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Managed Retreat and Flood Recovery: The Local Economic Impacts of a Buyout and Acquisition Program

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Abstract

Numerous coastal communities are grappling with the challenge of adapting to escalating disaster risks while maintaining a robust local economy. We study a major buyout and acquisition program in New York State following Hurricane Sandy and evaluate its impacts on a variety of property- and neighborhood-level outcomes. We find that the program generally increases nearby property values on average and also improves business performance and urban amenities in the broader neighborhood. These neighborhoods also attract higher-income property buyers. Compared to buyouts, home acquisitions—which facilitate resilient redevelopment of government-acquired properties—predominantly drive the positive economic effects. Our research design accounts for the confounding effects of Hurricane Sandy's destruction. By providing some of the first estimates on the general equilibrium effects of buyout and acquisition programs, our findings offer a more holistic perspective on their role in shaping the socioeconomic landscape of communities.

*We thank Margaret Walls for extensive comments and access to the NETS database and participants at NAREA 2023 Summer Conference and UAA 2023 Economic Workshop for helpful comments.

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1 Introduction

Billion-dollar disaster events have been on the rise in the United States in the last four decades due to a combination of climate change and increased exposure of population and assets (NOAA National Centers for Environmental Information, 2023). This trend is particularly pronounced in coastal areas, where continued population growth and robust economic development have left more people and properties exposed to hurricanes with more intense winds, storm surge, and tidal flooding. To curb the rapidly escalating trend of disaster damage, coastal communities must consider ways to reduce exposure through retreating from risky locations or making exposed assets more resilient to disaster losses.

In reality, many risky areas continue to see strong development, and adaptation has been limited (Bakkensen and Mendelsohn, 2016). On sunny days, a host of incentives and cognitive effects tend to render people reluctant to invest in risk mitigation measures or move to avoid potential disaster exposure.[1] When a disaster strikes, however, it opens up a window of opportunity for adaptation, as the risk becomes much more salient.

Buyout and acquisition programs are a prominent class of policies to facilitate post-disaster adaptation activities. Often funded by federal dollars, these programs purchase private properties following a disaster and enable homeowners who are unwilling or unable to rebuild or relocate. The terms "buyout" and "acquisition" are sometimes used interchangeably, yet they are distinct in their land use objectives and likely have different effects on the local communities (Siders, 2013). Acquisitions typically allow the parcels to be auctioned for redevelopment and therefore help maintain the community's housing stock and local tax base, whereas buyouts are intended to permanently remove the built structures in hazard-prone areas, thus enabling the creation of public space and natural flood buffers (Siders,

[1] Studies have suggested that households do not fully bear flood risks, due to subsidized flood insurance and implicit risk transfer through the mortgage system (Ouazad and Kahn, 2022; Gourevitch et al., 2023; Liao and Mulder, 2021). Others have shown that the effects of disasters on insurance take-up and housing values decay over time, consistent with biases in the cognitive process (Hallstrom and Smith, 2005; Gallagher, 2014; McCoy and Walsh, 2018). Consequently, households typically do not take full measures in response to the risk.

2013). These programs often serve dual purposes: they are intended to mitigate future disaster damage, by either reducing human exposure or enabling retrofitting for resilience enhancement, and help to promote disaster recovery by allowing affected homeowners to relocate to safer places and the affected communities to upgrade and reorganize their housing stocks.

Despite the growing interest in buyouts and acquisitions, little empirical evidence is available on how these programs affect local communities and what role they play in facilitating recovery and adaptation after disasters. Although both buyouts and acquisitions can lower future damage, they act through different mechanisms and might lead to distinct socioeconomic changes in the participating communities and neighborhoods. For example, buyouts can create more open space that improves local environmental amenities and buffers against flooding, but they can also signal a loss of community similar to foreclosure (Lin et al., 2009), trigger local resistance due to post-buyout relocation, and result in negative impacts on the local economy and tax base (Miao and Davlasheridze, 2022). Acquisitions, on the other hand, help retain local housing stocks, bring in new residents, and create a positive signal for resilience improvement, but they do not improve natural amenities or reduce the population and housing exposure to future disasters. As with any place-based policy, the net spillover effects of buyouts and acquisitions will be capitalized into the value of nearby properties; these changes in local amenities and real estate markets further entail a general equilibrium effect on the socioeconomic characteristics of local residents and the local business environment. As the direction of these local economic effects is theoretically ambiguous, it is crucial to conduct a comprehensive empirical evaluation across multiple key dimensions.

We focus on a major managed retreat and recovery effort, the NY Rising Buyout and Acquisition Program ("Program") and empirically examine its impact on local housing markets, demographics, and business establishments. Primarily funded by HUD through the Community Development Block Grant-Disaster Recovery (CDBG-DR) program and administered by the state government following Hurricane Sandy, the Program presents a particularly interesting case because it involves both acquisition and buyout policy actions. Specifically, we evaluate a rich set of outcomes reflecting property- and neighborhood-level changes in three main categories: (i) nearby property values; (ii) sociodemographic characteristics of new home buyers in the neighborhood; and (iii) nearby business survival and growth. To understand the Program's role in the local economic recovery process, we select this set of outcomes in parallel to those in the literature on Sandy's localized economic impacts (Ortega and Taşpınar, 2018; Gibson and Mullins, 2020; Ellen and Meltzer, 2024; Meltzer et al., 2021).

To investigate the causal treatment effect of the Program, we employ a difference-in-differences (DiD) design that exploits both temporal and spatial variations in the implementation of home buyouts or acquisitions. Our empirical strategy recognizes that Program participation is not random and recovery is heterogeneous across neighborhoods. To account for program participation driven by Hurricane Sandy's impacts, we limit our sample to areas exposed to its inundation or damage, so that we can compare the treatment areas to areas that are potentially eligible for the Program but did not get treated ultimately. In addition, our regression models control for an extensive set of damage measures. We also explore differences in the likelihood of participation across areas with different sociodemographic characteristics, which then inform our empirical model. Therefore, our estimates capture the differences in market dynamics in the post-Sandy recovery process between neighborhoods that are "treated" by nearby buyouts or acquisitions and "untreated" neighborhoods that have similar Sandy impacts and characteristics but are not close to any participating properties. In addition, we distinguish between the treatment effect of buyouts and acquisitions and how their effects vary with the intensity of Program participation.

Our primary findings are threefold. First, acquisitions have a significant and positive effect on nearby property values, with the impact increasing for properties located near more acquired homes and decreasing with distance from the acquired home. Over time, the effect of acquisitions persists and even strengthens. In comparison, the effect of buyouts is mostly insignificant and turns negative in areas of lower participation intensity. We also show that acquisitions and buyouts both increase the number of mortgage applications in their neighborhoods. In particular, acquisitions induce a greater number of home improvement loans.

Second, we find that mortgage applicants have a higher average income and a higher share of racial minorities following nearby acquisitions. Such effects are more pronounced in neighborhoods with more than five participating properties and remain relatively small and statistically insignificant in places with lower participation intensity. The effects of buyouts, while going in the same direction, are more muted and imprecisely estimated.

Last, our analysis of local businesses shows that acquisitions and buyouts also support nearby business environment. The Program increased the overall business growth rate in neighborhoods with participating properties compared to areas without, primarily by lowering business death rates. Our results also indicate that neighborhoods near buyouts and acquisitions saw a larger increase in the number of business establishments and job creation. Broken down by industry, acquisitions have a positive effect on service businesses and eating and drinking establishments, indicating an increase in nearby urban amenities; buyouts are less effective in boosting these industries. Acquisitions also lead to more construction businesses than buyouts do.

Overall, our findings suggest that the Program has yielded positive spillover effects by boosting property values and business performance in neighborhoods with participating properties. These changes are accompanied by an increase in higher-income and racial-minority home buyers. These spillover effects are largely driven by acquisitions, possibly because these properties are redeveloped for resilience improvement, which can contribute to a community's attractiveness to buyers and also create a coordination effect motivating more recovery and improvement activities in nearby homes. This finding is consistent with Fu and Gregory (2019), who find positive spillover effects from a household's rebuilding decision on its neighbors in the aftermath of Hurricane Katrina. Issler et al. (2020) similarly show the coordination externalities of rebuilding homes to a higher standard following major wildfires in California.

To provide an overall assessment, we conduct a back-of-the-envelope calculation of the cost-effectiveness of the Program. The results suggest that the acquisitions have yielded \$3.05 billion and buyouts \$0.67 billion in benefits stemming from enhanced resilience, higher property values, and job creation. Notably, these gains are largely attributable to the positive spillover effects on the local economy, underscoring the importance of considering the indirect economic consequences. The estimated benefit-cost ratio of the overall Program is 5.8, indicating its cost-effectiveness in boosting disaster recovery and enhancing resilience.

Our study contributes to the broad economics literature of natural disaster and climate adaptation. Existing research has primarily focused on the benefit and cost analysis of climate adaptation measures in non-disaster contexts (Agarwal et al., 2025; Ward et al., 2017; Fried, 2022; Hsiao, 2023; Benetton et al., 2023), with less attention given to adaptation measures in response to disaster, such as government-funded buyouts or acquisitions. In reality, the bulk of the adaptation actions are reactive and induced by disaster events (Miao and Popp, 2014). However, compared to non-disaster adaptation activities, it is particularly challenging to empirically separate the effect of disaster-induced adaptation from the direct impacts from disasters. Our study presents a framework that illustrates the selection mechanisms of disaster-triggered adaptation programs, thereby allowing us to identify the policy impacts while isolating the first-order disaster impacts. In this regard, our work further extends the existing research examining the multifaceted impacts of disasters, including their effects on housing values (Hallstrom and Smith, 2005; Atreya and Ferreira, 2015; Bakkensen and Barrage, 2017; Bin and Polasky, 2004; Bin et al., 2008; Zhang, 2016), migration and locational choices (Boustan et al., 2012; Kocornik-Mina et al., 2020), economic activities (Boustan et al., 2020; Hsu et al., 2018), and social well-being outcomes (Luechinger and Raschky, 2009; Rehdanz et al., 2015; Baryshnikova and Pham, 2019).[2] These studies typically estimate the overall impacts of disasters without separating out post-disaster policy

[2] For reviews of this literature, see Botzen et al. (2019); Kousky (2014).

responses, such as disaster assistance or adaptation actions. An emerging literature finds that the former (federal disaster assistance) mitigates adverse disaster impacts.[3] Our paper connects with this literature in providing a robust evaluation of another type of post-disaster response–adaptation policies aiming to enhance the resilience of communities.

This paper also connects to the literature that examines neighborhood effects from environmental quality improvements or reduction of environmental risks (Chay and Greenstone, 2005; Greenstone and Gallagher, 2008; Bayer et al., 2009; Zivin and Neidell, 2009; Neidell, 2009; Currie et al., 2015; Deschenes et al., 2017; Keiser and Shapiro, 2019; Ito and Zhang, 2020). Our analysis extends this literature by examining neighborhood spillover dynamics of climate adaptation actions that reduce exposure to future disasters. In particular, our findings pertaining to acquisitions–which enhance structural resilience of existing properties–add to the recent literature on the effects of resilient building standards (Baylis and Boomhower, 2021; Wagner, 2011; Bakkensen and Blair, 2022). Our findings on buyouts provide further insight into the willingness-to-pay for access to open space and natural infrastructure (Irwin, 2002; Klaiber and Phaneuf, 2010), particularly in the context of flood hazard protection (Bin et al., 2008; Beltrán et al., 2018; Sun and Carson, 2020; Taylor and Druckenmiller, 2022; Druckenmiller et al., 2024). As damages from hurricanes and flooding continue to escalate rapidly, flood resilience, involving both structural protection and non-structural protection, will play an increasingly crucial role in shaping both individual location choices and broader housing market dynamics.

More specifically, this paper contributes to the literature on managed retreat and government buyouts as one of the first to systematically assess the local economic impacts of buyout and acquisition programs in the context of post-disaster recovery. While buyouts and acquisitions can offer both benefits and harms to participants and communities (Siders, 2022;

[3] For instance, Davlasheridze and Geylani (2017) find that low-interest disaster loans provided by the Small Business Administration mitigated adverse disaster impacts on small business survivals. Gallagher et al. (2023) show that cash assistance provided to residents through the Federal Emergency Management Agency's (FEMA) Individual Assistance program can effectively alleviate the adverse effects of tornadoes on local businesses.

McNamara et al., 2018), quantitative evaluations of their socioeconomic impacts remain limited. Existing studies have examined post-buyout impacts on participating households (McGhee et al., 2020; Koslov et al., 2021), local government finance (BenDor et al., 2020; Miao et al., 2024), and land use and migration patterns (Zavar and Hagelman III, 2016; Martin and Nguyen, 2021).[4] Several recent studies have also explored the effect of buyouts on property values but yielded mixed findings. Martin and Nguyen (2021) find that post-hurricane buyouts in the 1990s in North Carolina led to declining long-term housing growth and White flight, while in Mecklenburg County, NC, Holloway and BenDor (2023) show that buyouts cause a slight increase in the values of nearby single-family residential properties. Hashida and Dundas (2023) also study the Program in NY state, and find that it caused a decrease in values of home adjacent to buyouts or acquisitions, which are qualitatively different from ours. As we show through extensive replication, the main difference lies in our research design which accounts for non-random participation in the Program with additional controls for the confounding, differential impact of Hurricane Sandy. This paper thus contributes to the literature by demonstrating the importance of controlling for the impacts of the triggering event, thereby providing a clearer understanding of the mixed findings. Our analysis also goes beyond housing prices and combines a more comprehensive set of community outcomes, including new resident demographics and business performance. Therefore, this paper provides a more holistic perspective on the Program's role in shaping the community socioeconomic landscape.

The rest of the paper proceeds as follows. Section 2 provides policy background and details of Hurricane Sandy and the NY Rising Program. Section 3 describes the data. Section 4 discusses empirical challenges stemming from endogenous participation. Section 5 presents our empirical approach and primary results, followed by Section 6, which provides

[4] Existing literature has predominantly focused on implementation of home acquisition and buyout, examining aspects such as program designs (Shi et al., 2022; Hino et al., 2017), participants' experiences and drivers of participation decisions (Binder and Greer, 2016; De Vries and Fraser, 2012), equity implications (Siders, 2019, 2022; De Vries and Fraser, 2012; Elliott et al., 2020), and nationwide distribution of buyout projects in relation to community characteristics (Mach et al., 2019; Miao and Davlasheridze, 2022).

an overall benefit-cost analysis. Section 7 concludes.

2 Background

2.1 Hurricane Sandy

Hurricane Sandy was a catastrophic storm that struck the northeastern United States in late October 2012. It caused at least $74 billion in direct damages (in 2020 dollars), triggered major disaster declarations for 12 states and the District of Columbia, and resulted in more than 120 deaths (Painter and Brown, 2017; Government Accountability Office, 2020). It severely affected downstate New York, including New York City, as the storm surge caused unprecedented flooding damages to residential and commercial facilities and transportation and other infrastructure along the coastline. Aerts et al. (2014) estimate that the storm caused $4.2 billion of damage to housing in New York City. Nearly 2 million energy customers lost power, further contributing to the economic disruption (FEMA, 2013). FEMA declared presidential disaster designations for 14 counties in New York state, including all five boroughs of New York City (Henry et al., 2013).

In addition to causing direct economic damages, Hurricane Sandy led to significant, disruptive impacts on local housing markets, business, critical infrastructure systems, and public health and well-being in New York, as documented in recent empirical studies (Ortega and Taşpınar, 2018; Gibson and Mullins, 2020; Meltzer et al., 2021; Comes and Van de Walle, 2014; Barile et al., 2020; Lin et al., 2016). Specifically, Ortega and Taşpınar (2018) estimate that it persistently reduced housing prices by about 9 percent in the flood zones. Ellen and Meltzer (2024) report a similar decline in home values. Their results also suggest heterogeneous price effects depending on income levels and storm-induced changes in the composition of homebuyers.[5] Meltzer et al. (2021) find that Sandy caused a persistent

[5] Gibson and Mullins (2020) and Cohen et al. (2021) find that undamaged properties at risk also experienced value depreciation after Hurricane Sandy.

negative shock to business establishments and sales revenues, with a particularly larger impact concentrated on retail businesses with more localized consumer bases.

2.2 The New York Rising Buyout and Acquisition Program

Soon after Hurricane Sandy, the New York state government established the Program to purchase properties that were substantially damaged or destroyed as part of a larger housing recovery effort that also provides protection against future storms. It was funded primarily by the HUD Community Development Block Grant-Disaster Recovery (CDBG-DR) through the Sandy Supplemental Appropriation. Administered by the NY Governor's Office of Storm Recovery, the Program consists of two components: (1) government buyout of damaged properties and turning the land into a natural coastal buffer (e.g., wetlands, open space, or stormwater management system) and (2) acquisition of selected properties for resilient redevelopment by private entities.[6]

The terms "buyout" and "acquisition" are sometimes used interchangeably, as both involve government purchase of private properties and allow homeowners who are unwilling or unable to rebuild after disasters or to relocate. Yet they are distinct in their land use objectives and likely have different effects on the local communities (Siders, 2013). Acquisitions typically allow the parcels to be auctioned for redevelopment and therefore help maintain the community's housing stock and local tax base, whereas buyouts are intended to permanently remove the built structures in hazard-prone areas, thus enabling the creation of public space and natural flood buffers (Siders, 2013). Both actions can promote community resilience: acquisition requires developers to build structures that meet more stringent flood-resistant standards, and buyouts directly reduce flood exposure by removing structures from harm's way and might also lower the hazard for remaining properties (Siders, 2013).

The Program operated in select neighborhoods, with voluntary participation of eligible homeowners whose one- or two-unit dwellings in disaster-declared counties sustained dam-

[6] For more details about the implementation of the Program, see Siders (2013).

age as a direct result of the storm. Under the buyout component, the state government purchased properties inside the designated Enhanced Buyout Areas (EBAs), which are high-risk locations in the floodplain determined to be among the areas most susceptible to future disasters.[7] Certain properties within the designated floodways (the portion of the floodplain with the greatest flood hazard) were also eligible. These properties were deemed not suitable for rehabilitation and converted into coastal buffer zones (restricted in perpetuity for uses compatible with open space, recreation, or wetlands management practices). The acquisition option was only available for properties with damage exceeding 50 percent of their value located inside 100-year and/or 500-year floodplains. Under this component, properties are eligible for redevelopment with resilience improvement, with their post-purchase disposition to be determined by state and local officials. To motivate voluntary participation in the Program, the state offers 100 percent of the pre-Sandy fair market values and additional incentives of 5–15 percent to property owners in EBAs.

Both components are intended for reducing exposure of properties and assets to flood risk. Buyouts achieve this purpose by directly removing properties and creating natural flood buffers, while acquisitions promote more resilient rebuilding. Specifically, the redevelopment of acquired properties must meet the elevation and floodplain development and design requirements stipulated in the New York State Building Code and any local building code. These requirements meet or exceed federal requirements to elevate substantially improved properties to one foot above base flood elevation. Moreover, all redevelopment should meet the requirement by New York State and local building code for any long-term resiliency or retrofit activities (e.g., the elevation of electrical systems and components above design flood elevation, securing fuel tanks, changing the use of below-design flood elevation spaces, installation of flood vents in basements and crawl spaces in flood zones, and/or installation of sewer backflow valves)(Governor's Office of Storm Recovery et al., 2022).

[7] These areas have a history of flooding and storm damages and often contain multiple contiguous parcels in the floodplain where property owners collectively voiced and documented interest in relocation. For instance, the residents in Oakwood Beach petitioned for a buyout and successfully leveraged connections in the state government to support buyouts (Siders, 2022).

3 Data

For the analysis, we assembled data on Sandy-induced inundation and damage, properties participating in the Program, and socioeconomic outcomes, including housing prices, demographics of population inflows, and business establishments. We describe our data sources and measurements next.

3.1 Housing Acquisitions and Buyouts and Sandy Damage

Program records were obtained through a "Freedom of Information Law" request from the New York State Governor's Office of Storm Recovery. This provides us with information on all participating properties, including key characteristics, such as whether it was an acquisition or buyout, administering municipal agencies, street addresses, purchase prices, and the dates associated with Program actions, such as the closing date. Participating properties totaled 1,289, of which 566 were acquisitions and 723 buyouts. We geocoded all property addresses to obtain their longitude and latitude coordinates.[8] In practice, we find that the two actions have occurred in properties from very similar neighborhoods.[9]

As the Program was directly triggered by Hurricane Sandy and most of the participating properties were damaged by the storm, its damage must be considered. We used geospatial data on structure-level inundation and damage assessments, both of which were provided by FEMA's modeling task force. The former were drawn from field-verified aerial imagery and constructed based on observations from permanent monitoring sites in the USGS network and the NOAA network, calculated as the difference between the observed water level and normal (predicted astronomical) tide level. The latter dataset contains damage estimates for all 147,702 buildings that were either in the Sandy inundation zone or were outside

[8] The geocoding was conducted using the USA Local Composite locator, available through the Business Analyst service of ArcGIS.

[9] Table A2 computes statistical differences in demographic measures between the census tracts that have only acquisitions relative to those with only buyouts. We do not find significant differences in most demographic measures between the two groups.

it but had aerial imagery damage determinations. For inundated or damaged properties, we observe their geo-referenced locations, cause of damage (wind, surge, or both), damage category (affected, minor damage, major damage, or destroyed), and flood depth.[10] An important advantage of the inundation and damage assessment data is that they include affected properties beyond those that applied for assistance. We matched each participating property with the nearest building point within 100 meters for which the damage assessments were determined.

Figure 1 displays the geographic distribution of participating properties and Sandy's inundation zone. Most properties are near or within the inundation zone and clustered along the coastline of Staten Island and Long Island. A small number of buyouts occurred in the inland of Rockland and Orange Counties, outside of storm surge areas.

Panels A and C of Table A1 display the summary statistics of participating properties and demographics of their corresponding census tracts. Acquisition and buyout properties do not differ significantly in their purchase price and participation dates. The racial composition of their census tracts is also similar; however, tracts with acquisitions have slightly higher income levels and higher property values. On average, tracts with acquisitions or buyouts have a larger population, a higher share of White residents and a lower share of Black residents, higher income levels, and lower housing values than areas with no participating properties.

[10] FEMA's Modeling Task Force utilized common methods from US Army Corp of Engineers and FEMA's Hazus program to develop damage estimates for Sandy. These data, combining aerial imagery with observed inundation depths, are considered by FEMA as the best measures of inundation events such as Sandy, which were used in other studies (Ortega and Taşpınar, 2018). The building damage levels are assigned as "affected" when a property's full verified loss is between $0 and $5,000 or water depth is between 0 and 2 ft, "minor damage" when loss is between $5,000 and $17,000 or water depth is between 2-5 ft, "major damage" when loss is greater than $17,000 or water depth is above 5 ft, and "destroyed" when the aerial imagery reveals the majority of the exterior walls collapsed or based on the FEMA Individual Assistance inspector's estimates.

3.2 Housing Transactions

We obtained data on the universe of property transactions in New York State between 2005 and 2020 from CoreLogic (2021 version), one of the most comprehensive datasets of housing transaction records available.[11] The Corelogic data combine housing transaction observations with county tax assessments of individual properties, which allows us to observe the date and sale price for each transaction and property-level structural characteristics, such as the property type, year of construction and renovation, square footage of building area, numbers of bedrooms or bathrooms, and other amenity features incorporated in assessments. Each property or parcel point is geographically identified by its street address, which we geocoded to obtain the exact georeferenced location.

We took the following steps to process these data. First, we limited our sample to one-to-four-family dwellings, including single-family house, town home, duplex, triplex, and quadruplex.[12] We excluded transactions with prices below \$10,000 or above \$5,000,000, which account for 2 percent of all transactions. We also eliminated duplicates derived from repeated reporting, defined as transactions of the same property at the same sale price and date, as one transaction might be recorded multiple times by different agents (buyer, seller, and county assessor). To determine the flooding exposure to Hurricane Sandy for each property, we assigned it with the damage measures of the nearest building point within 500 meters for which FEMA provided a damage assessment.[13]

As described above, the vast majority of participating properties are located in areas inundated or impacted by Sandy. To construct our estimation sample, we need to identify potentially treated areas, some of which might not be ultimately treated but meet the

[11] The CoreLogic database includes comprehensive transaction records dating back to 1995. However, to maintain balanced sample periods before and after Hurricane Sandy (2012), we restrict the analysis to transactions that occurred between 2005 and 2020.

[12] In a robustness check, we show that restricting the analysis to single-family housing generates similar results.

[13] The median distance from a property to a building point with FEMA damage assessment is 57 meters. The 5th and 95th percentiles are 0 meters and 293 meters, respectively. Most properties are matched within 100 meters of the FEMA damage assessment point. Using alternative distance cutoffs at 100m or 200m yields very similar estimates, as discussed later in the robustness checks.

Program's criteria for participation. Therefore, we restricted the sample to census blocks that contain properties that are either located within the inundation zone, classified under FEMA's damage assessments, or within 1,000 meters of any participating properties.[14] This narrows our analysis to 4,032 potentially treated census blocks, with a sample consisting of 352,861 transactions.

However, due to missing key housing characteristics for a substantial portion of properties in Nassau and Suffolk counties, we exclusively on properties with repeat sales between 2005 and 2020, using property-level fixed effects in the regressions to control for time-invariant structural characteristics of each home.[15] This restriction reduces our final sample to 182,708 transactions, with their summary statistics presented in Panel B of Table A1.

3.3 Home Mortgage Applications

To examine the migratory responses in neighborhoods with buyouts and acquisitions, we used home mortgage application data over 2000–2020, obtained from the electronic database of the National Archives. The data contain comprehensive information on every application received by lenders that are required to report under the Home Mortgage Disclosure Act.[16] For each application, the record includes loan amount, mortgage application year, census tract, loan purpose, and whether a loan was approved. The data also contain applicants' socioeconomic characteristics, such as race and ethnicity, gender, and annual income, collected through a voluntary survey during the application.

[14] We define treated properties as those being within 1,000m of a participating property. To determine the threshold, we followed the approach from Muehlenbachs et al. (2015), Dundas (2017) and Hashida and Dundas (2023) and estimate a price function based on distance to the nearest participating property and split the residuals by pre- and post-Program status. Figure A1 suggests that the Program does not have a significant effect on nearby property values beyond 1000 meters. Within 1000 meters, property values increased after the Program was implemented. Given this definition, 94% of the treated properties are located in areas exposed to the storm inundation or damage.

[15] The county assessment records from Nassau and Suffolk counties have limited building characteristics, where key information such as the number of bedrooms and bathrooms are unavailable for most properties.

[16] The criteria for which lenders are subject to the disclosure requirement changed over time. See https://www.consumerfinance.gov/compliance/compliance-resources/mortgage-resources/hmda-reporting-requirements/ for more information.

We limited the mortgage sample to properties within the 16 counties that were either exposed to the storm (had been inundated or received FEMA damage assessments) or contained a participating property. As some application entries in early years were coded by hand, to best avoid potential mistyping and measurement errors, we excluded mortgages below $5,000 and recode annual income levels below $1,000 or above $10,000,000 as missing values. This results in 8,072,856 applications across 3,090 census tracts; 81 tracts contained at least one participating property. As the mortgage data do not indicate property addresses, we aggregated the loan-level data into a panel of census-tract-by-year observations, which allows us to track the demographic patterns of homebuyers in different neighborhoods over time. The summary statistics of the panel are presented in Panel D of Table A1.

A census tract is defined as treated if it contains any acquisition or buyout; there are 81 such census tracts. These census tracts contain a total of 64,239 residential properties that lie within 1,000m of any participating property, or are potentially impacted by the Program. These properties account for 45.5% of the housing units in these census tracts.

3.4 Business Establishments

We obtained data on the universe of business establishments from the National Establishment Time-Series (NETS) database, one of the most comprehensive databases of establishment-level information in the United States. The data cover all industries for 1990–2019, containing detailed information on business characteristics, such as geo-referenced location (longitude and latitude), industry (six-digit SIC code), employment, and estimated sales. The data are updated annually and provide a host of time-series information on business performance, including births, deaths, and employment. We restricted our business samples to within 1,000 meters of the inundation zone to focus on areas that could be potentially treated by the Program.

To capture changes in business outcomes near the buyout or acquisition sites, we aggregated the establishment-level data into a panel of hexagons, each with a 100-meter radius (equivalent to 6.42 acres, similar to a large block), by creating a grid that covers the entire study area and then aggregating the data within each hexagonal cell.[17] This approach enables us to compute multiple measures of business activities for each hexagon in each year, including the number of active establishments, births, deaths, and total employment. We also constructed these measures for select industries categorized by two-digit SIC code, such as construction and services. The summary statistics of the business performance data at the hexagon-year level are presented in Panel E of Table A1.

4 Endogenous Program Participation

An empirical challenge we face in identifying the causal effect of the Program on neighborhood outcomes is that participation in the Program is not random. For instance, a primary driver of participation is Sandy's damage to a property and the surrounding neighborhood. As suggested by the literature and in our data, properties with greater damages are more likely to be bought out. Participation might also correlate with other local characteristics, such as flood risk and demographics. As a result, the "treated" properties, businesses, and neighborhoods (i.e., those located near buyouts and acquisitions) are more likely to have experienced severe impacts of Hurricane Sandy. We take into account these possible confounding factors in our choice of the study area and estimation models. To make sure we are comparing treated entities with others similarly impacted by Sandy, we limited our analysis to areas exposed to the storm inundation or damage. However, unobserved factors might still be driving participation among this set of properties. Next, we discuss the implications of such selection effects for causal identification, empirically test for them, and use the results

[17] Our aggregation approach is similar to Walls and Wibbenmeyer (2023), which used the same data. Compared to aggregating the data by census tracts, this approach keeps the size of the aggregation unit uniform and captures rich spatial variation at a granular level. Another option is to aggregate by census blocks. However, census blocks tend to be in highly irregular shapes in our study area, as they are defined by visible features like roads, streets, streams, and railroad tracks, as well as non-visible boundaries like property lines, and city, township, school district, and county limits. The irregular shapes make it challenging to define any distance-based treatment measure because the underlying spatial relationship can be highly heterogeneous.

to inform our empirical design.

4.1 Selection Effect by Hurricane Sandy's Direct Impacts

As part of the efforts to support recovery from Sandy, the Program was designed to target areas that were most impacted or had a more strenuous recovery. The correlation between Sandy's impacts and the siting of acquisitions and buyouts might negatively bias our estimate of the Program's effects. Figure 2 illustrates this issue. Panel A depicts a scenario in which Program participation is not correlated with the direct impact of Hurricane Sandy. In this case, we can recover the Program effect by simply comparing the differential response between the treatment and control groups relative to their pre-disaster levels. Panel B presents a situation in which properties in more heavily impacted areas are more likely to participate. Ignoring such differential impacts of Hurricane Sandy while making the same comparison would lead to a downward bias in the results, as illustrated by the red arrow, even though the true effect is positive, as indicated by the black arrow. Next, we directly test for these differential impacts based on treatment status using our property transaction data.

We first estimate a cross-sectional regression to examine the correlation between treatment and observable damage from Sandy:

$$\text{Damage}_i = \beta \text{Treat}_i + \alpha_j + \epsilon_i \tag{1}$$

The variable of interest, Treat_i, is a binary indicator of whether a property i is treated, defined as being within 1,000m of a participating property. The outcome Damage_i represents a set of measures of Sandy damage, including the binary indicator of being inundated, inundation depth, and FEMA's categorical damage assessments. The regression also includes census tract fixed effects (α_j). As shown in Columns (1)–(4) of Table 1, treated properties experienced greater inundation depth, were less likely to have no damage, and were more likely to report either major or minor damage.

Furthermore, we find that merely controlling for these observable damage measures does not fully capture Sandy's differential impact on treated properties. To see this, we use the repeated sale sample to compute the residuals of log property prices after controlling for Sandy-induced damage, and property, quarter of sale year, and year by county fixed effects, to capture the components in property values that cannot be explained by these observables. We then test whether there is still a difference in the residualized prices between the treatment and control groups after the storm:

$$\text{Resid}\ln(\text{Price}_{it}) = \beta\text{Treat}_i \cdot \text{PostSandy}_y + \alpha_i + \alpha_{cy} + \alpha_q + \epsilon_{it} \tag{2}$$

Each observation corresponds to a transaction for property i in county c at time t of quarter q and year y. Treat_i denotes whether a property was in the treatment group, and PostSandy_t is an indicator that denotes whether the transaction happened after the hurricane. The coefficient of interest is β, which is essentially a DiD estimate for comparing the treatment and control properties before and after Sandy, after explicitly accounting for its damage. The results, shown in Column (5) of Table 1, indicate that after controlling for all observable damage variables, properties close to participating properties still saw a larger price drop (4 percent) relative to properties further away.

To sum up, our two tests show that treated areas not only suffer greater damage but also experienced more severe economic consequences and more difficult recovery, even when accounting for the extent of direct damage. In light of our discussion of Figure 2, these tests highlight the importance of properly disentangling Sandy's differential impacts on the treatment group from the Program's effects. In addition, although we investigate this issue using property transaction data, the spatial pattern of Sandy's impacts likely applies more broadly to other outcomes. Therefore, we also incorporate the same considerations into our analysis of other outcomes.

4.2 Selection Effect by Socioeconomic Characteristics

Another empirical challenge is the potential selection bias associated with socioeconomic characteristics. Myriad factors might influence which properties ultimately participate in the Program. For instance, the literature suggests that wealthier and more urban localities are more likely to administer voluntary buyouts (Elliott et al., 2020; Mach et al., 2019), in part because they may have stronger government capacity and more resources to obtain funding and offer buyouts (Kraan et al., 2021). By contrast, minority and low-income populations are disproportionately exposed to disaster risk, more vulnerable to disaster impacts, and less likely to be insured or have the ability to recover (Wing et al., 2022). Given that the Program is voluntary, lower-income households might be more open to accepting a buyout or acquisition offer. In addition, local governments' concerns about the loss of income and property tax revenue can also influence selecting targeted properties based on pricing and demographic characteristics of communities rather than on property damage and recovery needs.

We test whether communities' socioeconomic and demographic characteristics, along with storm damage, influence Program participation by estimating a cross-sectional regression:

$$\mathbf{I}(\text{Program}_j) = \beta_1 \text{Demog}_j + \beta_2 \mathbf{I}(\text{Sandy})_j + \beta_3 \text{Damage}_j + \alpha_c + \epsilon_i \qquad (3)$$

The outcome variable is a binary indicator of whether a census tract j contains at least one participating property (= 1 and 0 otherwise). Demog_j represents a socioeconomic variable of interest. We run a series of regressions to examine, in turn, median household income, median housing values, the share of White or Black residents among the total population as well as those affected by Sandy. $\mathbf{I}(\text{Sandy})_j$ is an indicator of whether census tract j intersects with the inundation zone. Damage_j includes various measures of the storm's damage to residential properties, including the number and percentage of households affected by flooding, number of housing units in each damage category, and average flood depth. The model also includes county fixed effects (α_c) to control for time-invariant, cross-county heterogeneity that might affect participation.

The results are presented in Table 2. None of the economic factors, including the median household income level and median housing value, influence the likelihood of having a participating property in the census tract. However, tracts with a higher share of White residents are more likely to see a buyout or acquisition. In contrast, the racial composition of the affected populations does not appear to be significantly associated with Program participation.[18]

5 Estimation Models and Results

As the Program was a government intervention in response to Hurricane Sandy, our empirical framework extends a standard DiD framework to incorporate both "treatments" by Hurricane Sandy and the Program and also control for both observed storm damage and unobserved factors that correlate with Sandy's differential impacts and recovery paths across neighborhoods. As we examine different outcomes, which entail different units of analysis and level of aggregation, we tailor our empirical models accordingly. We discuss our empirical model and estimation results for each of the outcomes of interest in this section.

[18] For robustness, we also examine other economic and demographic factors, such as population density measured by the number of housing units, average rental price, and average income level of different races. We find that these factors are not significantly correlated with acquisitions and buyouts. We also explore the selection effect from different dimensions, such as Program intensity, measured by the number of participating properties, and the timing of the first buyout or acquisition. We similarly do not find any significant relationship between the economic and demographic variables and these aspects of Program participation. Therefore, conditional on being treated, the intensity and timing of acquisitions and buyouts are not further contingent on economic or demographic features.

5.1 Impact on Property Values

5.1.1 Model

We estimate Equation (4) to examine the effect of home buyouts and acquisitions on nearby property values after Hurricane Sandy.

$$\begin{aligned}\ln(\text{Price}_{it}) =& \beta_1 \text{Treat}_i \cdot \text{PostProg}_{it} + \beta_2 \text{Treat}_i \cdot \text{PostSandy}_t + \beta_3 \text{Damage}_i \cdot \text{PostSandy}_t \\ &+ \alpha_i + \alpha_{cy} + \alpha_q + \epsilon_{it} \end{aligned} \tag{4}$$

Each observation corresponds to a transaction for property i in time t, with the outcome variable being the log of sales price Price_{it}. Treat_i denotes whether a property was in the treatment group (i.e., within 1,000m of a buyout or acquisition).[19] PostProg_{it} is a binary variable coded as 1 if the property was transacted after the first adjacent buyout or acquisition within 1,000m, and 0 otherwise.[20] Note that this variable in our baseline model captures the average program effect regardless of whether the property is near a buyout or an acquisition. In our extension which distinguishes between the effect of buyouts and acquisitions, we include two separate binary variables indicating whether a property was sold after the first adjacent buyout or after the first adjacent acquisition, respectively. For properties beyond 1,000m, we do not assign a treatment date, so this indicator never turns on, but we provide an alternative approach in a robustness check. This reduces the primary interaction term of interest, $\text{Treat}_i \cdot \text{PostProg}_{it}$, to a single indicator PostProg_{it}. The variable PostSandy_{it} is an indicator that denotes whether the transaction happened after the hurricane. To control for storm damages, the model includes an indicator for whether the property was inundated and a set of damage measures that are interacted with the post-Sandy indicator. As shown in Section 4.1, this set of controls does not fully capture the differential impacts of Sandy on treated properties driven by unobserved factors.

[19] We also estimate how the treatment effect changes with distance to understand whether this distance is sufficient for capturing the geographic extent of the effect.

[20] For robustness, we also test alternative adjacency distances, including 800 meters and 1,200 meters.

Therefore, we also include the interaction term $\text{Treat}_i \cdot \text{PostSandy}_t$ to control for Sandy's differential effects on participating properties and control groups and their heterogeneous recovery responses. Additionally, we include property fixed effects, denoted as α_i, to control for time-invariant property characteristics. We include county by year fixed effects, denoted as α_{cy}, to control for the unique trajectory of each community before Sandy and as it recovers. This approach also captures any community-specific factors that may have influenced the selection process. We also include quarter-of-sale-year fixed effect, denoted as α_q, to control for seasonal fluctuations in real estate markets.

As described above, our sample consists of all properties with repeated sales between 2005 and 2020, located in areas impacted by Sandy that potentially meet the criteria of the Program. Among them, the treatment group consists of properties within 1,000m of at least one participating property, and the control group includes all other properties. If a property is close to multiple participating properties, we use the earliest closing date of all nearby buyouts or acquisitions to construct the post-treatment variable. We expect the Program's treatment effects to differ between acquisitions and buyouts and decay with distance for both, which we empirically examine below.

The key identifying assumption in a DiD framework is that the outcome in the treatment and control group should evolve in parallel trends if the treatment had not taken place (Bertrand et al., 2004). An important partial test of this assumption is to examine the pre-treatment trends of the outcome in the two groups. If the pattern shows that the two trends are diverging even prior to treatment, then the parallel trends assumption is unlikely to hold. We conduct this test by estimating an event study model, using a set of event-time indicators to flexibly capture the relative trajectory of the treatment group compared to the control group, both before and after the Program.

5.1.2 Result

Table 3 reports our estimate from various specifications. Our baseline specification in Column (1) indicates that a home acquisition or buyout significantly increases property values within 1,000m by 5.13 percent. Furthermore, we find that the positive price effect increases with Program participation intensity. Column (2) suggests that properties close to more than the median number of participating properties experience a 5.38 percent increase in value, compared to an average increase of 4.94 percent for properties near fewer than the median.[21]

We also examine the impacts of buyout and acquisition separately on nearby property values and find that the positive effect is driven by acquisitions, as indicated in Column (3). In Column (4), we find that properties adjacent to an above-median number of acquisitions within 1,000m (6 acquisitions) experienced a 6.66 percent growth in value, compared to a 3.55 percent increase in locations with fewer acquisitions, consistently suggesting that the price effect increases with program intensity. Conversely, the average effect of buyouts is statistically insignificant in Column (3). Its effect becomes significant and negative with lower participation intensity, as properties close to a below-median number of buyouts within 1,000m (4 buyouts) experienced a 4.69 percent of reduction in value. Standalone buyout properties may provide limited amenity values from additional open space, and may even be poorly maintained, potentially leading to unkempt or neglected areas that generate negative effects. Nonetheless, this effect became insignificant in locations with an above-median number of buyouts.

We conduct a number of robustness tests to validate these findings and report the results in Table A3. First, we exclude properties located more than 4,000m from acquisition and buyout properties from the control group, allowing us to compare treated properties only to their geographically similar counterparts. This reduces the sample size by approximately half. Second, instead of having the post-treatment variable only for the treatment group, we define a "treatment date" for all untreated properties using the date of the first acquisition

[21] The median number of participating properties within a 1,000m radius is 7.

or buyout in the same county, if any, or the average date of all buyouts and acquisitions for the remaining properties.[22] Third, we follow Hashida and Dundas (2023) and focus exclusively on transactions of single-family housing only, which reduces our sample size by approximately one third. Fourth, to assess the sensitivity of our results to the distance used for defining treatment, we conduct two additional tests, re-defining the Program treatment based on the presence of the first buyout or acquisition within the ranges of 800 and 1,200 meters, respectively.[23] Finally, we use two alternative distance cutoffs (100 and 200m meters) to match each property with the nearest FEMA damage assessment. Across all these tests, we show that the significant positive effect of acquisitions on nearby property values remains robust, driving the average Program effect.

We further examine the spatial heterogeneity of the price effect by interacting the treatment indicators in the baseline specification with 50-meter bins defined by the distance from the nearest acquisition or buyout.[24] The coefficients on each distance bin are presented in the top panel of Figure 3, which shows that the average Program effect decays by distance. Within a close range (up to 250 meters), property values increase by up to 10 percent. The effect then exhibits a decaying pattern over distance to about 800m, where the estimate becomes negative and imprecisely estimated.[25] In the middle and bottom panels, we show that the effect of acquisitions is significant and large (up to 20 percent) within a 250-meter range and decays with distance. In contrast, the effects of buyouts are generally negligible, with the only significant and negative impact observed on properties located 200 meters away.

Next, we examine whether the price effects change over time using an event study framework, defining the event as the first acquisition or buyout within a close neighborhood of 300 meters. We normalize the response to zero just before Hurricane Sandy, setting the

[22] Defining post-treatment status for control properties based on their nearest buyout or acquisition also yields similar results.

[23] Hashida and Dundas (2023) uses a 1,200m radius to define the treatment.

[24] We also include the interactions on the post-Sandy indicator, as the specification follows a DiD framework with multiple treatments.

[25] The only exception is the estimate for 850m, which is positive and statistically significant. However, given the lack of a consistent pattern with the adjacent estimates, it likely represents noise.

reference year to three years before the Program.[26] To address the staggered treatment issue, we employ the two-stage estimation framework from Callaway and Sant'Anna (2021). As shown in the top panel of Figure 4, Hurricane Sandy caused an immediate decline in the value of affected properties and those near the Program, which was spatially recovered within one year. The Program has an immediate, positive effect on nearby property values, and the effects do not attenuate but instead continue to strengthen during our study period.[27] Seven years after the Program, nearby properties experienced an average 20 percent growth in value relative to the pre-disaster level, compared to properties in control areas. The other two panels show that the persistent effects were driven by acquisitions (middle panel); the buyout effects on nearby property values were negligible in both the short and long runs (bottom panel).[28] In these regressions, we also estimate the pretrends in the treatment group relative to the control and find that the pretrend is statistically indistinguishable from zero for 10 years. As discussed above, the lack of divergent pretrends supports our key identifying assumption of parallel trends between the treatment and control groups.

Overall, our analysis suggests that acquisitions have a significant and lasting positive effect on nearby property values, with the greatest impact observed in close proximity and gradually decreasing with distance. Furthermore, these effects persist and even strengthen over time. In contrast, the effects of buyouts on nearby property values are much smaller, quickly decay over space, and attenuate over time. These findings suggest that acquisitions, through redevelopment and resilience improvement, can yield significant positive spillover effects by increasing nearby housing values. In comparison, buyouts' effect is largely insignificant and only turns negative for properties near lower participation. This is possibly because

[26] The average closing date of acquisition and buyout programs is June 2016, approximately three years after Hurricane Sandy.

[27] For the recovery effect between Hurricane Sandy and the completion of the first batch of buyouts and acquisitions, in addition to the real estate market rebounding due to housing reconstruction, another possible driver is that the Program might have been publicized or in its initial outreach phase at the time, which might have generated a small effect on the housing market. Such an anticipatory effect might create a small downward bias in our estimates.

[28] The persistent effect of acquisition likely reflects gradual improvement in the neighborhood environment that unfolded over time. In the conclusion, we further discuss this dynamic pattern in conjunction with other results.

buyouts are often seen as a signal of risky locations, which could offset the environmental amenity they create, especially when these amenities are limited in smaller buyout site.

These findings are qualitatively different from Hashida and Dundas (2023), whose estimates suggest negative effects of the Program and especially from acquisitions. Our research design differs from their analysis in both sample construction and model specification. The key differences in our analysis include: (1) our sample excludes properties without any Sandy impact from the control group, and (2) our model accounts for differential impacts from Sandy on the treatment and control groups using a rich set of controls including the interaction term between post-Sandy indicator and treatment group. As discussed in Section 4.1, these choices are informed by theoretical reasoning and empirical evidence. In Appendix B, we disentangle how each of these aspects contributes to the contrasting findings. We show that the models are the key drivers of our respective findings, as each model reproduces the corresponding findings in both our own sample and a replication of the sample in Hashida and Dundas (2023). This exercise highlights the importance of controlling for selection into participation based on disaster impacts in the context of post-disaster recovery programs.

5.2 Changes in Homebuyer Demographics

As buyouts and acquisitions affect nearby property values, these neighborhoods might have accompanying demographic changes. Understanding who is moving to these neighborhoods can provide valuable insights into shifts in property values and neighborhood dynamics. For instance, if new homebuyers after a buyout or acquisition are wealthier, they may further influence property values by instigating changes in perceived neighborhood desirability (Kuminoff et al., 2013; Kahn et al., 2010). In this analysis, we use mortgage application data from HMDA to examine the Program's effects on migration inflows at the census tract level, focusing on the number of mortgage applications and income and racial characteristics of mortgage applicants, to understand the sociodemographic changes in the neighborhoods where buyouts or acquisitions occurred after Sandy. We also investigate home improvement activities by existing homeowners as proxied by mortgages for home improvement and refinance loans in these data.

5.2.1 Estimation Model

We analyze migration responses using the following specification:

$$\begin{aligned}Y_{jt} =& \beta_1 \text{Treat}_j \cdot \text{PostProg}_{jt} + \beta_2 \text{Treat}_j \cdot \text{PostSandy}_t + \beta_3 \text{Damage}_j \cdot \text{PostSandy}_t + \\ & \sum_{\tau \geq 2012} \beta_{4\tau} \text{WhiteShr}_j \cdot \mathbf{I}(y = \tau) + \alpha_c \cdot f(t) + \alpha_j + \alpha_t + \epsilon_{jt} \end{aligned} \tag{5}$$

where Y_{jt} is either the number of applications or homebuyers' demographics for census tract j and year t, aggregated from HMDA loan applications. The treatment indicator PostProg_{jt} denotes whether year t is after the first Program action in the tract, and the indicator PostSandy_t is analogous to the previous specification. Treat_j also denotes the treatment, which is coded as 1 if the census tract j had any participating properties and 0 otherwise.[29] Similar to before, we did not define a treatment timing for tracts without any participating property, reducing the interaction term ($\text{Treat}_j \cdot \text{PostProg}_{jt}$) down to a single indicator PostProg_{jt}. We also include a set of Sandy damage measures Damage_j—including the inundation indicator, number of residential properties in each damage category of FEMA's assessments, and average inundation depth—interacted with PostSandy_t. Drawing upon our discussion in Section 4.2, we explicitly control for the census tract's baseline racial characteristics, as those may be correlated with participation. Thus, we incorporate the 2010 level of the White population share interacted with year indicators for the post-Sandy period, denoted as $\sum_{\tau \geq 2012} \beta_{4\tau} \text{WhiteShr}_j \cdot \mathbf{I}(y = \tau)$. The model also includes the census-tract and year fixed effects and county-specific quadratic time trends.

[29] Similar to before, we also estimate a variant of this equation that includes separate indicators for buyouts and acquisitions to distinguish their effects.

5.2.2 Result

Table 4 presents the estimation results. Panel A focuses on all applications for home purchase. Column (1) shows that census tracts with participating properties have a greater turnover of households: the number of mortgage applications increased by an average of 14 per year, equivalent to approximately 29 percent of the mean. Column (2) shows that these increases are largely driven by acquisitions, whose average effect is three times as large as that of buyouts. Columns (3)–(6) indicate that applicant demographics are skewed toward high-income households, with the average income increasing significantly by 3 percent compared to the control group. We also compare the income levels of mortgage applicants with the county's median level in 2015 and find that the share of above-median-income applicants rose noticeably (by 2.2 pp) in treated census tracts compared to others. Moreover, acquisitions and buyouts appear to have attracted more homebuyers from racial minority groups. Although the treated tracts were dominated by White households, Column (7) shows that they had a 0.5 pp increase in the share of Black mortgage applicants. These demographic changes are all concentrated among census tracts with acquisitions, as indicated by the even-number columns.

Panels B and C report results based on home improvement and refinance loans, respectively. Both loans allow homeowners to tap into their home equity to obtain funds necessary for home repair and improvement. We find that census tracts with participating properties experienced an average addition of 2.5 and 17.5 home improvement and refinance loans, respectively, relative to those without any participating property. Compared to the mean of these outcomes, these effects are equivalent to a 23 percent and a 27 percent increase, respectively. These effects are also concentrated among tracts with acquisitions; the estimated effects of buyouts are small and statistically insignificant. Changes in applicant demographics are generally in the same direction as the home purchase loans but less pronounced and statistically insignificant. The only exception is the share of Black applicants for home improvement loans, which increased by 1.15 pp. Again, this effect is almost entirely driven by acquisitions.

As an extension, we also analyze the treatment effect by Program intensity. We divide the treated communities into high and low intensity based on whether they have at least five participating properties.[30] As shown in Table A4, we find that the Program's effect is more prominent in places with more participating properties.

As coastal census tracts might have different dynamic responses in the recovery from Hurricane Sandy from inland ones, we conduct a robustness check by restricting the sample to 778 census tracts within 5,000m from the coastline. The results are reported in Table A5. Although the sample now only contains a quarter of the census tracts from the full sample, we find similar patterns: a significant increase in the number of loans, an increase in median income and the share of Black applicants especially for census tracts with acquisitions.

5.3 Business Establishment Growth and Job Creation

Our analysis of the Program's effects on business establishments are conducted at the hexagon (with a radius of 100m) by year level. We examine four different outcomes related to establishment performance and employment. Specifically, we measure establishment growth by the growth rate of the number of active establishments, birth rate by the number of new establishments as a share of existing establishments, death rate by the number of deaths as a share of all existing establishments, and job creation by the growth of total employment.

[30] The acquisitions and buyouts are concentrated in a handful of census tracts, causing a rightward skew in the average number of participating properties per tract. Therefore, we use the median level, which is 4, for all treated tracts.

5.3.1 Model

We use the following specification to analyze the Program's effects on various establishment outcomes mentioned above, denoted by Y_{ht}, in hexagon h at the end of year t:

$$\begin{aligned} Y_{ht} =& \beta_1 \text{Treat}_h \cdot \text{PostProg}_{ht} + \beta_2 \text{Treat}_h \cdot \text{PostSandy}_t + \beta_3 \text{Damage}_h \cdot \text{PostSandy}_t + \\ & \sum_{\tau \geq 2012} \beta_{4\tau} \text{WhiteShr}_{j(h)} \cdot \mathbf{I}(y = \tau) + \alpha_c \cdot f(t) + \alpha_h + \alpha_t + \epsilon_{ht} \end{aligned} \tag{6}$$

The treatment indicators PostProg_{ht} and PostSandy_t, and the damage controls, Damage_h, are analogous to the previous specification. Treat_h indicates whether hexagon i was ever treated, defined as within 1,000 m from a participating property.[31] Similar to before, the interaction term $\text{Treat}_h \cdot \text{PostProg}_{ht}$ is identical to a single indicator PostProg_{ht} because we assign no treatment timing to untreated hexagons. In this specification, we also include the 2010 tract-level White population share variable interacted with year indicators for the post-Sandy period, $\sum_{\tau \geq 2012} \beta_{4\tau} \text{WhiteShr}_{j(h)} \cdot \mathbf{I}(y = \tau)$, to control for potential selection into acquisitions or buyouts based on race. Finally, the model also includes a full set of hexagon and year fixed effects and county-specific quadratic trends.

5.3.2 Result

We begin by evaluating the Program's effect on establishments across all industries. The results are presented in Panel A of Table 5. We find that establishments located in close proximity (1,000m) to a participating property were more adversely affected by the storm, as indicated by the coefficients on the interactions between the Program treatment and post-Sandy indicator. The growth of the number of establishments dropped by half relative to the sample mean of 2.3 percent, and the growth of jobs dropped by 6 percentage points, which is close to a 70% decrease with respect to the sample mean. This is consistent with

[31] Again, we also estimate a variant of this equation that includes separate indicators for buyouts and acquisitions to distinguish their effects.

Meltzer et al. (2021), who find that Sandy has a persistent negative impact on commercial activities.

We also find that these negative impacts are alleviated by the Program. Column (1) shows that the Program generates a positive effect on the number of establishments, with treated areas experiencing a 0.92 pp increase in the growth rate of the number of establishments after implementation, offsetting most of the negative storm effects. Columns (2) and (3) further break down this effect in terms of establishment birth and death rates. The results show that this effect is primarily driven by a 0.65 pp reduction in the establishment death rate, with a less significant contribution of a 0.33 pp increase in the establishment birth rate, as shown in Columns (2)–(3). Employment in treated areas also grows faster: Column (4) shows that the treated areas experienced a relative increase of 3.7 pp in the employment growth rate, which is more than 40 percent of the baseline.

In Panel B of Table 5, we estimate the separate effects of acquisitions and buyouts. In Columns (1)–(3), we find that the positive effects on the number of establishments, including the higher birth rate and lower death rate, are predominantly driven by acquisitions. In contrast, the estimated effects of buyouts are negative, small, and statistically insignificant. When we turn to employment, buyouts appear to have a larger positive impact, increasing the employment growth rate by 4.5 pp (50.8 percent of the mean). The estimated effect of acquisitions is an increase of 2.2 pp but statistically insignificant.

We further investigate the heterogeneous responses of business performance in select industries to nearby home acquisitions or buyouts. As previous results show an increase in nearby housing values and homebuyer income in the treated areas, neighborhood amenities might also change, such as the supply of goods and services. To capture urban amenities, we repeat the analysis but focus on establishments in Services (SIC code 70-89) and Eating and Dining Places (58). We also look at Construction (SIC code 15-17), an industry that might be directly impacted by buyouts and acquisitions. These results are presented in Table 6. Panel A shows a similar pattern: acquisitions significantly increase the number of establishments, raising the birth rate and reducing the death rate in service industries. Buyouts also induce a large reduction in service business death rate and a large increase in their employment. Panel B shows that acquisitions have a positive effect on construction business growth through lowering the death rate, but buyouts mainly reduce the birth rate. These results are consistent with the previous finding that acquisitions tend to boost nearby construction activities but buyouts dampen them. In Panel C, we find that acquisitions helped more restaurants, bars, and coffee shops survive, consistent with an influx of higher-income residents, whereas buyouts have small and statistically insignificant effects.

Overall, we find that buyouts and especially acquisitions generate positive impacts on nearby business establishments and urban amenities. This is consistent with the direction of neighborhood changes in our previous findings. These findings also align with those of Gallagher et al. (2023), despite their focus on a different policy intervention (FEMA's Individual Assistance) and disaster scenario (tornadoes). There are several remarkable similarities. First, although neither program directly targets businesses, they have substantial impacts on businesses and employment within affected areas. Second, this impact primarily stems from supporting the survival of existing businesses rather than fostering new businesses. Third, industries reliant on local demand experience the most pronounced effects, as observed in both studies. These common findings highlight that post-disaster assistance directed towards residents can also support local businesses.

6 Back-of-the-Envelope Analysis

To contextualize our findings for policy considerations, we conduct a back-of-the-envelope benefit-cost analysis of the Program. This analysis compares the economic benefits, including direct benefits from damage reduction and indirect ones from property and labor market impacts, against the financial costs of the Program.

In terms of costs, the New York State Action Plan allocated \$637.32 million in total funding for the Program (State of New York, 2023). The direct expenditure on acquisitions was \$211.16 million, which could be substantially recovered through auctions, and the direct cost of buyouts was \$274.86 million.[32] When accounting for the other expenses on buyout-site maintenance and administrative processes proportionally based on the number of participating properties, we estimate that the total cost is \$277.60 million for acquisitions and \$359.72 million for buyouts.[33]

To evaluate the Program's resilience benefits, we use the estimated benefit-cost ratio (BCR) of similar projects funded by FEMA's Hazard Mitigation Grant Program (HMGP) since 2000.[34] The average BCR for HMGP-funded home elevation projects is 2.004, while for buyout projects it stands at 2.066. Applying these figures as proxies for the BCRs of acquisitions and buyouts, respectively, we calculate that resilience benefits amounting to \$556.26 million from acquisitions and \$743.18 million from buyouts. It should be noted that these BCRs are ex-ante estimates, typically more conservative compared to other conventional cost-benefit estimates (e.g., Multihazard Mitigation Council 2005).

In the property market, we focus on 131,549 residential properties within 1,000m of an acquisition or buyout, using the 2013 assessment roll data for residential properties in New York State. Among these properties, 90,085 have a nearby acquisition, with a total market value of \$31.657 billion USD in 2013. The acquisitions increase the values of these properties by a total of \$1.621 billion USD.[35] Similarly, the 11,923 properties treated by a nearby buyout experience a devaluation of 86.7 million USD, and the 29,525 properties treated by both see

[32] We calculate the direct costs by totaling the purchase prices for the acquisition and buyout properties, respectively.

[33] Subtracting the direct expenditures for purchasing the properties, the Program incurred \$151.3 million in other expenses, of which we attribute 43.9% (566/1289) to acquisitions and 56.1% to buyouts.

[34] Data accessed at: https://www.fema.gov/openfema-data-page/hazard-mitigation-assistance-projects-v3. According to FEMA (2023), the benefits of a hazard mitigation project are any future costs or losses that can be avoided by completing a mitigation project, which include but are not limited to avoided physical damage, avoided injury or death, avoided displacement costs or emergency management costs, avoided NFIP administration costs, any reduced or eliminated future need for volunteer labor. They also include social benefits (reduced mental stress, anxiety or lost wages due to disaster-induced displacement) and ecosystem benefits (improved air quality, recreational space).

[35] The average treatment effect coefficient for acquisitions is 0.051, as reported in Table 3: $31.657 \times 0.051 \approx 1.621$.

an appreciation of 194 million USD.[36] In aggregate, the Program increases nearby property values by about \$1.73 billion USD, or 0.26% of the state's total residential property values.

We also quantify the benefits from the creation of jobs due to the Program as informed by the discussion in Bartik (2012). In the labor market, our assessment includes 42,255 active establishments within 1,000m from an acquisition or buyout. These establishments employed 197,260 workers in 2013, with an average annual wage of \$48,086 per worker.[37] Among them, establishments in acquisition-only treatment areas add 3,143 new jobs, increasing total annual wages by \$151.14 million USD.[38] Similarly, the treatment effect is 421 new jobs and additional wages of \$20.23 million USD in buyout-only areas, and 1,608 new jobs and additional wages of \$77.35 million USD in areas treated by both acquisitions and buyouts.[39] Assuming conservatively that these additional jobs last 3 years and applying a discount rate of 7%[40], the present value of the total wage gains is \$698.39 million USD. It should be noted that this calculation does not consider a potential crowd-out of employment elsewhere, the (stigma-adjusted) reduced leisure value or reservation wage of the newly employed, or employer net loss (Bartik, 2012). These considerations might imply that only a fraction of the total wage gain should count towards the job creation benefits of the Program.

To wrap up this section, we summarize the costs and benefits of the Program in Table 7 with the corresponding 95% confidence intervals. Our calculations show that, in addition

[36] Properties treated by buyout only have a total value of \$3.454 billion USD and the estimated treatment effect is -0.025 in Table 3 (with the caveat that this estimate is statistically insignificant): $3.454 \times (-0.0251) \approx -0.0867$. Properties treated by both program actions have a total value of \$7.446 billion USD and we apply the sum of the two estimates here: $7.446 \times (0.0512 - 0.0251) \approx 0.194$.

[37] We calculate the average wage across all treated census tracts using data from the 2013 American Community Survey, by dividing the aggregate annual wages by total number of employed population between 25 and 64.

[38] The baseline number of jobs in these areas is 144,173, and the job growth rate coefficient for buyouts is 0.0218 in Table 5. The treatment effect is given by $144173 \times 0.0218 \approx 3143$.

[39] The baseline number of jobs in these areas is 9,497. The treatment effect is given by $9497 \times 0.0443 \approx 421$.

[40] Both of these assumptions are made to yield a conservative calculation of the job creation benefits of the Program. Regarding job duration, past studies examining the labor market impacts of local economic growth have generally used a time frame of 1-10 years (Bartik, 1991). Our choice of a 3-year duration lies at the lower end of this range. On the discount rate, for 20 years OMB has advised federal agencies to use both 3 percent and 7 percent in their policy analyses through their Circular A-4 guidance, until it was revised down to 2 percent in 2023.

to the direct resilience benefits, acquisitions have generated \$2.5 billion in indirect benefits while buyouts generate slightly negative indirect losses of \$71.2 million. The total economic benefits amount to \$3.05 billion for acquisitions and \$672 million for buyouts, both significantly exceeding their costs. The overall BCR of the Program is 5.8. Particularly for acquisitions, the majority of these benefits are attributable to the positive economic impacts of the Program, which highlights the importance of taking these indirect effects into account. However, it is important to acknowledge the important limitations in these findings. They rely on strong assumptions across various components and may not account for all the indirect impacts of the Program. Therefore, we suggest that readers consider these figures as ballpark estimates.

7 Conclusion

We empirically examine the economic impacts of the NY Rising Buyout and Acquisition Program following Hurricane Sandy on local housing markets and business establishments as they recovered from the storm. We find that the Program contributes to housing market recovery in affected areas, with property values increasing more rapidly near participating properties, and this effect is driven by acquisitions. Both buyouts and acquisitions influence the demographics of new homebuyers, attracting more mortgage applications from higher-income households and racial minorities. Furthermore, we also find that the Program supports local business growth, primarily by aiding establishment survival.

Our results suggest that across all three economic outcomes, acquisitions generated a stronger effect in terms of increasing nearby property values, attracting wealthier households to move in, stimulating more home improvement activities in their neighborhoods, and supporting local business growth. Therefore, the overall positive spillover effect yielded by the Program on nearby properties and businesses is predominantly driven by its acquisition component. This is likely because acquisitions require resilience improvements and upgrades, thereby making the neighborhood more attractive and stimulating further recovery activities through a coordination effect, so they are more effective in catalyzing post-storm recovery. This is also consistent with the dynamic pattern of the treatment effect on property values: as these changes unfold gradually, the effect of acquisition on nearby property values persists and strengthens over time.[41] Nevertheless, our back-of-the-envelope cost-benefit calculation suggests that both acquisitions and buyouts can generate significantly more economic benefits than the costs they incurred, showing strong support for the cost-effectiveness of both.

Overall, our research extends the buyout literature and informs the ongoing debate on managed retreat by providing comprehensive empirical evidence on how these policy tools can shape the socioeconomic landscape of communities when they recover from disaster shocks and adapt to future risks. Our economic analysis also provides important insights on assessing the distributional impacts and justice implication of the retreat programs.

One limitation of our analysis is that we do not observe the resilience features of the properties, such as first floor elevation, fortified roofs and windows. This limits our ability to explicitly examine whether properties near buyouts or acquisitions have become more resilient, and whether the Program's effect on their values differ based on their resilience quality. We believe these questions are important avenues for future research.

Another caveat for interpreting our findings is that buyout and acquisition programs tend to vary in design and implementation at the discretion of the local government body. The Program can be different from buyout programs implemented in other localities and jurisdictions, and our results might not be readily generalizable to other settings. For example, our findings suggest that the Program's role in aiding post-disaster economic recovery makes up the majority of the Program's benefits, which might not be the case for a pre-disaster program. To understand how different features contribute to the overall success and cost-effectiveness of these programs, similar evaluations of other local buyout programs are

[41] Recent urban economics research also suggests an increase in the share of high-income households can lead to a further influx of high-income households and persistent neighborhood change (Kahn et al., 2010; Malone, 2020). Such dynamics might also be at play here.

needed.

Our empirical design—which accounts for disaster damage, selection into the Program, and heterogeneous recovery responses—can serve as a valuable, generalizable framework for evaluating other post-disaster buyout and acquisition programs. More generally, our framework can be applied to examine the effect of mitigation or resilience programs that were directly triggered by a large disaster shock, as in the case of Hurricane Sandy. As mitigation efforts such as floodplain buyouts are strongly motivated by recent disaster experiences or damage (Miao and Davlasheridze, 2022), studies assessing the impacts of such programs should appropriately account for these motivating and potentially confounding factors. Our framework might be less generalizable to situations in which floodplain retreat or buyouts are conducted more regularly (such as in Harris County, Texas), because participation in these cases might be driven more by unobserved administrative factors such as program outreach.

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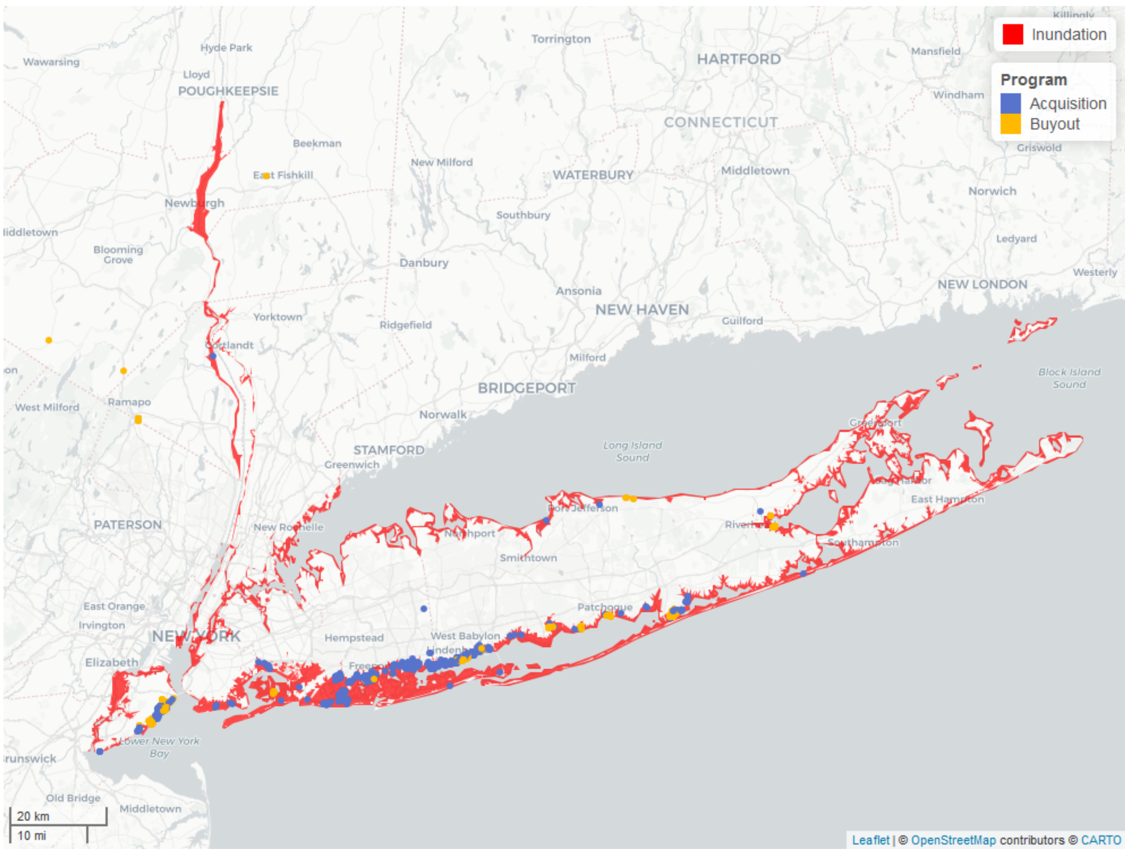
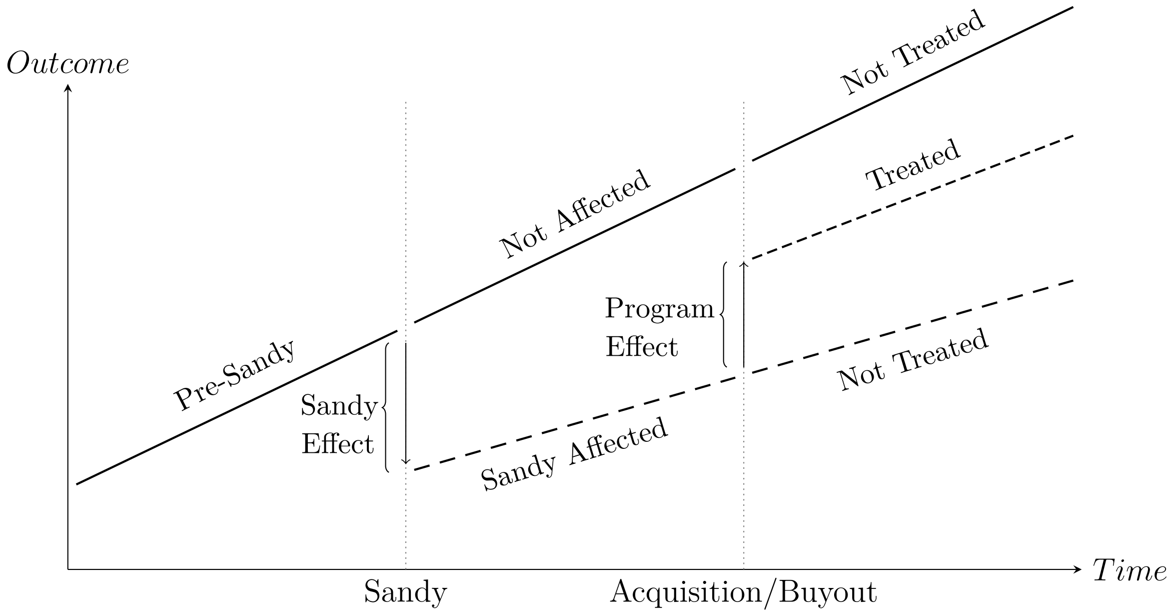
Figure 1: NY Rising Buyout and Acquisition Program and Sandy Inundation Zone



Notes: Red areas display the inundation zone of Hurricane Sandy. Colored points are properties participating in the Program, with blue indicating acquisition and yellow indicating buyout.
Map data © OpenStreetMap contributors, available under the Creative Commons Attribution-ShareAlike 2.0 license (CC BY-SA 2.0). Map rendering by Leaflet.
Details at www.openstreetmap.org/copyright and leafletjs.com.

Figure 2: Omitted Variable Bias from Sandy's Impact

Panel A: No Correlation between Program Participation and Sandy Impact



Panel B: Program Participation Correlated with Sandy Impact

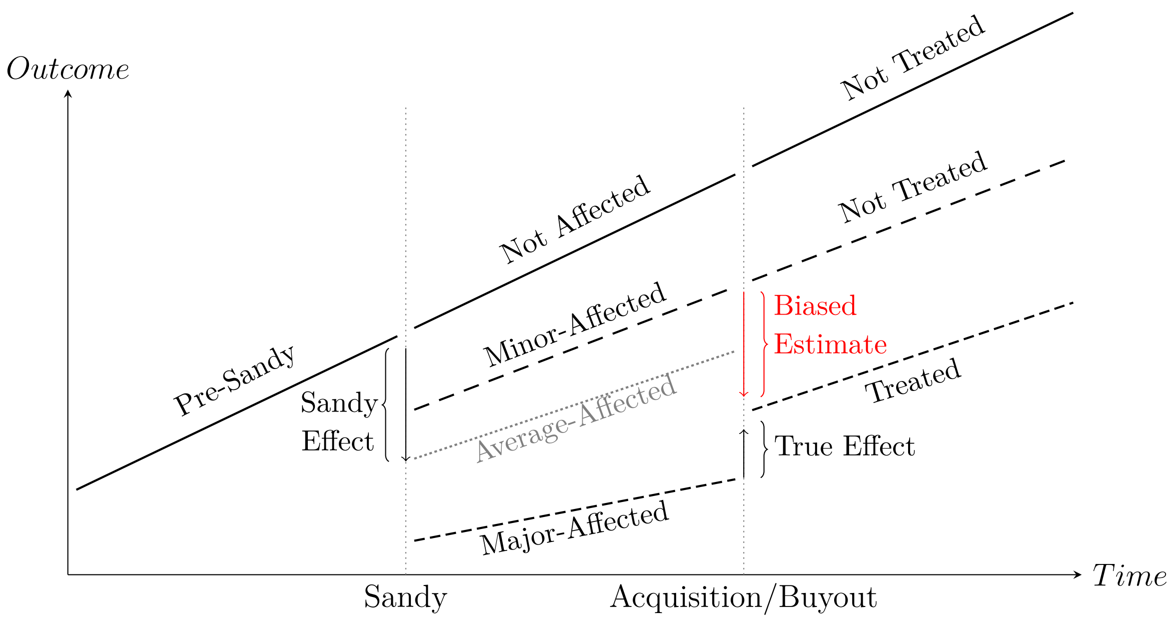
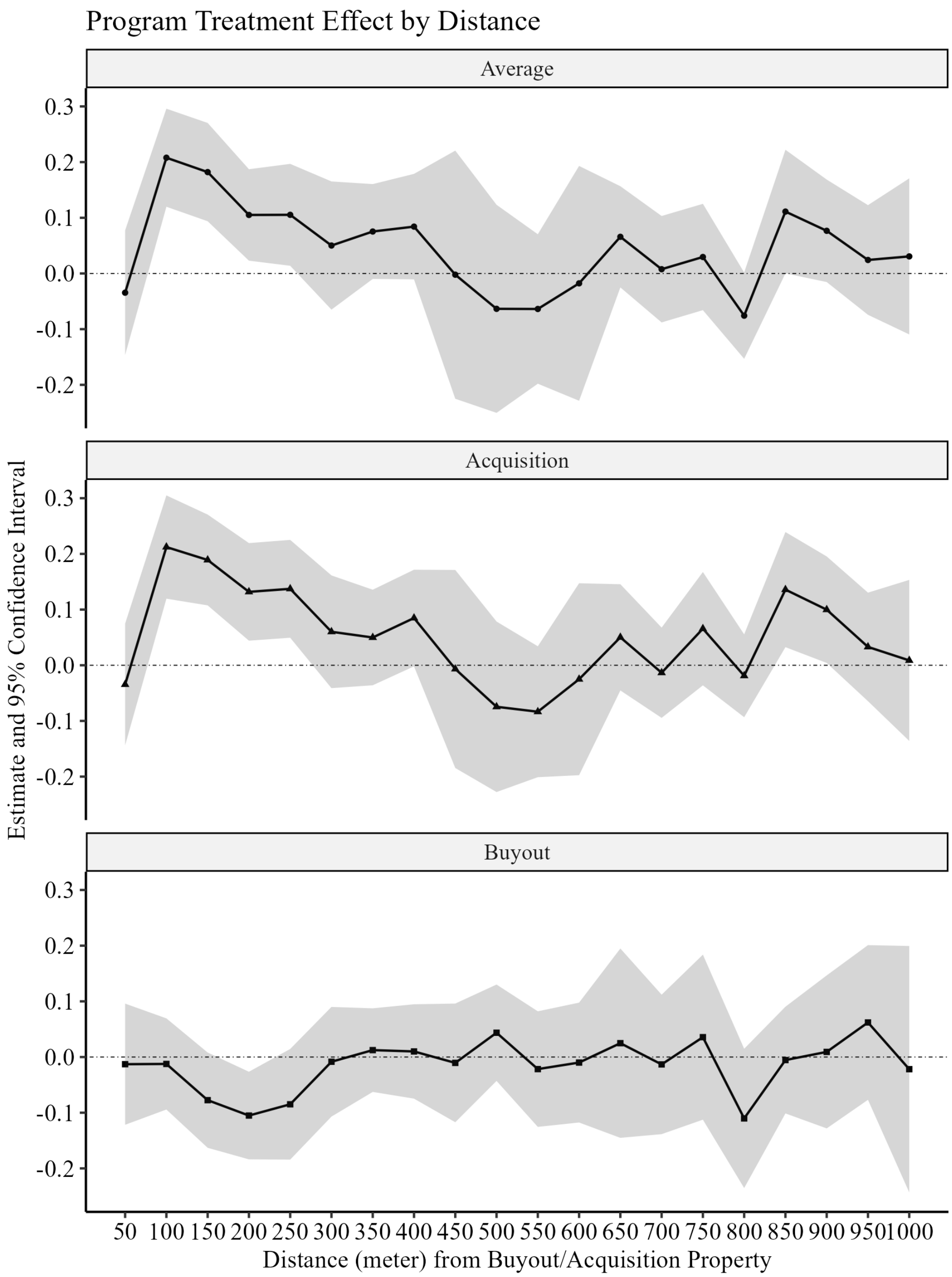
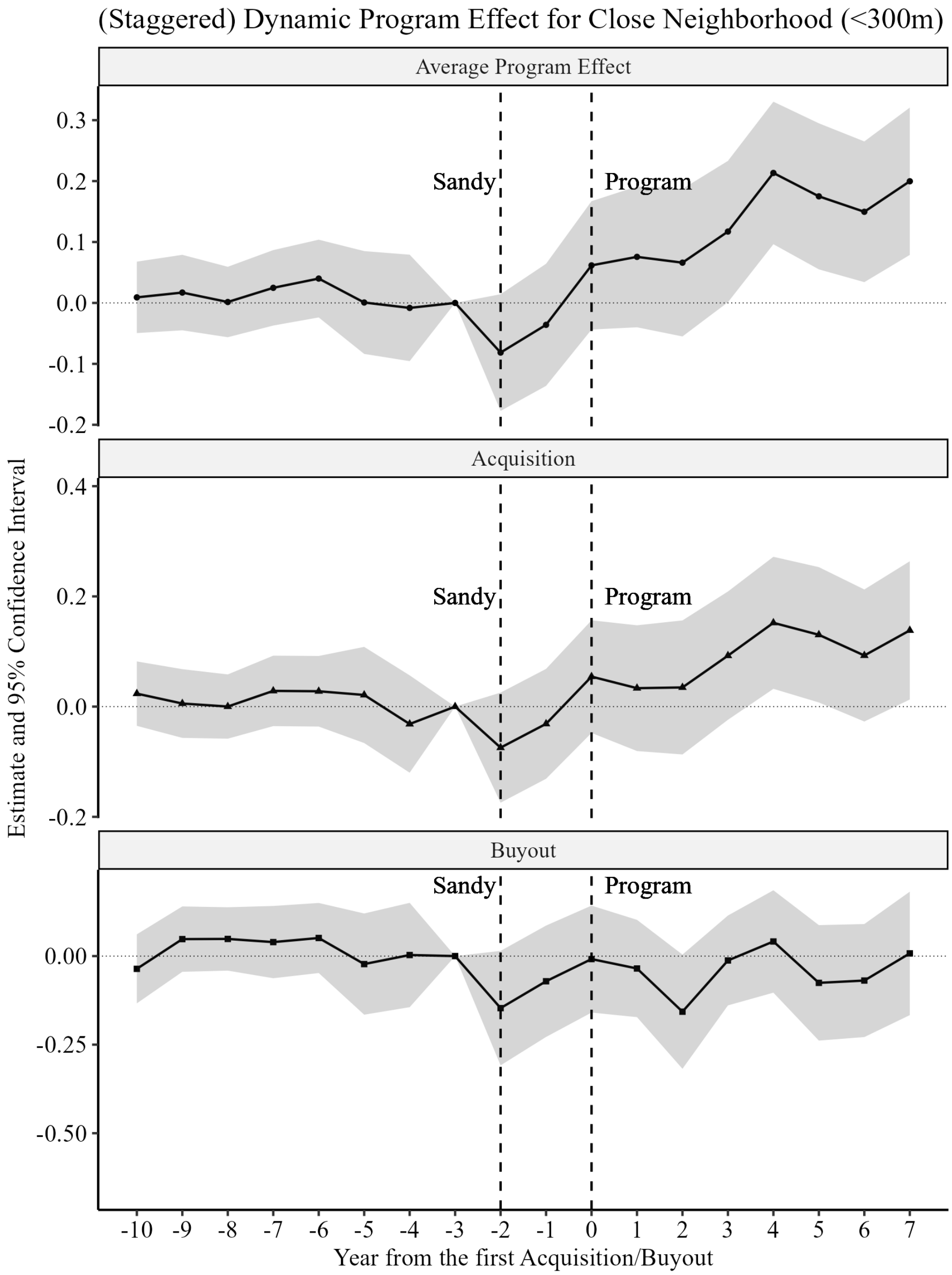


Figure 3: Program Effect on Log Property Value by Distance



Notes: The three panels display the coefficients from a regression on the log of sales price, where the post-Program treatment indicator is interacted with indicators for 50m distance bins from the nearest acquisition or buyout. The rest of the specification parallels Equation (4), with Sandy's impact controlled for by distance bins. The top panel shows the effect of either acquisitions or buyouts, the middle panel focuses on acquisitions only, and the bottom panel focuses on buyouts only. Shades represent 95 percent confidence intervals constructed using two-way standard errors clustered at the census tract level.

Figure 4: Dynamic Program Effect on Log Property Value



Notes: The three panels display the coefficients of an two-stage staggered regression based on Callaway and Sant'Anna (2021), with the event defined as the first home acquisition or buyout within 300m. The top panel shows the average effect of acquisition or buyout, the middle panel focuses on acquisitions only, and the bottom panel focuses on buyouts only. The sample spans from 2005-2020, and the coefficient for year -3 (one year before Sandy on average) is normalized to 0. Shades represent 95 percent confidence intervals constructed using standard errors clustered at the census tract level.

Table 1.
Program Participation and Sandy's Impact

	Inun Depth (1)	No Damage (2)	Minor Damage (3)	Major Damage (4)	Resid Log(P) (5)
Treat	0.238***	−0.106***	0.0479***	0.0123**	
	(0.0624)	(0.0282)	(0.0177)	(0.00512)	
Treat × Post-Sandy					−0.0387***
					(0.0127)
N	247,061	247,061	247,061	247,061	180,764
Tract FE	Y	Y	Y	Y	
Property FE					Y
County-Year FE					Y
Quarter FE					Y
Damage Controls					Y

Notes: Columns (1)-(4) report the estimation results from Equation (1). The dependent variable for each column is indicated in the column title. The sample consists of all properties in the potentially treated census blocks, and each regression includes the census tract fixed effect. Standard errors are clustered at the census tract level. Column (5) reports the estimation result from Equation (2), where the sample is all transactions for properties with repeated sales between 2005 and 2020. The outcome is the residualized log property prices after controlling for damage controls, and property, county-by-year, and quarter fixed effects. Standard errors are clustered at the census tract level. * p<0.1, ** p<0.05, *** p<0.01

Table 2.
Program Participation and Economic and Demographic Factors

	Economic Factor		Demographic Factor			
	log(Income)	log(HU Value)	% Wh	% Wh Affected	% BL	% BL Affected
	(1)	(2)	(3)	(4)	(5)	(6)
Demog	0.237	0.109	2.15*	1.88	-2.66	-2.27
	(0.199)	(0.0848)	(1.12)	(1.38)	(1.64)	(1.92)
Pseudo R^2	0.618	0.617	0.623	0.620	0.622	0.620
Observations	4,367	4,367	4,367	4,367	4,367	4,367
County FE	Y	Y	Y	Y	Y	Y

Notes: This table reports estimates from Equation (3). The dependent variable is an indicator for having a participating property (buyout or acquisition) in the census tract. The key regressor in each column is an economic or demographic factor indicated in the column title. The sample consists of census tracts. Each regression includes the full set of damage controls, including the number and percentage of households affected by flooding, number of housing units in each damage category, and average flood depth, as well as the county fixed effects. Standard errors are clustered at the county level. * p<0.1, ** p<0.05, *** p<0.01

Table 3.
The Effect of Acquisitions and Buyouts on Log Property Values

	Average Effect		Effect by Program Action			
	(1)	(2)	(3)		(4)	
			Acquisition	Buyout	Acquisition	Buyout
Treat × Post-Program	0.0513**		0.0512***	−0.0251		
	(0.021)		(0.0181)	(0.0178)		
× low intensity		0.0494**			0.0355*	−0.0469**
		(0.0228)			(0.0203)	(0.0211)
× high intensity		0.0538**			0.0666***	0.0125
		(0.0226)			(0.0211)	(0.0205)
Treat × Post-Sandy	−0.0941***	−0.0939***	−0.0794***		−0.0743***	
	(0.0264)	(0.0266)	(0.0226)		(0.0232)	
Adj R^2	0.577	0.577	0.577		0.577	
N	180,764	180,764	180,764		180,764	
Sandy Damage	Y	Y	Y		Y	
Property FEs	Y	Y	Y		Y	
County-Year FEs	Y	Y	Y		Y	
Quarter FEs	Y	Y	Y		Y	

Notes: This table reports estimation results on the effect of acquisitions and buyouts on property values. The sample includes all transactions on properties with repeated sales between 2005-2020, in census blocks potentially eligible for the acquisition and buyout programs. The dependent variable is the log of sales price. Columns (1)–(4) represent different model specifications. Column (1) shows the baseline specification in Equation (4). Column (2) estimates the differential effects by Program intensity, with the "high" and "low" categories defined using the median number of participating properties within 1,000m (7 participating properties). Column (3) separately estimates the effects of acquisitions and buyouts. Column (4) further distinguishes effects of each action by intensity, with the "high" and "low" category thresholds defined as the medians (6 acquisitions and 4 buyouts). Each specification controls for the property, county-by-year, and quarter fixed effects. Standard errors are clustered at the census tract level. * p<0.1, ** p<0.05, *** p<0.01

Table 4. The Effects of Acquisitions and Buyouts on Mortgage Applications

	# Loans		Log(Med Income) Mean = 11.4		% > Med Inc Mean = 63.8		% Black Appl Mean = 13.0	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Applications for Home Purchase (Mean #Loans = 48.6)								
Treat × Post-Program	14.1**		0.0299***		2.24*		0.503**	
	(5.02)		(0.00983)		(1.18)		(0.240)	
Acquisition		15.4***		0.0297***		2.61**		0.661**
		(4.62)		(0.0102)		(1.11)		(0.242)
Buyout		5.98*		−0.000132		−0.616		0.0754
		(3.30)		(0.0196)		(0.986)		(0.626)
N	90,741	90,741	76,994	76,994	77,488	77,488	76,840	76,840
Adj R^2	0.684	0.684	0.832	0.832	0.650	0.650	0.804	0.804
Panel B: Applications for Home Improvement (Mean #Loans = 10.7)								
Treat × Post-Program	2.50**		0.0284		1.12		1.15***	
	(1.12)		(0.0286)		(2.30)		(0.221)	
Acquisition		2.37***		0.0450		1.53		1.16***
		(0.822)		(0.0300)		(2.07)		(0.208)
Buyout		1.37		−0.0287		−0.674		0.401
		(0.824)		(0.0432)		(3.24)		(0.789)
N	90,489	90,489	72,151	72,151	73,286	73,286	70,468	70,468
Adj R^2	0.678	0.678	0.561	0.561	0.327	0.327	0.708	0.708
Panel C: Applications for Refinance (Mean #Loans = 65.6)								
Treat × Post	17.5**		0.0102		0.786		0.439	
	(6.87)		(0.00629)		(0.750)		(0.377)	
Acquisition		20.3***		0.00946		1.03		0.453
		(5.50)		(0.00970)		(1.01)		(0.394)
Buyout		4.36		0.00326		−0.823		0.780
		(3.96)		(0.0159)		(1.19)		(0.851)
N	90,762	90,762	76,913	76,913	77,431	77,431	76,705	76,705
Adj R^2	0.687	0.687	0.804	0.804	0.585	0.585	0.863	0.863

Notes: This table presents estimation results on the effect of acquisitions or buyouts on mortgage applications following Equation (5). Outcomes associated with applications for home purchase are shown in Panel A, home improvement in Panel B, and refinance in Panel C. The dependent variable is the number of mortgage applications in Columns (1) and (2), the log of median household income of applicant in (3) and (4), the share of applicants with household income above the county's median in (5) and (6), and the share of Black applicants in (7) and (8). Each regression controls for the census tract and year fixed effects, county-specific quadratic time trends, and the share of White populations in 2010 interacted with year indicators for the post-Sandy period. Standard errors are clustered twoway at the census tract and year level. * p<0.1, ** p<0.05, *** p<0.01

Table 5. The Effects of Acquisitions and Buyouts on Businesses

	No. of estab. growth rate (1)	Estab. birth rate (2)	Estab. death rate (3)	No. of jobs growth rate (4)
Panel A: Average Program Effect				
Treat × Post-Program	0.00916***	0.00332	−0.00645**	0.0369**
	(0.00312)	(0.00222)	(0.00207)	(0.0144)
Treat × Post-Sandy	−0.0116***	−0.0115***	−0.00103	−0.0609***
	(0.00332)	(0.00243)	(0.00201)	(0.0180)
Adj. R^2	0.074	0.107	0.103	0.010
Panel B: Effect by Program Action				
Treat × Post-Acquisition	0.0115***	0.00582**	−0.00574**	0.0218
	(0.00330)	(0.00240)	(0.00206)	(0.0138)
Treat × Post-Buyout	−0.00194	−0.00437	−0.00361	0.0443***
	(0.00461)	(0.00350)	(0.00268)	(0.0167)
Treat × Post-Sandy	−0.0119***	−0.0117***	−0.00104	−0.0599***
	(0.00329)	(0.00243)	(0.00194)	(0.0175)
Adj. R^2	0.074	0.107	0.103	0.010
N	430,892	462,911	462,911	413,243
Outcome mean	0.023	0.104	0.082	0.088
Hexagon FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y
White × Year Indicators	Y	Y	Y	Y

Notes: This table presents estimation results on the effects of acquisitions and buyouts on businesses following Equation (6). Panel A reports the average effect, and Panel B reports the effects of acquisitions and buyouts separately. The dependent variable is the growth rate of the number of establishments (i.e., the change in the number of active establishments as a share of all active establishments in the previous year) in Column (1), birth rate of establishments (i.e., the number of new establishments as a share of all active business in the previous year) in (2), death rate of establishments in (3), and growth rate of total employment in Column (4). Each specification controls for hexagon and year fixed effects, county-specific quadratic time trends, and the share of White populations in 2010 interacted with year indicators for the post-Sandy period. Standard errors are clustered at the hexagon level. * p<0.1, ** p<0.05, *** p<0.01

Table 6. The Effect on Businesses by Industry

	No. of estab. growth rate (1)	Estab. birth rate (2)	Estab. death rate (3)	No. of jobs growth rate (4)
Panel A: Service (53% of all businesses in 2010)				
Treat × Post-Acquisition	0.0149***	0.00801**	−0.00673**	0.00474
	(0.00469)	(0.00332)	(0.00305)	(0.0222)
Treat × Post-Buyout	0.00334	−0.00207	−0.00780**	0.0524***
	(0.00611)	(0.00454)	(0.00363)	(0.0191)
N	140,523	150,470	150,470	130,417
Outcome mean	0.033	0.1117	0.086	0.0107
Panel B: Construction (8% of all businesses in 2010)				
Treat × Post-Acquisition	0.0205***	0.00117	−0.0195***	0.0135
	(0.00763)	(0.00450)	(0.00596)	(0.0201)
Treat × Post-Buyout	−0.0105	−0.0135***	−0.00268	−0.0224
	(0.00879)	(0.00513)	(0.00673)	(0.0259)
N	66,238	71,129	71,129	58,602
Outcome mean	−0.028	0.058	0.088	0.023
Panel C: Eating and Dining Places (3% of all businesses in 2010)				
Treat × Post-Acquisition	0.0212*	0.00749	−0.0130**	0.0123
	(0.0111)	(0.00939)	(0.00531)	(0.0352)
Treat × Post-Buyout	0.00574	0.00892	0.00310	0.0553
	(0.0128)	(0.0112)	(0.00688)	(0.0361)
N	16,861	18,107	18,107	16,565
Outcome mean	0.032	0.076	0.038	0.117
Hexagon FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
County Quadratic Trends	Y	Y	Y	Y
White × Year Indicators	Y	Y	Y	Y

Notes: This table presents estimation results on separate effects of acquisitions and buyouts on business outcomes for select two-digit SIC industries, following a variant of Equation (6). Panel A focuses on establishments in Service (SIC code 70-89), Panel B on Construction (SIC code 15-17), and Panel C on Eating and Dining Places (58). Each specification controls for hexagon and year fixed effects, county-specific quadratic time trends, and the share of White populations in 2010 interacted with year indicators for the post-Sandy period. Standard errors are clustered at the hexagon level. * p<0.1, ** p<0.05, *** p<0.01

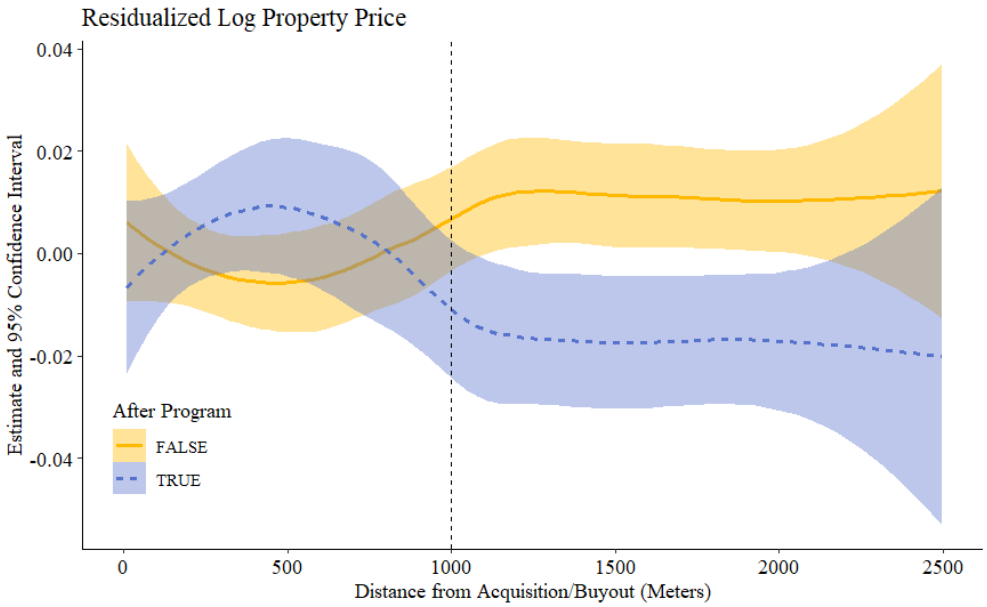
Table 7. Summary of Benefits and Costs

	Acquisition	Buyout	Program
Costs (millions USD)			
Total	277.60	359.72	637.32
Benefits (millions USD)			
Total	3054.35	671.96	3726.31
	(1568, 4541)	(186, 1158)	(2162, 5290)
Direct	556.26	743.18	1299.44
	(485, 627)	(465, 1021)	(1013, 1586)
Indirect	2498.09	-71.22	2426.86
	(1113, 3983)	(-470, 327)	(890, 3964)
Property values	2002.07	-273.59	1728.48
	(615, 3389)	(-654, 107)	(290, 3166)
Employment	496.01	202.37	698.38
	(-33, 1025)	(83, 321)	(156, 1241)
Direct BCR	2.004	2.066	2.039
	(1.749, 2.259)	(1.293, 2.839)	(1.589, 2.489)
Total BCR	11.003	1.868	5.847
	(5.648, 16.357)	(0.517, 3.219)	(3.393, 8.301)

Notes: This table presents a summary of a back-of-the-envelope cost-benefit analysis of the Program. The upper panel displays the costs and estimated benefits in millions of 2013 US dollars. For areas treated by both actions, we distribute the benefits based on the estimated treatment effects of each. The bottom panel presents the corresponding benefit-cost ratios (BCR). The BCR of direct resilience benefits are calculated based on FEMA's HMGP projects since 2000. The 95% confidence intervals are presented under the corresponding benefits and BCR estimates.

A Additional Tables

Figure A1: Residualized Property Value over Distance to Nearest Program



Notes: The sample includes 74,122 housing transactions with at least three sales during the research period. Log property value is residualized by Sandy damage level, inundation depth, and the property, county-by-year, and quarter fixed effects. Smoothing curves are obtained through a locally weighted scatterplot smoothing LOWESS. The solid and dotted lines represent the residualized values for transactions sold before and after the first Program, respectively. Shades show the 95 percent confidence intervals.

Table A1.
Summary Statistics

Panel A: Acquisitions and Buyouts

	All Programs (N=1,289)				Acquisition (N=566)		Buyout (N=723)	
	Mean	SD	Min	Max	Mean	SD	Mean	SD
Purchase Price ($)	377,050	159,986	4,536	893,199	373,068	142,298	380,168	172,607
Closed Date (Days)	2015-06	429	2013-07	2019-05	2015-08	318	2015-05	494
Demolition/Auction Date					2016-04	335	2017-01	545

Panel B: Housing Transaction

	All Transactions (N=180,764)				Treated (N=42,663)		Control (N=138,101)	
	Mean	SD	Min	Max	Mean	SD	Mean	SD
Sales Price (1,000$)	548.13	447.65	10	5000	442.23	261.91	580.85	486.39
Sales Year	2011.79	5.36	2004	2021	2012.25	5.38	2011.65	5.35
post-Sandy = 1	0.49	0.5	0	1	0.52	0.50	0.48	0.50
Inundated = 1	0.21	0.41	0	1	0.60	0.49	0.09	0.28
Inundated Depth	0.69	1.68	0	28	2.07	2.32	0.27	1.12
# Programs < 1,000m	4.26	19.14	0	325	18.04	36.10	0	0
# Buyouts < 1,000m	1.81	14.85	0	294	7.66	29.82	0	0
# Acquisitions < 1,000m	2.45	7.57	0	69	10.38	12.67	0	0
Dist to Program (m)	7398.92	9069.33	8.06	60737.58	410.03	293.3	9557.96	9374.72
# Repeat Sales	2.65	0.98	2	10	2.64	0.95	2.66	1.00

Panel C: Demographic Distribution of Neighborhoods by Program Action

	All Census Tracts (N=3,081)				Acquisition (N=70)		Buyout (N=29)	
	Mean	SD	Min	Max	Mean	SD	Mean	SD
Population	3,872.85	1,799.79	8	17,228	4,513.54	1,263.3	4,163.03	1,125.39
% White Population	0.56	0.33	0	1	0.86	0.16	0.89	0.11
% Black Population	0.21	0.28	0	1	0.06	0.11	0.04	0.07
Med Yearly Income (1,000$)	66.18	33.77	8.69	250.00	89.03	28.19	81.07	20.22
Med Housing Value (1,000$)	502.88	198.29	10.00	1,000.00	479.32	136.43	434.39	99.84
Avg # Bedrooms	2.4	0.65	0.33	4.47	2.97	0.46	2.85	0.35
% Population Inundated	0.07	0.19	0	1	0.54	0.31	0.41	0.32

Table A1.
Summary Statistics (Continued)

Panel D: Mortgage Applications

	All Census Tracts (N=64,890)				Treated (N=1,701)		Control (N=63,189)	
	Mean	SD	Min	Max	Mean	SD	Mean	SD
# Loans	124.41	148.10	0.00	1,784.00	214.29	191.75	121.99	145.99
Avg Loan (1,000$)	460.10	1,968.86	6.00	190,655.00	280.11	94.12	464.66	1,993.36
Annual Income (1,000$)	126.34	64.54	4.00	944.50	124.97	38.22	126.37	65.07
% > Median Income	67.39	16.53	0.00	100.00	60.79	13.55	67.56	16.56
% White Applicant	0.55	0.33	0.00	1.00	0.83	0.19	0.54	0.33
% Black Applicant	0.17	0.26	0.00	1.00	0.05	0.10	0.18	0.26
# Damaged Properties	16.90	104.00	0.00	1,677.00	328.12	404.79	8.53	63.42
% White Population	0.56	0.33	0.00	1.00	0.87	0.15	0.55	0.33
% White Affected	0.63	0.28	0.00	0.98	0.85	0.15	0.61	0.28
% Black Population	0.21	0.28	0.00	1.00	0.05	0.10	0.21	0.29
% Black Affected	0.18	0.23	0.00	0.92	0.06	0.11	0.19	0.24

Panel E: Business Establishments

	All (N=518,868)				Treated (N=83,412)		Control (N=435,456)	
	Mean	SD	Min	Max	Mean	SD	Mean	SD
# Active Estab.	12.65	41.43	0	1,732	7.22	11.06	13.69	44.88
# Estab. Births	1.22	4.61	0	313	0.68	1.6	1.32	4.98
# Estab. Deaths	1.11	4.69	0	925	0.61	1.42	1.2	5.08
# Estab. Growth	0.02	0.29	-1	7	0.03	0.3	0.02	0.28
Total Employment	89.07	623.66	0	100,771	32.16	119.61	99.97	678.21
Employment per Estab.	4.88	26.75	0	8,765	4	18.03	5.05	28.11
Inundated = 1	0.34	0.47	0	1	0.71	0.46	0.27	0.44
# Damaged Properties	1.5	6.97	0	124	5.97	12.94	0.64	4.6
# Programs < 1,000m	11.22	81.51	0	2,625	69.8	192.97	0	0
# Buyouts < 1,000m	5.07	68.39	0	2,543	31.51	168.12	0	0
# Acquisitions < 1,000m	6.16	27.53	0	391	38.29	59.03	0	0

Sources: New York State Governor's Office of Storm Recovery, FEMA's Modelling Task Force, Corelogic database (2021 version), National Archives of Federal Reserve Board of Governors Division of Consumer and Community Affairs, National Establishment Time-Series (NETS) database.

Table A2.
Differences in Demographics Across Census Tracts with Acquisitions and Buyouts

	All (N=3081)	Acquisition − Buyout
Population (1,000)	3872 (1685)	157 (719)
Pop Density (1000/km^2)	9700 (12812)	−307 (423)
% White Pop	0.56 (0.33)	−0.070 (0.055)
% Black Pop	0.21 (0.26)	0.043 (0.033)
Yearly Income (1,000$)	66.18 (31.05)	7.76 (9.28)
Housing Value (1,000$)	502.88 (243.91)	3.28 (6.34)
Damaged=1	0.14 (0.35)	0.26 (0.16)
% Property Damaged	0.01 (0.06)	−0.096 (0.24)

The first column presents the mean and standard deviation (in parentheses) of demographic variables for all census tracts. The second column reports the mean differences (corresponding standard error in parentheses) between census tracts with only acquisitions and those with only buyouts. The differences are calculated after controlling for county fixed effects.

Table A3.
Robustness Checks: The Effect on Log Property Values

	Average (1)	Acquisition (2)	Buyout (3)
Panel A: Within 4,000 m of the Programs (N=93,224)			
Treat × Post-Program	0.0325*	0.0333**	−0.0317*
	(0.0192)	(0.0165)	(0.0176)
Panel B: Pseudo treatment date assigned to untreated properties			
Treat × Post-Program	0.0483*	0.0534**	−0.0307*
	(0.026)	(0.0208)	(0.0179)
Panel C: Single Family Housing (N = 122,280)			
Treat × Post-Program	0.0751***	0.0688***	−0.0356*
	(0.0144)	(0.0138)	(0.0188)
Panel D: 800 meters cutoff in treatment defining			
Treat × Post-Program	0.0458**	0.0459**	−0.0233
	(0.0226)	(0.0203)	(0.019)
Panel E: 1,200 meters cutoff in treatment defining			
Treat × Post-Program	0.0413**	0.0379***	−0.0219
	(0.0193)	(0.0145)	(0.0161)
Panel F: 100 meters cutoff in FEMA matching			
Treat × Post-Program	0.0511**	0.0501***	−0.0228
	(0.021)	(0.0181)	(0.018)
Panel G: 200 meters cutoff in FEMA matching			
Treat × Post-Program	0.0512**	0.0511***	−0.0229
	(0.021)	(0.0181)	(0.0179)
Damage Controls	Y	Y	Y
Property FEs	Y	Y	Y
County-Year FEs	Y	Y	Y
Quarter FEs	Y	Y	Y

Notes: This table presents robustness checks on the effect of acquisitions and buyouts on property values. Panel A limits the sample to property transactions within 4,000 meters of any acquisition or buyout. Panel B assigns a pseudo treatment date to all control properties. Panel C focuses on the sample of single family housing only. Panels D and E define the treatment status and timing based on the presence of the first buyout or acquisition within the ranges of 800 and 1,200 meters, respectively. Panels F and G use the cutoffs of 100 and 200 meters in matching each property with the nearest FEMA damage assessment, respectively. Standard errors are clustered at the census tract level. * p<0.1, ** p<0.05, *** p<0.01

Table A4.
The Effects on Mortgage Applications by Treatment Intensity and Action

	# Loans Mean = 48.6		Log(Med. Income) Mean = 11.4		% > Med Inc Mean = 63.8		% BL Appl Mean = 13.0	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program								
× low intensity	7.27*		0.0272***		1.50		0.476	
	(4.17)		(0.00932)		(1.170)		(0.357)	
× high intensity	24.0***		0.0333*		3.38**		0.382	
	(4.99)		(0.016)		(1.47)		(0.882)	
Acquisition								
× low intensity		9.68**		0.0223*		1.65*		0.704*
		(3.98)		(0.0128)		(0.950)		(0.364)
× high intensity		25.9***		0.0505**		4.78***		0.510
		(5.11)		(0.0178)		(1.41)		(0.828)
Buyout								
× low intensity		4.15		0.028		0.511		0.520
		(6.17)		(0.0205)		(0.875)		(0.646)
× high intensity		1.13		0.0545*		3.74		0.392
		(1.63)		(0.0283)		(2.27)		(1.33)
N	90,741	90,741	76,994	76,994	77,488	77,488	76,840	76,840
Adj R^2	0.684	0.684	0.832	0.832	0.765	0.765	0.804	0.804
Census-tract FEs	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
County Quadratic Trend	Y	Y	Y	Y	Y	Y	Y	Y
White × Year Indicators	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table presents estimation results on the effect of acquisitions and buyouts on home purchase mortgage applications by intensity, with treatment intensity classified as "low" or "high" depending on whether a census tract has more or fewer than five participating properties. Standard errors are clustered twoway at the census tract and year level. * p<0.1, ** p<0.05, *** p<0.01

Table A5.
The effects on Mortgage Applications, Coastal Areas (# Tracts = 778)

	# Loans Mean = 47.0		Log(Med Income) Mean = 11.5		% > Med Inc Mean = 67.9		% Black Appl Mean = 5.6	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post-Program	17.8***		0.0156		0.640		1.23*	
	(4.62)		(0.0132)		(1.18)		(0.595)	
Acquisition		15.6***		0.0219**		1.49		1.12*
		(4.49)		(0.00963)		(1.24)		(0.612)
Buyout		16.8***		−0.0209		−2.03		0.554
		(4.10)		(0.0208)		(1.31)		(1.33)
N	13986	13986	12134	12134	12171	12171	12124	12124
Adj R^2	0.701	0.702	0.820	0.820	0.632	0.633	0.779	0.779

Notes: This table presents estimation results on the effect of acquisitions and buyouts on mortgage applications for home purchase following Equation (5). The sample includes only census tracts located within 5,000 meters of the coastal line. The dependent variable is the number of mortgage applications in Columns (1) and (2), the log of median household income of applicant in (3) and (4), the share of applicants with household income above the county's median in (5) and (6), and the share of Black applicants in (7) and (8). Each regression controls for the census tract and year fixed effects, county-specific quadratic time trends, and the share of White populations in 2010 interacted with year indicators for the post-Sandy period. Standard errors are clustered twoway at the census tract and year level. * p<0.1, ** p<0.05, *** p<0.01

B Replication of Main Results in Hashida and Dundas (2023)

In this appendix section, we discuss the key distinctions between our analysis of property values and that of Hashida and Dundas (2023), which produce contrasting findings. Our analyses differ in both the sample construction and the regression model specification. Below, we replicate the sample construction of Hashida and Dundas (2023) and demonstrate that the main difference in our findings arises from our model, which accounts for the differential effects of Hurricane Sandy on the treatment and control groups. As illustrated in Figure 2, it is particularly important to control for these confounding effects because of the nature of the Program as a disaster recovery policy.

Our sample differs from Hashida and Dundas (2023) in both temporal and geographic extents. In Hashida and Dundas (2023), the sample consists of all repeat sales from 1995 to 2020 across five counties (Nassau, Suffolk, Richmond, Kings, and Queens). Our sample also contains only repeat sales but spans from 2005 to 2020, which is a balanced distribution before and after Sandy. We also restrict the control group to properties within neighborhoods potentially eligible for the Program's treatment, including only census blocks with at least one inundated, damaged, or Program-participating property.

Our regression model as specified in Equation (4) includes controls intended to capture the differences in Sandy's impact on the treatment and control groups: a post-Sandy indicator interacted with a set of damage measures (Damage$\times$PostSandy) and with the treatment indicator (Treat$\times$PostSandy). As detailed in Section 4.1, these choices are informed by both theoretical considerations and empirical evidence demonstrating the presence of differential impacts. This is a major deviation from the regression model in Hashida and Dundas (2023), which includes neither terms.

To facilitate a direct comparison between the two analyses, we replicate the sample construction of Hashida and Dundas (2023). Their final sample includes 245,121 transactions from Zillow's ZTRAX database. In comparison, our sample constructed from the CoreLogic database includes 265,435 transactions. This slight difference in sample size is likely due to the more complete historical coverage of Corelogic over ZTRAX.

Table B1 presents our replication of Hashida and Dundas (2023)' main results (Panel A of Table 4) using their model specification. Overall, the coefficients in our replication in Panel A are very similar both qualitatively and quantitatively to their results as shown in Panel B. Crucially, our replication reproduces key qualitative findings, such as the negative effect of Program on nearby property values and the pattern of effect size decaying over distance. This confirms that there is no substantive difference between ZTRAX and Corelogic, which is expected given that both are sourced from administrative records of housing transactions and county tax assessments.

Next, we further disentangle the respective role of sample construction and modeling by estimating both models using both samples. In Table B2, Columns (1) and (2) present estimates from our model and the model in Hashida and Dundas (2023), respectively, based on their sample, while Column (3) and (4) use our sample. The results show a clear pattern: the model in Hashida and Dundas (2023) consistently produces negative estimates, while our estimates are positive regardless of the sample. This demonstrates that the different modeling choices are the main driver of the contrasting results. Nevertheless, sample construction also plays a non-trivial role. Comparing Columns (1) and (3), we estimate a much larger positive effect using our sample. Note that the sample of Hashida and Dundas (2023) includes all repeat sales regardless of whether they were affected by Hurricane Sandy in the first place. As we discuss in 4.1, including properties completely unaffected by Hurricane Sandy in the control group introduces a negative bias. To see this graphically, in Figure 2, this is akin to conflating the topmost line ("Not Affected" and "Not Treated") with the control line ("Minor-Affected" and "Not Treated").

In conclusion, our replication exercises highlight the importance of accounting for selection into the Program based on Sandy-induced damage to accurately identify the treatment effect.

Table B1.
Replications of Main Results in Hashida and Dundas (2023)

	<50m	<60m	<70m	<100m	100-400m	400-800m	800-1000m	1000-1200m
Panel A. Replication of HD23 Table 4 Panel A								
Treat×Post	−0.170***	−0.160***	−0.137***	−0.114***	−0.062***	−0.053***	−0.044***	−0.041**
	(0.021)	(0.022)	(0.023)	(0.0198)	(0.010)	(0.010)	(0.012)	(0.016)
N	237,034	237,416	238,006	239,464	244,947	243,224	238,280	238,428
Adj R^2	0.798	0.797	0.797	0.797	0.797	0.799	0.799	0.799
Panel B. Table 4 Panel A of HD23								
Treat×Post	−0.141***	−0.125***	−0.107***	−0.106***	−0.069***	−0.061***	−0.039***	−0.029*
	(0.038)	(0.037)	(0.034)	(0.028)	(0.015)	(0.012)	(0.014)	(0.017)
N	222,296	222,734	223,150	224,453	229,359	228,367	223,656	220,238
Adj R^2	0.794	0.793	0.793	0.794	0.794	0.795	0.795	0.795
Property FE	Y	Y	Y	Y	Y	Y	Y	Y
County-Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports estimation results on the average acquisition and buyout effect on property values. The dependent variable is the log of sales price. The samples construction follows the approach in Hashida and Dundas (2023). The treatment timing is defined based on the presence of the first acquisition and buyout within a 1,000m radius. Panel A uses the constructed sample from CoreLogic and employs the primary model in Hashida and Dundas (2023). Panel B presents the original results from Panel A of Table 4 in Hashida and Dundas (2023). Each specification controls for property, county-by-year, and quarter-of-year fixed effects. Standard errors are clustered at the census tract level. * p<0.1, ** p<0.05, *** p<0.01

Table B2.
Average Program Effect on Log Property Values based on Different Model and Sample

	HD2023 Sample		Our Sample	
	(1)	(2)	(3)	(4)
Treat × Post-Program	0.0263**	−0.064***	0.0513**	−0.0595***
	(0.0105)	(0.00757)	(0.021)	(0.0109)
Treat × Post-Sandy	−0.0816***		−0.0941***	
	(0.0128)		(0.0264)	
N	265,435	265,435	180,764	180,764
Adj R^2	0.795	0.795	0.577	0.576
Damage × Post-Sandy	Y		Y	
Property FEs	Y	Y	Y	Y
County-Year FEs	Y	Y	Y	Y
Quarter FEs	Y	Y	Y	Y

Notes: This table reports estimation results on the average Program's on property values. Columns (1)–(4) represent different sample constructions and model specifications. Columns (1) and (2) use the sample construction approach from Hashida and Dundas (2023), while Columns (3) and (4) follow our preferred sample construction used throughout the analysis. Columns (1) and (3) show results based on our baseline model specified in equation (4). Columns (2) and (4) use the model specification from Hashida and Dundas (2023). Each specification controls for the property, county-by-year, and quarter fixed effects. Standard errors are clustered at the census tract level. * p<0.1, ** p<0.05, *** p<0.01