

EYES ON THE FLOODPLAIN: FLOOD RISK DISCLOSURE, NEIGHBORHOOD CHANGE, AND CRIME

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ABSTRACT. We study how the arrival of new information on disaster risk produces long-run changes in the economic structure of affected communities. Leveraging plausibly exogenous variation in flood risk map updates, we use an event study approach to show that provision of more accurate flood risk information causes long-run decreases in property crime. We also demonstrate that communities which receive updated flood risk information experience long-run changes in population, house prices, median income, and composition of business establishments. We test several plausible mechanisms for the observed crime reduction, and we find these economic changes are most consistent with an eyes-on-the-street theory of crime. The effects of flood risk information are amplified when the updated information arrives after a physically destructive flood, suggesting a role for information salience. Together, these results show disaster risk information produces lasting changes in economic conditions.

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All errors are our own.

1. Introduction

Does the provision of disaster risk information to local communities produce lasting changes in economic outcomes? A large body of work has found sizable impacts of natural disasters, but these effects are often short-lived, such as with insurance takeup ([Gallagher, 2014](#); [Kousky, 2017](#)), state-level economic growth ([Strobl, 2011](#)), house prices ([McCoy and Walsh, 2018](#); [Bakkensen et al., 2019](#)), homeownership ([Sheldon and Zhan, 2019](#)), employment ([Deryugina, 2017](#)), and voting ([Coury, 2025](#)). Meanwhile, disaster risk information is often not salient, with flood risk in particular not fully capitalized in housing markets ([Hino and Burke, 2021](#); [Bakkensen and Barrage, 2022](#)).

In this paper, we study how the provision of updated, formal information on flood risk produces changes in long-run economic outcomes with a focus on crime. Specifically, we use plausibly exogenous variation in the rollout of updated flood risk maps from the National Flood Insurance Program (NFIP) in an event study framework to identify the effect of flood risk information on crime. We show updated flood risk maps are associated with a relatively small but economically meaningful long-term decrease in property crime.

While the connection between flood risk information and crime may seem tenuous at first glance, there are several reasons to expect long-run changes in crime which operate through the effects of flood risk information on other economic outcomes. We investigate these channels by first reproducing an important set of results that prior studies have already demonstrated: updated flood maps are associated with declines in house prices ([Gourevitch et al., 2023](#); [He, 2025](#)) and increases in population moving toward subsidized flood insurance ([Peralta and Scott, 2024](#)). We also find updated flood risk information is associated with decreases in homeownership and average income. These results are consistent with [Bakkensen and Ma \(2020\)](#) who show that low income individuals are more likely to move in to risky floodplains. Furthermore, our results align with the effects of physical floods found by [Sheldon and Zhan \(2019\)](#) and [Sheldon and Zhan \(2022\)](#), who show decreases in homeownership and an increased propensity for low income individuals to remain after a disaster.

Having presented a set of housing, population, and income results which suggest that flood risk information decreases the amenity value of communities, we then test for the effect of updated flood risk information on a new set of business and employment outcomes. We show that although

updated flood maps cause a decrease in total employment (driven by a decrease in service sector employment), they are also associated with an increase in the total number of business establishments and an increase in the number of small businesses.

The findings on business composition are important because they help reconcile the observed long-run decreases in property crime with the reverse gentrification response in the housing market. The literature on the economics of crime has proposed several causal factors which affect property crime rates. These factors include (1) the number of police officers, who act as a deterrent ([Evans and Owens, 2007](#); [Mello, 2019](#)); (2) wages for unskilled male labor ([Gould et al., 2002](#)), which affects the opportunity cost of crime; (3) neighborhood inequality, which increases the availability and visibility of potential targets ([Demombynes and Özler, 2005](#); [Metz and Burdina, 2018](#)) and; (4) an eyes-on-the-street theory of crime wherein crime is deterred by the presence of possible witnesses ([Jacobs, 1961](#); [Chang and Jacobson, 2017](#); [Carr and Doleac, 2018](#); [McMillen et al., 2019](#)). We test each of these channels, finding that counties which receive updated flood risk information experience (1) a long-run decrease in the number of police officers; (2) lower wages for unskilled male labor; (3) higher neighborhood inequality and; (4) an increase in the number of small businesses and a more diverse mix of commercial establishments, which have been shown to deter crime by attracting foot traffic ([Chang and Jacobson, 2017](#); [Brinkman and Mok-Lamme, 2019](#); [Braakmann and Zambiasi, 2026](#)). Of these results, only the findings on business composition can plausibly explain the decrease in property crime - the other factors are all associated with higher crime rates. We interpret these findings as supporting an eyes-on-the-street theory of crime reduction. We show suggestive evidence for this interpretation using data from the American Time Use Survey; while our estimates are noisy, we find some evidence that people spend more time outdoors and taking care of non-household children in counties which receive updated flood risk information.

While we are the first to show the impact of updated flood risk information on outcomes such as crime, we are also interested in how the timing of map updates relative to the timing of floods produces long-run changes in economic activity. We find that property crime responds more strongly to risk information, but violent crime declines more strongly in response to physical floods. Furthermore, we show the effect of flood risk information on both property and violent crime is amplified when the map update occurs following a major flood. Most similar in this regard is [Peralta and Scott \(2024\)](#), which finds that the impact of updated flood maps on in-migration is larger for communities with a longer history of floods. To separate the effect of physical destruction from the

effect of updated information, we show the results are only marginally attenuated when we control for a full set of time dummies to net out the dynamic effects of physical floods. Because disasters are also correlated with broader safety net inflows beyond disaster aid ([Deryugina, 2017](#)), we also show our results are robust to controlling for the amount of disaster aid and government assistance entering a county.

These findings speak to the manner in which experience and information interact to shape individuals' economic decisions. Much of the literature on disasters has focused on how disasters affect salience of risk along the lines of [Bordalo et al. \(2012\)](#), who theorize that agents place disproportionate weights on outcomes which command outsized attention. People learn about risk from disasters or near-miss events. These risks then become reflected in prices ([Hallstrom and Smith, 2005](#); [Ortega and Taspinar, 2018](#)). Learning about flood risk can also occur through the provision of updated risk information. Though outdated flood maps and insurance premiums act as price signals ([Mulder, 2024](#)), flood risks are undercapitalized ([Hino and Burke, 2021](#); [Bakkensen and Barrage, 2022](#)), partially because individuals respond differentially to good versus bad updates about their property's flood risk ([Conell-Price and Mulder, 2024](#)). Furthermore, increased salience of disaster risk may not be efficient when it comes to policy. Communities which experience wildfires demand greater prevention at the precise time they are least vulnerable ([Wibbenmeyer et al., 2019](#)); voters reward politicians for disaster aid but not for prevention ([Healy and Malhotra, 2009](#)). Though the literature has established that individuals learn from both experience and formal information independently, there is little evidence on how individuals learn from the combination of the two.

Although we focus on the effect of updated flood risk maps for communities already enrolled in NFIP, our paper is also related to studies which examine the effects of NFIP more broadly. Flood insurance premiums and mandatory participation affect house prices, transaction volumes, and the willingness of lenders to offer mortgages ([Blickle and Santos, 2022](#); [Hennighausen et al., 2023](#)). Participation in NFIP shifts housing development from coastal to inland areas ([Browne et al., 2019](#)). The mitigation efforts required to participate in NFIP have led to lower flood fatalities, especially amongst lower socioeconomic status individuals ([Lim and Skidmore, 2019](#)).

We estimate the effect of updated flood risk information on crime over the long run. A few studies have looked at the short-term effects of physical floods on crime. [Zahnow et al. \(2017\)](#) find floods in Australia increased property crime by displacing it into nonflooded affluent neighborhoods.

Floods and earthquakes in developing countries are also associated with increases in crime as state capacity is weakened following disasters (Silverio-Murillo et al., 2024; Kalsi et al., 2025). Most relevant is Zahran et al. (2009), who show that hurricanes in Florida are correlated with decreases in crime across a broad array of categories, with the exception of domestic violence.

We leverage plausibly exogenous variation in the rollout of updated flood risk maps to examine how new risk information produces lasting changes in crime and economic activity. We show that counties receiving updated flood risk map experience long-run decreases in property crime, a reverse gentrification response in the housing market, and a shift toward a more diverse array of small businesses. We advance the extensive literature on flood insurance by looking at novel outcomes, particularly crime, and carefully controlling for historical floods and disaster aid when estimating the impact of updated flood risk information. We form an important connection to the literature on the economics of crime by demonstrating the long-run decreases in crime which occur in response to flood risk information are most likely explained by an eyes-on-the-street theory of crime. Together, our results demonstrate that the provision of accurate flood risk information produces substantial long-run impacts on the economic structure of target communities.

2. Background

The National Flood Insurance Program (NFIP) was created in 1968 as a partnership between private insurers and the federal government. Federal incentives encourage communities to enroll in NFIP. Community enrollment makes property owners eligible to purchase insurance through the program. Premiums are set nationally based on flood zone. The program intended to subsidize insurance for existing structures in order to maintain property values. Notable features of NFIP include a chronic lack of uptake, even for properties within the designated Special Flood Hazard Area (SFHA) - the statutory 100-year floodplain - which are mandated to maintain flood insurance as a condition of any federally backed mortgage (Kousky, 2018); less educated households are especially unlikely to purchase flood insurance (Atreya et al., 2015). A comprehensive overview of NFIP can be found in Michel-Kerjan (2010a).

The first effort by the NFIP to provide information to communities at risk of severe flooding in accordance with the Flood Disaster Protection Act of 1973 was to coarsely classify communities using Flood Hazard Boundary Maps (FHBM) on the basis of their locations near 100-year floodplains. Following this, FEMA upgraded FHBM to Flood Insurance Rate Maps (FIRMs). A FIRM is the

official regulatory document that delineates flood hazard areas, assigns actuarial risk zones, and determines mandatory flood insurance purchase requirements under the National Flood Insurance Act of 1968. The process for issuing FIRMs was opaque and depended on factors such as age of the current map, value of assets in the region, political lobbying to maintain the “veil of ignorance”, and technical constraints on surveys and digitization. [Peralta and Scott \(2024\)](#) leverages the initial rollout of FIRMs as a source of exogenous variation for enrollment in NFIP.

Our identifying variation is similar to that of [Peralta and Scott \(2024\)](#) and the argument for exogeneity of the timing of rollout is primarily based on idiosyncratic prioritization and delays in the bureaucratic process. [Peralta and Scott \(2024\)](#) use the first appearance of FIRMs, many of which were created in the 1980s, as an instrument for NFIP enrollment. Many of these maps became badly outdated; by 2008, half of them had not been updated in 15 years ([Michel-Kerjan, 2010a](#)). FEMA therefore undertook two major waves of mapping activity during in the mid-2000s. The dominant wave was the Map Modernization Program (MapMod), a federally appropriated initiative launched in 2003 and funded at approximately \$1.4 billion between 2005 and 2010 ([Bellomo and Ryon, 2010](#)). MapMod converted paper-based FIRMs into Digital Flood Insurance Rate Maps (DFIRMs) using newly available LiDAR topographic data, substantially improving map accuracy and triggering the rezoning of properties across the national coverage area. The second wave reflects post-flood remapping efforts concentrated in the early 2010s.

We use the updated maps to gauge the effect of the provision of better flood risk information for communities which already had the option of enrolling in NFIP (i.e, they already had a FIRM). This allows us to separate the effects of better information through FIRMs from direct effects of NFIP, access to which is not changing for our treated areas. Appendix Figure [A1](#) shows there is no change in NFIP claim payouts and frequency following map updates. Figure [1](#) documents the temporal distribution of our treatment cohorts while Figure [2](#) shows the cumulative county-level NFIP update distribution. Our use of the continuous rollout of updated maps is most similar to [Hino and Burke \(2021\)](#), who follow a similar approach to demonstrate the under-capitalization of flood risk.

3. Data

3.1. Treatment Variable: FEMA Flood Insurance Rate Map Updates

We define treatment events at the county level as the year in which any of the county’s communities received a substantially updated FIRM. The treatment variable is obtained from the NFIP’s Community Status Book (CSB), a publicly maintained administrative database recording the participation status and current effective FIRM for each of the roughly 25,000 NFIP-participating communities in the United States. Because NFIP communities are sub-county administrative units encompassing incorporated municipalities, townships, and unincorporated county areas, we aggregate to the county level by taking the earliest updated FIRM effective date across all communities within a county. Our treatment variable G_i denotes the calendar year in which county i ’s FIRM was first updated during the sample window. Counties whose communities received no FIRM update between 2000 and 2019 are assigned $G_i = 0$ and serve as the never-treated control group.

The 133 never-treated counties do not exhibit any pronounced regional clustering, as documented in Figure 3. Broad geographic dispersion implies that the control group is not systematically drawn from a single regional climate or flood-risk regime, and its members did not simply opt out of the NFIP. Table A2 presents summary statistics. On average, never-treated counties have lower property crime rates, higher employment, higher home values (but lower homeownership), and lower manufacturing activity than treated counties. These baseline level differences are not in themselves a problem for identification; we inspect pre-trends in our event study graphs to examine whether the parallel trend assumption is likely to hold.

3.2. Crime Outcomes

Our primary outcome is the county-level property crime rate, defined as the number of burglaries, larcenies, and motor vehicle thefts per 100,000 residents. We also examine the violent crime rate, comprising murder, rape, robbery, and assault.

Crime data come from the FBI Uniform Crime Reporting (UCR) Program’s *Offenses Known and Clearances by Arrest* monthly files, obtained from the ICSPR (Kaplan, 2025). The raw data are structured as crime rates at the law enforcement agency $\times$ calendar month level, spanning January 2000 through December 2019. Because a county may contain multiple agencies, we construct a county-year panel using by first taking the of sum crime incident counts across months within each agency-year. The UCR data also includes population estimates. We sum both crime counts

and population estimates across agencies within each county-year and compute the crime rates per 100,000 residents.

3.3. Economic Variables

Income and poverty. Annual county-level estimates of median household income and the poverty rate come from the Census Bureau’s Small Area Income and Poverty Estimates (SAIPE) program, which provides model-based annual estimates for all U.S. counties. We use year-2000 values of both variables as time-invariant baseline controls in our mechanism regression specifications.

Business characteristics. We aim to evaluate adjustments in local economic activity, firm density, and the labor market driven by flood map updates. We obtain annual wages and average firm sizes from the U.S. Census Bureau’s County Business Patterns (CBP). We construct counts of total business establishments and retail-specific establishments using the CBP. We classify establishments into industries on the basis of the text records for both the county aggregate (all private industries) and the retail sector specifically from 2000 to 2019.

We also trace the volume and composition of employment across sectors using a separate panel constructed from the CBP county-level files. We track total private employment, manufacturing employment, and a broadly defined service sector, which is residually calculated as total private employment minus employment in agriculture, mining, construction, and manufacturing.¹

We obtain county-level data on housing values, homeownership, and household demographics from the American Community Survey (ACS). We use the 5-year estimates for the primary analysis, which provide comprehensive coverage for all U.S. counties but are consistently available only from 2009 onwards. We supplement this with the 1-year estimates, which cover counties with populations exceeding 65,000 and are available from 2005 onwards. For all 5-year ACS regressions, we restrict the estimation window strictly to 2009–2019, discarding all prior years where data is back-filled in the master panel. Similarly, for the 1-year ACS specifications, the estimation window is restricted to 2005–2019.

Police employment is constructed from the FBI Law Enforcement Officers Killed and Assaulted (LEOKA) program’s annual agency-level files, which report full-time sworn officer and

¹Census disclosure avoidance rules can introduce measurement error when employment is suppressed. For these missing data, we impute the counts by assigning the structural midpoint of the published employment size-class range (e.g., size class “A” [0–19] is assigned 10; class “B” [20–99] is assigned 60; and class “C” [100–249] is assigned 175).

total sworn employee counts. We collapse to the county-year level by summing across all reporting agencies within a county.

3.4. Physical Flood Shock Measures

To isolate the informational content of FIRM updates from the concurrent effects of actual flood events, we construct two complementary measures of physical flood exposure at the county level.

FEMA Presidential Disaster Declarations. We extract all Major Disaster Declarations involving flood, hurricane, coastal storm, tropical storm, typhoon, or dam/levee break from FEMA’s OpenFEMA administrative database, restricting to declarations falling within our 2000–2019 study window. For counties experiencing multiple qualifying events, we retain only the year of the first declaration, consistent with the absorbing-state requirement of the staggered DiD framework. This variable, G_i^{flood} , defines an alternative treatment timing capturing the first occurrence of catastrophic physical destruction in each county, enabling a parallel event-study analysis of the direct crime effects of severe flooding, independent of any mapping event.

NOAA Storm Events cumulative damage. From the NOAA Storm Events Database, we aggregate reported property damage from flood, hurricane, storm surge, and tropical storm events at the county-year level from 2000–2019. We compute each county’s total historical physical flood damage over the full sample period and use the median and the 90th percentile of this distribution to partition counties into low- and high-damage sub-samples.

3.5. Disaster Aid

To assess whether FIRM updates are correlated with the fiscal footprint of actual disaster events, we assemble a county-by-year panel of federal disaster aid for 2000–2019. Following [Deryugina \(2017\)](#), we distinguish between two distinct channels through which disasters generate public expenditure: *direct* disaster aid, disbursed through official disaster declarations, and *indirect* aid, comprising the broader social-insurance transfers that flow to affected populations in the years after a shock.

Direct disaster aid. We measure direct aid using administrative records from OpenFEMA, FEMA’s open data portal. Direct aid has two components. The first is *Individual Assistance* (IA) under the Individuals and Households Program, which provides grants to households for housing repair, rental support, and other disaster-related needs; we obtain approved individual

assistance dollars from the *Housing Assistance Program - Owners* and *-Renters* datasets. The second is *Public Assistance* (PA), which reimburses state and local governments for debris removal, emergency response, and the repair of public infrastructure; we obtain obligated public assistance dollars from the *Public Assistance Funded Projects Details* dataset. Both files identify the recipient county directly. We assign each aid dollar to the calendar year of the underlying disaster’s incident-begin date, using the *Disaster Declarations Summaries* file. For every aid measure we construct two versions: an all-hazards total and a flood-related total restricted to declarations whose incident type is a water-driven hazard.² All dollar amounts expressed in 2019 dollars.

Indirect aid. We measure indirect aid using the personal current transfer receipts reported in the Bureau of Economic Analysis Regional Economic Accounts, which provides annual county-level transfer payments by program category. Mirroring the outcome set in [Deryugina \(2017\)](#), we extract total government transfers to individuals including: retirement and disability insurance, medical benefits (and, separately, Medicare and public assistance medical care), income maintenance benefits (and, separately, SSI, SNAP, and other income maintenance), unemployment insurance compensation, veterans’ benefits, and education and training assistance, as well as transfer receipts of individuals from businesses, which incorporate private insurance settlements. We pay particular attention to the “other government transfers to individuals” line, as it is the BEA category that absorbs federal disaster relief payments and is therefore the transfer item most likely to respond mechanically to a genuine disaster.

Our direct and indirect measurements closely follow [Deryugina \(2017\)](#).³ The resulting panel covers 3,140 counties over 2000–2019. Direct aid is highly zero-inflated, as a county receives disaster aid only in years with a declared disaster affecting it. Direct aid is greater than zero in 27% of county-years and flood-related aid is greater than zero in 8% of county-years; indirect transfer receipts, by contrast, are positive in essentially every county-year. To express aid in intensive terms, we scale every dollar measure by the same contemporaneous county population estimate used to construct our property-crime rates. Using a common denominator ensures that the aid

²We classify as flood-related the incident types Flood, Hurricane, Coastal Storm, Tropical Storm, Typhoon, and Dam/Levee Break. We deliberately exclude the “Severe Storm” category, which is the largest declaration type but mixes wind, tornado, and flood events; including it does not materially alter our results.

³Notably, OpenFEMA contains data at the county level for our study period from 2000–2019. These data were only available at the level of disaster declaration for the 1979–2002 study period of [Deryugina \(2017\)](#), so we do not have to impute aid. While the OpenFEMA IA and PA series capture the two largest federal disaster-aid programs, it excludes smaller channels such as Small Business Administration disaster loans, whereas the declaration-level totals used by [Deryugina \(2017\)](#) are in principle comprehensive. Because IA and PA account for the overwhelming majority of federal disaster outlays, this omission is immaterial for our purposes.

outcomes are directly comparable to the main crime outcomes and that any population dynamics induced by FIRM updates enter the placebo tests in exactly the way they enter the primary analysis.

3.6. Urban–Rural Classification

We employ the USDA Economic Research Service’s 2013 Rural-Urban Continuum Codes (RUCC) to classify urban-rural areas for heterogeneity analyses. We classify counties with RUCC codes 1–3 as metropolitan (“urban”) and those with codes 4–9 as non-metropolitan (“rural”), following the standard convention in the spatial economics literature.

3.7. Local Economic Opportunity

To probe the idea that improved labor-market prospects raise the opportunity cost of crime, we assemble a county-year panel of economic-opportunity measures drawn from three distinct sources:

Unemployment (BLS). County unemployment rates and labor-force counts are the annual averages from the Bureau of Labor Statistics’ Local Area Unemployment Statistics (LAUS) program, available for every county over 2000–2019. These are *residence-based* measures and are distinct from the establishment-based employment counts used elsewhere in the paper.

Earnings and young-male employment (Census LEHD QWI). Wage and young-male employment measures come from the Census Bureau’s Quarterly Workforce Indicators (QWI), built from the Longitudinal Employer–Household Dynamics (LEHD) administrative records ([Abowd et al., 2009](#)). We use average monthly earnings of stable jobs, which we aggregate from quarterly to annual frequency and employment-weight across demographic cells. From the sex-by-education tabulation we construct average earnings of *non-college* workers (less than a bachelor’s degree) for all workers and, separately, for men.⁴ From the sex-by-age tabulation we construct average earnings and employment levels of young men (ages 19–34 and 19–24), the demographic group most relevant to property crime. QWI is *workplace-based* (jobs located in the county) and covers 2000–2019, though coverage begins later than 2000 for states that joined the LEHD program in later years.

Inequality and young-male employment rates (ACS). Two measures come from the American Community Survey (ACS) five-year estimates. The first is the Gini index of household income inequality, available at the county level for 2010–2019. The second is a *residence-based* employment

⁴QWI tabulates worker education only for those aged 25 and older, and does not provide a three-way sex-by-age-by-education cross; the male non-college earnings measure therefore covers men aged 25+, and we cannot isolate non-college men aged 19–34 specifically.

rate for young men, which we compute from the ACS sex-by-age-by-employment-status table as the share of men in a given age band who are employed. These measures become available in 2009.

3.8. Sample Construction and Summary Statistics

Our estimation sample is a county-year panel spanning all U.S. counties from 2000 to 2019. After merging the UCR crime panel with the FEMA treatment variable, the full sample comprises 62,815 county-year observations across 3,141 counties. Of these, 3,008 treated counties contribute 60,160 observations, and 133 never-treated counties contribute 2,655 observations.

We report summary statistics for full sample in Tables 1 and 2. Table A2 presents summary statistics for treated and never-treated counties along with balance tests referenced above. The mean property crime rate is 1902 per 100,000 residents in treated counties and 1378 per 100,000 in never-treated counties. Treated and never-treated counties exhibit modest pre-treatment differences on baseline economic characteristics: never-treated counties have somewhat lower median household income, higher poverty rates, and fewer establishments. These differences motivate our choice to condition on year-2000 baseline covariates in mechanism-related specifications, as detailed in the following section.

4. Empirical Strategy

We exploit the staggered, county-level rollout of FEMA Flood Insurance Rate Map updates across the United States to estimate the causal effect of government flood risk disclosure on local property crime. The central identification challenge is that counties receiving FIRM updates may differ systematically from those that do not, and may have been on divergent crime trajectories before any map revision. We organize our empirical strategy in two steps: we first formulate the canonical two-way fixed effects (TWFE) specification as a benchmark, then proceed to the use of the heterogeneity-robust estimator of Callaway and Sant’Anna (2021) as our primary approach.

A two-way fixed effects baseline. The natural starting point for panel estimation with a staggered binary treatment is the static TWFE specification:

$$(1) \quad Y_{it} = \alpha_i + \gamma_t + \beta \cdot D_{it} + X_i' \delta + \varepsilon_{it}$$

where Y_{it} is the property crime rate per 100,000 residents in county i and year t ; α_i are county fixed effects absorbing all time-invariant cross-sectional differences in baseline crime; γ_t are year

fixed effects capturing common national trends; $D_{it} = \mathbf{1}[t \geq G_i, G_i > 0]$ is an indicator equal to one for treated counties from their FIRM update year onward; and X_i is a vector of pre-treatment baseline covariates described below. Under parallel trends and homogeneous treatment effects, $\hat{\beta}$ consistently estimates the average treatment effect on the treated (ATT). To examine the temporal dynamics of the treatment effect and test the parallel trends assumption, we also estimate the event-study generalization of equation (1):

$$(2) \quad Y_{it} = \alpha_i + \gamma_t + \sum_{\substack{k \geq 10 \\ k = -5 \\ k \neq -1}} \beta_k \cdot \mathbf{1}[t - G_i = k] + X_i' \delta + \varepsilon_{it}$$

where k indexes years relative to the FIRM update, $\beta_{-1} = 0$ is the omitted base period, and we bin $k \leq -5$ and $k \geq 10$ at the endpoints to avoid noise from sparse long-run cohorts. Under homogeneous treatment effects, the path $\{\hat{\beta}_k\}$ traces the causal effect at each relative time period, and flat pre-period estimates ($k < 0$) would provide visual evidence that the parallel trends assumption is upheld.

Applying equations (1) and (2) to our data yields a static post-treatment coefficient of $\hat{\beta} = -17.10$ (SE = 42.68), negative but statistically indistinguishable from zero, and a dynamic event study with similarly flat and imprecise post-period estimates. We present the TWFE fixed results in Appendix Table A4. These results, however, cannot be interpreted as evidence of a null effect. With staggered treatment adoption, TWFE implicitly uses already-treated cohorts as control units for later-treated cohorts, assigning negative implicit weights to some cohort-time ATTs when treatment effects are heterogeneous across cohorts (Goodman-Bacon, 2021). In our setting, FIRM updates unfold over two decades and their effects on property crime plausibly grow over time as residential sorting and commercial restructuring accumulate. Under these dynamics, TWFE averages over “contaminated comparisons”; already-treated counties serve as implicit controls during their own post-treatment periods, systematically attenuating the aggregate estimate toward zero. Our reliance on the CS-DID framework below is empirically consequential: the heterogeneity-robust estimator recovers a large and statistically significant crime reduction that the traditional TWFE fails to detect.

The Callaway–Sant’Anna estimator. We therefore adopt the difference-in-differences estimator of Callaway and Sant’Anna (2021), which resolves the contamination problem by recovering cohort-specific average treatment effects on the treated (ATT) and aggregating them using clean comparisons against a well-defined control group, without mixing already-treated units into the

counterfactual. This methodological flexibility is essential in our setting: FIRM updates are staggered across counties over two decades, and their effects on community composition and crime may evolve over many years.

Let $G_i \in \{0, g_1, g_2, \dots\}$ denote the treatment cohort of county i , where $G_i = g$ indicates that county i 's Flood Insurance Rate Map (FIRM) was first updated in year g during the study window, and $G_i = 0$ denotes never-treated counties. In our primary specifications, the outcome of interest, Y_{it} , is defined as the property crime rate (offenses per 100,000 residents) in county i and year t . For each treatment cohort g , the cohort-time ATT is:

$$(3) \quad ATT(g, t) = \mathbb{E}[Y_{it}(g) - Y_{it}(\infty) \mid G_i = g]$$

where $Y_{it}(g)$ is the potential property crime rate for county i in period t under treatment cohort g , and $Y_{it}(\infty)$ is the corresponding counterfactual crime rate under never-treated status. We aggregate these cohort-time ATTs into a single event-study path by averaging across cohorts, weighting by cohort size:

$$(4) \quad \theta(\ell) = \sum_g \mathbb{P}(G_i = g \mid G_i \neq 0) \cdot ATT(g, g + \ell), \quad \ell \in \{-5, -4, \dots, \geq 10\}$$

The aggregated path $\theta(\ell)$ traces the average treatment effect from five years before to ten or more years after the FIRM update, capturing both the pre-period validation window and the long-run dynamic response. We estimate the cohort-time ATTs using the doubly robust inverse probability weighting (DR-IPW) estimator proposed by [Sant'Anna and Zhao \(2020\)](#), which combines an outcome regression model and a propensity score model for cohort assignment.⁵

Our treatment variable records the first effective FIRM date within county i during 2000–2019. As [Figure 1](#) documents, the distribution of treatment cohorts exhibits two clusters corresponding to the MapMod wave (approximately 2007–2012) and subsequent post-disaster remapping, with no single calendar year accounting for a disproportionate share of treated counties. This broad temporal dispersion is essential for the CS-DID framework: it ensures that event-study estimates are identified from genuinely staggered adoption across multiple cohorts rather than from a single policy event, and it allows us to separately estimate treatment effects at each relative time period without conflating cohorts. Our main specification uses the 133 never-treated counties ($G_i = 0$) as

⁵DR-IPW is consistent if either model is correctly specified, providing robustness against moderate misspecification of either estimating equation.

the comparison group. Despite representing a small share of the national sample, these counties are geographically dispersed across nearly all states and exhibit no pronounced regional concentration (Figure 3).

The plausibility of our identifying variation rests on the contention that the timing of FIRM updates across counties is not systematically driven by contemporaneous local conditions that also affect property crime. Three institutional features of FEMA’s administrative process support this claim.

First, the MapMod Program that dominates treatment timing during our sample period was a top-down federal initiative whose county-level sequencing was determined by FEMA’s internal contracting schedule and the geographic phasing of LiDAR data collection campaigns, not by county-level requests or prevailing economic or crime conditions (Peralta and Scott, 2024). Consistent with Peralta and Scott’s description of FEMA’s “lengthy, plausibly exogenous process of mapping risky communities,” the MapMod rollout proceeded according to a predetermined priority ordering based on map age, not on real-time local conditions.

Second, the FIRM production process is administratively lengthy, typically spanning three to five years from program initiation through the discovery, preliminary map, and appeal phases to the final effective date (Michel-Kerjan, 2010b; Pralle, 2019). This multi-year bureaucratic lag between the administrative trigger and the regulatory implementation further weakens any link between contemporaneous local economic or crime conditions and the treatment date: a county’s crime rate in year t cannot plausibly determine its FIRM effective date in year $t + 3$ through $t + 5$.

Third, and most directly, we validate the parallel trends assumption empirically in the pre-period event study window ($\ell \in \{-5, -4, -3, -2, -1\}$).

Identification requires the conditional parallel trends assumption:

$$(5) \quad \mathbb{E}[Y_{it}(0) - Y_{i,t-1}(0) \mid X_i, G_i = g] = \mathbb{E}[Y_{it}(0) - Y_{i,t-1}(0) \mid X_i, G_i = 0]$$

For our primary crime specifications, we set $X_i = \emptyset$ and rely on the unconditional version of this assumption: as documented above, the pre-treatment event-study estimates are statistically indistinguishable from zero across all five pre-period windows without any covariate adjustment, directly validating parallel trends from the data.

For mechanism specifications, where Y_{it} is replaced by socioeconomic or structural outcomes, the unconditional pre-event paths exhibit isolated, marginally significant departures in a small

number of pre-period windows. We address this by conditioning on year-2000 baseline covariates X_i . Specifically, we choose to condition on variables which reflect the two dimensions on which treated and never-treated counties exhibit the largest pre-treatment differences (Table A2). These are: (i) for median household income and the poverty rate, we condition on supply-side business-structure controls (log establishment count, log average wage, and log average firm size); (ii) for total establishment count, retail establishments, average wage, and average firm size, we condition on median household income and the poverty rate; (iii) for housing value, household count, and age composition – outcomes that respond simultaneously to both residential demand and commercial supply conditions, we condition on median household income, poverty rate, log establishment count, and log average firm size; and (iv) for all remaining mechanism outcomes, the unconditional pre-event paths are already flat and no controls are included. All covariates are fixed at year 2000 and cannot be affected by FIRM updates occurring later in the panel.

All primary specifications weight county-year observations by contemporaneous annual population to ensure that our estimates represent the aggregate policy impact on the average resident. A common concern with employing time-varying weights in a difference-in-differences framework is endogenous weighting; specifically, if FIRM updates induce systemic residential sorting, shifting populations could mechanically alter the composition of the sample weights post-treatment.

To address this concern, we also estimate our models using year-2000 baseline population weights. The baseline-weighted results yield nearly identical coefficients and statistical significance, establishing that our use of contemporaneous population weights does not introduce endogenous composition bias.

A key identification concern is that FIRM updates may be correlated with recent or anticipated physical flood events. If FEMA remaps a county shortly after a major disaster, our estimated treatment effect could confound the informational content of the new map with the economic disruption wrought by physical destruction – displacement of residents, destruction of commercial capital, and community disruption. We address this potential confound in two ways.

Subsample analysis by physical damage. Using cumulative NOAA Storm Events property damage over 2000–2019, we partition treated counties at the median of historical physical flood damage. Counties below the median experienced minimal physical destruction from flooding; their FIRM update therefore constitutes near-exclusively a risk-disclosure and insurance-cost shock, with negligible accompanying physical displacement. We additionally report a 90/10 split at the 90th

percentile damage cutoff as a more extreme stress test of this comparison (see Appendix Figure A13).

Controlling for physical flood event timing. We augment the baseline CS-DID specification by controlling for a set of event-study indicator variables relative to the timing of each county’s first FEMA Major Disaster Declaration for flood or hurricane: $\mathbf{1}[\text{year} - G_i^{\text{flood}} = \ell]$ for $\ell \in \{-2, 0, 1, 2, 3\}$. These dummies absorb the direct impact of physical flood shocks on crime rates, including both the acute disruption at $\ell = 0$ and the short-run recovery dynamics at $\ell = 1$ through 3, while leaving residual variation in the FIRM update timing G_i to identify the informational effect. If the crime reduction remains substantively similar to the baseline estimate under this specification, it provides additional evidence that the FIRM update’s effect on property crime operates primarily through the information channel.

5. Results

We are interested in exploring how the timing of new, formalized flood risk information causes long-run changes in economic outcomes. We examine this interaction through the lens of the effect of flood risk information on crime. Our results are organized as follows: first, we show the effect of updated flood maps on property crime. Next, we document the effect of updated maps on outcomes related to residential sorting. Then, we test for the effect of updated maps on firms and employment. Having demonstrated a wide range of economic consequences of flood risk information, we attempt to isolate the mechanism by which flood information reduces property crime by testing a variety of outcomes which have established causal links to property crime in the economics literature. Then, we document the effect of physical floods on crime. Finally, we show the effect of risk information on property crime is robust to controlling for the timing of physical floods and government transfers.

5.1. The effect of flood risk information on crime.

We estimate the effect of flood risk information on crime using the CS-DID estimator of Callaway and Sant’Anna (2021). We choose this estimator because it is robust to the presence of heterogeneous treatment effects when there is staggered rollout of treatment. In our setting, concerns about heterogeneous treatment effects are particularly valid. Figure 1 shows the staggered rollout of map updates. Meanwhile, a large body of literature on natural disasters finds disasters induce a spike-and-decay response in a range of economic outcomes, most notably with the insurance take-up

response to floods found by [Gallagher \(2014\)](#).⁶ This is a key form of treatment effect heterogeneity which may vary by treatment cohort.

Figure 4 shows the event study where treatment time is defined relative to the year a county receives an updated flood risk map through the NFIP. Pre-period coefficients are not statistically different from zero, which suggests that counties which received new flood risk information were experiencing similar trends in property crime prior to the map update as counties whose flood risk profile was not updated. The year of the map update is associated with a decrease of 50 property crimes per 100,000 residents, which is 2.6% of the mean property crime rate of around 1900 incidents per 100,000. This change in property crime rate steadily increases in magnitude over time; after ten years, counties with updated maps experience on average 478 fewer property crimes per 100,000 than counties without updated flood risk information. This corresponds to a decrease of 25%, which is comparable to the addition of around 30 additional police officers based on the estimate of [Mello \(2019\)](#).

5.2. Changes in Economic Outcomes.

We next explore the broader economic changes induced by the updated flood maps to provide context for the observed decline in property crime.

First, we look at the effect of map updates on population. We study the effect of FIRM flood map updates for communities already eligible for the NFIP. The original FHBMs to FIRMs upgrades in the 1980s and 1990s increased population as access to subsidized insurance caused people to move to risky floodplains, an effect first shown by [Peralta and Scott \(2024\)](#). In Figure 5a, we reproduce their findings in our more recent context which uses variation primarily from the MapMod project which improved the original FIRMs. [Peralta and Scott \(2024\)](#) find population increases by 5% per one standard deviation increase in historical flood risk; we similarly show a long-run population increase of 5-7% in counties receiving updated flood maps. That our results so closely match that of [Peralta and Scott \(2024\)](#) is reassuring given their study uses an instrumental variables approach with fundamentally different identification assumptions to the CS-DID empirical design.

Next, we look at the effects of map updates on the housing market via home prices and homeownership rates. These impacts have been previously explored in closely related studies. For example,

⁶See also: The effect of natural disasters on state-level economic growth ([Strobl, 2011](#)), house prices ([McCoy and Walsh, 2018](#); [Bakkensen et al., 2019](#)), homeownership ([Sheldon and Zhan, 2019](#)), employment ([Deryugina, 2017](#)), voting ([Corry, 2025](#)), provision of air quality information ([Corry et al., 2024](#)), among others.

Hino and Burke (2021) use similar identifying variation to show that there is a substantial price penalty from map updates in markets where buyers are more aware of flood risk. Gourevitch et al. (2023) also show that flood risk is underpriced in part because deficiencies in FIRMs translate into a lack of information about flood risk. Furthermore, floods themselves are associated with lower homeownership rates (Sheldon and Zhan, 2019). Figure 5d shows that flood map updates are associated with a decrease in house values of around 8%, confirming the findings of Hino and Burke (2021) using self-reported data in the ACS. The homeownership rate in treated counties declines by around 1% (Figure 5c), which is about one quarter of the decline observed among migrants to disaster-hit areas (Sheldon and Zhan, 2019). Previous work has shown that low-income individuals are more likely to sort into risky floodplains (Bakkensen and Ma, 2020) and are more likely to remain following floods (Sheldon and Zhan, 2022). These results would predict that median income would fall in flood-prone areas when updated risk information becomes available. In Figure 5b, we show that is indeed the case. Median income falls by around 8% ten years removed from a map update.

So far, we have shown that updated flood risk information is associated with increases in population and decreases in home values, homeownership, and median income. These results have either been shown before in similar contexts or are directly predicted by the prior studies cited above. This stylized story of reverse gentrification occurring in counties following map updates is well-established, but it raises important questions in the context of our new results on flood risk information and crime. Gentrification typically follows reductions in crime (O’Sullivan, 2005) as safety increases the value of urban amenities (Albouy et al., 2020). In our setting, we observe reductions in crime alongside decreases in income and house prices.

We therefore investigate new relationships between flood risk and economic outcomes. In Figure 6b, we show that total employment declines even as overall population increases. We decompose this decline in employment by sector. Figures 6c and 6d show this stark decline in total employment is driven by declines in service employment; there is no change in manufacturing employment. Furthermore, we observe that updated flood risk information is associated with an 18% increase in the number of business establishments (Figure 7a) and a 14% decrease in average firm size (Figure 7b). This set of results is also in line with predictions from the economics literature on gentrification. For example, gentrification is associated with increases in service sector employment (Lester and Hartley, 2014) and increased business churn (Glaeser et al., 2023); this churn is likely to have a

larger effect on small businesses which have higher exit rates than larger firms (Rossi-Hansberg and Wright, 2007). We find decreased service sector employment and an increase in the number of small businesses in counties that experience the opposite pressure on house prices, income, and demographics in response to updated flood risk information.

5.3. Plausible channels through which flood risk information affects property crime.

While there is a clear relationship between formalized flood risk information and property crime, it is unreasonable to believe the release of updated maps directly causes decreases in crime. Our goal is therefore to uncover the channels through which flood risk information affects crime. We therefore examine a series of outcomes which have been shown to causally affect property crime in the economics literature.

5.3.1. *Possible Channels: Police Officers.* Given the strong causal relationship between the number of police officers and crime (Evans and Owens, 2007; Mello, 2019), we first examine whether the number of officers is changing in response to flood risk information. Communities experiencing disasters may benefit from intergovernmental transfers and possess additional resources to spend on police. For example, Liao and Kousky (2022) find that local governments increase public safety spending following California wildfires, although the net effect on municipal budgets is negative. In Figure 8, we show that the number of sworn police officers within a county follows a pattern consistent with the predictions from wildfires from Liao and Kousky (2022). By year ten, the number of police officers declines by a large and statistically significant 4%.

5.3.2. *Possible Channels: Opportunity Cost of Crime for Young Unskilled Men.* The hypothesis that the opportunity cost of crime affects the number of criminal offenses goes back at least to seminal work by Becker (1968). More recent papers have shown that wages and employment rates particularly for young men (proxies for the opportunity cost of crime) causally affect property crime rates (Gould et al., 2002; Britto et al., 2022). In Figure 9a, we show that there is no change in the unemployment rate for men aged 19-34. In Figure 9b, we find no statistically significant change in the wages for non-college educated males. Standard errors are wide; if anything, we find wages for non-college men decline, consistent with the overall decrease in median income.

5.3.3. Possible Channels: Availability of Targets. The availability and visibility of potential victims has also been shown to affect property crimes. Under this hypothesis, the presence of more affluent individuals, or proximity of rich neighborhoods to poor neighborhoods, increases property crime by increasing the potential rewards for committing crimes such as burglary and theft. Neighborhood inequality has been used as a proxy for changes in the availability of targets (Demombynes and Özler, 2005; Metz and Burdina, 2018). We therefore test whether inequality is changing in response to updated flood risk information. Figure 10 shows that county-level inequality (Gini coefficient) increases by 0.02 relative to a mean of 0.44.

5.3.4. Possible Channels: Eyes-on-the-street Theory of Crime Reduction. So far, the results on number of police officers, opportunity cost of crime, and availability of targets would all point toward increased property crime rates. What then explains the decrease in property crime following the provision of formalized flood risk information? The eyes-on-the-street theory of crime (Jacobs, 1961) holds that the presence of witnesses and bystanders on the street make streets safer, as they raise the likelihood of being held accountable for crimes committed. Evidence supporting this theory has been found in several recent empirical studies (Chang and Jacobson, 2017, Carr and Doleac, 2018, McMillen et al., 2019). The public health literature has documented strong correlations between business diversity and frequency of walking trips (Boer et al., 2007; McConville et al., 2011). In Figure 11, we show that the diversity of businesses increases whether we measure it via a commercial entropy index over the distribution of establishments across three-digit NAICS subsectors (Figure 11a) or amenity share (Figure 11b).⁷ We also find noisy, weak evidence from the American Time Use Survey that people spend more time outside and more time taking care of non-household children in treated counties (Appendix Figure A2).

5.4. Effect on Violent Crime

Thus far we have focused on property crime because it is more strongly linked to economic conditions than violent crime, which is much more impulsive (Ludwig and Schnepel, 2025). However, research which has established the effects of economic conditions, particularly unemployment, on

⁷We define the business amenity share as the share of the county’s establishments in consumer-facing, foot-traffic sectors. These include NAICS 722, food services and drinking places (restaurants, bars, and coffee shops), 445 is food and beverage stores (groceries), 812 is personal and laundry services, and 713 is amusement, gambling, and recreation (including fitness facilities). A narrower core variant (NAICS 722 and 445 only) yields similar results.

property crime also find smaller effects for violent crime ([Raphael and Winter-Ebmer, 2001](#); [Britto et al., 2022](#)). We therefore test for the impact of updated flood risk information on violent crime.

Figure 12 depicts the event study results. Standard errors are wide and estimates are not distinguishable from zero, so we are hesitant to say flood map updates have an impact on violent crime. The point estimates suggest, if anything, violent crime decreases following map updates. The estimated coefficient of a decrease in 104 violent crimes per 100,000 represents a decline of 10.8% of the mean of 957 violent crimes per 100,000. Meanwhile, the property crime rate declines by 24.9%. Notably, this ratio of changes in property crime to changes in violent crime almost exactly matches the ratio found in [Britto et al. \(2022\)](#) for workers who were laid off in the previous year.

5.5. Comparing Flood Map Updates to Physical Floods

The argument for exogeneity of treatment relies on idiosyncratic bureaucratic factors being the primary determinant of treatment timing. These factors include the age of the previous map and the long time lags from map initiation to completion, as described in [Peralta and Scott \(2024\)](#). However, FEMA’s MapMod project also lists recent flood experience as being a potential reason for prioritization of communities for map updates. Though it takes three to five years to produce an updated map, it is possible that map updates could be correlated with recent flood experience. To that end, we aim to separate out the effect of physical floods from the effect of the informational flood risk treatment.

We begin by exploring the effect of physical floods on crime. We define treatment events using FEMA Presidential Disaster Declarations with types listed as flood, hurricane, coastal storm, tropical storm, typhoon, or dam/levee break. Figure 13 shows that violent crime is much more responsive to physical floods than property crime. While property crime exhibits a decrease of 5.6% which largely returns to baseline within five years, violent crime exhibits a statistically significant decline of 196 incidents per 100,000 residents, a decline of 19.8%.

We are not aware of any papers which test for the long-run effects of floods on either property crime or violent crime; much of the literature has found increases in crime over the very short-term ([Zahnow et al., 2017](#); [Kalsi et al., 2025](#)). Most similar is [García Hombrados \(2020\)](#) who studies the

long-term effect of the 2010 Chilean earthquakes. He finds declines in property crime rates of 15-30% most plausibly explained by strengthened community ties and neighborhood crime prevention measures.

5.6. Salience of Flood Map Updates

A wide range of studies tracing to [Gallagher \(2014\)](#) finds natural disasters such as floods have large salience effects - there is a strong response to natural disasters which returns to baseline over time. It is plausible to expect that people may pay more attention to flood map updates, which do not command large-scale attention when released, if they have already been primed to think about flood risk following a recent flooding disaster.

We test the hypothesis that formalized flood risk information should have greater impact when it arrives at a time when flood risk has already been made salient. We do this by leveraging the quasi-random variation in map updates. Because map development is a slow process, some updated maps will arrive in counties that have recently experienced a flooding disaster, while some maps arrive in counties which have never experienced a flooding disaster. We separately estimate the impact of updated maps for these two groups of counties. We define high-salience counties as those which experienced a physical flood shock in the five years preceding its map update. The low-salience group consists of counties which have never been flooded. [Figure 14](#) depicts the results. For both property crime and violent crime, the effect of map updates on property crime is much stronger in recently flooded counties.

The results shown in [Figure 14](#) suggest that new flood risk information is much more impactful when it arrives following a disaster. Of course, one major concern with interpreting the results in this manner is that [Figure 14](#) captures the combined effects of physical flooding with the map updates, while [Figure 13](#) shows that physical flooding affects crime, especially violent crime. Therefore, to estimate the effect of formalized flood risk information alone, we define event timing relative to the year a county receives a map update while controlling for a set of time dummies relative to the most recent flood.⁸ Results are shown in [Figure 15](#), which depicts four sets of event study coefficients. The blue and green series are identical to [Figure 13](#). The red and yellow series are the coefficient estimates when controlling for long- and short-term controls for physical flood timing respectively. Standard errors become very large as the effective sample size shrinks - there are

⁸specifically, we present event study results controlling for two sets of indicators for time from the most recent flood event. Short term controls span $t-2$ to $t+4$; long-term controls span $t-5$ to $t+10$.

very few counties which both received map updates before 2009 (and thus we observe ten years in the post-event period) AND also experienced a major flood in the preceding five years. The effect of map updates in recently-flooded counties is marginally attenuated for property crime in high salience counties, which is perhaps unsurprising given the relatively small effects of physical floods on property crime in Figure 14. Although noisy, the effect of map updates remains stronger in recently-flooded counties than never-flooded counties even when controlling for the dynamic effects of the physical flood, which points toward an important role for salience in amplifying the effect of map updates. The effect on violent crime is much more strongly attenuated by the physical flood controls, as we expect given the strong long-term effect of physical floods on violent crimes—there is no difference between high- and low-salience counties.

5.7. Controlling for Government Transfers

Another potential source of omitted variable bias could arise from disaster aid, along with more general forms of government assistance, which flow into counties following disasters (Deryugina, 2017). In Appendix Figure A1, we show there is no change in the number of claims or the volume of claim payouts following the map update, which supports our understanding of the map update as predominantly an informational treatment. Given the declines in employment, median income, and house values, we would expect government assistance to increase following map updates. We would also expect government assistance to be positively correlated with flood experience and negatively correlated with property crime, which could potentially generate the observed decrease in property crime.

We first establish that government assistance is indeed increasing following map updates. Figure 16 shows that transfers to individuals increase by 7% in the long run. This is the Bureau of Economic Analysis category that absorbs federal disaster relief. There is no statistically significant change in transfers to businesses, although the trend follows a slight increase after the map update.

The inclusion of disaster aid is important given the possible correlation between map updates and physical floods discussed above. In Appendix Figures A3 and A4, we show that government transfers are either flat or increasing across a broad range of individual categories such as SNAP, education and training assistance, and disability insurance. We then wish to test whether the observed decrease in property crime in response to map updates can be entirely explained by increases in government transfers to treated counties. We control for government transfers in

the event studies of Appendix Figure A5. Specifically, we re-estimate the Callaway–Sant’Anna specification with a BEA transfer aggregate added as a covariate, entered as the log of real per-capita (2019 USD) receipts. We report this control in two forms: its fixed base-year (2000) value and its contemporaneous, time-varying value; Figure A5 does so for government transfers to individuals and for total personal transfer receipts. The main effect of these controls is to decrease precision. Effects of map updates on property crime are no longer statistically different zero; however, the standard errors are too wide to draw any meaningful conclusions, although the overall pattern of effects remains similar. We conduct similar robustness checks by controlling for alternative transfer measures separately, including retirement and medical benefits, medicare and medicaid, income maintenance benefits, supplemental security income, SNAP, other income maintenance, unemployment insurance, and veteran’s benefits. We report the results in Appendix Figures A6 to A10. The findings are similar, these estimates are not precise enough for us to draw any conclusions regarding the effect of NFIP update on crime, conditional on government transfers.

6. Conclusion

How does updated information on disaster risk affect crime and long-term economic outcomes? We examine this question through the lens of flood risk. Using plausibly exogenous variation in flood risk map updates from the NFIP, we show that the provision of formalized flood risk information causes long-run reductions in property crime, even as the effects of physical floods are relatively short-lived. The effect of risk information is greatly enhanced when it arrives after recent floods at a time when disaster risk has been made particularly salient.

We then investigate the channels through which updated flood risk information plausibly affects crime. We corroborate prior studies which show that flood insurance availability drives population increases in risky floodplains, and that house prices, income, and homeownership decline when updated flood risk information becomes available. We provide new evidence that flood risk information also changes the composition of businesses in these communities: the number of small businesses and retail establishments increases, there is a more diverse range of businesses, and employment declines are driven entirely by the service sector.

The set of results is suggestive of an eyes-on-the-street theory of crime reduction. We show supporting evidence for this conclusion by demonstrating that most factors related to changes in property crime such as the number of police officers, availability of targets, and opportunity cost

of crime move in the opposite direction that would predict declines in property crime; business diversity and the number of small businesses move in the correct direction, and both of these factors are associated with increased walkability of neighborhoods.

Our results highlight the many long-term effects of flood risk and the provision of information by establishing an important link between the economics of natural disasters and the economics of crime. Policymakers should anticipate wide-ranging impacts on communities when considering when and how to optimally provide information on disaster risk.

7. Declaration of Generative AI Use

Statement: During the preparation of this work the authors used Claude Code and ChatGPT in order to organize and format Latex tables. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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Tables

Table 1. Summary Statistics, Full Sample: Crime, Demographic, Housing, Labour-Market and Activity Variables

Variable	N	Mean	SD	Min	Max
<i>Panel A: Crime outcomes</i>					
Property crime rate (per 100k)	62,657	1,880.67	1,403.92	0.00	75,063.72
Violent crime rate (per 100k)	62,657	956.23	820.19	-20.71	18,548.14
<i>Panel B: Demographics and housing</i>					
Population	62,815	99,584	346,665	0	10,181,378
Median household income	62,689	43,760	12,688	15,025	151,806
Median housing value (1-year ACS)	16,660	192,476	115,156	49,400	1,233,600
Owner-occupied share (1-year ACS)	16,660	0.606	0.092	0.171	0.855
<i>Panel C: Labour market</i>					
Unemployment rate	62,466	5.983	2.665	1.100	29.100
Total employment	62,671	37,105	136,729	0	4,007,163
Service employment	62,596	30,848	118,857	0	3,531,915
Manufacturing employment	62,671	4,063	12,944	0	608,230
Employment rate, young males 20-34	34,490	71.987	14.219	2.052	100.000
Wage, non-college males	61,589	3,424	867	926	19,215
Gini index	31,353	0.440	0.036	0.200	0.707
<i>Panel D: Policing, commercial activity and time use</i>					
Sworn full-time officers	62,815	237.00	1,101.97	0.00	40,477.00
Commercial diversity (entropy-3D)	62,568	3.380	0.383	0.000	3.779
Amenity share (broad)	62,671	0.127	0.035	0.000	1.000
Total establishments	62,658	2,329.6	7,941.7	1.0	285,383.0
Average firm size	62,658	12.11	4.75	0.00	93.30
Outdoor activity, minutes per day (ATUS)	4,718	11.81	19.14	0.00	300.00
Non-household childcare, minutes per day (ATUS)	4,718	13.43	28.10	0.00	420.67
<i>Panel E: Flood exposure and NFIP claims</i>					
Flood property damage	62,815	1,497,628	75,569,760	0	10000511000
NFIP claim payouts	62,815	957,093	41,756,709	0	6682758806
NFIP claim count	62,815	17.39	433.90	0.00	59,999.00
<i>Panel F: Baseline (year-2000) covariates</i>					
Baseline population	62,800	91,190	322,407	0	9,519,338
Baseline median income	62,640	36,361	9,006	15,231	91,210
Baseline poverty rate	62,640	13.305	5.590	1.700	42.200
Baseline establishments	62,600	6.4	1.5	0.7	12.3
Baseline average firm size	62,280	2.42	0.40	0.34	4.54
Baseline average wage	62,280	10	0	9	12
Counties	3141				
County-years	62,815				

Notes: The sample is the full county-year panel, 2000–2019, pooling ever-treated and never-treated counties. All variables are on their original scale; the analysis uses logs of population, median income, housing value, wages, officers, establishments, firm size and flood damage. Baseline values are constant within county. Transfer and disaster-aid variables are reported in Table 2. N varies across rows because the auxiliary panels (ACS, CBP, UCR, ATUS, BEA) cover different county-years.

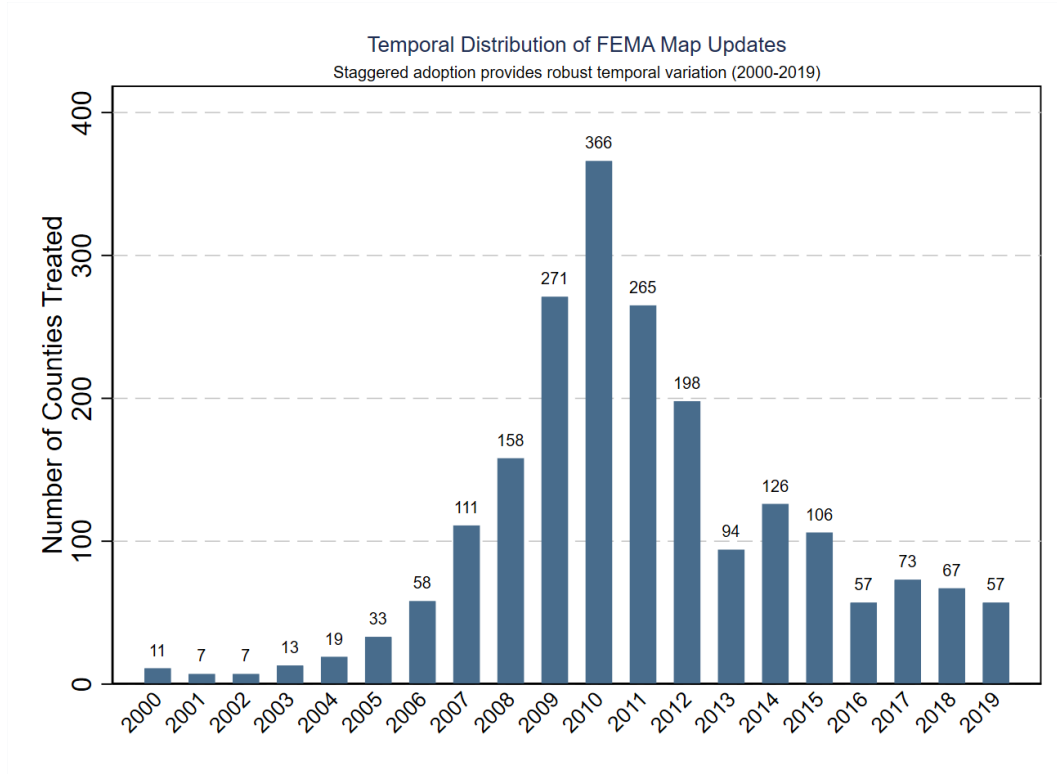
Table 2. Summary Statistics, Full Sample: Government Transfers and Disaster Aid

Variable	N	Mean	SD	Min	Max
<i>Panel A: Direct disaster aid (per capita, 2019 USD)</i>					
Direct disaster aid per capita	61,556	25.06	381.68	-5.04	43,218.69
Baseline (2000) direct disaster aid per capita	61,540	11.77	71.54	-0.15	1,254.30
<i>Panel B: BEA transfer receipts (per capita, 2019 USD)</i>					
Total personal transfer receipts	61,520	8,481	4,278	1,422	261,655
Government transfers to individuals	61,520	8,256	4,230	1,133	258,568
Retirement and disability insurance	61,520	3,099	932	320	27,178
Medical benefits	61,520	3,637	2,370	281	165,124
Medicare	61,520	2,035	774	145	24,023
Public assistance medical care (Medicaid)	61,520	1,560	1,932	0	140,703
Income maintenance benefits	61,520	782	788	41	54,869
Supplemental Security Income	61,520	172	150	0	5,796
SNAP / food stamps	61,520	182	216	0	13,662
Other income maintenance, incl. TANF	61,520	218	409	13	33,169
Unemployment insurance	61,520	177	194	0	11,653
Veterans' benefits	61,520	275	197	4	6,054
Education and training assistance	61,520	148	127	13	2,484
Other gov. transfers, incl. disaster relief	61,520	139	536	0	50,578
Transfers from businesses	61,520	91	47	5	2,270
<i>Panel C: Baseline (year-2000) per-capita value of each transfer category (2019 USD)</i>					
Total personal transfer receipts	61,440	6,022	3,105	1,422	152,491
Government transfers to individuals	61,440	5,737	3,040	1,133	148,808
Retirement and disability insurance	61,440	2,492	665	320	13,089
Medical benefits	61,440	2,274	1,368	281	65,844
Medicare	61,440	1,186	365	184	4,920
Public assistance medical care (Medicaid)	61,440	1,071	1,210	0	60,759
Income maintenance benefits	61,440	549	531	44	24,297
Supplemental Security Income	61,440	156	153	0	5,205
SNAP / food stamps	61,440	85	133	0	6,308
Other income maintenance, incl. TANF	61,440	130	215	13	10,789
Unemployment insurance	61,440	107	122	0	5,173
Veterans' benefits	61,440	161	94	7	2,564
Education and training assistance	61,440	100	69	13	1,164
Other gov. transfers, incl. disaster relief	61,440	53	841	0	37,489
Transfers from businesses	61,440	165	44	32	2,130
Counties	3141				
County-years	62,815				

Notes: The sample is the full county-year panel, 2000–2019, pooling ever-treated and never-treated counties. All variables are real (2019 USD) per-capita amounts; the analysis uses the inverse hyperbolic sine of per-capita disaster aid and $\ln(1 + x)$ of each per-capita transfer category. Baseline values are constant within county. Outcome and covariate variables are reported in Table 1. N varies across rows because the auxiliary panels (ACS, CBP, UCR, ATUS, BEA) cover different county-years.

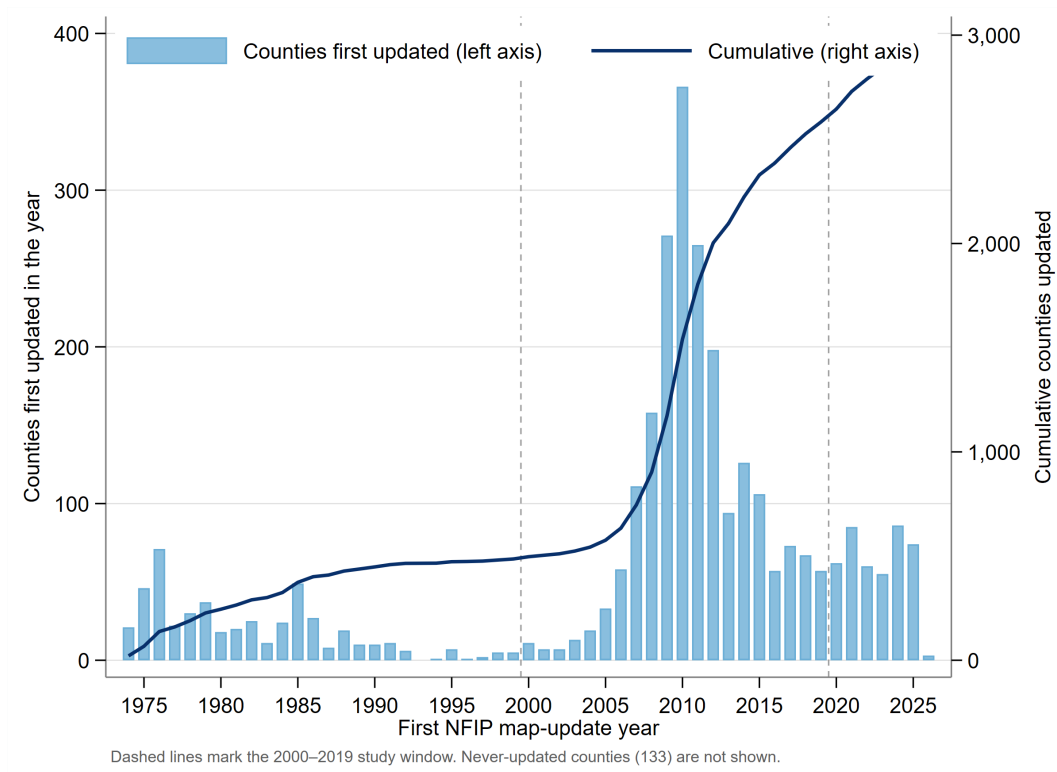
Figures

Figure 1. Temporal Distribution of FEMA Map Updates, 2000–2019



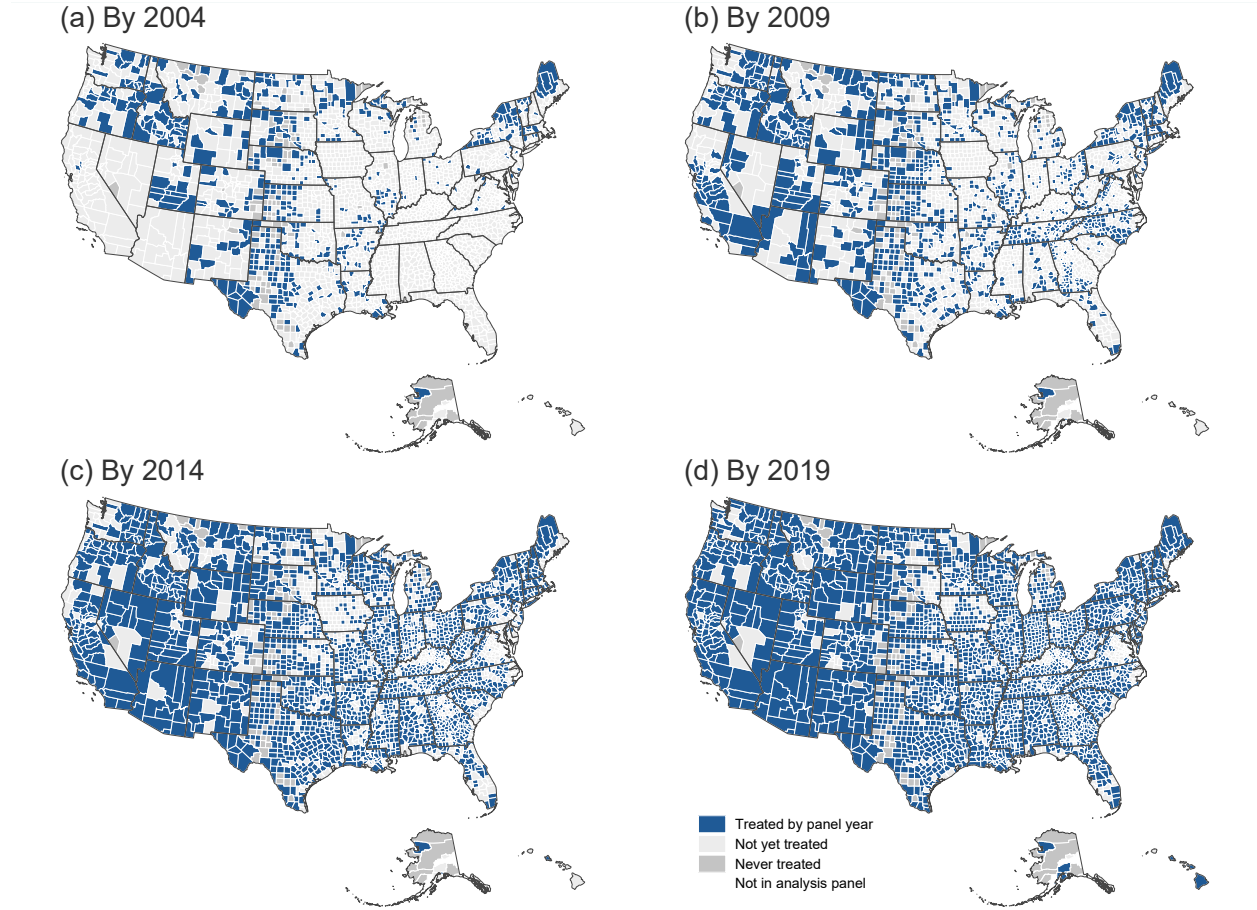
Notes: Each bar reports the number of counties whose first FEMA flood-map (FIRM) update occurred in that year. Every county is counted exactly once, in its own map-update year, and the sample covers first updates between 2000 and 2019. Because these updates are spread across many years rather than concentrated in a single one, treatment timing is staggered; this variation in the timing of map updates is what identifies the effect of updated flood-risk information in the difference-in-differences design. The underlying counts are reported in Table [A1](#).

Figure 2. First FEMA Map Updates by Year, 1974–2026, and Cumulative Count



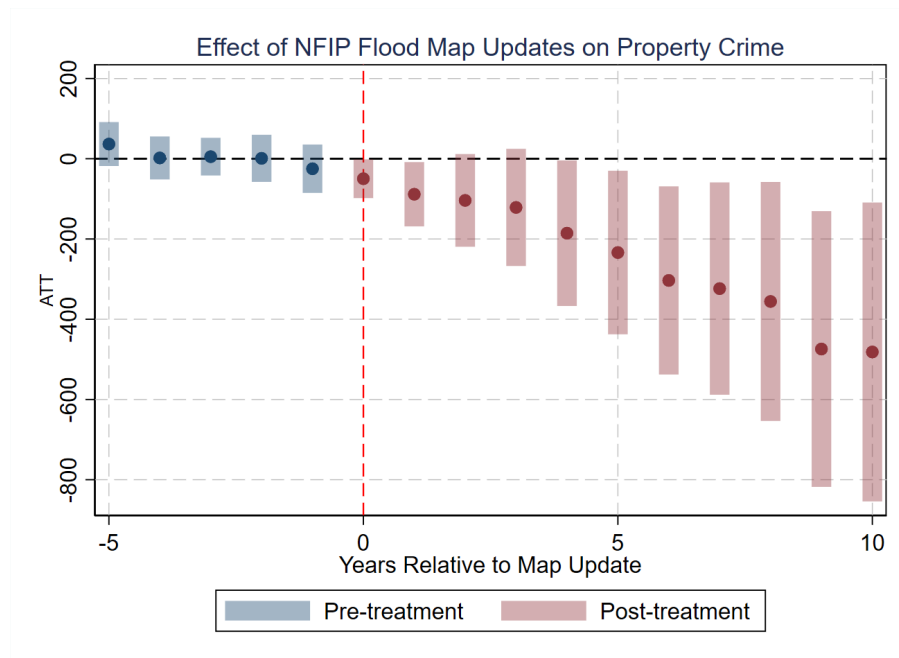
Notes: The figure places the study window in the full history of the NFIP: bars report the number of counties receiving their first map update in each year from 1974 onward (left axis) and the line reports the cumulative number of counties updated (right axis). Dashed lines mark the 2000–2019 study window; the 133 counties that never receive an update are not shown. The 2000–2019 cohorts are shown in Figure 1.

Figure 3. Spatial Rollout of FEMA Map Updates, 2000–2019



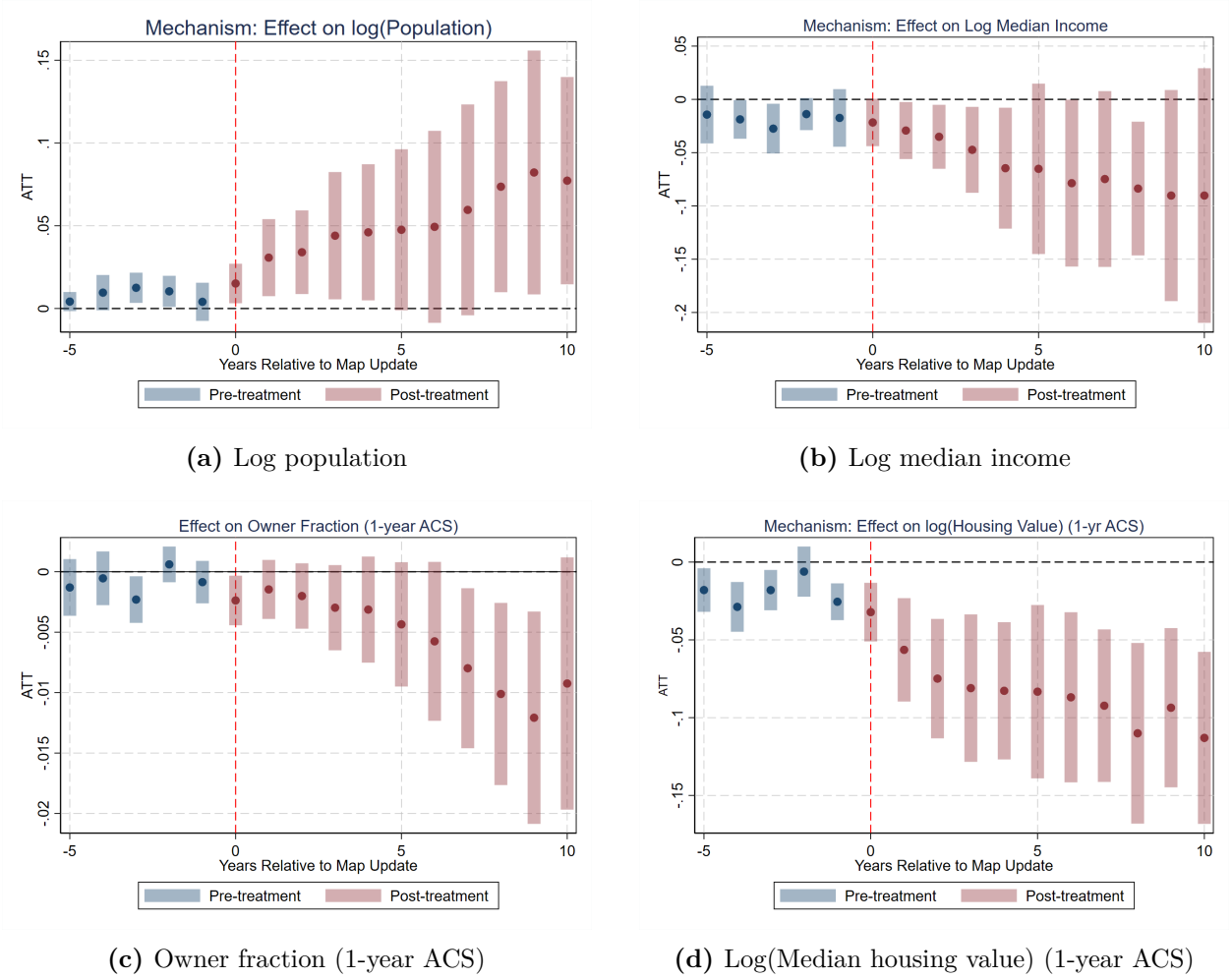
Notes: Each panel maps the counties that had received their first FEMA flood-map (FIRM) update by the year shown: (a) 2004, (b) 2009, (c) 2014, and (d) 2019. Dark blue counties are treated by the panel year, light counties have not yet been treated by that year, and gray counties never receive a map update during the sample period; counties outside the analysis panel are left white. Alaska and Hawaii are shown as insets. Treatment spreads across all regions of the country rather than being confined to a few states, and by 2019 most counties in the sample have received an updated map. The corresponding annual counts are shown in Figure 1, and the counts and year-2000 population shares by Census region are reported in Table A19.

Figure 4. Effect of NFIP Flood Map Updates on Property Crime



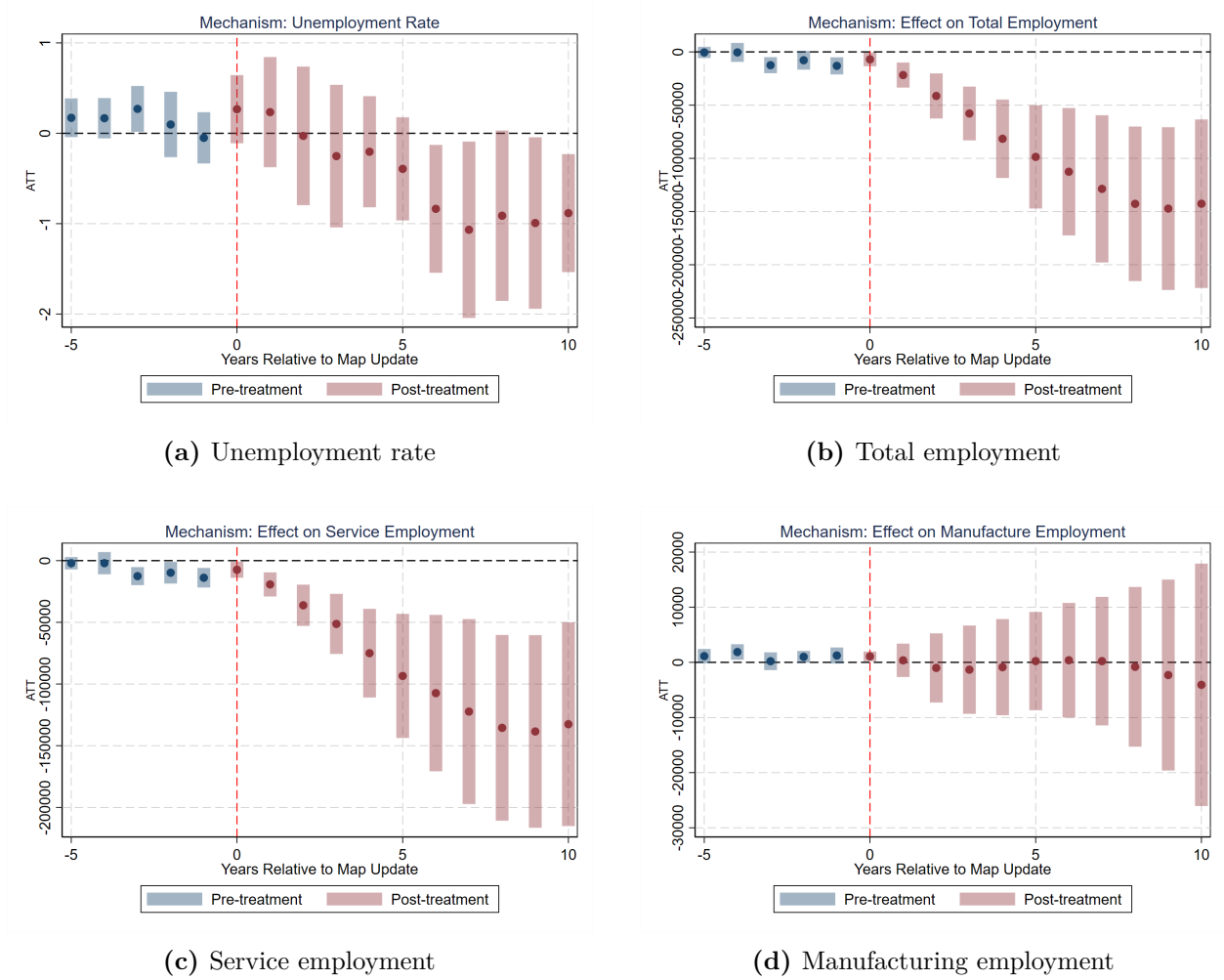
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on the property crime rate, obtained from the CS-DID estimator of [Callaway and Sant’Anna \(2021\)](#). The property crime rate is the combined number of burglaries, thefts, and motor vehicle thefts per 100,000 county residents. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in column (1) of Table [A5](#).

Figure 5. Effect of NFIP on Economic Outcomes Part 1



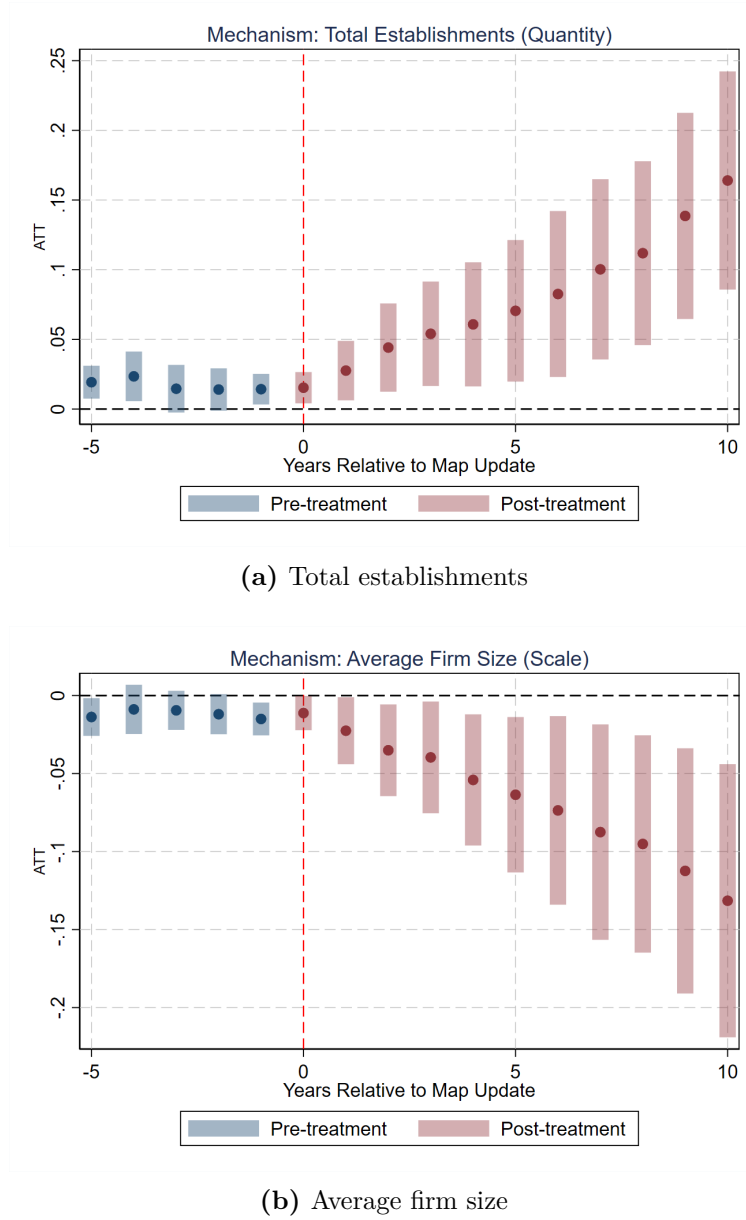
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on economic outcomes, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Panel (a) reports the effect on log population, controlling for base-year (2000) population, income, and poverty rate. Panel (b) reports the effect on log median income, controlling for base-year (2000) establishments, wages, average firm size, poverty rate, and income. Panels (c) and (d) report the effects on the fraction of owner-occupied homes and on log(median housing value); both use 1-year ACS data, which is published only for high-population (exceeding 65,000) counties. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table A6.

Figure 6. Effect of NFIP on Economic Outcomes Part 2



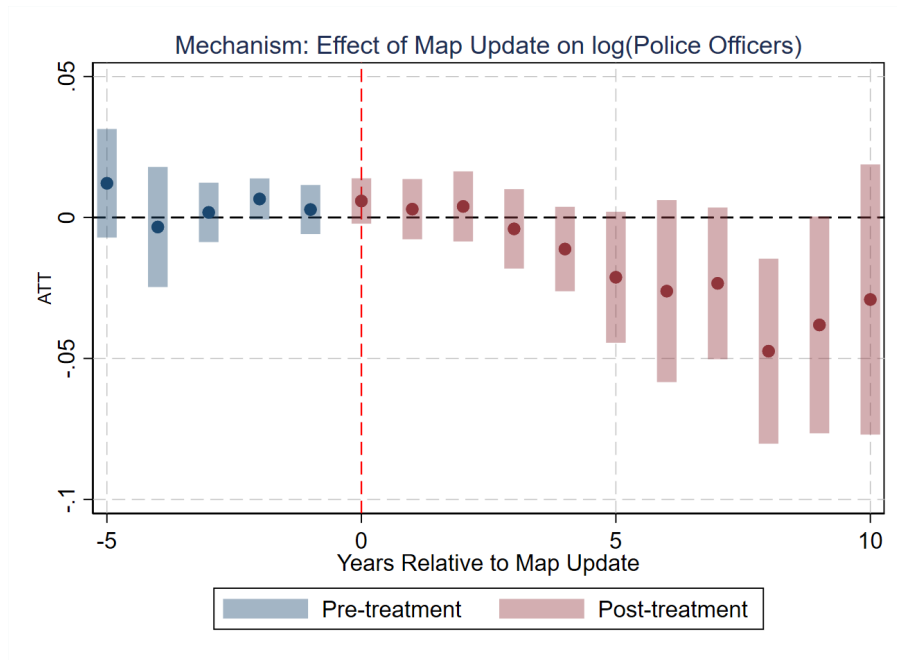
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on employment outcomes, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Panel (a) reports the effect on the unemployment rate, controlling for base-year (2000) income, poverty rate, establishments, and average firm size. Panel (b) reports the effect on total employment, and panels (c) and (d) report the effects on service and manufacturing employment. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table [A7](#).

Figure 7. Effect of NFIP on Establishment Count and Firm Size



Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on the total number of establishments and on average firm size, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Both panels control for base-year (2000) income and poverty rate. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table A8.

Figure 8. Effect of map update on log(Police officers)



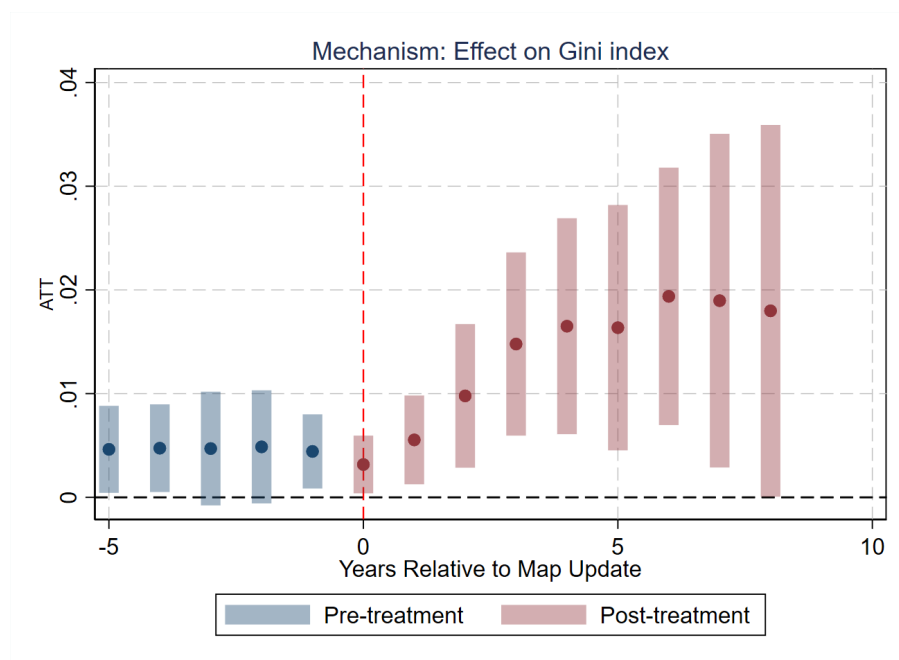
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on the log number of police officers, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in column (1) of Table [A9](#).

Figure 9. Young Male Economic Opportunity



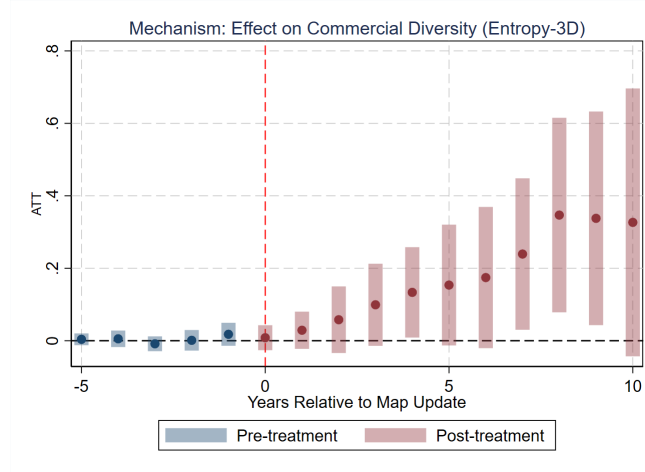
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on economic opportunity for young men, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Panel (a) reports the effect on the employment rate of young men (aged 19–34), and panel (b) reports the effect on the log wage of men without a college degree. Both panels control for base-year (2000) income, poverty rate, total establishments, and average firm size. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (2) and (3) of Table [A9](#).

Figure 10. Effect on the Gini Index

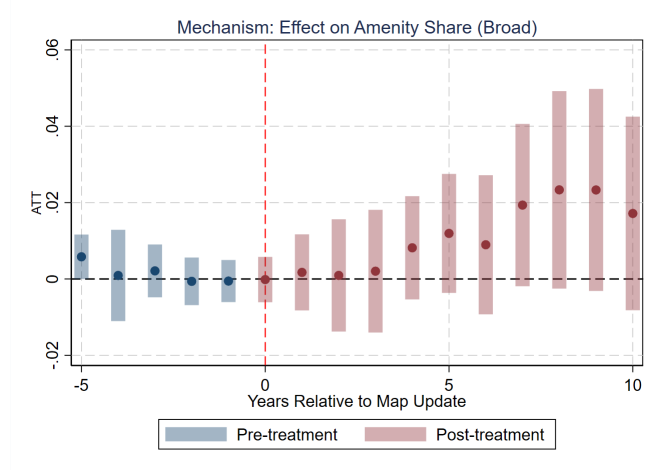


Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on the Gini index, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in column (4) of Table [A9](#).

Figure 11. Mechanisms: Eyes on the Street



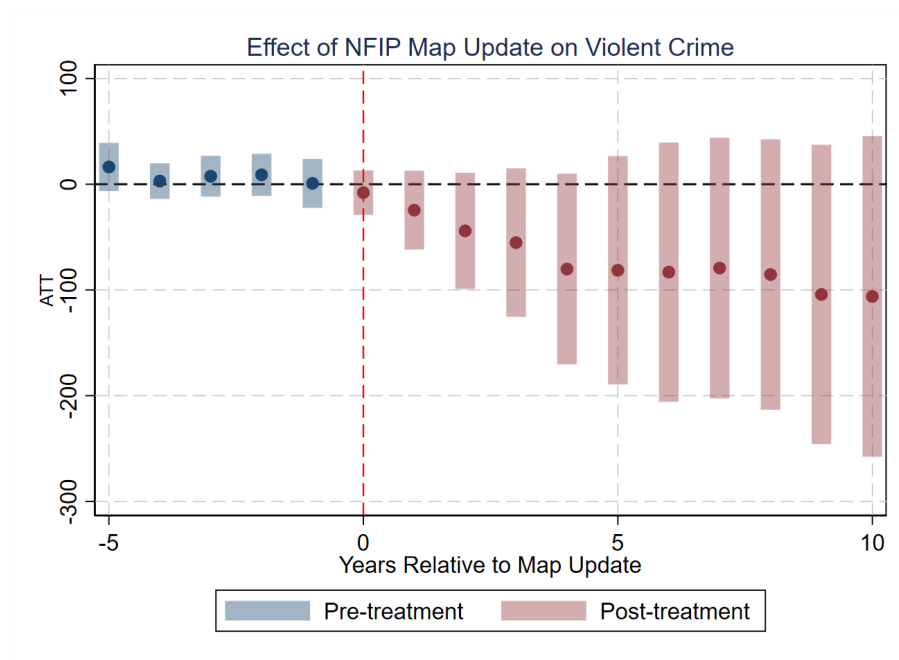
(a) Commercial diversity (entropy-3D)



(b) Amenity share (broad)

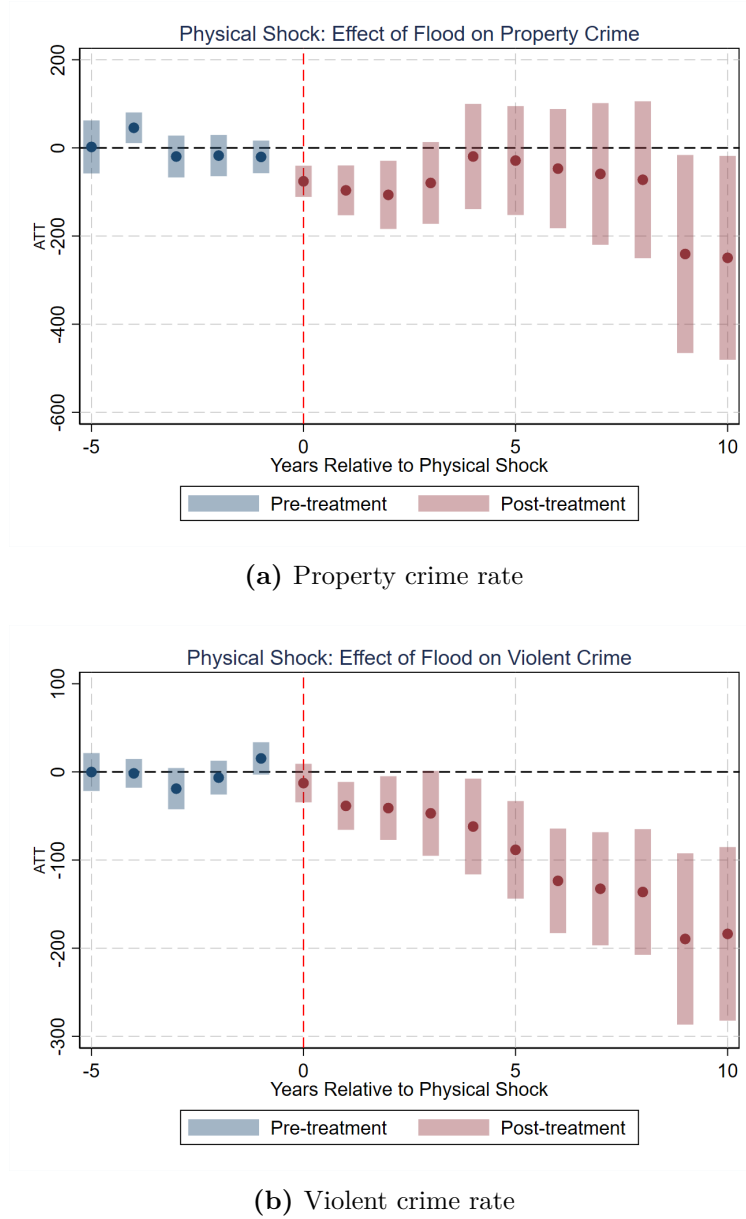
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on measures of local social activity, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). The two panels together test the eyes-on-the-street mechanism, under which a greater presence of people in public space deters crime. Panel (a) reports the effect on commercial diversity, measured by the Shannon entropy of establishments across three-digit NAICS subsectors, which rises as establishments are spread more evenly over more subsectors. Panel (b) reports the effect on the amenity share, the fraction of a county's establishments in consumer-facing, foot-traffic sectors. Both panels control for base-year (2000) income, poverty rate, establishments, and average firm size. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (1) and (2) of Table [A10](#).

Figure 12. Effect of NFIP Map Update on Violent Crime



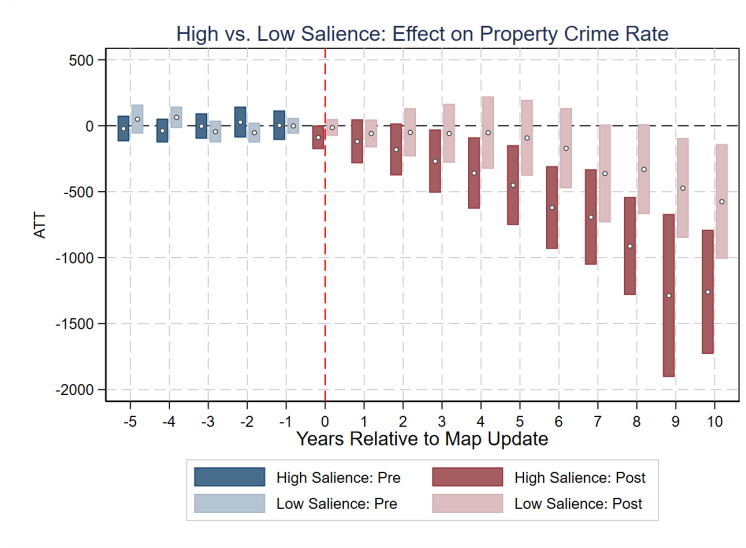
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on the violent crime rate, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). The violent crime rate is the combined number of murders, rapes, robberies, and assaults per 100,000 county residents. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in column (2) of Table [A5](#).

Figure 13. Effect of the Physical Flood Shock on Crime

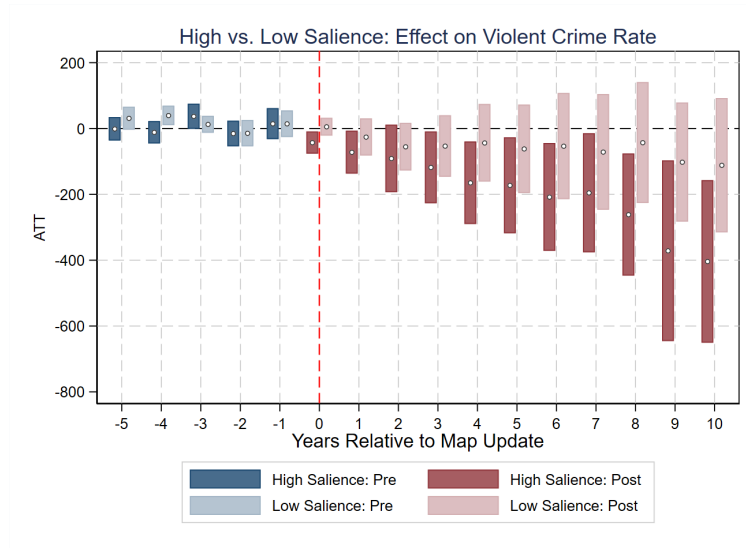


Notes: The figure plots event-study estimates of the effect of the first physical flood, rather than of the map update, on crime, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#); event time is therefore measured relative to the physical shock. Panel (a) reports the effect on the property crime rate and panel (b) the effect on the violent crime rate, each per 100,000 county residents. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table [A12](#).

Figure 14. High vs. Low Salience: Effect on Crime



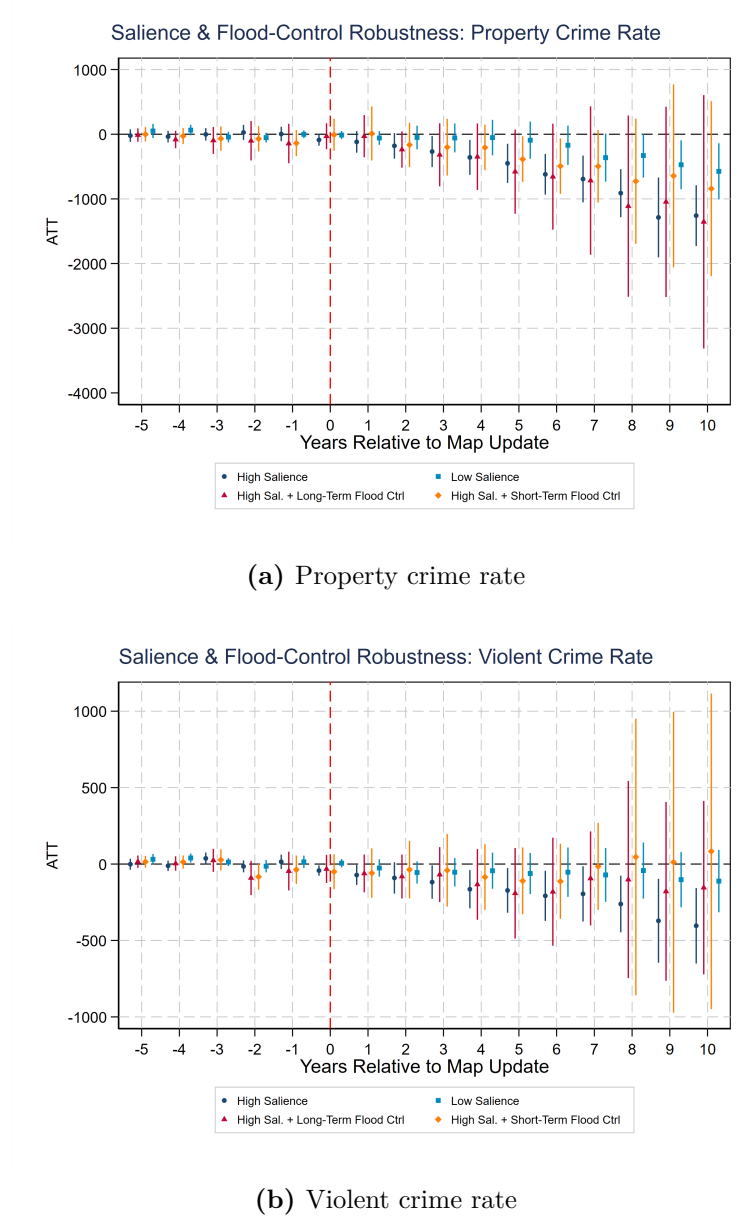
(a) Property crime rate



(b) Violent crime rate

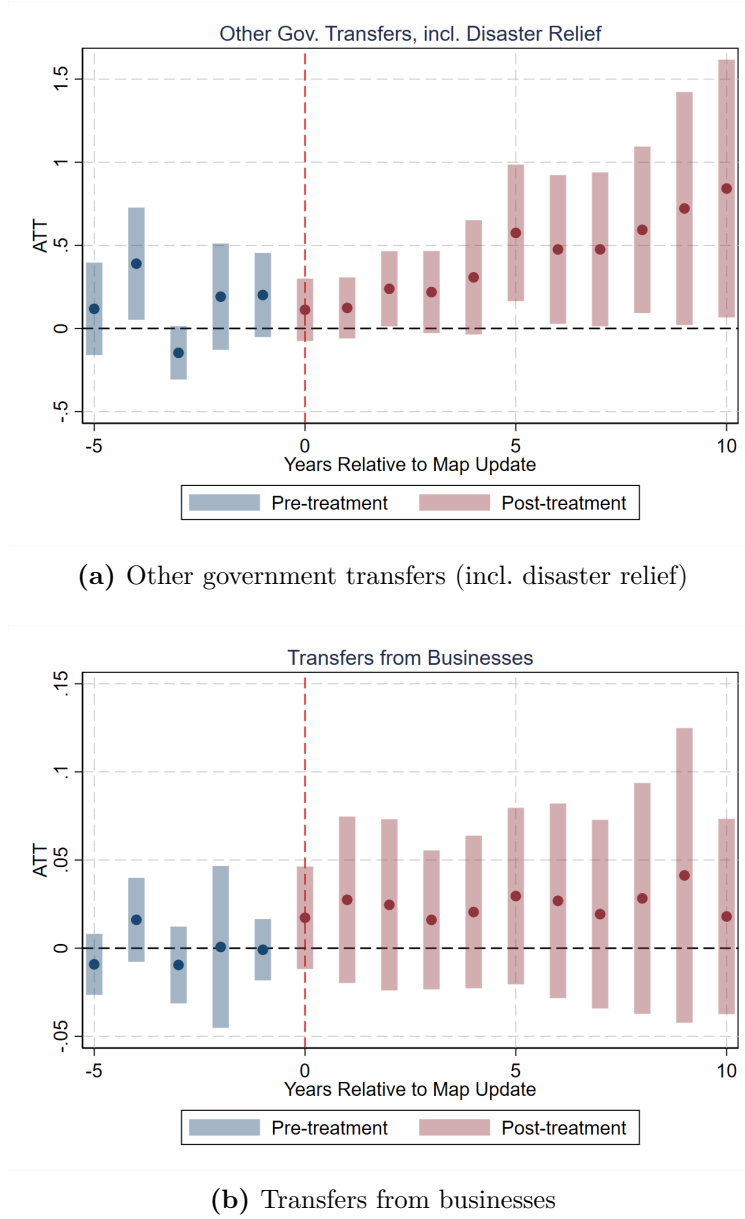
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on crime, estimated separately for two groups of counties, using the CS-DID estimator of [Callaway and Sant’Anna \(2021\)](#). A county belongs to the high-salience group if it experienced a physical flood shock in the one to five years before its map update, and to the low-salience group if it was never flooded. Panel (a) reports the effect on the property crime rate and panel (b) the effect on the violent crime rate. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in [Table A11](#).

Figure 15. Saliency and Flood-Control Robustness (Controlling for Physical Flood)



Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on crime for high- and low-saliency counties, and for high-saliency counties with long- and short-term controls for the physical flood, obtained from the CS-DID estimator of [Callaway and Sant’Anna \(2021\)](#). Long-term flood controls span the physical-flood path from $t-5$ to $t+10$ and short-term controls span $t-2$ to $t+4$, with $t-1$ the omitted baseline in both cases. Panel (a) reports the effect on the property crime rate and panel (b) the effect on the violent crime rate, each per 100,000 county residents. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in [Table A13](#).

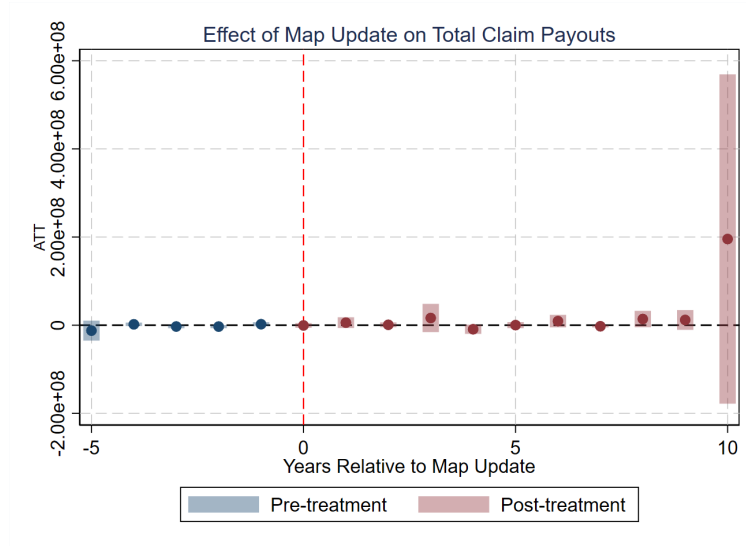
Figure 16. Effect of NFIP on Other Government and Business Transfers



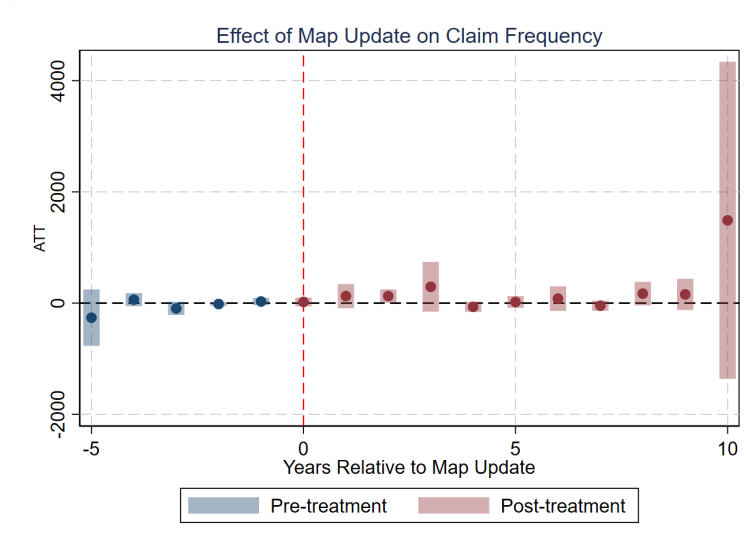
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on government and business transfer receipts, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Both outcomes are the log of real (2019 USD) per-capita transfer receipts plus one, and panel (a) is the BEA line that absorbs federal disaster relief. Both panels control for base-year (2000) income, poverty rate, establishments, and average firm size. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Panel A, columns (1) and (3), of Table [A15](#).

Appendix Figures

Figure A1. Effect of Map Updates on Flood Insurance Claims



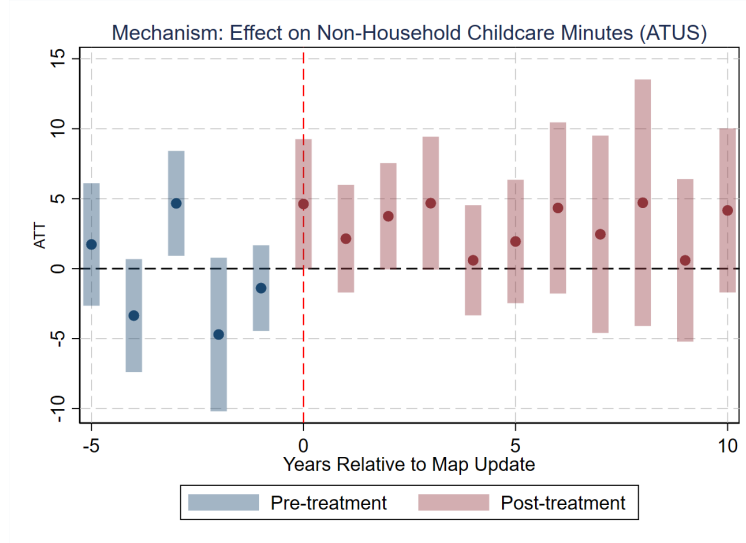
(a) Total claim payouts



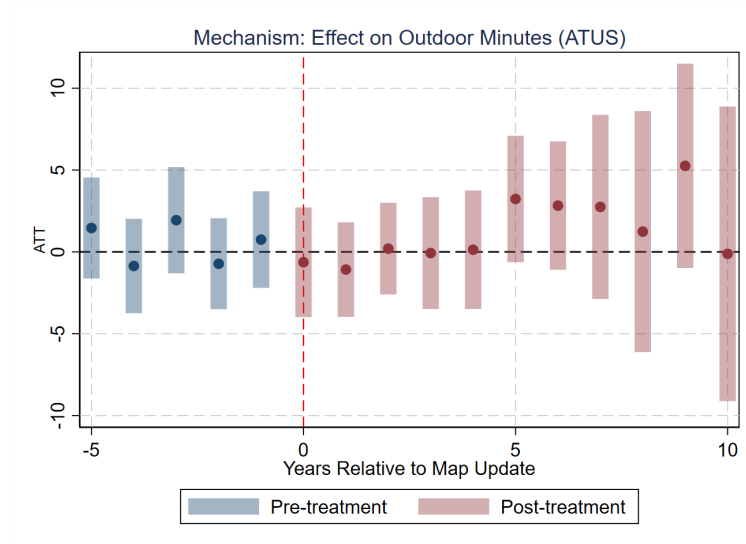
(b) Claim frequency

Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on flood insurance claims, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Panel (a) reports the effect on total claim payouts and panel (b) the effect on claim frequency. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table [A16](#).

Figure A2. Mechanisms: Eyes on the Street – ATUS



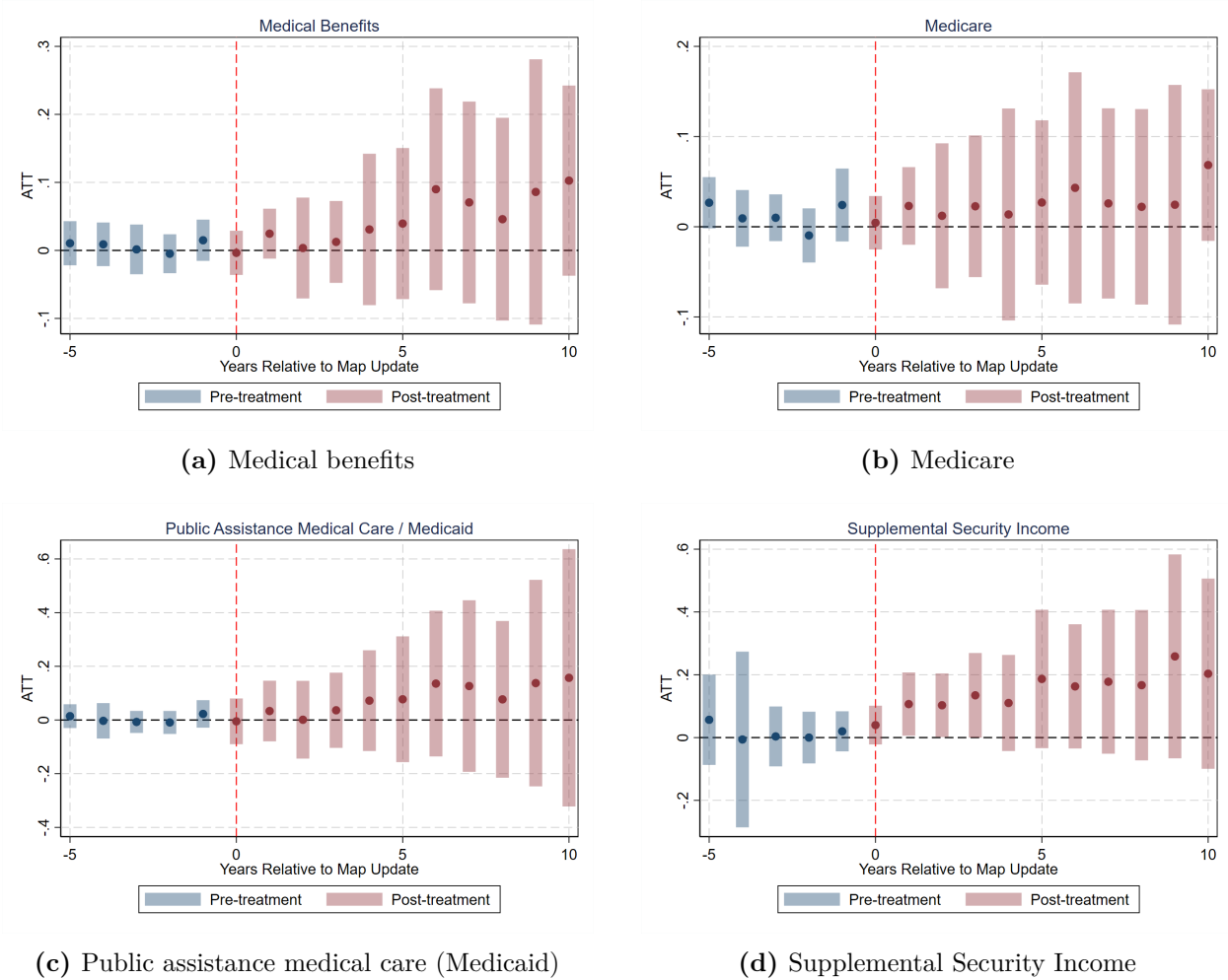
(a) Non-household childcare minutes (ATUS)



(b) Outdoor minutes (ATUS)

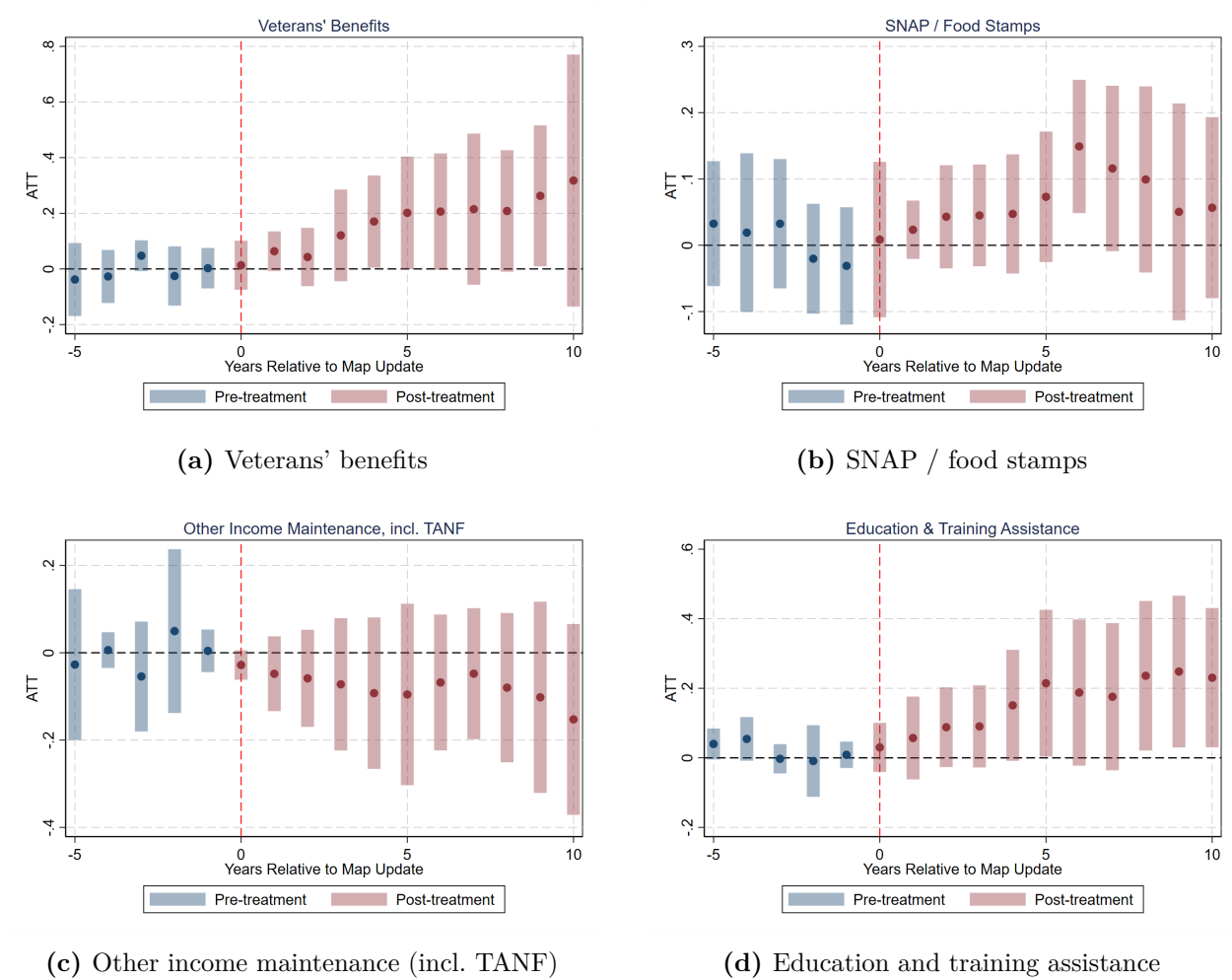
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on county-average daily time use from the American Time Use Survey (ATUS), obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). Panel (a) reports the effect on average minutes per day spent caring for non-household children and panel (b) the effect on average minutes per day spent on outdoor activity. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (3) and (4) of Table A10.

Figure A3. Effect of NFIP on Government Transfer Income Part 1



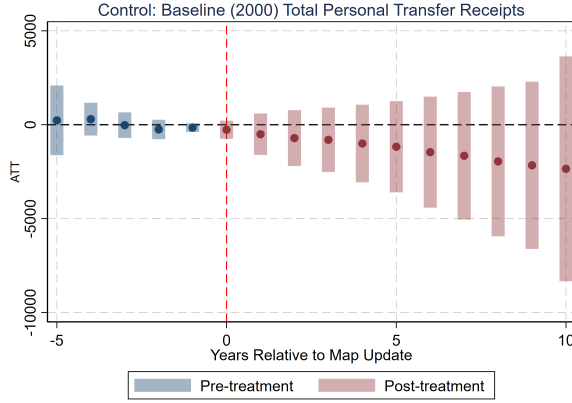
Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on medical and disability transfer receipts, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). All outcomes are the log of real (2019 USD) per-capita transfer receipts plus one, and all four panels control for base-year (2000) income, poverty rate, establishments, and average firm size. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table A15, columns (2) and (4) of Panel A and columns (7) and (8) of Panel B.

Figure A4. Effect of NFIP on Government Transfer Income Part 2

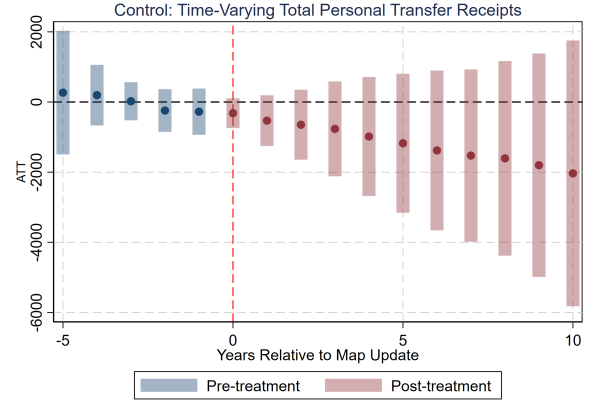


Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on income-support and other transfer receipts, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#). All outcomes are the log of real (2019 USD) per-capita transfer receipts plus one, and all four panels control for base-year (2000) income, poverty rate, establishments, and average firm size. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in Table [A15](#), column (5) of Panel A and columns (6), (9), and (10) of Panel B.

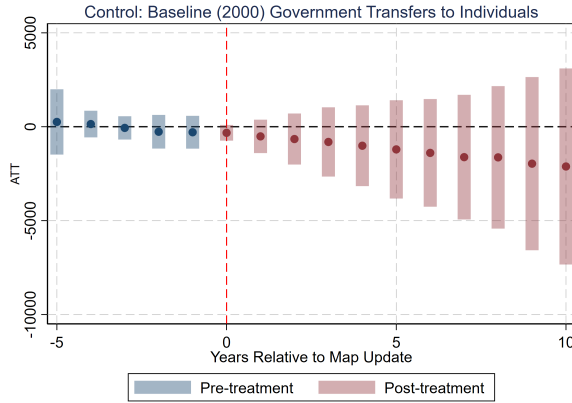
Figure A5. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Aggregate Transfers



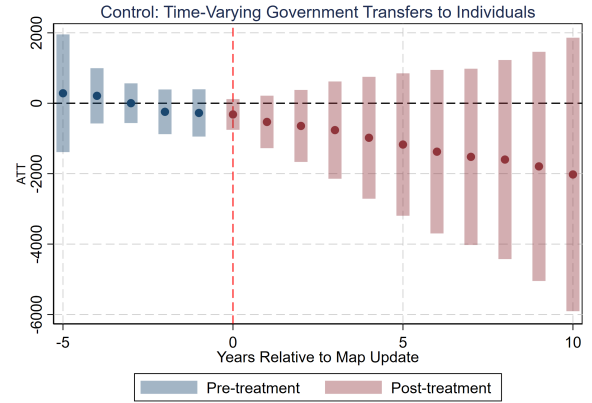
(a) Total personal transfer receipts, base-year control



(b) Total personal transfer receipts, time-varying control



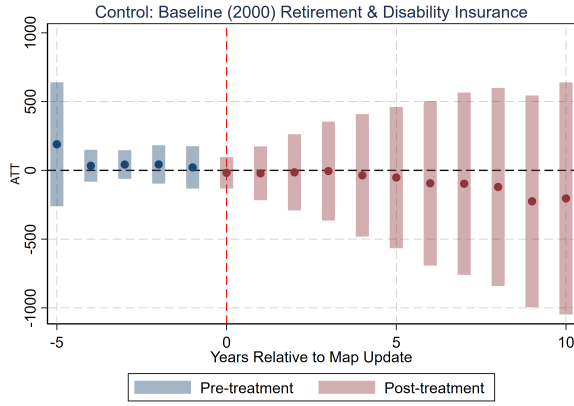
(c) Government transfers to individuals, base-year control



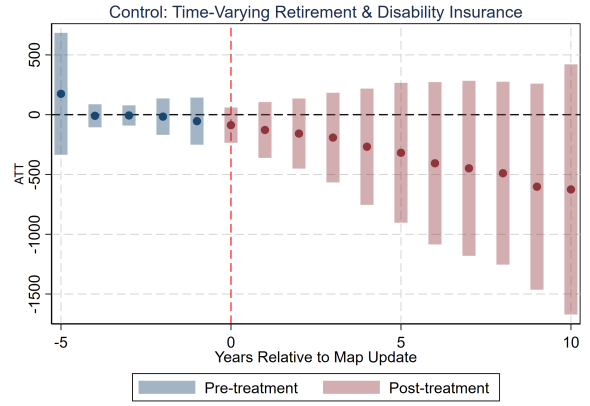
(d) Government transfers to individuals, time-varying control

Notes: Each panel plots CS-DID (Callaway and Sant'Anna, 2021) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (1) and (2) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

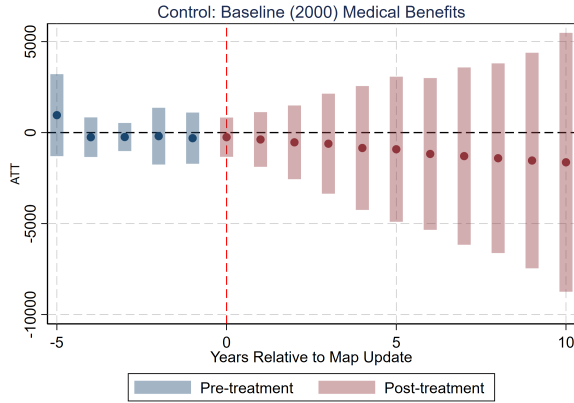
Figure A6. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Retirement and Medical Benefits



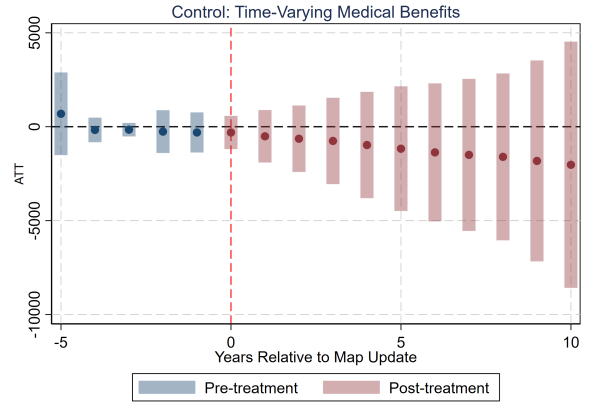
(a) Retirement and disability insurance, base-year control



(b) Retirement and disability insurance, time-varying control



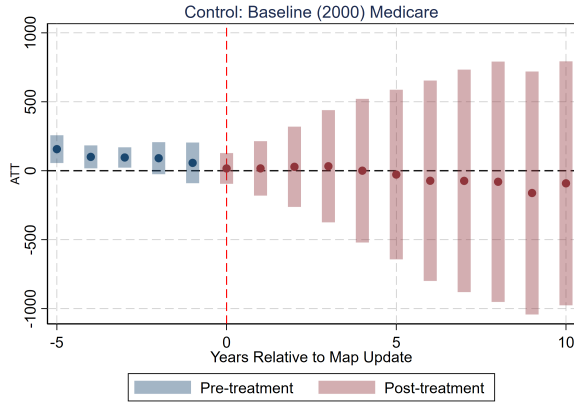
(c) Medical benefits, base-year control



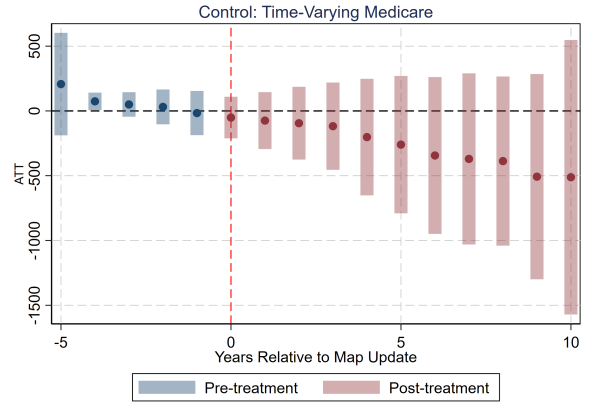
(d) Medical benefits, time-varying control

Notes: Each panel plots CS-DID (Callaway and Sant’Anna, 2021) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (3) and (4) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

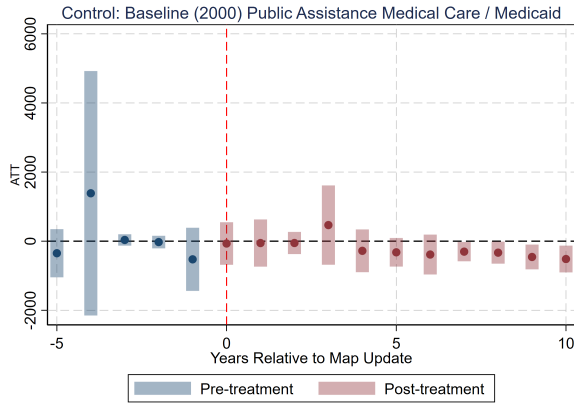
Figure A7. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Medicare and Medicaid



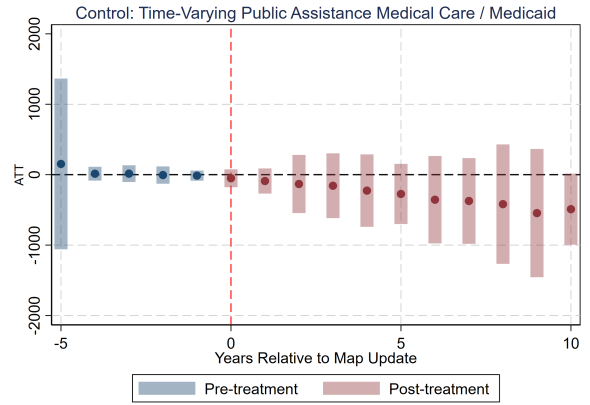
(a) Medicare, base-year control



(b) Medicare, time-varying control



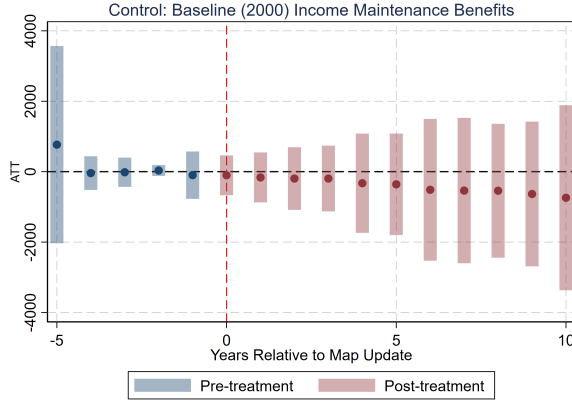
(c) Public assistance medical care (Medicaid), base-year control



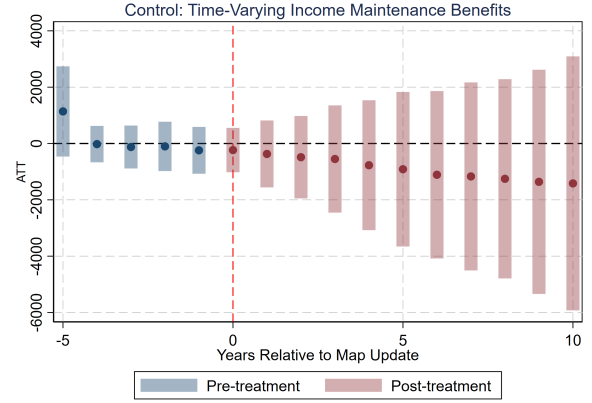
(d) Public assistance medical care (Medicaid), time-varying control

Notes: Each panel plots CS-DID ([Callaway and Sant'Anna, 2021](#)) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (5) and (6) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

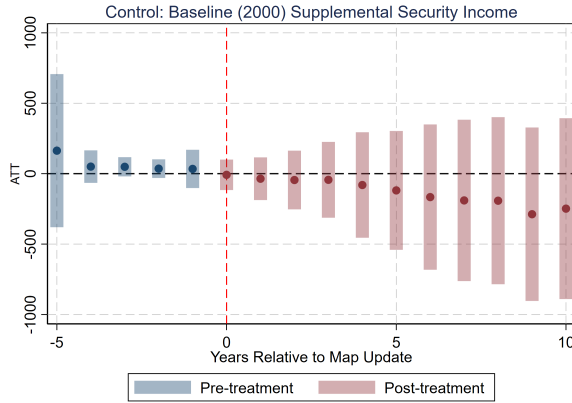
Figure A8. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Income Maintenance and SSI



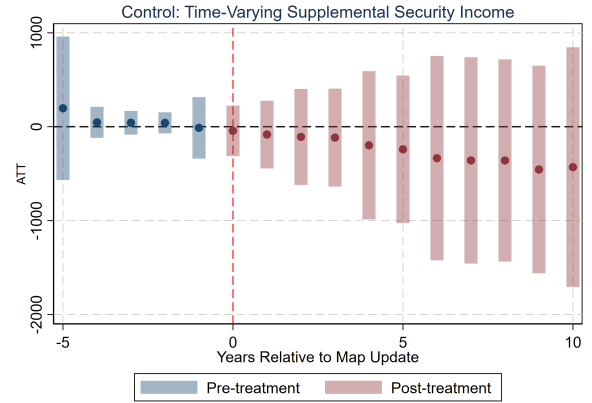
(a) Income maintenance benefits, base-year control



(b) Income maintenance benefits, time-varying control



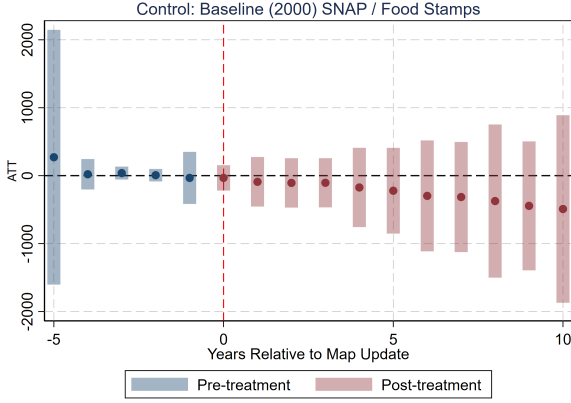
(c) Supplemental Security Income, base-year control



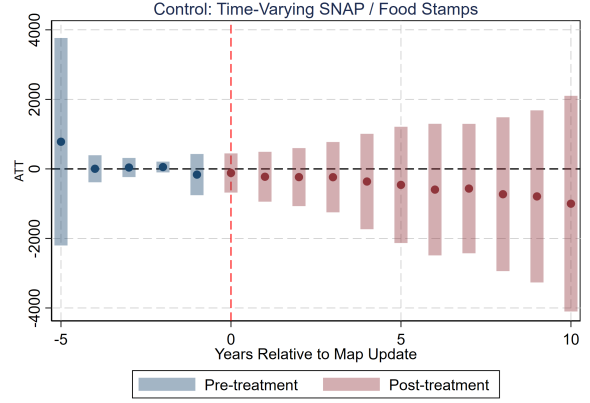
(d) Supplemental Security Income, time-varying control

Notes: Each panel plots CS-DID (Callaway and Sant'Anna, 2021) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (7) and (8) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

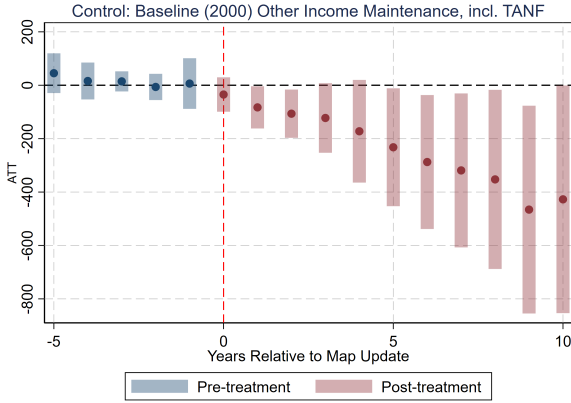
Figure A9. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: SNAP and Other Income Maintenance



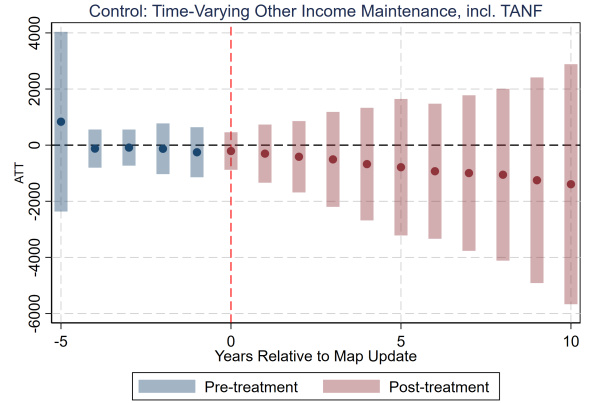
(a) SNAP / food stamps, base-year control



(b) SNAP / food stamps, time-varying control



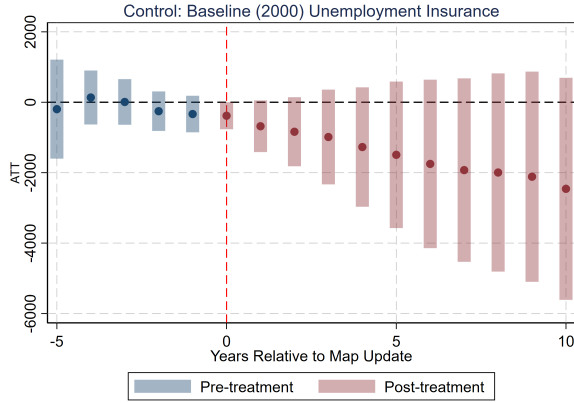
(c) Other income maintenance, incl. TANF, base-year control



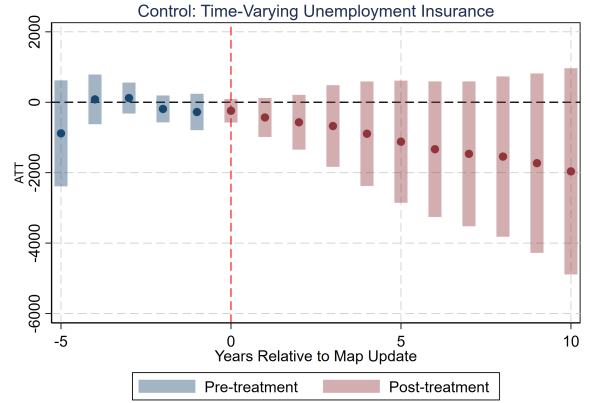
(d) Other income maintenance, incl. TANF, time-varying control

Notes: Each panel plots CS-DID (Callaway and Sant'Anna, 2021) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (9) and (10) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

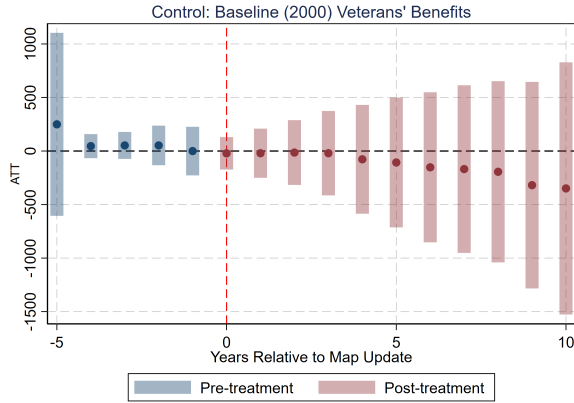
Figure A10. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Unemployment Insurance and Veterans' Benefits



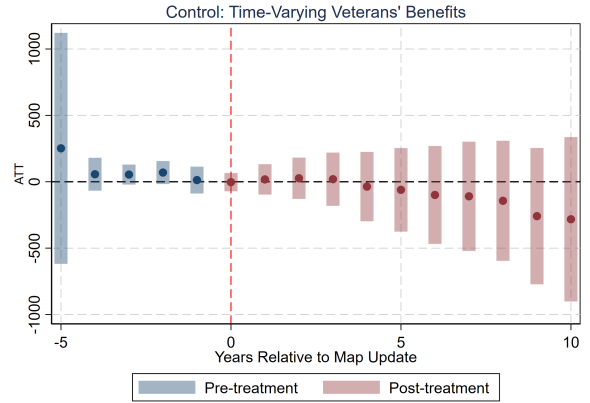
(a) Unemployment insurance, base-year control



(b) Unemployment insurance, time-varying control



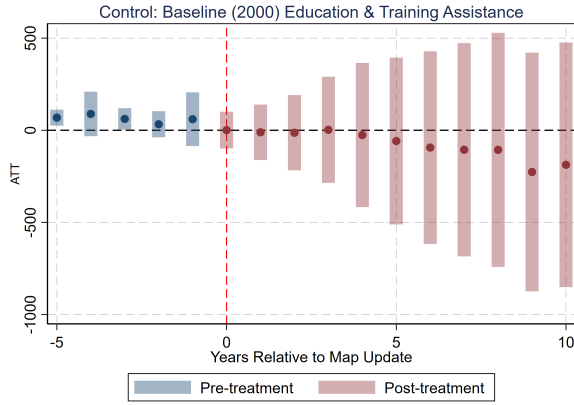
(c) Veterans' benefits, base-year control



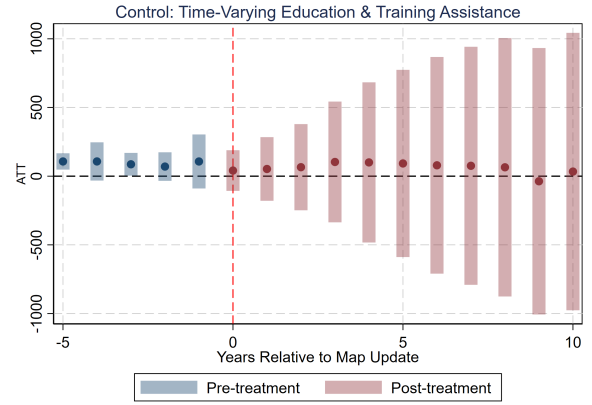
(d) Veterans' benefits, time-varying control

Notes: Each panel plots CS-DID ([Callaway and Sant'Anna, 2021](#)) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (11) and (12) of Table [A17](#) (left-hand panels) and of Table [A18](#) (right-hand panels).

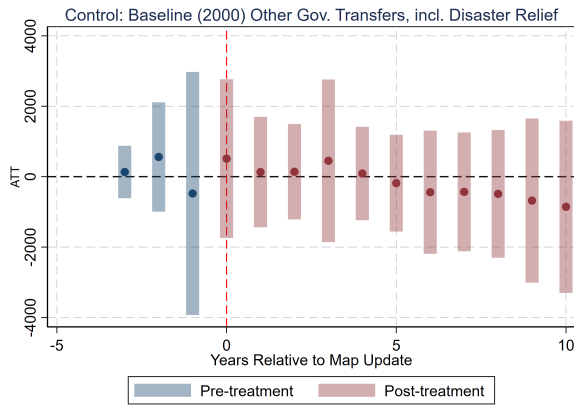
Figure A11. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Education and Other Government Transfers



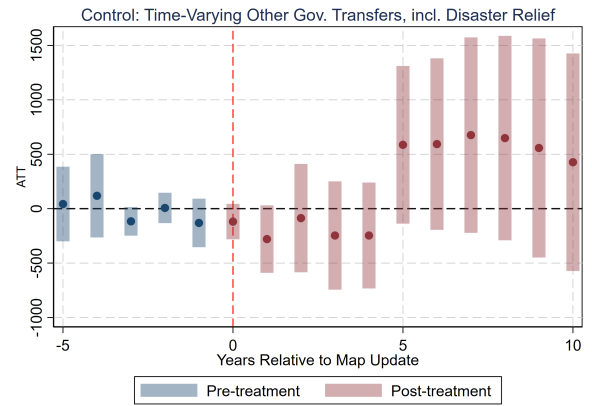
(a) Education and training assistance, base-year control



(b) Education and training assistance, time-varying control



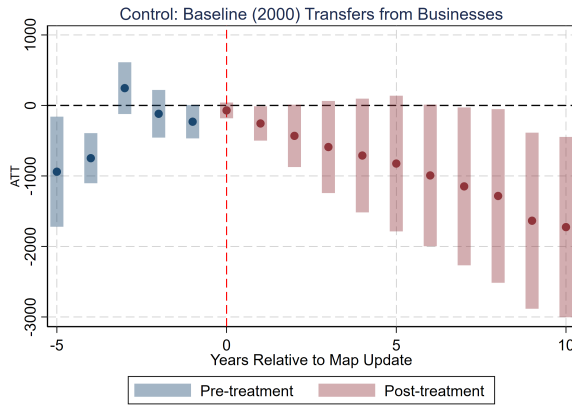
(c) Other government transfers, incl. disaster relief, base-year control



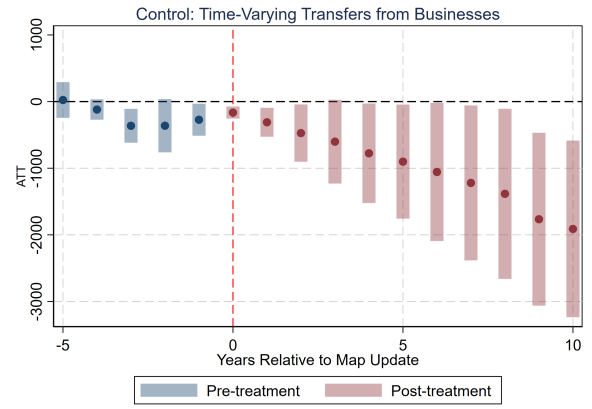
(d) Other government transfers, incl. disaster relief, time-varying control

Notes: Each panel plots CS-DID (Callaway and Sant'Anna, 2021) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in columns (13) and (14) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

Figure A12. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures: Transfers from Businesses



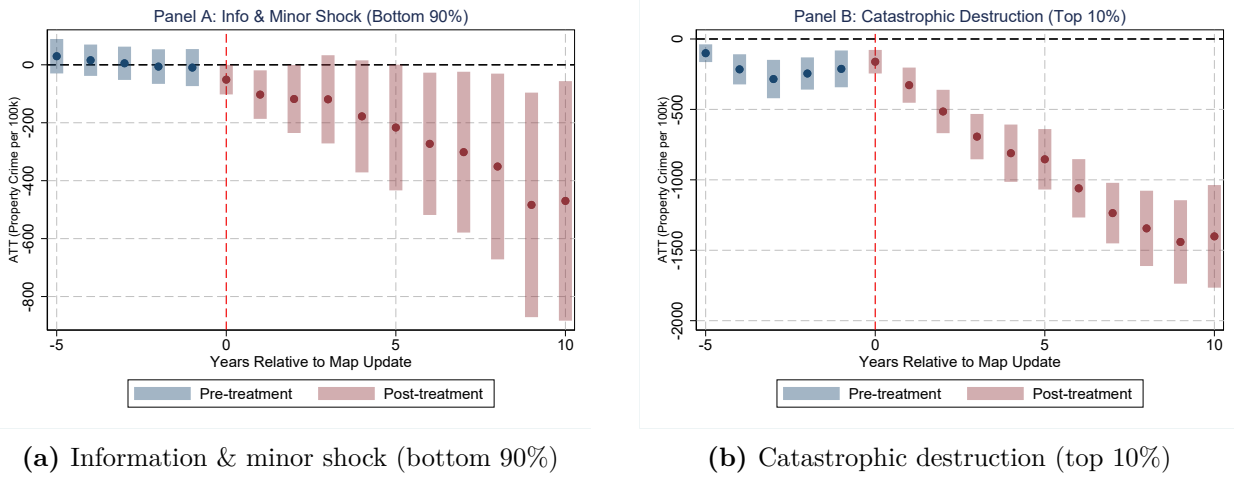
(a) Transfers from businesses, base-year control



(b) Transfers from businesses, time-varying control

Notes: Each panel plots CS-DID (Callaway and Sant’Anna, 2021) event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, adding one BEA transfer category (log real 2019 per-capita receipts) as a control: left column, base-year (2000) value; right column, contemporaneous (time-varying) value. No disaster-aid control is included. Annual county-level data, 2000–2019, weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in column (15) of Table A17 (left-hand panels) and of Table A18 (right-hand panels).

Figure A13. Heterogeneity by Historical Flood Damage: Property-Crime Effect of Map Updates



Notes: The figure plots event-study estimates of the effect of an NFIP flood-map update on the property crime rate, per 100,000 county residents, obtained from the CS-DID estimator of [Callaway and Sant'Anna \(2021\)](#), for counties split by cumulative historical flood property damage. Panel (a) pools counties with zero damage and those in the bottom 90% of the positive-damage distribution; panel (b) is the top 10%. Panel (b) retains a single never-treated comparison county, so its estimates rest on very limited control variation. The sample is annual county-level data for 2000–2019, and estimates are weighted by county population. Bars show 95% confidence intervals. The overall ATT and the pre- and post-treatment averages are reported in [Table A14](#).

Appendix Tables

Table A1. Temporal Distribution of FEMA Flood-Map Updates

Treatment year	Counties treated	Share of treated (%)
2000	11	0.5
2001	7	0.3
2002	7	0.3
2003	13	0.6
2004	19	0.9
2005	33	1.6
2006	58	2.8
2007	111	5.3
2008	158	7.5
2009	271	12.9
2010	366	17.5
2011	265	12.6
2012	198	9.4
2013	94	4.5
2014	126	6.0
2015	106	5.1
2016	57	2.7
2017	73	3.5
2018	67	3.2
2019	57	2.7
Total treated	2097	100.0
Never treated	133	—
All counties	3141	—

Notes: Each county is counted once, in the year of its first FEMA flood-map (FIRM) update. The dispersion of updates across years provides the staggered treatment timing that identifies the difference-in-differences design. Figure 1 plots these counts.

Table A2. Summary Statistics by Treatment Group: Crime, Demographic, Housing, Labour-Market and Activity Variables

Variable	Ever treated		Never treated		Difference	p-value	Obs.
	Mean	SD	Mean	SD			
<i>Panel A: Crime outcomes</i>							
Property crime rate (per 100k)	1,901.95	1,376.74	1,377.64	1,869.56	524.31***	<0.001	62,657
Violent crime rate (per 100k)	956.75	785.91	943.89	1,405.28	12.87	0.647	62,657
<i>Panel B: Demographics and housing</i>							
Population	100,385	319,681	81,430	726,252	18,955	0.181	62,815
Median household income	43,818	12,694	42,392	12,482	1,426***	<0.001	62,689
Median housing value (1-year ACS)	191,035	111,995	311,100	241,661	-120,065***	<0.001	16,660
Owner-occupied share (1-year ACS)	0.609	0.087	0.386	0.138	0.223***	<0.001	16,660
<i>Panel C: Labour market</i>							
Unemployment rate	6.002	2.638	5.526	3.211	0.477***	<0.001	62,466
Total employment	37,229	133,442	34,169	199,276	3,061	0.443	62,671
Service employment	30,807	114,776	31,827	192,114	-1,021	0.791	62,596
Manufacturing employment	4,177	13,160	1,350	5,182	2,827***	<0.001	62,671
Employment rate, young males 20-34	71.825	13.980	75.748	18.556	-3.923***	<0.001	34,490
Wage, non-college males	3,424	847	3,417	1,254	7	0.781	61,589
Gini index	0.440	0.035	0.436	0.049	0.004***	0.001	31,353
<i>Panel D: Policing, commercial activity and time use</i>							
Sworn full-time officers	228.80	880.25	422.81	3,337.83	-194.01***	0.003	62,815
Commercial diversity (entropy-3D)	3.397	0.353	2.963	0.708	0.434***	<0.001	62,568
Amenity share (broad)	0.126	0.033	0.132	0.076	-0.006***	<0.001	62,671
Total establishments	2,340.1	7,787.1	2,083.1	10,968.8	257.0	0.242	62,658
Average firm size	12.23	4.58	9.08	7.05	3.15***	<0.001	62,658
Outdoor activity, minutes per day (ATUS)	11.71	19.15	16.18	17.88	-4.47**	0.015	4,718
Non-household childcare, minutes per day (ATUS)	13.46	28.23	11.95	21.37	1.51	0.490	4,718
<i>Panel E: Flood exposure and NFIP claims</i>							
Flood property damage	1,561,196	77,218,033	57,238	1,527,252	1,503,958***	<0.001	62,815
NFIP claim payouts	980,467	42,573,259	427,458	13,532,341	553,009*	0.079	62,815
NFIP claim count	17.90	442.09	5.88	159.89	12.02***	<0.001	62,815
<i>Panel F: Baseline (year-2000) covariates</i>							
Baseline population	91,844	295,345	76,290	696,318	15,554	0.253	62,800
Baseline median income	36,471	9,037	33,745	7,763	2,726***	<0.001	62,640
Baseline poverty rate	13.227	5.516	15.162	6.885	-1.935***	<0.001	62,640
Baseline establishments	6.5	1.4	4.9	1.8	1.7***	<0.001	62,600
Baseline average firm size	2.44	0.38	2.04	0.53	0.40***	<0.001	62,280
Baseline average wage	10	0	10	0	0***	<0.001	62,280
Counties	3008		133				3141

Notes: The sample is the county-year panel, 2000–2019. Ever-treated counties are those with a FEMA flood-map update during the sample period; never-treated counties form the comparison group. Because the statistics cover the full panel, the ever-treated means include post-update years, so the difference column is descriptive rather than a test of pre-treatment balance. Differences are treated minus never-treated, with p-values from a two-sample t-test allowing unequal variances. Observation counts vary across rows because the auxiliary panels (ACS, CBP, UCR, ATUS, BEA) cover different county-years. Transfer, disaster-aid and baseline-transfer variables are reported in Table A3. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A3. Summary Statistics by Treatment Group: Government Transfers and Disaster Aid

Variable	Ever treated		Never treated		Difference	p-value	Obs.
	Mean	SD	Mean	SD			
<i>Panel A: Direct disaster aid (per capita, 2019 USD)</i>							
Direct disaster aid per capita	24.33	377.00	42.82	481.90	-18.50*	0.062	61,556
Baseline (2000) direct disaster aid per capita	11.72	71.20	13.11	79.40	-1.40	0.394	61,540
<i>Panel B: BEA transfer receipts (per capita, 2019 USD)</i>							
Total personal transfer receipts	8,421	2,462	9,961	17,874	-1,540***	<0.001	61,520
Government transfers to individuals	8,198	2,448	9,711	17,628	-1,514***	<0.001	61,520
Retirement and disability insurance	3,103	872	2,983	1,881	120***	0.002	61,520
Medical benefits	3,602	1,341	4,508	9,975	-906***	<0.001	61,520
Medicare	2,032	738	2,091	1,395	-58**	0.043	61,520
Public assistance medical care (Medicaid)	1,527	836	2,388	8,848	-862***	<0.001	61,520
Income maintenance benefits	769	385	1,099	3,496	-331***	<0.001	61,520
Supplemental Security Income	172	123	177	453	-5	0.597	61,520
SNAP / food stamps	179	124	266	900	-88***	<0.001	61,520
Other income maintenance, incl. TANF	209	119	445	1,978	-236***	<0.001	61,520
Unemployment insurance	175	151	222	636	-48***	<0.001	61,520
Veterans' benefits	277	192	246	290	30***	<0.001	61,520
Education and training assistance	151	129	80	65	71***	<0.001	61,520
Other gov. transfers, incl. disaster relief	121	278	572	2,306	-450***	<0.001	61,520
Transfers from businesses	91	42	101	120	-10***	<0.001	61,520
<i>Panel C: Baseline (year-2000) per-capita value of each transfer category (2019 USD)</i>							
Total personal transfer receipts	5,969	1,541	7,359	13,835	-1,390***	<0.001	61,440
Government transfers to individuals	5,686	1,519	7,042	13,513	-1,356***	<0.001	61,440
Retirement and disability insurance	2,493	630	2,473	1,257	20	0.445	61,440
Medical benefits	2,259	723	2,668	5,986	-409***	<0.001	61,440
Medicare	1,187	357	1,165	541	22**	0.050	61,440
Public assistance medical care (Medicaid)	1,055	519	1,491	5,611	-436***	<0.001	61,440
Income maintenance benefits	541	298	762	2,262	-221***	<0.001	61,440
Supplemental Security Income	156	122	167	486	-11	0.286	61,440
SNAP / food stamps	83	64	152	600	-69***	<0.001	61,440
Other income maintenance, incl. TANF	125	86	261	1,002	-136***	<0.001	61,440
Unemployment insurance	105	75	167	493	-62***	<0.001	61,440
Veterans' benefits	161	83	154	238	7	0.140	61,440
Education and training assistance	102	70	62	41	40***	<0.001	61,440
Other gov. transfers, incl. disaster relief	26	415	757	3,705	-732***	<0.001	61,440
Transfers from businesses	164	25	183	188	-19***	<0.001	61,440
Counties	3008		133				3141

Notes: The sample is the county-year panel, 2000–2019; treatment groups, the difference column and its p-value are defined as in Table A2. Panel B variables are the outcomes of Table A15 and the time-varying controls of Table A18; Panel C variables are the baseline controls of Table A17. Baseline values are constant within county, so their means are county-year weighted. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A4. Two-Way Fixed-Effects Estimates: Effect of Map Updates on Crime Rates

	(1) Property Crime Rate	(2) Violent Crime Rate
Panel A: Static TWFE		
Post map update	-17.101 (42.675)	-10.408 (19.572)
Observations	62,657	62,657
Within R^2	0.000	0.000
Panel B: Dynamic TWFE (event time relative to map update)		
$t \leq -5$	64.497 (48.156)	26.030 (19.419)
$t = -4$	20.948 (29.217)	4.271 (14.044)
$t = -3$	17.818 (24.701)	2.003 (11.704)
$t = -2$	21.131 (15.490)	2.043 (8.286)
$t = -1$ (reference)	0 —	0 —
$t = 0$	-13.784 (15.542)	-3.593 (9.016)
$t = 1$	-13.674 (26.720)	-7.543 (12.584)
$t = 2$	11.120 (36.853)	-8.565 (15.781)
$t = 3$	37.179 (45.578)	0.421 (19.817)
$t = 4$	25.889 (55.109)	-8.698 (24.998)
$t = 5$	31.199 (58.979)	5.955 (28.566)
$t = 6$	20.239 (69.156)	18.510 (30.925)
$t = 7$	28.525 (83.094)	21.184 (33.920)
$t = 8$	35.900 (96.392)	21.672 (37.258)
$t = 9$	4.906 (124.713)	20.985 (47.362)
$t \geq 10$	35.400 (131.100)	32.177 (47.046)
Observations	62,657	62,657
Within R^2	0.001	0.001
County Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Population weights (analytic)	✓	✓
Counties (clusters)	3,134	3,134

Notes: Crime rates are per 100,000 residents; annual county panel, 2000–2019. Panel A reports the coefficient on an indicator equal to one from the year of a county’s first FEMA flood-map update onward. Panel B reports the coefficients on event-time indicators relative to the map-update year, with $t = -1$ the omitted reference period and the end points binned ($t \leq -5$, $t \geq 10$); never-treated counties have all indicators equal to zero. All specifications include county and year fixed effects and are estimated by OLS with county population as analytic weights. Standard errors, clustered by county, in parentheses. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Effect of NFIP Flood-Map Updates on Crime Rates

	(1) Property Crime Rate	(2) Violent Crime Rate
Overall ATT	-227.606** (97.247)	-61.154 (43.871)
Pre-treatment	3.984 (23.770)	7.292 (7.657)
Post-treatment	-247.359** (101.988)	-68.347 (46.201)
County Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Observations	62,657	62,657
Joint pre-treatment test (p -value)	0.445	0.653

Notes: Crime rates are per 100,000 residents. Population-weighted, with never-treated counties as the comparison group. The corresponding event-study estimates are plotted in Figure 4, column (1), and Figure 12, column (2). Estimates come from the [Callaway and Sant'Anna \(2021\)](#) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. Mechanisms: Demographic and Housing Responses to Map Updates

	(1) log(Population)	(2) log(Median Income)	(3) log(Median Housing Value)	(4) Owner- Occupied Share
Overall ATT	0.0484** (0.0212)	-0.0614* (0.0314)	-0.0824*** (0.0205)	-0.0049** (0.0022)
Pre-treatment	0.0081*** (0.0024)	-0.0183* (0.0107)	-0.0193*** (0.0045)	-0.0009** (0.0004)
Post-treatment	0.0508** (0.0229)	-0.0619* (0.0319)	-0.0824*** (0.0207)	-0.0056** (0.0025)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	62,657	62,689	12,495	13,560
Joint pre-treatment test (<i>p</i> -value)	<0.001	<0.001	0.005	0.125

Notes: Column (1) is weighted by baseline (2000) population and matched on baseline demographics; column (2) uses cross-domain matching on baseline business characteristics; columns (3)–(4) use the 1-year ACS (2005 onward, high-population counties): column (3) is the log of the median housing value, and column (4) uses a not-yet-treated comparison group with wild bootstrap inference. The corresponding event-study estimates are plotted in Figure 5. Estimates come from the [Callaway and Sant’Anna \(2021\)](#) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7. Mechanisms: Labour Market and Sector Employment

	(1) Unemployment Rate	(2) Total Employment	(3) Service Employment	(4) Manufacturing Employment
Overall ATT	-0.344 (0.277)	-84055.9*** (21114.1)	-78913.8*** (21731.0)	-300.2 (4807.4)
Pre-treatment	0.131*** (0.049)	-6861.4*** (2295.4)	-8063.2*** (2604.5)	1093.2* (627.6)
Post-treatment	-0.461 (0.298)	-89278.6*** (22465.0)	-83528.6*** (23401.2)	-738.1 (5127.3)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	62,466	62,671	62,596	62,671
Joint pre-treatment test (<i>p</i> -value)	0.003	0.002	0.004	0.009

Notes: Column (1) conditions on baseline socio-economic covariates; columns (2)–(4) use County Business Patterns employment counts, 2000-2019. The corresponding event-study estimates are plotted in Figure 6. Estimates come from the [Callaway and Sant’Anna \(2021\)](#) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8. Mechanisms: Business Establishments and Firm Size

	(1) log(Total Establishments)	(2) log(Average Firm Size)
Overall ATT	0.0792*** (0.0247)	-0.0651*** (0.0251)
Pre-treatment	0.0171*** (0.0062)	-0.0118** (0.0054)
Post-treatment	0.0791*** (0.0245)	-0.0660*** (0.0251)
County Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Observations	62,658	62,296
Joint pre-treatment test (p -value)	0.020	0.221

Notes: Both columns use the cross-domain business specification, matching on baseline poverty and baseline median income rather than on the baseline level of the outcome itself. The corresponding event-study estimates are plotted in Figure 7. Estimates come from the [Callaway and Sant'Anna \(2021\)](#) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9. Mechanisms: Policing, Young-Male Labour Market and Inequality

	(1) log(Sworn FT Officers)	(2) Emp. Rate, Young Males	(3) log(Wage), Non-College Males	(4) Gini Index
Overall ATT	-0.0135 (0.0086)	-0.5230 (0.8517)	-0.0418 (0.0283)	0.0116*** (0.0036)
Pre-treatment	0.0040** (0.0019)	-0.2552 (0.2589)	-0.0123* (0.0074)	0.0047** (0.0019)
Post-treatment	-0.0171* (0.0095)	-0.5338 (1.0399)	-0.0493 (0.0329)	0.0136*** (0.0043)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	62,265	34,490	61,589	31,353
Joint pre-treatment test (<i>p</i> -value)	0.109	0.511	0.338	0.119

Notes: Column (1) is the log number of sworn full-time officers from the UCR police employment panel, 2000-2019. Columns (2)–(4) come from the economic opportunity panel and condition on baseline socio-economic covariates. The corresponding event-study estimates are plotted in Figure 8, column (1); Figure 9, columns (2)–(3); and Figure 10, column (4). Estimates come from the [Callaway and Sant’Anna \(2021\)](#) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10. Mechanisms: Commercial Composition and Time Use

	(1) Commercial Diversity	(2) Amenity Share	(3) Non-HH Childcare Mins (ATUS)	(4) Outdoor Mins (ATUS)
Overall ATT	0.1445** (0.0590)	0.0093 (0.0068)	3.032* (1.595)	0.971 (1.710)
Pre-treatment	0.0040 (0.0051)	0.0015 (0.0018)	-0.613 (0.628)	0.512 (0.319)
Post-treatment	0.1734** (0.0744)	0.0106 (0.0070)	3.089* (1.714)	1.249 (1.848)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	62,568	62,671	4,718	4,718
Joint pre-treatment test (<i>p</i> -value)	0.922	0.410	0.073	0.591

Notes: Columns (1)–(2) condition on baseline socio-economic covariates. Columns (3)–(4) are county-average minutes per day from the American Time Use Survey spent caring for non-household children (3) and on outdoor activity (4), estimated without covariates against a not-yet-treated comparison group given the limited matched sample. The corresponding event-study estimates are plotted in Figure 11, columns (1)–(2), and Figure A2, columns (3)–(4). Estimates come from the Callaway and Sant’Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11. Information Salience: Effect of Map Updates by Prior Flood Experience

	(1) Property: High Salience	(2) Property: Low Salience	(3) Violent: High Salience	(4) Violent: Low Salience
Overall ATT	-517.218*** (141.633)	-183.560 (125.455)	-170.833*** (66.070)	-49.101 (55.626)
Pre-treatment	-5.779 (31.985)	3.435 (29.980)	4.861 (10.974)	16.814 (10.523)
Post-treatment	-566.433*** (144.195)	-202.702 (125.839)	-190.899*** (68.243)	-55.865 (58.521)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	9,543	17,823	9,543	17,823
Joint pre-treatment test (<i>p</i> -value)	0.901	0.032	0.256	0.028

Notes: High salience: a physical flood occurred within five years before the map update. Low salience: no physical flood observed in the sample period. Crime rates are per 100,000 residents. The corresponding event-study estimates are plotted in Figure 14. Estimates come from the Callaway and Sant'Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12. Effect of the Physical Flood Shock on Crime

	(1) Property Crime Rate	(2) Violent Crime Rate
Overall ATT	-170.711** (71.338)	-130.559*** (30.288)
Pre-treatment	-1.923 (12.412)	-2.403 (5.395)
Post-treatment	-97.743 (60.935)	-95.953*** (26.083)
County Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Observations	62,657	62,657
Joint pre-treatment test (p -value)	0.059	0.619

Notes: Treatment is the first physical flood rather than the map update, so event time is measured relative to the physical shock. Crime rates are per 100,000 residents. The corresponding event-study estimates are plotted in Figure 13. Estimates come from the Callaway and Sant'Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13. Salience and Flood-Control Robustness

Panel A: Property Crime Rate				
	(1) High Salience	(2) Low Salience	(3) High + LT Flood Ctrl	(4) High + ST Flood Ctrl
Overall ATT	-517.218*** (141.633)	-183.560 (125.455)	-407.178 (268.657)	-290.705 (178.281)
Pre-treatment	-5.779 (31.985)	3.435 (29.980)	-86.963 (67.170)	-60.979 (42.646)
Post-treatment	-566.433*** (144.195)	-202.702 (125.839)	-584.326 (361.684)	-377.841 (241.136)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	9,543	17,823	9,543	9,543
Joint pre-treatment test (<i>p</i> -value)	0.901	0.032	0.685	0.894
Panel B: Violent Crime Rate				
	(1) High Salience	(2) Low Salience	(3) High + LT Flood Ctrl	(4) High + ST Flood Ctrl
Overall ATT	-170.833*** (66.070)	-49.101 (55.626)	-109.734 (85.510)	-58.461 (116.257)
Pre-treatment	4.861 (10.974)	16.814 (10.523)	-18.752 (24.122)	-13.026 (18.764)
Post-treatment	-190.899*** (68.243)	-55.865 (58.521)	-116.250 (110.754)	-33.581 (174.004)
County Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Observations	9,543	17,823	9,543	9,543
Joint pre-treatment test (<i>p</i> -value)	0.256	0.028	0.470	0.207

Notes: Long-term flood controls span the physical-flood path from t-5 to t+10 and short-term controls span t-2 to t+4, with t-1 the omitted baseline in both cases. Crime rates are per 100,000 residents. The corresponding event-study estimates are plotted in Figure 15. Estimates come from the Callaway and Sant'Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14. Heterogeneity by Historical Flood Damage: Property-Crime Effect of Map Updates

	(1) Info & Minor Shock (Bottom 90%)	(2) Catastrophic Destruction (Top 10%)
Overall ATT	-216.107** (102.507)	-857.845*** (85.096)
Pre-treatment	7.031 (25.165)	-211.658*** (17.814)
Post-treatment	-242.220** (108.411)	-895.082*** (77.163)
County Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Observations	56,917	5,740
Joint pre-treatment test (p -value)	0.758	<0.001

Notes: The outcome is the property crime rate per 100,000 residents. Counties are split by cumulative historical flood property damage: column (1) pools counties with zero damage and those in the bottom 90% of the positive-damage distribution; column (2) is the top 10% (damage above \$24.4 million). Column (2) retains a single never-treated comparison county, so its estimates rest on very limited control variation and should be read with caution. Population-weighted, with never-treated counties as the comparison group. The corresponding event-study estimates are plotted in Figure A13, panel (a), column (1), and panel (b), column (2). Estimates come from the Callaway and Sant'Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15. Placebo Tests: Effect of Map Updates on Government Transfer Receipts

Panel A					
	(1) Other Gov. Transfers	(2) Medical Benefits	(3) Business Transfers	(4) Supplemental Security Income	(5) SNAP / Food Stamps
Overall ATT	0.4082** (0.1703)	0.0363 (0.0413)	0.0257 (0.0228)	0.1415** (0.0710)	0.0480 (0.0414)
Pre-treatment	0.1506*** (0.0488)	0.0062 (0.0075)	-0.0006 (0.0036)	0.0148 (0.0192)	0.0066 (0.0210)
Post-treatment	0.4257** (0.1792)	0.0457 (0.0469)	0.0245 (0.0246)	0.1501* (0.0807)	0.0647 (0.0422)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	61,520	61,520	61,520	61,520	61,520
Joint pre-treatment test (<i>p</i> -value)	0.121	0.882	0.338	0.776	0.903
Panel B					
	(6) Other Income Maintenance	(7) Medicare	(8) Public Assistance Medical	(9) Veterans' Benefits	(10) Education & Training
Overall ATT	-0.0833 (0.0712)	0.0271 (0.0357)	0.0602 (0.0983)	0.1415* (0.0727)	0.1410** (0.0711)
Pre-treatment	-0.0043 (0.0278)	0.0121** (0.0057)	0.0035 (0.0109)	-0.0081 (0.0220)	0.0182 (0.0165)
Post-treatment	-0.0769 (0.0722)	0.0261 (0.0384)	0.0772 (0.1170)	0.1656* (0.0872)	0.1554** (0.0736)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	61,520	61,520	61,520	61,520	61,520
Joint pre-treatment test (<i>p</i> -value)	0.874	0.254	0.916	0.361	0.099

Notes: All outcomes are the log of real (2019 USD) per-capita transfer receipts plus one. Column (1) is the BEA line that absorbs federal disaster relief. A null ATT indicates that map updates did not shift the fiscal-transfer channels. The corresponding event-study estimates are plotted in Figure 16 (Panel A, columns (1) and (3)) and Figures A3 and A4 (remaining columns). Estimates come from the Callaway and Sant'Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16. Placebo Tests: Effect of Map Updates on NFIP Claim Activity

	(1) Total Claim Payouts	(2) Claim Frequency
Overall ATT	13356248 (11153436)	150.87* (86.54)
Pre-treatment	-2880979 (2379284)	-56.67 (52.99)
Post-treatment	21927747 (19286612)	215.42 (149.10)
County Fixed Effects	✓	✓
Year Fixed Effects	✓	✓
Observations	62,815	62,815
Joint pre-treatment test (p -value)	0.117	0.324

Notes: County-years with no NFIP claim are coded as zero rather than missing. A null ATT indicates that map updates did not coincide with changes in realised flood losses, supporting an information-based rather than a physical-risk interpretation. The corresponding event-study estimates are plotted in Figure A1. Estimates come from the [Callaway and Sant'Anna \(2021\)](#) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures, Baseline (2000) Controls

Panel A					
	(1) Total Transfers	(2) Gov. Transfers	(3) Retirement & Disability	(4) Medical Benefits	(5) Medicare
Overall ATT	-1100.045 (1207.243)	-1094.371 (1233.237)	-61.868 (242.462)	-876.926 (1774.305)	-15.372 (276.484)
Pre-treatment	20.132 (194.790)	-45.175 (176.294)	65.917 (51.971)	-8.356 (362.280)	100.168** (46.176)
Post-treatment	-1276.058 (1390.571)	-1204.690 (1340.901)	-80.879 (252.738)	-962.261 (1944.714)	-37.495 (291.193)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	62,657	62,657	62,657	62,657	62,657
Joint pre-treatment test (<i>p</i> -value)	0.780	0.837	0.757	0.793	0.021
Panel B					
	(6) Medicaid / Public Med.	(7) Income Maintenance	(8) SSI	(9) SNAP / Food Stamps	(10) Other Income Maint.
Overall ATT	-182.452 (164.325)	-365.137 (730.846)	-117.133 (188.864)	-224.016 (332.308)	-222.475** (99.043)
Pre-treatment	104.145 (227.056)	128.964 (134.703)	66.370 (47.602)	60.697 (130.867)	15.109 (20.671)
Post-treatment	-209.791 (147.460)	-391.958 (768.884)	-128.753 (203.441)	-241.168 (349.103)	-236.658** (110.095)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	62,657	62,657	62,657	62,657	62,657
Joint pre-treatment test (<i>p</i> -value)	0.866	0.636	0.328	0.164	0.785
Panel C					
	(11) Unemployment Ins.	(12) Veterans' Benefits	(13) Education & Training	(14) Other Gov. (Disaster)	(15) Business Transfers
Overall ATT	-1379.865 (930.734)	-101.980 (288.171)	-63.612 (197.430)	-433.798 (749.468)	-762.371* (389.364)
Pre-treatment	-128.100 (177.943)	79.807 (52.450)	62.369* (35.111)	71.538 (300.363)	-358.257*** (126.919)
Post-treatment	-1445.676 (976.042)	-131.261 (307.194)	-74.976 (212.134)	-159.811 (574.184)	-878.723** (415.503)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	62,657	62,657	62,657	62,657	62,657
Joint pre-treatment test (<i>p</i> -value)	0.842	0.358	0.008	0.778	<0.001

Notes: The outcome in every column is the property crime rate per 100,000 residents. Each column adds a single BEA personal-transfer category as the only covariate, measured as the county's year-2000 value of the log of real (2019 USD) per-capita receipts plus one; the same categories enter as time-varying controls in Table A18. No disaster-aid control is included. Column (14) is the BEA line that absorbs federal disaster relief. Population-weighted, with never-treated counties as the comparison group. The corresponding event-study estimates are plotted in the left-hand panels of Figures A5–A12. Estimates come from the Callaway and Sant'Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18. Robustness: Property-Crime Effect Controlling for Alternative Transfer Measures, Time-Varying Controls

Panel A					
	(1) Total Transfers	(2) Gov. Transfers	(3) Retirement & Disability	(4) Medical Benefits	(5) Medicare
Overall ATT	-1102.711 (962.552)	-1097.612 (984.508)	-303.936 (267.993)	-1083.246 (1633.398)	-228.026 (248.653)
Pre-treatment	-6.467 (162.258)	-4.237 (165.664)	17.774 (32.909)	-41.044 (213.951)	69.248** (28.383)
Post-treatment	-1160.611 (998.336)	-1156.450 (1020.565)	-338.376 (285.515)	-1151.984 (1691.539)	-265.451 (263.995)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	62,657	62,657	62,657	62,657	62,657
Joint pre-treatment test (<i>p</i> -value)	0.457	0.518	0.954	0.900	0.040
Panel B					
	(6) Medicaid / Public Med.	(7) Income Maintenance	(8) SSI	(9) SNAP / Food Stamps	(10) Other Income Maint.
Overall ATT	-262.966 (238.117)	-817.890 (1259.273)	-226.249 (386.548)	-453.218 (788.275)	-712.941 (1089.355)
Pre-treatment	32.332 (96.625)	129.817 (153.888)	62.798 (44.584)	143.302 (183.435)	48.868 (174.303)
Post-treatment	-283.085 (256.153)	-875.679 (1329.649)	-248.094 (409.724)	-483.393 (819.446)	-774.315 (1168.956)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	62,657	62,657	62,657	62,657	62,657
Joint pre-treatment test (<i>p</i> -value)	0.845	0.611	0.652	0.288	0.970
Panel C					
	(11) Unemployment Ins.	(12) Veterans' Benefits	(13) Education & Training	(14) Other Gov. (Disaster)	(15) Business Transfers
Overall ATT	-1034.596 (791.379)	-61.945 (150.434)	68.014 (302.829)	-35.357 (196.933)	-837.278** (375.046)
Pre-treatment	-229.767 (258.312)	88.851 (82.541)	95.642* (53.886)	-15.888 (68.304)	-218.694* (115.093)
Post-treatment	-1087.885 (819.211)	-84.680 (155.223)	60.987 (321.569)	228.291 (296.296)	-960.283** (416.228)
County Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Observations	62,657	62,657	62,657	62,657	62,657
Joint pre-treatment test (<i>p</i> -value)	0.596	0.292	0.002	0.481	0.003

Notes: The outcome in every column is the property crime rate per 100,000 residents. Each column adds a single BEA personal-transfer category as the only covariate, measured as the contemporaneous log of real (2019 USD) per-capita receipts plus one (the estimator evaluates covariates at their pre-treatment value); the same categories enter as baseline (2000) controls in Table A17. No disaster-aid control is included. Column (14) is the BEA line that absorbs federal disaster relief. Population-weighted, with never-treated counties as the comparison group. The corresponding event-study estimates are plotted in the right-hand panels of Figures A5–A12. Estimates come from the Callaway and Sant’Anna (2021) doubly-robust IPW estimator. The joint pre-treatment test is a Wald test that the coefficients from t-5 to t-2 are jointly zero, with t-1 the omitted baseline. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19. Geographic Diffusion of FEMA Flood-Map Updates, 2000–2019

		Treated by 2004		Treated by 2009		Treated by 2014		Treated by 2019		Never
Region	Counties	N	%	N	%	N	%	N	%	treated
Panel A: Number and share of counties										
Northeast	217	59	27.2	99	45.6	165	76.0	199	91.7	4
Midwest	1,055	181	17.2	347	32.9	700	66.4	820	77.7	57
South	1,424	169	11.9	507	35.6	1,044	73.3	1,194	83.8	40
West	442	134	30.3	220	49.8	313	70.8	369	83.5	30
All counties	3,141	543	17.3	1,174	37.4	2,223	70.8	2,583	82.2	133
Panel B: Share of year-2000 population in treated counties (%)										
Northeast	56.2		12.8		39.3		62.9		79.8	14.2
Midwest	65.5		6.1		34.8		75.0		88.8	1.2
South	100.9		4.7		36.0		68.9		85.3	1.2
West	63.8		7.0		58.4		84.5		92.0	0.2
All counties	286.3		7.1		41.3		72.6		86.5	3.5

Notes: Numerical counterpart to the cumulative maps in Figure 3. A county is treated by year t if its first FEMA flood-map (FIRM) update took effect in or before t (updates before 2000 included); never-treated counties have no update through the end of the panel; the remainder are not yet treated at t (including cohorts after 2019). Panel A reports counts and the share of each region's counties; Panel B reports the share of the region's year-2000 population living in counties treated by t , with the Counties column giving the region's year-2000 population in millions, and the last column the population share in never-treated counties. Regions are the four Census regions; the three panel units outside the 50 states and DC (Guam, Puerto Rico and one unassigned code) enter only the All-counties rows.