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

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Wei Guo† Yanjun (Penny) Liao‡ Qing Miao§

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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 a buyout or acquisition increases nearby property values 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—have a more pronounced economic effect. 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.

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1 Introduction

Billion-dollar disaster events have been rising in the United States in the last four decades due to a combination of climate change and greater 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 to 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 may 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 (i.e., within 1 km) 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 et al., 2020; 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 participation driven by Hurricane Sandy's impacts, we limit our sample to areas exposed to its inundation or damage and control for an extensive set of damage measures. We also explore differences in the likelihood of participation in areas with different sociodemographic characteristics, which we use to 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, both acquisitions and buyouts support, to varying degrees, housing market recovery in Sandy-stricken areas, as the value of properties close to participating properties increase relative to comparable properties. The recovery effects differ by program activity. Acquisitions have a significant and positive effect on nearby property values, which decreases with distance from the acquired home. Over time, the effect of acquisitions persists and even strengthens. In comparison, the positive effect of buyouts is much smaller in size, decays faster over space, and attenuates over time. Our findings are robust to using only repeated sales, alternative model settings, and control groups. Consistent with the recovery in home values, 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 the nearby business environment. The Program increased 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, potentially because these properties are redeveloped for resilience improvement, which may 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 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 $2.88 billion and buyouts $2.01 billion in benefits stemming from enhanced resilience, higher property values, and job creation. Notably, these gains are largely attributable to the positive spillover effects to the local economy, underscoring the importance of considering the indirect economic consequences. The estimated benefit-cost ratio of the overall Program is 7.7, which strongly suggest that both actions are cost-effective in boosting disaster recovery and enhancing resilience.

This paper contributes to the emerging literature on managed retreat and government buyouts as one of the first to systematically assess the economic impacts of buyout and acquisition programs on local communities in the context of post-disaster recovery. Notably, the literature has predominantly focused on implementation, 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).

It has been recognized that buyouts and acquisitions can offer both benefits and harms to participants and communities (Siders, 2022; McNamara et al., 2018), but quantitative evaluations of their socioeconomic ramifications have been scarce. Several studies have assessed the post-buyout impacts on participating households (McGhee et al., 2020; Koslov et al., 2021). At the community level, one study employs a modeling approach to evaluate the effects on local government finance (BenDor et al., 2020). Two other studies examine changes to land use, housing stock, and demographics in the neighborhoods where buyouts took place (Zavar and Hagelman III, 2016; Martin and Nguyen, 2021).[2] However, little attention has been paid to the other local economic impacts and how different program actions (buyout and acquisition) might affect a community in different ways and lead to distinctive outcomes. The most relevant to our study is Hashida and Dundas (2023), which finds that the Program caused a decrease in values of homes adjacent to buyouts or acquisitions and that its effect attenuates at the neighborhood scale and dissipates after 1 km. These findings are qualitatively different from ours. As we discuss in detail in the results section, the divergence of our findings is associated with our research design that accounts for the nonrandom participation in the Program (considering its disaster recovery nature) with additional controls for the confounding, differential impact of Hurricane Sandy. Our research also goes beyond housing prices and combines a wider variety of community outcomes, including new resident demographics and business performance, in a more comprehensive evaluation of the Program. Our empirical findings consistently show a more pronounced, positive economic effect of acquisitions than buyouts on these outcomes. These findings provide deeper insights into the distinction between the two program actions and also a more holistic perspective on their roles 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 an overall benefit-cost analysis. Section 7 concludes.

[2] Martin and Nguyen (2021) tracked long-term neighborhood changes in North Carolina when buyouts were implemented after hurricanes in the 1990s. They found that housing growth declined, existing housing deteriorated, more Black populations moved into the nearby neighborhoods, and White residents moved out.

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 damages to housing in New York City. Nearly 2 million energy customers lost power, further contributing to the economic disruption (Federal Emergency Management Agency, 2013). The Federal Emergency Management Agency declared 14 New York counties disaster areas, including five in New York City (Henry et al., 2013).

In addition to the direct economic damages, Hurricane Sandy caused significant, disruptive impacts on the local housing markets, business, critical infrastructure systems, and public health and well-being in New York, as documented in recent empirical studies (Ortega and Taṣpınar, 2018; Gibson and Mullins, 2020; Meltzer et al., 2021; Comes and Van de Walle, 2014; Barile et al., 2020; Lin et al., 2016). In particular, Ortega and Taṣpınar (2018) estimate that it persistently reduced housing prices by about 9 percent in the flood zones. Ellen et al. (2020) report a similar decline in home values. Their results also suggest heterogeneous price effects depending on the income levels and storm-induced changes in the composition of homebuyers. Meltzer et al. (2021) find that Sandy caused a persistent 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 is 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.[3]

The terms "buyout" and "acquisition" are sometimes used interchangeably, as both approaches involve government purchase of private properties and enable homeowners who are unwilling or unable to rebuild after disasters 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 damage 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

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

disasters.[4] 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. The state offers 100 percent of the pre-Sandy fair market values and additional incentives of 5–15 percent to property owners in EBAs.

3 Data

For the analysis, we assemble 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 FOIL 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

[4] 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).

and latitude coordinates.[5]

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 use geospatial data on inundation and structure-level damage assessment data, 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 either were in the Sandy inundation zone or were outside it but had aerial imagery damage determinations. 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. An important advantage of the damage assessment data is that they include affected properties beyond those that applied for assistance. We match 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 against the Sandy inundation zone. Most properties are near to or within the inundation zone and clustered along the coastline of Staten Island and Long Island. A handful of buyouts were 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 census tracts. Acquisition and buyout properties do not differ significantly in their purchase price and participation dates. The racial composition of their census tracts are also similar; tracts with acquisitions have slightly higher income levels but lower property values. On average, tracts with acquisitions or buyouts have a larger population, a higher share of White residents and lower share of Black residents, higher income levels, and lower housing values than areas with no participating properties.

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

3.2 Housing Transactions

We obtain data on the universe of property transactions in New York State between 1997 and 2020 from Zillow's ZTRAX database (2021 version), one of the most comprehensive datasets of housing transaction records available. The data were created by combining housing transaction observations with county tax assessments of individual properties. This 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.

To construct our estimation sample, we exclude observations that feature transactions with prices below \$10,000 or above \$2,000,000, which account for 3.6 percent of all transactions. We also eliminate transactions that occurred less than three months after the previous sale, as one transaction might be recorded multiple times by different agents (buyer, seller, and county assessor) at different time points. To determine the flooding exposure to Hurricane Sandy for each property, we assign it with the damage measures of the nearest building point within 500 meters for which FEMA provided a damage assessment. [6] We restrict the sample to areas impacted by Sandy and only include properties that are located within the inundation zone, classified under FEMA's damage assessments, or within 1,000 meters of any participating properties. The final sample consists of 467,229 transactions; their summary statistics are presented in Panel B of Table A1.

3.3 Home Mortgage Applications

To examine the migratory responses in the neighborhoods with buyouts and acquisitions, we rely on home mortgage application data over 2000–2020, obtained from the electronic

[6] The median distance from a property to a building point with FEMA damage assessment is 57 meters. Most properties are matched within 100 meters of the FEMA damage assessment point

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.[7] 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.

We limit 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 exclude 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 a participating property. As the mortgage data do not indicate property addresses, we collapse 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.

3.4 Business Establishments

We obtain 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 restrict our business samples to within 1,000

[7] 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.

meters of the inundation zone.

To capture changes in business outcomes near the buyout or acquisition sites, we collapse 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.[8] 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 construct 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 neighborhood. As suggested by the literature and in our data, properties with greater damages are more likely to be bought out. Participation might also be correlated with other local characteristics, such as flood risk and demographics. As a result, the "treated" properties, businesses, and neighborhoods (i.e., those in close proximity to buyouts and acquisitions) are themselves more likely to suffer from Sandy's destructive impacts. We take into account these possible confounding factors in our choice of the study area and estimation models. To make sure we are comparing neighborhoods with damages to others similarly impacted by Sandy, we limit our study area to within 1,000 meters of the inundation areas. 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 to

[8] This results in a manageable number of businesses per hexagon while still capturing rich spatial variation.

inform our empirical design.

4.1 Selection Effect by Hurricane Sandy's Direct Impacts

As part of the efforts to promote recovery from Sandy, the Program by design was targeted toward 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 where Program participation is not correlated with the direct impact of Hurricane Sandy, in which we can recover the Program effect by simply comparing the differential response between the treatment and control groups relative to their predisaster levels. Panel B presents a situation in which properties in more heavily impacted areas are more likely to participate. Ignoring such differential impacts from Hurricane Sandy while doing the same comparison will downward bias 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 data.

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

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

The variable of interest, Treat_i, is a binary indicator of whether a property i is treated, defined as being within 1 km 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 categorical damage assessments. The regression also includes standard property characteristics (X_i) and 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 minor or major 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 compute the residuals of log property prices after controlling for property characteristics, damage, and year by census tract 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:

$$\text{Resid}\ln(\text{Price}_{it}) = \beta_1\text{Treat}_i + \beta_2\text{Treat}_i \times \text{PostSandy}_t + \alpha_{jt} + \epsilon_{it} \tag{2}$$

Each observation corresponds to a transaction for property i in census tract j at time t. 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_2, which is essentially a DiD estimate for comparing the treatment and control properties before and after Sandy, after explicitly accounting for its damage. We include only transactions between 2011 and 2014, before the Program was implemented. 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 (5 percent) relative to properties further away.

To sum up, our two tests show that treated areas not only saw greater damage but also experienced more severe economic consequences and difficult recovery, conditional on the 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 end up participating 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:

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

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 the demographic and economic factors of interest, including median household income and median housing values and the share of White or Black residents among the total population and those affected by Sandy. $\mathbf{I}(\text{Sandy})_j$ is an indicator for 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 significantly 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.[9]

5 Estimation Models and Results

As the Program was a government intervention in response to Hurricane Sandy, our empirical framework expands upon a standard DiD framework to incorporate both "treatments" by Hurricane Sandy and the Program and 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.

[9] 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.

$$\begin{aligned}\ln(\text{Price}_{it}) =& \beta_1 \text{Treat}_i \cdot \text{PostProg}_{it} + \beta_2 \text{Treat}_i \cdot \text{PostSandy}_t + \beta_3 \text{Treat}_i + \\ & \beta_4 \text{Damage}_i \cdot \text{PostSandy}_t + \beta_7 X_i + \alpha_{jt} + \epsilon_{it} \end{aligned} \tag{4}$$

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 or within 1 km of a buyout or acquisition. PostProg_{it} is a binary variable coded as 1 if the property was transacted after the first adjacent buyout or acquisition. For properties beyond 1 km, 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. Therefore, we also include $(\text{Treat}_i \cdot \text{PostSandy}_t)$ to control for Sandy's differential effects on participating properties and control groups and their heterogeneous recovery responses. Additionally, we control for property characteristics X_i, including the year of building or renovation, number of bedrooms, lot size acreage, and building square footage. We further include the census tract by year fixed effects, denoted by α_{jt}, 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. As described, we define the treatment group as being within a 1 km radius of at least one participating property. The control group includes all other properties affected by Sandy, including those impacted by inundation or included in FEMA's damage assessment. 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. We further examine the temporal dynamics of the treatment effect by using an event study model.

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 a 1 km radius by 3.66 percent. Furthermore, we find that the positive price effect increases with Program participation intensity. Column (2) suggests that properties close to more than 20 participating properties experience an 8.95 percent increase in value, compared to an average increase of 3.18 percent for properties exposed to fewer than 20.[10]

We also estimate a specification without controlling for differential responses to the storm using the interaction term of treatment and post-Sandy, to recreate the model in Hashida and Dundas (2023). This test, reported in Table A3, shows a negative estimated effect of the Program on property values, which is highly similar to the finding in Hashida and Dundas (2023). We thus infer that the difference in our findings is mainly driven by inclusion of this interaction term. As we demonstrated in Section 4.1, after controlling for a set of observables including Sandy damage, the residual housing values of treated properties still saw a larger decline following Sandy than those of the control group, suggesting the differential impacts of Sandy driven by unobserved factors. This interaction term is intended to account for these unobserved factors and actually makes a difference in estimation.

[10] We select a threshold of 20 participating properties to mirror the average number of acquisitions and buyouts within a 1 km radius, which is 18.7 for all treated properties.

We also examine the impacts of buyout and acquisition separately on nearby property values and find that the positive effect is mostly driven by acquisitions, as indicated in Column (3). Column (4) further suggests that the intensity of acquisitions and buyouts also play a role. Properties adjacent to more than 10 acquisitions experienced a 7.84 percent growth in value, compared to a 3.01 percent increase in locations with fewer acquisitions. Similarly, only properties with more than 10 buyouts within 1,000 m experienced a significant growth (averaging 4.3 percent); the effect was insignificant for locations with fewer buyouts.[11]

Additionally, we conduct three robustness tests to validate these findings; Table A2 presents the results. First, we exclude properties beyond 5 km from acquisition and buyout properties from the control group, allowing us to compare treated properties only to their geographically similar counterparts. Second, we limit our estimation sample to repeated sales only (a property was sold at least once before and once after Sandy). This strategy reduces the sample size by less than one third while better controlling for unobservable time-invariant characteristics of the properties. Finally, 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 or buyout in the same county, if any, or the average date of all buyouts and acquisitions for the remaining properties. Across all three tests, we show that the significant effect of acquisitions and buyouts on property values, differential effect by Program intensity, and dominant effect of acquisitions remain robust.

We further examine the spatial heterogeneity of the price effect by interacting the treatment indicators in the baseline specification with 50 m bins defined by the distance from the nearest acquisition or buyout.[12] The coefficients on each distance bin are reported in the top panel of Figure 3, which suggest that the average Program effect decays by distance. Within a close range (up to 250 m), property values increase by up to 10 percent. However, beyond 300 m, the effect becomes imprecisely estimated and small. In addition, in Figure 3

[11] The choices of threshold are drawn from the average number of participating properties within a 1 km radius for the treatment group, which are 9.29 and 9.41 for acquisitions and buyouts, respectively.

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

(middle and bottom panels), we show that the separate effects of acquisitions and buyouts both decay with distance, with the effect of acquisitions being significant and large (up to 15 percent) within a 500 m range. In contrast, the effects of buyouts are much smaller and become negligible after 100 m.

Last, we test whether the price impacts of acquisitions and buyouts change over time using an event study framework and defining the event as the first acquisition or buyout within a close neighborhood of 200 m. We normalize the response to 0 just before Hurricane Sandy, setting the reference year to three years before the Program.[13] As shown in the top panel of Figure 4, we find that 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. Six years after the Program, nearby properties experienced an average 20 percent growth in value relative to the pre-disaster level, compared to properties farther away. In Figure 4, we also show that the persistent effects were primarily driven by acquisitions (shown in the middle panel); the buyout effects on nearby property values were negligible in both the short and long runs (in the bottom panel).

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' positive effect is much smaller and transitory, possibly because they are often seen as a signal of risky locations or a loss of community, which may offset the benefits of the environmental amenity they create.

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

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. 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.

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 percentage points (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, they had a 0.5 pp increase in the share of Black mortgage applicants (see column (7)). 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, which is crucial 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.[14] As shown in Table A4, we find that the Program's effect is more prominent in places with more participating properties.

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 100 m) 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.

[14] 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. 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 km) 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 almost 70%. This is consistent with 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. These results suggest

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 acquisitions generate positive impacts on nearby business establishments and urban amenities, which is consistent with the direction of neighborhood changes in previous findings. The effect is particularly pronounced for acquisitions.

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.[15] 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 for buyouts.[16]

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.[17] The average BCR for HMGP-funded home elevation projects is 2.015, while

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

[16] 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.

[17] Data accessed at: https://www.fema.gov/openfema-data-page/hazard-mitigation-assistance-projects-v3.

for buyout projects it stands at 2.058. Applying these figures as proxies for the BCRs of acquisitions and buyouts within the Program, our analysis revealed resilience benefits amounting to $559.36 million from acquisitions and $740.3 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 1,000 m of an acquisition or buyout, using the 2013 assessment roll data for residential properties in New York State. The combined value of these properties was approximately $42.56 billion in 2013. The acquisitions led to an $1.74 billion increase in their property values, which amounts to 0.24% of the total value of the state's housing stock. Buyouts had a less remarkable impact, increasing property values by $93 million, or 0.01% of the state's total residential property values.[18]

In the labor market, our assessment includes 42,255 active establishments within 1,000 m from an acquisition or buyout. These establishments supported 197,260 jobs in 2013, with an average annual wage of $48,086 per worker.[19] The acquisitions directly resulted in 4,296 new jobs, adding $206.6 million to total annual wages. The buyouts led to 8,741 new jobs and an annual wage increase of $420.3 million.[20] Assuming conservatively that these additional jobs last 3 years and applying a discount rate of 7%, the present values of these wage gains are $580.14 million for acquisitions and $1.18 billion for buyouts.

In summary, our calculations show that acquisitions generated economic benefits of approximately $2.88 billion and buyouts yielded about $2.01 billion, both significantly exceeding their costs. The overall BCR of the Program is 7.7. Moreover, the majority of these

[18] We calculate the total impacts on property values by multiplying the combined market value of the treated properties ($42.56 billion) by the average impact coefficients reported in Table 3: 0.0410 for acquisitions and 0.00221 for buyouts, respectively.

[19] 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.

[20] The increase in new jobs is calculated by multiplying the total employment in treated firms (197,260 jobs) with the job growth rate coefficients from Table 5: 0.0218 for acquisitions and 0.0443 for buyouts. We then determine the wage impact by multiplying the additional jobs by the average wage.

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 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 both buyouts and acquisitions contribute to housing market recovery in affected areas, with property values increasing more rapidly near participating properties. Both interventions 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 possibly 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. However, our back-of-the-envelope cost-benefit calculation suggests that both actions generated economic benefits that are several times 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.

A caveat for interpreting our results 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 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.

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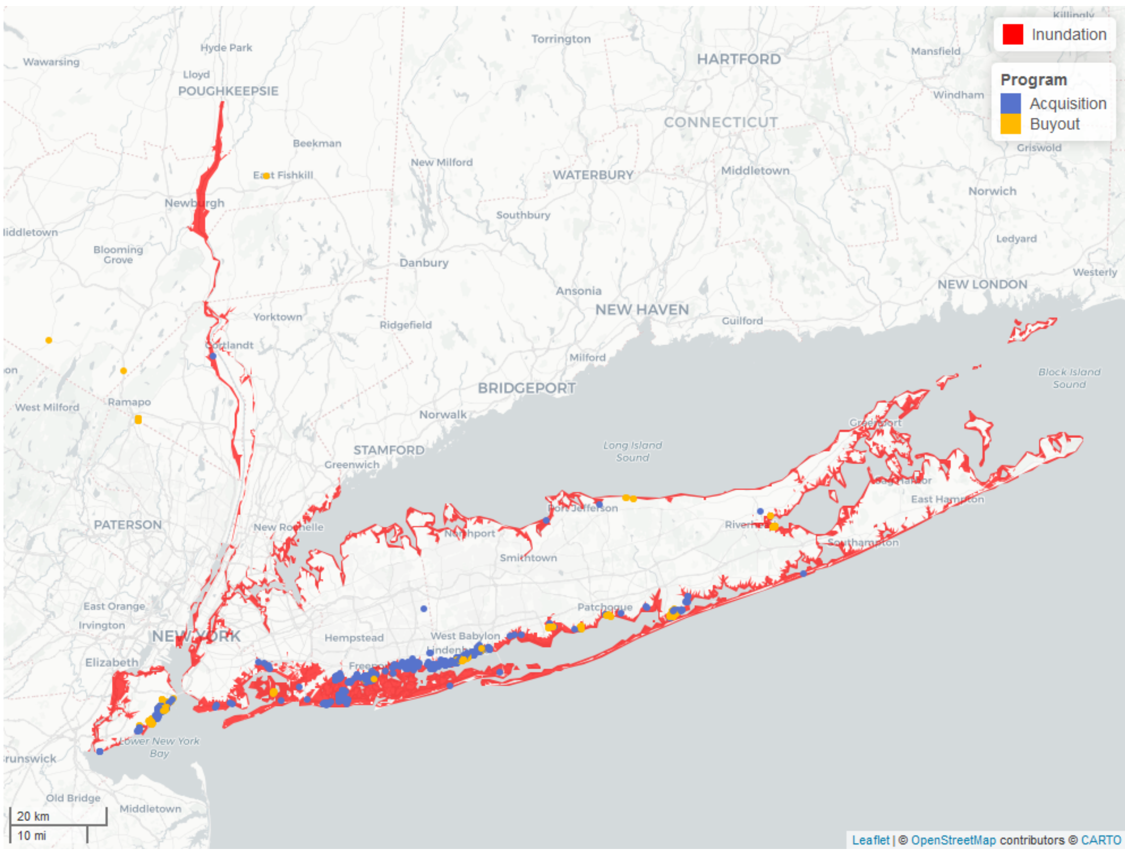
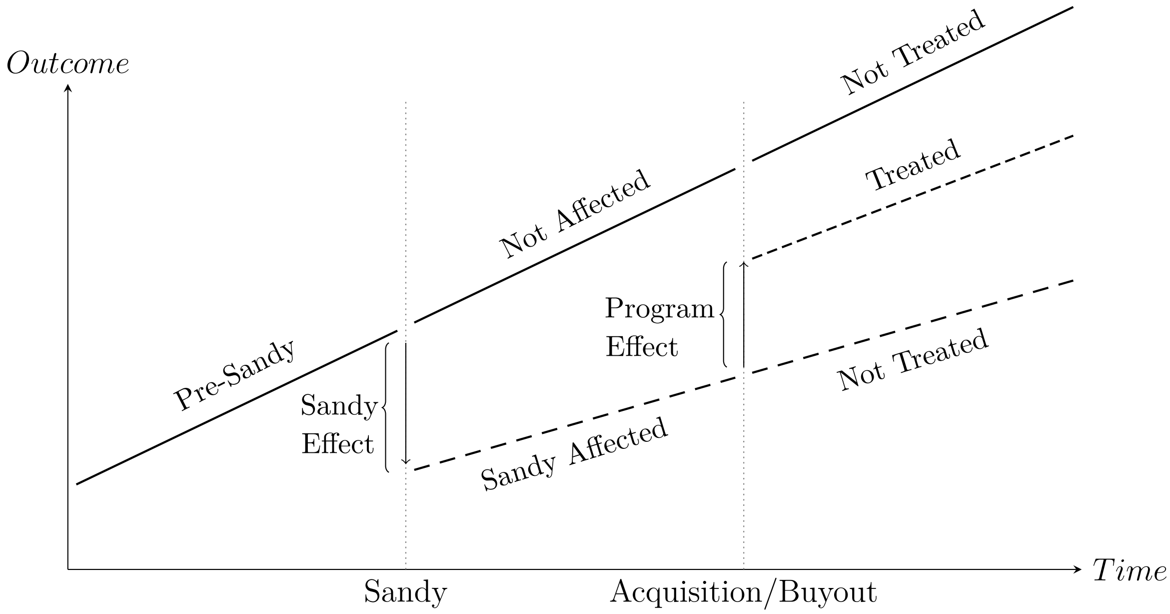
Figure 1: NY Rising Buyout and Acquisition Program and Sandy Inundation Zone



Notes: Red areas display the inundation zone of Hurricane Sandy. Colored points are the geographical location of properties participating in the Program, with blue color indicating acquisition and yellow indicating buyout.

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

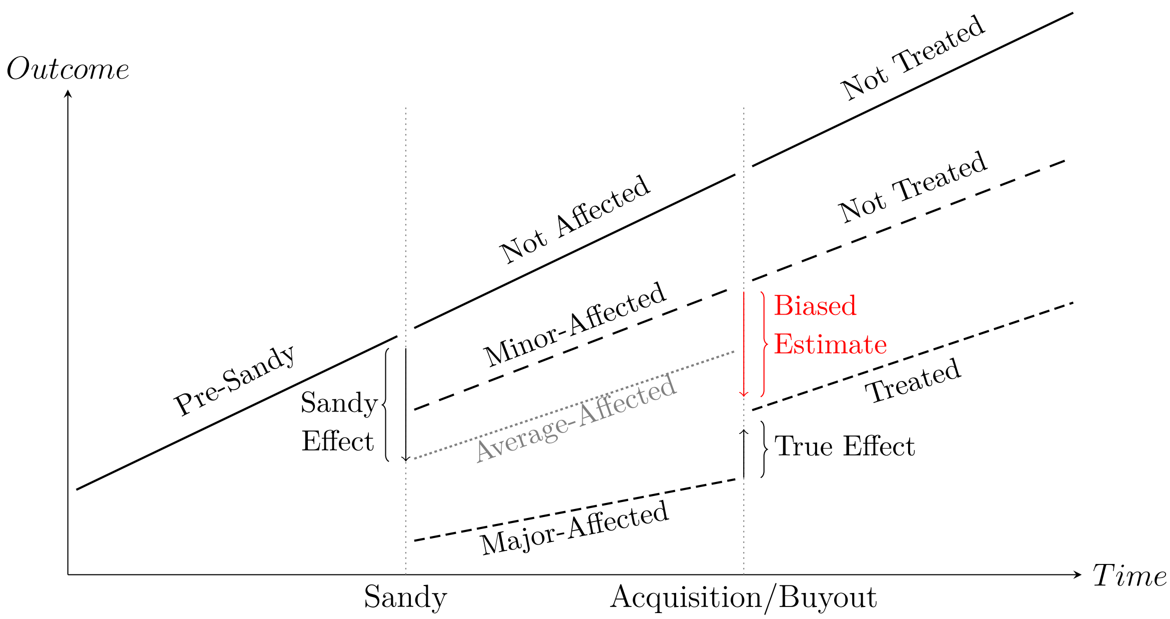
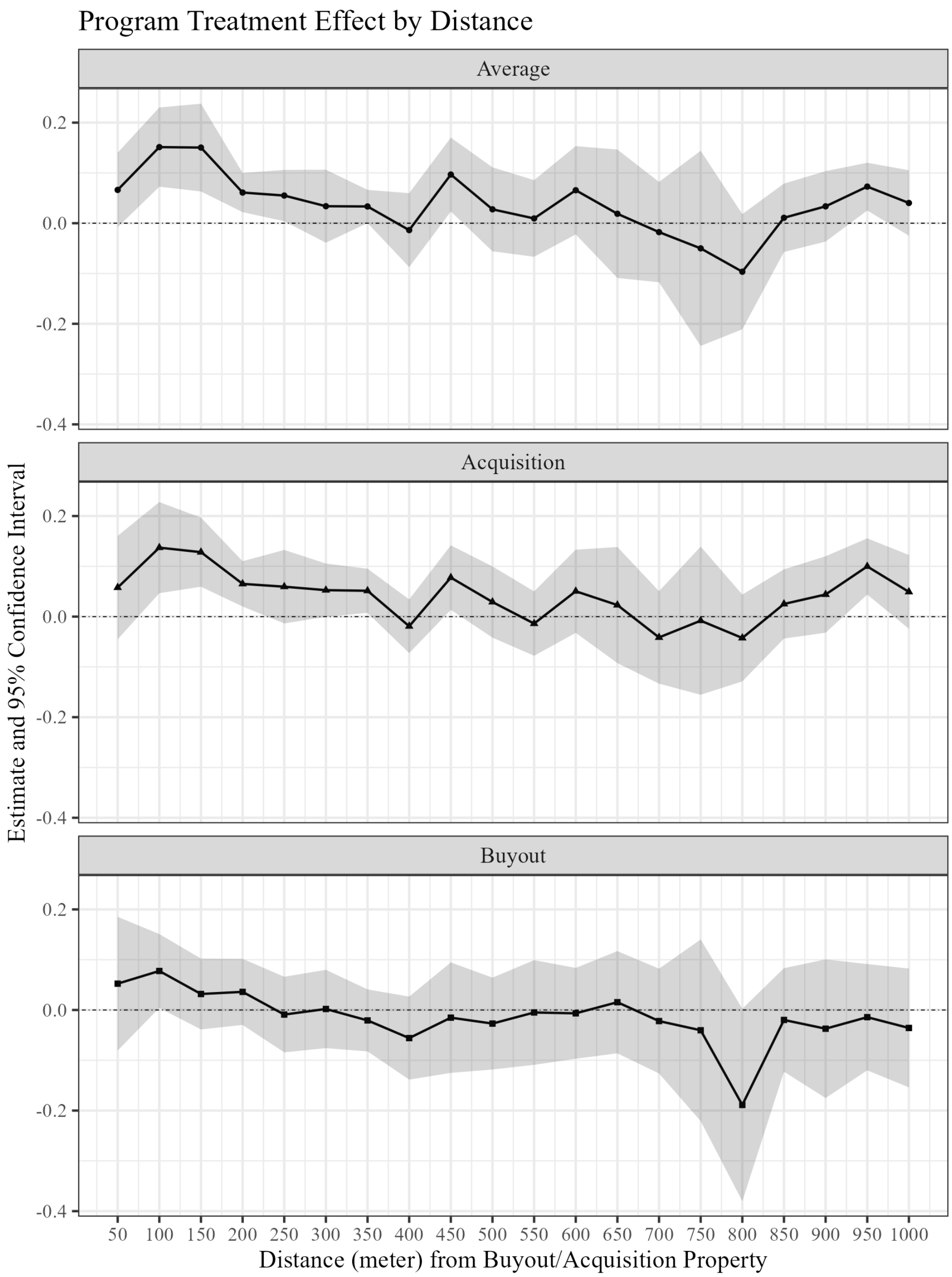
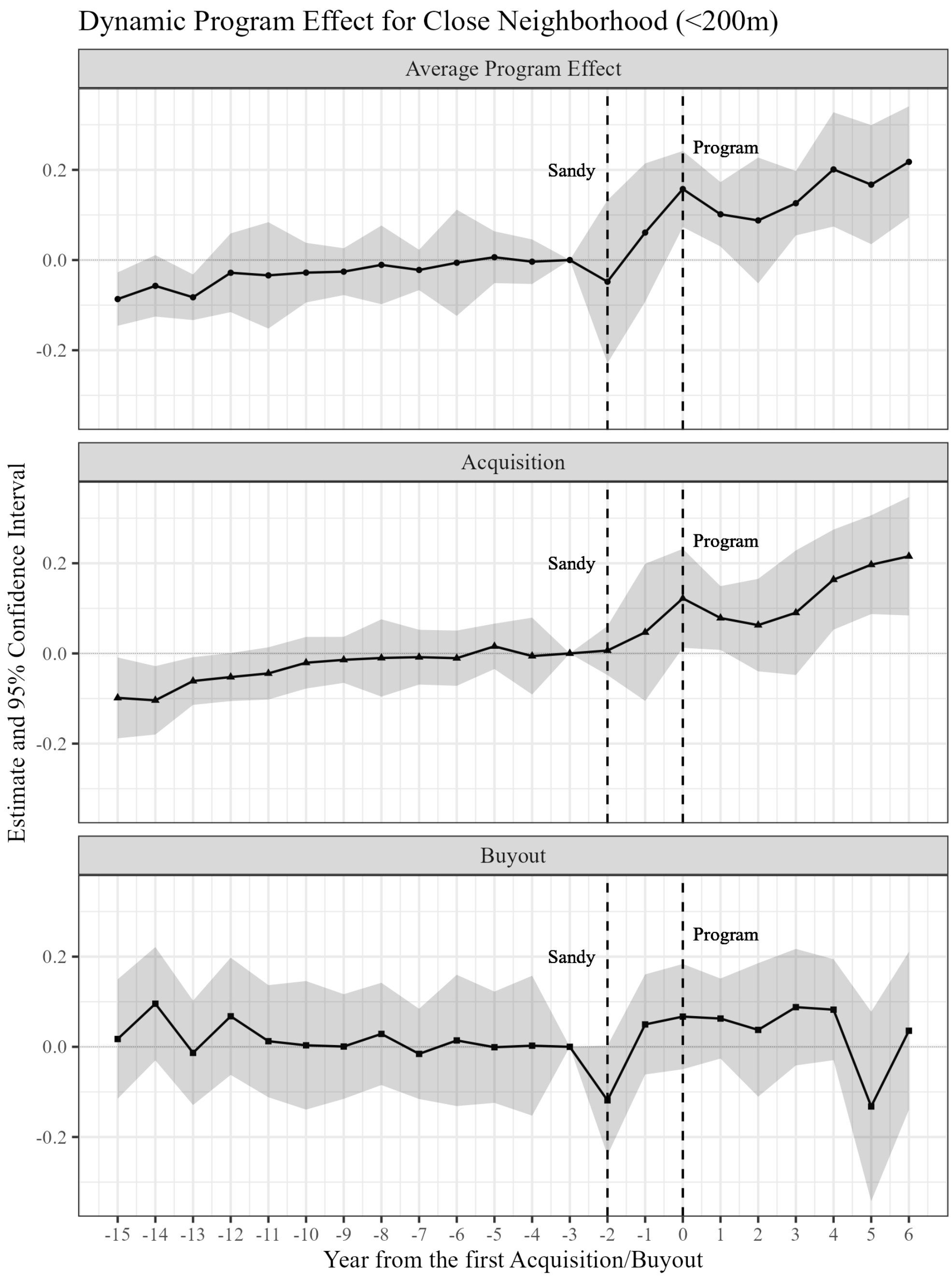


Figure 3: Program Effect on Log Property Value by Distance



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 50 m 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 acquisition or buyout, 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 and year level.

Figure 4: Dynamic Program Effect on Log Property Value



Notes: The three panels display the coefficients of an event study regression with the event defined as the first home acquisition or buyout within 200 m. The top panel shows the effect of either acquisition or buyout, the middle panel focuses on acquisitions only, and the bottom panel focuses on buyouts only. The sample spans from 15 years before to six years after the treatment (Program), and the coefficient for year -3 (one year before Sandy on average) is normalized to 0. Shades represent 95 percent confidence intervals constructed using two-way standard errors clustered at the census tract and year 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	0.432***	−0.181***	0.0891**	0.0334**	0.0552*
	(0.124)	(0.0414)	(0.039)	(0.0134)	(0.02)
Treat × Post					−0.0513***
					(0.00143)
N	275,175	275,175	275,175	275,175	49,456
Tract FE	Y	Y	Y	Y	
Tract-Year 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 individual properties, and each regression includes the full set of property characteristics and census tract fixed effects. Standard errors are clustered at the census tract level. Column (5) reports the estimation result from Equation (2), where the outcome is the residualized log property prices after controlling for property characteristics, damage controls, and census-tract-by-year fixed effects. The sample includes all transactions spanning from 2011 to 2014. Standard errors are clustered twoway at the census tract and year 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 estimation results from equation (3). The dependent variable is the 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	0.0366***		0.0410***	0.00221		
	(0.0036)		(0.00821)	(0.0197)		
× low intensity		0.0318***			0.0301**	−0.00941
		(0.00436)			(0.0110)	(0.0247)
× high intensity		0.0895*			0.0784***	0.043***
		(0.0452)			(0.0231)	(0.0123)
Treat × Post-Sandy	−0.00303	−0.00780***	−0.00439*		−0.00394	
	(0.00236)	(0.00211)	(0.00215)		(0.00367)	
Adj R^2	0.612	0.612	0.612		0.612	
N	467,186	467,186	467,186		467,186	
Sandy Damage	Y	Y	Y		Y	
Property Char.	Y	Y	Y		Y	
Tract-Year FEs	Y	Y	Y		Y	

Notes: Estimation results for the effects of acquisition and buyout programs on property value. 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 "low" and "high" categories representing properties close to less or more than 20 programs within a 1 km radius. Column (3) separately estimates the effects of acquisitions and buyouts. Column (4) further distinguishes effects of each action by intensity, with the "low" and "high" divisions made by using the threshold of 10 acquisitions or buyouts within 1 km. Each specification controls for a full set of property characteristics and census-tract-by-year fixed effects. Standard errors are clustered twoway at the census tract and year 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	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	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 for the effects of an acquisition or buyout 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 the 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 for the separate effect 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

Appendix

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=467,229)				Treated (N=90,163)		Control (N=377,066)	
	Mean	SD	Min	Max	Mean	SD	Mean	SD
Sales Price (1,000$)	423.64	353.49	10.00	4,000.00	373.10	237.38	435.73	374.97
Sales Year	2006.02	7.34	1995.00	2020.00	2006.14	7.42	2005.99	7.32
post-Sandy = 1	0.25	0.44	0.00	1.00	0.26	0.44	0.25	0.43
Inundated = 1	0.51	0.50	0.00	1.00	0.84	0.37	0.43	0.50
Inundated Depth	0.61	1.57	0.00	28.00	2.06	2.41	0.27	1.02
# Programs < 1km	3.61	20.66	0.00	328.00	18.70	43.94	0.00	0.00
# Buyouts < 1km	1.82	17.15	0.00	294.00	9.41	38.10	0.00	0.00
# Acquisitions < 1km	1.79	6.39	0.00	65.00	9.29	11.90	0.00	0.00
Dist to Program (m)	6,853.00	7,054.66	0.00	33,877.38	441.52	288.97	8,386.09	7,033.42
# Rooms	2.01	3.38	0.00	55.00	3.62	3.64	1.62	3.20
Year Built	1950.67	32.43	1728.00	2018.00	1957.69	29.80	1949.00	32.81
Building Area (sq. ft.)	1,984.29	2,186.98	100.00	895,015.00	1,804.84	832.94	2,027.20	2,398.17
Lot Size (acre)	0.22	0.84	0.00	52.62	0.18	0.55	0.23	0.90

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 < 1km	11.22	81.51	0	2,625	69.8	192.97	0	0
# Buyouts < 1km	5.07	68.39	0	2,543	31.51	168.12	0	0
# Acquisitions < 1km	6.16	27.53	0	391	38.29	59.03	0	0

Source: New York State Governor's Office of Storm Recovery, FEMA's Modelling Task Force, Zillow's ZTRAX 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.
Robustness Checks: The Effect on Log Property Values

	Average Effect		Effect by Program Action			
	(1)	(2)	(3)		(4)	
			Acquisition	Buyout	Acquisition	Buyout
Panel A: Within 5 km of the acquisition and buyout programs ($N = 268,143$)						
Treat × Post-Program	0.0352***		0.0407***	−0.0017		
	(0.00514)		(0.0105)	(0.0171)		
× low intensity		0.0318***			0.0290***	−0.0136
		(0.00464)			(0.00952)	(0.0217)
× high intensity		0.0831*			0.0822***	0.0364***
		(0.0447)			(0.0239)	(0.00834)
Adj R^2	0.553	0.553	0.553		0.553	
Panel B: Repeated sales only ($N = 306,141$)						
Treat × Post-Program	0.0552***		0.0461**	0.0260		
	(0.0141)		(0.0172)	(0.0281)		
× low intensity		0.0572***			0.0444**	0.02
		(0.0182)			(0.0202)	(0.0305)
× high intensity		0.0527			0.0509*	0.088***
		(0.0503)			(0.0259)	(0.0131)
Adj R^2	0.580	0.580	0.580		0.580	
Panel C: Pseudo treatment date assigned to untreated properties ($N = 467,229$)						
Treat × Post-Program	0.046***		0.0449***	0.0381***		
	(0.00912)		(0.00529)	(0.00646)		
× low intensity		0.0441***			0.0388***	−0.00118
		(0.008)			(0.0133)	(0.0215)
× high intensity		0.0903*			0.0814***	0.0548***
		(0.0442)			(0.018)	(0.00979)
Adj R^2	0.599	0.599	0.599		0.599	
Sandy Damage	Y	Y	Y		Y	
Property characteristics	Y	Y	Y		Y	
Census-tract-Year FEs	Y	Y	Y		Y	

Notes: Robustness checks for the effect of acquisitions and buyouts on property values. Panel A limits the sample to property transactions within 5 km of any acquisition or buyout. Panel B is based on repeated sales only. Panel C assigns a pseudo treatment date to all control properties. The definition for low- and high-intensity treatment is analogous to the baseline model. Standard errors are clustered twoway at the census tract and year level. * p<0.1, ** p<0.05, *** p<0.01

Table A3.
The Effect on Log Property Value Based on Model in Hashida and Dundas (2023)

	Average Effect		Acquisition		Buyout	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	−0.0293**		−0.0404**		0.0282	
	(0.0126)		(0.0146)		(0.0204)	
× low intensity		−0.0287**		−0.036**		0.0121
		(0.0125)		(0.0135)		(0.0219)
× high intensity		−0.0327		−0.0308		0.0666*
		(0.0305)		(0.0304)		(0.0349)
Adj R^2	0.585	0.585	0.585	0.585	0.585	0.585
N	467,186	467,186	467,186	467,186	467,186	467,186
Sandy Damage	Y	Y	Y	Y	Y	Y
Property Char.	Y	Y	Y	Y	Y	Y
County-Year FEs	Y	Y	Y	Y	Y	Y
Census tract FEs	Y	Y	Y	Y	Y	Y

Notes: This table presents estimation results on the log of property value using a similar specification as in Hashida and Dundas (2023). The primary difference from our baseline model is removing the interaction between the the post-Sandy indicator and the treatment indicator, which controls for differential impacts of Sandy on properties treated by the Program. Standard errors are clustered twoway at the census tract and year 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 for the effect of acquisitions and buyouts on home purchase mortgage applications by program intensity, with treatment intensity classified as "low" or "high" depending on whether a census tract has more or fewer than five program actions. Standard errors are clustered twoway at the census tract and year level. * p<0.1, ** p<0.05, *** p<0.01