS were tracked for multiple years after random assignment. Comprehensive surveys were conducted at the time of random assignment (baseline) and subsequently at 20, 37, and 120 months (10 years) after assignment.[7] The surveys capture extensive information about respondent demographics, health (both physical and mental), labor market outcomes (e.g., employment and earnings), welfare program participation, risky behaviors (including drug abuse and crime), location, neighborhood quality, and family behavior, including IPV. Detailed data on family composition and family members was also collected, such as relationship status, family member demographics, health, labor market outcomes, and criminal behaviors. The 20- and 37-month surveys are available for approved researchers to access through the US Census Federal Statistical Research Data Centers (FSRDCs), while the 120-month survey is currently unavailable.[8] Therefore, in this project we use information from the baseline survey and outcomes measured in the 20- and 37-month follow-up surveys.

As far as we know, the FOS dataset is the only US panel dataset incorporating information on both housing assistance and IPV incidence, and its longitudinal nature offers us an ideal resource to investigate our research question. Not only can we analyze the impact of housing subsidies on

[7] As of February 2026, the 120-month follow-up survey data collection is yet to be finalized.

[8] The magnitude of respondent attrition in the 120-month follow-up survey and its potential subsequent impact on our ability to produce precise 10-year follow-up results is unclear. Therefore, we believe our approach, which is focused on the 20- and 37-month outcomes, provides meaningful insights into the dynamic effects of housing assistance on IPV as we can implement methods to effectively address lower degrees of attrition. However, our use of the 20- and 37-month responses uses outcomes that may be considered more medium- rather than long-term.

IPV for three years after study enrollment, but we can also explore of the dynamic mechanisms influencing these outcomes. The population of the FOS study, where the majority consists of female-headed households with children who have some experience of being homeless, provides insights into the effect of housing subsidies on IPV and their lasting influence on gender inequality and children's outcomes.

It is worth noting that panel survey data including IPV is not only novel in the US but is also unique among other developed countries. For example, in EU countries, survey data on IPV is usually cross sectional. Non-survey panel data of IPV are more likely to be built from administrative records such as police and hospital records. These administrative records likely suffer from greater underreporting issues or capture only the most serious cases (European Union Agency for Fundamental Rights, 2015), while the FOS preserves respondent confidentiality and any reported experiences with IPV are not subject to further mandatory legal reporting and proceedings. The only panel survey data in developed countries that we know of is the Australian Longitudinal Study on Women's Health (ALSWH), which is collected from the 1946-51 cohort between 1996 and 2016.[9] However, as previously noted, these EU and Australian panel datasets do not include a direct link between experiences with IPV and housing assistance, which is a further novel feature of the FOS dataset.

3.4 Descriptive statistics

We use the FOS baseline survey to provide summary statistics on respondents by housing assistance program assignment. The means and Benjamini-Hochberg adjusted p-values for differences in sample characteristics between each housing program treatment group and UC are reported in Table 1.[10]

Overall, more than 90% of respondents are female, with an average age of approximately 30 years old. About 20% of the sample is white, and more than 25% have at least some college education, while less than 40% are high school dropouts. At the time of the baseline survey, 38%

[9]The ALSWH survey included questions about violent relationships with a partner/spouse, with 10,966 women answering IPV questions over six surveys.

[10]Since we have multiple treatments, we use Benjamini-Hochberg adjusted p-values to correct for multiple comparisons. The means and unadjusted p-values are reported in Online Appendix Table A.1.

of respondents were married or cohabiting, and the average number of children in a household was two. On average, respondents reported more than one year of prior homeless experience. We do not find statistically significant differences across most demographic characteristics between the UC group and the housing treatment groups.

With respect to labor market characteristics, only 18% of the sample was working at the time of the baseline survey, although more than 95% had some prior work experience. The fact that more than one-fourth of respondents have at least some college education and nearly all have prior work experience, yet a vast majority are currently unemployed, indicates that these women likely experienced negative labor market shocks or face childcare constraints that prevent them from affording rent. These descriptive statistics suggest that the homeless women with children in this study may not represent an exceptional population, but instead reflect characteristics of many American women with children who face economic vulnerability.

In terms of welfare program participation, 40% of respondents were receiving TANF benefits, 88% were receiving SNAP benefits, and 36% were receiving WIC benefits. As for their health status, around 10% of respondents described their overall health status as "poor." More than 20% reported experiencing serious psychological distress, and a similar share reported substance abuse (drug or alcohol) at the time of the baseline survey. Across most variables, we find no statistically significant differences in labor market, welfare program participation, or health variables between the control group and housing treatment groups.[11]

The baseline survey also collected information on intimate partner violence (IPV) since adulthood. Strikingly, roughly 50% of respondents reported ever experiencing IPV, a higher rate than estimates from surveys such as the NISVS.[12] The higher IPV prevalence among homeless individuals in our sample highlights their vulnerability to domestic violence and underscores the importance of stronger policy attention to this issue. We find no significant differences in IPV experience in adulthood across treatment and control groups. Altogether, the lack of significant differences across these various treatment and control group characteristics at the time of the

[11]One exception is respondent earnings, where the control group appears to have higher average earnings at baseline than respondents in the SUB treatment.

[12]It is worth noting that our sample is younger compared to the NISVS sample. The difference in incidence of IPV would be even larger if we accounted for the age difference between the FOS and the NISVS.

baseline survey supports the internal validity of the random assignment process.

4 Empirical Specification

Our empirical specification uses the random treatment assignment mechanism of the FOS to investigate the impact of housing access on intimate partner violence. Since treatment assignment does not map one-to-one with intervention takeup, our preferred empirical method is two-stage least squares (2SLS). In the first stage, we estimate:

$$HS_{ij} = \gamma_0 + \gamma_1 R_{ij} + \mathbf{X}_i \Gamma + \mu_t + \mu_r + \epsilon_{it} \tag{1}$$

where HS_{ij} is a dummy variable indicating whether respondent i ever uses housing assistance program type j after random assignment, where $j =$ SUB, CBRR, or PBTH, i.e., our takeup measure.[13] To increase our statistical power, we also estimate this specification where $j =$ ANY housing assistance program. In other words, we estimate whether a respondent uses any of the housing support programs, given random assignment to any of them.

The term R_{ij} is the random assignment of respondent i into housing assistance program type j at the time of the baseline survey. The terms μ_t and μ_r represent time period t (survey wave) and region r (study site) fixed effects, respectively. The term ϵ_{it} is the error term. We discuss the control variables included in vector $\mathbf{X}_i$ when describing Equation 2 below. We pool our data across follow-up surveys, and cluster standard errors at the respondent level.

The parameter of interest in the first stage is γ_1, which measures the effect of the random assignment of a respondent to an assistance program on the actual use of that program. This parameter can inform us of the takeup rate of a housing assistance program due to the random treatment assignment mechanism.

[13] In our first-stage estimates using the public-use FOS dataset reported in Section 5, we use the takeup variable as measured in the Program Use and Living Situation survey, which notes whether respondents enroll in their randomly assigned program at any point between random assignment and 37 months afterwards. For our final 2SLS estimates, our first stage uses the takeup variable (reported directly from housing assistance program providers) right after the baseline survey to ensure we focus on the sample of respondents who are most likely to have taken up housing assistance because of the study. We discuss our rationale for reporting the first stage estimates from the FOS public-use dataset in greater detail in Section 5.

Table 1: Summary Statistics

	(1) UC	(2) ANY	(3) BH adj. p-value	(4) SUB	(5) BH adj. p-value	(6) CBRR	(7) BH adj. p-value	(8) PBTH	(9) BH adj. p-value
Demographic:									
Female	0.93	0.91	0.69	0.93	0.97	0.91	0.69	0.89	0.12
Age group	3.83	3.72	0.60	3.69	0.60	3.72	0.60	3.76	0.60
White	0.20	0.21	1.00	0.22	1.00	0.19	1.00	0.20	1.00
College	0.27	0.27	1.00	0.26	1.00	0.28	1.00	0.27	1.00
High school	0.33	0.38	0.08	0.39	0.08	0.39	0.08	0.36	0.29
Dropout	0.40	0.35	0.06	0.36	0.20	0.33	0.02	0.36	0.26
Married/Cohabited	0.38	0.38	1.00	0.36	0.99	0.38	1.00	0.42	0.96
No. of Children	1.97	2.01	1.00	1.93	1.00	2.02	1.00	2.13	0.20
Homeless years	1.38	1.29	1.00	1.35	1.00	1.27	1.00	1.26	1.00
Labor Market:									
Working now	0.18	0.17	1.00	0.13	0.12	0.19	1.00	0.19	1.00
Ever worked	0.96	0.96	1.00	0.96	1.00	0.98	0.76	0.95	1.00
Earnings Cat.	1.54	1.50	1.00	1.35	0.02	1.58	1.00	1.60	1.00
Earnings $5000+	0.16	0.15	1.00	0.11	0.02	0.18	1.00	0.18	1.00
Health:									
Poor health	0.09	0.09	1.00	0.10	1.00	0.09	1.00	0.07	1.00
SPD	0.24	0.21	0.18	0.23	0.64	0.18	0.04	0.20	0.32
Substance abuse	0.21	0.21	1.00	0.19	1.00	0.21	1.00	0.24	0.56
Welfare Takeup:									
TANF	0.40	0.42	1.00	0.45	0.28	0.40	1.00	0.39	1.00
SNAP	0.89	0.87	0.30	0.87	0.54	0.88	0.54	0.85	0.16
WIC	0.36	0.36	1.00	0.36	1.00	0.36	1.00	0.38	1.00
Violence:									
IPV	0.50	0.49	1.00	0.50	1.00	0.49	1.00	0.47	1.00
N	746	1536	2282	599	1345	569	1315	368	1114

Notes: This table reports the summary statistics for respondents by assignment types. The age group variable is defined in 5-year bins: 1=under 20, 2=20-24, 3=25-29... and the final bin, 8=above 50. "Earnings Cat." refers to the categories of earnings, using $5000 bins: 1=no earnings, 2=below $5000, 3=$5000-$9999...and the final bin, 8=above $20000. The IPV variable refers to *ever* experiencing intimate partner violence as an adult. SPD is a dummy variable of whether the respondent has "serious psychological distress." IPV and substance abuse are calculated from the restricted-use FOS data and are part of disclosure clearance number 12783. All other estimates are calculated from the public use FOS data. All sample sizes are calculated from the public use FOS data.

Using the first-stage predicted value of HS_{ij}, we estimate the second-stage equation below:

$$Y_{ijt} = \beta_0 + \beta_1 \hat{HS}_{ij} + \mathbf{X}_i\Gamma + \mu_t + \mu_r + \epsilon_{it} \tag{2}$$

In Equation (2), Y_{ijt} represents the outcome of interest for an individual i who is utilizing housing assistance program j or is in the UC group at time t. In our main results, this outcome is a dummy variable indicating whether the respondent reported experiencing IPV during the six months prior to the respective follow-up survey month.

To explore mechanisms, we also run specifications where Y_{ijt} is a variable related to: 1) independent housing status; 2) financial and mental stress, and physical health; 3) family and cohabitation status; and 4) labor market outcomes.

First, to examine housing status, we explore whether respondents have secured independent housing in every month from random assignment to the 37th month after assignment. Independent housing is defined as living in a house or apartment that the respondent owns or rents. This outcome allows us to observe whether the impact of housing access on IPV is associated with a higher likelihood of respondents living in independent, safe, and stable shelter, where they can live apart from abusers.

Second, we examine mental health using multiple measures. One measure is whether respondents report experiencing financial stress. We also generate an indicator for serious psychological distress, which can be, for example, symptoms of post-traumatic stress disorder (PTSD) or emotional and mental health issues. Finally, we use an indicator for whether respondents report abusing substances (alcohol or drugs) over the year prior to the survey date. Regarding physical health, we use an indicator variable that captures whether the respondent reports that they are in poor health.[14] We analyze these outcomes to better understand whether housing access improves psychological and physical well-being, which in turn could reduce victims' reliance on abusive partners for support; or separately, whether such improvements lower tensions between victims and their partners that could inflame physical and verbal assaults.

[14]Our reported results define this health variable in terms of its complement (i.e., whether they report being in good health).

Third, to explore family and cohabitation status, we document whether respondents report being single (not married or cohabiting) in the current survey. We analyze this outcome to explore whether any potential effect of housing assistance on IPV is due to the dissolution of existing relationships or to the avoidance of abusive relationships.

Fourth, we consider the following labor market and economic variables as outcomes. We use a dummy variable denoting respondent employment status, a continuous variable indicating the average weekly hours worked at a respondent's primary job, and a continuous variable representing a respondent's labor market earnings. Our focus on these variables is to discern the influence of housing assistance on respondent labor market and economic autonomy.[15]

The term X_i in both Equations 1 and 2 represents a broad set of individual-level control variables. We consider two sets of control variables. Our first set includes individual-level characteristics such as age, race, gender, education level, total number of children at the time of the baseline survey, and total number of years the respondent had been homeless at the time of the baseline survey. Our second set of control variables includes all of the variables described above and baseline measures of our outcomes of interest. Specifically, we add baseline measures of IPV experience, marital status, employment status, annual earnings, receiving welfare program benefits, social and psychological distress, and alcohol or drug dependence. All of our reported results use the second set of controls.[16]

5 Results

5.1 Effects of assignment on takeup

We report the first-stage estimates of impact of random assignment on program takeup, using publicly available FOS Program Use and Living Situation survey data in Table 2. We use the

[15] In related analyses, we explore potential changes in respondent reliance on social safety net programs. We use a series of dummy variables indicating respondent use of welfare programs, including the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) as outcomes. This analysis aims to understand the connection between housing assistance and participation in welfare initiatives.

[16] Our results are robust to using the more limited set of controls described in the first set. Online Appendix Table A.8 reports "sign and significance" results for our main IPV outcome analysis using the first set of controls.

FOS Program Use and Living Situation survey as it includes information on whether respondents were assigned to ANY of the housing assistance programs and whether they ever participated in their housing program assignment, from the time of random assignment to the 37-month survey date.[17]

We find that approximately 65% of respondents assigned to ANY of the three types of housing use housing support resources. For program-specific takeup rates, we find that SUB has the highest takeup rate at 81%, compared to 59% for CBRR and 51% for PBTH. The differences in coefficients suggest that, relative to short-term assistance, respondents are more likely to take up long-term, housing-focused assistance.

Table 2: First Stage: Impact of Random Assignment on Housing Assistance Program Takeup

	(1) ANY takeup	(2) SUB takeup	(3) CBRR takeup	(4) PBTH takeup
ANY assignment	0.655***			
	(0.0121)			
SUB assignment		0.805***		
		(0.0162)		
CBRR assignment			0.591***	
			(0.0206)	
PBTH assignment				0.511***
				(0.0261)
N	2300	1300	1300	1100
F statistic	2912	2464	819	383

Notes: This table uses the public available FOS program use data to show the first stage results of ANY and different types of housing assistance program assignments on the corresponding program takeups. Robustness standard errors is reported in parentheses. Sample sizes are rounded for consistency with reported results using restricted-use data, according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

5.2 Effects of housing access on IPV

Table 3 reports the 2SLS estimates of the impact of housing assistance on IPV, for ANY housing support program and for each assistance program separately. We find that participation in ANY

[17] We report results using public-use data to reduce the publication of estimates using restricted-use data, particularly estimates that could be produced nearly identically with public information. Our approach is motivated by, and follows, Census recommendations around disclosure avoidance. Our subsequent results use take-up information provided in the restricted-use FOS datasets.

of the housing programs reduces the incidence of IPV experienced in the past six months by 5.6 percentage points, a reduction that is statistically significant at the 1% level. Because 50% of all respondents report experiencing IPV since adulthood at the time of the baseline survey, this reduction is equivalent to a 11.2% reduction relative to the baseline mean. However, it is important to note that the baseline survey asks about ever experiencing IPV in adulthood, while the follow-up survey asks about experiences with IPV in the past six months. As an alternative comparison, we document the reduction in IPV relative to the control group (UC) mean in the 20- and 37-month surveys. On average, 9% of respondents in the control group reported experiencing IPV in the past six months. Thus, our estimate is equivalent to a 62% reduction relative to the control group mean.

Next, we examine the effects of each program type separately. Participation in SUB reduces the incidence of IPV by 6.6 percentage points, a result that is statistically significant at the 1% level. For CBRR and PBTH, we also find negative impacts on IPV, although these estimates are not statistically significant. Comparing across program types, the more precisely estimated effect of SUB suggests that long-term housing assistance that takes the form of a housing choice voucher may be more effective in reducing the incidence of IPV. At the same time, the magnitude of the PBTH effect also suggests that the intensive non-housing resources provided as part of this intervention could also contribute to a reduction in IPV; yet, the imprecision of this estimate and relatively lower takeup rate may point to the challenges of getting families to agree to participate in such services. Unfortunately, our paper cannot disentangle the source of the imprecision for the PBTH estimate.

Because our main results show that participation in ANY housing assistance program has a significant negative impact on IPV, and that these effects appear to be clearest in SUB, our analysis moving forward will focus on the effects of participation in ANY housing assistance program and in SUB in particular.

Table 3: Impact of Housing Assistance on IPV

	(1) ANY vs. UC	(2) SUB vs. UC	(3) CBRR vs. UC	(4) PBTH vs. UC
ANY takeup	-0.056***			
	(0.021)			
SUB takeup		-0.066***		
		(0.018)		
CBRR takeup			-0.031	
			(0.031)	
PBTH takeup				-0.079
				(0.054)
Control group mean	0.09	0.09	0.09	0.09
No. of Obs.	3600	2200	2000	1700

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of different types of housing assistance programs on the incidence of IPV. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. We define individual-level characteristics included as controls in the following way. For race, we use three groups: white, black, or other race; education level is a categorical variable indicating the highest educational attainment of the respondent. We characterize the education level into three categories: high school dropout, high school graduates, and some college or above. Region is a categorical variable indicating the study site of the shelter where the respondent is enrolled in the study. Age is defined in five-year intervals. Total number of children is categorized by respondents having two or more children versus one or none; total number of homeless years is categorized by respondents who have been homeless for less than one year versus one or more years. The baseline measure of IPV included as a control is measured as any past experience with this form of violence. The control variable for earnings is a indicator for respondents with annual earnings greater than $5,000. Standard errors are clustered at the individual level and reported in parentheses. Estimates and sample sizes are part of disclosure clearance number 12603. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

5.3 Robustness: Attrition bias

Respondents may stop responding to follow-up surveys for multiple reasons. If this attrition varies systematically by baseline characteristics between our treatment and control groups, it may bias our main estimates.

To examine the role of attrition bias, we first compare the overall attrition rates in the 20- and 37-month surveys for assignment to ANY treatment, SUB treatment, and our control group. The results are reported in Online Appendix Table A.2. We find that the attrition rate is highest for the control group at 14%, while the attrition rate for those assigned to ANY is 10% and SUB is 6%. It is possible that the differences in attrition across groups is related to the generosity of housing

assistance.

Next, we examine whether the baseline characteristics of the attritors are significantly different than non-attritors, and specifically test whether such characteristics differ within the control and treatment groups. For this exercise, we compare the baseline characteristics for attritors vs. non-attritors within the UC and ANY assignment groups, respectively. The results are reported in Table A.3. We see that within UC, attritors are more likely to be white, have less substance abuse issues, and are less likely to participate in other welfare programs that non-attritors; within ANY treatment assignment, attritors are more likely to be white, have fewer number of homeless years, are less likely to participate in other welfare programs.

For our main analysis, one particular concern is whether the difference in baseline IPV experiences between attritors and non-attritors in the UC is different from the same gap between attritors and non-attritors among the treatment group. What we see is that attritors in the UC group have lower baseline experiences IPV compared to non-attritors, while attritors in ANY treatment have higher baseline IPV than their non-attriting counterparts. If we think baseline measures of IPV (ever experiencing IPV in adulthood) are positively correlated with IPV in the 6 months prior to a follow-up survey, this attrition bias will cause us to overestimate the impact of housing access on IPV.

To address this concern, we construct "Lee bounds," a bounding estimator that brackets the true effect of housing assistance on IPV. Specifically, we follow the methods outlined in Lee (2009) to identify the excess 0.044 (0.085) proportion in the non-attriting ANY (SUB) treatment group with no previous baseline IPV experience relative to UC. We then randomly trim the treatment sample based on extreme values of our outcome; that is, we explicitly select observations in the treatment group who *did not* report experiencing IPV over the six months prior to the follow-up surveys to trim from the sample. We then use the trimmed treatment group sample together with the control sample to re-estimate the effect. We believe this estimate serves as the lower bound (in absolute terms), given that we trim the sample directly on values of the outcome of interest; namely, removing observations with the most extreme values that contribute to the estimated effect. We also follow Duflo *et al.* (2007) and interpret our main estimates in Section 5.2 above as

the upper bound (in absolute terms). To reduce simulation error and produce statistical inference for our lower bound estimator, given that we have "ties" among the observations that could be trimmed, we repeat the random trimming process 100 times to create a distribution for the lower bound estimator, and take the mean as the lower bound estimate and the variation across the 100 draws to estimate the standard error. As reported in Online Appendix Table A.4, we find that the lower bound of the effect of ANY housing treatment on IPV is -0.051 p.p.; the lower bound of the effect of SUB treatment on IPV is -0.059 p.p. This suggests that our main results are robust to attrition bias.

5.4 Addressing Concerns with Self-Reported Outcomes

One alternative interpretation of our results is that housing assistance may not affect the incidence of IPV but instead makes respondents less likely to report IPV in surveys. We believe this interpretation is unlikely in our context due to two main reasons. First, the FOS surveys not only preserves respondent confidentiality, but are collected by parties that are not subject to further legal reporting and procedures, in contrast to legal requirements that may exist for police or hospital administrators in certain countries. While victims' identities are intended to be protected in these administrative records, these latter legal processes may ultimately reveal victims' identities to certain individuals, and subject victims to further scrutiny while forcing them to revisit their trauma. Therefore, we believe these privacy features of the FOS data protect the respondents to a greater extent than datasets that might rely on police or hospital administrative records, and encourage respondents to report their IPV experience more credibly in both the treatment and control groups.

Second, we do not believe access to housing assistance itself will distort respondent incentives away from truthfully reporting their IPV experiences. If anything, outside of the context of the Family Options Study, receiving housing assistance may be positively correlated with reporting experiences with IPV. This may be due to factors such as the Violence Against Women Act (VAWA), which provides housing protections for individuals who have experienced domestic violence, dating violence, sexual assault, or stalking. In particular, VAWA is intended to keep victims

safe and reduce the risk of homelessness by 1) protecting them from eviction or denial of housing based on violence experienced; 2) allowing victims to request an emergency transfer to another housing unit for safety; and 3) ensuring violent situations remain confidential.[18]. Through these protections, domestic violence victims may have greater chances of securing housing assistance. Altogether, this pattern of reporting would operate in a direction against our main finding.

6 Potential Mechanisms

Next, we study the underlying mechanisms through which housing subsidies may influence IPV. We differentiate between direct and indirect channels. We first focus on the direct channel of increasing rates of independent, stable, and safe housing for respondents. Next, we examine the indirect effects of housing assistance on respondents' (1) financial and mental stress, as well as physical health; (2) partnership status; and (3) labor market outcomes. For our analysis of indirect mechanisms, we further leverage a causal mediation analysis framework to quantify their roles.

Independent housing represents a direct channel because it is the core mechanism explicitly targeted by housing programs. When victims gain independent housing, they obtain a concrete alternative to living with or depending on abusers for shelter, directly removing a key barrier to leaving abusive relationships. Economic stress, mental health, partnership status, and labor market outcomes represent indirect channels because they are complementary consequences that may arise from housing access. These outcomes are not the primary goals of housing programs, but may still be mediating factors through which housing assistance affects IPV.

6.1 Independent Housing

To examine whether housing assistance programs provide independent housing that makes respondents less likely to rely on abusers for shelter, we use FOS Public Program Use and Living Situation survey data to estimate Equation 2, replacing the main IPV outcome with whether the respondent has independent housing. Independent housing is defined in the survey as living in a house or apartment that the respondent owns or rents. The Program Use and Living Situation

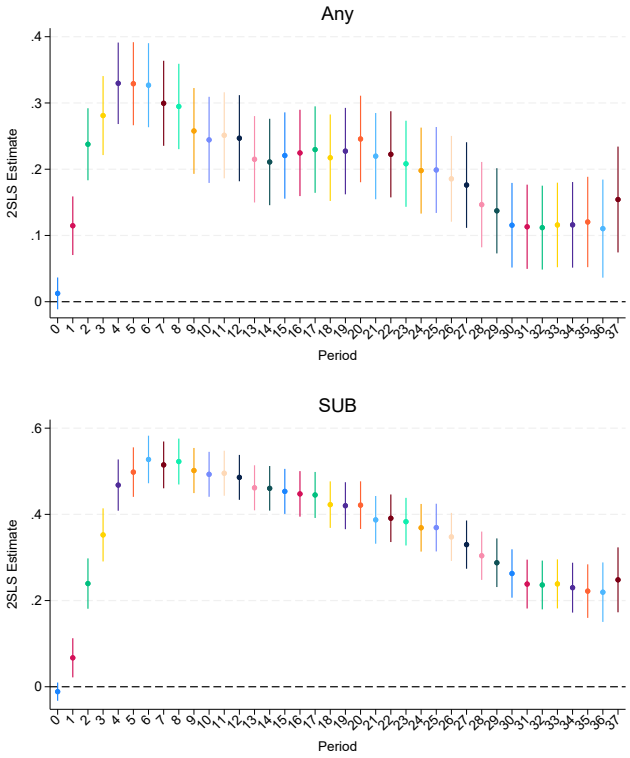
[18]See https://www.hud.gov/helpingamericans/housingchoicevoucherstenants for more details

survey is a retrospective survey that asks respondents about their living situation every month from the month of random assignment until 37 months after. Figure 2 reports the monthly 2SLS estimates from this exercise.

We find that participation in ANY housing assistance program and SUB has an immediate positive effect on respondent independent housing. Participation in ANY housing assistance program increases independent housing by over 30 p.p. within the first 6 months after random assignment, then decreases to approximately 20 p.p. in the middle periods (months 15-25), and subsequently decreases and stabilizes above 10 p.p through month 37. Given that the mean independent housing rate for the UC group is 32% at 6 months, 51% at 20 months, and 57% at 37 months after random assignment, these effects represent increases of 94%, 39%, and 25% relative to the control group mean at the corresponding time periods.[19] The effect size is larger if respondents participate in the SUB program: a 50 p.p. increase in independent housing 4-10 months after assignment, declining to roughly 40 p.p. 20-28 months after random assignment, and then stabilizing above 20 p.p. through month 37. This larger effect is consistent with the longer-term benefits of housing vouchers for recipients.

[19]We do not interpret these effects relative to independent housing rates in the baseline survey, as almost all respondents (regardless of treatment or control) have no independent housing and live in a homeless shelter at that time.

Figure 2: Impact of Housing Assistance on Independent Housing



Notes: This figure uses public-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of housing assistance programs on an indicator variable of whether the respondent is living in a house or apartment that they own or rent. The top panel estimates the effect of assignment to ANY housing assistance program, while the bottom panel explores the effect of SUB. Estimates are plotted monthly from the month of random assignment until 37 months after. All estimates are from 2SLS specifications. Standard errors are clustered at the individual level and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

These findings suggest that housing assistance substantially increases respondent rates of living in independent housing, with longer-term effects particularly evident for participants in the SUB program. Independent housing provides a stable and safe living environment that enables respondents to live separately from and avoid abusers, directly contributing to reductions in IPV incidence.

6.2 Financial and Mental Stress, and Physical Health

To examine the effects of housing access on individual economic and mental stress, as well as physical health, we use Equation 2 but replace the main IPV outcome variable with measures of

economic stress, serious psychological distress (SPD), substance abuse (including drug and alcohol use), and self-rated overall health (covering both physical and mental health). Prior research has shown that financial stress, mental health and substance abuse could contribute to the incidence of IPV (Bhalotra *et al.*, 2025; Yu *et al.*, 2019; Cafferky *et al.*, 2018). Table 4 reports the 2SLS estimates from this exercise.

We find that participation in ANY housing assistance program and SUB in particular reduces economic stress. The baseline survey does not include a measure of economic stress, and because this variable is normalized to a range between -1 and 1, we interpret the effects in terms of standard deviations relative to the control group across follow-up surveys. Specifically, in the 20- and 37-month follow-surveys, the mean of economic stress in the control group is -0.14 and the standard deviation is 0.49. Thus, the estimated reduction in economic stress for ANY (SUB) participants corresponds to approximately 0.22 (0.32) standard deviations.

We also find that both ANY housing treatment and SUB participation reduce social and psychological distress and substance abuse. Only the effect of SUB on SPD is statistically significant, equivalent to a 13% reduction in psychological distress relative to the control group mean.[20] In addition, both treatments have a positive but statistically insignificant impact on self-rated overall health.

Taken together, the evidence suggests that housing access substantially reduces economic stress and may improve general mental health. These improvements could reduce reliance on abusive partners for support, or separately, decrease tensions between partners and thus reduce the incidence of IPV. It is worth noting, however, that there is likely a bidirectional relationship between general mental health issues and IPV: mental health challenges affect IPV, and IPV incidents could simultaneously worsen mental health. However, we note that the most robust effect that we find regarding respondent mental health—when economic stress is our outcome—is likely less subject to reverse causality concerns. Therefore, later in the paper we will focus on economic stress as an underlying mechanism when performing a causal mediation analysis that uses the sequential timing of when economic stress and IPV incidence variables are measured to quantify

[20]SPD is measured using a Likert-scale, with a mean value of 7 in follow-up surveys.

the effect of housing assistance on IPV through this indirect channel.

Table 4: Impact of Housing Assistance on Health

	(1) Economic stress	(2) SPD	(3) Substance abuse	(4) Self-rated health
Panel A: ANY vs. UC				
ANY takeup	-0.108***	-0.508	-0.0242	0.0257
	(0.0330)	(0.375)	(0.0219)	(0.0319)
N	3600	3600	3600	3600
Panel B: SUB vs. UC				
SUB takeup	-0.156***	-0.907***	-0.0287	0.0144
	(0.0291)	(0.342)	(0.0203)	(0.0288)
N	2200	2200	2200	2200

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of housing assistance programs on mental health and self-rated health. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. Standard errors are clustered at the individual level and reported in parentheses. Serious psychological distress (SPD) in the follow-up survey is measured in scale with a mean of 7. Estimates are part of disclosure clearance number 12603. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

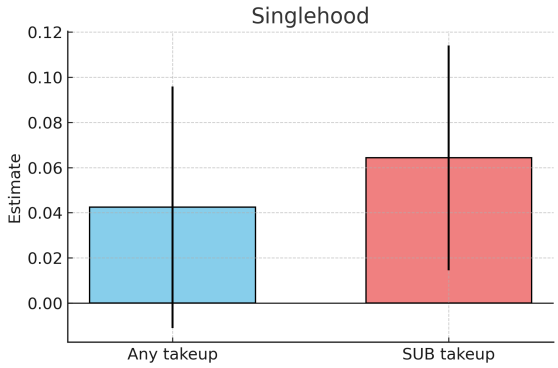
6.3 Partnership

In the 20- and 37-month surveys, respondents were asked about their marital and partnership status. Specifically, they reported whether they were currently single (i.e., not married or living in a marriage-like situation). This measures whether the respondent has a long-term, stable partner. The effects of participation in ANY program and in SUB on being single are reported in Figure 3. We find that housing assistance increases the likelihood of respondents being single. For example, SUB participation increases the likelihood of being single by 6.4 p.p ($p < 0.05$).[21] One interpretation of this result is that housing assistance, particularly long-term assistance, could reduce IPV by dissolving abusive relationships. Alternatively, the increase in singlehood and its benefits for reducing IPV may operate outside of ending abusive relationship among married or

[21]The estimated increase in singlehood resulting from participation in ANY housing assistance program is 4.2 p.p but not statistically significant.

cohabiting couples; that is, housing access could allow victims of IPV to avoid entering short-term housing arrangements with unstable or dangerous individuals. We examine these different interpretations in Section 6.5.

Figure 3: Impact of Housing Assistance on Partnership



Notes: This Figure uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of housing assistance programs on the a indicator variable of being currently being single. All regressions control for the full set of variables discussed in Equations 1 and 2. Standard errors are clustered at the individual level and reported in parentheses. Estimates are part of disclosure clearance number 12603. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

6.4 Labor market outcomes

Another potential channel through which housing subsidies may affect IPV is the labor market. If housing assistance improves respondents' labor market outcomes, it could increase their bargaining power within relationships, thus reducing IPV (Aizer, 2010; Anderberg *et al.*, 2016). The effect of labor market outside options on IPV is the focus of much of the prior economic literature (Aizer, 2010; Anderberg *et al.*, 2016; Bhalotra *et al.*, 2025; Erten and Keskin, 2021; Sanin, 2021a).

We estimate the impact of ANY housing assistance and SUB on employment status, working hours, and annual earnings, with results reported in Table 5. We find that receiving long-term housing assistance, such as a housing voucher (SUB), reduces employment by 5.6 percentage points. The point estimate is marginally significant, but is directionally consistent with findings from other studies that document a negative impact of housing vouchers on women's employment (Jacob and Ludwig, 2012; Zhang, 2025). We also find a negative, though imprecisely

estimated, effect of housing assistance on annual earnings.

Our results do not support the hypothesis that housing assistance reduces IPV by strengthening women's labor market position. If anything, housing assistance lowers women's employment and earnings, suggesting that housing access operates through independent channels in reducing IPV.[22]

One interpretation of these results is that housing access negatively impact victims' abilities to achieve economic self-sufficiency, particularly relative to improving labor market outside options. However, for a policy aiming to improve women's labor market outside options to be successful, we argue that it requires time, and multiple, often uncontrollable, dynamic factors to align at the individual and market level. At the individual level, victims may need to undertake meaningful human capital investment; at the market level, they must be entering a sufficiently robust labor market. In contrast, a well-targeted housing policy can be more direct. Not only does it immediately remove women from living arrangements associated with greater IPV risk, but it can also preserve their human capital and labor market outside options. That is, without housing access, women may remain exposed to IPV, which can substantially erode their labor market outside options, as documented by a growing body of evidence on the lasting repercussions of domestic violence (e.g., Adams *et al.*, 2024; Bindler and Ketel, 2022). Altogether, from the perspective of women's long-run self-sufficiency and welfare, housing access can serve as an effective and immediate intervention.

[22]In a related but separate channel, we also explore whether housing assistance affects the use of other welfare programs. The effect of housing assistance on other welfare program participation is ex-ante ambiguous. Housing assistance and other welfare benefits may act as substitutes or complements. Second, housing assistance may serve as an information channel, encouraging respondents to learn about and apply for other social safety net programs. For example, the PBTH intervention explicitly incorporates support services related to the broader safety net, making it likely to be combined with other welfare programs as a bundled treatment. Because of these ambiguous effects, we report these results in Online Appendix Table A.6. We find evidence that participation in ANY housing assistance program and in SUB increases takeup of TANF and SNAP. Such increases in access to complementary benefits may, in turn, contribute to reductions in IPV by improving respondents' economic resources and feelings of financial stress.

Table 5: Impact of Housing Assistance on Labor Market Outcomes

	(1) Working	(2) Usual hours worked per week	(3) Annual earnings
Panel A: ANY vs. UC			
ANY takeup	-0.0159	-0.141	-103.2
	(0.0318)	(0.979)	(474.4)
N	3600	3600	3600
Panel B: SUB vs. UC			
SUB takeup	-0.0558*	-2.099**	-534
	(0.0285)	(0.961)	(424.4)
N	2200	2200	2100

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of housing assistance programs on individual labor market outcomes. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. Standard errors are clustered at the individual level and reported in parentheses. Estimates are part of disclosure clearance number 12603. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

6.5 Mediation analysis: Effects through economic stress and partnership

In this section, we explore the mediating effect of indirect channels of economic stress and partnership status on the impact of housing access in reducing in IPV. Instead of simply using the mediating variables as outcomes for our main empirical strategy as is done above, we take advantage of the panel data structure of the FOS and apply the mediation analysis framework proposed by Bhalotra *et al.* (2022). This framework leverages the sequential timing of treatment, the potential mediators of interest, and the incidence of IPV, to quantify the effect of the underlying mechanisms. We explain the framework using economic stress as an example mediator below; we will also report results with partnership status (i.e., being single) as the mediator.

Gelbach Mediation Analysis: Econometric framework. To conduct the Gelbach Mediation Analysis used in Bhalotra *et al.* (2022), we exploit the sequential timing of the treatment assignment (ANY or SUB) during the baseline survey, the mediator (e.g., economic stress) measured in

the 20-month survey, and the outcome (IPV) measured in the 37-month survey. We believe the causal mediation analysis framework provides useful insights into the economic significance of the indirect channels. However, a key limitation is that the mediator variables are measured only in the 20-month survey, whereas in practice these mediators evolve continuously from the point of random assignment through the end of the study. For example, using mediators measured at month 20 could miss both earlier dynamics (e.g., the immediate stress relief upon receiving housing) and subsequent changes (e.g., partnership transitions between months 20 and 37) that could affect IPV. The mediation estimates therefore capture only a snapshot of each channel's influence at a single intermediate point, rather than the cumulative exposure to each mediator throughout the study period. As a result, we believe the magnitude of these estimates should be interpreted as suggestive of a specific trajectory for indirect mechanism effects, and likely serve as a lower bound for the role of an indirect channel.

Formally, we estimate the following equations:

$$Y = T\tau + Z\beta + X\lambda + \nu, \tag{3}$$

$$Z = T\gamma + X\lambda + \epsilon, \tag{4}$$

where Y is the incidence of IPV measured in the 37-month survey, T is a treatment indicator (ANY or SUB), Z is a scale measure of economic stress recorded in the 20-month survey, and X is a vector of baseline covariates. The contribution of mediator Z to the treatment effect is quantified as $\hat{\delta} = \hat{\gamma}_z \hat{\beta}_z$, where $\hat{\gamma}_z$ is the estimated effect of the intervention on the mediator from Equation (4), and $\hat{\beta}_z$ is the estimated association between the mediator and the outcome from Equation (3). We compare this product to the total treatment effect of T on Y (from our main results) to estimate the share of the effect of housing on IPV transmitted through economic stress.[23]

Gelbach Mediation Analysis: Economic stress. We report estimates from Equations (3) and (4) using economic stress as the mediator in Table 6. Column (1) shows that receiving any housing assistance reduces economic stress in the 20-month survey. Column (2) shows that economic

[23]The vector of baseline covariates included in X is identical to the set of controls described in Section 4.

stress measured in the 20-month survey is positively correlated with IPV in the 37 month-survey. The implied contribution of the economic-stress channel is $\hat{\delta} = \hat{\gamma}_z \hat{\beta}_z = -0.006$, (SE= 0.0031) which accounts for approximately 10% of the overall effect of ANY housing assistance on IPV.

Columns (3) and (4) report the corresponding estimates when exploring the mediating effect of SUB housing assistance on IPV. The implied contribution of the economic stress channel in this specification is $\hat{\delta} = \hat{\gamma}_z \hat{\beta}_z = -0.005$ (SE = 0.0112), representing about 8% of the total effect of SUB on IPV, though not statistically significant.

Table 6: Gelbach Mediation Analysis on Economic Stress

	(1) Econ stress 20-mo	(2) IPV 37-mo	(3) Econ stress 20-mo	(4) IPV 37-mo
ANY takeup	-0.153***	-0.0487*		
	(0.0422)	(0.0288)		
Econ stress 20-mo		0.0362**		0.0273
		(0.0176)		(0.0240)
SUB takeup			-0.189***	-0.0424
			(0.0374)	(0.0263)
N	1600	1600	950	950

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to equations (3) and (4) for being single as mediators. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. Columns (1) and (2) shows the effect of any treatment, and columns (3) and (4) shows the effect of SUB treatment. Robust standard errors clustered at the household level are reported in parentheses. Estimates are part of disclosure clearance number 12783. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Gelbach Mediation Analysis: Partnership. Table 7 reports results exploring the mediating effect of partnership status, specifically if respondents report being single, on the impact of housing assistance in reducing IPV.

First, unlike the results in Section 6.3, we do not detect a significant effect of treatment on being single in the 20-month survey. This difference may be driven by differences in sample construction: the earlier result reflects an average effect of housing assistance on partnership status across both follow-up surveys, as we pool data from the 20- and 37-month surveys. In contrast, the mediation analysis isolates the 20-month survey measure of partnership status. In

other words, the effects of housing assistance on partnership status may be more gradual.

Second, Columns (2) and (4) show a positive association between being single during the 20-month survey and the incidence of IPV reported in the 37-month survey, conditional on treatment status. This result appears inconsistent with a story that housing assistance contributes to the dissolution of established abusive relationships. In short, the positive correlation between single status and IPV would suggest that the role of housing assistance in increasing singlehood would result in more people in a partnership status that is at greater risk of domestic violence.

Table 7: Gelbach Mediation Analysis on Partnership

	(1) Single 20-mo	(2) IPV 37-mo	(3) Single 20-mo	(4) IPV 37-mo
ANY takeup	-0.00902	-0.0492*		
	(0.0308)	(0.0269)		
Single 20-mo		0.0428**		0.0520**
		(0.0194)		(0.0249)
SUB takeup			0.00984	-0.0469**
			(0.0287)	(0.0238)
N	1800	1800	1100	1100

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to equations (3) and (4) for being single as mediators. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. Columns (1) and (2) shows the effect of any treatment, and columns (3) and (4) shows the effect of SUB treatment. Robust standard errors reported in parentheses. Estimates are part of disclosure clearance number 12783. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

To reconcile the results that housing assistance reduces IPV and increases singlehood, but singlehood is, on average, a positive predictor of experiences with IPV, one possible interpretation is that housing assistance allows those who are single to avoid shorter-term and transitional housing arrangements where they may come across abusive and dangerous individuals. These shorter-term and transitional relationships are unlikely to be captured by our measure of partnership status at a single point in time (the 20-month survey), nor are they necessarily reflected in the IPV outcome measured at 37 months, which records whether the respondent experienced IPV

in the prior six months (i.e., months 31–37).[24] Partnership dynamics are also fluid, as respondents may enter and exit relationships multiple times between surveys. These relationship transitions may themselves be important mediating events. The causal mediation analysis, which relies on a single snapshot of partnership status, is therefore limited in its ability to capture the full dynamic interaction between partnership changes and IPV risk over the study period.

In support of our explanation above, Online Appendix Table A.7 reports the results from heterogeneity analyses where we estimate our main specification with IPV as an outcome but do so separately for three different subsamples: those who were single at baseline, those who were married/cohabited at baseline but who are no longer married/cohabited at the time of the follow-up survey, and those who were married/cohabited at baseline and remained married/cohabited. The results are reported in terms of coefficient "sign and significance." We find reductions in IPV as a result of housing access are concentrated among respondents who were single at the time of the baseline survey. In other words, while singlehood is positively correlated with IPV overall, among single individuals, housing assistance allows respondents to establish more secure independent living and avoid transitional sheltering arrangements where they may be exposed to greater risk of IPV.[25]

7 Effects on Children

Housing assistance may affect children's outcomes, either directly or indirectly through its impact on IPV. A large literature has shown that IPV imposes substantial harm on children and their peers (Bhuller *et al.*, 2024; Carrell *et al.*, 2018; Moss *et al.*, 2023). In our analysis, we focus specifically on the indirect effect of housing assistance on children, operating through its impact on IPV.

For this analysis, we link respondents to their children's baseline, 20-, and 37-month follow-up survey responses. Summary statistics for children at the time of the baseline and follow-up

[24]The measure of singlehood in the FOS captures the absence of a married or marriage-like partner, and likely reflects long-term partnership status rather than shorter-term or transient relationships that may also expose respondents to violence.

[25]We do find evidence that is directionally consistent with housing assistance reducing IPV incidence by allowing respondents to dissolve formal relationships (Panel B), particularly for the two treatment effects with the most robust effect on IPV overall, ANY and SUB. However, these effects are not statistically significant.

surveys are presented in Online Appendix Table A.5. We begin by following the same approach described above for adults, starting with the 2SLS specification noted in Section 4 but where we use the children's sample and replace the main IPV outcome variable with children's outcomes.[26] The outcomes we examine include whether the child is currently enrolled in school/childcare, has repeated a grade since the the study began, has a disability (either physical, emotional, or mental health-related), or currently lives with a parent. We also examine the child's self-reported health and their score on the Strengths and Difficulties Questionnaire (SDQ), which is a measure of overall social and behavioral development.[27] Table 8 reports the results of this analysis and shows that the only statistically significant effect of housing assistance on children across both ANY and SUB is through improvements in their behavioral development.[28] The negative coefficients for ANY and SUB suggest strong reductions in overall behavioral developmental issues.

Next, we apply the same mediation framework used for adults but now treat household IPV measured at 20 months as the mediator and children's SDQ score at 37 months as the outcome. We focus on this measure because it is the only children's outcome where we find statistically and economically meaningful effects of both ANY and SUB housing assistance. The results are reported in Table 9. Consistent with our main results, we find that both ANY and SUB housing assistance significantly reduce household IPV at the time of the 20-month survey. We also find that both ANY and SUB housing assistance treatments benefit children's SDQ scores, lowering children's SDQ scores by nearly 0.20 standard deviations relative to the control group. We also find that household IPV at 20 months is positively associated with children's SDQ score at 37 months when looking at the sample of households in SUB or UC, though the association is not statistically significant. These results are directionally consistent with prior literature that shows the negative impact of IPV on children's outcomes (Bhuller *et al.*, 2024; Carrell *et al.*, 2018).

[26] With this sample of children, we add child demographic controls to the existing set of control variables described for the adult sample in Section 4. These additional control variables include baseline survey measures of children's age, gender, whether they were currently living with a parent, currently in school, and had a disability (physical, emotional, or mental health-related).

[27] The SDQ total score (in z-score units) aggregates children's responses to a range of social and behavioral development categories, including conduct, emotion, hyperactivity, pro-sociality, and peer-relations problems.

[28] Table 8 also shows improvements in self-reported health among children for both ANY and SUB, although only the latter effect is statistically significant ($p<0.05$).

Table 8: Impact of Housing Assistance on Children's Outcomes

	(1) In School	(2) Repeat Grade	(3) Disability Diagnosis	(4) Health Scale	(5) SDQ Score	(6) Lives with Parent
Panel A: ANY vs. UC						
ANY takeup	-0.0155	-0.0011	-0.0007	0.0637	-0.185**	0.0010
	(0.0225)	(0.0252)	(0.00465)	(0.0555)	(0.0762)	(0.0119)
N	4200	3500	200	5000	4500	5100
Panel B: SUB vs. UC						
SUB takeup	-0.0168	0.0061	0.0011	0.105**	-0.199***	0.0048
	(0.0215)	(0.0232)	(0.00443)	(0.0520)	(0.0703)	(0.0106)
N	2400	2000	100	3000	2600	3000

Notes: This table uses the restricted-use FOS data from the 20- and 37-month follow-up surveys to estimate the impact of housing assistance programs on children's outcomes. Outcomes include, whether the child is currently enrolled in school/childcare, has repeated a grade since the the study began, has a disability (either physical, emotional, or mental health-related), the child's total SDQ score, and whether the child currently lives with a parent. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. In addition, all regressions include additional controls for children's characteristics, including their age, gender, whether they were currently living with a parent, currently in school, and had a disability (physical, emotional, or mental health-related) at the time of the baseline survey. Standard errors are clustered at the household level and reported in parentheses. Coefficient estimates are part of disclosure clearance number 12900. Sample sizes are estimated from public use FOS data, and rounded for consistency with results from restricted-use data, according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Altogether, our results suggest that the reduction of IPV is a channel through which housing access, particularly a housing choice voucher provided in SUB, could affect children's behavioral development. If we follow the mediation analysis framework to quantify its contribution, the SUB effect is $\hat{\delta} = \hat{\gamma}_z \hat{\beta}_z = -0.011$ (s.e.=0.010), equivalent to 6% of the overall impact of housing assistance on children's behavioral outcomes, though not statistically significant.

Table 9: Gelbach Mediation Analysis on Children's Outcomes

	(1) IPV 20-mo	(2) Child SDQ score	(3) IPV 20-mo	(4) Child SDQ score
ANY takeup	-0.0405*	-0.197**		
	(0.0235)	(0.0962)		
IPV 20-mo		-0.00464		0.173
		(0.104)		(0.146)
SUB takeup			-0.0660***	-0.196**
			(0.0204)	(0.0890)
Observations	2000	2000	1200	1200

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to equations (3) and (4) for being single as mediators. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. In addition, all regressions include additional controls for children's characteristics, including their age, gender, whether they were currently living with a parent, currently in school, and had a disability (physical, emotional, or mental health-related) at the time of the baseline survey. Columns (1) and (2) shows the effect of ANY treatment, and columns (3) and (4) shows the effect of SUB treatment. Robust standard errors reported in parentheses. Estimates are part of disclosure clearance number 12783. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

8 Policy Discussion and Conclusion

There are severe consequences for victims of intimate partner violence. The physical, psychological, and economic costs can be large in magnitude and persist over the long term (Leemis *et al.*, 2022). These costs have prompted important existing economic and policy-relevant research on the value of improved outside options in the labor market to reduce the incidence of IPV. Our paper contributes a complementary perspective by demonstrating that outside options beyond the labor market, specifically access to housing, can significantly reduce IPV.

We document that housing access reduces IPV through both direct and indirect channels. Housing assistance directly provides victims with independent and stable shelter to live apart from abusive situations. Housing access indirectly lowers economic stress and reduces exposure to abusive or unstable relationships. Notably, housing access does not appear to improve labor market outcomes in our context, underscoring that housing operates as a distinct and independent channel for addressing IPV. Understanding the relative importance of these indirect chan-

nels is policy-relevant. It suggests that resources should not only be directed toward expanding housing capacity but also toward programs that address significant mediating pathways, such as financial stress relief and the promotion of stable, healthy long-term relationships.

Our results introduce a solution for addressing costly IPV experiences for individuals and the broader public. In doing so, our paper also highlights the multidimensional value of housing policy. Not only can housing solutions address larger societal goals of advancing economic opportunity for families and in particular children (e.g. Chetty *et al.*, 2016; Chetty and Hendren, 2018; Chyn, 2018), but can also directly benefit women who face disproportionally higher risk of experiencing IPV. In conducting a benefit-cost analysis to determine the value of similar forms of housing programs and policies, we believe that our findings emphasize a critical benefit that should be considered.

By quantifying the impact of an additional approach to reducing experiences with IPV, our paper also invites a broader discussion about the different tools available to policymakers to reduce IPV and improve gender inequality. While improving women's labor market outside options can be effective in preventing IPV, past work has suggested there are some contexts where it has the potential to increase IPV (Erten and Keskin, 2021; Bhalotra *et al.*, 2021b). This strategy is also both time- and resource-intensive. For example, it may require long-term investments in women's human capital to fundamentally enhance their labor market potential. Even with investment, the availability of labor market outside options is contingent on broader labor market conditions. Further, abusers may use violence or other coercive tactics to actively sabotage women's labor market prospects. In contrast, housing access offers an immediate and effective means of addressing this urgent social issue, while also advancing long-term reductions in gender inequality.

References

Adams, A. (2025). Recent developments: The economics of gender-based violence. *The 2025 ASSA Annual Meeting*.

—, Huttunen, K., Nix, E. and Zhang, N. (2024). The dynamics of abusive relationships. *The Quarterly Journal of Economics*, **139** (4), 2135–2180.

Adams, A. E., Sullivan, C. M., Bybee, D. and Greeson, M. R. (2008). Development of the scale of economic abuse. *Violence against women*, **14** (5), 563–588.

Aizer, A. (2010). The Gender Wage Gap and Domestic Violence. *American Economic Review*, **100** (4), 1847–59.

Alesina, A., Brioschi, B. and La Ferrara, E. (2021). Violence against women: a cross-cultural analysis for africa. *Economica*, **88** (349), 70–104.

Amaral, S. and Bhalotra, S. R. (2017). *Population sex ratios and violence against women: The long-run effects of sex selection in India*. Tech. rep., ISER Working Paper Series.

Anderberg, D., Mantovan, N. and Sauer, R. M. (2023). The Dynamics of Domestic Violence: Learning About the Match. *The Economic Journal*, **133**, 2863–98.

—, Rainer, H., Wadsworth, J. and Wilson, T. (2016). Unemployment and Domestic Violence: Theory and Evidence. *The Economic Journal*, **126** (597), 1947–1979.

Arenas-Arroyo, E., Fernandez-Kranz, D. and Nollenberger, N. (2021). Intimate Partner Violence Under Forced Cohabitation and Economic Stress: Evidence From the COVID-19 Pandemic. *Journal of Public Economics*, **194**, 104350.

Baker, C. K., Billhardt, K. A., Warren, J., Rollins, C. and Glass, N. E. (2010). Domestic violence, housing instability, and homelessness: A review of housing policies and program practices for meeting the needs of survivors. *Aggression and violent behavior*, **15** (6), 430–439.

—, Cook, S. L. and Norris, F. H. (2003). Domestic violence and housing problems: A contextual analysis of women's help-seeking, received informal support, and formal system response. *Violence against women*, **9** (7), 754–783.

Bhalotra, S., Brito, E., Clarke, D., Larroulet, P., Pino, F. *et al.* (2021a). *Dynamic Impacts of Lockdown on Domestic Violence: Evidence From Multiple Policy Shifts in Chile*. Tech. rep., World Institute for Development Economic Research (UNU-WIDER).

—, Britto, D. G., Pinotti, P. and Sampaio, B. (2025). Job displacement, unemployment benefits and domestic violence. *Review of Economic Studies*, **92** (6), 3649–3681.

—, Kambhampati, U., Rawlings, S. and Siddique, Z. (2021b). Intimate partner violence: The influence of job opportunities for men and women. *The World Bank Economic Review*, **35** (2), 461–479.

—, Karlsson, M., Nilsson, T. and Schwarz, N. (2022). Infant health, cognitive performance, and earnings: Evidence from inception of the welfare state in sweden. *Review of Economics and Statistics*, **104** (6), 1138–1156.

Bhuller, M., Dahl, G. B., Løken, K. V. and Mogstad, M. (2024). Domestic violence reports and the mental health and well-being of victims and their children. *Journal of human resources*, **59** (S), S152–S186.

Bindler, A. and Ketel, N. (2022). Scaring or scarring? labor market effects of criminal victimization. *Journal of Labor Economics*, **40** (4), 939–970.

Bobonis, G. J., González-Brenes, M. and Castro, R. (2013). Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control. *American Economic Journal: Economic Policy*, **5** (1), 179–205.

Bollman, K., Chakraborty, J., Lakdawala, L. K. and Nakasone, E. (2026). Frosty climate, icy relationships: Cold and intimate partner violence in peru. *Journal of Development Economics*, **180**, 103710.

Brassiolo, P. (2016). Domestic Violence and Divorce Law: When Divorce Threats Become Credible. *Journal of Labor Economics*, **34** (2), 443–477.

Bullinger, L. R., Carr, J. B. and Packham, A. (2021). COVID-19 and Crime: Effects of Stay-At-Home Orders on Domestic Violence. *American Journal of Health Economics*, **7** (3), 249–280.

Cafferky, B. M., Mendez, M., Anderson, J. R. and Stith, S. M. (2018). Substance use and intimate partner violence: a meta-analytic review. *Psychology of Violence*, **8** (1), 110.

Campbell, J. C., Glass, N., Sharps, P. W., Laughon, K. and Bloom, T. (2007). Intimate partner homicide: Review and implications of research and policy. *Trauma, Violence, & Abuse*, **8** (3), 246–269.

Carrell, S. E., Hoekstra, M. and Kuka, E. (2018). The long-run effects of disruptive peers. *American Economic Review*, **108** (11), 3377–3415.

Chang, H., Chao, S.-F., Chen, K.-M. and Lin, M.-J. (2023). When Love Hurts: The Impact of Intimate Partner Violence on Later Life Outcomes. *Available at SSRN 4475647*.

Chetty, R. and Hendren, N. (2018). The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects. *Quarterly Journal of Economics*, **133** (3), 11071162.

—, — and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, **106** (4), 855–902.

Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review*, **108** (10), 3028–56.

Corradini, V. and Buccione, G. (2023). Unilateral divorce rights, domestic violence and women's agency: Evidence from the egyptian khul reform. *Journal of Development Economics*, **160**, 102947.

Duflo, E., Glennerster, R. and Kremer, M. (2007). Using randomization in development economics research: A toolkit. *Handbook of development economics*, **4**, 3895–3962.

Erten, B. and Keskin, P. (2018). For Better or for Worse?: Education and the Prevalence of Domestic Violence in Turkey. *American Economic Journal: Applied Economics*, **10** (1), 64–105.

— and — (2021). Female employment and intimate partner violence: Evidence from syrian refugee inflows to turkey. *Journal of Development Economics*, **150**, 102607.

European Union Agency for Fundamental Rights (2015). Violence against women: an eu-wide survey. *Luxembourg: Publications Office of the European Union*, **358**.

García-Ramos, A. (2021). Divorce laws and intimate partner violence: Evidence from mexico. *Journal of Development Economics*, **150**, 102623.

HUD (2025). The family options study: Project overview. `https://www.huduser.gov/portal/hmlessrch_conv/Homeless_Families_Study.pdf`, accessed: 2010-12-10.

Jacob, B. A., Kapustin, M. and Ludwig, J. (2015). The impact of housing assistance on child outcomes: Evidence from a randomized housing lottery. *The Quarterly Journal of Economics*, **130** (1), 465–506.

— and Ludwig, J. (2012). The effects of housing assistance on labor supply: Evidence from a voucher lottery. *American Economic Review*, **102** (1), 272–304.

Jaffe, P., Zerwer, M. and Poisson, S. (2002). Access denied: The barriers of violence and poverty for abused women and their children's search for justice and community services after separation. *The Center for Children & Families in the Justice System of the London Family Court Clinic*, **18**.

Lagomarsino, B. C. and Rossi, M. A. (2024). Jue insight: The unintended effect of argentina's subsidized homeownership lottery program on intimate partner violence. *Journal of Urban Economics*, **142**, 103612.

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, **76** (3), 1071–1102.

Leemis, R. W., Friar, N., Khatiwada, S., Chen, M. S., Kresnow, M.-j., Smith, S. G., Caslin, S. and Basile, K. C. (2022). *The National Intimate Partner and Sexual Violence Survey: 2016/2017 Report on Intimate Partner Violence*. Tech. rep., CDC.

Leslie, E. and Wilson, R. (2020). Sheltering in place and domestic violence: Evidence from calls for service during covid-19. *Journal of public economics*, **189**, 104241.

Ludwig, J., Duncan, G. J., Gennetian, L. A., Katz, L. F., Kessler, R. C., Kling, J. R. and Sanbonmatsu, L. (2013). Long-term neighborhood effects on low-income families: Evidence from moving to opportunity. *American economic review*, **103** (3), 226–231.

Lyon, E., Bradshaw, J. and Menard, A. (2011). *Meeting Survivors' Needs Through Non-Residental Domestic Violence Services & Supports: Results of a Multi-State Study*. Tech. rep., Natioanl Institute of Justice.

Meyer, B. D., Wyse, A., Grunwaldt, A., Medalia, C. and Wu, D. (2021). *Learning about homelessness using linked survey and administrative data*. Tech. rep., National Bureau of Economic Research.

Moss, K. M., Loxton, D. and Mishra, G. D. (2023). Does timing matter? associations between intimate partner violence across the early life course and internalizing and externalizing behavior in children. *Journal of interpersonal violence*, **38** (19-20), 10566–10587.

National Domestic Violence Hotline (2025). *Emergency housing for domestic violence victims*. Tech. rep., National Domestic Violence Hotline: https://www.thehotline.org/resources/emergency-housing-for-domestic-violence-victims/.

Papageorge, N. W., Pauley, G. C., Cohen, M., Wilson, T. E., Hamilton, B. H. and Pollak, R. A. (2021). Health, human capital, and domestic violence. *Journal of Human Resources*, **56** (4), 997–1030.

Sanin, D. (2021a). Do Domestic Violence Laws Protect Women From Domestic Violence? Evidence From Rwanda. *Working Paper*.

— (2021b). *Paid Work for Women and Domestic Violence: Evidence from the Rwandan Coffee Mills*. Tech. rep., Technical report, Working Paper.

Schwab-Reese, L. M., Peek-Asa, C. and Parker, E. (2016). Associations of financial stressors and physical intimate partner violence perpetration. *Injury epidemiology*, **3** (1), 6.

Sharp, N. (2008). What's yours is mine: The different forms of economic abuse and its impact on women and children experiencing domestic violence. *London: Refuge*.

Stevenson, B. and Wolfers, J. (2006). Bargaining in the Shadow of the Law: Divorce Laws and Family Distress. *The Quarterly Journal of Economics*, **121** (1), 267–288.

Stylianou, A. M. (2018). Economic Abuse Within Intimate Partner Violence: A Review of the Literature. *Violence and Victims*, **33** (1), 3–22.

Tur-Prats, A. (2019). Family Types and Intimate Partner Violence: A Historical Perspective. *Review of Economics and Statistics*, **101** (5), 878–891.

— (2021). Unemployment and Intimate Partner Violence: A Cultural Approach. *Journal of Economic Behavior & Organization*, **185**, 27–49.

WHO (2021). *Violence Against Women Prevalence Estimates, 2018*. Tech. rep., World Health Organization.

Wilson, P. R., J Thorpe Jr, R., Sharps, P. and Laughon, K. (2021). The relationship between housing instability and intimate partner violence: A retrospective study. *Public health nursing*, **38** (1), 32–39.

Yu, R., Nevado-Holgado, A. J., Molero, Y., D'Onofrio, B. M., Larsson, H., Howard, L. M. and Fazel, S. (2019). Mental disorders and intimate partner violence perpetrated by men towards women: A swedish population-based longitudinal study. *PLoS medicine*, **16** (12), e1002995.

Zhang, N. (2025). In-kind housing transfers and labor supply: a structural approach. *Journal of Labor Economics*, **43** (2), 585–633.

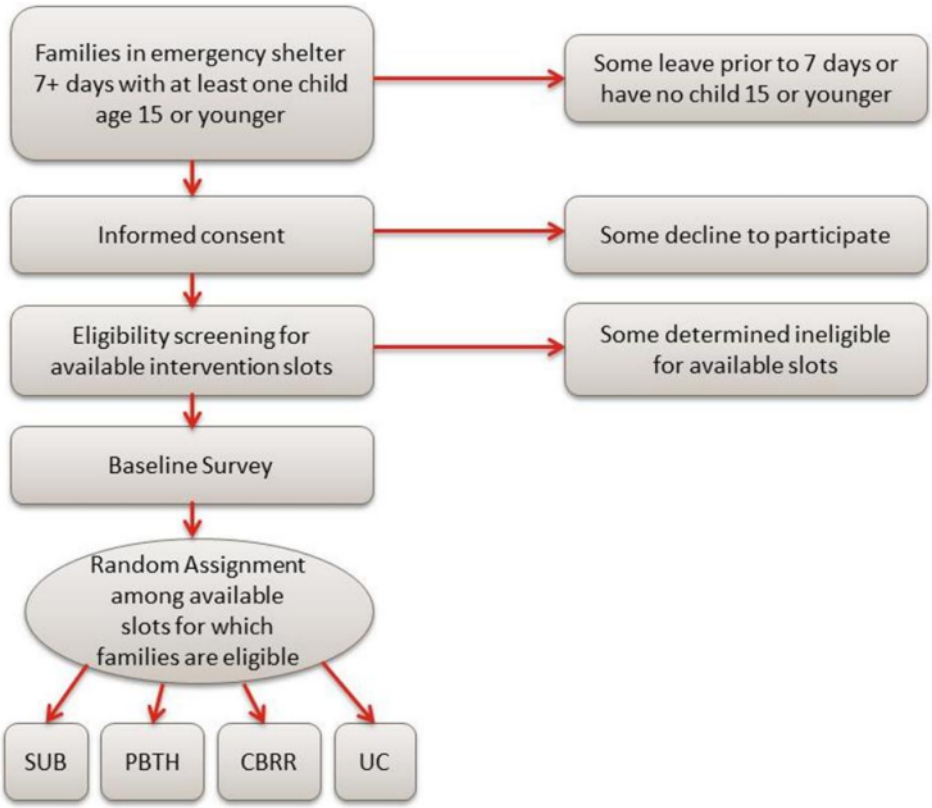
Online Appendix

A Additional Figures and Tables

Figure A.1: Sites of Random Assignment

Notes: This figure depicts the distribution of the 12 sites where a total of 2,282 families (including over 5,000 children) were enrolled in the study from emergency shelters. Data and figure are provided as part of the HUD Family Options Study documentation. Source: https://www.huduser.gov/portal/family_options_study.html#pdr-research-design

Figure A.2: Random Assignment Process



Notes: This figure depicts the design of the random assignment process.
Source: https://www.huduser.gov/portal/publications/pdf/HUD_501_family_options_Data_Collection_Analysis_Plan_v2.pdf

Table A.1: Summary Statistics: Adult Respondents

	(1) UC	(2) ANY	(3) p-value	(4) SUB	(5) p-value	(6) CBRR	(7) p-value	(8) PBTH	(9) p-value
Age group	3.83	3.72	0.15	3.69	0.15	3.72	0.26	3.76	0.57
Female	0.93	0.91	0.23	0.93	0.97	0.91	0.30	0.89	0.03
White	0.20	0.21	0.88	0.22	0.42	0.19	0.57	0.20	0.96
College	0.27	0.27	0.98	0.26	0.62	0.28	0.73	0.27	0.90
High school	0.33	0.38	0.02	0.39	0.03	0.39	0.02	0.36	0.29
Dropout	0.40	0.35	0.02	0.36	0.10	0.33	0.01	0.36	0.26
No. of Children	1.97	2.01	0.50	1.93	0.46	2.02	0.47	2.13	0.05
Homeless years	1.38	1.29	0.36	1.35	0.76	1.27	0.34	1.26	0.40
IPV	0.50	0.49	0.95	0.50	0.97	0.49	0.97	0.47	0.34
Married/Cohabited	0.38	0.38	0.86	0.36	0.33	0.38	0.80	0.42	0.24
Working now	0.18	0.17	0.54	0.13	0.03	0.19	0.65	0.19	0.55
Ever worked	0.96	0.96	0.90	0.96	0.81	0.98	0.19	0.95	0.37
Earnings Cat.	1.54	1.50	0.41	1.35	0.00	1.58	0.58	1.60	0.46
Earnings $5000+	0.16	0.15	0.49	0.11	0.00	0.18	0.45	0.18	0.44
TANF	0.40	0.42	0.47	0.45	0.07	0.40	0.98	0.39	0.63
SNAP	0.89	0.87	0.10	0.87	0.27	0.88	0.33	0.85	0.04
WIC	0.36	0.36	0.85	0.36	0.94	0.36	0.92	0.38	0.61
Poor health	0.09	0.09	0.83	0.10	0.31	0.09	0.96	0.07	0.35
SPD	0.24	0.21	0.06	0.23	0.64	0.18	0.01	0.20	0.16
Substance abuse	0.21	0.21	0.96	0.19	0.36	0.21	0.90	0.25	0.14
N	746	1536	2282	599	1345	569	1315	368	1114

Notes: This table reports the summary statistics for respondents by housing assistance program treatment assignment. IPV refers to ever experiencing intimate partner violence as an adult. SPD refers to "Serious psychological distress." IPV and substance abuse are calculated from the restricted use FOS data, and are part of disclosure clearance number 12783. All other estimates calculated from the public use FOS data. All sample sizes are calculated using the public use FOS data.

Table A.2: Attrition Rate for UC and Housing Assistance Program Treatment Conditions

	(1) UC	(2) ANY	(3) p-value	(4) SUB	(5) p-value
Attrition Rate	0.14	0.10	0.03	0.06	0.00
N	746	1536	2282	599	1345

Notes: This table reports the attrition rate for UC, ANY assignment, and SUB treatment and the p-values for the difference between UC and ANY assignment, between UC and SUB. Attrition is calculated using the public use FOS data.

Table A.3: Baseline Characteristics: Attritors vs. Non-attritors

	(1)	(2)	(3)	(4)	(5)	(6)
		UC			ANY	
	Attritor	Non-attritor	p-value	Attritor	Non-attritor	p-value
Demographic:						
Female	0.91	0.93	0.49	0.84	0.92	0.00
Age group	1.61	1.69	0.38	1.69	1.63	0.37
White	0.27	0.19	0.08	0.29	0.20	0.01
College	0.27	0.27	0.93	0.25	0.27	0.64
High school	0.32	0.33	0.77	0.35	0.39	0.35
Dropout	0.42	0.40	0.72	0.40	0.34	0.17
Married/Cohabited	0.42	0.38	0.49	0.43	0.38	0.19
No. of Children	2.05	1.94	0.37	1.93	1.99	0.48
Homeless years	1.31	1.40	0.75	0.94	1.34	0.02
Labor Market:						
Working now	0.14	0.18	0.26	0.21	0.16	0.12
Ever worked	0.97	0.96	0.66	0.94	0.97	0.07
Annual Earnings Category	1.47	1.56	0.51	1.65	1.48	0.08
Earnings $5000+	0.14	0.17	0.47	0.18	0.15	0.30
Health:						
Poor health	0.11	0.08	0.40	0.12	0.09	0.19
Serious psychological distress	0.23	0.24	0.73	0.22	0.21	0.72
Substance abuse	0.15	0.22	0.08	0.22	0.21	0.77
Welfare:						
TANF	0.43	0.40	0.62	0.36	0.43	0.11
SNAP	0.84	0.90	0.07	0.78	0.88	0.00
WIC	0.24	0.38	0.01	0.27	0.37	0.01
Violence:						
IPV	0.416	0.510	0.077	0.522	0.482	0.34
N	101	645	746	161	1375	1536

Notes: This table summarizes baseline characteristics for attritors vs. non-attritors within UC and within ANY assignment, respectively. IPV and substance abuse estimates are calculated from the restricted-use FOS data and are part of disclosure clearance number 12783. All other estimates are calculated from the public use FOS data. All sample sizes are calculated from the public use FOS data.

Table A.4: Lee Bounds: Impact of Housing Assistance on IPV

	(1) ANY vs. UC Upper	(2) ANY vs. UC Lower	(3) SUB vs. UC Upper	(4) SUB vs. UC Lower
ANY takeup	-0.0561***	-0.0513***		
	(0.0209)	(0.00005)		
SUB takeup			-0.0660***	-0.0585***
			(0.0180)	(0.0001)
N	3600	3500	2200	2100

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of housing assistance programs on the incidence of IPV. All estimates are from 2SLS specifications, with the final analytical sample trimmed following procedures described in Lee (2009). All regressions control for the full set of variables discussed in Equations 1 and 2. Standard errors are estimated using the variation across the 100 draws, and are clustered at the individual level and reported in parentheses. Estimates are part of disclosure clearance number 12783. Sample sizes in columns 1 and 3 are part of disclosure clearance number 12603. Sample sizes are calculated using attrition number based on public use FOS data and sample size estimates from columns 1 and 3. Sample sizes are rounded according to Census disclosure avoidance methods requirements. **** p<0.01, ** p<0.05, * p<0.1*

Table A.5: Summary Statistics: Children Sample

	(1) UC	(2) ANY assign.	(3) p-value	(4) SUB	(5) p-value	(6) CBRR	(7) p-value	(8) PBTH	(9) p-value
Panel A (Baseline)									
Age	5.73	5.57	0.38	5.33	0.07	5.76	0.91	5.68	0.84
Boy	0.51	0.53	0.41	0.53	0.51	0.52	0.76	0.54	0.28
In school	0.95	0.94	0.26	0.94	0.38	0.95	0.95	0.91	0.05
Disabled	0.11	0.11	0.79	0.11	0.71	0.12	0.79	0.10	0.54
Living w/ respondent	0.95	0.95	0.65	0.95	0.85	0.96	0.64	0.96	0.67
Months apart	29.25	21.68	0.23	29.00	0.98	14.79	0.06	18.94	0.31
N	807	1822	2629	738	1545	654	1461	430	1237
Panel B (Follow-up)									
Highest grade	6.08	6.03	0.70	5.91	0.28	6.21	0.42	5.95	0.48
Repeat a grade	0.91	0.92	0.54	0.92	0.34	0.92	0.23	0.89	0.29
Excellent health	0.45	0.48	0.05	0.52	0.00	0.48	0.10	0.42	0.08
Emotional problems	0.33	0.25	0.03	0.22	0.02	0.24	0.07	0.30	0.60
Prosocial behavior	-0.16	-0.11	0.17	-0.09	0.11	-0.10	0.19	-0.15	0.92
Total behavior	0.56	0.44	0.00	0.41	0.00	0.44	0.01	0.51	0.35
N	1548	3470	5018	1395	2943	1256	2804	819	2367

Notes: This table reports the summary statistics for children of the respondents by housing support program assignment. All estimates and all sample sizes are calculated use public use FOS data. Follow-up survey estimates are pooled across the 20- and 37-month surveys.

Table A.6: Impact of Housing Assistance on Other Safety Net Takeup

	(1) TANF	(2) SNAP	(3) WIC
Panel A: ANY vs. UC			
ANY takeup	0.0587**	0.0542**	0.0405
	(0.0293)	(0.0247)	(0.0299)
N	3600	3600	3600
Panel B: SUB vs. UC			
SUB takeup	0.0658**	0.0463**	0.0303
	(0.0277)	(0.0225)	(0.0277)
N	2200	2200	2200

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of housing assistance programs on individual labor market outcomes. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. Standard errors are clustered at the individual level and reported in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Estimates are part of disclosure clearance number 12603. Sample sizes are rounded according to Census disclosure avoidance methods requirements.

Table A.7: Heterogeneity: Impact of Housing Assistance on IPV, by Baseline Partnership Status

	(1) ANY vs. UC	(2) SUB vs. UC	(3) CBRR vs. UC	(4) PBTH vs. UC
Panel A: Single at Baseline				
ANY takeup	-***			
SUB takeup		-***		
CBRR takeup			-*	
PBTH takeup				-**
Panel B: Married at Baseline - No Longer Married				
ANY takeup	-			
SUB takeup		-		
CBRR takeup			-	
PBTH takeup				+
Panel C: Married at Baseline - Still Married				
ANY takeup	+			
SUB takeup		+		
CBRR takeup			+	
PBTH takeup				+

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of different types of housing assistance programs on the incidence of IPV. Panel A reports the results for the subsample of respondents who are single at the time of the baseline survey. Panel B reports the results for the subsample of respondents who are married/cohabited at the time of the baseline survey and are no longer married/cohabited at the time of the follow-up survey. Panel C reports the results for the subsample of respondents who are married/cohabited at the time of the baseline survey and remain married/cohabited at the time of the follow-up survey. All estimates are from 2SLS specifications. All regressions control for the full set of variables discussed in Equations 1 and 2. Standard errors are clustered at the individual level and reported in parentheses. Estimates and sample sizes are part of disclosure clearance number 12924. Sample sizes are rounded according to Census disclosure avoidance methods requirements. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.8: Alternative Controls: Sign & Significance Results for Impact of Housing Assistance on IPV

	(1) ANY vs. UC	(2) SUB vs. UC	(3) CBRR vs. UC	(4) PBTH vs. UC
ANY takeup	-***			
SUB takeup		-***		
CBRR takeup			-	
PBTH takeup				-

Notes: This table uses the restricted-use FOS data from the 20- and 37-month adult follow-up surveys to estimate the impact of different types of housing assistance programs on the incidence of IPV. All estimates are from 2SLS specifications, and reported as "Sign and Significance" following Census disclosure output procedures. All regressions control for the first, smaller set of variables discussed in Equations 1 and 2 and further detailed in the footnotes of Table 2. Standard errors are clustered at the individual level and reported in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Estimates and sample sizes are part of disclosure clearance number 12718.

B Background on the Family Options Study

The Family Options Study (FOS) evaluates different forms of housing assistance and services across multiple sites. The study focuses on identifying which approaches most effectively supported outcomes such as housing stability, family preservation, child and adult well-being, and self-sufficiency. Eligible families were randomly assigned to specific interventions to provide evidence of their impact. The study was conducted in twelve communities between September 2010 and January 31, 2012. According to the US Department of Housing and Urban Development (HUD), it was the largest experimental study on homelessness at that time (HUD, 2025).[29]

Study participants were recruited from twelve communities, focusing on families who were seeking assistance from an emergency shelter and who had been in the shelter for at least seven days prior to the study recruitment and enrollment period. Participation in the study was voluntary; families who declined to participate were not denied usual emergency shelter assistance

[29]It was commissioned by the US Department of Housing and Urban Development to assess the impact of housing and service interventions on homeless families. The commission was in response to Congress's 2006 Fiscal year Appropriations Bill. HUD awarded a contract to Abt Associates in 2008 to conduct the study (HUD, 2025).

and services based on their decision. Families who agreed to participate completed a baseline survey and were randomly assigned to one of four housing and services intervention conditions. Families were subsequently tracked and contacted to complete follow-up surveys, approximately 20-, 37-, and 120-month after random assignment (HUD, 2025).