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To the Organizing Committee
Emily Breza, Davide Viviano, and Arun Chandrasekhar
Workshop on Development Economics and Networks for Early-Career Researchers
National Bureau of Economic Research

Dear Members of the Organizing Committee,

I am writing to apply to the Workshop on Development Economics and Networks for Early-Career Researchers, to be held on December 5, 2026 in Cambridge. I am an Assistant Professor of Economics at Ashoka University, having completed my Ph.D. in Economics at the University of California, Irvine in 2024, and I am therefore within five years of receiving my degree.

I would like to be considered on the basis of the enclosed working paper, "Do Politics Shape Economic Recovery through Federal Disaster Assistance?" (with Yash Khaitan and Derek Tran). The paper studies how the political economy of government resource allocation shapes local economic recovery. We estimate the causal effect of federal disaster aid on post-disaster county GDP, using county-level political competition under a winner-take-all electoral system as an instrument for the aid a county receives. We find that each additional dollar of aid raises local GDP by \$1.11–\$1.16 in the following year, and that the political allocation of aid operates only where politicians have electoral incentives to target it. The questions at its core, namely how public transfers are allocated across places and whether they translate into real recovery, are central to the applied and identification-focused work this workshop convenes, and I would gain a great deal from the feedback of its participants and senior lecturers.

As requested, a letter of recommendation will be provided by the following faculty member.

Professor Ivan Jeliazkov
Associate Professor of Economics and Statistics
Department of Economics, University of California, Irvine
ivan@uci.edu

Thank you very much for considering my application. I would be glad to provide any further materials the committee may need.

Sincerely,

Parush Arora
Assistant Professor of Economics, Ashoka University

Motivational Statement

Parush Arora, Assistant Professor of Economics, Ashoka University
Workshop on Development Economics and Networks for Early-Career Researchers

My research focuses on econometric methodology, with a common thread running through it, namely the question of how we credibly identify causal effects when the assignment of a policy or transfer is driven by the very outcomes we care about. I completed my Ph.D. at the University of California, Irvine in 2024 under Ivan Jeliazkov, and I now work on causal inference, Bayesian estimation, and empirical economics.

The paper I am submitting, "Do Politics Shape Economic Recovery through Federal Disaster Assistance?", is representative of this agenda. Federal disaster aid is allocated in proportion to damage, so naive estimates of its effect on recovery are contaminated by negative selection, since the hardest-hit places receive the most aid but would also have recovered most slowly. To break this, we build an instrument from county-level political competition, exploiting the winner-take-all structure of U.S. presidential elections. In non-swing states, local electoral competitiveness plausibly affects growth only through the aid channel, since it cannot move the state's electoral votes. Using this design we estimate that a dollar of aid returns \$1.11–\$1.16 in local GDP the following year, with the effect concentrated in the year after the disaster and strongest close to elections. Along the way we document that the political targeting of public resources operates only where incentives exist for it, a result about the distribution of government transfers across places that I think speaks directly to a development audience concerned with how, and to whom, public resources actually flow.

I am eager to attend this workshop for two reasons. First, the questions that motivate my work, namely the allocation of public transfers, the political economy of who receives them, and the design of credible identification strategies when treatment is endogenously assigned, are exactly the questions the development and empirical-methods community has pushed forward, and I want to test my identification arguments against that community's standards. Second, in general, the chance to present, to receive detailed feedback, and to engage with senior researchers and peers at an early stage of my faculty career would be invaluable in sharpening my research skills.

My broader research interests include difference-in-differences under staggered adoption, instrumental variables, and empirical economics like the current paper or the central-bank interventions in credit markets (published in the *Journal of Banking & Finance*, 2025). Across these projects I care most about methods that let us learn credible causal quantities from observational data, the shared toolkit of this workshop. I would be grateful for the opportunity to take part.

PARUSH ARORA

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EDUCATION

University of California, Irvine — June 2024
Ph.D. in Economics
Specialization in Econometrics, Bayesian Analysis and Causal Inference.

Madras School of Economics — May 2018
Masters of Arts in Economics

Hans Raj College, University of Delhi — May 2016
Bachelors of Arts in Economics

PROFESSIONAL EXPERIENCE

Ashoka University, India — Aug 2024 - Present
Assistant Professor

Tenure-track position involving research and teaching.

Amazon.com Inc, Seattle — June 2023 - Sept 2023
Economist Intern

PUBLICATIONS

Arora, P., & Tran, D. (2025). Central bank intervention and bank liquidity: Evidence from the paycheck protection program. Journal of Banking & Finance, 107522.

WORKING PAPERS

"What the first-stage IV cannot detect?"

"Trading Scope for Credibility in Difference-in-Differences" with Abhishek Chand

"Regularized Opinion Pools for Density Forecasts under Bayesian Inspired Framework"

"Extending the Versatility of Non-Parametric VAR Models: Bayesian Specification, Estimation and Dynamic Factor"

"Bayesian Factor Augmented Vector Autoregressive Model: Specification, Estimation and Inference." with Ivan Jeliazkov

"Do Politics Shape Economic Recovery through Federal Disaster Assistance?" with Yash Khaitan and Derek Tran

"Estimating Treatment Effects under Staggered Timing using GMM" with Rishabh Bijani

"Specification in Flexible TWFE in Staggered Timing and Partial Heterogeneous Effects" with Rohan Wagle

"Understanding Agricultural Land Market Distortions in India: Quantitative Modelling, Empirical Identification and Welfare Implications." with Rohan Gudibande

"Clustered Forecast Density Aggregation under Opinion Pool." with Gianni Amisano

PHD COURSES (CGPA = 3.96)

Microeconomic Theory I, II and III,

Macroeconomic Theory I, II and III

Statistics & Econometrics I, II and III

Statistical Computational Methods

Industrial Organization

Bayesian Statistics I and II

Machine Learning and Big Data

Business Cycle History

Advanced Macroeconomics

Do Politics Shape Economic Recovery through Federal Disaster Assistance?

Parush Arora[1] Yash Khaitan[2] Derek Tran[3]

Abstract

This paper estimates the causal effect of FEMA grants and SBA loans on post-disaster economic recovery in U.S. counties during 2007–2021. We find that each additional dollar of disaster aid increases local GDP by $1.11–$1.16 in the following year. Identification relies on county-level political competition as an instrument, exploiting the winner-take-all electoral system, which weakens political incentives in non-swing states. Consistent with the exclusion restriction, political competition affects GDP only in swing states, while remaining relevant for disaster aid allocation. The effect is stronger closer to elections, and the economic impact dissipates beyond one year.

Keywords: Fiscal Policy, Natural Disasters, Federal Aid, FEMA, SBA, Economic Recovery
JEL Codes: R11, R15, Q54, Q58, H84

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We thank seminar attendees at Ashoka University and the Madras School of Economics, and conference attendees at Ahmedabad University, for their valuable comments. We used Refine.ink to check the paper for consistency and clarity.

Natural disasters cause approximately 12 billion dollars in damage to the United States every year. Although the economic toll of natural disasters constitutes less than 0.1% of the US's annual Gross Domestic Product (GDP), one in twenty events has a local GDP impact of 2% or more within affected jurisdictions.[1] Damages caused by disasters have been increasing around 5.6% per year since 2000 and real disaster costs have been growing faster than GDP (Deryugina, 2017). To help impacted parties recover from these disasters, the Federal Emergency Management Agency (FEMA) provides grants while the Small Business Administration (SBA) offers low-interest disaster loans. This federal aid, on average, amounts to 23.4% of the recorded yearly damages, which is approximately $4 billion[2]. Given the size of the disaster aid program, it is important to understand its effect on GDP recovery and whether the benefits justify the cost.

Our paper investigates how FEMA grants and low-interest rate loans by the SBA affect GDP recovery at the county-level post-disaster. We believe that the timing of our paper is appropriate given the current political scenario. Since early 2025, there have been reports that approximately 2000 employees left FEMA through terminations or voluntary departures. $11 billion in disaster reimbursements to 45 states were postponed from FY2025 to FY2026[3]. In April 2025, the government ended the Building Resilient Infrastructure and Communities (BRIC) program, one of FEMA's grant programs[4]. Recently, FY2026 budget proposes a $646 million cut to FEMA[5]. The ongoing reforms to FEMA underscore the need for rigorous evidence on the effectiveness of such aid. This paper contributes to that evidence base with the aim of informing future policy design.

Our analysis needs to account for potential confounding factors that could affect GDP and federal aid. Ideally, if federal aid were randomly distributed among disaster-affected counties, a TWFE model would have identified the average treatment effect. However, the decision made by FEMA or SBA to grant federal aid is accomplished through an internal selection criterion. In addition to the amount of damages caused by the disaster, there are other factors that can affect the allocation decision. Using the two-way fixed-effects (TWFE) model, we find that the effect of federal aid quantity on GDP recovery is statistically insignificant. The FEMA selection criteria allocates aid in proportion to the damage caused by natural disasters to the county. Thus, selection bias may cancel the federal aid's recovery effect, attenuating the estimated effect toward zero since counties with more severe damage receive more aid but also face slower counterfactual recovery.

To deal with endogeneity, we estimate the causal effect using an instrumental variable (IV) approach with county and time fixed effects. We construct a novel instrument for federal aid using county-level vote margins, restricting the sample to non-swing states where this variation is plausibly

[1] 2% is chosen as the cutoff point since it is the benchmark for a recession set out by the IMF. When looking at severe recession, defined as a 5% impact to local GDP, this figure represents 1.8% of all counties.

[2] On annual average, SBA loans amounted to 2.34 billion whereas FEMA aid amounted to 1.62 billion from 2003 to 2021.

[3] Source: Disaster Relief Fund: Monthly Report (September)

[4] https://www.congress.gov/crs-product/IN12609

[5] https://www.whitehouse.gov/omb/information-resources/budget/the-presidents-fy-2026-discretionary-budget-request/

exogenous to local GDP. We argue that swing counties in non-swing states affect GDP growth only through the higher likelihood of receiving federal aid.

For the exogeneity condition, we argue that this instrument is exogenous in non-swing states. Even though economic theory predicts a positive association between political competition and economic outcomes, we argue that the incentive for politicians to perform better disappears in a non-swing state. This is due to the presidential election setup in the U.S. states, where the winning candidate receives all of the state's electoral votes. The political competition among counties should not affect the candidate's behavior in non-swing states since the dominant party's candidate is expected to take the office irrespective of how the county votes. In swing states, by contrast, additional effort in a competitive county can shift the statewide outcome at the margin, creating incentives for targeted economic delivery. We find evidence consistent with this hypothesis and find a significant positive association between GDP growth rate and political competition among counties in swing states, but no significant evidence for non-swing states.

In the first stage, we find that, within non-swing states, swing counties are more likely to receive larger aid allocations, satisfying the relevance condition. We find the effect to be stronger when the disaster timing is closer to the election cycle and strongest a quarter before the election. Interestingly, this effect disappears in the swing states. A potential explanation may be the difference in the election strategy between swing and non-swing states. It is plausible that non-swing states disproportionately distribute the federal aid to swing counties to gain their favor for elections, whereas swing states follow a uniform strategy with regard to distributing federal aid.

In the second stage, we find that a 1% increase in federal aid, on average, results in a 3.2 basis point increase in the local GDP growth rate for the next calendar year. This translates to a return of \$1.11–\$1.16 for every dollar spent on federal aid. The effect attenuates sharply thereafter, and while a modest positive coefficient re-emerges in year three, the bulk of the recovery effect is concentrated in the year immediately following the disaster.

We contribute to three strands of research. First, we are the first to estimate the causal effect of federal aid on the recovery of counties following a natural disaster. Generally, the literature in this field only looks at the firm and household-level (Billings et al., 2019) without considering the effect on the aggregate economy of the affected county. For example, Davlasheridze and Geylani (2017) show that small businesses have a higher chance of survival when a local region is provided with disaster loans. In contrast, evidence from Kousky et al. (2018) shows that individuals reduce insurance purchases if they receive a disaster relief loan. Watson (2024) find that businesses approved for the SBA loan had 2.8–3.9 times higher likelihood of surviving a hurricane than businesses that do not receive disaster relief loans. Davlasheridze and Geylani (2017) find that for every additional dollar per business spent on disaster loans, four small businesses survive. The closest paper to ours is Roth Tran and Wilson (2025), who estimate the combined causal effect of the disaster and aid on economic outcomes, whereas we identify the effect of aid alone.

Second, we contribute to the literature on how political competition affects welfare, which is sparsely explored from an empirical point of view. In our process of building an IV, we discover that the positive association between political competition and GDP growth holds only in swing states (the association is significant and four times as large as the corresponding estimate for non-swing states), consistent with the winner-take-all electoral system that lowers incentives for local performance in non-swing states. Besley et al. (2010) develop a theoretical model showing that insufficient political competition leads to anti-growth policies such as higher taxes and reduced capital spending. Their empirical analysis of U.S. state panel data confirms a strong link between low political competition and low income growth. Ma and McLaren (2018) empirically show that the U.S. tariff structure favors industries located in swing states, estimating that voters in non-swing states are valued at only 77% of their swing state counterparts. While this literature has focused on state-level outcomes or specific policy domains like tariffs, we extend it by analyzing the effect of political competition on GDP growth at the county-level.

Third, we find evidence that political competition affects federal aid distribution at the county-level. Although previous research found evidence that political factors play a role in the allocation of aid at the state level (Garrett and Sobel, 2003; Stramp, 2013), we are the first to document that this factor is relevant at the county-level as well. Interestingly, we find that the effect is significant only when the county belongs to a non-swing state. A potential new insight is that non-swing states target electorally competitive counties, whereas swing states uniformly focus on all the counties. A thorough investigation of the mechanism is outside the scope of the paper and we leave it as a topic for future research.

The remainder of the paper is organized as follows. In the next section, we describe the institutional background with respect to FEMA and discuss related literature. Section 3 discusses the data. Section 4 presents the baseline results and instrumental variable analysis. Section 5 presents some further analyses. Section 6 concludes the paper.

I. Background

A. Institutional Background

The framework for Federal Disaster Assistance was established by the Robert T. Stafford Act of 1988. When a disaster occurs and its magnitude exceeds the response capacity of the state and local governments, the disaster declaration process is initiated by a formal request from the state governor to the president. Prior to this, the governor must have activated the state emergency plan and ensured that local response efforts are underway (Beauchesne, 2001). Following the request, FEMA assesses the scale of the damage and advises the president on whether a federal disaster declaration is warranted. The president may then issue an emergency declaration, a major disaster declaration, or decline the request. Once a declaration is approved, FEMA administers assistance

through the Disaster Relief Fund (DRF) and coordinates the broader federal response. Figure 1 summarizes this sequence, from the initial disaster through the governor's request to the presidential decision and the programs each declaration type activates.

Each type of presidential declaration activates a different set of federal funding mechanisms. Emergency declarations are intended for incidents requiring immediate federal assistance and are generally subject to a statutory funding threshold of $5 million, although assistance exceeding this amount may be authorized following a presidential determination under the Stafford Act. In contrast, *major disaster declarations* activate an array of FEMA programs under the Disaster Relief Fund, such as the Public Assistance (PA), Hazard Mitigation Grant Program (HMGP), and Individual Assistance (IA) programs, each of which targets a different phase of recovery. Public Assistance (PA) provides grants to state and local governments, as well as eligible non-profit organizations, to cover the costs of debris removal, emergency protective measures, and the repair or reconstruction of damaged public infrastructure, such as roads, utilities, schools, and hospitals. Hazard Mitigation Grant Program (HMGP) funds projects aimed at reducing future disaster risk, thereby strengthening long-term community resilience. Individual Assistance (IA), in contrast, delivers direct financial support to households and individuals through FEMA's Individuals and Households Program (IHP), which provides grants for temporary housing, home repair, and other disaster-related expenses not covered by insurance.[6]

FEMA PA is distributed to state and local governments for infrastructure repair or mitigation projects whereas FEMA IA is allocated to individuals. Individuals and firms must apply for assistance, demonstrate eligibility, and undergo a federal verification process. Under IHP, FEMA verifies each applicant's identity, occupancy, and insurance coverage before assigning inspectors to conduct on-site or remote assessments of property damage. Based on these inspections, FEMA determines eligibility and grant amounts, which typically cover temporary housing, home repairs, and essential expenses not compensated by insurance. Applicants receive a formal determination letter detailing approved assistance or reasons for denial, and may appeal decisions or submit additional documentation.

In parallel, the SBA processes disaster loan applications from homeowners, renters, and businesses. The SBA assesses creditworthiness, verifies property losses through its own inspectors, and determines loan amounts according to uninsured damages. The approved loans are then disbursed directly to the applicants, often in multiple installments as reconstruction progresses. FEMA and SBA coordinate closely throughout this process to prevent duplication of benefits, ensuring that grants and loans compensate for different portions of verified losses.

[6] IA consists of multiple sub-programs, of which IHP is the largest and most significant component. The other components of IHP include Disaster Unemployment Assistance (DUA), Crisis Counselling and Training Program (CCP), Disaster Legal Services (DLS), Disaster Case Management (DCM), and Voluntary Agency Coordination (VAC). (https://www.fema.gov/sites/default/files/documents/fema_iappg-1.1.pdf)

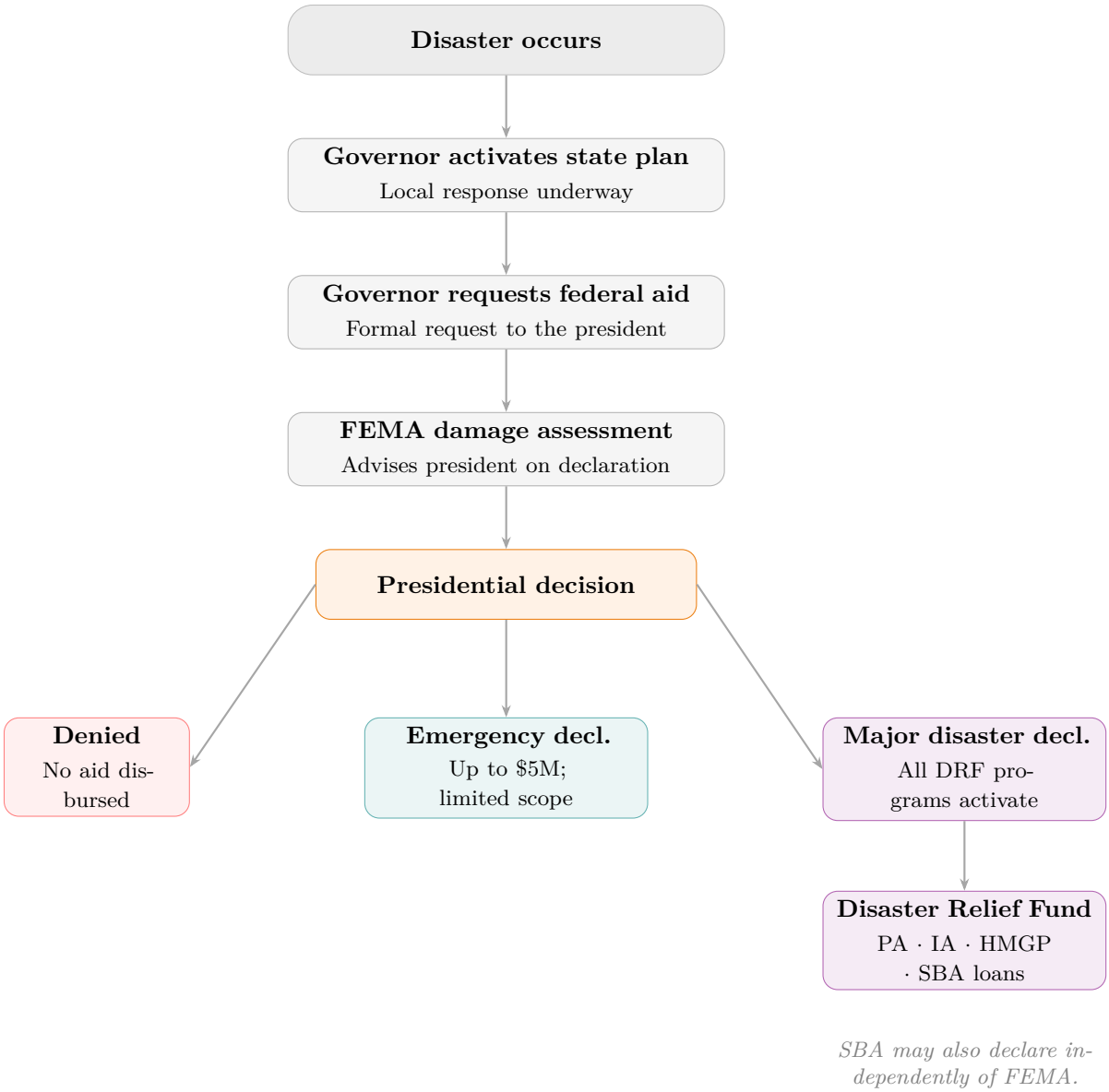


Figure 1: Federal disaster aid declaration process. The Stafford Act (1988) governs the full process. Major disaster declarations activate FEMA's Disaster Relief Fund programs (PA, IA, and HMGP) and generally make SBA disaster loans available. SBA disaster loans are administered separately by the Small Business Administration and are not financed through FEMA's Disaster Relief Fund.

Even though the internal criteria that affect the distribution of aid are unknown to the public, some of the prominent factors can be identified through the data. To contextualize, Figure 2 presents the distribution of a few county-specific factors by aid status—whether a county received non-zero federal aid or not post-disaster. Panel (a) plots the distribution of the log of damages where on average, counties with higher damage are more likely to receive federal aid (gray). This finding is consistent with the official damage-assessment framework carried out by FEMA before granting aid. Panels (b) and (c) show that counties with higher populations or GDP are more likely to receive aid.

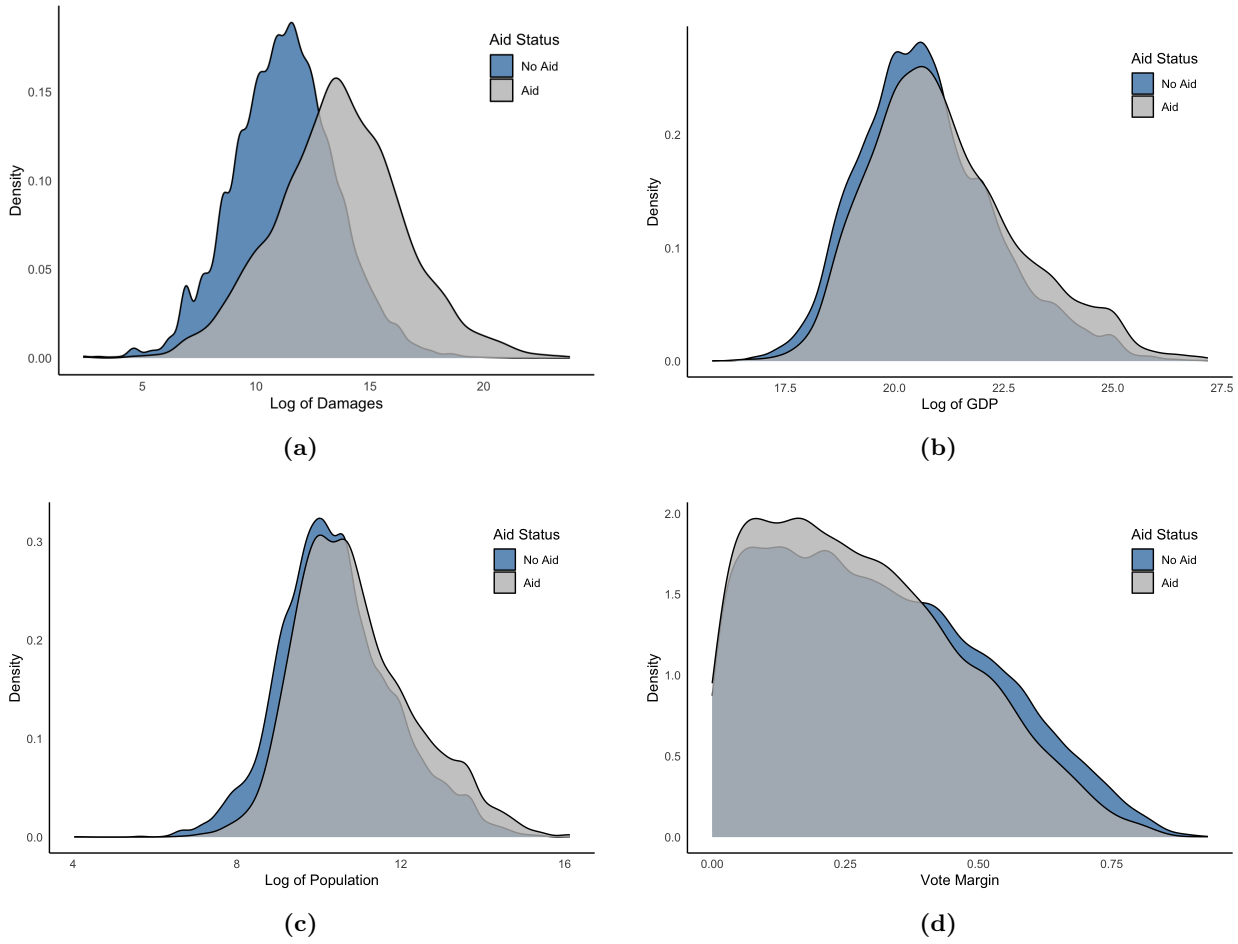


Figure 2: Distribution of county-specific factors by aid status. The gray distribution represents all the counties that were given aid, and the blue represents counties that were not given aid. Panel **(a)** plots the log-transformed total damages reported by SHELDUS. Panel **(b)** plots the distribution of log-transformed county GDP. Panel **(c)** plots the distribution of log-transformed county population. Panel **(d)** plots county-level presidential election vote margins.

This may follow from panel (a) as counties with greater economic activity or more residents are more likely to suffer losses, triggering the need for federal assistance. Apart from these factors, FEMA notes on its website that part of the selection criteria depends on citizenship status, household income and dependents, other insurance payouts, as well as occupancy and identity verification.

Panel (d) highlights distribution with respect to political competition proxied by vote margin.[7] From the data, we observe that the average county-level vote margins are lower for counties that receive aid. This correlation between political competition and aid receipt aligns with findings by Garrett and Sobel (2003) and Stramp (2013), who documented political influences on disaster aid at the state level. Their findings motivate our use of county-level vote margins as an instrument.

B. Related Literature

Most of the studies that focus on the effects of disasters on economic outcomes find a negative effect in the short to medium-term, while a smaller strand of these studies find positive effects in the long run. For the positive effects, one potential channel is "build back better", in which destroyed capital is replaced with newer, more efficient technology, which can lead to neutral, or even positive, long-term outcomes (Hornbeck and Keniston, 2017; Groen et al., 2020). The difference in these outcomes can potentially be explained by factors such as adaptation (Neumayer et al., 2014), institutional quality (Kahn, 2005; Noy, 2009), and most importantly, the infusion of post-disaster aid and other transfers (Deryugina, 2017; Roth Tran and Wilson, 2025).

Despite the massive scale of post-disaster aid, previous literature has largely focused on the general long-term effect of disasters themselves (Hsiang and Jina, 2014) rather than the specific mechanism of how aid drives local economic recovery. Most of these studies constructed comparable groups of counties that did and did not experience disasters to estimate the causal effect (Deryugina, 2017). The difference-in-differences approach works in their setting because the disaster itself is plausibly more exogenous than federal aid as federal aid is correlated with the unobserved severity of the damage and the undisclosed selection criteria.[8] Constructing comparable groups of counties that received varying levels of federal aid conditional on being hit by a disaster is more challenging. Furthermore, we cannot use the disaster itself as an instrument for aid since it could possibly affect economic recovery through other channels such as insurance payouts, migration, or changes in local investment.

A study closest to ours is Roth Tran and Wilson (2025), who analyze county-level data and estimate the impulse responses of the disaster that triggered FEMA aid on economic outcomes.

[7] $\text{VotesMargin}_c = \frac{|\text{Votes}_{D,c} - \text{Votes}_{R,c}|}{\text{Votes}_{D,c} + \text{Votes}_{R,c}}$ is the Vote Margin for county c where D refers to Democrat and R refers to Republican.

[8] Areas along the Gulf Coast, the Atlantic Coast, and the Mississippi River are particularly prone to flooding. The West Coast and the central United States along the New Madrid fault line are at higher risk of earthquakes. Finally, Florida and the Carolinas, along with other counties near the Atlantic coast are often affected by hurricanes.

They find that counties that receive federal aid post-disaster experience a long-run income boost. Their analysis considers counties that received FEMA aid post-disaster as a treatment group and counties without disasters as a control group. Their estimate can be interpreted as a combined effect of disaster and aid on economic outcomes. In comparison, we include all disaster-affected counties and assign to the treatment group only those that received federal aid. Thus, our estimate can be interpreted as the causal effect of federal aid alone on GDP recovery.

Another parallel stream of literature draws upon the political economy that underpins economic growth. Studies (Besley et al., 2010; Ma and McLaren, 2018) find that political competitiveness is positively associated with economic outcomes through the channel of strong political accountability. Competition forces politicians to appeal to swing voters, whose decisions are based on the parties' economic policy choices. This pressure incentivizes parties to shift away from special-interest policies and rent-seeking toward better governance and public goods provision. We argue that this accountability mechanism breaks down at the county-level in non-swing states. Under the winner-take-all Electoral College system, county-level political competition carries no electoral consequence when statewide outcomes are predetermined, eliminating the incentive for politicians to deliver better local economic performance. This breakdown in non-swing counties allows us to use political competition as an instrument for federal aid allocation. We argue that since political competition in non-swing states does not affect local economic outcomes directly, it can only affect economic outcomes through the channel of federal aid allocation. We test and confirm this claim empirically in Section 4.

A related strand of literature on aid distribution demonstrates that political factors play a role in the allocation of federal aid. Studies of federal disaster relief find that politically important districts receive a disproportionate share of government funds (Garrett and Sobel, 2003; Stramp, 2013). This political influence operates through two primary channels. First, executive-level manipulation occurs as presidents can influence disaster declaration decisions or increase federal cost-share reimbursements to benefit electorally important swing states, particularly near an election. Second, members of key congressional oversight committees steer disproportionately larger funding allocations to their home states. Consistent with this, Figure 3 shows stark color discontinuities at state boundaries, suggesting that political factors rather than disaster exposure alone may drive declaration decisions. Much of this literature operates at the state or national level, leaving less clarity around how these dynamics function at the local level. We extend this literature to the county-level, showing that political competition predicts aid allocation even within states, and providing the first causal estimates of how politically-driven aid affects local economic recovery.

II. Data

The primary source of disaster-related data for this paper comes from FEMA and SHELDUS. Spatial Hazard Events and Losses Database for the United States (SHELDUS) is a county-level

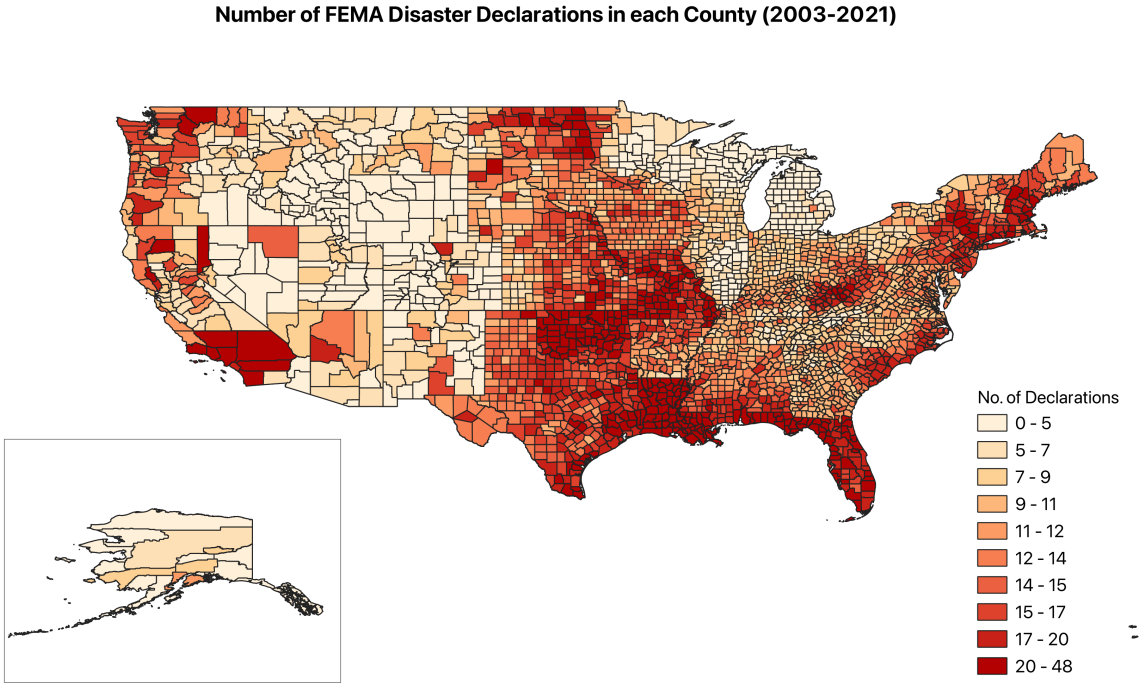


Figure 3: Spatial Distribution of FEMA Disaster Declarations across the United States

dataset compiled by the Hazards and Vulnerability Research Institute at the University of South Carolina. It aggregates data from multiple federal sources to provide standardized estimates of hazard-related losses across the U.S. since 1960. It covers natural hazards such as thunderstorms, hurricanes, floods, wildfires, and tornadoes and contains information about the date of an event, affected location (county and state) and the direct losses caused by the event (property and crop losses, injuries, and fatalities). FEMA provides information on which natural disaster was approved or denied for government aid, as well as the total approved amount of aid.[9] Figure 3 presents a map of the total number of FEMA disaster declarations for each county during the study period, providing a visual representation of regional variation in disaster exposure.

To construct a comprehensive dataset of natural disasters in the United States containing losses and aid responses, we integrate information from both the FEMA and SHELDUS databases. FEMA offers detailed disaster-level data, while SHELDUS provides county-level information aggregated at the quarterly level. Due to differences in granularity between the two sources, our merged dataset does not retain unique disaster identifiers. Instead, we structure the data at the county-quarter

[9] We use obligated amounts from FEMA's Individual Assistance (IA) and Public Assistance (PA) programs. See Section I for program details.

level, enabling consistent alignment across these sources for analysis.[10]

Data on Small Business Administration (SBA) disaster loans are obtained from the SBA website. These loans typically compensate for approximately 11% of the total damages caused by a disaster.[11] In addition to low-interest loans, individuals residing in federally declared disaster areas are eligible for mortgage and housing assistance programs that are not available in non-declared areas. This variation in federal aid, conditional on disaster declaration status, enables comparison of economic outcomes between counties affected by the same disaster but receiving different levels of support. As SBA loan data is available only at the annual level, we distribute loans across quarters in proportion to each quarter's share of annual damages, and then merge this with our county-quarter-level disaster panel[12].

Table 1: Sample Sizes by Subpopulation

Subpopulation	Counties	Disasters	Observations	Mean Aid	Mean Damage
Full Panel	2,953	1,317	153,520	0.90	4.61
Non-Swing States	1,905	1,014	99,024	1.17	6.32
Swing States	1,048	303	54,496	0.48	1.95
Positive Damage	2,938	1,066	63,083	0.90	4.61
Positive Damage, Non-Swing	1,891	796	38,351	1.17	6.32
Positive Damage, Swing	1,047	270	24,732	0.48	1.95
Received Aid	2,669	1,140	12,687	5.76	25.34
Damage + Aid	2,457	949	9,844	5.76	25.34
Main Regression Sample	1,854	739	35,213	1.23	6.70

Notes. Mean Aid and Mean Damage are in millions of dollars. Non-Swing States excludes competitive states as well as Maine and Nebraska. Positive Damage and Received Aid restrict to county-quarters with nonzero damages or aid, respectively. Damage + Aid requires both. Regression Sample is the paper's estimation sample.

We obtain county-level annual outcome data from the Bureau of Economic Analysis (BEA), including measures for households, businesses, population, unemployment, and GDP. These outcomes are merged with disaster data from FEMA and SHELDUS at the county-quarter level to construct the primary panel used in our analysis. We structure the analysis at this quarterly frequency (repeating the annual BEA data for each quarter within a given year) because our identification strategy relies on an instrumental variable defined by the distance in quarters from an election.

Additionally, we incorporate county-level presidential election vote shares from the MIT Election Data and Science Lab (MEDSL) and campaign spending data from the Federal Election Commission

[10] Sources and Frequency of all variables can be found in Table A.1.

[11] Not all damages are approved for compensation by the SBA. Individuals must submit loan applications, which may reflect losses smaller than those recorded in SHELDUS. This discrepancy can arise if insurance covers part of the loss or if the individual opts out of taking on additional debt. On average, SBA loans amount to 49% of the applicant's verified loss, with a median of 44%.

[12] Results are robust to alternative allocation rules, including equal distribution across quarters within a year.

(FEC), which we aggregate to the quarterly level to align with our disaster dataset. For each county-election year, we calculate the vote margin as the absolute difference between the presidential Republican and Democratic vote shares, which captures the degree of political competition at the county level. For non-election years, we assign each county the vote margin from the most recent presidential election. Additionally, we classify states as "swing states" based on whether they changed partisan control of electoral votes at least once during the 2000–2020 period. Table 2 reports summary statistics for the key variables.

Table 2: Summary Statistics for all Variables (Population: Full Panel)

Variable	Mean	SD	Min	Median	Max
Unemployment Insurance	22,269,990	97,883,176	1,116	3,604,826	5,943,422,725
Number of Jobs	61,108	212,183	55	12,542	6,582,546
Population	104,730	331,344	59	26,733	10,123,521
Damages	1,892,757	109,502,419	0	0	19,967,412,753
Income per Capita	40,301	11,291	15,084	38,173	268,016
Total Applications	69	2,870	0	0	615,608
Election Spending	31,072	1,078,835	0	0	247,507,366
Votes Margin	0.32	0.21	0.00	0.30	0.94
County GDP	5,181,793,144	23,087,864,237	6,775,000	811,476,000	711,059,151,000
IHS Non-Disaster Federal Spending	104,157,796	872,980,785	-708,153,233	2,907,028	31,337,787,315
Records	1	3	0	0	186

Notes. All monetary variables, Unemployment Insurance, Damages, Income per Capita, Election Spending, County GDP, and Non-Disaster Federal Spending, are expressed in real 2017 dollars, deflated using the BEA GDP implicit price deflator. County GDP is reported directly by the BEA in chained 2017 dollars. Negative values for Non-Disaster Federal Spending reflect deobligations of previously awarded federal funds as recorded in USAspending.

Our final dataset is a balanced panel at the county-quarter level spanning Q1 2007 to Q4 2021.[13] The full sample comprises 153,520 observations spanning 2,953 counties and 1,317 unique FEMA-declared disasters. Our specification includes controls for local economic conditions: total county-level Unemployment Insurance payments (government transfers to displaced workers), population, employment, and per-capita income. To account for disaster exposure and aid-seeking behavior, we control for Total Applications (FEMA aid requests) and Records (number of disaster event records in the county-quarter). We also include Election Spending to capture local political campaign activity. Table 1 details sample sizes across subpopulations. Our preferred IV specifications focus on non-swing states with positive recorded damages (38,351 observations across 1,891 counties).

As disasters are rare but high-impact events, the median county-quarter experiences zero damages and zero aid applications. However, the mean damage is $1.89 million, indicating a highly right-skewed distribution. This motivates log-transforming damage and aid variables in the subsequent analysis. Our outcome and control variables also show substantial variation. The average county-quarter in our sample has a population of 104,730 and an annualized GDP of $5.2 billion. We also observe significant variation in presidential vote shares, with the average vote

[13] The 2007 start date is determined by the availability of non-disaster federal assistance data. The extended sample used in Section B, which drops the non-disaster federal assistance control to recover earlier years, is not summarized separately.

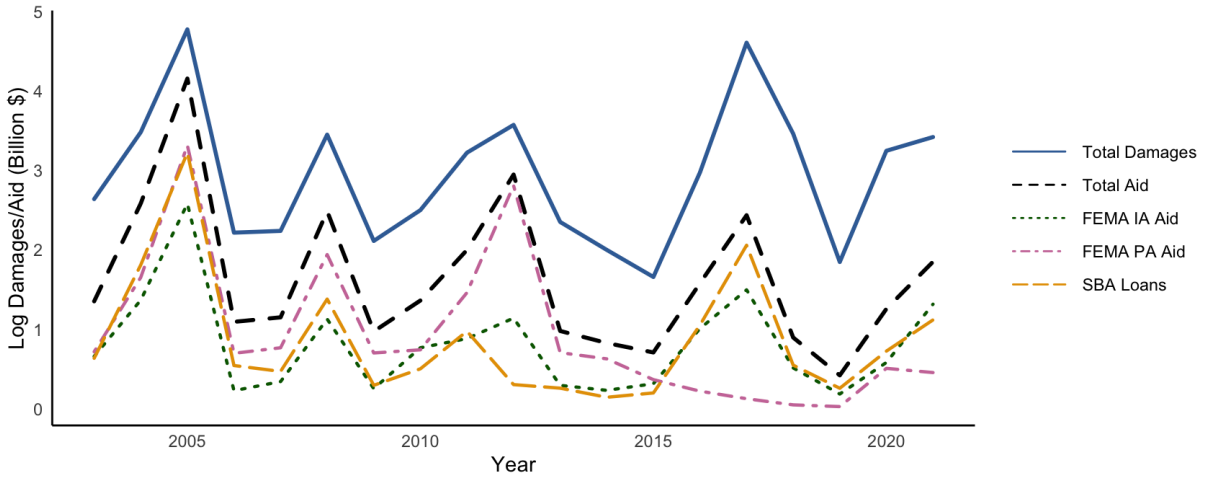


Figure 4: Graph of the total damages caused by natural disasters each year (blue) along with aggregate information such as total funding, FEMA aid, and loans given by the SBA.

margin at 0.32 and a standard deviation of 0.21. This variation in vote margins is essential for our identification strategy.

Figure 4 plots total annual disaster damages, with 2017 recording the highest losses at approximately $99 billion, driven predominantly by hurricanes (95.2% of that year's total). Of this total, 13.2% of all disaster damages were covered by FEMA and SBA funding. Insurance coverage also falls well short; Dixon et al. (2020) find that local governments receive only 28% of repair costs on average, leaving a substantial unmet funding gap.

III. Analysis and Results

A. Baseline Specification

We begin with the TWFE specification to identify the effect of federal aid on county-level GDP growth rate:

$$\Delta \log (\text{GDP})_{c,t+} = \beta \log\ (1+\text{Aid})_{c,t} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \epsilon_{c,t} \tag{1}$$

where $\log\ (1+\text{Aid})_{c,t}$[14] is the log of federal aid received by county c in quarter t, X_{ct} represents time-varying county-level covariates and γ is the coefficient associated with X_{ct}. The coefficient τ_t represents quarter fixed effects, which we include to absorb macroeconomic shocks, nationwide

[14] We use $\log(1 + \text{Aid})$ rather than $\log(\text{Aid})$ to retain zero-aid county-quarters. Recent critiques of this transformation apply when it defines the outcome variable, where the estimated percentage effect becomes unit-dependent. Aid is our outcome only in the first stage, where the coefficient has no causal interpretation and merely generates predicted values for the second stage. In the second stage, where we interpret the coefficient causally, Aid is the regressor, the standard and unaffected use of this transformation.

trends, and seasonal fluctuations that affect all counties simultaneously. The coefficient δ_c represents county fixed effects to control for the time-invariant characteristics of a county, such as geographic vulnerability to disasters, long-run economic composition, and baseline institutional capacity.

The dependent variable $\Delta \log(\text{GDP})_{c,t+}$ is defined as $\log(\text{GDP}_{c,y+2}) - \log(\text{GDP}_{c,y+1})$, where y is the calendar year in which the disaster occurs[15]. Since county-level GDP from the BEA is available only at an annual frequency, this measure captures annual GDP growth in the year following the disaster year, regardless of which quarter within year y the disaster takes place. This construction ensures that the recovery window is identical for all observations, since it always spans calendar year $y+1$ to $y+2$ even though the elapsed time between the disaster quarter and the start of year $y+1$ naturally varies. Specifically, a disaster in Q1 of year y precedes the recovery window by three quarters, whereas a disaster in Q3 precedes by only one quarter. The coefficient of interest β therefore captures a combination of the aid effect across these different gap lengths. Because the dependent variable extends two years beyond the disaster year, we restrict the covariates to observations through 2019, which drops the final two years. We adopt this cutoff to avoid contamination from the COVID-19 period, during which the disaster aid distribution pattern is unlikely to hold, since the pandemic was itself an emergency that prompted the government to disburse large volumes of aid outside the usual allocation process.

The results for Eq. 1 are presented in panel (1) of Table 3 where the aid coefficient is statistically insignificant. This is consistent with a negative selection bias, since FEMA and the SBA allocate larger aid packages to counties that suffer more severe disasters, and these same counties would have experienced slower recovery even absent federal assistance. If aid genuinely accelerates recovery, this negative correlation between aid intensity and counterfactual growth biases OLS estimates toward zero (or negative), masking the true positive effect. The TWFE specification cannot resolve this endogeneity because county and time fixed effects do not absorb the within-county, time-varying severity of individual disaster events.

Panel (2) of Table 3 reports the specification with aid disaggregated into its three components, FEMA Individual Assistance (IA) grants, FEMA Public Assistance (PA) grants, and SBA disaster loans. The estimated effect is insignificant for FEMA IA and SBA, while FEMA PA carries a negative and significant coefficient. We caution against a causal reading of this result. The TWFE model does not correct for the selection bias described above, and the bias is likely most severe for FEMA PA, which reimburses local governments for infrastructure repair and therefore scales directly with the severity of physical damage a county sustains. Under negative selection, the counties

[15] An alternative would be to measure growth from the disaster year itself, i.e. $\log(\text{GDP}_{c,y+1}) - \log(\text{GDP}_{c,y})$. We avoid this construction because $\text{GDP}_{c,y}$ conflates pre- and post-disaster output within the same calendar year. For a disaster occurring late in year y (e.g. Q4), most of year y's GDP is generated before the shock, so the baseline is largely unaffected and the resulting growth rate captures both the initial damage and any subsequent recovery. In contrast, for a disaster early in year y (e.g. Q1), the damage is already embedded in $\text{GDP}_{c,y}$, so the same growth measure primarily reflects recovery. Using $\text{GDP}_{c,y+1}$ as the base avoids this contamination, since by year $y+1$ the initial shock has been absorbed regardless of disaster timing within year y. We discuss in Section F how this choice shapes the interpretation of the estimated effect.

receiving the largest PA reimbursements are precisely those facing the deepest damage and the slowest counterfactual recovery, so the coefficient absorbs the shock rather than the aid response.

B. Instrumental Variable

To deal with the endogeneity concern regarding selection bias, we turn to an instrumental variable approach to isolate the causal effect of federal disaster aid on economic recovery. Our identification strategy uses the variation in political competitiveness (vote margin) between counties as an instrument for the amount of federal disaster aid received. Ideally, we want to assign the disaster affected counties into treatment (with aid) and control group (without aid) where the assignment criterion is related to the political competition (relevance condition) but not related in any way to counties' GDP, making the assignment as good as random (exogeneity condition).

The relevance condition is supported by economic theory. Several studies suggest that politically competitive areas are more likely to receive a disproportionate share of government funds (Garrett and Sobel, 2003; Stramp, 2013), and we believe that the same should hold true for federal disaster assistance at the county level. Swing counties may receive preferential treatment in federal aid allocation relative to non-swing counties.

However, the exogeneity condition is difficult to defend directly, as a substantial body of literature documents that political competition is associated with economic welfare. Politicians have an incentive to choose welfare-maximizing policies over rent-seeking when electoral outcomes are uncertain (Besley et al., 2010). To test for the violation of exogeneity condition, we estimate the following TWFE model:

$$\Delta \log (\text{GDP})_{c,t+} = \lambda \text{Margin}_{c,t-} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \epsilon_{c,t} \tag{2}$$

where $\text{Margin}_{c,t-}$ represents the vote margin of the last presidential election for county c at time t, $\Delta \log (\text{GDP})_{c,t+}$ is the annual GDP growth rate for the next year and λ is the coefficient capturing effect of $\text{Margin}_{c,t}$ on GDP recovery. The results of this estimation are presented in panel (1) of Table 4. We observe significant negative effects of the Vote Margin on GDP growth rate suggesting that political competition is positively associated with GDP growth rate. We also estimate this model on two subsamples. First, county-quarters in which a disaster occurred (panel (2)) and second, county-quarters with no disasters and no aid (panel (3)). The effect is negative and significant in panel (2). Panel (3) isolates the direct association between political competition and GDP growth by restricting to periods where no disaster occurred and no aid was disbursed, thus acting as the most appropriate test for the exogeneity condition. Here the coefficient is statistically insignificant but remains economically large, of a magnitude comparable to the other panels. We read this as suggestive rather than conclusive evidence on the exogeneity condition, since the point estimate points toward a direct association between political competition and GDP growth even though we

Table 3: Regression of GDP Growth on Federal Assistance

Dependent Variable:	$\Delta \log(\text{GDP})_{c,t+}$	
Model:	Aggregated Aid (1)	Disaggregated Aid (2)
Variables		
Log(1 + All Aid)	-0.0002	
	(0.0001)	
Log(1 + IA Aid)		-0.0001
		(0.0003)
Log(1 + PA Aid)		-0.0004***
		(0.0002)
Log(1 + SBA Loans)		-8.78×10^{-5}
		(0.0002)
Log Unemployment Insurance (Lag 1Y)	-0.0081***	-0.0081***
	(0.0022)	(0.0022)
Log Number of Jobs (Lag 1Y)	-0.1194***	-0.1194***
	(0.0205)	(0.0205)
Log Population	0.0860***	0.0861***
	(0.0294)	(0.0293)
Log Damages	0.0001	0.0001
	(0.0002)	(0.0002)
Log Income per Capita (Lag 1Y)	-0.1852***	-0.1850***
	(0.0207)	(0.0207)
Log Applications	0.0007*	0.0012*
	(0.0004)	(0.0007)
Log Records	0.0007	0.0007
	(0.0007)	(0.0007)
Log Election Spending	0.0002	0.0002
	(0.0001)	(0.0001)
IHS Non-Disaster Federal Spending (Lag 1Y)	4.94×10^{-5}	4.87×10^{-5}
	(0.0001)	(0.0001)
Fixed-effects		
County	Yes	Yes
Quarter	Yes	Yes
Observations	57,800	57,800
R^2	0.19528	0.19536
Within R^2	0.02025	0.02035

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

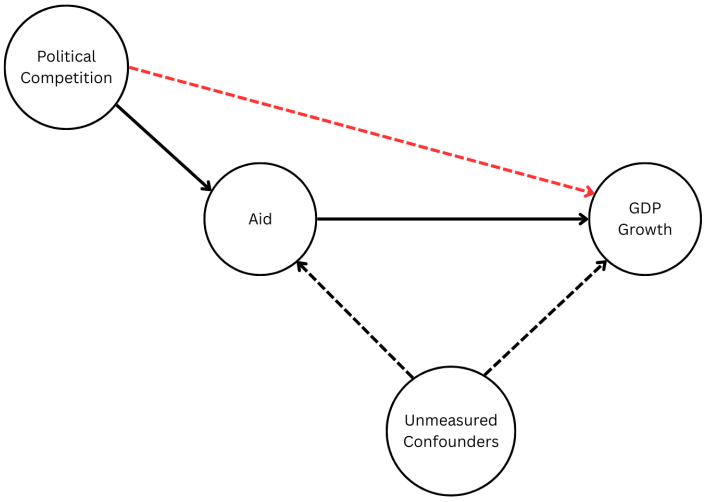


Figure 5: Exogeneity of IV

cannot reject a null effect at conventional levels.

C. Exogeneity Condition: Political Competition and GDP Growth Rate

To overcome the concern of correlation between political competition and GDP growth rate, we limit our analysis to the subset of counties located in non-swing states. A state is defined as a swing state if the winning party in the state changed at least once during our study period[16]. In non-swing states, county-level political competition does not influence presidential election outcomes, as electoral votes are allocated on a winner-take-all basis and statewide vote shares determine the outcome. Consequently, local politicians have no incentive to strategically improve local economic conditions in response to electoral competitiveness. This ensures that, within non-swing states, political competition affects economic outcomes only through the aid channel. We illustrate this mechanism in Figure 5, where the direct path from political competition to GDP growth can be eliminated if we restrict our data to non-swing states. The interpretation of our instrument is as follows: political competition among counties in non-swing states affects GDP growth only through the federal aid channel.

Figure 6 plots the distribution of county-level vote margin separately for swing and non-swing states. The two distributions differ in shape, with non-swing states placing substantially more mass at higher vote margins while swing states concentrate near the competitive end. This confirms that the instrument carries usable variation within the non-swing sample, where margins range widely rather than clustering at a single value.

[16] Figure C.1 maps the resulting classification of states as swing or non-swing. Maine and Nebraska are excluded because they allocate electoral votes by congressional district rather than by statewide winner-take-all, so the mechanism underlying our identification does not apply to them.

Table 4: Regression of GDP Growth on Vote Margin

Dependent Variable:	$\Delta \log(\text{GDP})_{c,t+}$		
Model:	All Data (1)	Disasters (2)	No Disasters No Aid (3)
Variables			
Margin	-0.0254***	-0.0443***	-0.0165
	(0.0083)	(0.0086)	(0.0111)
Log(1+Aid)	-1.31×10^{-5}	-0.0002	
	(0.0001)	(0.0001)	
Log Unemployment Insurance (Lag 1Y)	-0.0071***	-0.0075***	-0.0063**
	(0.0020)	(0.0022)	(0.0025)
Log Number of Jobs (Lag 1Y)	-0.1435***	-0.1256***	-0.1644***
	(0.0280)	(0.0208)	(0.0362)
Log Population	0.0750**	0.0745**	0.0734
	(0.0349)	(0.0293)	(0.0450)
Log Damages	-0.0001	0.0001	
	(9.41×10^{-5})	(0.0002)	
Log Income per Capita (Lag 1Y)	-0.2037***	-0.1821***	-0.2092***
	(0.0178)	(0.0206)	(0.0201)
Log Applications	0.0005	0.0007*	0.0004
	(0.0003)	(0.0004)	(0.0018)
Log Records	0.0017**	0.0008	0.0079***
	(0.0007)	(0.0007)	(0.0025)
Log Election Spending	0.0005***	0.0002	0.0007***
	(0.0001)	(0.0001)	(0.0002)
IHS Non-Disaster Federal Spending (Lag 1Y)	-4.27×10^{-6}	7.96×10^{-5}	-5.09×10^{-5}
	(0.0001)	(0.0001)	(0.0002)
Fixed-effects			
County	Yes	Yes	Yes
Quarter	Yes	Yes	Yes
Fit statistics			
Observations	141,324	57,800	80,881
R^2	0.13673	0.19624	0.13992
Within R^2	0.02705	0.02142	0.03001

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

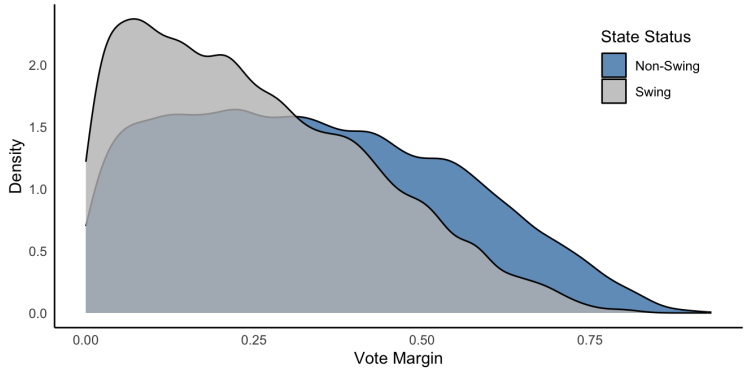


Figure 6: Distribution of county-level vote margin by swing and non-swing states.

We examine the violation of exogeneity condition in our new setting by checking whether county-level political competition directly influences GDP growth in periods when disaster aid cannot operate as a channel. We regress GDP growth on county vote margins using only the subset of county-quarter observations with no disasters and no aid, ensuring that no federal aid was disbursed in these quarters. We estimate Equation 2 separately for swing and non-swing states, allowing us to compare whether political competition significantly affects GDP growth in environments where electoral incentives differ. The results of this estimation are presented in Table 5.

We find that the coefficient on vote margin is statistically insignificant for non-swing states, but negative and significant for swing states. This pattern aligns with our hypothesis that incentives for political actors to influence local economic conditions through improved governance or targeted policy efforts are present only where county-level competition is relevant, which is only in swing states. In non-swing states, where statewide outcomes are effectively predetermined, county-level political competition does not translate into differential economic performance.

D. Exogeneity Condition: Indirect Channels

The validity of our instrument rests on the exclusion restriction: the county-level vote margin may influence post-disaster economic recovery only through the volume of disaster aid it induces. Formally, this requires $\text{Cov}(\text{Margin}_{c,t}, \epsilon_{c,t} \mid \mathbf{X}_{c,t}, \delta_c, \tau_t) = 0$ where $\epsilon_{c,t}$ is the idiosyncratic error term from equation 2. The primary threat to this is that political competitiveness is correlated with structural features of the local economy that independently shape how quickly a county rebounds after a disaster, opening indirect channels through which the instrument could influence local GDP recovery without operating through aid. Restricting the sample to non-swing states removes the broadest of these, the macro-political channel whereby federal and state policy responses to nationally pivotal states co-move with local margins. Within this sample, we condition on the vector of controls $\mathbf{X}_{c,t}$, where, to the best of our ability, a subset is chosen to account for the indirect channels we can identify.

Table 5: Estimated impact of political competition on GDP growth for the subset of data with no disasters.

Dependent Variable:	$\Delta \log(\text{GDP})_{c,t+}$	
	Swing States	Non-Swing States
Model:	(1)	(2)
Margin	-0.0385***	-0.0023
	(0.0101)	(0.0161)
Log Unemployment Insurance (Lag 1Y)	0.0026	-0.0073*
	(0.0026)	(0.0037)
Log Number of Jobs (Lag 1Y)	-0.0978***	-0.1963***
	(0.0303)	(0.0479)
Log Population	0.0674	0.0748
	(0.0418)	(0.0589)
Log Income per Capita (Lag 1Y)	-0.2069***	-0.2054***
	(0.0295)	(0.0234)
Log Applications	0.0019	0.0017
	(0.0029)	(0.0022)
Log Records	0.0010	0.0101***
	(0.0034)	(0.0033)
Log Election Spending	0.0003	0.0007***
	(0.0002)	(0.0002)
IHS Non-Disaster Federal Spending (Lag 1Y)	-0.0001	-2.52×10^{-5}
	(0.0002)	(0.0002)
Fixed-effects		
County	Yes	Yes
Quarter	Yes	Yes
Fit statistics		
Observations	26,800	54,081
R^2	0.17483	0.13707
Within R^2	0.02385	0.03185

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The most salient concern is that county-level political competitiveness affects GDP recovery through non-disaster federal assistance, independently of disaster aid. Electorally competitive counties may attract a disproportionate share of federal funds through procurement contracts, grants, and direct payments unrelated to disasters because politicians have an incentive to direct resources toward counties where marginal voters can influence electoral outcomes, even within non-swing states where the statewide outcome is predetermined. These flows stimulate local economic activity in their own right, meaning a more competitive county could exhibit faster post-disaster recovery simply because it attracts more non-disaster federal assistance, not because it received more disaster aid. We therefore control for non-disaster federal assistance, explicitly excluding FEMA and SBA flows, to reduce the risk that vote margin affects recovery through this pathway rather than through the disaster aid it induces.

A second indirect channel runs through local employment. Politically competitive counties may attract greater business investment over time because firms anticipate stronger governance and more reliable public goods provision in places where electoral accountability is higher. This raises local employment levels and increases the diversity of the county's labor base, both of which independently accelerate post-disaster recovery by enabling reallocation of workers across sectors, retaining population, and sustaining local demand. We therefore control for total employment to limit the extent to which the variation in recovery driven by the instrument reflects the underlying depth of the local labor market rather than the disaster aid channel.

The third channel runs through campaign spending. Politically competitive counties attract substantially more campaign expenditure since candidates and parties concentrate their advertising, staffing, and outreach resources in places where the electoral outcome is uncertain. This spending circulates directly through the local economy via advertising purchases, venue rentals, staff salaries, and logistics, injecting money into local businesses independently of any federal disaster response. A post-disaster recovery measurement that coincides with an election cycle could therefore partially reflect this campaign-driven economic stimulus rather than the effect of disaster aid. We therefore control for presidential campaign spending to attenuate the influence of campaign-driven economic activity, so the instrument better isolates the aid-specific consequence of political competition.

The remaining variables in $\mathbf{X}_{c,t}$ address the selection bias that originally motivated the instrument. Disaster damages are the most important of these, as they simultaneously govern the size of the aid package and the magnitude of the shock a county must recover from, which is the primary source of the bias our instrument is built to overcome. We control for population to capture the scale of the local economy, since larger counties produce more output and tend to draw larger aid packages. Unemployment insurance payments track the cyclical condition of the local labor market and the volume of transfers reaching displaced workers. Per-capita income measures local earnings from wages, salaries, and transfers, which feeds into FEMA eligibility assessments through household income while also tracking the wage base whose recovery the aid is meant to support. The count of aid applications measures local aid-seeking behavior and how disaster exposure converts into

realized aid, and it also proxies for the underlying proactiveness of a county's residents in mobilizing to secure federal resources. The number of disaster records separates county-quarters with many small events from those dominated by a single severe one.

Several of these controls, namely non-disaster federal spending, per-capita income, unemployment insurance, and total employment, are plausibly contemporaneous or post-disaster outcomes rather than predetermined characteristics, which would make them bad controls if entered at their current-period values. We therefore enter these four variables at a one-year lag, so that they capture the pre-disaster state of the local economy that shapes both aid allocation and recovery rather than the response to the disaster itself. This preserves their role in addressing selection bias while removing the risk that conditioning on them absorbs part of the recovery we seek to measure.

With these paths held fixed, $\text{Margin}_{c,t}$ in non-swing states plausibly isolates some exogenous variation in $\log(1+\text{Aid})_{c,t}$ that can be exploited to identify its effect on $\Delta \log(\text{GDP})_{c,t+}$.

E. Relevance Condition: Political Competition and Federal Aid

For the first stage of our 2SLS estimation, we regress log of Federal Aid on the county-level vote margin given as

$$\log(1+\text{Aid})_{c,t} = \alpha \text{Margin}_{c,t} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \epsilon_{c,t} \tag{3}$$

where, α is the estimated impact of political competition on the allocation of disaster aid. The results from Equation 3 are presented in Table 6. We find the coefficient to be negative and significant, which represents a strong positive association between political competition and aid distribution. Counties with a lower vote margin in the previous election get a higher aid allocation compared to counties with a higher vote margin. Since our IV is exogenous only in non-swing states, columns (2) and (3) present first stage results for Equation 3 when data is restricted to only swing and non-swing states respectively.

We observe that results persist for the sample of non-swing states where the effect of political competition on disaster aid is negative and statistically significant with an F statistic of 42. Counties with tighter vote margins receive substantially more federal assistance following a disaster. Surprisingly, the relationship is statistically insignificant within swing states with a low F statistic of 0.66. This points toward the existence of heterogeneity in how political incentives shape the aid distribution. One plausible interpretation is that non-swing states strategically channel disproportionate aid toward their swing counties in an effort to influence future electoral outcomes. Swing states, by contrast, may pursue a more uniform allocation strategy given that the entire state is electorally competitive.

To further investigate, we analyze the political bias in aid allocation relative to the timing of disaster with respect to the next presidential election, allowing political incentives to vary

Table 6: Relevance Condition

Dependent Variable:	$\text{Log}(1+\text{Aid})_{c,t}$		
	All Data	Swing	Non-Swing
Model:	(1)	(2)	(3)
Variables			
Margin	-1.130***	-0.2106	-1.616***
	(0.2638)	(0.3982)	(0.3576)
Log Unemployment Insurance (Lag 1Y)	-0.0089	0.1888**	-0.1917**
	(0.0553)	(0.0841)	(0.0840)
Log Number of Jobs (Lag 1Y)	0.1823	-0.3252	0.3457
	(0.4035)	(0.6310)	(0.5079)
Log Population	-0.0389	-0.7797	0.6497
	(0.6068)	(0.9401)	(0.7640)
Log Damages	0.1494***	0.1306***	0.1666***
	(0.0091)	(0.0135)	(0.0120)
Log Income per Capita (Lag 1Y)	0.1479	0.5436	-0.0516
	(0.3048)	(0.5617)	(0.3656)
Log Applications	2.237***	2.166***	2.273***
	(0.0173)	(0.0257)	(0.0226)
Log Records	0.0240	0.2334***	-0.1416***
	(0.0347)	(0.0531)	(0.0443)
Log Election Spending	-0.0058	-0.0159*	0.0054
	(0.0070)	(0.0094)	(0.0102)
IHS Non-Disaster Federal Spending (Lag 1Y)	-0.0119***	-0.0014	-0.0183***
	(0.0043)	(0.0066)	(0.0057)
Fixed-effects			
County	Yes	Yes	Yes
Quarter	Yes	Yes	Yes
Fit statistics			
F-test (1st stage)	38.025	0.65771	42.051

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

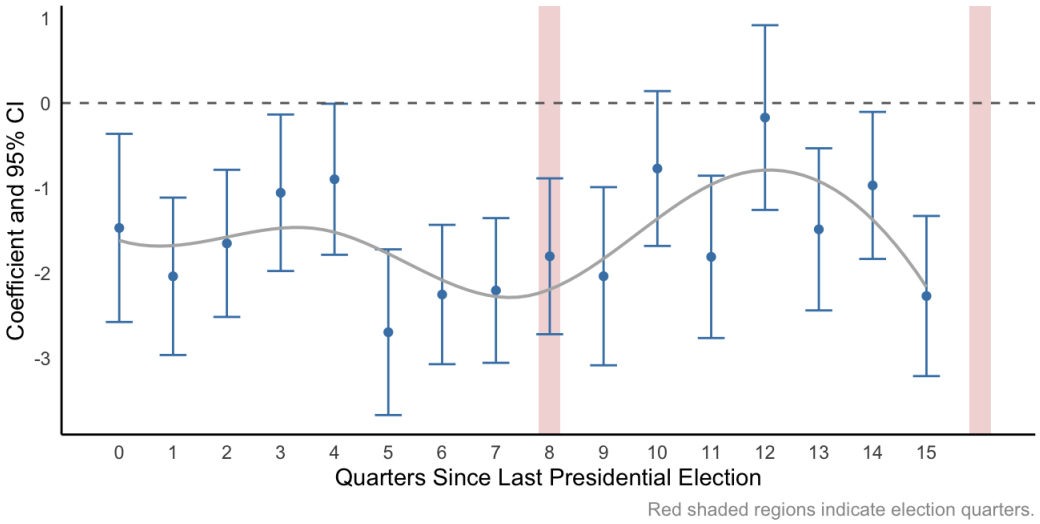


Figure 7: Coefficients for Equation 4. Data are restricted to non-swing states. The horizontal axis represents the number of quarters since the last presidential election. The red vertical line at the 8th quarter coincides with the congressional midterm elections, during which all House seats, one-third of Senate seats, and gubernatorial offices in many states are contested. The red vertical line at the 16th quarter denotes the subsequent presidential election. The vertical axis reports coefficient estimates with 95% confidence intervals.

systematically over the electoral cycle. We expect the effect to be stronger the closer the disaster timing is to the election. We modify the first-stage equation and restrict the data to the subset of non-swing states, consistent with the exogeneity conditions established earlier.

$$\log\left(1+\text{Aid}\right)_{c,t} = \sum_{q=0}^{15} \alpha_q(\text{Margin}_{c,t} \times \mathbb{I}_t(\text{PostElection} = q)) + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \epsilon_{c,t} \tag{4}$$

Here, the interaction between county-level vote margin and indicator variables for the number of quarters elapsed since the last presidential election captures how the effect of political competition on aid evolves over the electoral cycle, allowing us to detect heterogeneous effects of political competition on disaster aid allocation. We plot the coefficients from the estimation of Equation 4 in Figure 7.

We observe that there is a cyclical pattern to the effect itself. In the quarters immediately preceding the election, the tendency to allocate disaster aid to politically competitive counties increases sharply. The results are consistent with the idea that policy makers try to take advantage of voter recency bias. Aid that is distributed shortly before an election is more visible and therefore more likely to influence voter evaluations. The effect is weakest in the 4^{th} and 12^{th} quarters, which is the farthest one can get from any elections. In contrast, immediately after an election, when both federal and state-level electoral incentives are at their lowest, the effect starts to fade away in the following quarters.

F. Second Stage: Federal Aid and GDP Recovery

We now estimate the second-stage regression of GDP growth on the log of federal disaster aid, specified as,

$$\Delta \log (\text{GDP})_{c,t+} = \beta \log \widehat{(1+\text{Aid})}_{c,t} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \epsilon_{c,t} \tag{5}$$

where, $\log\widehat{(1+\text{Aid})}_{c,t}$ denotes the fitted values of the disaster aid obtained from the first-stage estimation. The results, reported in Table 7, indicate a positive and statistically significant effect of federal disaster aid on one-year-ahead GDP growth. The estimated coefficient of 0.0316 implies that a 1% increase in disaster aid leads to a 3.2 basis-point increase in county-level GDP growth following the receipt of aid.

The choice of $\text{GDP}_{c,y+1}$ as the base year warrants a note on interpretation. We use year $y+1$ as the base because by then the initial shock has been absorbed regardless of the disaster's timing within year y, which removes the mixed pre and post shock baseline that year y would introduce. This same feature means that $\text{GDP}_{c,y+1}$ is itself a post-disaster quantity that may already embed the earliest phase of recovery and some of the aid disbursed in the intervening quarters. The baseline coefficient should therefore be read as the effect of aid on growth from a post-shock base rather than as the full recovery response measured from the moment of impact. It captures the later phase of recovery that unfolds beyond year $y+1$, and it will understate the immediate response to the extent that aid acts within the disaster year itself. The short-run estimates in Section A, which measure growth from the disaster year y to year $y+1$ and isolate this earlier window, are consistent with this reading, since they recover a distinct and earlier segment of the recovery path rather than the same effect over a shorter horizon.

Two features of the design shape how this coefficient should be read. First, with county and quarter fixed effects, the instrument draws on within-county movements in presidential vote margin across election cycles inside the non-swing, positive-damage sample. The estimate is therefore a Local Average Treatment Effect rather than an average treatment effect for disaster aid as a whole. It reflects the recovery response among counties whose aid actually shifts in response to political targeting through the vote margin, which are not necessarily representative of all disaster-affected counties, and it does not speak to the effect of aid in swing states or in counties whose allocations are insensitive to political competition. Second, our aid measure combines FEMA Individual Assistance, FEMA Public Assistance, and SBA disaster loans into a single dollar figure, and these programs differ in their allocation rules, eligible recipients, and channels through which they reach the local economy. The coefficient thus captures the effect of a composite dollar of federal assistance whose internal mix is held at the sample average, and the identifying variation combines counties moving onto aid at the extensive margin with counties receiving larger dollar volumes at the intensive margin. We read the coefficient as the recovery return on an additional aid dollar for this politically

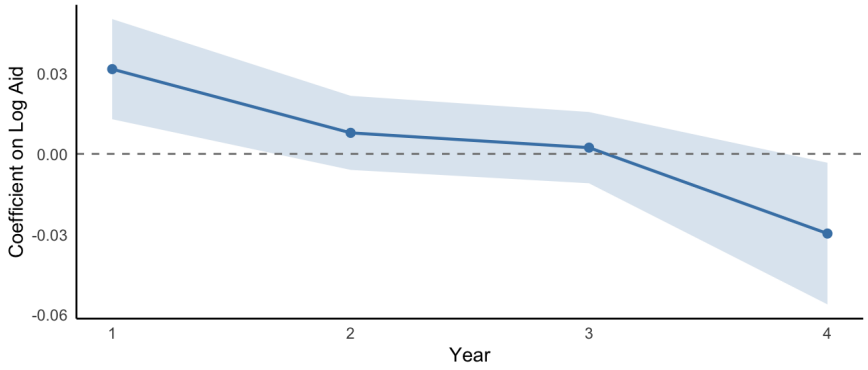


Figure 8: Long-term impact of disaster aid on GDP growth. Each point plots the coefficient on instrumented log aid from a separate second-stage regression in which the dependent variable is GDP growth measured one, two, three, and four years ahead of the disaster year. The shaded band gives the 95% confidence interval, with standard errors clustered at the county level.

responsive subpopulation, and we retain the combined measure because the first stage lacks the strength to separately instrument three distinct programs, a disaggregation we leave to future work with program-specific instruments.

We also examine potential long-term effects of federal disaster aid on economic performance by replacing the dependent variable with two-year, three-year, and four-year-ahead GDP growth rates. The results are presented in Figure 8 where we plot the coefficients of each of these regressions along with their 95% confidence intervals. The estimate is large and clearly positive in the first year, then attenuates sharply second year onward. Taken together, the pattern points to a recovery effect that is concentrated in the year immediately following the disaster and that dissipates over the longer horizon rather than compounding into sustained higher growth.

G. Fiscal Multiplier of Disaster Aid

Using our estimates of causal effect, we calculate the fiscal multiplier of disaster aid to assess the return per dollar of federal spending at the county level. Based on disaster affected counties in non-swing states, our coefficient of 0.032 translates into a fiscal multiplier of approximately 1.1 when restricting to counties with positive recorded damages (see Table 8). Since our estimate is based on the counties with non-zero aid, we re-estimate the multiplier for the subsample of counties with both positive damage and positive aid in non-swing states and obtain a fiscal multiplier of approximately 1.16 (see Table 8).

To contextualize our estimates, we compare the recovery effect of federal aid with disaster-induced economic losses documented in the literature. At the U.S. county level, Strobl (2011) estimates that hurricanes reduce annual economic growth by 0.45 percentage points in affected coastal counties, though these effects net out at the state and national levels. Belasen and Polachek (2008) document employment declines of 1.5–5% in directly affected Florida counties depending on hurricane intensity.

Table 7: Second Stage IV Regression

Dependent Variable:	$\Delta \log(\text{GDP})_{c,t+}$
Model:	(1)
Variables	
$\widehat{\text{Log (1+Aid)}}$	0.0315^{***}
	(0.0095)
Log Unemployment Insurance (Lag 1Y)	-1.49×10^{-4}
	(0.0048)
Log Number of Jobs (Lag 1Y)	-0.1500^{***}
	(0.0295)
Log Population	0.0461
	(0.0427)
Log Damages	-0.0052^{***}
	(0.0016)
Log Income per Capita (Lag 1Y)	-0.1832^{***}
	(0.0291)
Log Total Applications	-0.0719^{***}
	(0.0216)
Log Records	0.0070^{***}
	(0.0022)
Log Election Spending	-2.52×10^{-5}
	(0.0004)
IHS Non-Disaster Federal Spending (Lag 1Y)	$5.79 \times 10^{-4**}$
	(0.0003)
Fixed-effects	
County FE	Yes
Quarter FE	Yes
Fit statistics	
Observations	35,213
R^2	0.19737
Within R^2	0.02245

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8: Fiscal Multipliers of Disaster Aid

Sample	Fiscal Multiplier
Positive Damage	1.1072
Positive Damage + Aid	1.1649

Notes: The fiscal multiplier is $\tilde{Y}(\beta/100)/\tilde{A}$, where $\tilde{Y}$ is the median county GDP, $\tilde{A}$ is the median disaster aid conditional on positive aid, and β is the IV elasticity of GDP growth with respect to aid. In the *Positive Damage* row $\tilde{Y}$ is the median GDP over all counties with positive damage; in *Positive Damage + Aid* it is the median over counties that additionally received aid. Sample restricted to non-swing states.

Boustan et al. (2020) find that severe disasters increase out-migration by 1.5 percentage points and lower housing prices by 2.5–5%, consistent with falling local productivity and labor demand. Over longer horizons, Deryugina (2017) estimates that stronger hurricanes cause per capita earnings losses exceeding $4,300 in net present value over the following decade, with employment effects persisting up to ten years. Jerch et al. (2023) document that hurricanes similarly reduce local government tax revenues, expenditures, and debt financing capacity over a comparable horizon. At the cross-country level, Noy (2009) finds that disasters reduce output growth by approximately 9 percentage points in developing countries, with substantially smaller effects in developed economies where stronger institutions and higher incomes buffer the initial shock.

These estimates generally capture the *net* effect of disasters—combining the initial shock with subsequent recovery channels, including federal aid. Our fiscal multiplier implies that every dollar of federal disaster aid generates approximately $1.10–$1.16 in local GDP within the following year. Since federal aid in our sample covers approximately 23.4% of recorded damages, aid-driven GDP recovery compensates for a meaningful share of short-run losses. The multiplier exceeding unity implies that the GDP return on federal disaster spending is positive. However, since the effect is concentrated in the first year and fades over the longer horizon (Figure 8), federal aid alone cannot address the persistent medium- and long-run consequences documented by Deryugina (2017) and Hsiang and Jina (2014). The longer-run income recovery found by Roth Tran and Wilson (2025), who document that disasters triggering federal aid raise personal income per capita over an eight-year horizon, may instead reflect insurance payouts, private investment, and the replacement of destroyed capital with more productive technology (Hornbeck and Keniston, 2017).

IV. Further Analysis

A. Different Response Times

The baseline outcome measures GDP growth from year $y+1$ to $y+2$, which may reflect delayed recovery rather than the immediate response to aid. A related concern is that the elapsed time between the disaster and the start of the measurement window varies with the disaster quarter,

so aid disbursed after a Q1 disaster has roughly three more quarters to materialize before the window opens than aid disbursed after a Q4 disaster. The baseline coefficient is therefore a weighted average of the aid effect across these uneven run-up lengths rather than the effect over a single fixed interval. To isolate a shorter-run effect measured over a consistent window, we re-estimate the full specification on disasters occurring early in the calendar year and redefine the outcome as growth from the disaster year y to year $y+1$.

Table 9: Short-Run Second-Stage IV Estimates

	First Stage		Second Stage	
Dependent Variable:	Log(1+Aid)		Δ log(GDP)	
Sample:	Q1	Q1 & Q2	Q1	Q1 & Q2
	(1)	(2)	(3)	(4)
Margin	-2.3667^{***}	-2.6916^{***}		
	(0.5334)	(0.4166)		
$\widehat{\text{Log(1+Aid)}}$			0.0154^{**}	0.0121^{**}
			(0.0077)	(0.0053)
Controls	Yes	Yes	Yes	Yes
County & Quarter FE	Yes	Yes	Yes	Yes
Observations	7,620	19,553	7,620	19,553
First-stage F	21.49	63.18		

Notes: Second-stage IV estimates for early-year disaster quarters, where aid is instrumented by the county presidential vote margin. Columns (1) and (2) report the first stage (dependent variable: log aid; coefficient on Margin); columns (3) and (4) report the second stage (dependent variable: one-year-ahead GDP growth, $\log(\text{GDP}_{t+1}) - \log(\text{GDP}_t)$; coefficient on instrumented log aid). All specifications include county and quarter fixed effects and the full vector of one-year lagged controls, which are not reported. Sample: non-swing states, disaster quarters, 2007–2021. Clustered (county) standard errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1.

We restrict the sample to Q1 disasters in the first specification and to Q1 and Q2 disasters in the second. For these early-year disasters the disaster-year GDP already embeds the shock, so growth from y to $y+1$ measures the immediate recovery rather than a lagged response, and holding the window fixed removes the uneven run-up that the baseline averages over. Table 9 reports the results. The instrumented aid coefficient is 0.0154 for the Q1 sample and 0.0121 for the Q1 and Q2 sample, both positive and significant, with first-stage F statistics of 21.49 and 63.18.

These short-run estimates are smaller than the baseline coefficient of 0.032. The difference is consistent with the interpretation of the two outcomes. The baseline measures growth over the $y+1$ to $y+2$ window, by which point the immediate disruption has been absorbed and the recovery is well underway, whereas the fixed-interval outcome here spans the disaster year itself and captures the earlier phase of recovery when output is still depressed and aid is only beginning to circulate through the local economy. A smaller immediate effect that builds over the subsequent year is what we would expect if aid supports a recovery that accumulates rather than completing within the first

twelve months, and it aligns with the fading longer-horizon pattern documented in Figure 8.[17]

B. Larger Sample

Our main specification includes non-disaster federal spending as a control, which closes the indirect channel through which vote margin could raise recovery by attracting federal funds unrelated to disasters. This variable is available only from 2007 onward, so including it forces the estimation sample to begin in 2007 and discards the earlier years of data. To check that our results are not an artifact of this shorter window or of the single control that constrains it, we re-estimate the full specification over the longer 2003 to 2021 sample with non-disaster federal spending excluded. Dropping the control recovers four additional years of observations at the cost of leaving one indirect channel unaddressed, and comparing the two samples tells us whether the headline findings depend on the shorter period or on that control.

Table 10: Instrumental Variable Estimates (Robustness Sample 2003–2021)

	First Stage		Second Stage
Dependent Variable:	Log(1+Aid)		Δ log(GDP)
Sample:	Swing	Non-Swing	Non-Swing
	(1)	(2)	(3)
Margin	−0.1594	−0.7866***	
	(0.3296)	(0.2532)	
$\widehat{\text{Log(1+Aid)}}$			0.0531**
			(0.0209)
Controls	Yes	Yes	Yes
County & Quarter FE	Yes	Yes	Yes
Observations	30,210	46,662	46,662
F-test (1st stage)	0.49	16.84	

Notes. Clustered (county) standard errors in parentheses. All specifications include county and quarter fixed effects and the full vector of controls (disaster damages, lagged unemployment insurance, lagged number of jobs, population, lagged income per capita, applications, records, and election spending), which are not reported. Columns (1) and (2) report the first-stage regressions. Column (3) reports the second-stage 2SLS estimate with aid instrumented by county vote margin. Sample spans 2003–2021 and excludes the non-disaster financial assistance control. Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 10 presents the exogeneity test, first stage, and second stage for this larger sample. The pattern established in the main analysis holds throughout. In the exogeneity test, the coefficient on vote margin is negative and significant in swing states but insignificant in non-swing states, consistent with the claim that county-level political competition affects GDP growth only where

[17] We also estimated the analogous fixed-interval specification for late-year disasters, measuring growth from $y + 1$ to $y + 2$ for disasters occurring in Q4 and in Q3 and Q4. The first stage is weak in these subsamples, with F statistics of 5.94 and 0.02 respectively, so the instrument does not isolate usable variation in aid and we do not interpret the resulting second-stage estimates. Appendix B discusses why instrument strength varies across quarters.

it is electorally relevant. The first stage remains strong in non-swing states, where vote margin has a negative and significant effect on aid with an F statistic of 16.84, while the swing-state first stage stays weak with an F statistic of 0.49. The second stage returns a positive and significant aid coefficient of 0.0531, larger than the 0.0315 estimate from the main sample. This larger magnitude is expected. With non-disaster federal spending no longer controlled for, the aid coefficient now absorbs part of the recovery that operates through the funds a competitive county attracts outside the disaster response, so the estimate captures the effect of disaster aid together with the correlated non-disaster spending that the main specification holds fixed.

C. States with Aligned Presidential and Gubernatorial Outcomes

Our exclusion restriction assumes that county vote margin affects GDP growth in non-swing states only through the disaster aid channel. The winner-take-all structure of presidential elections supports this assumption at the presidential level, since a county's competitiveness cannot alter a non-swing state's presidential outcome. County competitiveness could in principle still matter for other offices, most notably the governorship, which is decided by a statewide popular vote. If a state votes reliably for one party in presidential elections yet elects governors from the opposite party, county competitiveness could plausibly operate through the gubernatorial race and open a pathway to local economic outcomes that does not run through disaster aid.

This concern requires a partisan divergence between the two offices to operate. It can only arise in a state where the governorship is contested by a party other than the one that consistently wins the presidential vote, since only then does county competitiveness carry a gubernatorial incentive distinct from the presidential one already captured by our instrument. We therefore isolate the states where this divergence never occurs. Restricting to the non-swing states in which the same party won every gubernatorial election held between 2002 and 2018 (Algara and Amlani, 2021) leaves eleven states, namely Alabama, Delaware, Idaho, Mississippi, North Dakota, Oregon, South Carolina, South Dakota, Texas, Utah, and Washington. In each of these states the presidential party stayed constant throughout our sample period by the definition of a non-swing state, and the gubernatorial party independently stayed constant as well. No state remains in which a governor of a different party could respond to county competitiveness, so the gubernatorial channel is theoretically closed within this subsample.

Table 11 reports the first and second stage estimates on this restricted sample. The first stage remains strong, with an F statistic of 44.08, and the second stage aid coefficient is 0.0322, significant at the 1 percent level. This estimate is almost identical to the 0.0315 coefficient from our main specification. Closing the gubernatorial channel therefore leaves both the strength of the instrument and the magnitude of the recovery effect essentially unchanged, which indicates that the main result is not driven by the states where this channel could have been active and that the restriction does not come at the cost of a weaker or materially different estimate.

Table 11: Instrumental Variable Estimates: Aligned Non-Swing States

	First Stage	Second Stage
Dependent Variable:	Log(1+Aid)	Δ log(GDP)
	(1)	(2)
Margin	-3.5201^{***}	
	(0.7624)	
$\widehat{\text{Log(1+Aid)}}$		0.0322^{***}
		(0.0100)
Controls	Yes	Yes
County & Quarter FE	Yes	Yes
Observations	12,102	12,102
First-stage F	44.08	

Notes. IV estimates restricted to the 11 non-swing states whose governor's party matched the state's presidential party in every gubernatorial election, 2002–2018 (AL, DE, ID, MS, ND, OR, SC, SD, TX, UT, WA). Column (1) reports the first-stage regression of log aid on vote margin among disaster county-quarters. Column (2) reports the second-stage estimate, with aid instrumented by vote margin. All specifications include county and quarter fixed effects and the full vector of one-year lagged controls, which are not reported. Sample window: 2007–2021. Clustered (county) standard errors are reported in parentheses. Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

V. Conclusion

This paper provides new causal evidence on the role of federal disaster assistance in local economic recovery. Using a comprehensive county-level panel of U.S. disasters from 2007 to 2021, we study how FEMA grants and SBA disaster loans affect post-disaster GDP growth. We instrument for aid using county-level political competition, exploiting the fact that this relationship satisfies the exclusion restriction only in non-swing states under the winner-take-all Electoral College. Conditioning on counties affected by disasters, we estimate the causal effect of federal aid on county-level GDP recovery. Our IV estimates imply that a 1% increase in disaster aid leads to a 3.2 basis-point increase in GDP growth in the year following the disaster, with effects dissipating in subsequent years. The multiplier exceeding unity implies that the GDP return on federal disaster spending is positive. However, since the effect is concentrated in the first year and fades over the longer horizon (Figure 8), federal aid alone cannot address the persistent medium- and long-run consequences documented by Deryugina (2017) and Hsiang and Jina (2014). The longer-run income recovery found by Roth Tran and Wilson (2025)—who document that disasters triggering federal aid raise personal income per capita over an eight-year horizon—may instead reflect insurance payouts, private investment, and the replacement of destroyed capital with more productive technology (Hornbeck and Keniston, 2017).

From a research perspective, our heterogeneous findings of political competition on federal aid distribution between swing and non-swing states can be a topic of future research. Researchers can also use the identification strategy to estimate effect of federal aid on other important macroeconomic

variables. From a policy perspective, our results suggest that federal disaster assistance can play a meaningful role in accelerating short-run economic recovery but that its effects may not translate into long-term growth. This highlights the importance of designing aid programs that not only provide immediate relief but also support sustained economic rebuilding. Against the backdrop of ongoing structural reforms to federal disaster assistance, this paper provides evidence on the causal effect of FEMA and SBA assistance on county-level recovery outcomes, with implications for the design and targeting of disaster relief policy.

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Appendix

A. Data

Table A.1: Data Sources

Variable	Frequency	Source
Presidential Election Vote Shares	Quadrennial	MEDSL
Gubernatorial Election Vote Shares	Quadrennial	Algara & Amlani (2021)
GDP	Annual	BEA
Population	Annual	BEA
Number of Jobs	Annual	BEA
Unemployment Insurance	Annual	BEA
Income Per Capita	Annual	BEA
SBA Loans	Annual	SBA
Damages	Quarterly	SHELDUS
Records	Quarterly	SHELDUS
Election Spending	Quarterly	FEC
Non-Disaster Federal Spending	Quarterly	USASpending
FEMA Aid	Monthly	OpenFEMA
Total Applications	Monthly	OpenFEMA

B. Seasonality of Aid

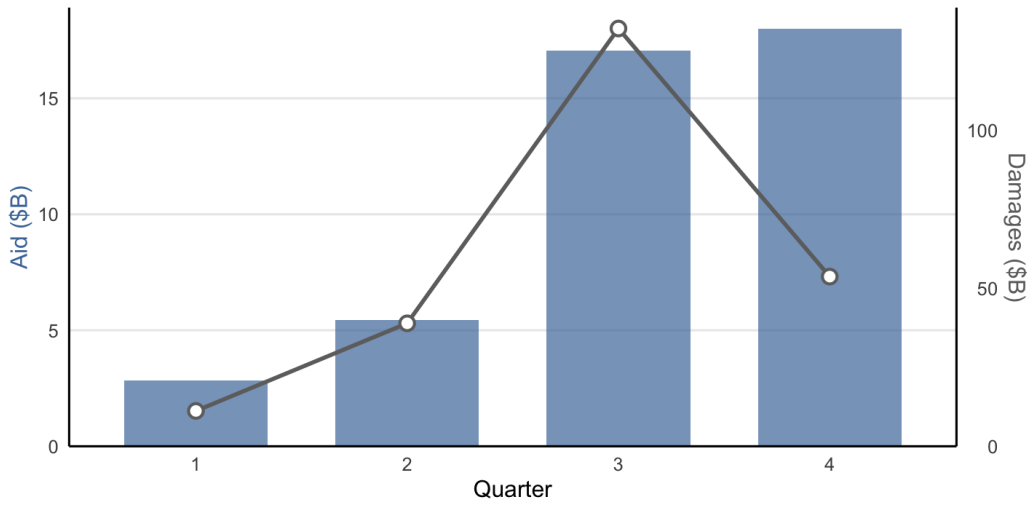


Figure B.1: Aggregate Disaster Aid (bars) and Damages (line) by Quarter

The strength of our instrument varies systematically across the quarters in which disasters occur, and the seasonal structure of disaster aid offers a plausible account of why. Hurricane season runs from roughly June through November, so both aid and damages tend to concentrate in the third and fourth quarters, as Figure B.1 shows, with aggregate aid and total damages both rising in the second half of the year. When

damages are this severe, the aid response could be shaped largely by the scale of the disaster and the need for emergency relief, which may leave less room for the political discretion our instrument relies on. In the earlier quarters, where disasters tend to be smaller and less seasonal, aid allocation might be comparatively more sensitive to political considerations. This pattern could help explain why vote margin predicts aid well in the Q1 and Q1 to Q2 samples, with first-stage F statistics of 21.49 and 63.18, but poorly in the late-year subsamples, where the corresponding statistics fall to 5.94 and 0.02.

C. Swing vs Non-Swing States

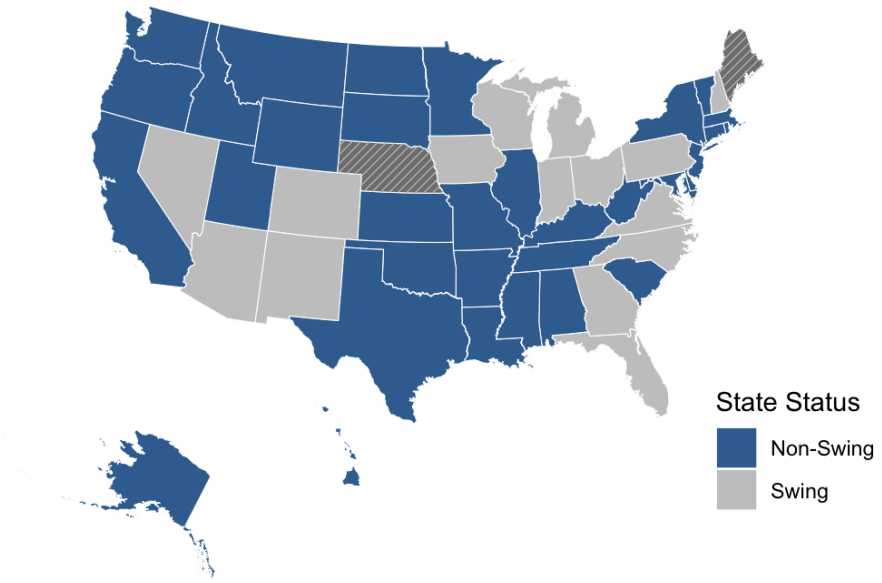


Figure C.1: Classification of states as swing or non-swing. Non-swing states, shown in blue, are those in which the winning presidential party did not change over the study period. Swing states, shown in grey, are those in which the winning party changed at least once. Maine and Nebraska, shown with hatching, are excluded from the classification because they are the only two states that allocate electoral votes by congressional district rather than on a statewide winner-take-all basis, so the winner-take-all logic underlying our instrument does not apply to them.