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The Effect of Natural Disasters on Economic Activity: A Half-Century Analysis in US Counties
Critical Assessment of Methodology and Findings in Boustan et al. (2020)

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Abstract – This study critically assesses the methodology and findings of Boustan *et al.* (2020), which examine the long-term economic effects of natural disasters on U.S. counties. Using a historical dataset spanning nearly a century, the authors find that severe disasters significantly impact migration and housing markets. Our replication effort, while constrained by data availability (that switch our focus on a half-century period), corroborates some of their conclusions but also reveals important methodological limitations. While we do find that some severe disasters are correlated with an increase in emigration and a decrease in incomes, we highlight concerns regarding the potential endogeneity of some variables (*e.g.* disaster declarations), the lack of a causal identification strategy, and the oversimplification of mechanisms explaining the results. Our study therefore provides insights into the long-term economic effects of disasters and highlights the importance of data accessibility in empirical research. Future research should refine disaster impact estimation by employing more rigorous econometric techniques in order to elicit reliable effects, and should be prone to rely on stronger theoretical foundations.

Key words – Natural disasters, economic impact, migration, housing prices, data replication, methodological challenges.

1 Introduction

Extreme events are defined as disasters issued from weather extremes, which lie in the tails of the distribution of weather events. It is nowadays certain that human-induced greenhouse gas emissions have led to an increased frequency and/or intensity of some weather extremes since pre-industrial time, which results in extreme events such as droughts, tropical cyclones or fire weather [1].

Climate change and the disasters that come with it induce increasing costs as the frequency rise [2]. Extreme events reduce crop production [3], increase the risk of diseases or death [4], disrupt infrastructures [5] and lead to population movements [6], notably with an income-heterogeneity in the response to disasters [7, 8].

To support and develop a wise adaptation to climate change, understanding the migratory consequences of natural disasters is necessary. Using standard econometrics methods on an extensive database, Boustan *et al.* (2020) [9] investigate the impact of natural disasters on internal migration in the United States, seeking to exhibit economic mechanisms that could underlie the migratory responses to disasters. The following section presents the main results of the paper and its methodology. After a critical summary, we aim to reproduce the study and discuss the limits of our replication.

2 Critical Assessment of Methodology and Findings in Boustan et al. (2020)

2.1 Presentation of the Original Study

The climate macroeconometrics literature that use cross-country panel regression to elicit the impacts of extreme events on the economy is not very convincing: they find results ranging from near-recovery to long-lasting effects on GDP ([10, 11, 12, 13]).

Here, the authors narrow their focus by aiming to determine the effects of natural disasters on the local economy. They choose to analyze a single country, the United States, which allows to control for institutional and geographic features. They gather a huge amount of data: their time series of disasters starts in 1920 and unifies data from the American National Red Cross (1920 - 1964), the National Archives II at College Park, MD (1950 - 1964) and FEMA (1953 - 2019, with county data starting in 1964), leading to a count of more than 10 000 disasters over the century at the county level, the smallest geographical unit for which data is available.

The disasters are classified into six categories: floods, winter storms, hurricanes, tornadoes, forest fires, and other disasters, each of them having spatial particularities (*e.g.* tornadoes in the Midwest, hurricanes in Florida and the Gulf of Mexico[1], floods along the Mississippi River, etc.). Descriptive statistics show that the number of declared disasters increased strongly since 1990 (it has tripled between 1990 and 2010), mostly because of winter storms and hurricanes. Still, floods and tornadoes account for 70% of the disasters in the dataset. Importantly, they define severe disasters as those with 25+ deaths, which represent the median count for disasters with known fatality counts (the fatalities being counted as soon as there are more than 10 deaths, drawn from EM-DAT). Coastal and more populated counties faces more severe disasters, as we can see figure 1. On the contrary, counties with numerous lakes or on high altitude are less exposed.

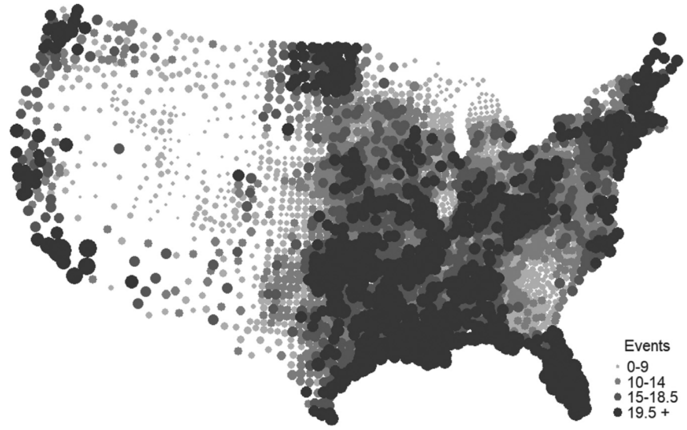

Figure 1: Number of incidents (1930-2010) by counties in the USA, from Boustan et al. (2020).

To explore the impact of disasters on the local economies, the authors use a variety of socioeconomic data, the most important of which being the net migration, the housing prices and the wages. Although they claim that aggregating over a decade allows them to explore long-lasting effect after any rebuilding, new investments, or disbursement of disaster relief funds, we shall point out the fact that the data on housing prices taken from the Censuses of Population and Housing are decadal, and therefore would not have allowed them to capture the short-term effects of extreme events on the housing market.

They are able to retrieve the first effects of disasters on migration at the county level since 1930; though a precise database provides net migration adjusted for birth, mortality and immigration from abroad since 1950 (WISC). Socioeconomic data are taken from the National Historical Geographic Information System (NHGIS), which provides population, poverty rates, family income (as a proxy for wages), housing stocks, and house values by county (expressed in the paper in 1982-1984 dollars) from 1970 to 2010. Based on this dataset, we thus should keep in mind that the data become more reliable in the second half of the XX$^{\text{th}}$century.

To estimate the effect of disasters on the local

[1] Or Gulf of America under the Trump administration...

economy, the authors use a linear regression framework, controlling for county fixed effects, decade fixed effects, state-specific linear time trends and its interaction with the initial county population.

We can intuitively claim that an extreme event would reduce firms' productivity by destroying their productive capital or disrupt their supply chains, destroying also housing stocks and diminishing consumer amenities. Adopting the framework of rational expectations, the authors also suggest that a shock from an extreme event could change residents' expectations about future disaster risks, potentially leading to a migration response.

Econometrically, Boustan *et al.* elicit long-lasting effects of natural disasters on local economies. These events increase the net out-migration rates by 1.5 percentage points during a decade and decrease housing prices by 2.5 to 5%. After them, family incomes and housing prices are lower, while the migration rate and the poverty rate have increased. The authors argue that it implies a reduction in firm productivity, hence lower wages, which feed out-migration and the decrease of housing prices. However, we could also suppose that lower housing prices would attract more (poor) people, as it might be suggested by the increase in poverty rates, eventually compensating the net out-migration.

More interesting results lie in the discrepancies between the expected rational results and what is actually observed. A standard hypothesis about people expectations would be that a rare event in a very low risk area would be perceived as an idiosyncratic shock, thus not affecting beliefs about future disaster risks. On the contrary, a new disaster in a low underlying risk area would convey new information about future disaster risks, leading to a potentially stronger migration response, while a new disaster in a county used to face those risks would not convey new information and therefore not affect potential migration. However, using a propensity score based on geographic characteristics, the authors find no evidence of a differential migration response to disasters in high vs low risk areas.

Still, by testing the changing response to extreme events before and after 1980, the authors find that the migration response to milder events has increased over time. They argue that because the frequency of disasters has sharply increased since the eighties, residents might infer that each new event is associated with a higher risk of future disasters, and therefore, a stronger response to events that were previously ignored (the mild events carrying few material effects) would be an evidence of this learning process. If we extrapolated this reasoning, we could argue that it could be associated with a greater understanding of climate change and its consequences. However, we should point out the huge weakness of the idea of a learning about future risk that would increase the migration response: not only it is an extrapolation of the authors' interpretation of the break in events frequency, but it also does not translate very strongly in the estimated coefficients. Their claim is non-significant for the majority of disasters: only the effect of flood before and after 1980 is associated with two statistically significant estimates, which take as value respectively 8.10^{-3} and -8.10^{-3}, which are small effects. When they use lags and lead of the severity variable, they also don't find that future disasters have an effect on present out-migration.

We mentioned how all severe disaster between 1940 and 2010 at the county-by-decade level increased by 1.5 p.p. the net out-migration rate. Interestingly, still considering the full sample, the authors find that storm and tornadoes have no effect on migration flows, and that floods even generates in-migration (*i.e.* more people migrates to a county after a flood, despite the controls for population growth, births and deaths). To explain this phenomenon, they suggest that area prone to flooding receive new infrastructures, although this explanation seems contradictory with their will to observe long-lasting effect after any rebuilding, especially when they perform other regressions to control for the construction of new infrastructures after a disaster which could influence counties' dynamics, especially if the extreme event occurs at the beginning of the decade. We could concede that because the rebuilt infrastructures are expected to last more than a decade, they could still have a attracting effect on people, although it starts to be a bit far-fetched. It nonetheless casts some doubt about the interpretability of a simple panel-data regression to explain migration over a century. As floods happen mostly in Mississippi and New Orleans, looking at particularities among the economy of these states could probably enlighten their results.

We could also advance the idea that because they focus on every declared extreme events, the strength of their estimates might be affected compared to studies that only look at the most extreme disasters, *e.g.* Hurricane Katrina.

Nonetheless, their best achievement lies in the multiple robustness tests performed under various specifications to confirm their results. For instance, by showing robustness to alternative fatality thresholds until 500+ deaths – where the emigration become even stronger, they are able to exhibit a non-linear behavior in migration associated with the severity of the extreme events. To take into account technical changes in urbanism (*i.e.* more robustness of areas facing extreme events), they use an alternative specification where the disaster severity is defined as above median fatalities for a given decade, and find that the results are robust to this alternative definition. The same holds when they instrument the disaster exposure with historical available climate variables, reinforcing the credibility of their disaster declaration and exposure database.

Eventually, when disaggregating by age categories, Boustan *et al.* show that middle age responds more strongly to disasters, with a higher propensity to migrate, whether it declines as people get older (they have a lower mobility).

2.2 Highlighting some other Strengths and Weaknesses

2.2.1 Strengths

Let us come back on the main strengths of the paper. We should congratulate the authors on providing so many robustness checks along a simple but valid methodology. They considered each county as a separate economy subject to a location specific-shock on date t, which was necessary as aggregating would have put too much weight on heavily populated urban areas. They also controlled for counties population to absorb for the higher number of death in more populated counties, and found qualitatively similar results. They did the assumptions that damages don't have long-term effects on birth rates or death rates over a decade, which seems plausible. Boustan *et al.* defined a fatality threshold rather than an absolute count of deaths to avoid endogeneity, knowing that 30% of counties experience a severe disaster in a decade, the severity depending on the level of economic development.

They also build an interesting theoretical framework, rising three main relationship between observable damages and dependent variables.

First, they assume that damages on natural capital would lead to less labor demand, which would in turn lower wages and lead to out-migration, eventually decreasing home prices, even though at short-term, the rebuilding process could increase labor demand, population and housing prices. This assumption can help to justify the need to estimate the net effect of disasters after potential reconstruction effects. The authors also argue that at the decadal level, the housing stock adjusts to follow population variation, which is a very interesting point. They highlight the fact that the price effect of housing will be strongest for the poor, who are more sensitive to housing prices and can trade off a higher real income for higher disaster risks, *i.e.* emigrating less than other socioeconomic categories.

Still in the idea of residents anticipation, a non-expected negative shock on local amenities would lead to out-migration, which would decrease home prices. Because firms productivity would not be affected, we could observe a potential increase in wages to attract workers back, which would be a long-term effect.

Finally, they guess that damages on the housing stock would lead to higher home prices, even though it could depend on the demand for living in the area and the remaining housing stock. The potential reconstruction and more long-lasting regulation on land-use could lead to home price variation in the long-term. However, they also don't find that out-migration is a response to rising housing prices that would follow the destruction of the housing stock.

Despite these good intuitions, it is clear that their model suffers from many flaws and therefore cannot capture the mechanisms they supposed were existing.

2.2.2 Weaknesses

From their theory, they mentioned the idea that the destruction issued by a disaster might change the local economy equilibrium, potentially from an inefficient one established through path dependence to a more efficient one, as a kind of literal creative destruction. They support their claim with empirical evidence, before and after San Francisco and Boston faced urban fires (late XIX - early XX). Looking at the demographics, they argue that in growing area, natural disasters could encourage population growth. In what were previously low productivity places that can retain inefficiently high population levels for decades (because of the existence of a long-lived housing stock), the equilibrium would be reset through a lowered population – if enough houses are destroyed after the disaster, because it would cause out-migration. The destruction of the housing stock in a way would "free" many people from a suboptimal economic equilibrium (*e.g.* inefficient mix of commercial and residential space), which would be the reason why slow-population growth places might see higher out-migration after damages from an extreme event.

However, they actually find that taking into account the interaction of high growth area and natural disaster, stronger out-migration emerges from high-growth areas (*i.e.* places with a high rate of employment or high population growth). Authors testify their surprise at this result, while it seems making more sense to us. They suppose that high-growth places are more incline to respond with higher net-migration because of the people's mobility in the region. It indeed makes more sense as people in economically healthier regions could move out more easily if a strong extreme event strikes. Also, because they are often in the area for fewer years on average, the authors claim that people have more potential for learning new information about their environment. We disagree with this hypothesis that didn't convinced us in the first place, and instead suppose that because of the higher economic activity and the greater mobility, people might have less emotional attachment to an economically dynamic area where many of them might have move because of their job rather than because they feel rooted.

The previous results should still be challenged due to the way they proxy local productivity as employment growth, which is defined as:

$$\Delta_{\text{employment}_{ijt}} = \frac{\sum_{l=1}^{L} \alpha_{\text{employment}_{i,l,t=1930}} \cdot \gamma_{l,t}}{\alpha_{\text{employment}_{l,t=1930}}}$$

with $\gamma_{t,l}$ the national growth rate in employment for the industry l and decade t and $\alpha_{\text{employment}_{l,t=1930}}$ the share of workers in county i who worked in industry l in the base year 1930.

Because unemployment data is not available at the county level and might be endogenous, they build "time-varying economic conditions" with initiation industrial composition at the county level with national employment trends. We suggest here that it might completely fail to capture structural changes in the economy, *e.g.* a strong tourism development on specific places that were not visited a century ago, the development of the spatial industry, etc.

About the core justification of the change-of-equilibrium theory, we also wanted to mention the following question: instead on looking at slow-growth vs. high-growth areas, wouldn't it be more relevant to look at the potential changes in growth speed before and after the natural damages, eventually accompanied with less net out-migration? Which is certain, it is that the authors' hypothesis on the equilibrium change is not supported by their results and overall doesn't seem very credible.

Another excessive interpretation could be found in the relationship between extreme events and poverty rates. They find that disasters increase the local poverty rate by 0.8 percentage points, which is one of the lowest effect provided in this study, where most of the effects already seem very low. Because of the regression framework, we have to remind that they cannot draw any causality from this result. They suggest that poverty rates increase because of a negative flow of household above the poverty line and in the opposite a positive flow of people looking for lower housing prices, and also mention the idea that natural disasters increase the probability that the existing population falls into poverty. While the previous assumptions seem completely plausible, they end up with the claim that "lower demand due to persistent natural disasters leads to falling rents and acts as a poverty magnet". While it could be realistic once again, it remains a very strong statement compared to the level of proof, especially of causality, that the authors provide. In the following replication, we will aim to be careful and precocious about the claims that we can bear from the degree of proof we will provide.

3 Replication of the Study

Our primary objective in this replication was to reproduce the results obtained in the original study while ensuring that we captured the same overarching trends. However, we quickly encountered a significant challenge: the availability of data. Accessing the necessary datasets proved to be difficult, and the original study provided minimal details regarding data preprocessing.

To overcome this issue, we first sought to collect as many of the datasets used in the original study as possible. Once gathered, we processed and analyzed these datasets to conduct our study on a reduced dataset. When necessary, we supplemented the data with external sources to ensure the completeness and reliability of our analysis while staying as close as possible to the original methodology.

3.1 About the Data

One of the most challenging aspects of this project was the collection and identification of relevant data to replicate the study. In the absence of a response from the original authors and with certain datasets restricted behind paywalls, we focused on obtaining the most crucial datasets available. All the data used in this replication are structured as panel data, allowing us to directly apply the econometric methods discussed in our coursework. To simplify our research, we restricted our dataset collection to the years 1950–2010.

Despite extensive searches, including the use of sources cited by the authors, certain datasets remain unavailable. This raises critical questions about data accessibility and the scientific process itself: while publishing research findings is essential, it is equally important to establish systems that, if not making datasets public, at least ensure their structure and availability over time[2]. A study that cannot be replicated shouldn't be validated by peer review.

3.1.1 FEMA and Disaster Data

The most fundamental dataset for this study is the **FEMA Disaster Declarations Summaries dataset**, which we retrieved from OpenFEMA (see the bibliography for more details) [14]. FEMA (Federal Emergency Management Agency) is the U.S. federal agency responsible for coordinating disaster response and relief efforts. Its dataset provides information on all officially declared disasters in the U.S. from **1950 to 2010**, categorized by county. This dataset is crucial because it forms the basis for identifying natural disaster events, a key explanatory variable in our analysis.

To check the integrity and comparability of our data, Table 1 presents a comparison between our dataset and the summary statistics provided in Boustan *et al.* (2020). Our dataset records the number of disasters for each type per decade and per county, ensuring consistency in measuring disaster frequency over time. This approach allows for a meaningful comparison with Boustan *et al.* (2020), who employ a similar methodology in their analysis.

While some values are relatively close (e.g., flood

[2]For instance, zenodo is becoming widely used in research to store code

Table 1: Comparison of Disaster Statistics: Our Dataset vs. Boustan *et al.* (2020)

Incident Type	Our Data (1960-2010)		Boustan *et al.* (1930-2010)	
	Mean	**Std Dev**	**Mean**	**Std Dev**
Flood	0.513	0.732	0.484	0.851
Snowstorm	0.217	0.562	0.724	1.570
Hurricane	0.465	0.949	0.312	0.913
Tornado	0.0863	0.260	0.207	0.572
Fire	0.162	2.630	0.095	0.528
Other	NA	NA	0.010	0.105
Total	NA		1.830	

and hurricane frequencies), there are notable discrepancies, particularly in tornadoes and fire incidents.

One potential reason for these differences is the classification of events. Boustan *et al.* categorize winter storms separately, whereas our dataset uses "Snowstorm" which may or may not correspond to the same definition. Similarly, the way disasters are recorded over time (*e.g.*, changes in FEMA records) could contribute to variations.

Despite these limitations, the general trends in disaster frequency appear to be consistent, suggesting that our dataset captures key elements of the study while highlighting potential variations in classification and data coverage.

Additionally, we visualized the geographical distribution of disaster occurrences across U.S. counties (fig. 2). Our results indicate that the proportional distribution of disasters across counties closely aligns with the original study's findings (see fig. 1. The heatmap highlights regional variations in disaster frequency, with higher concentrations in disaster-prone areas such as the Gulf Coast and Tornado Alley, consistent with Boustan *et al.* (2020).

Regarding older disaster records from the **Red Cross**, we were unable to locate any accessible datasets. Furthermore, linking data from **EM-DAT (The International Disaster Database)** to U.S. counties proved unfeasible due to missing geographical identifiers. The original paper does not provide details on how this linkage was established, raising concerns about the replicability of their severe disaster categorization.

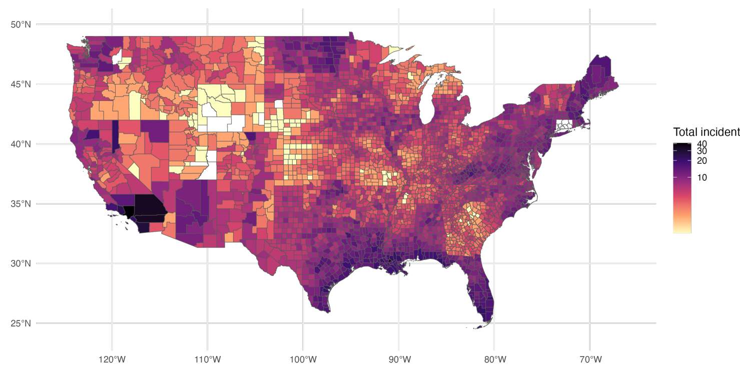

Figure 2: Heatmap of Disaster Occurrences by County (1960-2010). The spatial distribution of disasters closely resembles the findings of Boustan *et al.* (2020).

3.1.2 Net Migration Data

Another crucial dataset in our replication is the **Net Migration by Decade dataset** [15], which provides county-level migration estimates from **1950 to 2010**. These data are essential because they represent the primary **dependent variable** in the study, allowing us to examine how migration patterns respond to natural disasters.

The dataset, obtained from the **Applied Population Laboratory at the University of Wisconsin-Madison**, aggregates migration data by decade. This structure facilitates our replication, as it closely follows the methodology of the original paper. However, it is important to note that the dataset underwent a **major revision in 2024**, which led to noticeable changes in reported migration patterns. This update may introduce discrepancies between our findings and those of the original study.

3.1.3 NHGIS and Socioeconomic Data

The final dataset we utilized is from the **National Historical Geographic Information System (NHGIS)** [16], which compiles historical U.S. Census data at the county level. NHGIS provides various socioeconomic indicators, of which we focused on two key variables:

- **Median Household Income (B79)**: Available for the years **1980, 1990, and 2000**.
- **Persons Below Poverty Level (CL6)**: Available for the years **1970, 1980, 1990, and 2000**.

NHGIS data use **Nominal Geographic Integration**, meaning that geographic units are matched based on administrative names and codes, without adjustments for boundary changes over time. This may introduce inconsistencies in time-series comparisons, an issue also acknowledged in the original study.

While NHGIS provides valuable socioeconomic insights, its **temporal limitations** restrict our ability to conduct a full-scale replication. The original study faced similar constraints, but without access to their full dataset, it is unclear how they addressed these issues.

3.1.4 Summary of Data Challenges and Replication Goals

Ultimately, we successfully obtained most of the critical datasets used in the original study, though with some differences in scope and temporal coverage. The main limitations include:

- **Missing employment rate data**, which was used in the original study but is not readily available from public sources.
- **Inability to match severe disaster data from EM-DAT with U.S. counties**, due to missing geographic identifiers.
- **Shorter time span for certain socioeconomic indicators**, particularly income and poverty measures.

Given these constraints, we set a **realistic replication objective**: rather than perfectly reproducing the exact numerical results of the original paper, we aim to verify whether our dataset allows us to observe the same **broad trends** identified in the study. The following sections outline our methodology and findings.

3.2 Replication Methodology

Using the available data, we aim to replicate the econometric framework presented in Boustan *et al.* (2020) as faithfully as possible. The original study employs the following linear regression model:

$$\begin{aligned} Y_{ijt} = \mu_i + \xi_t + \beta_1 Disasters_{ijt} \\ + \beta_2 \Delta employ_{ijt} + \beta_3 (X_{ij} \cdot t) + U_{ijt} \end{aligned} \quad (3.1)$$

where:

- Y_{ijt} represents the dependent variables, including net migration rate, logarithm of median housing prices (or rents), median family income, and poverty rate at the county level.
- μ_i captures county fixed effects.
- ξ_t represents decade fixed effects.
- $Disasters_{ijt}$ is the count of disasters in county i of state j over a given decade.
- $\Delta employ_{ijt}$ is the estimated county employment growth from decade $t-10$ to t, based on initial industrial composition.
- $(X_{ij} \cdot t)$ represents the interaction between initial county population and a linear time trend.
- U_{ijt} is the error term.

Due to the absence of some specific parameters in our dataset, we opt for a simplified explanatory model:

$$Y_{ijt} = \mu_i + \xi_t + \beta \cdot Disasters_{ijt} + U_{ijt} \tag{3.2}$$

While we acknowledge the simplification of the original framework, our primary objective is to establish a model that can pass robustness checks and capture the general trends observed in Boustan *et al.*. By focusing on the core relationship between disaster occurrences and economic outcomes, we aim to verify whether our results align with the broader findings of the original study.

The complete implementation of our replication code, the full dataset, along with all scripts used for data processing and econometric analysis is available on our GitHub repository.

For any questions regarding our methodology or implementation, we are available for further discussion[3].

3.2.1 Panel Data Construction

As a first step, we constructed a county-level panel dataset for the United States. Each county is identified using its unique Federal Information Processing Standards (FIPS) code and is tracked across multiple decades. This structure ensures consistency with the original study while allowing us to examine the long-term effects of natural disasters.

To replicate the disaster exposure variables from Boustan *et al.* (2020), we counted the number of disasters occurring in each county per decade, categorized into the following types: Floods, Snowstorms, Hurricanes, Tornadoes and Fires. These disaster categories match those used in the original study, enabling a direct comparison of results.

Additionally, we collected key economic indicators at the county level. Our dataset includes:

- **Decadal median income**: The median family income for each county at the end of each decade.
- **Net migration rates**: County-level net migration by age cohort, aggregated over each decade.

These economic indicators serve as both dependent variables and controls in our econometric models. By combining disaster incidence data with economic variables, we created a structured panel dataset suitable for analyzing the relationship between natural disasters and long-term economic trends.

For further details on the data processing steps, we provide specific code snippets in the appendix (section

3.2.2 Econometric Model

To analyze the impact of natural disasters on migration patterns, we employ a panel data regression model using the `plm` package in R. The model estimates the effect of disaster occurrences on net migration at the county level over multiple decades.

Model Specification
Our regression model takes the following form:

$$\begin{aligned} \text{total\_migration}_{ijt} = \mu_i + \xi_t &+ \beta_1 \cdot \text{nb\_fire}_{ijt} \\ &+ \beta_2 \cdot \text{nb\_flood}_{ijt} \\ &+ \beta_3 \cdot \text{nb\_tornado}_{ijt} \\ &+ \beta_4 \cdot \text{nb\_hurricane}_{ijt} \\ &+ \beta_5 \cdot \text{nb\_snowstorm}_{ijt} + U_{ijt} \end{aligned} \tag{3.3}$$

Choice of Panel Data Model
We opt for a **two-way fixed effects model** using the **within estimator**, which accounts for both county-level and decade-level unobserved heterogeneity. This specification is justified for several reasons:

- **County fixed effects** (μ_i) control for time-invariant characteristics that could influence both migration trends and disaster frequency (*e.g.* geographical features, long-term economic conditions).

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- **Decade fixed effects** (ξ_t) control for macroeconomic shocks, policy changes, and national trends affecting migration patterns over time.
- The **within estimator** eliminates potential bias arising from omitted variables that are constant within counties or decades.

Implementation in R
The model is estimated using the `plm` package, which is specifically designed for panel data analysis. The command *plm* specifies a **two-way fixed effects model** using the **within transformation**.

Justification for Excluding Other Controls
Although the original study includes additional economic controls (*e.g.* employment growth, interaction terms), we opted for a simplified specification due to data limitations. Our primary goal is to verify whether disaster occurrences significantly affect migration trends in a manner consistent with Boustan *et al.* (2020). Future extensions could incorporate additional controls to improve robustness.

Overall, this model allows us to isolate the relationship between natural disasters and migration dynamics while accounting for unobserved heterogeneity at the county and decade levels.

3.2.3 Tests

Performing tests on our model is crucial to ensure its robustness and validity. While our results seem to align with the trends observed in Boustan *et al.* (2020), it is essential to conduct tests to verify whether the model's assumptions hold true. Without such tests, our findings cannot be deemed conclusive. However, given that we have fewer control variables in our model compared to the original study, we expect some effects to be less pronounced.

The tests we conducted are as follows:

Hausman Test
The Hausman test is essential when working with panel data as it helps decide between using fixed effects or random effects models. This test is particularly important in our study because it determines whether the individual effects (specific to each county) are correlated with the explanatory variables. If they are, we should prefer the fixed effects model. This test is critical for distinguishing between the two model types, and given the panel data structure of our study, it is a key part of our econometric analysis.

This test compares the fixed effects model (`model_fe`) with the random effects model (`model_re`) and helps us decide which model is more appropriate.

Breusch-Pagan Test for Random Effects
Next, we applied the Breusch-Pagan test to check for heteroscedasticity in the random effects model. This test assesses whether the variance of the error terms is constant across observations. In the context of random effects, if heteroscedasticity is present, it would suggest that the random effects model may not be appropriate.

Wooldridge Test for Autocorrelation
We then conducted the Wooldridge test for autocorrelation in the panel data. Autocorrelation, or serial correlation, can occur when the residuals of our model are correlated across time within the same unit (county). This would violate the assumption of independent errors in panel data models. The Wooldridge test helps us identify if such autocorrelation exists.

Breusch-Pagan Test for Heteroscedasticity
In addition to checking for heteroscedasticity in the random effects model, we also performed the Breusch-Pagan test on the fixed effects model to ensure that the errors are homoscedastic (*i.e.* have constant variance).

Kolmogorov-Smirnov Test for Normality
We conducted the Kolmogorov-Smirnov (KS) test to check whether the residuals of the fixed effects model follow a normal distribution. This is important because many statistical tests, including the inference tests on the coefficients, assume that the residuals are normally distributed.

Variance Inflation Factor (VIF)
To assess the presence of multicollinearity among the explanatory variables, we calculated the Variance Inflation Factor (VIF). High VIF values indicate a high degree of multicollinearity, which can inflate the standard errors of the regression coefficients and distort statistical inference.

RESET Test for Model Specification
To test for potential misspecification of the model, we conducted the RESET (Regression Equation Specification Error Test) to check whether the functional form of the model is correctly specified. The RESET test evaluates whether adding higher-order terms (e.g., quadratic or cubic terms) could improve the model.

Newey-West Robust Standard Errors
Finally, we applied Newey-West standard errors to adjust for potential autocorrelation and heteroscedasticity in the residuals. This adjustment ensures that the standard errors of the estimated coefficients are robust, leading to more reliable inference.

By performing these tests, we ensure that our model is robust and the results are reliable. Each test addresses a specific aspect of model validity, ranging from heteroscedasticity and autocorrelation to multicollinearity and model specification. In particular, the Hausman test played a central role in guiding us towards the appropriate choice of model for our panel data analysis, ensuring the accuracy and relevance of our findings.

3.3 Results

3.3.1 Migration Model

We present the results of the two-way fixed effects model (within model) for the analysis of the impact of various types of disasters on total migration. The regression results indicate the relationship between migration and the occurrence of five disaster types: fire, flood, tornado, hurricane, and snowstorm. The model was estimated using panel data from 3166 counties, with 1 to 7 observations per county, for a total of 12189 observations. Below, we report the key coefficients and their statistical significance, as summarized in Table 2.

The results show that the coefficients for *nb_fire* and *nb_flood* are statistically significant at the 1% and 5% levels, respectively. Specifically, a fire event is associated with a significant emigration, with an estimated coefficient of − 6082.2. Conversely, a flood event is associated with an increase in migration in the county, with an estimated coefficient of 6822.1.

The coefficients for *nb_tornado*, *nb_hurricane*, and *nb_snowstorm* are not statistically significant at the 5% level, indicating that these types of disasters do not have a strong or consistent effect on migration in our sample.

Additionally, the model's R-squared value is quite low at 0.0018, indicating that the model explains only a very small proportion of the variation in migration. The adjusted R-squared value is negative, which suggests that the inclusion of the disaster variables does not improve the model fit when accounting for the fixed effects. This low explanatory power is consistent with our expectation, given the simplified model with fewer control variables.

The F-statistic is significant (p-value = 0.0053), suggesting that the model as a whole is statistically significant, but the individual variables' contributions vary in their significance.

In summary, our results suggest that certain types of disasters, particularly fires and floods, are related in a way to migration, while others, such as tornadoes, hurricanes, and snowstorms, show weaker effects. Further refinement of the model, possibly incorporating additional controls, might help improve the explanatory power of the model.

3.3.2 Tests Conducted for the Migration Model

Several tests were performed to assess the robustness of our model and ensure that the assumptions underlying the fixed effects specification are met.

- **Hausman Test:** The test result showed a significant chi-squared value of 136.1 with 5 degrees of freedom and a p-value less than

Variable	Estimate	Std. Error	t-value	Pr(>|t|)
nb_fire	−6082.2	2003.9	−3.0351	0.0024 **
nb_flood	6822.1	2915.0	2.3403	0.0193 *
nb_tornado	9398.6	6964.6	1.3495	0.1772
nb_hurricane	1457.9	2131.9	0.6839	0.4941
nb_snowstorm	3233.0	3957.0	0.8170	0.4139

Table 2: Regression results for the two-way fixed effects model of migration

$2.2e^{-16}$, indicating that the fixed effects model is preferred over the random effects model. This result confirms that the unobserved heterogeneity in migration data is correlated with the explanatory variables.

- **Breusch-Pagan Test for Random Effects:** The test result yielded a chi-squared value of 0.001082 with a p-value of 0.9738, which indicates that random effects are not statistically significant, further supporting the choice of fixed effects in our model.

- **Wooldridge Test for Serial Correlation:** The result showed an F-statistic of 1.3842 with a p-value of 0.2394, suggesting no evidence of serial correlation in the residuals at the 5% significance level.

- **Breusch-Pagan Test for Heteroscedasticity:** The result showed a chi-squared value of 12.042 with a p-value of 0.03422, indicating that heteroscedasticity is present at the 5% significance level. This suggests the need for robust standard errors to account for the variance in the errors.

- **Kolmogorov-Smirnov Test for Normality of Residuals:** The result showed a D-statistic of 0.53089 with a p-value less than 2.2e-16, indicating that the residuals significantly deviate from a normal distribution.

- **Newey-West Robust Standard Errors:** Finally, to account for heteroscedasticity and autocorrelation, we used Newey-West robust standard errors in the coefficient tests. The results showed that the coefficient for *nb_fire* is statistically significant at the 5% level, while the coefficients for other disaster types remained insignificant.

These tests provide crucial insights into the validity of our model. While the Hausman test strongly supports the use of fixed effects, other tests such as the Breusch-Pagan and Wooldridge tests indicate that the model may still have some issues, such as heteroscedasticity and non-normal residuals. For a detailed summary of the tests, please refer to Table 3.

3.3.3 Income Model

We present the results of the two-way fixed effects model (within model) for the analysis of the impact of various types of disasters on median income. The regression results indicate the relationship between income and the occurrence of five disaster types: fire, flood, tornado, hurricane, and snowstorm. The model was estimated using panel data from 3105 counties, with 1 to 3 observations per county, for a total of 6522 observations. The key coefficients and their statistical significance are summarized in Table 4.

The results show that *nb_hurricane* and *nb_snowstorm* are statistically significant at the 1% level, with coefficients of -289.307 and 721.484, respectively. Both indicate substantial effects on income, with hurricanes leading to a decrease in income and snowstorms leading to an increase.

The coefficients for *nb_fire*, *nb_flood*, and *nb_tornado* are not statistically significant at the 5% level, suggesting that these disasters do not have a strong or consistent effect on income.

Test	Statistic	p-value
Hausman Test	$\chi^2 = 136.1$	$p < 2.2 \cdot 10^{-16}$
Breusch-Pagan Test (random effects)	$\chi^2 = 0.0011$	0.9738
Wooldridge Test (serial correlation)	F = 1.3842	0.2394
Breusch-Pagan Test (heteroscedasticity)	BP = 12.042	0.03422
Kolmogorov-Smirnov Test (normality)	D = 0.53089	$p < 2.2 \cdot 10^{-16}$

Table 3: Summary of test results for the fixed effects model.

Variable	Estimate	Std. Error	t-value	Pr(>|t|)
nb_fire	−25.890	64.617	−0.4007	0.6887
nb_flood	117.782	82.929	1.4203	0.1556
nb_tornado	128.347	166.283	0.7719	0.4403
nb_hurricane	−289.307	47.610	−6.0766	$1.36 \cdot 10^{-9}$ ***
nb_snowstorm	721.484	74.210	9.7222	$< 2.2 \cdot 10^{-16}$ ***

Table 4: Regression results for the two-way fixed effects model of income.

The model's R-squared value is low at 0.0375, indicating that only a small portion of the variation in income is explained by the model. The adjusted R-squared value is negative, suggesting limited explanatory power. However, the overall F-statistic is significant (p-value $< 2.22 \cdot 10^{-16}$), indicating the model is statistically significant as a whole.

Comment on p-values

The extremely low p-values for *nb_hurricane* and *nb_snowstorm* seem to suggest very strong evidence against the null hypothesis, indicating that these variables are significantly correlated income. However, the large number of observations (6522) can contribute to very small p-values even for relatively small effects. This phenomenon is common in large datasets, where statistical significance can be achieved even when the practical significance of the coefficients is limited. Hence, while these results suggest a statistically significant relationship, further investigation may be needed to assess the practical relevance of these findings.

3.3.4 Model Tests

Several diagnostic tests were conducted to assess the validity of our fixed effects model:

- **Hausman Test:** Strongly supports the fixed effects model (p-value $< 2.22 \cdot 10^{-16}$).
- **Breusch-Pagan Test (Random Effects):** Indicates no significant random effects (p-value = 0.9738).
- **Wooldridge Test (Serial Correlation):** Shows no serial correlation (p-value = 0.2394).
- **Breusch-Pagan Test (Heteroscedasticity):** Reveals significant heteroscedasticity (p-value = 0.03422).
- **Kolmogorov-Smirnov Test (Normality):** Indicates non-normal residuals (p-value $< 2.22 \cdot 10^{-16}$).

These tests confirm the model's partial robustness but suggest issues with heteroscedasticity and non-normal residuals.

4 Discussion and Interpretation

4.1 Comparison with Original Findings

Our results provide valuable insights into the relationship between natural disasters and migration, as well as economic outcomes. Comparing our findings to those of Boustan *et al.* (2020), we observe both consistencies and discrepancies, largely attributable to differences in datasets, timeframes, and methodological approaches.

4.1.1 Impact on migrations

Regarding migration patterns, Boustan *et al.* (2020) found a significant negative impact of severe disasters on net in-migration rates, with wildfires (-0.013^{**}) and hurricanes (-0.008^{**}) being particularly influential. In contrast, our results suggest also a negative effect of wildfires (-6082.2, $p = 0.0024$) but an insignificant effect of hurricanes ($p = 0.4941$) on migration. This discrepancy may stem from differences in geographical scope and time periods covered in our study. About fires, we could still intuitively guess that they are more likely to destroy housing or environmental amenities (compared to snowstorms that might carry less damages or tornadoes that hit less densely populated areas), as the impressive southern California wildfires from last January can remind us[4].

Floods, which Boustan *et al.* (2020) found to have a small positive but insignificant effect on migration, show also a positive effect in our model (6822.1, $p = 0.0193$). It seems to confirm the attractivity of places subject to flooding, even though neither the authors nor us are able to bring a reliable explanation to this phenomenon. Meanwhile, tornadoes and snowstorms exhibit weak or non-significant effects in both studies.

4.1.2 Impact on Income

Turning to economic outcomes, Boustan *et al.* found mixed effects of disasters on family income. While wildfires (0.013^{**}) and tornadoes (0.018^{*}) were associated with income increases, hurricanes (-0.015^{**}) had a significant negative impact. On our side, hurricanes effectively exhibit a negative effect on income (-289.307, $p < 1.36 \cdot 10^{-9}$), aligning with Boustan *et al.*. However, in contrast to their findings, snowstorms have a significant positive effect on income in our study (721.484, $p < 2.2 \cdot 10^{-16}$), whereas Boustan *et al.* (2020) did not report such an effect.

Fires, which Boustan *et al.* linked to higher income, appear to have no significant impact in our study ($p = 0.6887$). Similarly, the effect of floods on income is not statistically significant in our results ($p = 0.1556$), whereas Boustan *et al.* found a weak positive effect.

The common denominator between their study and ours is therefore the anticorrelation between hurricanes and wages. Considering the list of disaster that we study, we could guess that this relationship emerges because hurricanes are the disasters most likely to destroy productive capital and *in fine* cause losses of income. Indeed, wildfires mostly affects forests, which are a tiny part of the productive sector, and tornadoes and floods don't happen in the richer states of the USA, compared to hurricanes in Florida, along the coast. We should nonetheless remain precocious considering the poor statistical strength of our model, and can highlight the fact that Boustan *et al.* rarely interpreted in their paper unusual results such as positive relationship between a disaster and a socioeconomic condition.

4.2 Limitations of the Replication

There are several limitations to our replication that must be acknowledged. First, the way we quantify disaster effects differs from Boustan *et al.* (2020), which prevents a direct term-by-term comparison. One key distinction is that they did not normalize their coefficients, whereas our study takes a different approach to standardizing variables. This difference in methodology affects the scale and interpretation of our estimates and may contribute to some of the observed discrepancies.

Second, we do not use the same dataset as Bous-

[4] see more infos here.

tan *et al.* (2020). Differences in data processing, the time period covered, and the availability of certain variables introduce inherent challenges in making precise comparisons. Some data used in their study were not accessible to us, and our dataset may have been constructed with slightly different filtering or aggregation techniques, potentially leading to variations in results.

Despite these differences, we are encouraged to find similar trends in our findings. The fact that we observe comparable migration and economic responses to disasters, even with distinct datasets and methodological choices, reinforces the robustness of these general patterns. This suggests that certain disaster effects on migration and income may be consistent, highlighting the broader relevance of these relationships.

4.3 Potential Improvements

Future research could enhance our approach in several ways:

- **Disaster severity:** One key finding in Boustan *et al.* (2020) is that the most significant effects on migration stem from severe disasters. However, due to data limitations, we were unable to quantify disaster severity in our study. Future work should focus on collecting and integrating severity indicators, such as economic damages, fatalities, or disaster intensity measures, to refine our understanding of how extreme events drive migration decisions.
- **Heterogeneous effects:** Migration and economic responses to disasters likely vary depending on regional characteristics and socioeconomic factors. Investigating how different demographic groups, income levels, or urban versus rural areas are affected by specific disasters would provide deeper insights into adaptation and resilience mechanisms.
- **Regional control:** Our results indicate some inconsistencies across disaster types, suggesting that additional regional controls could improve the robustness of our estimates. Incorporating variables related to local economic conditions, infrastructure, and government responses—similar to those used in Boustan *et al.* (2020)—could help isolate the true impact of disasters on migration and income.
- **Time period:** Expanding the dataset to cover a longer time horizon would allow for a better alignment with Boustan *et al.* (2020) and help verify whether observed trends persist across different decades. This would also provide a more comprehensive view of long-term migration patterns and economic shifts following disasters.

Given the methodological differences between our study and Boustan *et al.* (2020), addressing these limitations could help bridge the gap between our findings and theirs. Specifically, by refining disaster quantification, incorporating additional controls, and considering heterogeneous effects, an extended work could provide a clearer and more robust understanding of how disasters shape migration and economic outcomes.

4.4 Discussion of Methodological Choices

Besides our replication limits, we should also ackowledge the ones from the original study. As we mentioned previously, some good methodological choices have been made, especially in the choice of controls (*e.g.* distance to the coast to control for county-specific trends) and the robustness checks.

However, some last remarks should be addressed. First, let us denote that taking severe disasters as those with 25+ deaths (the median count for disasters with known fatality counts), only 151 are presents in the sample, which represent 1.5% of the total disasters. While they often mention the total number of disasters in the paper, they rarely explicit the shrink in observable data when they focus on severe disasters.

Moreover, in their model, one disaster can have multiple declarations if it affects multiple counties, and can therefore be double-counted. It would have been interesting to explore how this methodological choice affected the results, as it biases the given analysis towards the largest events. Especially, having a smaller temporal range (that might confuse too many structural changes into a single regression) but information about the migrating distance would have been very interesting, as local migration response might push people further if a whole region is affected by an extreme event.

We could also challenge their definition of what is a disaster. They claim that examining all disaster types (not just hurricanes) over a very long historical period allows to look at a broader range of effects than the usual extreme events consideration. Yet, they chose to exclude disasters caused by human activities, and include drought in the latter category. However, when it was included in their regression (as in the table 19 in their paper's appendix), they find a surprising significant positive effect on migration rate. They justify the exclusion of drought by considering it as a policy decision of water use rather than an environmental event. This does not sound very convincing, as at least forest fires could be associated with local environmental policies, as in forest management.

About the disaster declaration, Boustan *et al.* acknowledge that the FEMA disaster declaration process depends on the country political context, as it relies on a presidential decision, after the state determined that the damages exceeded their resources. Thus, states that are politically important for the president are more likely to receive a declaration and more funds, which could counter the bias by supporting in-migration. Authors claim that their results are not being driven by this bias, and that the estimated effect of severe disasters is robust to the definition of severe. They admit that mild natural catastrophes could be politically manipulated, but that all the largest have all received federal disaster designations.

While we agree with the fact that higher rate of disaster declaration might encourage higher flows of federal funds, it does not seem obvious that it would lead to in-migration. The sole justification that the political component of disaster declaration would bias against out-migration is not very satisfying. One could argue that more federal funds could help people keep the bare minimum in order to survive these events, and then move out with more ease because of the help they received (which would build into the authors' hypothesis of the switch above or below a poverty line: federal funds could help people stay above the poverty line, and then move out). Fortunately, there argue that there is no effect of FEMA payments on out-migration (see their appendix table 7). Again, we should temperate this claim: they indeed control the effect of a disaster on out-migration when the governor and the president have the same party. They don't find that disaster declaration driven by political considerations should lead to less out-migration, but only an lack of significativity in their regression coefficients. Again, the level of proof borne by the provided analysis might not be enough to rule out the possibility of a political effect. Instead, alternative approaches such as using scientific metrics of disaster severity (*e.g.* wind speed, flood depth) rather than fatality thresholds could have avoided this possible endogenous bias.

Eventually, we should question the whole econometrical analysis of the paper. The authors' goal is to show that natural disasters have long-lasting effects on local economy (especially on migration rate) : they are pretending to show a causal relationship between the two. Hence, choosing a panel data analysis over a century doesn't seem the most appropriate methodology, compared for instance to a DiD approach. While they claim that they check for parallel trends, they only do it through the addition of county-specific linear trends as control variables. They could have restricted their temporal and spatial range to instead provide causality with Rubin causal models such as DiD or Structural Equation Modelling. It might have been feasible to look at economic outcomes within counties before and after some disasters, and compare them to counties that did not experience a disaster in the decade.

5 Conclusion

Boustan *et al.* (2020) provide a valuable contribution to the literature on natural disasters and economic resilience by leveraging a rich historical dataset and identifying long-term migration and housing market effects. Their paper is still useful as it installs the idea of a global positive relationship between natural disasters and long-term out-migration, which is amplified by the increase in frequency of those disasters and if the area has a growing economy. Our replication efforts confirm some of the major trends in their findings but also highlight key methodological limitations, particularly concerning data availability and robustness of identification strategies. The primary discrepancies between our results and theirs stem from data access constraints and differences in econometric specifications. Future work could improve upon these findings by using a more refined identification strategy, incorporating more detailed measures of disaster severity, or exploring heterogeneous responses across demographic and socioeconomic groups. The implications of these findings extend beyond migration dynamics, raising important questions about long-term economic resilience in disaster-prone regions.

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6 Appendix

6.1 Code

The complete code used for this study is available on our GitHub repository. Interested readers can access it at the following link: `https://github.com/t-da-costa/Boustan-et-al-replication`

For any questions or clarifications regarding our methodology, we are available for discussion via GitHub or email.

In this section, we highlight three key segments of our code that we consider essential to our analysis:
(1) the construction of our panel dataset,
(2) the implementation of our econometric models, and
(3) the execution of robustness tests.
These steps are crucial in ensuring the validity and comparability of our results with those of Boustan *et al.* (2020).

6.1.1 Snippet 1: Data processing

```r
# Create 'decade' variable (round year to decade)
fema_data <- fema_data %>%
  mutate(decade = floor(year(incidentBeginDate) / 10) * 10)

# Keep only selected disaster types
categories <- c("Fire", "Flood", "Snowstorm", "Tornado", "Hurricane")
fema_filtered <- fema_data %>%
  filter(incidentType %in% categories)

# Count disasters by FIPS, decade, and type
fema_filtered <- fema_filtered %>%
  group_by(fips, decade, incidentType) %>%
  summarise(disasters_count = n(), .groups = "drop")

# Create binary disaster variables
fema_decade_summary <- fema_filtered %>%
  mutate(
    is_fire = ifelse(incidentType == "Fire", disasters_count, 0),
    is_flood = ifelse(incidentType == "Flood", disasters_count, 0),
    is_tornado = ifelse(incidentType == "Tornado", disasters_count, 0),
    is_hurricane = ifelse(incidentType == "Hurricane", disasters_count, 0),
    is_snowstorm = ifelse(incidentType == "Snowstorm", disasters_count, 0)
  ) %>%
  # Sum disasters by FIPS and decade
  group_by(fips, decade) %>%
  summarise(
    disasters_count = sum(disasters_count),
    is_fire = sum(is_fire),
    is_flood = sum(is_flood),
    is_tornado = sum(is_tornado),
    is_hurricane = sum(is_hurricane),
    is_snowstorm = sum(is_snowstorm),
    .groups = "drop"
  )
```

6.1.2 Snippet 2: Migration Model

```
# Merge FEMA, migration, and income data
panel_data <- migration_data_aggregated %>%
  left_join(fema_decade_summary, by = c("fips", "decade"))

# Convert data to panel format
panel_data_plm <- pdata.frame(panel_data, index = c("fips", "decade"))

# Estimate fixed-effects migration model
migration_model <- plm(total_migration ~ is_fire + is_flood + is_tornado +
                         is_hurricane + is_snowstorm,
                       data = panel_data_plm,
                       effect = "twoways",
                       model = "within")
```

6.1.3 Snippet 3: Tests

```
# Hausman test: fixed vs. random effects
phtest(model_fe, model_re)

# Breusch-Pagan test: random effects significance
plmtest(model_fe, type = "bp")

# Wooldridge test: serial correlation
pwartest(model_fe)

# Breusch-Pagan test: heteroskedasticity
bptest(model_fe)

# Kolmogorov-Smirnov test: residual normality
ks.test(residuals(model_fe), "pnorm")

# VIF test: multicollinearity detection
vif(model_fe)

# RESET test: model specification check
resettest(model_fe)

# Newey-West robust standard errors
coeftest(model_fe, vcov = vcovNW(model_fe))
```