

CLEANSING FLOODS? CREATIVE DESTRUCTION AND GOVERNMENT SPENDING AFTER DISASTERS*

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Abstract

Disasters devastate economies, yet productivity can improve in their wake. Why? Using confidential plant-level microdata from the US Census Bureau and an event study design, I trace the creative destruction process that follows large federally declared floods. Exits are concentrated among the least productive plants, whose used machines are then acquired by high-productivity entrants. Survivors upgrade their machinery as they rebuild and see productivity gains. Federal disaster spending facilitates this process by expanding financing access for nimble young and small firms that disproportionately fuel productive reallocation. Without it, financing constraints stifle creative destruction and productivity declines. Ultimately, the relative income gains from federal disaster assistance generate tax revenues far exceeding the policy's upfront cost, making it both efficiency-enhancing and fiscally sound. My findings reveal a novel *allocative efficiency* channel through which government spending supports post-disaster recovery, with critical implications for a warming world.

Keywords: Natural Disasters; Disaster Aid; Firm Dynamics; Creative Destruction; Allocative Efficiency

JEL Codes: Q54; H84; R11; L26

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1 Introduction

Large disasters destroy capital, displace workers, and disrupt business activity. We might expect such shocks to be unambiguously bad for affected regions, causing persistent declines in productivity and incomes. But reality often looks different: disaster-hit economies tend to bounce back quickly, and sometimes emerge even better off than before. How can improvement follow from destruction? This paper reconciles this apparent paradox.

Using confidential microdata from the US Census Bureau, I examine firm dynamics—the entry, growth, downsizing, and exit of firms—and aggregate productivity following large, federally declared floods. These are floods that triggered a presidential disaster declaration—a place-based designation that unlocks substantial disaster assistance for infrastructure rebuilding, household recovery, and low-interest loans to affected firms. As such, financing constraints, which would typically weaken recovery, are less likely to bind in this context. Since this expanded spending can increase household consumption and thus affect business churn, my primary focus on manufacturing—a tradable sector insensitive to local demand—provides a uniquely clean empirical setting to study productivity.

It is not *ex-ante* obvious whether government spending helps or hurts aggregate productivity. The answer depends on how it shapes creative destruction—the continual redistribution of resources from less to more productive firms that, in healthy economies, drives allocative efficiency and productivity growth. As a negative productivity shock, disasters amplify the *destructive* margin of this process by forcing exits. A long literature following Schumpeter (1939) characterizes such shocks as ‘cleansing’—disproportionately culling low performers and freeing up resources for more efficient use. For efficiency to improve, resources must simultaneously flow toward the economy’s *creative*, more productive margin (Caballero & Hammour, 1996), including to entrants, which are often credit-constrained yet dynamic. Federal spending could facilitate such reallocation, but it could also distort it: low-interest federal loans may sustain incumbent ‘zombie’ firms or crowd out efficient private lending, misallocating resources *away* from their best use and undermining recovery.¹ In fact, a common argument in current policy debates is that federal aid is, at best, redundant since states can fund recovery on their own.² My findings suggest otherwise.

I show that the post-disaster period sees accelerated creative destruction: the least productive plants exit, and their used machinery is acquired by more productive survivors and positively selected entrants. Federal spending facilitates this process by expanding

¹Credit market imperfections may partially dampen (Osotimehin & Pappadà, 2017) or even substantially undo (Barlevy, 2003) the cleansing effect of recessions. See also Caballero et al. (2008); Huang et al. (2020).

²E.g., see recent proposals to ‘wean’ states off FEMA assistance (Washington Post, 2025).

financing access and crowding *in* private credit, particularly for young and small firms. In its absence, productivity selection is blunted on both the exit and entry margins, and aggregate productivity suffers because financing constraints distort reallocation. Ultimately, relative to a counterfactual in which financing for post-flood recovery only comes from states and/or the private sector, federal assistance boosts allocative efficiency and household incomes, generating incremental tax revenues that fully offset its upfront costs.

My analyses mainly rely on the Census’ Annual Survey of Manufactures (“ASM”), which provides rich information on plant output, productivity, as well as book values of capital stock and investments in building and machinery assets.³ I use revenue-based Total Factor Productivity (“TFP”) as the primary indicator of productivity. Because the ASM surveys most plants in rotating five-year panels, I reconstruct complete plant life histories using the Longitudinal Business Database (LBD), which tracks the entry, exit, and annual employment and payroll outcomes of all U.S. establishments. Leveraging plausibly exogenous variation in the timing of flooding, I use an event study design to compare the post-flood outcomes of plants and industries in flooded counties to their pre-flood baselines in the 1977-2017 window. To ensure that I can plausibly identify the impact of a single, unanticipated flood, I exclude regions with high disaster frequency.⁴

I document three key results. First, exit rates rise after flooding, primarily among the least productive plants. Plants’ probability of shutting down within two years increases by 3.6 percentage points (p.p.) after flooding, representing a substantial—more than 30%—increase compared to the pre-disaster baseline. However, plants that were already in the bottom quartile of TFP distribution in their narrowly-defined industry *before* the disaster are far more likely to exit after it relative to their higher-productivity counterparts. This reflects a weeding out of less efficient forms of production after the negative productivity shock.

Second, surviving plants invest to replace their capital and see productivity gains. Conditional on not exiting, i.e., surviving for at least 3 more years, plants show a substantial and persistent increase in machinery retirement (i.e., sales and scrapping), in part because water accelerates metal corrosion. Survivors also increase their *net* capital investment, often in brand new machinery, suggesting that rebuilding investments more than compensate for the flood damages.⁵ Corroborating this, the qualitative survey I conducted on manufacturers

³The ASM’s intermittent information on plants’ capital retirement (i.e., sale and disposal) helps me approximate post-flood disinvestment, while its breakdown of capital expenditures as new versus used capital enables me to study capital replacement and reallocation.

⁴I restrict my sample to counties which experienced 1-3 declared floods over the 1976–2018 period, with at least 6 years between any two events.

⁵Survivors’ county-level total investment also rises after flooding, suggesting that these investments are not merely a reflection of survivor bias but that there is disaster-induced capital investment.

in the U.S. Midwest finds that firms see flooding as an “opportunity to modernize,” and that “... when replacing by necessity, [they would] stay current.” Survivors’ productivity reaches more than 11% above the pre-flood baseline by the 6th year after flooding, consistent with their positive selection and their capital upgrades.

Third, as disaster-driven exits expand the local supply of second-hand machines, young plants and new entrants acquire that used capital and build up their capacity. We may worry that used machines—especially in the post-flood context—might be lemons, or that their increased supply at cheap prices may attract low-productivity entrants. But I find no evidence of such phenomena. In fact, post-flood entrants have about a 14% higher TFP compared to pre-flood entrants. One explanation for this is managerial quality: using the Management and Organizational Practices Survey (“MOPS”), I show that young firms (aged 0-4 in the pre-flood year) follow more structured and prudent organizational practices in responding to crises—like flooding that disrupts their production process—relative to older firms.⁶ Ultimately, new and young firms disproportionately drive post-disaster creative destruction, a pattern I also document in non-tradable sectors relying on local demand.

At the aggregate level, post-disaster reallocation is consistent with improved allocative efficiency. To study this, I focus on the average TFP, and the interquartile ranges of TFP, as well as marginal revenue product of capital (MRPK) between plants operating within similar industries and locations. Measures of dispersion are useful here because, in a frictionless world, marginal productivity of inputs should be equal across firms, so deviations, such as higher dispersion, suggest inefficiencies.⁷ I find that the 75th-to-25th percentile gap in MRPK shrinks by roughly 1.1% after flooding, reflecting that capital inputs are, on average, reallocated toward plants which can make better use of them. Average productivity also rises, though estimated imprecisely, while TFP dispersion declines. Together with the plant-level evidence on productivity-selective exits and positively selected entrants, these aggregate patterns suggest that post-flood creative destruction improves allocative efficiency.

A key mechanism underlying these results is the expanded access to financing that federal spending facilitates. For a counterfactual set of severe but non-declared floods, I find that aggregate TFP declines by 0.73%, with no evidence of productive cleansing: exit rates rise but indiscriminately so across the productivity distribution, and entrants show no positive-selection. I find similar patterns in flooded counties whose declaration requests were denied, where TFP dispersion potentially even *rises*. These results suggest that, in the post-disaster setting, financing constraints are distortionary. In contrast, when federal

⁶The MOPS is an addendum to the ASM’s 2010 and 2015 waves. See [Buffington et al. \(2017\)](#).

⁷Prior work also uses dispersion measures to study allocative (in)efficiency. See [Hsieh and Klenow \(2009\)](#).

aid is available after disasters, bank loans increase by 3.1%, particularly in the smallest loan size categories, suggesting a *crowding in* of private credit for young and small firms. Consistent with this, small (single-unit) young firms—despite typically being vulnerable to financing constraints—see a substantially attenuated investment decline after declared floods, channeling their expenditures toward used capital. Government disaster spending supports allocative efficiency by cushioning and expanding credit access for the nimble young firms that disproportionately drive productive reallocation.

Federal disaster assistance facilitates creative destruction, but at what cost, especially considering that disaster risks are growing but government resources remain finite? Following [Hendren and Sprung-Keyser \(2020\)](#), I calculate the marginal value of public funds (“MVPF”) of federal disaster spending—the benefits to beneficiaries per dollar of net cost to the government. For this, I examine county-level income recovery and find that per capita incomes decline by 0.9% after large non-declared floods, but (suggestively) rise by about 0.7% after declared disasters. This implies a net income gain of roughly 1.7p.p. ($p < 0.01$) attributable to federal assistance. These incremental incomes generate additional tax revenues that fully offset the roughly \$38.5 billion in upfront costs that FEMA and SBA spent on disaster recovery for floods between 1993–2017, implying an *infinite* MVPF—the policy more than pays for itself while generating substantial welfare gains for affected communities. My sample excludes frequently-flooded coastal regions, so these results carry no implication for settings where relocation may dominate rebuilding as the optimal policy response. But they do speak to a broader set of US regions, including inland ones, where climate change drives higher risk of severe (though still infrequent) flooding: in these regions, post-disaster rebuilding assistance can both enhance allocative efficiency and more than recover its upfront costs via revenue.

Related literature: This paper contributes to several strands of literature, mainly that on how firms are impacted by weather shocks, how they respond, and recover. Much has been done in this area ([Barrot & Sauvagnat, 2016](#); [Carvalho et al., 2021](#); [Basker & Miranda, 2018](#); [Gallagher et al., 2023](#); [Collier, Howell, & Rendell, 2024](#)), with the findings often indicating a strong role of demand in mediating post-disaster business outcomes. My paper offers novel evidence based on a tradable sector—manufacturing—that is less sensitive to local demand, and characterizes the heterogeneity in plants’ behavior and outcomes by firm age and size, thereby expanding our understanding of the nexus of weather hazards and productivity. Complementing recent evidence on firms’ disaster management ([Norris Keiller & Van Reenen, 2024](#)) and, more broadly, on the role of management quality in firm dynamism ([Sadun et al., 2025](#)), my work provides new evidence on how managerial agility enables young

firms to respond to shocks and drive creative destruction in post-disaster settings.

This paper also enhances the literature on the impact of climate change on physical capital and growth. Early theoretical work on these areas worried that the fixed nature of physical capital would drive slow adjustment to rapid weather shocks and thus exacerbate welfare losses from climate change (see [Quiggin and Horowitz \(2003\)](#); [Yohe and Neumann \(1997\)](#); [Fisher and Hanemann \(1993\)](#)). My paper provides a much-needed empirical test, showing that capital appears to be fairly mobile—i.e., adjustable within plant and redistributable across plants—at least in the short run. More recent work has studied how global warming might affect aggregate growth via its impact on capital, primarily from a theoretical standpoint or from aggregate cross-country and/or global analyses (see [Nath et al. \(2024\)](#); [Casey et al. \(2024\)](#); [Bilal and Känzig \(2024\)](#)). [Bilal and Rossi-Hansberg \(2023\)](#) use predicted severe storm incidences to simulate a capital depreciation shock and study county-level migration and investment responses. My use of annual plant-level data and real flood incidence offers a micro-founded complement with greater detail on the dynamics and heterogeneity of post-disaster investment. Additionally, thanks to the ASM’s sporadic coverage of capital retirement (including scrapping), and its detailed information on the inventories of metal producers that allows me to closely study metal corrosion, this paper adds to this literature what is, to my knowledge, the most direct empirical evidence on the impact of weather shocks on capital depreciation—a critically understudied area in the climate economics literature.

Moreover, my work sheds new light on the credit market and welfare implications of government disaster assistance. With the exception of [Gallagher et al. \(2023\)](#), most other existing studies on post-disaster recovery focus exclusively on large, presidentially-declared storm-related disasters (see [Roth Tran and Wilson \(2023\)](#); [Deryugina \(2017\)](#)). My findings on non-declared floods help illustrate a causal role of federal disaster spending on regional outcomes. Recent work is increasingly showing that federal disaster assistance programs such as SBA disaster loans to firms ([Collier, Howell, & Rendell, 2024](#)) and households ([Collier, Hartley, et al., 2024](#)) crowd in private credit, mitigating the financial distress of disaster-affected communities. My paper shows evidence of crowd-in in the bank business lending context. Providing a richer look into manufacturing-sector productivity dynamics, my work also uncovers a novel, underappreciated channel through which government spending supports recovery in disaster-hit regions—a boost to allocative efficiency via the firm entry margin. My finding that government spending on disasters is entirely recouped via tax revenues places the policy among the most cost-effective programs in the [Hendren and Sprung-Keyser \(2020\)](#) unified MVPF framework, and offers a timely policy lesson given proposals to limit federal relief to states against the backdrop of growing disaster risks.

More broadly, this paper adds to the macroeconomics literature on reallocation across the business cycle and on the role of government spending after negative productivity shocks. [Eisfeldt and Rampini \(2006\)](#) show that capital reallocation is (counter-productively) cyclical when its benefits are, in fact, counter-cyclical, and infer the presence of frictions like financing constraints restricting reallocation during downturns. My findings put direct evidence behind this inference. Further, previous work on cleansing and credit markets focuses on recessions ([Foster et al., 2016](#); [Osotimehin & Pappadà, 2017](#)), while that on costly capital reallocation and government spending centers on demand shocks ([Ramey & Shapiro, 1998, 2001](#)). By leveraging the quasi-random timing of floods and comparing outcomes after declared versus non-declared floods, my paper offers more direct identification of how negative productivity shocks and government spending shape capital reallocation.

Lastly, this paper also contributes to the literature on entrepreneurship, physical capital, and financing. Prior work shows that young (and small) firms disproportionately purchase used capital from incumbents, in part due to financing constraints ([Eisfeldt & Rampini, 2008](#); [Ma et al., 2022](#)), often relying on descriptive designs and/or selected samples. Leveraging the representative coverage of the ASM and plausibly exogenous variation from flood timing, my study provides the first generalizable and causal evidence on how incumbent exits and capital upgrading increase used-capital purchases by new and young firms. A large body of work establishes that new and young firms, which face intense selection to survive or exit, drive overall economic reallocation and productivity growth ([Decker et al., 2020](#); [Haltiwanger et al., 2013](#)), require financing access for their experimentation ([Kerr & Nanda, 2009](#)), and amplify both negative and positive shocks ([Walsh, 2023](#)). My findings show that the post-disaster context heightens these dynamics: young firms’ managerial agility—coupled with expanded financing access from federal spending—enables them to capitalize on the disruption to scale, ultimately boosting aggregate reallocation and job creation.

2 Institutional Background

Large disasters often trigger federal disaster declarations in the US. In accordance with the Robert T. Stafford Disaster Relief and Emergency Assistance Act, 42 U.S.C. §§ 5121-5207 (the Stafford Act), a federal disaster declaration relates to major disaster events and emergencies, and it can only be made by the President of the United States. This process is initiated when a state governor, or as of 2017, a federally-recognized Indian tribal government, provides evidence that their state/locality cannot manage a certain disaster and requests federal assistance on behalf of the disaster-affected county/counties in their jurisdiction. The evi-

dence that will be presented to the president is a Preliminary Damage Assessment (PDA), within 30 days of the event, conducted jointly by the Federal, State/Tribal body that initiated the request and the Federal Emergency Management Agency ("FEMA") Regional Office that handles the request. FEMA provides assessments and advice,⁸ yet the declaration is at the discretion of the President.

Once the US president approves the request, federal funds can be used to repair public structures and to provide individual and business grants and loans.⁹ FEMA's stated policy goal is to *supplement* disaster-affected communities' recovery efforts. In particular, federal disaster assistance only covers damages that are not already covered by insurance. Disaster assistance is provided under four major programs: public assistance (e.g. debris removal, infrastructure reconstruction), individual and household assistance (e.g. legal assistance, counseling, and special unemployment payments), hazard mitigation (to reduce long-term exposure to similar risks), and low-interest disaster loans to households and firms under the Small Business Administration. Among these programs, the last one regarding loans is most relevant for firms as it is the only *direct* government recovery assistance they can receive. Federal assistance has been shown to increase business survival (Gallagher et al., 2023), and SBA disaster loans to businesses particularly crowd in private credit for borrower firms (see Collier, Howell, and Rendell (2024)). Therefore, manufacturers are expected to have broad access to financing after federally declared disasters. Other federal programs could also indirectly support firm recovery by repairing the infrastructure that's integral to manufacturers' activity, or enabling their employees to weather the storm of disasters.

While the (negative) impact of disasters on plant survival, productivity, and new entries may be more straightforward, the impact of federal disaster spending on business dynamics and productivity is ex-ante ambiguous. On one hand, federal disaster assistance may facilitate reallocation of economic resources from less to more productive plants.¹⁰ So if the positive role of federal spending wins out, then the firm entry and young firm activity margins—which are marked by the nimble and responsive nature of young firms—would be key channels through which aggregate productivity impacts materialize. On the other

⁸There are thresholds of the per-capita property damage that must be reached or surpassed for certain kinds of assistance such as broad-based public assistance to be unlocked. These vary across years, but are usually about 3 dollars per county resident (see (FEMA, 2023)).

⁹If the federal aid request is denied, the state/tribal government can appeal the decision within 30 days by providing supporting information about the inadequacy of their resources to respond to the disaster. However, denials are rare (Malewitz & Vock, 2013; CRS, 2017).

¹⁰For example, exogenous crisis events, such as the Great Boston Fire of 1872 (Hornbeck & Keniston, 2017) or the Mississippi River flooding of 1927 (Hornbeck & Naidu, 2014) have been followed by efficiency-enhancing reallocation and productive reorganization in affected regions. Models of firm dynamics conceptualize job creation and reallocation as a key aspect of productive firms that expand in response to positive business conditions, with new entrants and young firms playing an outsized role (see Haltiwanger et al. (2013)).

hand, however, the expansion of credit access after federal spending may avail cheap capital that ultimately sustains for lower-productivity units, or potentially crowds out more efficient private market lending, contributing to misallocative frictions.¹¹ As such, increased access to financing in the wake of major disasters may hurt aggregate productivity growth. These starkly different potential outcomes inherently call for an empirical analysis, whose data and methodology I discuss below.

3 Data and Empirical Strategy

3.1 Data

Data on firms:¹²

The Annual Survey of Manufactures (“ASM”): The primary data source I use is the ASM survey, which follows about 50,000-70,000 establishments for 5 years at a time, selecting a new sample in years ending with 4 and 9. The ASM is a smaller, annual complement to the Census of Manufactures (“CMF”), the full census of all $\approx 200,000$ manufacturing establishments that the Census Bureau conducts in years ending with 2 and 7. ASM sampling is based on plant size, with plants that have more than 500 workers surveyed annually. I use the version of the ASM data which include TFP information and the capital stock of manufacturing plants in each period, all of which were calculated as part of the Collaborative Microproductivity Project (see [Cunningham et al. \(2023\)](#)).

The ASM data have several features that make them suitable for my analyses. First, response is required by law, which implies that attrition or non-response related to disasters should not systematically affect my results. Second, the sample weight for each plant is provided, which allows me to account for sampling design in my analyses and work to obtain representative estimates. And third, misreporting is punishable by law, making the data reliable. One important note is that any imputations, i.e., data that are edited and entered by Census staff, are only known for Economic Census years and for non-Census ASM years from 2003 onward—flags for earlier non-Census ASM years are no longer available.¹³

¹¹E.g., [Chava et al. \(2023\)](#) find that lower entry costs are associated with higher rates of lower quality entrepreneurs who default on loans, particularly in states enacting business-friendly subsidies and taxes.

¹²These data are restricted-access datasets from the US Census Bureau and can be accessed by qualified researchers at Federal Statistical Research Data Centers (FSRDCs).

¹³Prior research has documented that substantial shares of ASM and CMF data are imputed due to item non-response, unit non-response, or implausible reported values, with capital-related variables—including book value of assets, capital expenditures, and capital retirements—among those with high imputation rates (see [White \(2014\)](#); [White et al. \(2018\)](#)).

Importantly for my study, the ASM offers information on plants' capital investment behavior separately for two categories of capital, machinery/equipment, and building/structures capital (see Figure 1). Machinery and equipment capital encompass: all production machinery and transportation equipment (automobiles, trucks, etc.), motors, lathes, punch presses, office equipment and machines; computers; furniture and fixtures for offices, cafeterias, dressing rooms; and warehouse equipment. Buildings and structures capital exclude the value of land itself, but encompass elevators, cranes, blast furnaces, brick kilns, fractionating towers, shipways and similar structures, as well as the usual factory office and warehouse type of buildings. While capital investment data are available for every year, the ASM provides an even more granular breakdown between used and new capital, within each machinery and building capital type category, up to 1996. The latter is what allows me to study the reallocation of used machines after disasters.

Figure 1: ASM survey questionnaire form from 1982: section on capital expenditures for building, and machinery assets as well as capital

Item 8 - Depreciable assets, capital expenditures, and retirements <i>Refer to the instruction manual for detailed instructions including how to report leasing arrangements</i>		1982									1981					
		Key	Total			Key	Buildings and other structures (exclude land)			Key	Machinery and equipment			Total		
			(b)				(c)				(d)			(e)		
			Mil.	Thou.	Dol.		Mil.	Thou.	Dol.		Mil.	Thou.	Dol.	Mil.	Thou.	Dol.
a.	Gross value of depreciable assets (usually original cost) at beginning of year	341				339				340						
b.	Capital expenditures for new buildings and machinery	344				342				343						
c.	Capital expenditures for used buildings and machinery	347				345				346						
d.	Retirements and disposition of depreciable assets (i.e., gross value of assets sold, retired, scrapped, destroyed, etc.)	353	(		)	351	(		)	352	(		)	(		)
e.	Gross value of depreciable assets at end of year (should equal lines 8a-8b-8c-8d)	356				354				355						

Another useful feature of the ASM data is that they have additional data on Management and Organizational Practices in years 2010 and 2015, which I use to study the nature and quality of plants' managerial responses to problems that arise in the production process (in my case, flooding). See Figure A.1 for the relevant question.

The Longitudinal Business Database ("LBD"): is the Census Bureau's main economic microdataset, providing information on the entry, exit, survival, employment, and payroll of the universe of business establishments in the U.S. The LBD is the longitudinal version of the broader Business Register dataset, which the Census Bureau maintains based on the tax records of businesses from the Internal Revenue Service ("IRS"). See [Chow et al. \(2021\)](#) for more details.

The annual nature of LBD makes a useful complement to the ASM data: first, given that the ASM is an unbalanced panel that generally only follows most plants for five years at a time, it is useful to observe the entry, exit, and survival of manufacturing plants in years in which they are not observed in the ASM. In particular, having the exit year information from

the LBD helps identify whether ASM plants that 'go missing' from the ASM sample reflect survey attrition or plant exit, which is itself a relevant post-disaster outcome to examine. I link ASM plant-year data with the LBD using internal plant identifiers. While the LBD is available starting in 1976, information on whether plants are entrants or continuers is only reliably tracked from 1977 onward, which is why my sample starts from the year 1977.

Data on flooding

FEMA Disaster Declaration Summaries: I observe the incidence of floods from the Federal Emergency Management Agency's disaster declaration records.¹⁴ Given that they are a federal record, these data provide an exhaustive and reliable list of all declared disaster events since the 1950s.

Spatial Hazard Events and Losses Database for the US ("SHELDUS"): For mechanisms analyses, in which I examine non-declared floods, I use data from SHELDUS, which provides annualized information on various hazard events and perils starting from 1960 along with the damage associated with those incidents, including the human and financial losses associated with those events and the duration of the incidents in days. I use the Version 20.0 SHELDUS database to identify flood hazards that were *not* associated with a presidential disaster declaration.

FEMA Declaration Denials: I also draw on FEMA's Declaration Denials dataset, which records instances in which a state government's request for presidential disaster assistance was denied.¹⁵ Because these records are at the state level and do not identify specific affected counties, I approximate county-level treatment by matching denied flood declarations to SHELDUS records of major flood incidents occurring in the same state and month-year, restricting to floods with at least 3 days of inundation.

Other data

Community Reinvestment Act (CRA) bank loan data: I use data on small business loans availed by the Federal Financial Institutions Examination Council (FFIEC) under the Community Reinvestment Act (CRA), which was enacted to encourage banks to meet the credit needs of the communities in which they operate. These data cover both secured and unsecured small business loans and provide, for each county, the numbers and dollar amounts of small business loans that banks originated by loan size—below 100k USD, 100k-250k USD and 250k-1 million USD—from 1996 onward. I follow seminal work around the size and credit-seeking behavior of young firms (see [Berger and Udell \(1998\)](#)) and use the smallest loan size category as a proxy for the smallest and/or youngest firms, which are widely un-

¹⁴FEMA's disaster declaration records can be accessed [here](#).

¹⁵FEMA's disaster declaration denial records can be accessed [here](#).

derstood to be more sensitive to access to financing—and vulnerable to financial constraints. *Business Dynamics Statistics (BDS)*: To complement the productivity analyses based on the tradable manufacturing sector, I draw on the BDS—the Census Bureau’s publicly available version of the LBD—to study business churn and employment dynamics in the non-tradable sectors driven by local consumption. The BDS provides annual measures of establishment and firm entries and exits as well as job creation and destruction by industry, location (including county), and firm and establishment size and age.

Bureau of Economic Analysis (BEA) income data: I use county-level personal income data from the BEA’s Regional Economic Accounts, specifically the CAINC1 table, which provides annual personal income and population estimates at the county level from 1969 onward. These data enable me to calculate per capita income for treated counties and track income recovery. The BEA constructs personal income from tax records and surveys, offering comprehensive coverage of all US counties and consistent measurement over time, making it well-suited for studying aggregate income effects of disaster shocks.

3.2 Sample

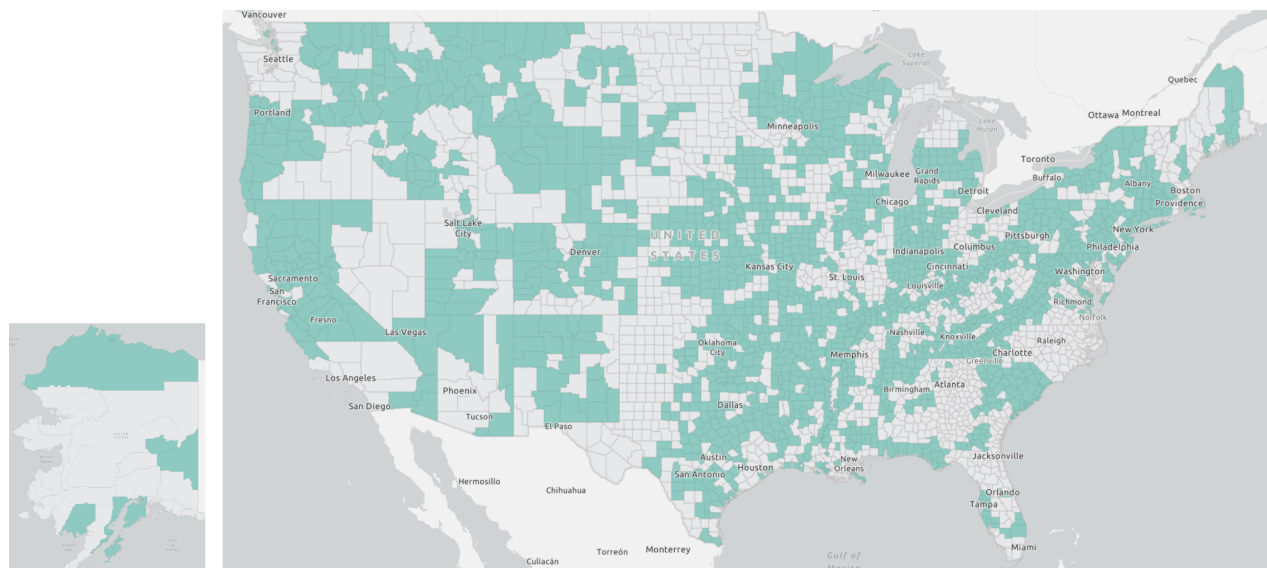
I start out with all ASM plants in the 1977-2017 period whose North American Industry Classification System (“NAICS”) industry code falls under the core manufacturing codes between 31-33, then apply some key restrictions to make the estimation sample more relevant for the purpose of my analyses. First, I apply basic restrictions to ensure that each plant-year observation has a TFP estimate, which helps to ensure that this important productivity outcome is always present, but also serves as a certification for data quality since the TFP-enhanced ASM data have undergone careful cleaning by Census researchers and have full data on plants’ capital stock calculated via the perpetual inventory method (see [Cunningham et al. \(2023\)](#)). Second, I keep only ASM plants which have a matching record in the LBD, and are marked as employer plants in the LBD. This restriction ensures that my analyses capture factories with multiple employees where production actually takes place, rather than, for instance, administrative offices of manufacturing firms.

Third, given the cyclical nature of disasters like floods, which often occur due to weather events, it is useful to isolate individual flood events as much as possible and avoid too much repetition. As such, I restrict my sample to plants located in counties which received at most 1 flood in any consecutive six years. Additionally, I keep only plants located in counties that received at least 1 federally-declared flood and at most 3 declared floods in the overall 1976-2018 period. Lastly, since my event window covers the four years before flooding and the

five years after it, I only keep these relevant years around the year of flood treatment. With these restrictions, I ultimately end up with 422,000 plant-year observations associated with about 58,500 unique firms.

Figure 2 gives the underlying *superset* of counties from which my sample of ASM plants is drawn. Due to US Census Bureau rules for confidentiality, there can be disclosure concerns around how much detail can be revealed about exactly which federally declared flood events and which affected counties with ASM-surveyed plants were included in my broad sample. As a workaround, this figure shows the superset by showing counties which had at least 1 and at most 3 declared flood in the 1976-2018 period and also had at least one manufacturing establishment reported in the BDS data in the 1977-2017 window. The figure represents 349 unique declared flood events across 1703 affected counties. These affected regions are spread out throughout the US, and are primarily caused by adverse weather events like severe storms and flash floods.

Figure 2: Counties with federally declared floods in the 1976-2018 period



Note: This map shows the underlying *superset* of counties from which my sample of all ASM plants (including exiters) is drawn. In particular, it shows, in teal, the counties which had 1-3 declared floods in the 1976-2018 period and also had at least one manufacturing establishment reported in the BDS data—the publicly-accessible equivalent of the restricted-access LBD. This map represents 349 unique floods, and 1703 unique counties that were affected.

My main sample contains approximately 422,000 plant-year observations corresponding to about 58,500 unique firms. From this sample, I leverage the information about plant age and exits to define subsamples that will allow me to study post-disaster responses and productivity outcomes among surviving plants and brand new entrants. This is because iso-

lating these business-as-usual patterns of capital (dis)investment from flood-induced capital readjustment and reallocation across plants of various ages effectively requires confronting the inherent link between firm life-cycle dynamics and capital adjustment: entrants/young plants have to invest in capital to build up their capacity, while soon-to-exit plants would likely ramp down investments and instead liquidate assets in anticipation of their closure. Thus, the subsample I use to study survivors' post-flood outcomes focuses on plants that both: survive for 3 or more years, and were age 0 or older in the year before the flood.

Table 1: Weighted summary statistics for overall sample

Variable	Mean	SD
Total revenue (in \$1000s)	21,190	159,100
Total output (in \$1000s)	22,380	271,000
Multi-unit firm	0.4103	0.4919
Production workers	69.84	219.5
All employees	97.5	346.8
Machinery capital stock (in \$1000s)	5,639	43,090
Total capital stock (in \$1000s)	9,483	65,830
N	422,000	

Statistics in this table were disclosed under (CBDRB-FY26-P2851-R12859).

Table 1 presents weighted summary statistics for my full sample of manufacturing plants. About 41 percent of these plants belong to multi-unit firms, with their output averaging roughly 22 million dollars, though standard deviations more than ten times larger suggest substantial dispersion. These plants hold about 5.6 million dollars in machinery capital stock and 9.5 million in total capital stock, again with considerable heterogeneity. The plants employ an average of about 98 employees, the majority of which are production workers directly engaged in manufacturing. Because the ASM is designed to capture a large share of manufacturing activity, unweighted moments are pulled toward larger establishments—for instance, the unweighted multi-unit share rises to 67 percent and average output roughly doubles. In contrast, the weighted moments reported here better characterize the typical plant in the manufacturing population, motivating my use of sampling weights in the regression analyses. Overall, my sample captures a broad cross-section of manufacturing establishments with substantial variation in size and capital intensity.

3.3 Empirical Strategy

I estimate the following main specification at the plant-year level:

$$Y_{p,t} = \sum_{j=-4, j \neq -1}^5 \gamma_j Flood_{p(c),t+j} + \alpha_p + \theta_{i,t} + \tau_{s,t} + \varepsilon_{p,t} \quad (1)$$

where $Y_{p,t}$ represents various outcomes of plant p , located in county c in year t , such as capital retirement. The main explanatory variable of interest is $Flood_{p(c),t+j}$, a binary dummy indicating whether the county c in which the plant is located received a declared flood j years ago. α_p denotes plant fixed effects—it helps account for plant-specific characteristics that do not change across time. $\theta_{i,t}$ denotes 4-digit NAICS industry code interacted with year: it helps to absorb potential trends faced by plants in related industries. Lastly, $\tau_{s,t}$ represents state-by-year fixed effects, which help to absorb geographic factors that may be experienced by plants located in the same state. $\varepsilon_{p,t}$ is the error term, which I cluster at the county level.

Identification comes from the assumption that the timing of the flood event is unanticipated, which is generally plausible given the exogenous timing of weather disasters, and that there are no other events to confound the effect of flooding in the event window. The latter assumption is also plausible given the narrow event window (i.e., 4 years before flooding and 5 years after it), and also the fact that other potential confounders related to business cycles, industry dynamics, or regulation would be orthogonal to the timing of an exogenous flood. Under these identifying assumptions, we can attribute changes in plant outcomes in the post-flood period to the impact of the disaster shock.

Following prior literature, all regressions based on ASM data are weighted by ASM sample weights (see [Ponticelli et al. \(2023\)](#); [Giroud \(2013\)](#); [Greenstone et al. \(2012\)](#)). The ASM sample design generally prioritizes larger plants, assigning a lower probability of selection to smaller plants. Weighting regressions helps account for such unequal sample selection probabilities and generate estimates that are more representative of the full population.

Given the variation in the timing of the flood treatment across regions, potential heterogeneity in treatment effects across time can spuriously give rise to apparent pre-trends in standard two-way fixed effects (“TWFE”) event study designs. To account for this, I conduct robustness checks using the [Sun and Abraham \(2021\)](#) event study approach, which interacts relative period indicators with cohort indicators, by using the last-treated as a control. My findings are generally robust to this approach, and I report Sun & Abraham estimates alongside the baseline TWFE results where available.

4 Main Results

4.1 Exits following disasters

Table 2 provides evidence on how disasters affect plant exits and the productivity attributes of exiters. The first column shows that, among all plants, the probability of exiting within two years increases by 3.6 percentage points (p.p.) in the 5 years following a flood compared to the 4 years before it. The second column of the table examines the heterogeneity in exit likelihood based on whether or not the plant ranked in the bottom 25% of the TFP distribution of its narrowly defined 6-digit NAICS industry in the year prior to a flood (-1 relative period). I find that, while 2-year exit likelihood generally increases after flooding, plants in the bottom quartile of the pre-flood year’s TFP distribution in their industry have an additional 1.1 p.p. higher likelihood of exit after a disaster relative to their counterparts in the top three quartiles. This suggests that it is the factories that were already marginal or low-performing during the pre-disaster period that are most likely to shut down after it.

Table 2: Plants’ 2-year exit likelihood increases after flooding, particularly among the least productive plants

	Likelihood to exit within 2 years	
	All plants	Heterogeneity
1.Post flood	0.036*** (0.005)	0.049*** (0.006)
1.LowestQuartile of TFP		-0.002 (0.007)
1.Post flood#1.LowestQuartile of TFP		0.011** (0.005)
N	422,000	257,000
Y-mean	0.11	0.10
Y-sd	0.32	0.29

This table shows plants’ likelihood to shut down within 2 years and how flooding affects it. The first column shows the event study coefficient for all plants in my overall sample, while the second column shows the heterogeneity in exit likelihoods based on whether the plant was in the lowest quartile of TFP distribution in its 6-digit NAICS industry in the year right before the flood. Thus, the sample for column 2 is the subset of plants which were observed in the ASM in the -1 relative period. Standard errors are reported in parentheses and are clustered at the county level. ***p < 0.01; **p < 0.05; *p < 0.1. Estimates in this table were disclosed under (CBDRB-FY24-P2851-R11684)

This phenomenon of excess exits among the lowest performing units appears remarkably consistent when looking at other indicators of firm productivity, including labor productivity (output per hour), value added per hour, and even firm-level revenue labor productivity

(using LBD data), suggesting that the negative productivity shock from disasters particularly distresses and eventually winnows out the least performing firms. These findings are consistent with prior evidence that exits are most concentrated among the lowest performing units during recessions, which also represent a negative productivity shock for firms (Foster et al., 2016).

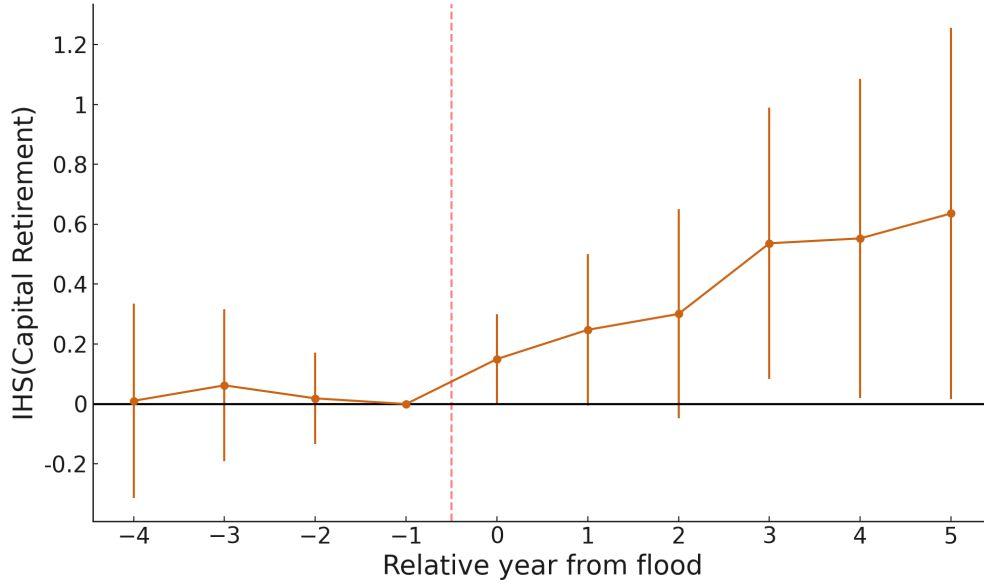
In terms of heterogeneity by plant characteristics, Appendix Table A.3 shows how this two-year exit likelihood might vary across plants that were, in the year right before the flood, young (age 4 or below) and/or a single-unit, standalone firm. I find that flooding, as before, raises exit risk, especially for young plants, but that single-unit status attenuates the increase. This suggests that the plants that are most likely to shut down after flooding are younger plants that belong to multi-unit parent firms, while small, young firms (i.e., single-unit young plants) relatively show more resilience.

4.2 Survivors’ Investment Response and Productivity Outcomes

Focusing on the subset of surviving plants—i.e., those which had already entered and were at least age 0 in the year *before* the flood, and that also continue in business for at least 3 more years—I then study their dynamics of capital retirement, investment, and productivity recovery. Figure 3 shows the total dollar value of machinery and building capital that is retired (i.e., scrapped, or sold). There are no pre-trends in capital retirement before the flood, which is consistent with our key identifying assumption about the exogenous timing of the shock. After the disaster, however, there is a substantial and persistent increase in capital disposal. The Census survey can technically include all capital sales, disposal, scrapping, as well as capital retirement from depreciation schedules under the broad ‘capital retirement’ term. Yet, given that exogenous flood shocks are entirely independent of the depreciation schedule of the assets of a plant, such a persistent increase in capital retirement after flooding must be a reflection of increased disposal and sales of capital. Moreover, this broader pattern holds even when further restricting the sample to plants which survive for much longer (e.g., 6 years), suggesting that this capital retirement is indeed a reflection of flood-related damage rather than exits.

This increase in capital retirement is consistent with the corrosive and damaging impact of water on metal. Table A.1 in the Appendix presents broader evidence of the damaging impacts of flooding on physical capital. It shows that, in addition to capital retirement, plants also see higher likelihood of incurring capital repair costs (2% more) as well as refuse (water and waste) removal costs after flooding. Further, I find that manufacturers

Figure 3: Persistent increase in capital retirement among surviving plants



This figure shows event study coefficients for capital retirement from 4 periods before flooding up to 5 periods treatment, based on plants that continue in business for at least 3 more years. 95% confidence intervals are computed based on standard errors clustered at the county level. Estimates in this figure were disclosed under (CBDRB-FY25-P2851-R11940)

processing ferrous (i.e., rust-prone) metals like iron lose work-in-process inventory of their metals, and subsequently, also employees relative to manufacturers that process corrosion-resistant metals like aluminum (see Table A.2). It is of course possible that large floods can render machinery unusable, leading to the increased capital retirement results above: for instance, disaster-related sudden power outages can cause machines to malfunction, or disaster-induced plant exits can propagate through the supply chains they leave behind and thus render other machines useless as survivors have to adjust their supply chains and adapt to different technology. Still, the aforementioned evidence on water removal and inventory destruction among ferrous metal producers demonstrates the corrosive and damaging impact of flooding that can induce capital retirement and replacement by survivors.

Table 3 summarizes the investment outcomes of plants following disasters, in particular, how net investment evolves—in raw and inverse-hyperbolic sine transformed terms. Those columns show that manufacturing plants invest more, including a 1.5% increase in investment *net* of capital retirement in post-flood years. These findings suggest that plants in flooded regions not only retire capital but also invest in capital, more than offsetting their retirements. This increase in investment is not a mere feature of survivorship bias: per Appendix Table A.4, capital investment (and net-investment) increase substantially at the

aggregate county-level after declared floods. Additionally, accounting for serial autocorrelation in investment by controlling for cubic polynomials of pre-flood investment (in the style of [Olley and Pakes \(1996\)](#) production function estimation) does not substantially change the results, confirming that these findings point toward disaster-induced investments rather than a continuation of pre-disaster investment pattern among survivors.¹⁶

Table 3: Surviving Plants’ Investment Response

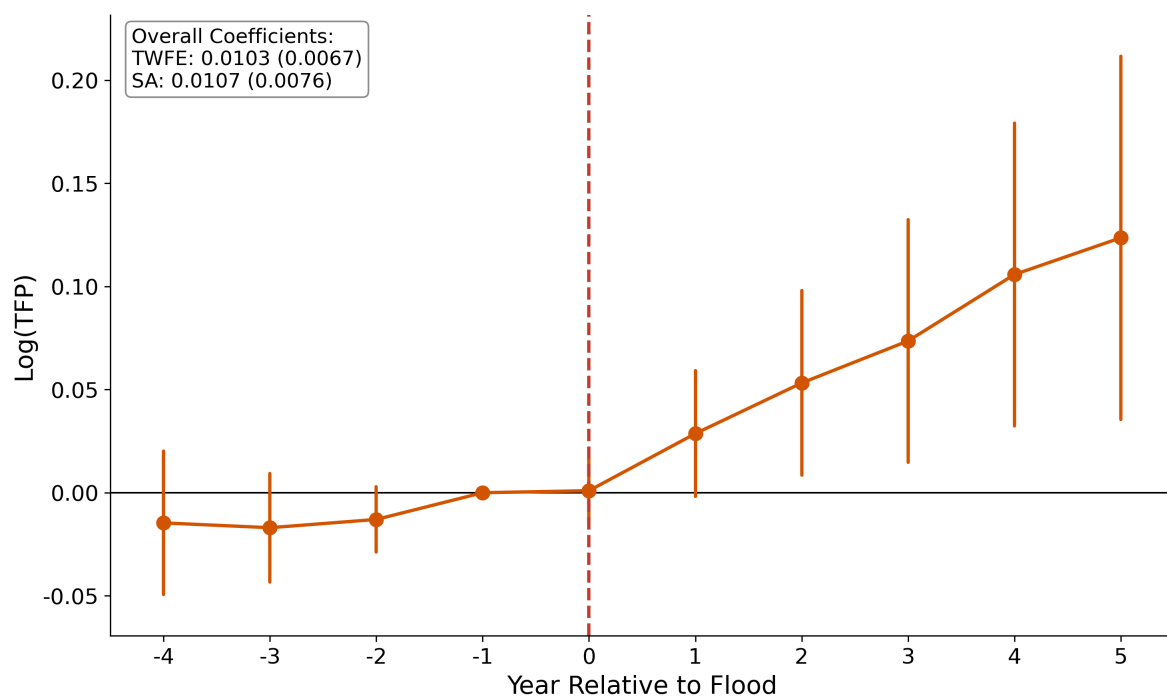
	Normalized Net Investment	IHS(Net Investment Normalized)
Post flood	0.019* (0.0101)	0.015** (0.007)
N	66,000	66,000

This table examines surviving plants’ investment after flooding. Normalized net investment is calculated as total capital investment minus total capital retirement, divided by the plant’s pre-flood year capital stock. Standard errors are reported in parentheses and are clustered at the county level. ***p < 0.01; **p < 0.05; *p < 0.1. Estimates in this table were disclosed under (CBDRB-FY25-P2851-R11949).

To illustrate the dynamics of total factor productivity, Figure 4 shows the time path of total factor productivity for surviving manufacturing plants, i.e., those which continue to operate for at least three years after being observed in the ASM data. The figure shows a persistent rise in TFP among these plants that, by the 6th year after the flood, reflects over an 11% increase over the pre-flood baseline TFP. The aforementioned findings about the weeding of the least productive plants as well as the investment in replacement capital could be contributing to this productivity increase, suggesting a role of both selection and capital upgrading in post-disaster productivity gains among survivors.

¹⁶As a robustness check, I restrict the sample to observations with non-imputed data in the years for which the Census Bureau’s edit-impute flags are available and find that the patterns documented above generally hold. That said, given the known limitations of imputation methods for capital-related variables in the ASM (see discussion in Section 3), the capital retirement and investment results in this paper are better interpreted as capturing broad patterns of post-disaster capital adjustment rather than precise magnitudes.

Figure 4: Post-flood TFP increase among surviving manufacturing plants



This figure shows event study results for how surviving plants' productivity evolves after a declared flood. Two-Way Fixed Effects coefficients are shown in the graph, with overall average treatment effect coefficients shown for both the TWFE and the Sun & Abraham approaches on the top left. Estimates in this figure were disclosed under (CBDRB-FY26-P2851-R12806).

4.3 New Entrants

An important implication of the aforementioned results is that they can increase the local supply of used capital as exiting plants liquidate their physical capital, i.e., machinery and buildings/structures, and survivors also sell their pre-existing machinery as they replace their capital. Both of these factors would contribute to a higher supply of second-hand capital, reducing its equilibrium price. A lower price of capital is likely going to be attractive for younger plants, which have been shown to buy used capital from incumbents in previous research, in part due to financing constraints that may make it harder to buy brand new capital (see [Eisfeldt and Rampini \(2008\)](#); [Ma et al. \(2022\)](#)).

Table 4 shows the investment behavior and productivity of post-flood single-unit entrants in comparison to pre-flood entrants. Here, I focus on single-unit entrants to isolate firms whose financing and organizational decisions are self-contained, rather than shaped by the resources of a larger (multi-unit) parent firm. The first column examines machinery investment dollars going toward brand new machinery as a share of all machinery investment as a function of flooding, while the second column shows the TFP of entrants. On average, the share of machinery going toward brand new machinery among post-flood entrants declines by 1.9 percentage points after flooding relative to pre-flood entrants. This suggests that, indeed, post-flood entrants do acquire the second-hand capital of older incumbents after flooding. Their productivity is also about 14% higher than pre-flood entrants, suggesting strong positive selection on the firm entry margin in the post-flood setting. Together, these findings illustrate that the post-flood setting sees a redistribution of capital from less to more productive firms.

Table 4: Post-disaster entrant firms purchase used machinery, and are positively selected

	(1)	(2)
	Share of New Machine Investment	Log(TFP)
Post flood	-0.019* (0.011)	0.139** (0.058)
N	1200	1500

This table shows event study coefficients for investment in brand new machines as a share of all machinery investment (column 1), and Log of TFP (column 2). These results are only based on the pre-1997 years to capture the ASM waves that report the breakdown between new and used capital expenditures. Since entrants are observed only once, these regressions include county fixed effects rather than plant fixed effects. Standard errors are reported in parentheses and are clustered at the county level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Estimates in this table were disclosed under (CBDRB-FY25-P2851-R12639).

Part of the success of new and young firms over older ones is explained by their nimble and responsive nature. Table 5 uses MOPS data from 2010 and 2015 to compare the monotonic scores of young (age 4 and below) versus older firms with regard to the quality of management’s response to problems in the production process. In particular, Column 1 in that table shows that younger firms have better strategies for responding to problems and setting up systems for the future compared to older ones: they have about a 10 percentage point lead over the older firms, which corresponds to a (suggestive) TFP gain. These findings are consistent with the widely documented phenomenon around how younger firms have to stay nimble and responsive to their environment, including by swiftly and conclusively responding to crisis shocks like flooding, to survive the pronounced selection pressure that they face relative to older, more established firms.

Table 5: Crisis management and productivity outcomes of young firms

	(1) Problem Fix	(2) Log(TFP)
Post flood	+	–
Young firm	–	+
Post flood X Young firm	0.097** (0.044)	0.089 (0.105)
N	8300	8300

This table shows event study coefficients for the monotonized score of plants for managing—and fixing—problems from the MOPS data (column 1), and Log of TFP (column 2). Standard errors are reported in parentheses and are clustered at the county level. ‘Young firm’ is time-invariant, indicating that the parent firm (whether single or multi-unit) of the plant was 4 years old or younger in the year before the flood. The numerical coefficients for the (non-interacted) post-flood and young firm dummies are intentionally shown only with sign and significance to minimize downstream disclosure risks and implicit sample concerns. ***p < 0.01; **p < 0.05; *p < 0.1. Estimates in this table were disclosed under (CBDRB-FY25-P2851-R12639).

4.3.1 Non-tradable Sector

Federal assistance likely expands household consumption. This means that non-tradable sectors that are sensitive to local demand will be affected after disasters, and may see greater firm entry as a result. While my previous analyses have focused on the tradable manufacturing sector to isolate impacts on plant fundamentals, in this section, I use the publicly available BDS (Business Dynamics Statistics) data to provide additional results on how firm dynamics evolve in non-tradable sectors relying on local demand, namely: “Accommoda-

tion and Food Services” (NAICS 72), “Retail Trade” (NAICS 44–45), and “Construction” (NAICS 23).¹⁷

Table 6 presents the results of this analysis, showing considerable churn and young firm activity after disasters. Post-flood counties experience measurable growth in locally demand-driven sectors, with firm counts rising by 1.1% and employment by 1.5%, indicating an aggregate expansion of non-tradable activity. Establishment churn intensifies: exits increase by 2.3%, but entries rise even more, by 3.5%. Net entry—defined as establishment entries minus exits in the BDS—increases after flooding (column 5). Because net entry can be negative and its mean is small (0.733 establishments), I interpret the coefficient of 0.054 in absolute terms rather than as a percentage: post-flood counties see a meaningful positive shift in net establishment formation, estimated in a separate regression from the gross entry and exit effects. Job creation by new establishments jumps by 4.4%, whereas job creation by incumbents remains essentially flat, reinforcing that the entry margin and young-firm activity plays a large role in post-disaster economic adjustment. Entrants’ contribution to total job creation also increases: the share of all new jobs created by entrants rises by 0.5 percentage points, a 1.6% increase relative to the baseline mean of about 31%. These results underscore that, during the post-disaster recovery period, entrepreneurial activity disproportionately drives economic churn and job creation in non-tradable sectors tied to local demand.

¹⁷This is consistent with prior work examining job creation activity in response to demand shocks (see Adelino et al. (2017)).

Table 6: Post-flood effects on firm and job dynamics

	Aggregate totals		Establishment entry and exit dynamics			Job creation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IHS(Firms)	IHS(Employment)	IHS(Exits)	IHS(Entries)	IHS(Net entries)	IHS(Job creation by births)	IHS(Job creation by continuers)	Share of all new jobs created by entrant establishments
Post flood	0.011*** (0.004)	0.015** (0.006)	0.023*** (0.007)	0.035*** (0.007)	0.054** (0.022)	0.044*** (0.011)	0.010 (0.007)	0.005** (0.002)
Constant	4.813*** (0.002)	6.998*** (0.004)	2.509*** (0.004)	2.535*** (0.004)	0.207*** (0.014)	3.782*** (0.007)	4.742*** (0.004)	0.318*** (0.001)
Observations	74,859	74,859	74,859	74,859	74,859	74,859	74,859	73,461
Y-mean	174.200	2,546.172	22.615	23.348	0.733	175.888	259.957	0.309
Y-SD	409.508	7,004.140	61.636	62.184	17.416	549.746	706.725	0.238

Notes: Standard errors are reported in parentheses and are clustered at the county level. ***p < 0.01; **p < 0.05; *p < 0.1.

5 Aggregate Implications

Table 7 examines aggregate productivity dynamics and income recovery following federally declared floods. The results suggest that disaster declarations facilitate productive reallocation of resources within affected regions. Notably, the dispersion of output per capital stock (MRPK) declines following declared disasters (column 3), indicating that capital is being reallocated away from low-productivity uses toward higher-productivity ones, which is a hallmark of efficient “cleansing” dynamics. The findings suggest that there is a 1.1% compression in the 75th-to-25th percentile gap among plants that belong to the same county and similar 4-digit industries. The Sun and Abraham estimate indicates an even stronger 2.7% decline in this interquartile range. In a frictionless economy, MRPK would be equalized across firms, but the observed shrinkage of the MRPK distribution thus implies that disaster-induced reallocation moves the economy closer to this efficient benchmark. While imprecisely estimated, the negative sign on the interquartile range of TFP (column 2) suggests a decline in productivity dispersion, as the positive sign on the mean TFP outcome (column 1) is consistent with post-disaster productivity recovering to pre-disaster baselines. Taken together, these findings suggest that resources like used machinery tend to flow toward more productive uses after federally declared disasters, supporting aggregate productivity recovery.

Table 7: Aggregate productivity and allocative efficiency following declared disasters

	(1)	(2)	(3)
	Log(TFP)	IQR(Log(TFP))	IQR(Log(Q/Capital stock))
<i>TWFE</i>			
Post flood	0.0032 (0.004)	−0.0006 (0.003)	−0.0114* (0.007)
<i>Sun & Abraham</i>			
Post flood	0.00004 (0.006)	−0.0066 (0.004)	−0.0274*** (0.008)
N	210,000	210,000	210,000

This table examines aggregate productivity and allocative efficiency indicators following declared disasters. The outcome in the first column is the log of mean TFP among plants in the same 4-digit industry within the same county; while columns 2 and 3 measure the interquartile range of TFP and of output per capital stock (MRPK) among those plants. Results are shown for both the Two-Way Fixed Effects (TWFE) and Sun & Abraham (2021) estimators. The regressions include fixed effects at the individual county-4-digit industry level as well as state-by-year and 3-digit industry-by-year fixed effects. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimates in this table were disclosed under (CBDRB-FY26-P2851-R12806).

6 Evidence on the Role of Financing Access

6.1 Recovery After Non-Declared Floods as a Counterfactual

The most straightforward way to isolate the role of financing in post-disaster recovery is to examine contexts where disaster-affected regions did not receive large federal assistance after flooding—a scenario for which non-declared disasters can serve as a proxy. I study this by using SHELDUS data to identify non-declared floods. In particular, I focus on disasters which, in the SHELDUS data, were recorded to last for 4 or more days, i.e., just above the median duration of declared floods of 3 days. This sample restriction serves two purposes: first, it helps ensure comparability of severity of disasters in recognition of the fact that non-declared disasters are often less severe than declared ones. Second, this restriction also helps ensure a large enough sample to enable the disclosure of results from the Census Bureau.

Table 8: Aggregate productivity and allocative efficiency following non-declared floods

	(1) Log(TFP)	(2) IQR(Log(TFP))	(3) IQR(Log(Q/Capital stock))
<i>TWFE</i>			
Post flood	−0.0073* (0.004)	−0.0059** (0.003)	−0.0026 (0.006)
<i>Sun & Abraham</i>			
Post flood	−0.0066 (0.004)	−0.0054* (0.003)	−0.0006 (0.006)
N	142,000	142,000	142,000

This table examines aggregate productivity and allocative efficiency indicators following non-declared floods. The outcome in the first column is the log of mean TFP among plants in the same 4-digit industry within the same county; while columns 2 and 3 measure the interquartile range of TFP and of output per capital stock (MRPK) respectively among those plants. Results are shown for both the Two-Way Fixed Effects (TWFE) and Sun & Abraham (2021) estimators. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimates in this table were disclosed under (CBDRB-FY26-P2851-R12806; -R12859).

Table 8 examines aggregate productivity dynamics following floods that did not receive federal disaster declarations. In contrast to declared floods, non-declared floods show a pattern that is inconsistent with productivity-enhancing reallocation. The coefficient on the interquartile range of MRPK (column 3), while directionally negative, is small and highly imprecisely estimated, suggesting no meaningful improvement in capital allocation after non-declared disasters. The first two columns indicate that mean TFP declines by approximately

0.73%¹⁸ and that the dispersion of TFP declines by about 0.6%. Because this decline is coupled with a suggestive decline in mean TFP, it indicates a compression from the top—such as less experimentation with high-risk-high-reward new ideas, less churn—rather than efficient reallocation from the bottom part of the productivity distribution. In contrast, in the declared setting (Table 7), the essentially flat/stable IQR(TFP) paired with stable mean TFP is more consistent with healthy turnover where both tails are active. Taken together, these results suggest that relative to declared floods where access to financing is more expansive, there is less experimentation and productivity-enhancing reallocation following non-declared floods, consistent with financing constraints stifling creative destruction and productivity recovery.

Table A.5 further illuminates the mechanisms for this dampened reallocation after disasters, pointing to both weakened cleansing on the plant exit margin and poorer selection of new firms at the entry margin as contributing factors. On the exit margin, while plant shutdowns also rise after non-declared floods—with an approximately 4.2 percentage point increase in the likelihood to exit within 2 years—the *composition* of exiters differs markedly from the declared disaster setting. Specifically, the interaction between post-flood timing and pre-flood low productivity status is positive but statistically insignificant (0.006, $p > 0.10$), indicating that low-productivity plants are no more likely to exit than their higher-productivity counterparts following non-declared floods. This stands in stark contrast to the declared disaster context, where bottom-quartile TFP plants face an economically and statistically significant higher exit probability of 1.1 percentage points relative to more productive plants. The absence of productivity-selective exits suggests that the “cleansing” margin of creative destruction—whereby negative shocks disproportionately force out inefficient producers—is substantially dampened when federal financing is unavailable.

The firm entry margin tells a similarly informative story. Table A.5 shows that, unlike the positively selected entrants observed after declared disasters, post-flood entrants in the non-declared setting exhibit no productivity increase relative to pre-flood entrants in their regions. While estimated imprecisely, the coefficient on log TFP is negative (−0.014, $p > 0.10$), in sharp contrast to the 14% TFP premium enjoyed by entrants following declared floods. This pattern is consistent with financing constraints binding on the entry margin: absent the expanded credit access that federal disaster declarations unlock, potential high-productivity entrants may lack the capital necessary to establish operations, while lower-quality entrepreneurs face reduced barriers to entry when used capital becomes avail-

¹⁸Comparing this to the declared flood setting, the difference in mean TFP recovery is approximately 1.05 percentage points ($p \approx 0.07$), suggesting that regions receiving federal disaster assistance experience meaningfully better productivity recovery than those that do not.

able at (likely) cheaper prices. The result is a failure of the entry margin to contribute to productivity-enhancing reallocation.

As a complementary exercise to the above non-declared flood analysis, I examine floods associated with *denied* federal disaster declarations and find similar patterns of dampened creative destruction. Denied declarations capture cases in which a state government requested presidential disaster assistance but the request was turned down. I obtain these denial records from FEMA’s Declaration Denials dataset. Because the denial data are recorded at the state level and do not identify specific affected counties, I approximate county-level treatment by matching denied flood declarations to SHELDDUS records of major flood incidents occurring in the same state and month-year. To further account for potential disaster severity gaps, I only focus on denied floods with at least 3 days of inundation. While these methods are approximations, the findings are still broadly consistent with the non-declared flood findings and reinforce the role of federal assistance in facilitating post-disaster creative destruction.

Table A.6 examines plant exits following approximated denied flood declarations. Plant exit rates rise: the likelihood of exiting within two years increases by approximately 3.7 percentage points ($p < 0.05$), representing a roughly 42% increase relative to the baseline exit rate of 8.9%. However, the interaction between post-flood timing and pre-flood low-productivity status is economically small (0.1 percentage points) and statistically insignificant ($p > 0.10$). Consistent with the broader non-declared flood results in Table A.5, this suggests that, absent federal disaster assistance, the productivity-selective cleansing observed after declared floods (Table 2) does not materialize: low-productivity plants are no more likely to exit than their higher-productivity counterparts.

Table A.7 examines aggregate productivity dynamics in county-industry cells associated with approximated denied flood declarations. Mean TFP declines following denied declarations, though the estimate is imprecise (column 1). Notably, and in contrast to the compression of productivity dispersion observed after declared disasters (Table 7), the dispersion of TFP increases: both the interdecile range (column 2) and the standard deviation of log TFP (column 4) rise significantly ($p < 0.10$), indicating a widening of the productivity distribution within narrowly defined county-industry cells. The interquartile range of TFP (column 3) is also positive, though imprecisely estimated. While MRPK dispersion appears to decline (in column 5), those coefficients are imprecisely estimated, and importantly, the fact that it coincides with suggestive rises in productivity dispersion indicates a failure of efficient reallocation.

Overall, my findings highlight the crucial role of federal disaster assistance in facil-

itating productive reallocation during disaster recovery. The lack of productivity-selective exits after non-declared means that scarce resources may remain trapped in less efficient uses, while the absence of positive selection on entry implies that the new establishments replacing exiters are no more productive than their predecessors. As such, absent the expanded financing access and broad-based rebuilding that accompany disaster declarations, flooded regions may see poor efficiency in resource reallocation and/or lower experimentation that ultimately restricts aggregate productivity recovery. This contrast between declared and non-declared floods provides compelling evidence that federal disaster spending is not merely transfers to affected communities, but rather enables economic renewal through productive restructuring after disaster-induced destruction.

6.2 Crowding in of Bank Loans

Employing the CRA data and using loan size as a proxy for firm size and age, I show that small-sized bank loans increase after flooding. If federal disaster spending expands access to financing for especially the smaller and younger plants, then we ought to see an increase in loans in the smallest loan size categories. This is exactly what my analysis finds.

Table 9 shows business loan dynamics following federally declared floods, focusing both on the total number and dollar amount of loans originated (first two columns), as well as the average loan size, and the breakdown of sub-\$100,000 versus over \$100,000 loan sizes (latter four columns). The findings indicate that the number of loans originated by banks to small businesses increases after federally declared floods by about 3.1%, but with a statistically insignificant increase of about 2% in the total loan dollars originated (column 2). This increase in lending is almost entirely driven by the smallest loan size category—the sub-100,000 USD loan size group—which also sees an increase of more than 3.5% in both dollar amounts of loans and the number of loans originated. Lastly, the share of loans in this category increases relative to larger loan sizes as well by about 0.3 percentage points, which is a 0.32% increase over the pre-flood baseline of 93.5 percent of loans in this small loan size category. Overall, these results suggest that bank loan supply increases after major, federally declared floods, and that this is particularly most pronounced in the smallest loan size categories, suggesting disproportionate benefits to young and small firms.

Table 9: Bank business loans following declared floods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IHS(Total lending)	IHS(Total Loans #)	IHS(Avg loan amount)	IHS(# of sub100k loans)	IHS(Total sub100k lending)	Share of sub100k loan dollars	Share of sub100k loan #
Post flood	0.019 (0.020)	0.031*** (0.012)	-0.013 (0.013)	0.035*** (0.012)	0.039** (0.016)	0.006 (0.005)	0.003** (0.001)
Observations	10272	10272	10270	10272	10272	10270	10270

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.3 Relationship between Firm Size, Plant Age, and Investment

Table 10: Investment, Investment Composition, and Productivity

	(1) IHS(Investment)	(2) Share of <i>New</i> Capital Investment	(3) Log(TFP)
Post flood	-0.029 (0.041)	0.003 (0.006)	0.003 (0.007)
Young plant	0.006 (0.101)	-0.031** (0.013)	-0.014 (0.028)
Post flood X Young plant	-0.215*** (0.046)	0.022*** (0.007)	0.028** (0.011)
Single-unit firm	0.025 (0.121)	0.03 (0.021)	-0.033 (0.023)
Post flood X Single-unit firm	-0.123*** (0.042)	-0.012 (0.007)	0.009 (0.006)
Young plant X Single-unit firm	-0.23 (0.349)	-0.011 (0.041)	0.065 (0.104)
Post flood X Young plant X Single-unit firm	0.206** (0.095)	-0.041** (0.021)	0.027 (0.026)
N	234,000	129,000	234,000

This table shows how plant-level investment and productivity evolve after flooding, with a focus on the heterogeneity by whether the plant was young (age 4 or below) or a single-unit firm in the year before the flood. Column 1 captures the inverse hyperbolic sine transformed version of total capital investment, Column 2 captures the share of dollars invested toward brand new capital (instead of used capital) as a share of total investment, and Column 3 captures the log of TFP. Plant age and single-unit firm status are defined as of each flood’s pre-event year. Plants observed across multiple flood cohorts may have different values across events, providing within-plant variation that identifies the main effects alongside plant fixed effects. Standard errors are reported in parentheses and are clustered at the county level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Estimates in this table were disclosed under (CBDRB-FY25-P2851-R12639).

Another indicator of the expansion of access to financing in the wake of large disasters is the relationship between firm size, plant age, and investment. Table 10 examines how plant-level investment and total factor productivity evolve following flooding, and how responses vary by pre-flood characteristics of plant age (with age 4 or below being young’) and firm size (single vs multi unit parent firm). The baseline (omitted) group is older (age 5 or above) plants that also belong to multi-unit parent firms. To strengthen inference, all characteristics are held fixed for each flood event, defined at the year before the disaster. The first column shows that, on average, investment sees a small, statistically insignificant decline after flooding in this baseline group. Young plants and single-unit plants experience larger post-flood investment declines than older plants belonging to multi-unit firms do after flooding. Importantly, the triple interaction ($\text{Post} \times \text{Young} \times \text{Single-unit}$) is positive and statistically significant, indicating that the investment decline for young single-unit firms is smaller than the sum of the individual young-plant and single-unit penalties would

predict—consistent with expanded access to financing in the post-disaster context disproportionately benefiting the firms that are typically most credit-constrained. The second column shows capital expenditures in brand new (as opposed to used) capital as an outcome: single-unit young firms are most likely to invest in used capital, while young plants belonging to older, multi-unit parent firms are more likely to buy brand new capital assets. My analyses focusing on a limited set of survey years for which the ASM provides more detailed breakdowns of machinery investments suggest that these single-unit young firms have roughly a 13% higher likelihood of buying computer-related equipment in post-flood years ($p < 0.05$). Column 3 shows that young plants belonging to multi-unit firms see TFP gains of about 2.8% after flooding. The triple interaction for young single-unit plants is positive but not statistically significant, suggesting that purchasing used rather than new capital does not lock them into outdated technology that hurts their productivity.

Overall, the fact that single-unit, young firms see attenuated investment declines suggests that post-disaster financing access helps prevent what would otherwise be a substantial investment collapse among the most constrained firms. Interpreted together, the interaction terms show heterogeneous rebuilding consistent with credit expansion enabling economic revival that is driven by younger plants.

6.4 Evidence from Survey on Firms

To complement the administrative data analyses, I conducted a qualitative survey of manufacturing firms in the Midwest, primarily in partnership with the Federal Reserve Bank of Chicago and the Wisconsin Center for Manufacturing and Productivity.¹⁹ The survey collected 23 complete responses from CEOs or other high-level executives of manufacturing firms. The findings corroborate insights from the quantitative analysis for two main reasons. First, financing constraints and uncertainty about technology returns are the primary barriers to capital investment under normal conditions. Second, in hypothetical flood scenarios, firms' propensity to upgrade machinery during rebuilding increases sharply with damage severity—while 70% would simply repair under minimal (5%) damage, 35% would upgrade under severe (65%) damage, particularly if they had ample access to financing. These findings suggest that disaster-induced production shutdowns reduce the opportunity cost of researching and implementing new technology while post-disaster insurance proceeds and government assistance could provide financing typically unavailable in normal times. Thus, declared disasters can create conditions that simultaneously lower investment barriers

¹⁹I thank Thomas Walstrum, Buckley Brinkman, and Joylynn Gilles for help with survey dissemination.

and expand access to capital, enabling productivity-enhancing upgrading and reallocation. See Appendix Section B for a more detailed discussion of this qualitative survey on firms.

7 Marginal Value of Public Funds

The preceding sections established that federal disaster assistance facilitates productivity-enhancing creative destruction. But a natural question for policymakers is whether these efficiency gains justify the fiscal cost. To evaluate this, I compute the marginal value of public funds (MVPF), following the framework of [Hendren and Sprung-Keyser \(2020\)](#), which compares beneficiaries’ willingness to pay for a policy to its net cost to the government. A key input to this calculation is the causal effect of federal disaster spending on aggregate income, which determines both the welfare gains to beneficiaries and the incremental tax revenues that offset the government’s upfront outlay. Thus, I estimate the income effects of declared versus non-declared floods using county-level per capita personal income data from the Bureau of Economic Analysis (BEA) Regional Economic Accounts.

Table 11 shows a stark divergence in income recovery across declared and non-declared flood counties. Per capita income suggestively rises by approximately 0.7% annually in the post-flood period after declared disasters (column 1). In contrast, however, per capita income declines by approximately 0.9% after non-declared floods (column 2). The findings are consistent with the creative destruction dynamics documented in preceding sections about these regions: the productivity-enhancing reallocation in declared flood regions may translate into broader income gains for affected communities, while dampened creative destruction in non-declared flood settings could plausibly translate into weaker economic recovery. Of course, local demand may also play an explanatory role: the fact that local demand expands and that new jobs are created (mainly by entrants) in declared flood regions could also contribute to rising per capita incomes, while shrinking household consumption and slower job creation in non-declared flood regions could become a drag on income recovery. In either case, the substantial difference in coefficients implies highly divergent recovery paths, with a net income gain of approximately 1.5 percentage points ($p < 0.01$) attributable to federal disaster assistance. This gap between recovery trajectories with and without federal aid forms the basis of the MVPF calculation that follows.

I calculate the MVPF of federal disaster spending as shown in Equation 2. I focus on a subsample covering the years 1993-2017, which is when data on federal disaster spending outlays are most reliably and readily available. The exact coefficient difference for this period is also roughly 1.5p.p. ($p < 0.05$). See Table B.8 for detailed calculations.

Table 11: Income recovery following declared versus non-declared floods (population-weighted)

	(1) Declared floods IHS(Income per capita)	(2) Non-declared floods IHS(Income per capita)
<i>TWFE</i>		
Post flood	0.0073 (0.0050)	−0.0093** (0.0037)
<i>Sun & Abraham</i>		
Post flood	0.0065 (0.0041)	−0.0087** (0.0034)
N	21,859	17,010
TWFE difference: 0.0166*** (t-stat $\approx$ 2.67; $p < 0.01$)		
SA difference: 0.0152*** (t-stat $\approx$ 2.85; $p < 0.01$)		
Subsample for years 1993–2017		
	Declared floods IHS(Income per capita)	Non-declared floods IHS(Income per capita)
<i>TWFE</i>		
Post flood	0.0125*** (0.0041)	−0.0028 (0.0042)
<i>Sun & Abraham</i>		
Post flood	0.0075* (0.0042)	−0.0075 (0.0055)
N	12,972	7,043
TWFE difference: 0.0153*** (t-stat $\approx$ 2.61; $p < 0.01$)		
SA difference: 0.0150** (t-stat $\approx$ 2.17; $p < 0.05$)		

This table examines county-level income recovery following declared floods (column 1) and non-declared floods (column 2), using population-weighted regressions. Results are shown for both the Two-Way Fixed Effects (TWFE) and Sun & Abraham (2021) estimators, with the 2018 flood cohort used as the control cohort in the SA method. Since authoritative information on FEMA spending is only available for 1993 and after, the bottom panel reproduces the regression results for just the year range 1993–2017 for consistency. The outcome in both columns is the inverse hyperbolic sine of per capita personal income in the county. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

$$\text{MVPF} = \frac{\text{Willingness to Pay (WTP) of beneficiaries}}{\text{Total upfront cost} - \text{Tax revenues}} \quad (2)$$

On the cost side, the primary upfront expenditure is federal outlays for flood-related disaster declarations, totaling roughly \$35 billion (in 2022 US dollars). I also account for the budgetary cost of SBA disaster lending. Applying the 14% credit-subsidy rate—which under the Federal Credit Reform Act represents the present value of the government’s expected net cost over the full loan life—to \$25.0 billion in estimated SBA disaster loan disbursements implies an additional \$3.50 billion in federal cost.²⁰ The total fiscal cost is thus \$38.5 billion.

On the revenue side, I translate the estimated income gains in disaster-declared counties into incremental federal receipts. The difference in post-flood income trajectories between declared and non-declared flood counties in the 1993–2017 subsample in Table 11 indicates a relative per capita income increase of 1.53% after flooding ($p < 0.01$). Given that the average pre-flood per capita income was roughly \$43,952, this net income impact coefficient implies an estimated annual incremental income of \$123.94 billion across the 1,492 treated counties in the BEA income regression, sustained over the six-year event-study window, yielding a cumulative incremental income of \$743.64 billion.²¹ Assuming an effective tax rate of 15%, this translates to incremental tax revenue of \$111.55 billion—well in excess of the \$38.50 billion gross fiscal cost. The difference implies a fiscal surplus of \$73.05 billion (i.e., net cost = −\$73.05 B).

Beneficiaries’ willingness to pay is defined as the post-tax portion of the incremental income gains attributable to federal disaster assistance, totaling \$632.09 billion.²² Because net fiscal cost is negative, the MVPF is effectively infinite: the policy pays for itself in budgetary terms, so any positive WTP implies an MVPF exceeding any finite bound.

²⁰This treatment uses the full present-value credit subsidy and thus overstates the fiscal burden realized within the 6-year post-flood window, since defaults and interest subsidies accrue over the full 30-year loan maturity. This conservative choice ensures the MVPF calculation is not sensitive to assumptions about the timing of subsidy realization.

²¹The aggregation multiplies the estimated percentage effect by average baseline per capita income, average county population, and the number of treated counties (see Table B.8, row F). I estimate the income regressions weighting by county population, so that the coefficient reflects the effect for the average person rather than the average county. As a robustness check, I re-estimate the income regressions without population weights; the unweighted income gap is 0.96 percentage points (see Appendix Table B.9), smaller than the population-weighted 1.53 percentage points used in the baseline calculation but still well above the breakeven threshold required for an infinite MVPF.

²²The BEA personal income measure used in the income regressions captures wage, salary, and proprietor income but does not include loan principal as income, so the estimated income gains and SBA lending are not double-counted. The SBA disaster loan program enters the MVPF calculation only on the cost side, via the credit subsidy, consistent with the fact that loan principal must be repaid and does not constitute a transfer to beneficiaries.

The resulting infinite MVPF indicates that federal disaster assistance more than pays for itself in budgetary terms while generating substantial welfare gains for affected communities. This places disaster assistance among the most cost-effective government programs documented in the MVPF literature. This underscores the broader policy relevance of my results: federal disaster spending facilitates productivity-enhancing reallocation—including by supporting entrepreneurial activity that makes good use of the resources shed by exiters, like used capital, and creates jobs—ultimately generating fiscal returns that outweigh upfront government costs.

One potential measurement concern here is that FEMA Individual Assistance grants to households are excluded from federal taxable income under 26 U.S.C. § 139,²³ yet may be captured in the BEA personal income measure as government transfer payments. To the extent that the estimated income gains partly reflect these non-taxable transfers, applying the effective tax rate to the full incremental income overstates tax revenue. But household grants constitute only a fraction of total federal disaster spending as other large spending categories like Public Assistance (e.g., debris removal, infrastructure repair) and hazard mitigation do not enter personal income. Even under the extreme assumption that the entire \$35 billion in FEMA outlays appeared in BEA personal income as non-taxable transfers, the implied tax revenue on the remaining incremental income would still be about \$106 billion—nearly triple the \$38.5 billion gross fiscal cost—leaving the infinite MVPF result unchanged.

Overall, the infinite MVPF result not only rests on conservative and reasonable assumptions but is also potentially a lower bound of the true bang-for-buck of government spending on disaster recovery. I use the full present-value credit subsidy to calculate SBA costs, and I define WTP solely as post-tax income gains (excluding any potential welfare gains to households from subsidized lending). The tax revenue calculation is also conservative in that it captures only taxes on personal income; corporate income taxes on retained profits—which would plausibly rise as productivity-enhancing reallocation can boost firm profits, and thus tax revenues—are not reflected in the BEA income measure. Importantly, to the extent that non-declared floods represent milder shocks than declared ones, the 1.5p.p. relative income gap may understate the causal effect of federal assistance on income recovery. The key threshold is that incremental tax revenues exceed upfront costs by about \$73 billion—my estimated income effect would need to be overstated by roughly two-thirds for the MVPF to become finite.

Note that these MVPF calculations only apply to regions like those in my sample, which excludes counties with frequent or repeated flooding to ensure clean identification. Moral

²³See <https://www.law.cornell.edu/uscode/text/26/139>.

hazard concerns around rebuilding are, understandably, more salient in coastal areas facing repeated events; and the question of whether relocation *away* from these regions instead of rebuilding them is more prudent in the long run merits its own closer study. However, while agnostic on this issue, my MVPF estimates still lend key policy insights relevant to a much broader set of regions for which climate change is expected to increase the risk of severe weather events—and with it, policy debates over the fiscal implications of rebuilding.

8 Conclusion

This paper documents the creative destruction process that follows large, infrequent floods. As a negative productivity shock, flooding amplifies the *destructive* margin of this process: exit rates rise, and the least productive plants are disproportionately culled. But for allocative efficiency to actually improve, resources must simultaneously flow toward the economy’s *creative* margin—to more productive survivors and positively selected entrants. I show that federal disaster spending facilitates exactly this: while the exogenous disaster disproportionately forces exits among low-productivity plants, federal assistance expands firms’ financing access and crowds in private credit, particularly for nimble, positively selected entrants and young firms that absorb the excess used capital and drive reallocation. Survivors also upgrade their capital as they rebuild and see productivity gains. Ultimately, capital is reallocated toward better use after flooding.

In counterfactual settings without federal assistance—including non-declared floods and denied declaration requests—this confluence breaks down. Exits are indiscriminate of productivity, entrants show no TFP advantage, and aggregate productivity declines. The divergence underscores that financing constraints are distortionary in the post-disaster setting, and that federal spending plays a key role in relaxing them. Using county-level income data and the [Hendren and Sprung-Keyser \(2020\)](#) framework, I find that the incremental tax revenues from income gains in declared-flood counties more than offset the upfront costs of disaster relief, implying an infinite marginal value of public funds. Federal disaster spending is not only efficiency-enhancing but fiscally sound.

The idea of cleansing floods is hardly new: in fact, it is a recurring theme across cultures and religions throughout human history, from Mesopotamian and Mesoamerican mythologies to the Abrahamic religions and Hinduism, all of which recount catastrophic floods that destroy and give way to rebirth. Since the seminal work of [Schumpeter \(1939\)](#), a long literature has also studied the economic analog in the context of crises like recessions. What this paper adds is a detailed account of how cleansing operates through firm

dynamics and subsequent reallocation of used capital, thereby reconciling how disaster-hit regions can emerge better off than before: this requires broad-based spending and liquidity access, which federal disaster assistance facilitates. In doing so, my paper identifies a novel, underappreciated role of policy and government spending in supporting allocative efficiency and productivity recovery after crises.

Disaster shocks combined with federal spending provide a useful, albeit tragic, context in which to study the frictions that inhibit productive reallocation. We may wonder: what market failures were inhibiting efficient reallocation *before* flooding? Could a cash influx to firms attain the same efficiency gains without the devastation of a flood? These are important questions, but beyond the scope of this paper: my results establish financing access as a *necessary* condition for allocative efficiency gains in the post-disaster context, but since we cannot observe federal aid in the absence of a disaster shock, whether it is also *sufficient* remains an open question. Given the broader trends of declining business dynamism in the US ([Akcigit & Ates, 2021](#)), and in particular that incumbents have grown less responsive to shocks in their environments ([Decker et al., 2020](#)), it is not obvious whether expanded credit alone would generate the same degree of churn and productive reallocation absent the extreme disruption that exogenous disasters impose. What my findings unambiguously establish is that, within the post-disaster context, well-funded and well-managed programs are critical for productivity recovery. This carries particular policy relevance given ongoing proposals to shift disaster management responsibilities—and their funding—from the federal government to states, which typically have lower financial resources than the federal government.²⁴ My findings show that inadequate post-disaster liquidity can suppress creative destruction and leave affected regions worse off. As climate change magnifies extreme weather events for US regions, including those with little historical exposure, ensuring adequate disaster recovery funding—regardless of its source—will only become more consequential.

²⁴See, e.g., recent analyses questioning whether state governments have the fiscal capacity to absorb disaster recovery costs currently borne by the federal government ([Urban Institute, 2025](#)).

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A Appendix

Figure A.1: MOPS survey questionnaire from 2015 asking about how the establishment responded to a problem

Section A - Management Practices		
1	In 2010 and 2015, what best describes what happened at this establishment when a problem in the production process arose?	
Examples: Finding a quality defect in a product or a piece of machinery breaking down.		
Mark one box for each year	2010	2015
We fixed it but did not take further action	☐	☐
We fixed it and took action to make sure that it did not happen again	☐	☐
We fixed it and took action to make sure that it did not happen again, and had a continuous improvement process to anticipate problems like these in advance	☐	☐
No action was taken	☐	☐

A.1 Calculation of plant age across the ASM and LBD

By combining information from the ASM and LBD, I calculate plant age in a similar manner as previous studies (see [Haltiwanger et al. \(2013\)](#)) by marking the first year of observation as the year of entry and letting the plant age naturally by 1 year in each following year after that. The only difference in my setting is that since I have ASM data from 1972, with full information on whether plants are age 0-4 or age 5 and above, in addition to the LBD data (which only start in 1976), I can cross reference the two datasets to construct a more accurate age for each manufacturing establishment. I proceed in the following two steps.

1. First, identify year of entry across ASM and LBD
 - Start with year first observed (as an employer business) in the LBD
 - If the year that the plant is observed in the ASM is earlier, I update the entry year to the year of first observation in the ASM data (if that year's value is flagged as young), and if it's not, to the ASM first year minus 5 years.
2. Then to identify age, deduct the year of entry from the 'current' year for any ASM plant year observation

A.2 Additional Tables and Figures

Table A.1: Plants retire capital; incur capital repair and refuse removal costs due to flooding

	Capital Retirement		Operating costs	
	Any retire?	IHS(Retired capital \$)	Any capital repair?	Any refuse removal?
Post flood	0.013 (0.013)	0.092** (0.046)	0.018** (0.009)	0.029** (0.014)
N	116,000	116,000	107,000	88,500
Y-mean	0.65	619.3K	0.86	
Y-sd	0.48	12,540K	0.35	

Estimates in this table were disclosed under (CBDRB-FY24-P2851-R11700; CBDRB-FY25-P2851-R11949)

Metal corrosion as a mechanism of flood damage:

To shed light on the physical damage to physical capital from flooding, Table A.2 shows the heterogeneity in outcomes of manufacturing plants belonging to industries that process more versus less corrosion-prone metals. This is because humidity accelerates chemical processes that corrode metal, particularly for ferrous metals like iron, and its alloys. In contrast, 'non-ferrous' metals like aluminum, copper, and silver are more resistant to corrosion because they form protective layers. Under this framework, looking at how work-in-process inventory changes within plant across ferrous and non-ferrous metal processing industries in the year of a flood would be a direct and meaningful way to test whether flood waters intensify metal corrosion, and what the downstream impacts may be.

Table A.2 shows some supporting evidence for the hypothesis that water damage from flooding can accelerate metal corrosion. According to the first three columns of the table, plants which process ferrous metals end up with a lower work-in-process inventory of iron at the end of the year compared to the work-in-process inventory they started out with at the beginning of the year relative to plants which process non-ferrous metals. Though estimated imprecisely, they are about 6 percentage points more likely to end post-flood years with lower inventory than they started out with at the beginning. This becomes a 10% drop once inventory change is normalized by pre-flood total work-in-process inventory size, still a significant indication that ferrous metal producers face inventory losses related to flood waters. This is consistent with how, particularly in its unfinished, processing stage, iron is even more sensitive to humidity and thus, more prone to rust and waste away.

This inventory loss is potentially consequential: the latter two columns of Table A.2 show that in the 5 years following a flood, plants which process ferrous metals have about 5% lower employment and payroll relative to non-ferrous metal producers. Taken together,

Table A.2: Due to the effect of moisture on metal corrosion, manufacturing plants in industries that process ferrous metals experience rust-related inventory loss and subsequently downsize

	Dependent variable: Inventory change			Employment and payroll	
	Any loss? (1)	IHS(Invent. change) (2)	IHS(Normalized invent. change) (3)	Log(Employment) (4)	Log(Payroll) (5)
Post flood X Ferrous	0.06 (0.042)	-0.667* (0.372)	-0.104** (0.048)	-0.049** (0.020)	-0.048** (0.022)
N	9,000	9,000	7,700	28,500	28,500
Y-mean	0.384	127.4	-0.242	146.2	5,520
Y-sd	0.486	8,404	10.74	463.7	21,800

This table shows how plant inventory, employment, and payroll evolve after flooding, with a particular focus on the heterogeneity across plants which process metals that are more prone to rust (ferrous metals) relative to less rust-prone metals. All results are based on the specific samples of metal processors in the ASM data (Columns 1-3), and LBD data (Columns 4-5). The dependent variable in column 1 is a binary indicator for whether end of year work-in-process inventory is lower than the beginning of year amount, while columns 2 and 3 are continuous measures of the difference between the end of year and beginning of year amounts, with column 3 further being normalized by the pre-flood year's beginning of year inventory to account for size. The dependent variables in columns 4 and 5 represent plant employment and total annual payroll. Standard errors are reported in parentheses and are clustered at the county level. ***p < 0.01; **p < 0.05; *p < 0.1. Results in this table were disclosed under (CBDRB-FY25-P2851-R11949)

these results are consistent with the interpretation that inventory-loss induced by flooding causes plants to downsize. Importantly, iron and steel production is itself a critical industry that provides raw materials for construction and other *durable* goods production. Thus, these inventory losses themselves are likely to have large implications in downstream *capital (durable) goods* production, and add more insight to the small but growing literature demonstrating the impact of climate change on investment goods (see [Casey et al. \(2024\)](#)).

Table A.3: Heterogeneity in likelihood to exit within 2 years by pre-flood plant age and parent firm size

	Exit within 2 years
Post flood	0.053*** (0.006)
Young plant	-0.048*** (0.016)
Post flood X Young plant	0.059*** (0.009)
Single-unit firm	0.007 (0.014)
Post flood X Single-unit firm	-0.017*** (0.005)
Young plant X Single-unit firm	-0.113* (0.062)
Post flood X Young plant X Single-unit firm	-0.011 (0.016)
N	257,000

This table shows event study results on how plants' 2-year exit likelihood is affected after flooding and the heterogeneity by plant age (age 0-4 being young) and firm size (single vs multi-unit parent firm). Plant age and single-unit firm status are defined as of each flood's pre-event year. Plants observed across multiple flood cohorts may have different values across events, providing within-plant variation that identifies the main effects alongside plant fixed effects. Standard errors are reported in parentheses and are clustered at the county level. ***p < 0.01; **p < 0.05; *p < 0.1. Estimates in this table were disclosed under (CBDRB-FY25-P2851-R12639)

Table A.4: County-level total investment increases following flooding

	(1) IHS(Capital Investment)	(2) IHS(Normalized Investment)	(3) IHS(Normalized Net Investment)
Post Flood	0.073* (0.044)	0.0312*** (0.008)	0.019** (0.009)
N	21,500	17,500	5,700

This table shows how county-level investment changes after flooding. Column 1 focuses on total capital investment, column 2 captures investment normalized by pre-flood year capital stock, and column 3 shows net investment (investment-retirement) normalized by pre-flood capital stock in the county. All outcome variables are shown in their inverse hyperbolic sine transformed formats. Standard errors are reported in parentheses and are clustered at the county level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Estimates in this table were disclosed under (CBDRB-FY25-P2851-R11949)

Table A.5: Dampened Creative Destruction Following Non-Declared Floods

	Exiters	Entrants	
	(1) Exit	(2) Log TFP	(3) New machine investment share
Post Flood	0.042*** (0.005)	−0.014 (0.035)	−0.006 (0.015)
Lowest Quartile of TFP	0.006 (0.009)		
Post Flood × Lowest Quartile of TFP	0.006 (0.007)		
Observations	151,000	3,900	2,800

Notes: This table examines creative destruction dynamics following floods that did not receive federal disaster declarations. Column (1) reports plant exit probabilities; the interaction term tests whether low-productivity plants (bottom TFP quartile in pre-flood year) are differentially likely to exit. Columns (2) and (3) examine characteristics of new entrants, showing their Log TFP outcomes and the share of machinery investment going toward brand new (as opposed to used) machines, respectively. Standard errors clustered at the county level in parentheses. Observation counts rounded per Census disclosure requirements. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimates in this table were disclosed under (CBDRB-FY26-P2851-R12835).

Table A.6: Plants' 2-year exit likelihood increases after denied flood declarations, but without productivity-selective cleansing

	Likelihood to exit within 2 years
1.Post flood	0.037** (0.016)
1.LowestQuartile of TFP	−0.031*** (0.012)
1.Post flood#1.LowestQuartile of TFP	0.001 (0.009)
N	48,500
Y-mean	0.089
Y-sd	0.28

Notes: This table shows plants' likelihood to shut down within 2 years following floods associated with denied federal disaster declarations, particularly focusing on the heterogeneity in exit likelihoods based on whether the plant was in the lowest quartile of TFP distribution in its 6-digit NAICS industry in the year right before the flood (the -1 relative period). Standard errors are reported in parentheses and are clustered at the county level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Estimates in this table were disclosed under (CBDRB-FY26-P2851-R12806).

B Evidence from Qualitative Survey on Firms

To complement my analyses based on administrative data, I conducted a qualitative survey on manufacturing firms in the US. While the data-driven analyses illustrate key insights into how firms respond to and recover from disasters, there is still much we do not know about firms' incentives and barriers around capital investment, as well as about the translation between book value of capital assets, which is what the Census data contain, and the market value, which is better suited to understand disaster impacts. Thus, the qualitative survey is useful to gain a fuller understanding of such important issues.

The survey was designed and disseminated in collaboration with various governmental and non-governmental organizations, primarily via the Federal Reserve Bank of Chicago and the Wisconsin Center for Manufacturing and Productivity.²⁵ To incentivize participation in the survey, I offered the incentive that a randomly-chosen respondent would get to donate \$1000 to a non-profit organization of their choice (among a pre-selected list of 50 organizations located in the Midwestern US), anonymously if preferred.²⁶ This is because

²⁵See Figure B.4 for a template of the survey invitation message.

²⁶The funds for the donation were secured via a grant from the Columbia Center for Political Economy. The list of 50 potential organizations among which the lottery winner could pick one donation recipient was

Table A.7: Productivity and allocative efficiency after denied declaration requests

	Productivity	TFP Dispersion Measures			Log(Q/K) Dispersion
	Log(TFP)	IDR	IQR	SD	IQR(Log(Q/K))
	(1)	(2)	(3)	(4)	(5)
<i>TWFE</i>					
Post flood	−0.023	0.036*	0.024	0.019*	−0.066
	(0.026)	(0.021)	(0.019)	(0.012)	(0.040)
<i>Sun & Abraham</i>					
Post flood	−0.021	0.013	0.016	0.012	−0.027
	(0.033)	(0.020)	(0.018)	(0.012)	(0.020)
N	43,000	43,000	43,000	43,000	43,000

Notes: This table examines aggregate productivity and allocative efficiency indicators following floods associated with denied federal disaster declarations. The outcome in column 1 is the log of mean TFP among plants in the same 4-digit industry within the same county. Columns 2–4 measure the interdecile range (IDR), interquartile range (IQR), and standard deviation (SD) of log TFP, respectively. Column 5 measures the IQR of log output per capital stock (MRPK). Results are shown for both the Two-Way Fixed Effects (TWFE) and Sun & Abraham (2021) estimators. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimates in this table were disclosed under (CBDRB-FY26-P2851-R12806).

the majority of contacts that received the direct survey invitation via the aforementioned partners were likely time-constrained (and high-earning) CEOs, owners, or other high-level executives for whom philanthropic incentives might be more appealing than traditional survey incentives like direct monetary rewards.

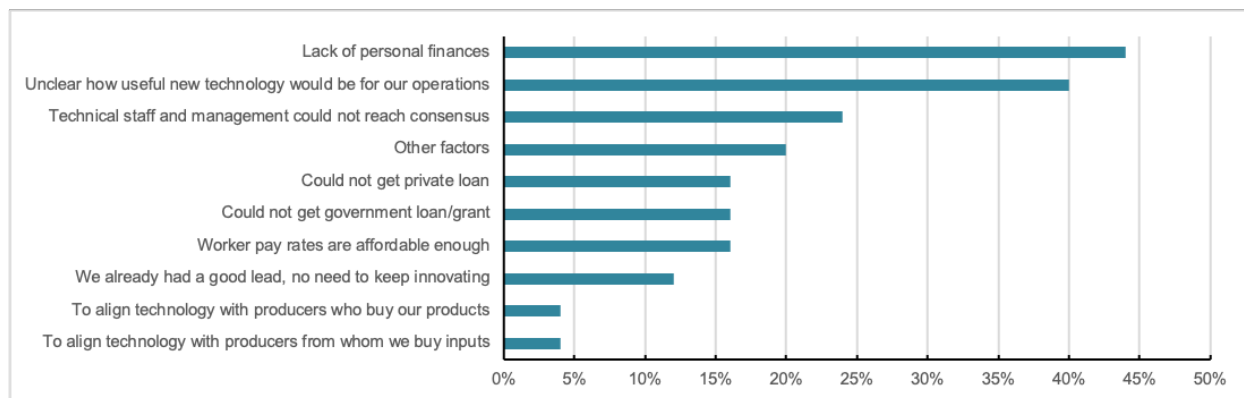
The survey starts out with questions about firm characteristics, such as industry, employment size, and assets, before delving deeper into more granular questions around capital investment and natural disasters. Another key component of this survey was a hypothetical exercise asking respondents to imagine a flood event that causes a range of different levels of loss—5%, 25%, and 65%—to the fair market value of their machinery, and asking them how they would react, namely: whether they would shut down, relocate, repair/restore their damaged machines, or entirely replace the damaged machines with technology of a higher, lower, or same grade as what they had before. These three hypothetical loss events were presented in random order to each respondent in order to avoid any potential impact of the chronological sequence of these loss scenarios on the answers. Next, came questions around respondents’ views on climate change and the risks it poses to their operations, including via weather-related natural disasters. Lastly, the survey contained an optional set of questions

prepared and IRB approved before the survey dissemination, with priority for themes like early childhood development, poverty alleviation, and environment & sustainability.

on firms' machinery maintenance protocols, climate views, and related policy preferences.

Twenty-three respondents completed the survey. The majority of participants are CEOs, owners, or other high-level executives at small and mid-size manufacturing firms located in the Midwest. Most respondents were part of small, single-unit manufacturing firms with fewer than 100 employees and an average age of more than 50 years since founding. More than 55% of all respondents are engaged in export trade.

Figure B.2: Barriers for investing in new equipment and technology

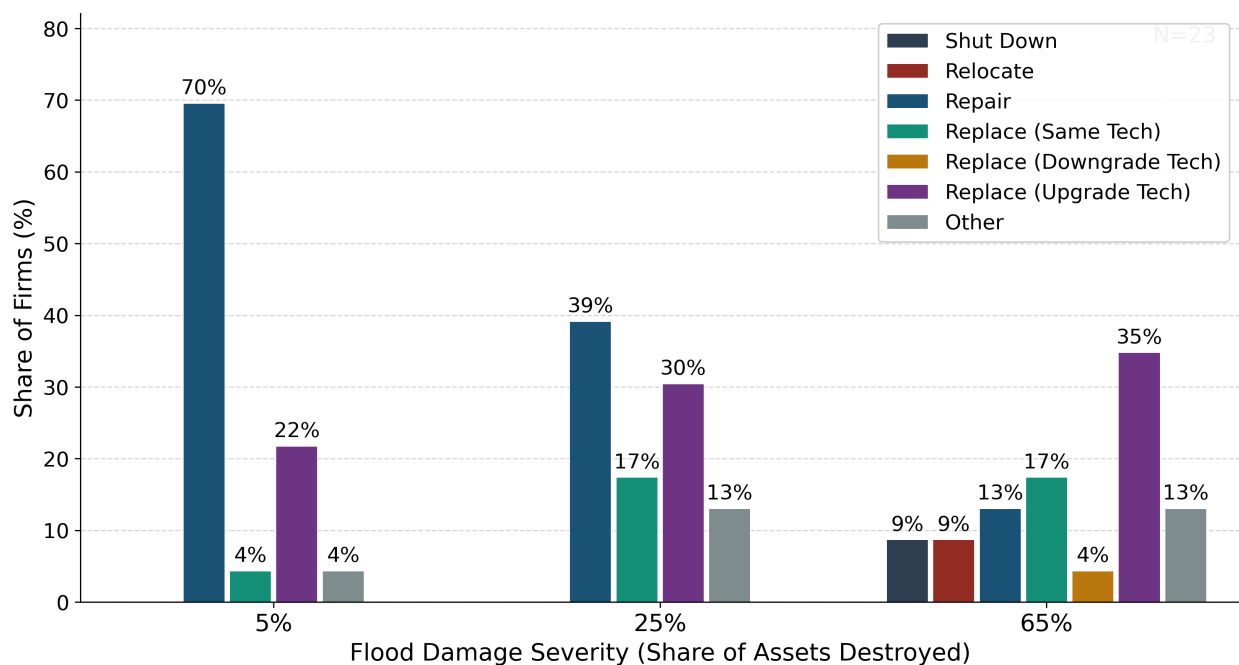


Though relatively small in number, the responses received lend two key qualitative insights that corroborate the quantitative findings discussed above. First, the top-most barrier for investment that firms cite is financing constraints, followed by uncertainty around how useful new technology would be for their operations. Given that financing is a necessary condition for investment, it makes sense that a lack of it was the predominant constraint that the majority of firms pointed to across categories like lack of personal finances and inability to secure private and/or government loans. The second most important barrier that firms pointed to concerns uncertainty about the return on new technology investments: this involves considerations around whether new equipment would integrate seamlessly with the existing machines and assembly lines, and would be worth the associated fixed costs of machinery installment such as having to tear down/reconfigure building structures and retrain workers, all the while pausing revenue-generating production.

Second, as shown in Figure B.3, firms' tendency to report that they would upgrade their machinery following a destructive damage increases substantially with the severity of a (hypothetical) disaster. In particular, in scenarios with minimal damage (5% loss), the overwhelming majority of respondents—about 70% to be precise—indicated an intention to pursue basic repairs, while the rest (22%) reported that they would upgrade the damaged machines. Under the 25% loss scenario, the response pattern showed substantially more variation. In particular, 39% of respondents favored repairs, while 30% indicated a preference

for machinery replacements that involve technology upgrades and 17% for replacements of the same technology grade.

Figure B.3: As flood severity increases, so does firms' propensity to upgrade machinery during the rebuilding phase



According to the right-most category in Figure B.3 with 65% loss scenario, firms' responses were even more varied, but the predominant majority—35% of respondents—expressed intent to upgrade and modernize their equipment rather than merely repair existing infrastructure. A smaller share of respondents reported that they would replace machinery with technology of the same grade (17%) or repair damages (13%). Others indicated that they would entirely shut down or relocate (9% each). In this 65% loss scenario, the least commonly chosen response was that firms would purchase machinery that involved technology downgrades (4%). Overall, these patterns in the responses show that as the hypothetical damage increases from 5% to 65%, it is no longer obvious that firms would simply repair damages, but their courses of action also generally show more heterogeneity. More importantly, the fact that rebuilding with upgrades emerges as the predominant post-disaster response in the most severe (65%) flood loss scenario suggests that firms would look to undertake major capital improvements after large, destructive disasters.

When asked why they would upgrade when rebuilding after a hypothetical disaster but not in their current reality (in the absence of a disaster), firms' responses highlight the unique ways in which the post-disaster environment may allow them broader room to

reoptimize their production technology and even alleviate common barriers to investment. Respondents report that insurance proceeds and government loans/assistance that would be availed after a (hypothetical) disaster would be a key mechanism through which they would pay for technological upgrades as they rebuild, but that they typically have to operate under more limited resources in the (non-disaster) status quo. The fixed costs of capital investment also matter: some respondents who indicated they would upgrade if their factory faced a major disaster stated that they would not want to shut down production (in the absence of a disaster). Another respondent explained that normally, “I would not have time to investigate new technology,” highlighting the time-intensive nature of studying technologies to resolve uncertainties around whether and to what extent a new technology may be useful for a firm’s operations. These factors are closely related to the second most important barrier firms raised for capital investment (uncertainties around the usefulness of new technology).

Taken together, these insights from the qualitative survey complement the quantitative findings in this paper. The qualitative findings broadly demonstrate that a disaster-induced disruption implies lower opportunity costs to allocating their resources toward researching new technology and rebuilding and away from revenue-generating production, while the concurrent federal disaster spending and insurance after flooding also give firms expanded access to financing that can allow them to pay for the rebuilding.

Figure B.4: Survey invitation email sent to manufacturing firms by the Federal Reserve Bank of Chicago

Opportunity to share information about investment decisions at manufacturing plants

Walstrum, Thomas <thomas.walstrum@chi.frb.org>
Cc: Tarikua Erda <te2289@columbia.edu>

Tue, Oct 22, 2024 at 12:08 PM

Dear CFSEC respondent,

I'm writing to invite you to take part in a short survey (unaffiliated with the CFSEC) about organizational practices around capital investment and technology adoption at manufacturing plants.

This survey is being conducted by Tarikua Erda, a doctoral student at Columbia University as part of her dissertation research, which studies manufacturing plants' capital investment behavior. The survey should take 10–15 minutes, and your participation is entirely voluntary.

If you complete the survey, you will be entered into a raffle to win the opportunity to choose a non-profit/charity to which Tarikua will donate \$1,000 on your behalf (anonymously if needed).

To participate, please click this link:



Please feel free to share this email and survey link to anyone else at your organization who may have relevant information to share about the questions.

I've copied Tarikua on this email in case you have any questions for her (tarikua.erdacolumbia.edu).

Thanks, as always, for sharing your knowledge to help us gain a better understanding of the economy.

Regards,

Thomas Walstrum
Principal Business Economist
Federal Reserve Bank of Chicago

B.1 MVPF Calculation and Detailed Notes

Table B.8: MVPF calculation details for federal spending on declared floods (1993–2017)

Row	Component	Value	Source / Calculation
<i>I. Estimated income effects (inputs)</i>			
(A)	Federal aid effect on per capita income	0.0153***	Declared vs. non-declared flood coefficients
(B)	Pre-disaster per-capita income (\$)	43,952	BEA (CAINC1) county income data
(C)	Average county population	123,530	BEA county population (for CAINC1)
(D)	Number of treated counties	1,492	In-sample counties in BEA income regression
(E)	Years of effect	6	Event-study window (relative years 0–5)
<i>II. Incremental income</i>			
(F)	Annual incremental income (\$B)	123.94	(A) × (B) × (C) × (D)
(G)	Total incremental income (\$B)	743.64	(F) × (E)
<i>III. Government costs and fiscal externality</i>			
(H)	FEMA flood spending (\$B)	35.00	CBO DRF outlays × flood-share allocation
(I)	SBA disaster loans disbursed (\$B)	25.00	Program totals from SBA/CRS reporting
(J)	SBA credit-subsidy rate	0.14	FCRA present-value rate (CRS-based input)
(K)	SBA budget cost (\$B)	3.50	(I) × (J)
(L)	Gross cost (\$B)	38.50	(H) + (K)
(M)	Effective tax rate	0.15	Implied from IRS/BEA mapping
(N)	Incremental tax revenue (\$B)	111.55	(G) × (M)
(O)	Net cost (\$B)	−73.05	(L) − (N)
<i>IV. Willingness to pay (WTP) and MVPF</i>			
(P)	Post-tax income gain (\$B)	632.09	(G) × (1 − (M))
(Q)	Total WTP (\$B)	632.09	= (P)
(R)	MVPF	∞	Infinite because (O) < 0 and (Q) > 0

Notes. All dollar amounts are in \$2022 and reported in billions; rows may not sum due to rounding. Annual incremental income in (F) is computed as the estimated percent effect on per-capita income (A) multiplied by baseline per-capita income (B), average county population (C), and the number of treated counties (D). Total incremental income (G) cumulates (F) over the six-year event-study window (E). FEMA flood spending (H) is constructed by combining CBO Disaster Relief Fund (DRF) *outlays* with an incident-type allocation based on publicly available FEMA disaster declaration and published-spending information; I use a conservative flood share when mapping total DRF outlays into flood-category spending. SBA budget cost (K) uses the full present-value credit-subsidy rate (J), which under the Federal Credit Reform Act represents the expected net cost to the government over the life of the loan. This is a conservative treatment: because defaults and interest subsidies accrue over the full 30-year loan maturity, the fiscal cost realized within the six-year event-study window is likely smaller. Incremental tax revenue (N) is computed as the effective tax rate (M) times total incremental income (G). Willingness to pay (P) equals the post-tax income gain; SBA disaster loans enter the calculation only on the cost side, via the credit subsidy, because loan principal must be repaid and does not constitute a transfer to beneficiaries. Because net fiscal cost (O) is negative, the MVPF (R) is effectively infinite: the policy pays for itself in budgetary terms, so any positive WTP implies an MVPF exceeding any finite bound.

Table B.9: Income recovery following declared versus non-declared floods (unweighted)

	(1) Declared floods IHS(Income per capita)	(2) Non-declared floods IHS(Income per capita)
<i>TWFE</i>		
Post flood	0.0057*** (0.0019)	−0.0043** (0.0018)
<i>Sun & Abraham</i>		
Post flood	0.0022 (0.0018)	−0.0049*** (0.0018)
N	25,476	20,855
TWFE difference: 0.0100*** (t-stat $\approx$ 3.82; $p < 0.01$)		
SA difference: 0.0071*** (t-stat $\approx$ 2.79; $p < 0.01$)		
Subsample for years 1993–2017		
	Declared floods IHS(Income per capita)	Non-declared floods IHS(Income per capita)
<i>TWFE</i>		
Post flood	0.0060* (0.0032)	−0.0036 (0.0037)
<i>Sun & Abraham</i>		
Post flood	0.0009 (0.0036)	−0.0063 (0.0043)
N	14,042	7,880
TWFE difference: 0.0096** (t-stat $\approx$ 1.96; $p < 0.05$)		
SA difference: 0.0072 (t-stat $\approx$ 1.28; $p = 0.20$)		

This table examines county-level income recovery following declared floods (column 1) and non-declared floods (column 2), using unweighted regressions. Results are shown for both the TWFE and Sun & Abraham (2021) estimators, with the 2018 flood cohort used as the control cohort in the SA method. Since authoritative information on FEMA spending is only available for 1993 and after, the bottom panel reproduces the regression results for just the year range 1993–2017 for consistency. The outcome in both columns is the inverse hyperbolic sine of per capita personal income in the county. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.