Does Government-Assisted Relocation Help Households

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December 2025

Abstract

Climate change is intensifying floods and storms, leading governments to consider relocating households away from high-risk areas. Yet there is little evidence on whether such programs reduce risk or improve household well-being. I evaluate the U.S. Federal Emergency Management Agency's property acquisition ("buyout") program using nationwide longitudinal records that track households before and after approval. By comparing participants with similar neighbors in the same flood zones, I identify the program's effects on relocation, flood exposure, and financial outcomes. Households that participate move sooner, relocate to safer areas, and experience modest short-term reductions in debt and improvements in neighborhood socioeconomic conditions. However, these benefits are concentrated among participants from wealthier and predominantly white neighborhoods, who relocate more quickly and achieve larger reductions in risk. A benefit–cost analysis indicates that buyouts are roughly cost-neutral to taxpayers, with potential for net savings under faster implementation and higher participation. These results show that government-assisted relocation can be an effective adaptation tool, but its social and economic benefits remain unevenly distributed.

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Competing Interest Statement: The authors declare no competing interest.
Acknowledgments: I am grateful for valuable feedback from STEP–HMEI fellow seminar participants and from participants in the Stanford Adaptation Conference (2024). I also thank Solomon Hsiang, Michael Oppenheimer, and members of the Global Policy Lab for their thoughtful comments and suggestions. I acknowledge support from Princeton's Industrial Relations Section for assistance with data access and data validation, and I am grateful to Valerie Ching, Lori Bougher, and Gene Piccola for outstanding administrative and technical support.

Introduction

Climate change is forcing societies to confront how and where people can safely live. One emerging approach is not to defend every place, but to help people and assets move away from areas of growing environmental risk. This strategy is known as managed retreat or government-supported relocation. Around the world, governments are expanding such programs, for example, coastal resettlement in the Philippines [1] and Indonesia [2], and floodplain resettlement schemes in Vietnam. [3] As climate change intensifies floods, fires, and storms, these programs are becoming an increasingly prominent yet controversial adaptation tool. A growing body of research has described elements of managed retreat in the U.S. [4;5;6;7] Yet little evidence has been presented to answer the basic question: Can we help people move out of harm's way without making them worse off?

In the U.S., the Federal Emergency Management Agency (FEMA) has operated a property buyout program for more than three decades, helping households in flood-prone areas permanently relocate. Because few countries operate retreat programs with comparable scale, duration, or administrative structure, FEMA's experience provides a rare opportunity to evaluate how assisted relocation affects household outcomes and to inform global adaptation policy. But two challenges have long hindered such evaluations. First, people relocate for many reasons, making it hard to know whether participants fare better than similar households who move—or stay—without government aid. Second, migration data rarely track where individuals live over long periods.

Here, I overcome both challenges by combining administrative FEMA records with commercially sourced individual-level data, tracking households before and after buyout approval and comparing participants with otherwise similar nonparticipants living in the same flood-exposed areas (Fig. 1a). This linked dataset enables the first nationwide causal evaluation of the buyout program, addressing both the lack of long-run migration data and the difficulty of identifying credible counterfactual households.

Ideally, we would observe two identical households—one receiving a government-funded buyout and one not—and follow their outcomes over time. Such an experiment is impossible, but the assembled dataset allows us to approximate it by comparing buyout recipients with neighbors exposed to the same flood risks who did not participate. Using this design, I estimate the program's effects on mobility, flood exposure, and financial well-being and pair these impacts with federal spending data to evaluate fiscal performance. The results show that the program generally pays for itself, with benefits that could be substantially larger under more timely and equitable implementation.

How the FEMA Buyout Program Works The FEMA property acquisition ("buyout") program aims to reduce future flood losses by purchasing damaged homes at pre-disaster market value and converting the land to permanent open space (Fig. SI1). [8;9] Since its inception in 1989, FEMA has funded more than 53,000 buyouts across all U.S. states (Fig. 1a, Fig. SI2), making it the nation's largest managed-retreat initiative. [10] The program is designed primarily to reduce property risk and federal disaster spending, but its effects on participating households remain largely unknown because FEMA does not track households after they sell their

homes. The buyout process is also complex and slow, averaging more than six years from disaster declaration to project closure (Fig. 1b), suggesting that real-world performance may differ substantially from theoretical expectations or idealized planning timelines.

Leveraging Program Structure for Causal Inference Individuals relocate for many reasons,[11;12;13;14] and residents of buyout areas differ systematically from the general population (Fig. SI3a).[4;6] To isolate the causal effects of the program, I implement a difference-in-differences design comparing buyout recipients with nonparticipants who lived in the same ZIP code and Special Flood Hazard Area (SFHA) at the time of approval (see Methods). This comparison holds constant local socioeconomic and environmental conditions. The program's structure further supports identification: within a county, neighbors with similar flood exposure are similarly eligible for assistance. Treated and control groups are well balanced on observable neighborhood characteristics (income, race, age, education; Table SI2 and SI3) and on individual financial characteristics (account balances, mortgage exposure, risk scores, and credit card balances; Table SI4). In addition, both groups exhibit highly similar pre-event trends in all outcomes (Fig. 2, Fig. SI3, Fig. SI4), indicating that the comparison is well suited for causal estimation.

I interpret the main estimates as reflecting the program's real-world performance (an "intent-to-treat"), including long processing times, incomplete participation, and households that ultimately opt out (Methods, Fig. SI5). Because not all approved households relocate, and those who do often move years after approval[15], this approach provides a conservative estimate of the program's impact. To complement this, I also estimate effects for the subset of households that do relocate (an "average treatment effect"). These results illustrate how the program performs when participation is successful and timely, which is relevant for benefit–cost analysis and comparisons with idealized program design. Across both sets of estimates, the findings show large, long-lasting increases in moves out of high-risk areas and persistent reductions in flood exposure, underscoring the program's potential even under imperfect implementation.

To assess broader financial implications, I examine four indicators commonly used in studies of economic mobility and credit access—mortgage holding, credit-card debt, available credit, and overall credit score (Fig. 1e-f, Fig. 2e-h, Fig. SI4.[16;17] These outcomes provide complementary evidence on housing stability, borrowing behavior, and financial resilience.

Finally, because equitable adaptation is a central policy concern, I analyze heterogeneity in program impacts across communities. Past research suggests that lower-income and racially diverse neighborhoods face higher administrative burdens and lower participation in hazard-mitigation programs.[4;5;7;18;19] I test whether participants from lower-income or majority-Black census tracts experience similar reductions in flood exposure and similar financial outcomes as those from more advantaged areas.

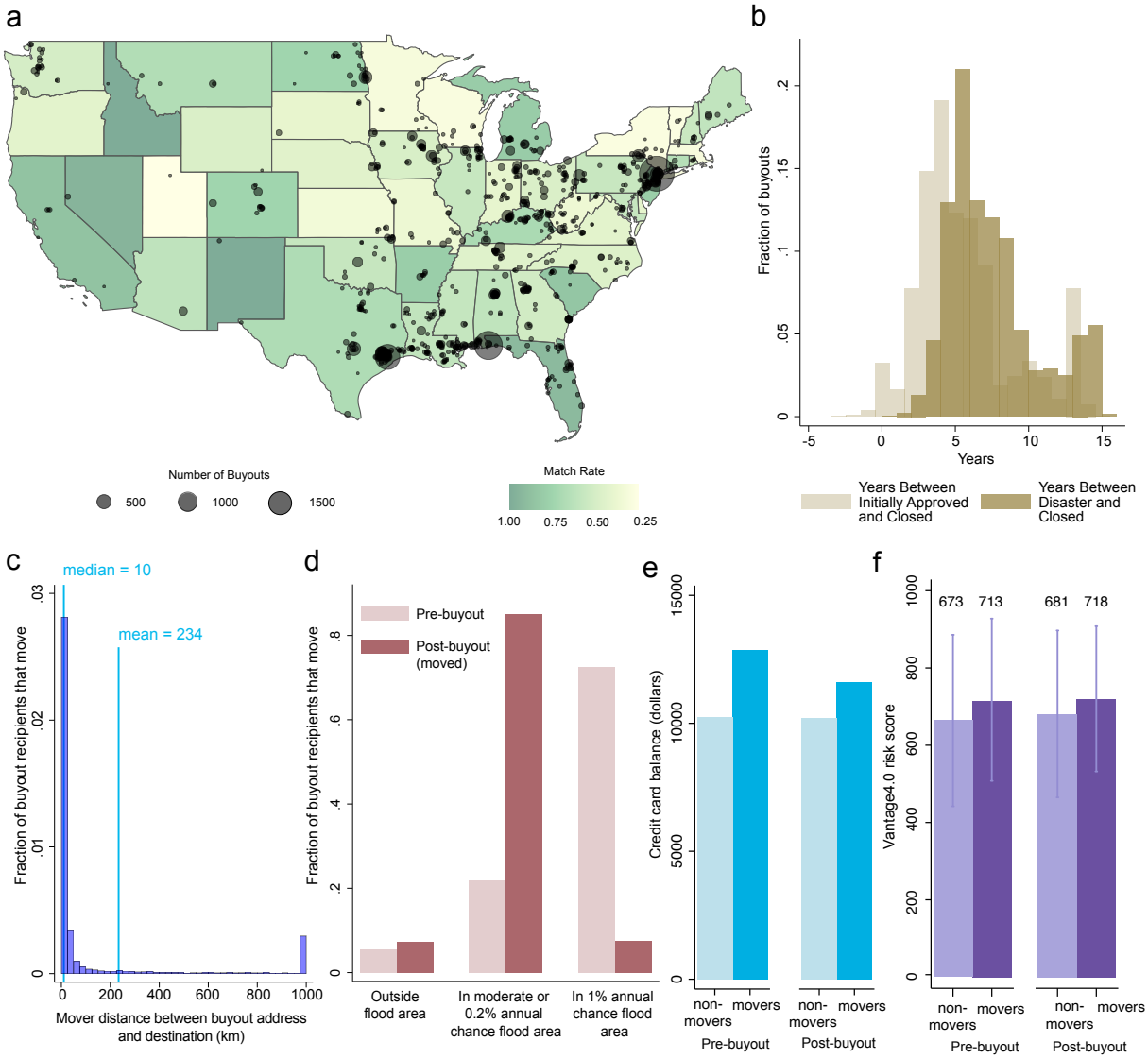


Figure 1: **Descriptive information on the buyout participants (treated sample).** (**a**) Locations and counts of buyout recipients between 2000–2017 in the matched sample (bubble size corresponds to the number of matches in a ZIP code). State-level match rates between Infutor and buyout addresses are shown (dark green = 100% match rate; light green = 25% match rate). (**b**) Distribution of the number of years between the start of the buyout process and buyout closure. Dark brown bars show the difference between the disaster year and the closure year; light brown bars show the difference between the approval year and the closure year. (**c**) Distance (km) between the buyout address and the new addresses for buyout participants who move. (**d**) Fraction of matched buyout participants living in the 1% annual-chance flood hazard area (100-year floodplain), the 0.2% annual-chance flood hazard area (500-year floodplain), and outside the mapped flood hazard area, both before the buyout (light maroon) and five years after the buyout, conditional on moving (dark maroon). (**e**) Average credit card balances (dollars) for buyout participants one year before the buyout ("Pre-buyout") and five years after ("Post-buyout"), shown separately for movers and non-movers. Differences are not statistically significant. (**f**) Average Vantage4.0 credit risk scores for buyout participants one year before ("Pre-buyout") and five years after ("Post-buyout"), shown separately for movers and non-movers. Capped bars indicate 95% confidence intervals; mean values are printed above bars.

Results

Buyouts Accelerate and Enable Relocation The FEMA buyout program markedly increases residential mobility (Fig. 2a). In the first five years after approval, participants are about 20 ±1.1 percentage points ($p < 0.01$) more likely to relocate than comparable households, and the effect remains 10–13 points higher even fifteen years later. Conditional on movers, relocation rates rise by roughly 85 points, indicating that nearly all approved households ultimately move (Fig. 3g). These results show that buyouts both accelerate relocations that would otherwise take a decade or more and enable additional moves that would not have occurred without assistance.

Most moves are short-distance: half of participants resettle within 10 km of their original homes (Fig. 1c). Compared with controls, buyout recipients are about 14 ±1.1 points ($p < 0.01$) more likely to live in a new ZIP code but not more likely to live in a different state (Fig. 2b). Thus, managed retreat under the program is largely local. The program reshapes neighborhood housing markets rather than producing large-scale out-migration. While such shifts can modestly erode local tax bases, they have limited implications for state revenues.[20;21;22]

Large Near-Term Reductions in Flood Risk Buyout participants overwhelmingly relocate away from high flood-risk zones. Within five years, about 65% of movers leave FEMA-designated Special Flood Hazard Areas (SFHAs), most resettling in adjacent low-risk zones (Fig. 1d). Relative to comparable households, the program raises the likelihood of residing outside the SFHA by roughly 13 ±1.4 percentage points ($p < 0.01$) shortly after approval, though the gap narrows to about 5 points after 15 years as control households also move (Fig. 2c). Fewer than one in ten buyout participants relocate to another SFHA property. Conditional on buyout movers, relocation to lower flood risk areas rise by roughly 55 points ($p < 0.0001$) (Fig. SI8c). These results indicate that FEMA buyouts substantially reduce direct flood exposure in the near term, with moderate attenuation over time as natural mobility equalizes risk across groups.

Participants Relocate to More Advantaged Areas Place matters for long-term outcomes, and buyouts may allow households to relocate to areas with stronger economic prospects.[23;24;25] To test this, I compare the census-tract characteristics of participants' origin and destination addresses with those of similar households, using pre-buyout data from the 2000 Decennial Census to avoid contamination by post-program changes.

Five years after approval, buyout participants are about 7.2 (±0.8, p< 0.001) percentage points more likely than the controls to live in tracts with higher per-capita income, 7.3 (±0.9, p< 0.001) point more likely to live in tracts with employment rates, and an 8.3 (±1.4, p< 0.001) points more likely to live in tracts with greater college attainment (Fig. 2c). These results indicate that participants tend to move to nearby but more economically advantaged neighborhoods, suggesting that the program can yield modest gains in opportunity alongside reductions in physical risk. However, conditional on buyout movers, relocation to more economically advantageous neighborhoods is not significant (Fig. SI8d).

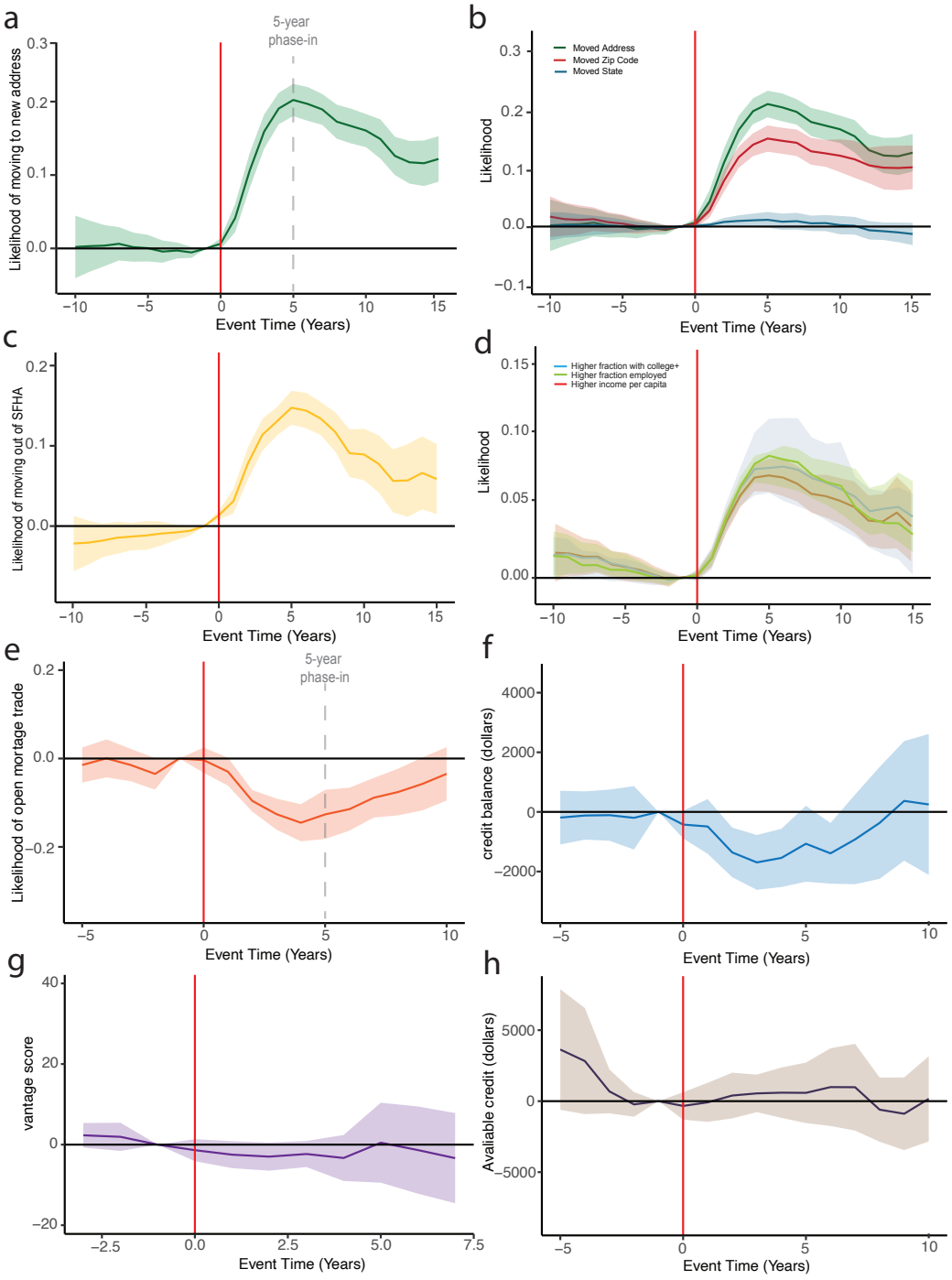


Figure 2: **Effect of the buyout on individuals' locations and financial outcomes.** (**a**) Estimated cumulative intent-to-treat (ITT) effect, with 95% confidence intervals, of participating in the buyout program (2000–2017) on moving to a new address, plotted from 10 years before to 15 years after the buyout. (**b–d**) Same as (a), but for: (**b**) moving to a new ZIP code (red) or a new state (blue); (**c**) moving out of a Special Flood Hazard Area (SFHA); (**d**) moving to census tracts with higher educational attainment (blue), higher employment (green), or higher income (red). (**e–h**) Effects of participating in the buyout program (2008–2017) on credit report outcomes: (**e**) number of mortgages; (**f**) total credit balance; (**g**) Vantage4.0 credit score (estimated for 2010–2017); (**h**) available credit.

Short-Term Debt Reductions, No Lasting Credit Impacts Buyout participation temporarily alters household borrowing patterns but does not meaningfully affect long-term credit access or creditworthiness. Before the program, participants held an average of 0.49±0.74 open mortgages (Table SI4). Four years after approval, they have 0.07±0.015 ($p < 0.001$) fewer mortgages than comparable households ($p < 0.001$; Fig. 2e), a decline that persists for roughly seven years before converging with controls—consistent with participants repurchasing homes several years after relocation.

Participants also experience a short-run decline in credit-card balances, with average reductions of about \$1,694±710 ($p < 0.05$) three years after the program, and \$1,069±733 ($p = 0.14$) after five years (Fig. 2f). The effect dissipates within a decade and is stronger among movers and in higher-income or whiter origin neighborhoods (Fig. 3e). Available credit and non-revolving debts (auto and student loans) show no significant change (Fig. 2h; Fig. SI6). These patterns suggest that some households use buyout proceeds to pay down debt (Fig. 1e) but that the liquidity effect is short-lived.

Contrary to concerns that long processing times might harm financial standing, credit risk scores remain stable or slightly improve. Mean Vantage 4.0 scores rise marginally—from 713 to 718 for movers and 672 to 681 for non-movers—with no statistically significant difference from controls (Fig. 1f, Fig. 2g, Table SI6). Overall, the buyout program appears to leave credit access and creditworthiness unaffected while providing a brief reduction in debt burdens during relocation.

Stronger Mobility Effects in Higher-Income Areas Program impacts differ modestly by neighborhood income. Contrary to expectations that wealthier households would rely less on assistance, buyouts have a somewhat stronger effect in higher-income tracts: residents there are about 21.1±3.5 percentage points ($p< 0.01$) more likely to relocate after approval, compared with 13.2±1.9 points ($p< 0.01$) in lower-income tracts (Fig. 3a). High-income participants are also more likely to move to new ZIP codes (Fig. SI7), although both income groups show similar gains in neighborhood quality. Five years after the program, participants from high- and low-income areas are roughly 6–7 points more likely than controls to live in tracts with higher per-capita income, employment, and education (Fig. 3c).

Credit effects differ slightly by income: while mortgage counts and credit-card balances do not vary significantly across groups, participants from lower-income tracts experience small but significant improvements in credit risk scores (+9 ±4 points, $p < 0.05$; Fig. 3f and Fig. SI9). This suggests that the program modestly strengthens financial standing for lower-income participants who may otherwise struggle to rebuild credit after disasters.

Lower Relocation and Risk Reduction in Majority-Black Areas Racial composition strongly shapes program outcomes. Residents of tracts with larger Black populations are markedly less likely to relocate after approval. In tracts with ≥50 % Black population, the relocation effect is 6.3±3.0 ($p=0.044$), roughly one-third of that observed in tracts with <20% Black residents (18.1±1.3, $p< 0.001$; Fig. 3a). Participants from higher-

fraction Black tracts also move more slowly, with relocation rates resembling those of controls for several years post-buyout (Fig. SI10). Yet among those who eventually move, destinations are comparable: movers from high-fraction Black tracts are just as likely to relocate to lower flood-risk areas as their counterparts from <20% Black census tracts (Fig. 3g).

Differences persist in risk reduction and financial outcomes. Households from majority-Black tracts are less likely to move out of flood zones (6.3±3.0, p< 0.001, Fig 3b) and see smaller gains in neighborhood income (3–4 vs. 8 points; Fig. 3c). Consistent with these patterns, I find no measurable improvement in credit outcomes for high-fraction Black tracts, while residents from lower-fraction tracts experience declines in mortgage counts (about 0.1, $p < 0.001$) and credit-card debt (about \$1,500, $p < 0.05$) consistent with debt repayment (Fig. 3d–e and Fig. SI11).

Together, these results highlight substantial inequities in access to and benefits from the buyout program: households in lower-income or majority-Black neighborhoods face slower relocation, smaller reductions in flood exposure, and weaker financial gains, underscoring the need for more inclusive and timely implementation.

Discussion

Flood-risk reduction and program performance

The FEMA property-buyout program substantially reduces household exposure to flood hazards but does not eliminate risk for all participants. Comparing buyout recipients with matched households in nearby flood-prone areas shows that recipients are significantly more likely to relocate to lower-risk zones, particularly those who were inside FEMA Special Flood Hazard Areas (SFHAs) before the program. This difference is not driven by greater mobility overall but by a stronger tendency among flood-exposed households to move to safer ground—possibly because they have firsthand experience of losses, face mandatory flood-insurance costs, or are more aware of flood-zone boundaries.

Despite this progress, a minority of participants relocate to new properties that remain within SFHAs, meaning that the program does not eliminate risk entirely. As development expands and climate change amplifies coastal flooding, even currently low-risk destinations may face increasing hazard exposure.[26;27;28] Strengthening post-buyout guidance and ensuring that updated flood maps inform relocation choices could enhance the program's long-term effectiveness.

Equity in access and outcomes

The buyout program's benefits are not evenly distributed. Participants from higher-income neighborhoods are more likely to relocate and to move to areas with higher income, education, and employment opportunities, while households from majority-Black or low-income neighborhoods move less frequently and experience smaller improvements in neighborhood quality (Fig. 3). These patterns highlight persistent inequalities in who benefits

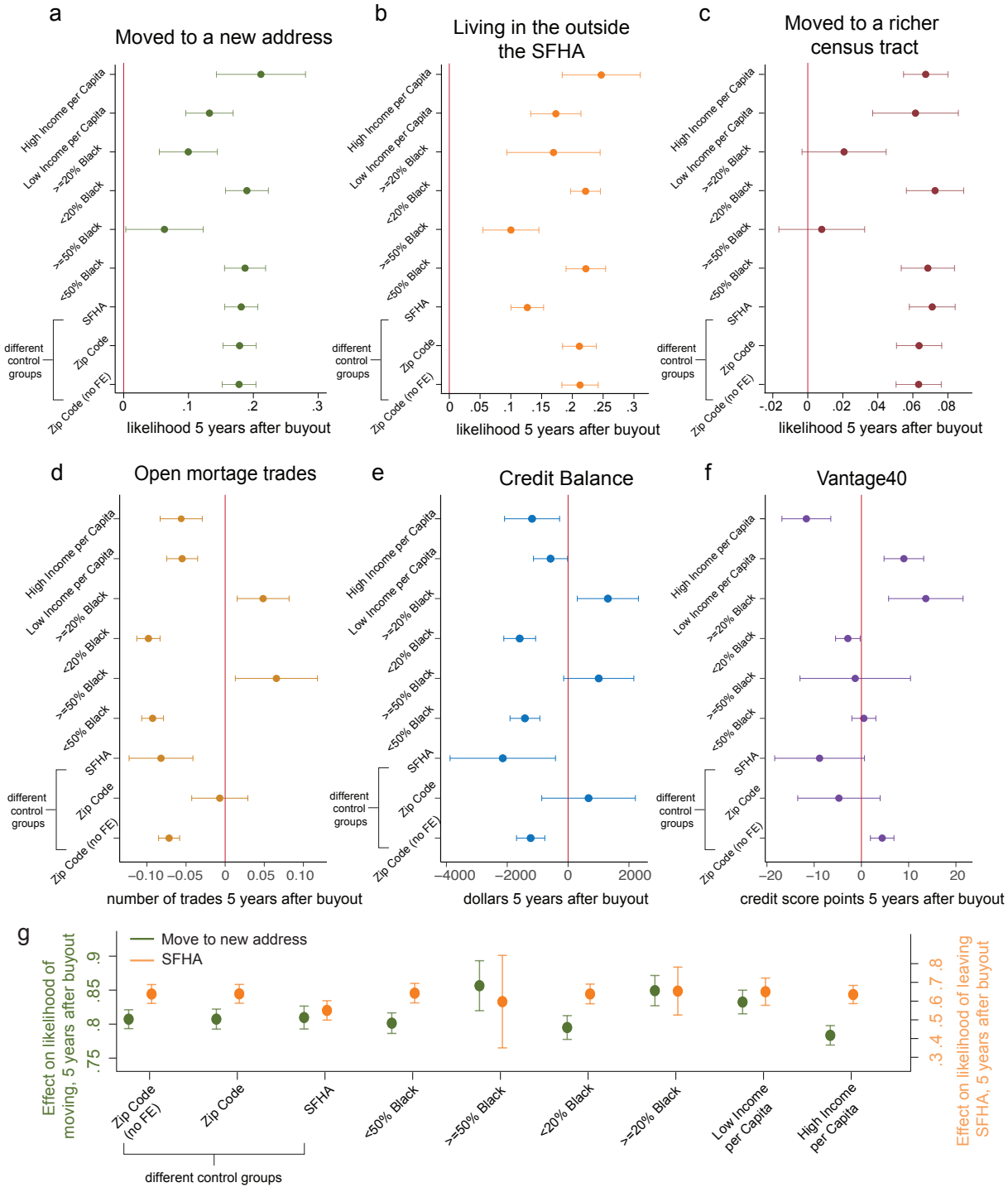


Figure 3: **Differential effects of the buyout across race and income**. Estimated cumulative effects five years after the buyout, relative to three versions of the control group: (1) all individuals living in buyout ZIP codes at the time of the buyout ("Zip Code (no FE)"); (2) individuals in the buyout ZIP code within the same state–cohort ("Zip Code"); (3) individuals in the buyout ZIP code and Special Flood Hazard Area (SFHA) within the same state–cohort ("SFHA"). Heterogeneous effects compare individuals living in high–fraction Black (≥50% Black and ≥20% Black) vs. low–fraction Black (<50% Black and <20% Black) census tracts, and high–income vs. low–income per-capita census tracts. Error bars show 95% confidence intervals. (**a**) Moving to a new address. (**b**) Residing outside the SFHA. (**c**) Relocating to a higher-income census tract. (**d**) Number of open mortgage accounts. (**e**) Credit card balance (USD). (**f**) Credit score (Vantage4.0). (**g**) Cumulative effect on moving to a new address and on moving out of the SFHA, restricting the treated group to movers (ATE).

most from voluntary retreat and hazard-mitigation programs.

Difference across neighborhood income levels The findings across income groups were surprising. I expected richer individuals to be more financially capable of moving on their own, while lower income individuals have less resources to pay for moving expenses. As a result, the effect of the buyout would be higher for those from lower income locations because they are less likely to move without the support of the program. This hypothesis is supported by the differences in the migration rates among control groups in higher and lower income census tracts. I find that the control group in higher income census tracts are more likely to move than those coming from low income census tracts (Fig. SI10a-b). However, the buyout's effect on moving is slightly greater for individuals in richer census tracts, but the difference is not statistically significant.

Difference across neighborhood race composition For individuals living in high fraction Black population ($\geq$50%) census tracts, the buyout program is less effective than for those in high-fraction white population areas. The likelihood of moving is lower for buyout participants in high-fraction Black census tracts, and it doesn't differ from the control group until three years post-buyout (Fig. SI10c-d). These individuals never catch up to those in high-fraction white areas, suggesting an even slower process for acquiring buyouts in high-fraction Black population tracts.

One possible explanation is that individuals living in high fraction Black population tracts struggle to purchase new homes at their buyout purchase price. According to OpenFEMA data, the average buyout payment is \$40,583 in high-fraction Black census tracts, compared to \$93,977 in high-fraction white tracts.[10] This disparity is likely due to historical redlining practices, resulting in lower property values for Black individuals. Consequently, it may be harder for these individuals to find comparable properties outside high-risk flood areas at their buyout price. The persistent decline in the number of mortgages for this group (Fig. 3e) supports this difficulty in purchasing homes and may explain why some individuals drop out of the program. Further exploration by FEMA and other scholars is recommended.

Program efficiency and broader benefits

A back-of-the-envelope cost–benefit analysis (Fig. 4) shows that the program's fiscal efficiency depends strongly on relocation outcomes. I evaluate three scenarios (Fig. 4a):

1. Everyone moves, assuming all buyout recipients relocate to non-flood-risk areas;
2. Based on address-change estimates, accounting for observed participation and compliance; and
3. Based on moves out of SFHAs, incorporating both partial participation and cases where movers resettle in other flood-prone zones.

When all participants relocate to safe areas (Scenario 1), the program yields a benefit–cost ratio of 1.06 (Fig. 4b), meaning that avoided disaster-relief and insurance payouts slightly exceed program costs. However, under Scenarios 2 and 3, reflecting real-world attrition and incomplete risk removal, the benefit–cost ratio falls below

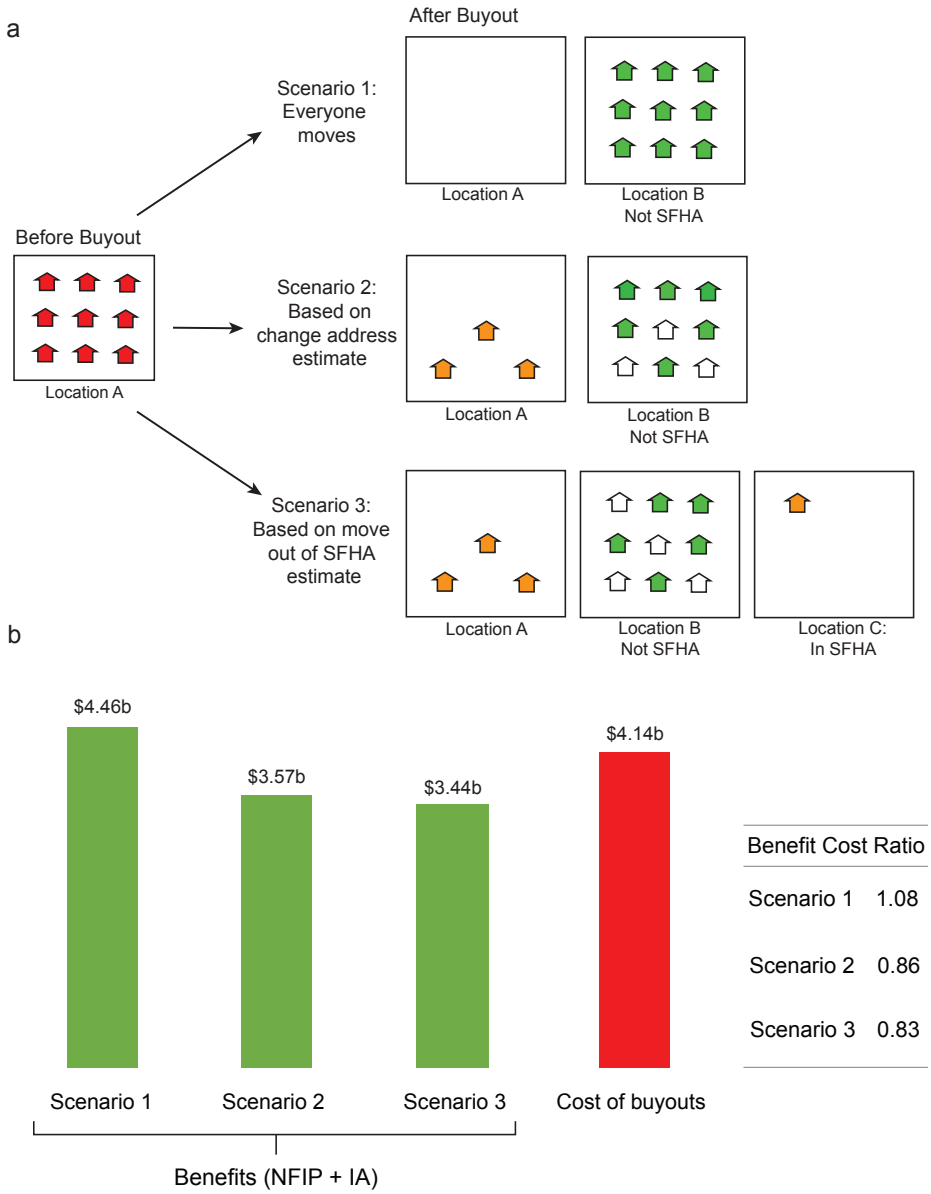


Figure 4: **Benefit–cost analysis of the buyout program**. (**a**) Illustration of the three benefit scenarios analyzed. "Location A" is the buyout location, where the red houses represent properties the government pays to acquire. In Scenario 1 ("Everyone moves"), Location A is empty and all buyout households relocate to Location B (non-SFHA). In Scenario 2 ("Based on change-of-address estimates"), Location A remains partially occupied, with the number of remaining houses determined by the adjusted estimated move rate ten years after the buyout. In Scenario 3 ("Based on moves out of SFHA"), Location A again remains partially occupied based on the adjusted ten-year move rate, and Location C (in SFHA) contains households who relocate to other high-risk areas. (**b**) Total benefits (green bars) and total costs (red bar), expressed in net present value using a 3% discount rate, shown for all three scenarios. Values are in billions of dollars. Numerical benefit–cost ratios for each scenario appear in the accompanying table.

1, indicating that administrative delays and noncompliance erode fiscal efficiency. These findings highlight that implementation speed and participant follow-through are central determinants of the program's return.

Because this calculation includes only avoided National Flood Insurance Program (NFIP) claims and FEMA Individual Assistance payments, it represents a conservative lower bound on total benefits. Even with this limited set of benefits, the program is approximately cost-neutral from the federal government's perspective. Additional gains—such as improved physical and mental health from reduced disaster exposure,[23] spillover reductions in neighboring flood damages, and ecosystem services from restored wetlands[29]—would likely raise the program's benefit–cost ratio above unity.[30] Together, these results suggest that a timely and well-administered buyout program can be fiscally self-sustaining while producing broader social and environmental co-benefits.

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Data Sharing Plans

- FEMA National Flood Hazard Layer maps are publicly available from FEMA (`https://www.fema.gov/flood-maps/national-flood-hazard-layer`).
- FEMA property buyout locations (as compiled by NPR) are publicly accessible at `https://www.npr.org/2019/03/05/696995788/search-the-thousands-of-disaster-buyouts-fema-didnt-want-you-to-see`.
- The Infutor consumer reference address database and credit bureau records used in this study contain personally identifiable information and are subject to proprietary licensing and privacy restrictions; these datasets cannot be made publicly available. Access to Infutor data can be requested directly from Infutor Data Solutions, and access to credit bureau data can be requested from the relevant vendor, subject to approval and data-use agreements.
- All code used in the analysis will be made available upon publication, together with a documented synthetic dataset that reproduces the structure of the analytical file and allows users to execute the full computational pipeline. This synthetic dataset contains no confidential or proprietary information. All additional materials needed to evaluate the conclusions in the paper are included in the manuscript or the Supplementary Materials.

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Supplemental Information

Data Sources

Infutor individual address data

To measure individual locations and moves, I utilize data from Infutor Data Solutions (Infutor), a private consumer reference data company. Infutor aggregates over 560 million records, spanning a panel of more than 260 million individual demographic profiles and addresses from the 1980s through August 2020 (though data completeness for the U.S. population begins in 2000). The data contains street addresses and the date of residence, first and last name, demographic information, and first 5 digits of individuals social security number. Demographic information covers gender, year and month of birth, and year and month of death where applicable. Information such as race, employment, income, and education are not available.

Infutor identifies and authenticates individual addresses using numerous sources such as the National Change of Address, voter registration, deed, property data, automotive sales phonebooks, magazine subscriptions, real estate data, and credit header files. Each address undergoes authentication from at least two direct sources before being recorded in the individual data profiles.

Census tract characteristics

I assembled census tract characteristics for the treated and control addresses for data validation, heterogeneity analysis, and estimating effects on neighborhood characteristics. These census tract characteristics are from the US Decennial Census (2000, 2010 and 2020).[31;32;33]

Equifax credit report data

I use individual-level credit and debt data from Equifax to evaluate changes in household finances. Equifax, one of the largest consumer credit repository and credit scoring companies in the U.S., collects comprehensive information about individual credit histories, including the number of loans, frequency of payments, and amounts owed. The consumer credit account information includes home loans, auto loans, credit card accounts, and student loans. Credit card accounts cover all types of credit issuers, including banks, bank card companies, credit unions, savings and loan associations, and department stores and other retailers. Equifax also provides the Vantage4.0 Risk Score, a trademarked measure of consumer credit risk ranging from 300 to 850. Higher scores indicate higher creditworthiness, with a score of 697 considered "prime," one tier below the best range (i.e., a good score). The Vantage4.0 Risk Score is developed by Equifax in collaboration with other credit bureaus.

FEMA buyout program data

Buyout program participants are identified using data sourced from the National Public Radio (NPR) under their 2017 Freedom of Information Act (FOIA) request. This dataset includes names, addresses, fiscal years (corresponding to the year of the disaster or funding appropriation), and purchase prices for houses acquired through FEMA property acquisition programs spanning 1989 to 2017. It encompasses acquisitions under FEMA's major programs including the Hazard Mitigation Grant Program (HMGP), Flood Mitigation Assistance (FMA), and Pre-Disaster Mitigation (PDM, now called the Building Resilient Infrastructure and Communities Grant Program) (Table SI1).

FEMA flood maps

Property-level flood risk is assessed using FEMA's National Flood Hazard Layer (NFHL) flood hazard maps, which dictate participation in the National Flood Insurance Program (NFIP) and provide comprehensive flood risk data across the United States. The Flood Insurance Rate Map (FIRM) and Letters of Map Change are components of these maps, encompassing GIS boundary layers of flood zones, base flood elevation, and floodway status for specific locations. Key risk classifications include the 1-percent-annual-chance flood event, the 0.2-percent-annual-chance flood event, and areas of minimal flood risk. Properties within the 1-percent-annual-chance flood event boundary, known as Special Flood Hazard Areas (SFHAs), are identified. I utilize these layers to determine whether properties in both treated and control groups fall within SFHAs, as well as to assess flood risk for properties individuals move to. This serves as a proxy for flood risk and provides property owners with clear notification of their participation requirement in NFIP.

Additional FEMA program data

I quantify additional costs associated with the FEMA buyout program, NFIP claims, and payments through FEMA's Individual Assistance (IA) program using the OpenFEMA database. The NFIP data provides individual-level flood insurance claims and policies, ensuring privacy by anonymizing data up to the census tract level, reported zip codes, and precise latitude and longitude to one decimal point.[10] The dataset covers the entire history of program claims from 1978 to the present day, along with policy records spanning from 2008 to 2019. Detailed information on building and content claims payments, as well as policy coverage specifics, are included. While the data records the year of the flood loss event, it does not include the date of fund disbursement to the household. Policy and claims data are anonymized to the census tract, reported zip code, and to one decimal point digit of latitude and longitude.

I also use the OpenFEMA datasets[10] for FEMA's IA programs, including Individuals and Households Program (IHP), which includes Housing Assistance (HA) and Other Needs Assistance (ONA), used in the benefit cost analysis. The dataset includes information on the number of grant applicants, year, county, zip code, inspections, severity of damage, assistance provided, owner or renter status, and grant amounts for each applicant. This data forms the basis for estimating the costs of individual assistance per household following a disaster.

Data Processing & Sample Construction

Constructing individual address histories

Infutor assigns an effective date to each address, representing the date when they confirm the individual's association with that address. I make corrections for the last date in the panel based on an individual's date of death (if relevant). Non-residential addresses, such as PO boxes, are omitted if they appear with the same effective date as another residential address for the same individual.

Measuring moves

To measure moves using the Infutor data, I construct a year-over-year migration variable based on whether the individual is observed at a new address within a given year. For example, if I observe a person i with two addresses (A_i^1 and A_i^2), each with an associated effective year, E_i^1 and E_i^2 respectively, where $E_i^1 < E_i^2$, I assume that individual i resides at address A_i^1 from E_i^1 to $E_i^2 - 1$ and at address A_i^2 from E_i^2 onwards. Consequently, I infer that the individual moved in year E_i^2. This produces a consistent year-over-year migration history for each individual.

In instances where Infutor reports multiple addresses with the same effective year, I determine the correct ordering using the dataset's supplemental timing fields—specifically, the effective-date correction variable, the calendar month (when available), and Infutor's internal rank ordering of addresses within a year. These tie-breaking rules ensure a unique and temporally consistent address sequence prior to constructing the migration indicator.

Matching FEMA buyout addresses to Infutor

To identify buyout participants in the Infutor individual address data, I matched the buyout addresses with the addresses in the Infutor data. The total number of buyouts in my sample from 1989–2017 is 41,004 properties, and from 2000–2017 is 19,169 properties. I performed the matching on the full sample of buyouts between 1989–2017 but, because the Infutor data is limited in the 1990s, I restricted my treatment to buyouts that occur in 2000–2017. I conducted robustness checks on subsets from 2010–2017, and the results are consistent with the main findings. I used three separate matching procedures:

1. exact match on address, surname, and year of the buyout;
2. fuzzy match on address and surname;
3. exact match on cleaned address and buyout year.

Exact match on address and surname. This method results in many unmatched properties because only 43% of NPR addresses include an owner name, and these names contain errors requiring cleaning. It also restricts matches to individuals sharing the surname of the recorded owner—missing renters, cohabitants, and

other adults. The match rate is 9% for both 1990–2017 and 2000–2017.

Probabilistic matching (fastLink). Using Levenshtein distance with a threshold of 0.93, I identified 7,631 people with matched buyout addresses. Of these, 5,862 moved to new addresses and 3,848 moved twice, totaling 24,148 addresses. Manual auditing revealed many false positives, even with conservative thresholds, leading me to exclude this method from the final dataset.

Exact match on cleaned address and buyout year (preferred). After cleaning and standardizing addresses, I conducted exact matching on full address fields, ensuring the individual lived at the buyout address during the buyout year (either disaster year or fiscal year when funding occurred). This approach yields a 47.5% match rate for 2000–2017, peaking at 80% in 2016 (Fig. SI13). I identified 14,999 participants across the U.S., averaging 833 treated individuals per cohort year. Unmatched addresses often correspond to entire buyout projects that never appear in Infutor, likely because residents moved before Infutor captured their residency. These missingness patterns are absorbed by fixed effects (Fig. SI14).

Match rates also vary across states. States with very few participants often show either very high or very low match rates depending on the timing of their buyouts. For example, Idaho has only 19 buyout properties—all occurring after 2010—and a 100% match rate. Utah, by contrast, has 87 buyouts but a much lower match rate (2%), primarily because its buyouts occurred earlier (pre-2010), when individuals were less likely to appear in the Infutor sample.

Constructing the control groups

Because participation in the buyout program is voluntary, the ideal counterfactual is the treated individual in a world where they did not receive a buyout.[34] As this cannot be observed, I construct two control groups using Infutor: (1) individuals living in ZIP codes where buyouts occur; (2) the subset of these individuals who also reside in SFHAs.

To implement these comparisons, I identify ZIP codes with buyouts in each year, then draw individuals living in those ZIP codes during that year. I match their addresses to FEMA flood hazard layers to determine SFHA status. The ZIP-only group includes 705,874 individuals; the SFHA-restricted group includes 47,973 individuals.

Balance improves substantially in the SFHA-restricted sample (Table SI2 and SI4), which also exhibits the closest pre-treatment alignment in event-study trends. For these reasons, the SFHA group is the preferred control group.

Validation and Measurement Checks

Representativeness of Infutor

Several data checks were conducted to ensure data accuracy. Similar to prior studies, I find that the data is representative of the overall US adult population.[35;36;37] Fig. SI12a–b shows the correlation between a 0.1% sample of the Infutor population and Census population in 1990, 2000, and 2010 at the state and county level. Over time, the completeness of Infutor data improves, showing consistently high correlations with census counts across the decades. The coefficients in Fig.s SI12a–b approach 0.001, equivalent to a perfect correlation between the census population and the 0.1% Infutor sample. These relationships also have very high R^2 (> 0.98) at both the state and county levels.

There are limitations: Infutor does not capture international moves; some demographic fields (e.g., birth dates) are missing for a subset of individuals; and homeowners and individuals with credit histories are overrepresented. Despite these limitations, the data remain one of the few nearly comprehensive address histories of U.S. adults.

Infutor under-counting of moves

Infutor move counts correlate with administrative migration data but undercount moves relative to IRS Statistics of Income (SOI) county-to-county migration data.[38;39] Infutor shows lower log odds of county out-migration compared to IRS data for 2000 and 2010, with differences ranging from 0.9 to 2 log odds (53% to 247% lower) after adjusting for population. One explanation is that IRS SOI includes the universe of tax filers, whereas Infutor relies more on credit-related data sources.

Infutor also undercounts moves relative to the American Community Survey (ACS).[40] The ACS reports when individuals moved to their current home (within one year, five years, ten years, etc.). Comparing these aggregates to the Infutor-derived likelihood of living in the buyout address prior to the buyout, Infutor undercounts moves within five years by 149% compared to the 2010 and 2015 ACS. This gap declines for moves within ten and fifteen years (79% and 43% undercounts, respectively), suggesting Infutor more reliably captures moves with longer lags.

These discrepancies are consistent across treated and control groups (Table SI5).

FEMA data quality + non-compliance

I verified the NPR buyout count against FEMA's publicly available data, OpenFEMA[10]. OpenFEMA is an online platform that hosts downloadable datasets including descriptions of disaster declarations, program spending at the project and property levels, community grant applications, and NFIP information. FEMA released these data to "promote transparency, reduces complexity related to public data requests, and improves how stakeholders interact with and understand the program".[10] OpenFEMA includes anonymized property-

and project-level information on participants in all of FEMA's hazard mitigation grant programs. I compared the count and timing of properties reported as "property acquisitions" in the OpenFEMA against the NPR FOIA data, and the decade-level counts of the number of buyouts is similar (Fig. SI2).

Public FEMA data records have several observable issues and errors evident in the OpenFEMA dataset (e.g., incorrect zip codes and misspelled state names), stemming from underlying problems with FEMA's record-keeping. A DHS OIG audit of the program data found FEMA hazard mitigation grant program records to be incomplete, inaccurate, and sometimes noncompliant with open space requirements. AECOM's digitization effort documented missing parcel data, incorrect addresses, and thousands of properties still containing structures decades after acquisition.

My own checks yield consistent evidence of underlying record issues: (1) 15% of buyout locations still show structures in Google satellite imagery (Fig. SI15); (2) 14% have addresses that cannot be located or are not mapped. These issues contribute to apparent low move rates among treated individuals and are incorporated into the adjusted estimates used in the benefit–cost analysis.

Adjusted move-rate calculations

Because Infutor undercounts moves, I adjust the estimated difference-in-differences effect on treated individuals using administrative benchmarks.

Using the ACS adjustment alone, the estimated likelihood of moving to a new address after the buyout (treated group) becomes:

- 56.3% within five years
- 71.8% within fifteen years

Including FEMA non-compliance and data errors further increases the adjusted move-rate. The DHS OIG audit revealed widespread data inaccuracies in FEMA records, and AECOM's digitization identified 8,000 properties with incorrect or missing parcel data and 2,800 properties that still had structures present despite being labeled as buyouts.[15] My own random sampling found that approximately 15% of listed buyout locations still had structures (Fig. SI15). An additional 14% of "removed" properties had non-searchable or nonexistent addresses.

Accounting for both Infutor undercounting and FEMA non-compliance:

- 74.3% likelihood of moving after five years
- 74% likelihood after ten years (56% Infutor-adjusted move rate + 18% estimated FEMA non-compliance contribution)

These adjusted values are used in the benefit–cost analysis.

Identification Strategy & Estimation

Conceptual identification

The goal of the analysis is to estimate the causal effect of receiving a buyout on relocation and financial patterns. The primary challenge is that participation in the buyout program is voluntary and, therefore, not randomly assigned. Previous studies have identified distinct demographic differences between buyout participants and their non-participating neighbors.[4;5] For example, buyout participants tend to reside in census tracts with higher median incomes, greater educational attainment, and larger proportions of White residents compared to non-participants in the same ZIP codes (Table SI2). Therefore, these groups may have exhibited a predisposition towards moving even in the absence of the buyout, potentially to more desirable locations, or they may possess a stronger inclination towards relocation.

Since the program is time-consuming and slow, there may be significant attenuation between the initial disaster event and the application process, program approval, and final closeout. Therefore, I interpret the main results as a measure of intent to treat (ITT), or as a measure of encouragement and intention to participate. This is inherently a conservative estimate of the program's effectiveness, but it highlights important issues related to the non-randomness of program approval, attenuation, and, ultimately, who relocates. I also present results conditional on participants moving after approval, which can be interpreted as the average treatment effect (ATE) for compliers. While the ATE estimate is likely a biased overstatement of the program's impact, it provides a more accurate estimate of avoided costs to the government for use in the benefit–cost analysis. Conditioning the treated group on those who move may be a closer estimate of those who were fully treated.

Individuals move for many reasons,[11;12;13;14] and residents of buyout areas differ from the general population.[4;6] These differences raise concerns that buyout participants may have relocated—or improved their financial situation—even in the absence of the program. To address this, I construct comparison groups of nonparticipants drawn from the same ZIP codes and, in a stricter version, the same Special Flood Hazard Areas (SFHAs) in the year of the buyout. This approach holds local flood risk, policy environment, and neighborhood characteristics constant.

After conditioning on cohort year and county, the treated and control samples are closely aligned on observable census-tract characteristics—including income, education, age structure, and racial composition—as well as financial indicators such as open mortgages, credit card balances, and credit risk scores. Differences that appear in raw ZIP-only comparisons are substantially reduced or eliminated once these conditioning variables are applied. The two groups also exhibit parallel trends in the pre-buyout period. Together, these patterns support the use of a difference-in-differences framework, in which identification relies on similar pre-treatment trends across groups. Tables SI3, SI4, and SI6 summarize balance on observables, and Figures 2 and SI3 show the pre-period event-study coefficients.

Difference-in-differences model

To estimate the difference in means, or the intent to treat (ITT), for the outcomes of interest, I use linear regression. I run four versions of the model with different location and time interacted fixed effects. I do not find statistically significant differences in the effect on moving between the different versions. Table SI7 shows the main effect of moving to a different address for treated individuals five years after the buyout across the four models. Therefore, Equation 1 is my preferred specification.

$$y_{it} = \sum_{\tau=-10}^{15} \beta_\tau \cdot \mathbb{1}[buyout_{i\tau}] + \mu_{s\times m} + \gamma_c + \delta_t + \epsilon_{it} \tag{1}$$

where y_{it} denotes the outcome for individual i in year t, and $\mathbb{1}[buyout_{i\tau}]$ is an indicator equal to one if individual i received a buyout τ years before or after year t ($\tau = t - m_i$). The coefficients β_τ trace the dynamic effects of buyout participation relative to the year prior to treatment. The model includes state-by-cohort fixed effects ($\mu_{s\times m}$), county fixed effects (γ_c), and year fixed effects (δ_t). Standard errors are clustered at the state level.

Primary outcomes include indicators for whether an individual (1) remains at their buyout address, (2) lives within the same ZIP code or state, (3) resides in an SFHA, or (4) lives in a census tract with above-median household income (2000 Census baseline).

Recognizing that full program participation requires individuals to vacate their properties, the main effects of this analysis are best interpreted as ITT estimands. I also report ATE estimates conditional on treated individuals who moved at some point after $t = 0$, using the same regression but defining treatment only for movers.

Robustness

Control Groups I re-estimate the event-study models using both control groups: (1) individuals in ZIP codes where a buyout occurred in the cohort year, and (2) those individuals residing in SFHAs. The SFHA-restricted group exhibits stronger pre-treatment alignment with the treated group, and is therefore used in the main results. ZIP-only estimates are shown for comparison (Fig. 3, Table SI7, Fig. SI3).

Model Variants I estimate four versions of the difference-in-difference model, adjusting location and time fixed effects. The estimated effects on moving are statistically indistinguishable across specifications. Table SI7 summarizes the main effect at five years post-buyout.

Estimation of heterogeneous effects

To test for heterogeneous treatment effects, I subset the effect based on census tract demographics and income using a triple-difference specification. I interact event-time indicators with treatment and an indicator for

whether the individual lived in a high- or low-income tract or high- or low-fraction Black census tract. The regression is:

$$y_{it} = \sum_{\tau=-10}^{15} (\theta_{\tau}^{low_frac_Black} \cdot \mathbb{1}[buyout_{i\tau}] \cdot Q_i^{low_frac_Black} + \theta_{\tau}^{high_frac_Black} \cdot \mathbb{1}[buyout_{i\tau}] \cdot Q_i^{high_frac_Black}) + controls + \epsilon_{it} \quad (2)$$

where $Q_i^{low_frac_Black}$ and $Q_i^{high_frac_Black}$ indicate the demographic categories of interest in the pre-buyout year. The coefficients separately capture dynamic effects for each subgroup.

Benefit–Cost Analysis

In the back-of-the-envelope calculation, I estimate the costs and potential savings from FEMA's buyout programs (1989–2019) across three scenarios (Fig. 4a):

1. assuming all buyout participants relocate to non-flood-risk areas, thereby avoiding any flood events (0% chance);
2. accounting for the adjusted estimated move rate among buyout recipients, where all movers relocate to non-flood-risk areas (Fig. SI16);
3. incorporating the adjusted move rate from Scenario 2, with some movers opting for high flood-risk areas, based on my estimated results (Fig. 1d).

Benefits are calculated based on the savings derived from avoided expenditures on the National Flood Insurance Program (NFIP) claims and FEMA Individual Assistance (IA) payments post-disaster.[10] In the counterfactual scenario where the buyout did not occur, I assume that those properties would have maintained full occupancy for 70 years. Spending on NFIP is estimated using historical averages of building and content claims per property from 1970–2019, while IA spending is based on cumulative expenditures under the Individuals and Households Program (IHP), encompassing Housing Assistance (HA) and Other Needs Assistance (ONA) programs from 2002–2022.[10] It is assumed that participants in the buyout program were also NFIP policyholders who paid average NFIP premiums (adjusted for historical rate increases).[10] Therefore, claims payments are considered net of premiums. Additionally, I assume that all buyout properties are classified as severe repetitive loss (SRL) properties, experiencing at least two flood events in the last decade.[8]

Costs are based on the reported amount the federal government spent purchasing property buyouts between 1989 and 2020, reported in FEMA data sources.[10] Because 25% of the property payment values in the FEMA data sources are missing, I also estimate an adjusted version of the costs based on the mean spending across all years of the program. The total spending on properties during this period (unadjusted) was $4.5 billion, and with the adjustment, it is $5.1 billion. This cost measure excludes unaccounted-for administrative expenses

from federal, state, and local governments, meaning it is likely an underestimate of total program costs. I do not include administrative costs in benefits either, maintaining consistency across scenarios. Costs are identical across all three scenarios because all buyout participants receive payments; inefficiency arises from individuals not moving or relocating into flood-risk areas.

Once the cost and savings schedules are estimated, I convert all amounts to net present value using a 3% discount rate, consistent with the recommendation of the U.S. Office of Management and Budget.[41] It is important to note that these results are highly sensitive to the discount rate because the benefit schedule spans a long time horizon (70 years for each property), whereas the costs are incurred in the year of the buyout.

In this context, I assume that all buyout participants are marginal, implying they are indifferent between converting their parcel into open space or continuing to reside in it. I do not consider additionality concerns, assuming the program operates as a Pigovian subsidy where benefits can be quantified through avoided costs from flood-related risks. However, if individuals are inframarginal—meaning they would have engaged in flood risk–reducing behavior or converted the property into open space without the buyout program—there could be social benefit losses or inefficiencies. Observing that some individuals would have moved even without the buyout suggests the program may not operate optimally, potentially leading to inefficiencies. Furthermore, there may be selection issues where individuals more aware of the program are more likely to move independently.

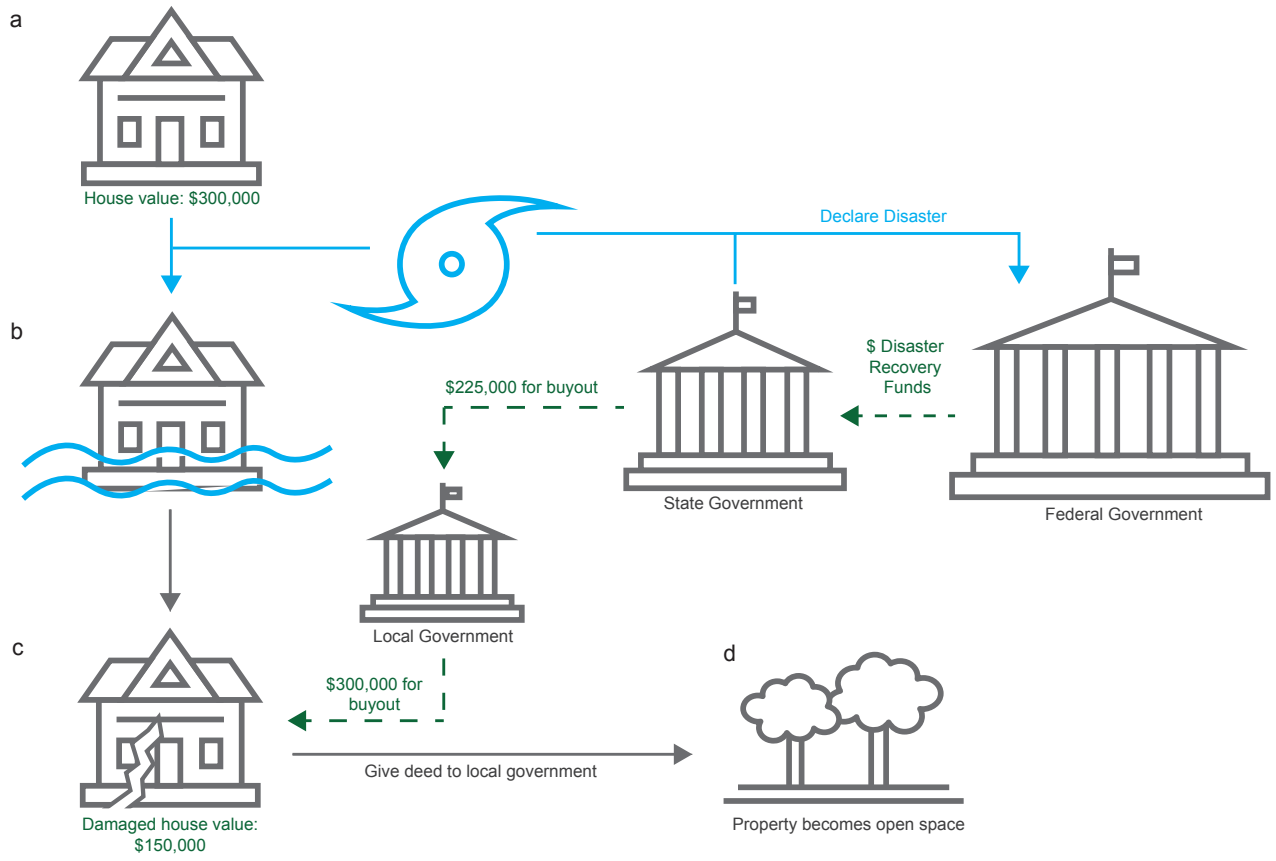


Figure SI1: **Diagram of the FEMA buyout process through the Hazard Mitigation Grant Program.** (**a**) Pre-disaster home in the Special Flood Hazard Area (SFHA) with a market value of $300,000. (**b**) A hurricane event causes the home to flood, triggering the Governor's disaster declaration request. (**c**) Following a Presidential Disaster Declaration, the federal government allocates disaster-relief funding. The state solicits applications from local governments and allocates 75% of the pre-disaster market value for the buyout ("$225,000"). Local governments provide the remaining cost share ("$300,000 for buyout"). The damaged structure is valued at 50% of its pre-disaster value ("$150,000"). (**d**) The property owner transfers the deed to the local government; the house is demolished and converted to open space.

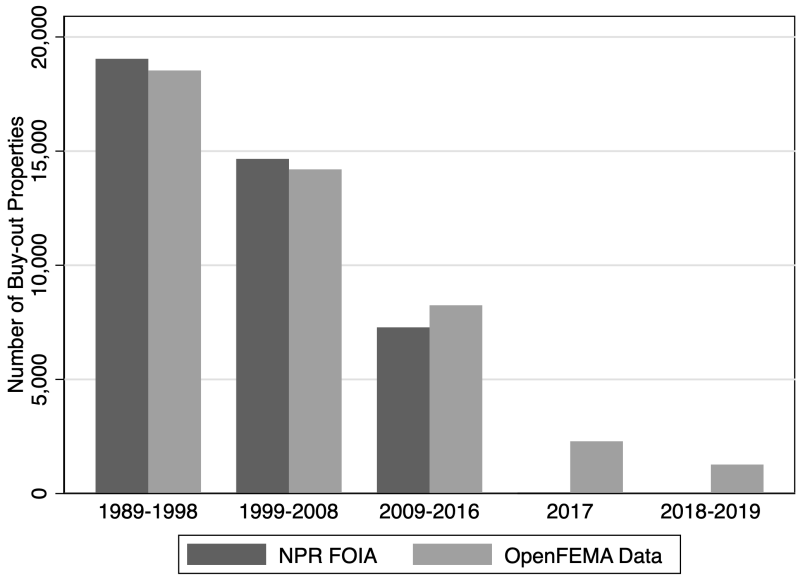


Figure SI2: **Comparing NPR and OpenFEMA buyout property data.** Counts of buyout properties per decade in the NPR FOIA dataset (dark gray) and the OpenFEMA dataset (light gray)[10].

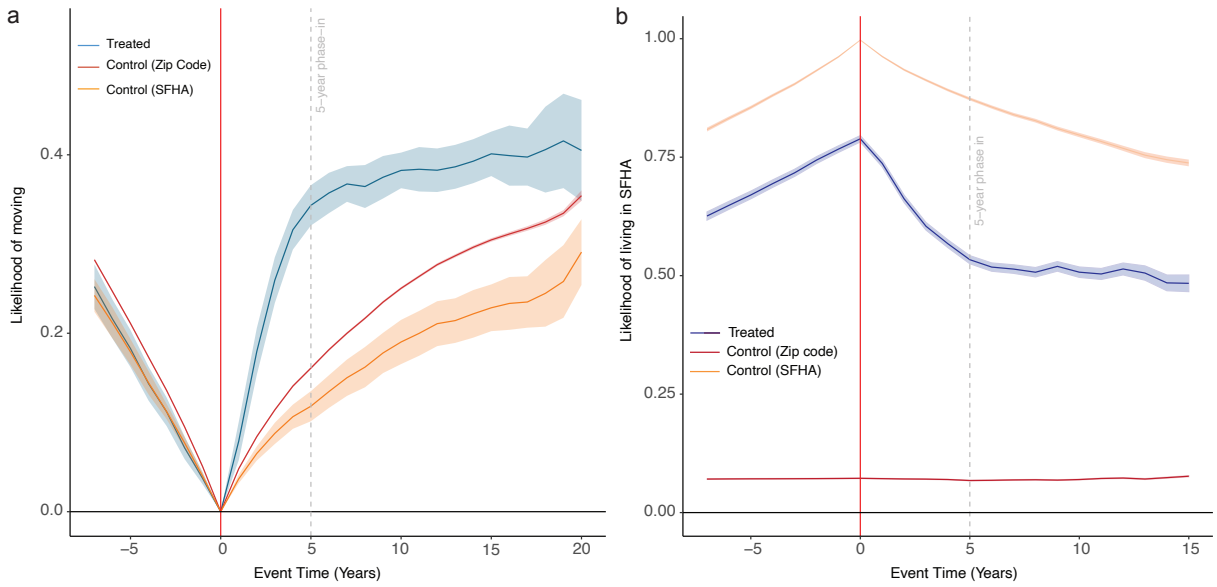


Figure SI3: **Cumulative average likelihoods of moving pre- and post-buyout.** (**a**) Cumulative effect of the buyout on the likelihood of living at a non-buyout address for treated individuals (blue) and two control groups: (1) a random subset of individuals living in the buyout ZIP code in the buyout year (red), and (2) those individuals who also reside in SFHAs (yellow). (**b**) Cumulative effect of the buyout on the likelihood of living in an SFHA. Shaded areas show 95% confidence intervals.

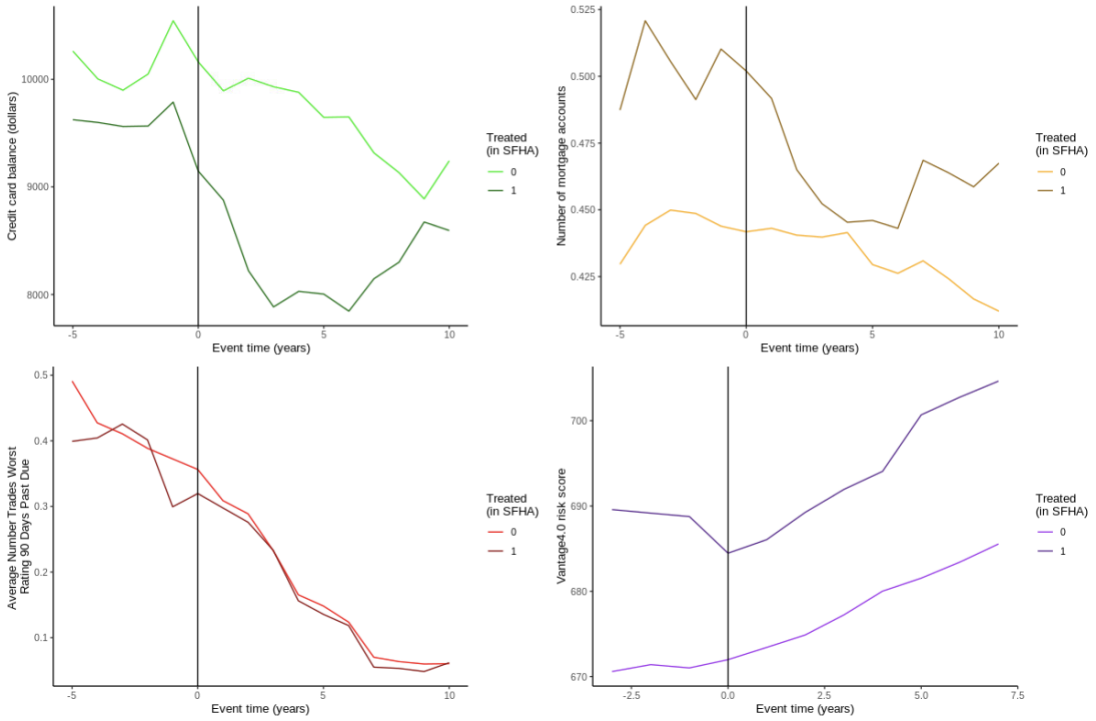


Figure SI4: **Average financial outcomes before and after the buyout, treated and control groups.** Mean financial outcomes for treated individuals and SFHA control individuals in event time. (**a**) Credit card debt. (**b**) Number of mortgages. (**c**) Number of trades with a worst rating of 90 days past due. (**d**) Vantage 4.0 risk score. Shaded areas show 95% confidence intervals.

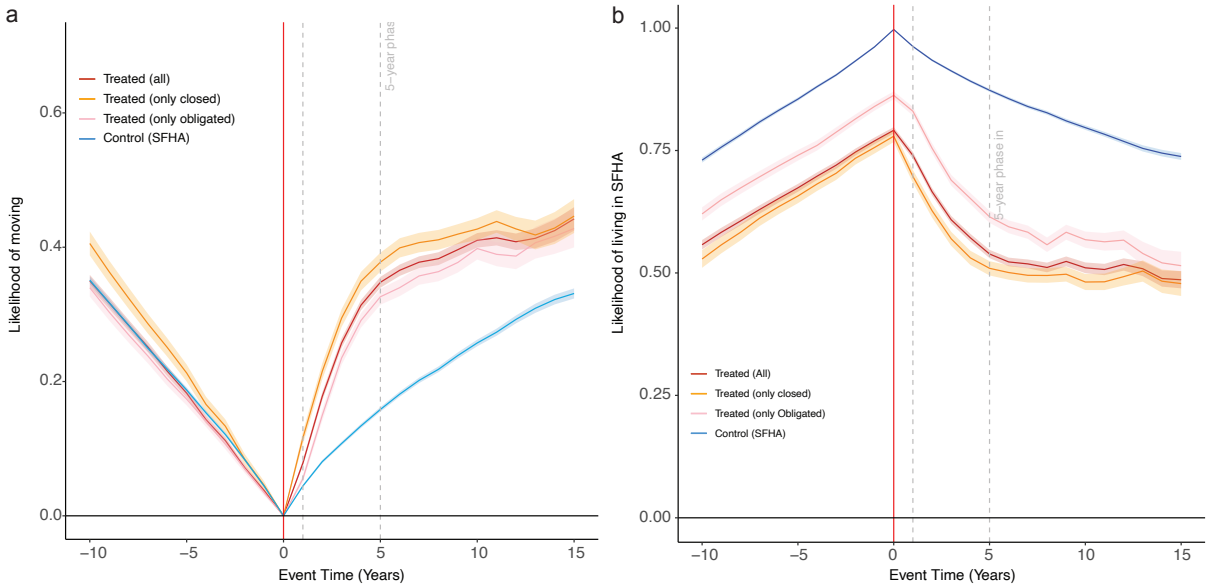


Figure SI5: **Cumulative average likelihoods of moving by FEMA project status.** (**a**) Cumulative likelihood of moving for treated individuals, disaggregated by FEMA marking: full set of buyouts (red), "closed" buyouts (yellow), and "obligated" buyouts (pink), compared with SFHA controls (blue). (**b**) Cumulative likelihood of living in an SFHA for the same groups. Shaded areas show 95% confidence intervals.

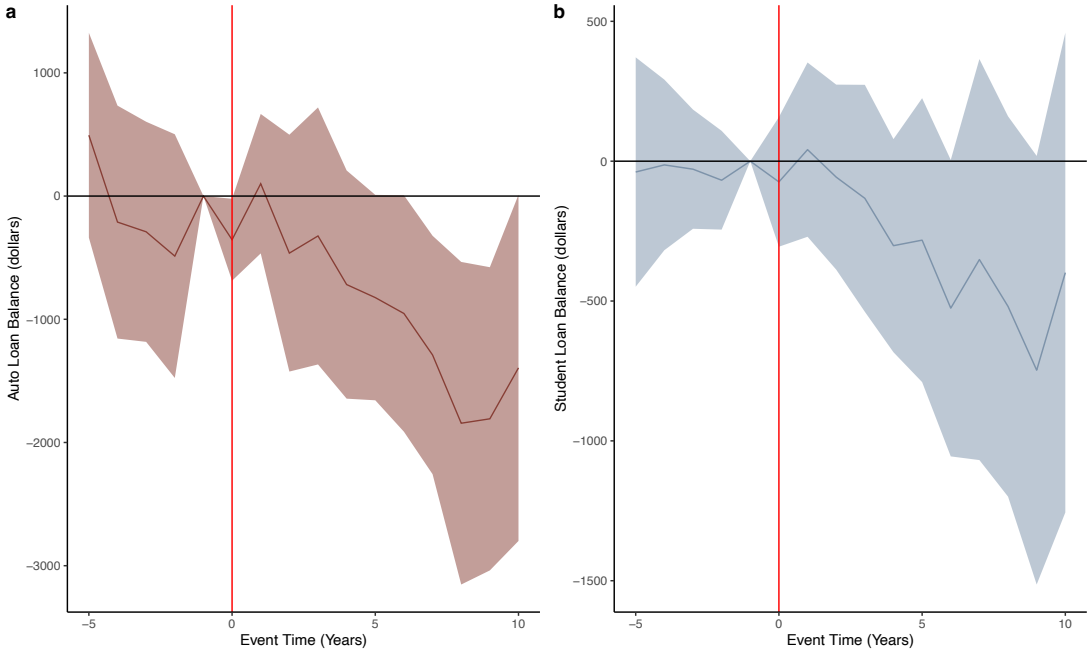


Figure SI6: **Effect of buyouts on auto and student loan balances.** Estimated marginal effects up to ten years after the buyout. (**a**) Auto loan balances. (**b**) Student loan balances. Shaded areas are 95% confidence intervals.



Figure SI7: **Five-year buyout effects on moving to a new ZIP code or a new state.** Estimated cumulative effects five years post-buyout relative to three control definitions: (1) individuals in the buyout ZIP code ("ZIP Code (no FE)"), (2) individuals in the buyout ZIP code and same state–year cohort ("ZIP Code"), and (3) individuals in the buyout ZIP code, same state–year cohort, and in SFHAs ("SFHA"). Heterogeneous effects are shown for census tracts with high vs. low Black population shares and high vs. low income. Whiskers show 95% confidence intervals.

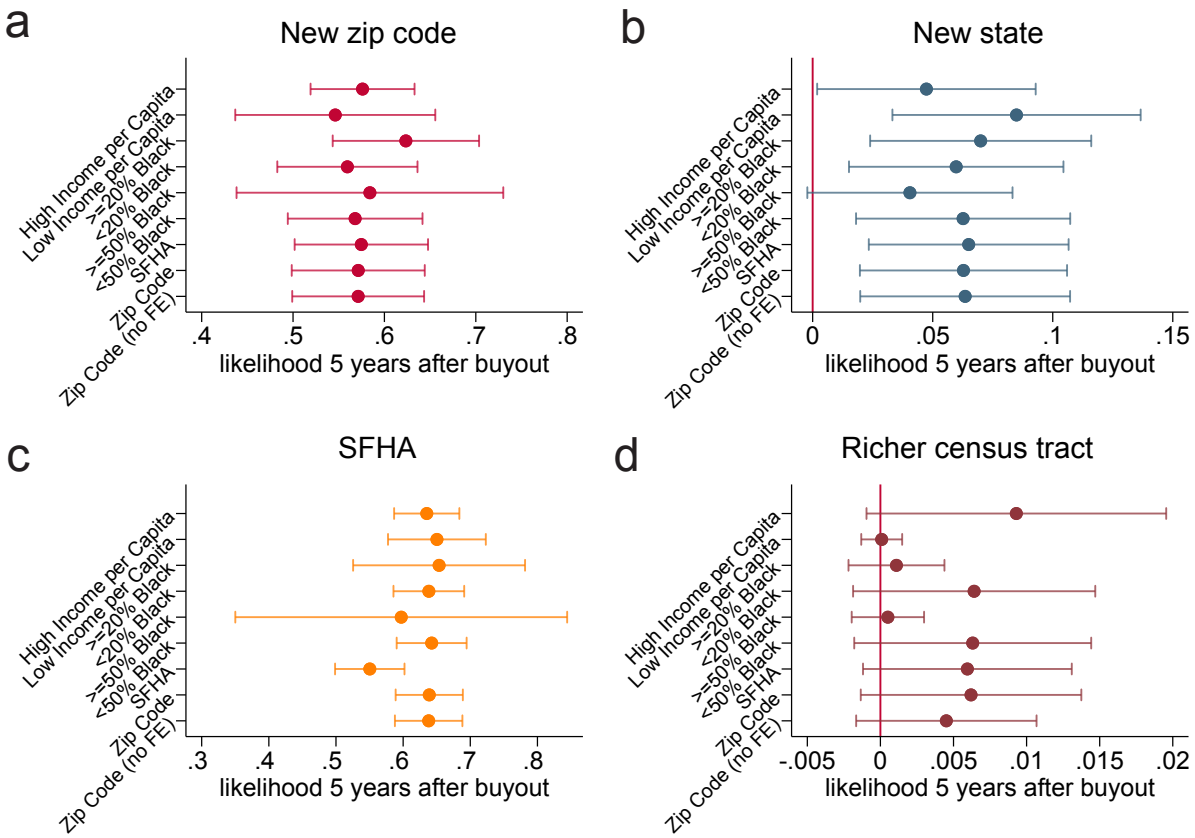


Figure SI8: **Five-year buyout effects among households that relocate.** Estimated cumulative effects five years post-buyout relative to three control definitions. Heterogeneous effects shown for high/low income tracts and high/low fraction Black tracts. Panels: (**a**) new ZIP code; (**b**) new state; (**c**) out of SFHA; (**d**) higher-income tract. Whiskers show 95% confidence intervals.

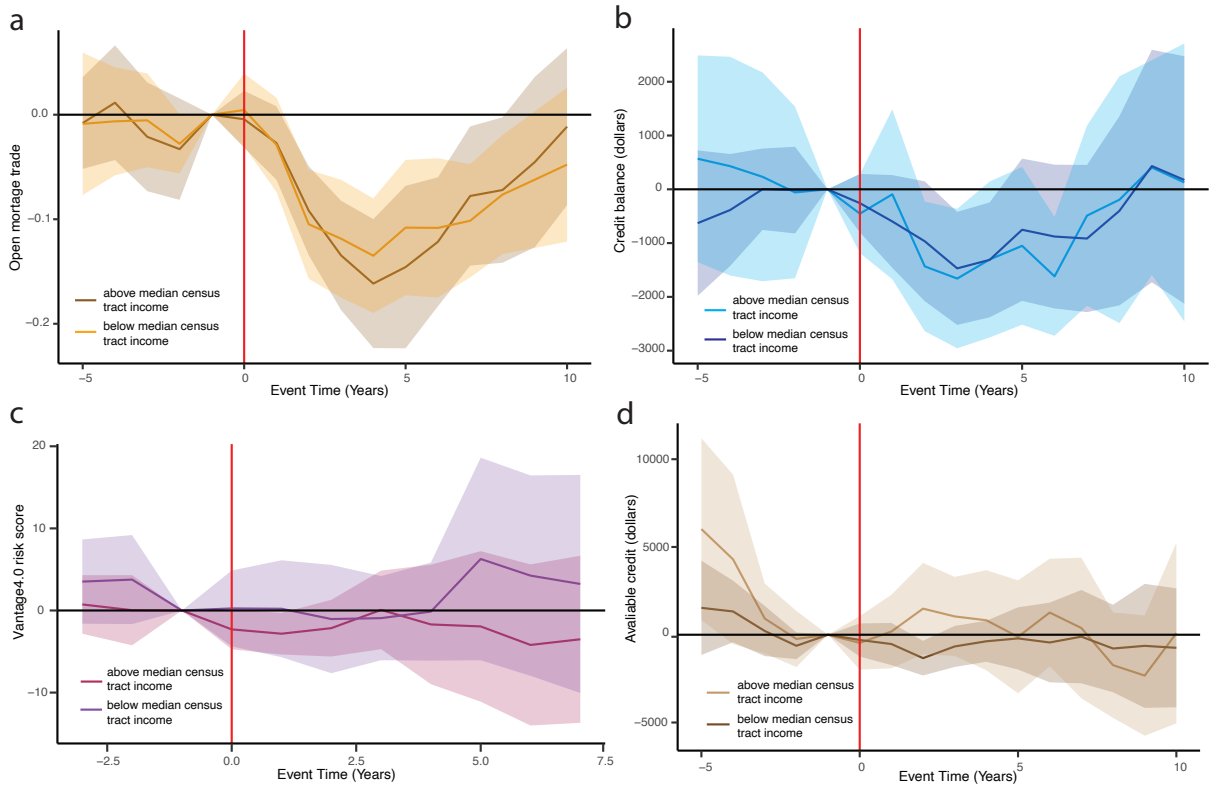


Figure SI9: **Heterogeneous effects of buyouts by census tract characteristics.** Effects separately estimated for high vs. low income tracts and high vs. low fraction-Black tracts. (**a**) Mortgages. (**b**) Credit card balances. (**c**) Vantage 4.0 credit scores. (**d**) Available credit. Shaded areas show 95% confidence intervals.

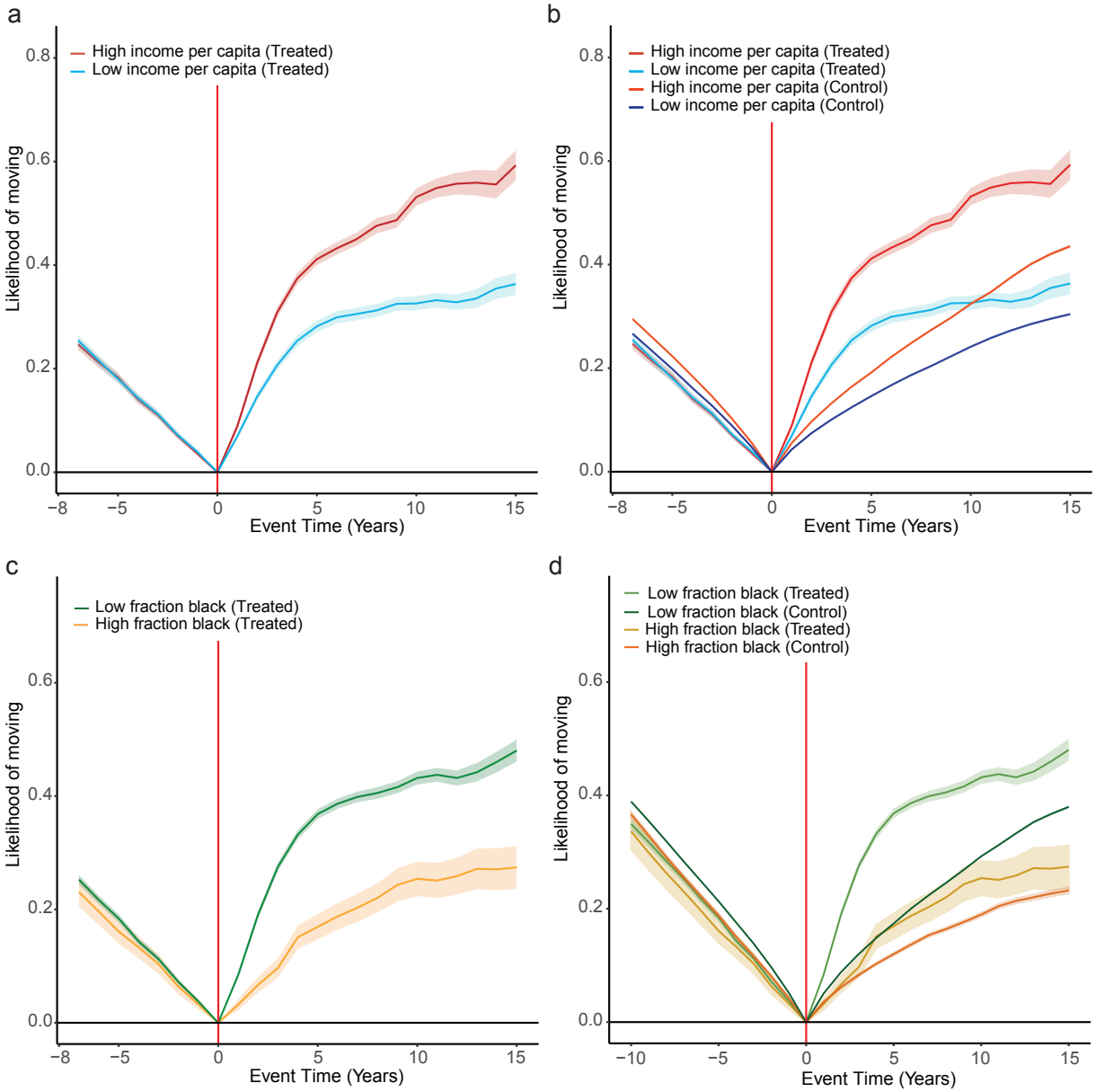


Figure SI10: **Differential effect of the buyout on moving for the treated and the control groups by neighborhood race and income composition**. Cumulative effects of the buyout on moving to a new address for the treated and control groups and 95% C.I. (**a**) Effects for buyout participants living in high (>=median) and low (<median) income per capita census tracts prior to the buyout. (**b**) Estimated moving effect for treated and control individuals living in high and low income census tracts prior to the buyout. (**c**) Effects for the buyout participants ("Treated") living in (pre-buyout) high-fraction (>= 50%) and low-fraction (< 50%) Black population census tracts. (**d**) Effects for the treated and the control individuals living in the high and low-fraction Black population census tracts.

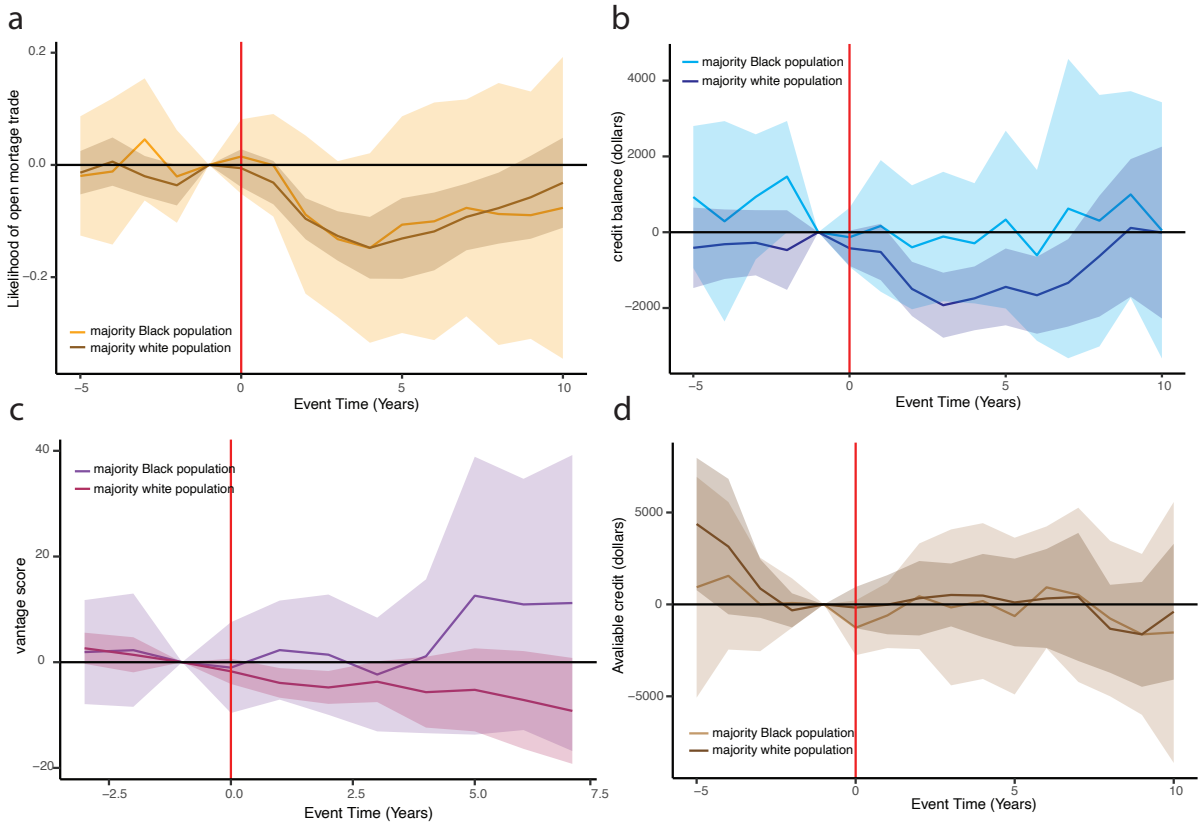


Figure SI11: **Differential effect of the buyout on credit report outcomes by high and low fraction Black population census tracts**. Effects of the buyout on individuals in higher or lower fraction Black population census tracts relative to comparable individuals. Color bands are the 95% C.I. (**a**) Impacts on number of mortgages. (**b**) Impacts on credit card balances (dollars). (**c**) Impacts on Vantage4.0 credit risk scores.(**d**) Impacts on available credit (dollars).

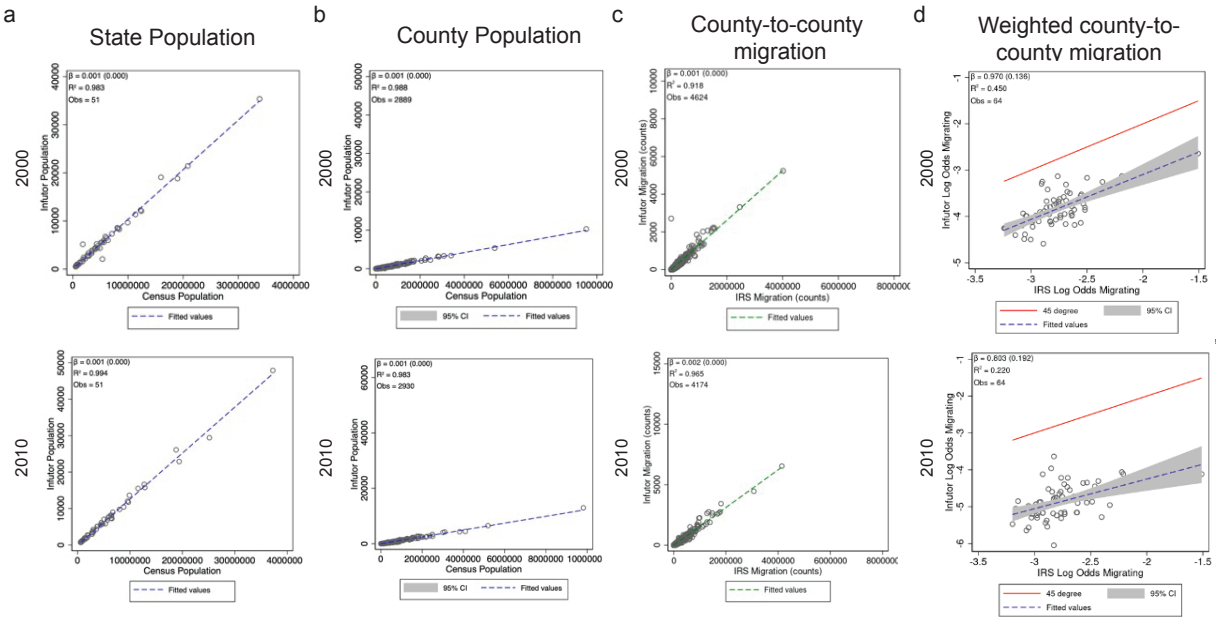


Figure SI12: **Comparison of Infutor and public Census and IRS data**. (**a**) Comparison of Infutor state population (based on a 0.1% random sample) against Decennial Census population in 2000 and 2010 ($N = 51$, $R^2 > 0.98$, $\beta = 0.001$). (**b**) Comparison of Infutor county population counts (based on a 0.01% random sample) against Decennial Census population in 2000 and 2010 ($N = 2930$, $R^2 > 0.98$, $\beta = 0.001$). (**c**) Comparison of Infutor county-to-county out migration (based on a 0.01% random sample) against Internal Revenue Service's county-to-county migration counts in 2000 and 2010 ($N = 4624$, $R^2 > 0.91$, $\beta = 0.001$). (**d**) Population weighted log odds comparison of Infutor county-to-county out migration in Louisiana against Internal Revenue Service's county-to-county migration counts in 2000 ($N = 54$, $R^2 = 0.45$, $\beta = 0.970$) and 2010 ($N = 64$, $R^2 = 0.22$, $\beta = 0.803$) and the 45° line.

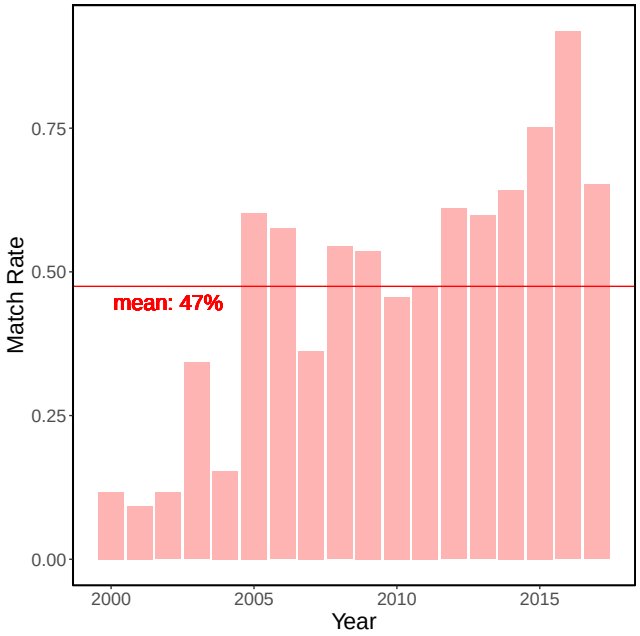


Figure SI13: **Buyout (treated) address match rate over time**. Match rate between the NPR FEMA buyout data and the Infutor Address data, by year of the buyout, from 2000 - 2017 and the mean match rate across all years.

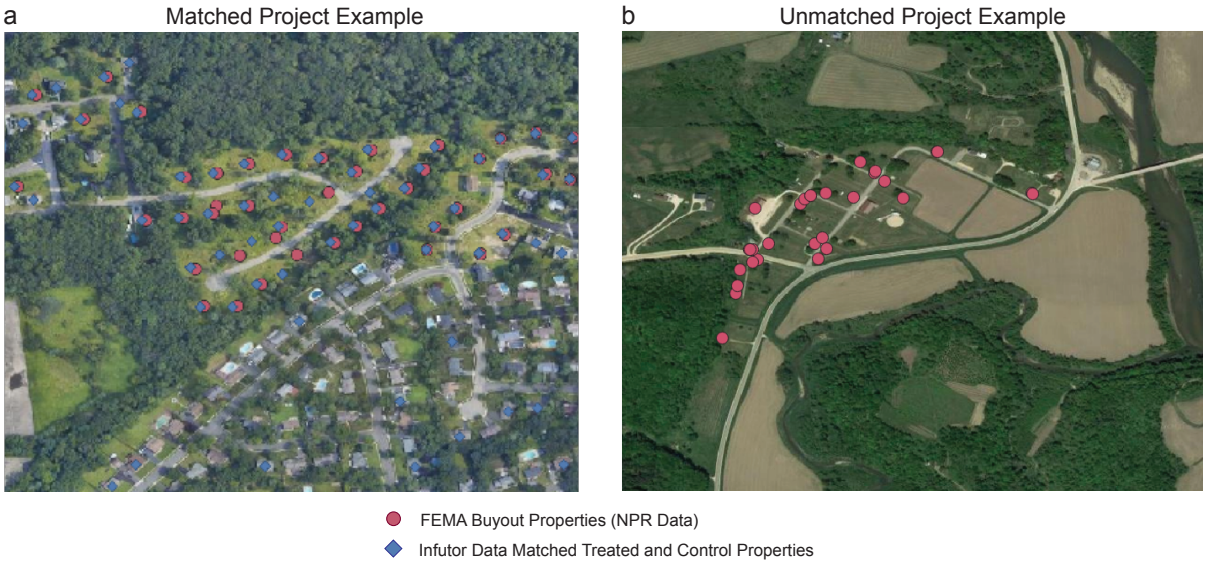


Figure SI14: **Examples of matched and unmatched buyout projects.** (**a**) Example of a buyout project with successful matches between NPR FEMA addresses (red circles) and Infutor addresses (blue diamonds). (**b**) Example of a buyout project with no matched Infutor addresses.



Figure SI15: **Example satellite images of properties marked as buyouts.** (**a**) A property marked as a buyout with evidence of demolition. (**b**) A property marked as a buyout but with a structure still present decades later.

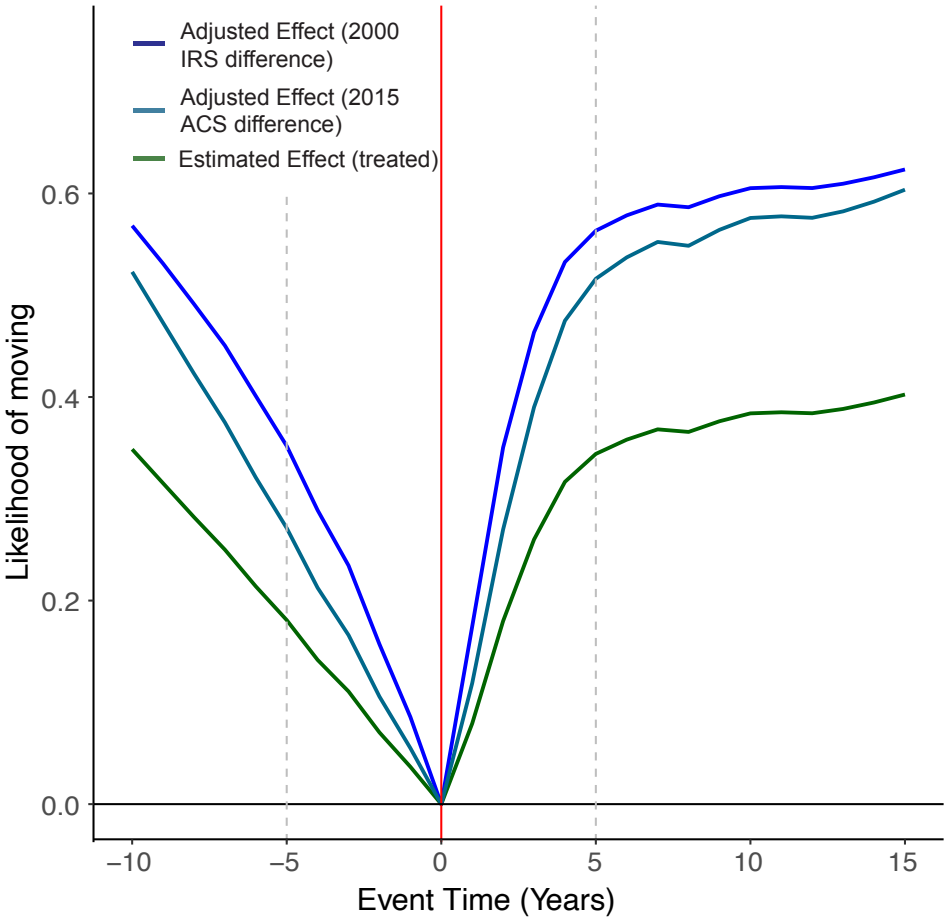


Figure SI16: **Adjusted average likelihood of moving to a new address among buyout participants.** Green line shows the unadjusted likelihood of moving. Light blue line shows the adjustment based on the difference between Infutor move rates and the ACS 2015 "moved within 5 years" rate (Table SISI5). Dark blue line shows the adjustment based on the difference between Infutor move rates and the 2000 IRS county-to-county migration rate.

Program Name	Total Projects (Acquisition and Non-Acquisition)	No. Final Acquisition Properties	Federal Share Obligated	Avg. Cost Share
HMGP	22,732	43,619	$2.71B	74%
FMA	2,978	2,895	$505.86M	83%
PDM	4,534	2,213	$189.52M	75%
Total	**30,488**	**48,738**	**$3.42B**	**76%**

Table SI1: **Total acquisitions and costs across major FEMA buyout programs.** Summary of the three largest FEMA programs that fund property acquisitions: (1) Hazard Mitigation Grant Program (HMGP); (2) Flood Mitigation Assistance (FMA); and (3) Pre-Disaster Mitigation (PDM). The table reports the total number of projects (including both acquisition and non-acquisition activities), the number of finalized property acquisitions ("No. Final Acquisition Properties"), the federal funds obligated ("Federal Share Obligated"), and the average federal cost share for each program.

	Means			Median		
varname	Control	Treated	p-value	Control	Treated	p-value
property flood factor	2.07	2.77	0.00	2.00	3.00	0.00
total population	5,551	5,090	0.00	5,167	4,856	0.00
log total population 2000	8.52	8.45	0.00	8.55	8.49	0.00
fraction white	0.77	0.77	0.03	0.86	0.88	0.00
fraction black	0.16	0.13	0.00	0.05	0.03	0.00
fraction age 16+	0.50	0.51	0.00	0.51	0.51	0.00
family income per capita (dollars)	21,265	21,031	0.00	19,669	19,902	0.19
fraction 16+ and employed	0.51	0.50	0.00	0.51	0.52	0.06
fraction under 18	0.25	0.26	0.00	0.26	0.26	0.00
fraction over 65	0.12	0.12	0.03	0.11	0.12	0.00
fraction with college degree +	0.16	0.14	0.00	0.13	0.13	0.00
fraction with HS and some college	0.45	0.46	0.00	0.46	0.47	0.00
fraction with less than HS	0.04	0.05	0.00	0.03	0.04	0.00

Table SI2: **Balance tests for treated and control groups without any controls.** Means and median of the treated and control groups and the p-values of the difference between the means and medians for a set of observable census-tract level characteristics.

	Buyout zip code				Buyout zip code and SFHA			
Census Tract variables	Control	Treated	p-value	Normalized Difference in Means	Control	Treated	p-value	Normalized Difference in Means
property flood factor	2.04	2.73	0.00	-1.98	2.86	2.66	0.00	0.60
total population 2000	4,539	4,042	0.00	0.19	4,058	3,790	0.01	0.15
log total population 2000	8.33	8.25	0.00	0.16	8.24	8.20	0.12	0.11
fraction black	0.83	0.84	0.78	0.09	0.22	0.21	0.36	0.13
fraction white	0.12	0.11	0.66	-0.02	0.74	0.76	0.32	-0.13
fraction 16+	0.49	0.49	0.76	-0.05	0.49	0.49	0.79	-0.18
family income per capita (dollars)	21,757	21,233	0.26	0.03	21,261	21,390	0.74	-0.24
fraction 16+ and employed	0.48	0.48	0.59	0.03	0.47	0.48	0.43	-0.30
fraction under 18	0.25	0.25	0.44	-0.09	0.27	0.28	0.07	0.02
fraction over 65	0.14	0.15	0.45	0.02	0.13	0.13	0.96	0.00
fraction college degree +	0.14	0.13	0.00	0.14	0.12	0.12	0.48	-0.17
fraction HS and some college	0.47	0.48	0.00	-0.15	0.46	0.47	0.01	-0.05
fraction less than HS	0.05	0.05	0.25	-0.08	0.06	0.05	0.00	0.27

Table SI3: **Balance tests for treated and control groups with county and cohort fixed effects.** Means for treated and control groups and the p-values of their differences are shown for observable census-tract characteristics. Two control groups are reported: (1) individuals living in the buyout ZIP code in the year of the buyout ("Buyout ZIP code"); and (2) individuals in the buyout ZIP code and in the Special Flood Hazard Area ("Buyout ZIP code and SFHA"). The normalized differences in means compare treated and control groups (without county and cohort controls), using the Imbens and Rubin (2016) normalization procedure.

	Year before buyout			Year of buyout		
	Control: Non-SFHA	**Control: SFHA**	**Treated**	**Control: Non-SFHA**	**Control: SFHA**	**Treated**
Total balance conditional on open account (dollars)	\$47,303	\$42,438	\$50,160	\$52,177	\$47,075	\$53,312
	(\$95,263)	(\$87,833)	(\$88,136)	(\$103,589)	(\$95,713)	(\$92,787)
Total balance (dollars)	\$41,186	\$35,202	\$42,896	\$45,295	\$39,141	\$45,395
	(\$90,296)	(\$81,571)	(\$83,394)	(\$98,117)	(\$89,036)	(\$87,694)
Mortage balance (dollars)	\$158,750	\$142,919	\$140,884	\$160,888	\$143,936	\$142,976
	(\$166,356)	(\$174,959)	(\$143,703)	(\$167,611)	(\$161,883)	(\$133,229)
Credit card balance (dollars)	\$12,281	\$10,920	\$11,490	\$12,269	\$10,745	\$10,866
	(\$33,061)	(\$39,128)	(\$24,952)	(\$33,457)	(\$36,621)	(\$23,538)
Vantage4.0 risk score	691	671	689	691	672	684
	(112)	(112)	(111)	(111)	(112)	(111)
Mean number of mortgages	0.53	0.444	0.51	0.527	0.44	0.502
	(0.767)	(0.709)	(0.758)	(0.759)	(0.692)	(0.74)
Percent of sample with a mortgage	40%	33%	38%	40%	34%	38%
Average Number Trades Worst Rating 90 Days Past Due	0.367	0.418	0.344	0.37	0.397	0.37
	(1.313)	(1.403)	(1.236)	(1.319)	(1.322)	(1.309)
n	269,209	16,776	5,975	271,456	16,920	6,014

Table SI4: **Financial baseline sample characteristics**. Means of credit report data for the treated and two control groups, within the same zip code and special flood hazard area ("Control: SFHA"), and just within the same zip code ("Control: Non-SFHA"), the year before the buyout (event time -1) and the year of the buyout (event time 0). Standard errors in parentheses.

	Moved in within 5 years	Moved in within 10 years	Moved in within 15 years
ACS 2015	43%	-	71%
ACS 2010	45%	63%	-
Infutor (Treated)	18%	35%	50%
Infutor (SFHA Control)	18%	34%	50%
Percent Difference between Infutor and ACS	-149%	-79%	-43%
Adjusted Infutor (Treated) IRS 2010 error	62%	80%	88%
Adjusted Infutor (Treated) IRS 2000 error	35%	57%	71%

Table SI5: **Comparing and adjusting estimated Infutor migration rates**. American Community Survey (ACS) 2015 and 2010 measures of move rates—moving to the current address within 5, 10, or 15 years—are compared to pre-buyout move rates derived from the matched Infutor address panel for buyout participants and the control group. Percent differences between ACS and Infutor migration rates are shown. Adjusted Infutor (treated) migration rates are computed using the gap in weighted log-odds of migrating between the Internal Revenue Service (IRS) county-to-county migration data (2010 and 2000) and Infutor migration rates (Figure SI12).

	Mortgage number		Credit Card Debt		Credit Scores	
Year Before	**Movers**	**Non-movers**	**Movers**	**Non-movers**	**Movers**	**Non-movers**
Control: non-SFHA	0.55	0.52	13,182	11,907	687	691
	(0.77)	(0.77)	(34,372)	(32,493)	(109)	(112)
Control: SFHA	0.48	0.74	12,400	10,383	665	671
	(0.43)	(0.70)	(44,856)	(36,816)	(111)	(112)
Treated	0.59	0.82	12,832	10,232	713	673
	(0.82)	(0.69)	(10,231)	(23,917)	(104)	(112)
5 years after						
Control: non-SFHA	0.55	0.52	13,271	12,458	685	681
	(0.78)	(0.75)	(37,027)	(32,252)	(107)	(110)
Control: SFHA	0.47	0.41	12,362	10,459	695	701
	(0.71)	(0.66)	(46,949)	(34,113)	(107)	(110)
Treated	0.51	0.51	11,477	9,441	718	681
	(0.64)	(0.71)	(23,651)	(22,690)	(94)	(108)

Table SI6: **Means of credit report values across movers and non-movers**. Means of credit report data for the treated ("Treated") and two control groups, within the same zip code and special flood hazard area ("Control: SFHA"), and just within the same zip code ("Control: Non-SFHA"), split across those that moved and did not move after the year of the buyout. Standard errors in parentheses.

	m1	m2	m3	m4
treated	-0.016	-0.011	-0.011	-0.007
	(0.004)	(0.004)	(0.004)	(0.005)
5 years after buyout	0.116	0.114	0.114	0.112
	(0.003)	(0.003)	(0.003)	(0.003)
treated * 5 years after buyout	0.178	0.179	0.177	0.175
	(0.013)	(0.013)	(0.013)	(0.013)
cohort FE	no	yes	yes	yes
state FE	no	yes	yes	yes
state cohort interaction	no	no	yes	yes
county FE	no	no	no	yes
N	1,382,226	1,382,226	1,356,244	1,356,244

Table SI7: **Comparison of the main results across four versions of the model.** Results from four model specifications with different sets of fixed effects, all estimated using the control population living in the Special Flood Hazard Area (SFHA). Standard errors are shown in parentheses below point estimates. The table reports the key coefficients of interest: the effect of being treated ("Treated"), the effect five years after the buyout for the control group ("5 years after the buyout"), and the estimated main effect of the buyout program five years after the buyout relative to the control group ("Treated × 5 years after the buyout").