Web Appendix for

Storms and Jobs: The Effect of Hurricanes on Individuals' Employment and Earnings over the Long Term

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9.1 Worker Data

In order to examine longitudinal outcomes for individuals potentially affected by Hurricanes Katrina and Rita, this paper makes use of restricted-access administrative and survey data brought together at the U.S. Census Bureau. The combined dataset tracks quarterly labor-market outcomes and includes a variety of demographic variables. The structure of the combined dataset permits us to examine individuals before and after the storms and to examine storm effects over a seven-year period. The large sample size also allows us to obtain precise parameter estimates and enables us to examine subsamples of the population.

We begin with an extract from the 2000 Census long-form microdata and American Community Survey (ACS) microdata (from January 2003 to July 2005) of persons who were aged 25–59 in 2005 and at least 25 when they responded to the survey. The 2000 long-form, or Sample Census Edited File, contributes approximately 90 percent of the respondents overall, but the ACS provides all of the respondents under age 30 in 2005.[1] The lower bound for age reduces the likelihood of non-employment reflecting college attendance and improves the likelihood that reported educational attainment reflects attainment as of 2005. The upper bound for age reduces the likelihood of retirement within the study period. From the survey responses, we obtain demographic information (age, sex, race, and ethnicity) and educational attainment. In order to match the survey records to administrative data, we make use of a unique personal identifier, called a Protected Identification Key (PIK). The Census Bureau uses federal administrative data to probabilistically match survey responses to a PIK, based on a comparison of personally identifying information.[2] For this combined survey sample, approximately 90 percent of records have a PIK match.

For each person in the survey sample, we determine a pre-hurricane residential location, using a PIK-linked address file based on federal administrative records. The Census Bureau produces an annual Composite Person Record (CPR) residence file, which provides a single residence location for a PIK in a given year (Abowd et al., 2009).[3] For the extract of survey respondents with a PIK, 96 percent match to a CPR record that provides at least county-level precision and 79 percent match to a Census tract and block location. Because the majority of

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[1] The ACS expanded its sampling by threefold in 2005, so the majority of ACS responses are from that year, even though only the first seven months are used.

[2] In less than 1 percent of cases, multiple responses may be matched to the same PIK. In this event, we randomly retain the PIK of only one respondent.

[3] The LEHD program uses residences provided in the CPR for imputations and as a place of residence for jobs data in the LEHD Origin-Destination Employment Statistics, available in the Web tool *OnTheMap*.

CPR records are sourced from the addresses on federal income-tax returns (which are typically filed in the first four months of the year), the 2005 locations are a good representation of pre-storm location. We limit the sample to survey respondents with both a PIK and an administrative residence location in 2005 that is precise to the county level or better.

We reweight survey responses based on the relative prevalence of demographic characteristics at the national level in 2005 and based on the likelihood of a person having a link to the CPR with county-level geography or better.[4] We use the new sample weights for computations reported in the paper, including summary statistics and regressions.

We then match the survey records, by PIK, to earnings records from the Longitudinal Employer-Household Dynamics (LEHD) program for jobs held between 2003 quarter 3 (also denoted as 2003:3, the first quarter with all of our treatment-area states available) and 2012 quarter 3 (2012:3).[5] The LEHD program produces a set of microdata Infrastructure Files using employment data provided by states along with federal administrative data and survey data (Abowd et al., 2009). The Bureau of Labor Statistics (BLS), which uses the same coverage frame for the Quarterly Census of Employment and Wages (QCEW), has described unemployment insurance (UI) covered jobs as accounting for "over 96 percent of total wage and salary civilian jobs" (BLS, 1997). In recent years, coverage was reported at more than 95 percent (BLS, 2017) and 97.3 percent (BLS, 2018). Sectors and worker classes not covered by the LEHD data include self-employment, the postal service, the armed forces, family workers, and some non-profit and agricultural workers (BLS, 2017).[6]

States that have joined the Local Employment Dynamics (LED) Partnership provide the Census Bureau with two employment files each quarter.[7] UI earnings records list the quarterly earnings of each worker from each of his or her employers. The LEHD program compiles the records as an Employment History File (EHF), with a record in the file for each job, identified by the combination of a worker (PIK) and employer, which is identified by a State Employer Identification Number (SEIN). We use the Person History File (PHF), which supplements the EHF information with the Unit-to-Worker (U2W) establishment assignments.[8] An SEIN may be

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[4] First, we estimate the number of 2005 persons that each survey respondent with a PIK in our age range represents (based on combinations of age, sex, and race/ethnicity categories). Then we estimate a logistic regression with the dependent variable indicating a match to a CPR residence at the county level or better and indicators for sex, age cohorts, and race/ethnicity as explanatory variables. Hispanics, younger respondents, and those with high school education or less are less likely to have a linked residence. We retain only the records with a PIK and linked residence, and we use the product of the inverse of the predicted retention probabilities from both reweighting schemes to reweight the remaining survey records. The resulting sample has very similar weighted characteristics as the original, unweighted extract.

[5] The Quarterly Workforce Indicators, produced from the LEHD data, first report earnings for Mississippi in 2003:3 and first report for Texas, Louisiana, and Alabama in 1995:1, 1995:1, and 2001:1, respectively.

[6] See Stevens (2007) for a discussion of coverage in UI earnings records for a set of states.

[7] We do not include data on federal workers, which were only available for LEHD from 2010 onward. All 50 states, the District of Columbia, Puerto Rico, and the Virgin Islands joined the LED Partnership by 2012. The time series of LEHD earnings records begins in 1985, but not all states provide data in every year. By 2003, there are data for 47 states. Jobs with earnings in Arizona and the District of Columbia were not available at the beginning of the series, but they are included in later years. Jobs with earnings in Massachusetts are not included in the study. These coverage issues should have only a small effect on our analysis because the treatment and control samples do not include any individuals whose 2005 residence was in Arizona, the District of Columbia, or Massachusetts. Because Mississippi first provided earnings records for 2003:3, that quarter is the first one used in the study.

[8] We combine the PHF files across states, identify highest-earning jobs, and account for successor/predecessor transitions using the Person History Enhanced Across SEIN and Non-SEIN Transitions (PHEASANT) process.

further linked to the Employer Characteristics File (ECF), which is produced from the same source data that employers submit to BLS for the QCEW. The employer file lists the industry, ownership, employment, and location of establishments.

To focus our study on workers with ties to the labor market covered by LEHD data, we require that survey respondents have a job spanning July 1, 2005 (the beginning of the quarter in which the storms occurred).[9] For that job (or the highest-earning one in 2005:2 if a worker had multiple such jobs), we link to the employer's industry (NAICS code) and establishment location.[10] We link over 90 percent of workers to a workplace Census tract or block, and approximately 99 percent are linked to a workplace county. We use the industry and workplace information to examine differential effects of the storms on workers, given their pre-storm employment.

In constructing the sample for our main analysis, the July 1 job restriction reduces the sample to 57.9 percent of all the survey respondents that link to LEHD earnings histories ever over the study period (after imposing the restrictions based on age and residence data). Workers eliminated from the sample by this earnings restriction may be employed, but out of scope. LEHD earnings include some high-earning records that can distort earnings measures in particular quarters. For this reason, and to focus on the earnings outcomes of typical workers, we topcode quarterly earnings levels to $500,000 (in 2005:2 dollars).

Average earnings (without any controls) for the treatment sample and the matched control sample before and after the storms are shown in Figure A5. Before the storms, average earnings is lower for the treatment sample than the control sample, but the difference in average earnings is fairly stable across quarters. In the aftermath of the storms, average earnings for the treatment sample fell relative to the control sample. However, the gap in average earnings between the treatment and control samples closed over time, and by the fourth year after the storms (2006:4) average earnings is larger in the treatment sample than in the control sample. Beyond that, average earnings is typically larger in the treatment sample.[11]

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[9] Using the LEHD data, we identify workers with earnings from the same employer in the adjacent quarters 2005:2 and 2005:3. The LEHD program uses this definition to tabulate beginning-of-quarter employment, with the reasoning that a worker with the same job in adjacent quarters is employed at the seam of those quarters. We use the Successor Predecessor File to span the adjacent quarters in cases where an employer identifier may have changed due to restructuring.

[10] We link earnings records by SEIN to the unit-level version of the Employer Characteristics File. For jobs at single-unit employers, the link is straightforward. For jobs at multi-unit employers, we use the Unit-to-Worker (U2W) imputation, applied by the LEHD program to assign establishments to workers when establishment assignments are unknown (for all states except Minnesota). The imputation assigns an establishment to a worker only if the establishment exists during the worker's tenure at the employer, and it uses establishment size and proximity to a worker's place of residence as explanatory factors, attempting to replicate the size distribution of establishments and the observed distribution of commute distances. We use the first of ten draws from the imputation model. In general, the use of imputed workplace data would be expected to attenuate any estimates relating to workplace-damage measures. For our linking, we use the U2W draws listed on a downstream version of the Employer History File, called the Person History File, which we also use for extracting earnings records.

[11] The general upward trend in quarterly earnings before the storm is due to our requirement that workers are employed at the same job in 2005:2 and 2005:3, which is associated with more weeks worked and longer job tenure in the vicinity of those quarters than earlier in the pre-storm period (when we do not require that individuals be employed). The long-run decline in average earnings for both samples is due to requiring that sample members be employed just prior to the storm (job spanning July 1, 2005) but not requiring that they be employed after the storm. The regression model we use to estimate earnings effects of the storms compares differences in changes across the

We define industries using 2007 NAICS Industry Sectors, as listed here by the first two digits of the code.

- Agriculture and resources: 11 and 21.
- Construction: 23.
- Manufacturing: 31-33.
- Leisure, Accommodations: 71, 72.
- Healthcare: 62.
- Professional services: 51–55.
- Local services: 44–45, 56, 81.
- Trade, Transportation, Utilities: 22, 42, 48–49.
- Public, Education: 61, 92.

9.2. Damage Data

We use two sources of damage data in the analysis. The U.S. Department of Housing and Urban Development (HUD, 2006) compiled the first source, which tabulates the number of occupied housing units in counties of Texas, Louisiana, Mississippi, and Alabama with storm damage to real and personal property. The damage assessments for Katrina and Rita were based on inspection of housing units to determine eligibility for Federal Emergency Management Agency (FEMA) housing assistance and were also referenced in requests to Congress for recovery funding (Richardson and Renner, 2007). The assessment avoided over-counting of household claims by including only records based on direct inspection or remote sensing of flood depth and by removing records that duplicate an address or report a second home. For our analysis, we use the share of housing units in a county with substantial damage, estimated as being in excess of $5,200.[12] Our defined treatment area consists of counties that include 99.0 percent of the occupied housing units with substantial damage. Figure A1 displays this county-level damage share for the set of 122 counties in these four states, with darker shading indicating counties with a greater share of damaged units. The darkest regions of the map are coastal areas in the vicinity of where Katrina (in eastern Louisiana and coastal Mississippi) and Rita (in western Louisiana) made landfall.

Our second source is more spatially detailed. Following Hurricanes Katrina and Rita, the Mapping and Analysis Center (MAC) at FEMA (2005) carried out a spatially detailed damage assessment, including 22 counties affected by the storms. MAC uses remote sensing and aerial reconnaissance (high-resolution satellites and airplane flyovers, including information from the National Geospatial-Intelligence Agency) as well as several tiers of ground-truth assessment (regional, neighborhood, and per-building) to map the extent and intensity of damage. Assessments of flooding are based on the observed extent of standing water from imaging data, and a time-series of images is used to establish the duration of flooding. MAC disseminates this analysis to responders throughout the federal government in the form of Geographic Information

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treatment and control samples relative to the baseline quarter, so the long-run decline is absorbed in the quarter effects.

[12] In HUD (2006), this is "major" (between $5,200 and $29,999) or "severe" ($30,000 or higher) damage. Shares are based on the total number of occupied housing units according to the 2000 Census. Using the same damage data, Groen and Polivka (2008) found that a 10-percentage-point increase in the share of housing units in a county with severe damage is associated with a 5.3-percentage-point rise in the unemployment rate among Hurricane Katrina evacuees.

Systems (GIS) shapefiles (Womble et al., 2006; Richardson and Renner, 2007; Klemas, 2009). See Figure A2 (Panels B and C) for examples of mapping reports provided by FEMA.

These 22 counties cover 72.5 percent of occupied housing units in our defined treatment area and 93.0 percent of those with substantial damage as indicated in the HUD (2006) data. This set includes the 10 counties with the highest rate of substantial damage and the 10 most populous counties in terms of occupied housing units. The counties not covered by FEMA (2005) tend to be further inland and have sporadic damage. As an alternative assessment of coverage, the American Red Cross carried out a subsequent ground analysis (see Gabe et al., 2005). In Mississippi, they found that counties not covered by the FEMA (2005) assessment contain approximately 25.8 percent of the total number of structures destroyed or experiencing major damage from Hurricane Katrina.[13] For comparison, the HUD (2006) data indicate that 14.4 percent of housing units in Mississippi with substantial damage were not assessed by FEMA (2005). Though they differ in methods, both assessments indicate that FEMA (2005) data cover the majority of significant damage to structures.

FEMA publicly released a series of shapefiles and map reports on damage and flooding that have been widely used for contemporary and retrospective analysis (Gabe et al., 2005; NLIHC, 2005; Logan, 2006; Solomon et al., 2006; Richardson and Renner, 2007; Jarmin and Miranda, 2009; Stevenson et al., 2010; Aydin et al., 2014; Basker and Miranda, 2018). Using these data, Solomon et al. (2006) found mold-spore concentrations in New Orleans to be roughly double in flooded areas, with the highest concentrations inside homes. Also using these data, Stevenson et al. (2010) found that before Katrina in the Gulf Coast, housing stock was highly correlated with the number of building permits issued in affected areas, though the correlation was lower in areas with the most severe damage.

This more-detailed, GIS-based measure indicates the degree and type of damage occurring in sub-county areas defined by sets of latitude and longitude coordinates. FEMA designated structures in the areas as having Limited Damage, Moderate Damage, Extensive Damage, or Catastrophic Damage or being Flooded, described as follows (see Gabe et al. [2005] for detailed descriptions of the areas affected by each type of damage):

- Limited Damage: Generally superficial damage to solid structures (e.g., the loss of tiles or roof shingles); some mobile homes and light structures are damaged or displaced.
- Moderate Damage: Solid structures sustain exterior damage (e.g., missing roofs or roof segments); some mobile homes and light structures are destroyed, and many are damaged or displaced.
- Extensive Damage: Some solid structures are destroyed, most sustain exterior damage (e.g., roofs are missing, interior walls are exposed); most mobile homes and light structures are destroyed.
- Catastrophic Damage: Most solid and all light or mobile structures are destroyed.
- Flooded area: Area under water.
- Undamaged: Areas not covered by the above categories.

[13] Gabe et al. (2005) report on a presentation by the American Red Cross as well as subsequent communications. For Mississippi, the Red Cross counts 4,609 dwellings destroyed outside of the FEMA (2005) area (out of 68,729 statewide) and 29,917 suffering major damage outside of the FEMA (2005) area (out of 65,237 statewide). The structures in these categories add up to 34,526 destroyed or damaged units in counties not assessed by FEMA (2005) out of 133,966 statewide, or 25.8 percent.

For our sub-county analysis, we define "major damage" areas as locations with Extensive or Catastrophic Damage as well as areas in and around New Orleans with flooding that persisted beyond September 10, 2005. We define "minor damage" areas as locations with Limited or Moderate Damage as well as areas with less-persistent flooding (including New Orleans areas where flooding receded by September 10, 2005).

FEMA released several vintages of sub-county damage mapping in 2005. For this study, we use three vintages of geographic files. For Hurricane Katrina, we use both the September 10 and September 11 files. For Hurricane Rita, we use the September 29 file.[14] We consider flooding in the September 10 and September 29 files to be minor damage and code the flooding in the September 11 file as major damage because only those locations had long-term flooding.

For illustrative purposes, Figure A3 displays maps of two affected areas by FEMA damage category, with red indicating major damage, dark blue indicating minor damage, and green indicating land areas with no specified damage. Panel A, which depicts the New Orleans area, shows mostly flooding damage, with minor damage in the areas where flooding receded quickly and major damage in the zones where it persisted. Panel B, which depicts the Gulf coast of Mississippi, shows mostly storm surge and wind damage, with catastrophic and extensive damage directly along the coast.

For the 22 surveyed counties with detailed damage data, we identify the set of Census blocks subject to either type of damage (major or minor) and assume that the most severe damage type applies to all addresses located within each block. We regard the remainder of blocks in the surveyed counties as having no damage. For blocks in the remainder of the 63-county treatment area, damage is uncertain but likely to be of lower frequency and intensity.

To implement this mapping, we use ArcMap 10.1 (ESRI software) to intersect the damage areas of these shape files (FEMA, 2005) with TIGER/Line shapefiles for Census 2000 tabulation blocks in our treatment counties.[15] A Census tract is a geographically compact and demographically homogeneous tabulation area with a target population of 4,000 residents, analogous to a neighborhood. Tracts consist of blocks, which are bounded by features such as streets, streams, and jurisdiction boundaries and often correspond with one or two city blocks in an urban area (there is no target population for a block, but there are typically dozens of blocks within a tract). Our residence addresses are geocoded to Census 2000 tabulation geography, while the workplace addresses are geocoded to Census 2010 tabulation geography. We use separate intersection files for each tabulation year to classify workers' residences and workplaces as damaged.

For the treatment sample, Table A2 gives the distribution of damage types associated with each worker, by 2005 residence block and workplace block. The top two rows indicate addresses with positive evidence of damage. Most instances of major damage are long-term flooding or Catastrophic Damage. Minor damage is split between short-term flooding and

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[14] Our GIS files for these snapshots have the following names: damage_10sep05_1000 (Sept. 10), katrina_receded_flooding_11sep05 (Sept. 11), and damage_29sep05_1000 (Sept. 29). FEMA released these files as events unfolded but does not maintain them or provide additional information on the creation of the files. Javier Miranda provided the copies used here based on the data used in Jarmin and Miranda (2009).

[15] Because addresses geocoded to Census blocks are already so spatially precise, we do not make a distinction of whether an address is located in the exact part of a block that intersects with the damage shape files. One concern with a coordinate-based measure is that some addresses can be geocoded to a street of a block but cannot be precisely located along the street. Another concern is that properties extend beyond the exact coordinates of an address. Furthermore, the exact extent of damage areas may be less certain than the shape files indicate.

Moderate and Limited Damage. The middle two rows indicate addresses where damage is possible but uncertain—due to either an imprecise residence or workplace address in a surveyed county or an address in a county not surveyed. All addresses for our sample are precise to at least the county level. The lower two rows indicate addresses with no damage, which were either in a surveyed county or outside the treatment area altogether (workplace only). Areas with no reported damage (shaded as green in Figure A3) also include sparsely populated areas that were not subject to structural damage (but may have had strong winds or flooding). Overall, 70 percent of residences and 58 percent of workplaces were within surveyed (FEMA, 2005) counties of the treatment area (the sum of rows one, two, three, and five in Table A2).

Figure A4 presents more-detailed views of the maps in Figure A3, overlaid with boundaries of Census blocks. Panel A shows downtown New Orleans, including the French Quarter. Panel B shows an area of Gulfport, Mississippi, including beachside resorts, residential housing, and shipping terminals. Census-block boundaries are often consistent with city streets, so the maps also provide a good indication of the infrastructure layout in these areas and provide a scale for the extent of damage to urban areas. For this study, any address in a block including any minor or major damage is assumed to be subject to that damage, with major damage taking precedence over minor damage.

Table A5 gives our estimates of the effects of each damage category on earnings in the short, medium, and long term. The table includes results from Table 2 in the main text plus three additional damage categories listed in Table A2: "uncertain," "none," and "outside treatment area" (for workplace damage only). We find that, compared with major and minor damage, earnings effects for these other statuses are generally more favorable and in line with the overall outcomes. Those with uncertain damage and those that were outside the treatment area had higher point estimates for earnings effects in all time periods relative to the "none" category. This difference may reflect that those in the "none" category were generally in counties that sustained more damage. Figure A7 gives the quarterly effects of damage to workplaces and complements Figure 3, which gives effects for damage to residences.

9.3. Treatment Group, Control Group, and Propensity-Score Model

The treatment group is defined as individuals who meet our employment criterion and resided, in 2005, in a county that experienced substantial damage from either Katrina or Rita. Specifically, the treatment area is the set of 63 counties (or parishes) with HUD (2006) reporting substantial damage or FEMA (2005) reporting any damage (see Figure A2, Panel A). The treatment counties (shown in Figure 1 in light shading), which stretch from Texas to Alabama, included 1.8 million occupied housing units, of which 278,957 (15.8 percent) had substantial damage.

We use a propensity-score methodology to identify a set of control counties with worker characteristics, earnings trends, and economic conditions similar to those of the treatment counties prior to the storms. The propensity-score model includes a subset of the variables in Table 1, omitting some variables that varied little across regions and combining some categorical values of others to increase statistical power. The primary source of the county-level characteristics for the propensity-score model is our matched survey-administrative worker data, including the requirement of continuous employment at a job from 2005:2 to 2005:3. We use these data to construct county-level means of variables for demographic characteristics (shares by race/ethnicity and educational attainment), industry composition (based on the pre-storm job),

and average quarterly earnings for each of the eight quarters from 2003:3 to 2005:2.[16] In the propensity-score model, agriculture and natural resources are separate categories because trends in energy prices may affect local areas differently depending on their employment shares in natural resources (Marchand, 2012). In the summary statistics in Table 1 and in our industry analysis, we combine agriculture and natural resources into a single category because there is a relatively small share of employment in each of these industries.

Given the cyclical dynamics of the 2000s, with a housing boom through 2006 and the Great Recession beginning in 2007, it is important that we match not only the population characteristics but also pre-storm economic conditions. Therefore, we include four additional county-level measures: (1) the percent of individuals who were employed just prior to the storms (defined using the same condition as our sample), (2) the unemployment rate in 2004, (3) the change in housing prices from 2000:2 to 2005:2, and (4) the change in total population from July 1, 2000, to July 1, 2005.

- Percent employed in 2005:2. This is the percent of individuals living in the county in 2005 who were continuously employed at a job from 2005:2 to 2005:3. The source of this measure is our matched survey-administrative worker data.
- Unemployment rate in 2004. The source of this measure is annual county-level estimates by BLS (Local Area Unemployment Statistics).
- Housing-price change from 2000:2 to 2005:2. This measure is based on Federal Housing Finance Agency (FHFA) All-Transactions House Price Indexes, which are derived from appraisal values and sales prices. These FHFA indexes are quarterly, not seasonally adjusted, and available for 401 metropolitan areas (or metropolitan divisions) and 47 nonmetropolitan balance-of-state areas. For counties located in metropolitan areas, we use the FHFA index for that metropolitan area (or metropolitan division). For other counties, we use the FHFA index for the relevant nonmetropolitan area. The symmetric and bounded measure of change we use is 100*(hpi2005 – hpi2000) / [(hpi2000 + hpi2005)/2], where hpi2000 and hpi2005 are the index values for 2000:2 and 2005:2, respectively.
- Population change from 2000 to 2005. This measure is based on Census Bureau population estimates at the county level, which have a reference date of July 1. The measure of change we use is 100*(p2005 – p2000) / [(p2000 + p2005)/2], where p2000 and p2005 are the population estimates for 2000 and 2005, respectively.

For the county-level dataset used to estimate our propensity-score model, we restrict the set of counties to the 63 counties in the treatment area and 2,393 other counties in the continental United States.[17] We estimate a logit model with a binary outcome, where counties in the treatment area have the indicator 1 and all other counties have the indicator 0. This method

[16] In calculating the means, we use sample weights indicating the count of persons in 2005 represented by each record. Industry shares are based on the highest-earning job held from 2005:2 to 2005:3.

[17] In defining the set of potential controls, we exclude all counties in Texas, Louisiana, Mississippi, and Alabama because these states include the treatment counties and we do not want our control group to capture geographic spillovers to areas adjacent to the treatment counties. We exclude all counties in Florida because it is adjacent to the treatment area and was affected by another 2005 hurricane (Wilma). We exclude Alaska, Hawaii, Puerto Rico, and the Washington DC metropolitan area because we are concerned about issues of seasonality and data completeness in those areas (the LEHD data does not include federal workers). We also exclude approximately 100 counties with fewer than 150 person records in the underlying survey data.

estimates the association between county characteristics and the treatment area.[18] The coefficient estimates are reported in Table A1. To select the control sample, we use the parameter estimates to predict (within sample) the probability that each county might be a treatment county. We sort the control candidates by propensity score in descending order and select the top 5 percent of counties using sample weights aggregated by county (so that counties representing 5 percent of the candidate sample population are chosen). Our control area includes 287 counties in 28 states.[19] Figure 1 displays the control counties (in dark shading), which are concentrated in the coastal Southeast and Mid-Atlantic, Appalachia, and along the Mississippi river, with a scattering across northern Michigan, the Great Plains, and western mountain regions.

9.4. Control Suitability

While it is apparent from an inspection of Table 1 that the matched control sample improves upon the potential control sample in terms of alignment with the treatment sample, we use standardized differences to quantify the improvement and to facilitate comparisons across characteristics. Table A11 presents the characteristics of the treatment sample, the matched control sample, and three alternate control samples. Figure A10 depicts the geographic areas used to define the three alternate control samples (described in Section 9.8). To quantify the dissimilarity of each control sample from the treatment sample, Table A12 presents a measure of how each of the control samples diverge from the treatment sample, both in the aggregate and by characteristics (each defined by a single variable or a grouping of related variables).

The standardized difference (see Austin, 2009; Imbens and Rubin, 2015) of any variable that is continuous at the person level (e.g., earnings, county population change from 2000 to 2005, county unemployment rate in 2004) is calculated as

$$d(\text{continuous}) = \frac{\bar{x}_{treatment} - \bar{x}_{control}}{\sqrt{\frac{1}{2}\cdot(s^2_{treatment} + s^2_{control})}} \ ,$$

where $\bar{x}$ is the sample mean and s^2 is the sample variance. We calculate the sample mean and variance across persons in the sample, using sample weights. Note that some characteristics, such as population change, are common to all persons in the same county. The standardized difference for a categorical variable, defined as one or more dichotomous, indicator variables (e.g., female, age bins, race/ethnicity categories), is calculated as

$$d(\text{dichotomous}) = \frac{\hat{p}_{treatment} - \hat{p}_{control}}{\sqrt{\frac{1}{2}\cdot(\hat{p}_{treatment}(1-\hat{p}_{treatment}) + \hat{p}_{control}(1-\hat{p}_{control}))}} \ ,$$

where $\hat{p}$ is the prevalence (or mean) of a categorical variable with a value between zero and one.

We compute an index of the standardized differences, a Root Mean Squared Error (RMSE), for each characteristic as

$$RMSE(k) = 100 \cdot \sqrt{\frac{1}{M_k}\sum_{m=1}^{M_k}(d(k_m))^2} \ ,$$

where k is a characteristic consisting of M_k continuous or indicator variables, indexed $m = 1$ to M_k. For measuring divergence, we treat the eight pre-storm quarterly-earnings variables as a

[18] In the logit model, we use the sample weights (aggregated by county) so that counties with a larger sample population have a greater effect on the estimates.

[19] Each county includes at least 150 person records in the sample, with a median of approximately 650, and the largest state accounts for approximately 20 percent of the control-sample records.

single characteristic, with equal weight on each quarter. The index is always positive and treats each of the M_k components with equal weight. For an aggregate divergence measure for all characteristics combined, we index the characteristics by $k = 1$ to K, assign equal weight to each characteristic, and compute the integrated index (RIMSE) as:

$$RIMSE = 100 \cdot \sqrt{\frac{1}{K}\sum_{k=1}^{K}[\frac{1}{M_k}\sum_{m=1}^{M_k}(d(k_m))^2]} \ .$$

The first row of Table A12 presents the integrated index, giving a divergence index of 25.82 for the potential control sample and 7.57 for the matched control sample. This drop in the index confirms that the matching process provides a control sample that is more similar to the treatment sample. The matched control sample also has a lower divergence index than each of the alternate control samples (see Section 9.8). The matched control sample improves on the potential control sample on almost every characteristic, with the exceptions of sex and population change from 2000 to 2005. The biggest improvements are for race/ethnicity and housing-price change.

9.5. Effects by Worker Subgroup

Table 3 examines results for subgroups defined by pre-storm industry; in Table A9 we provide additional results for subgroups based on pre-storm earnings and attachment to employment as well as demographic characteristics. We find a similar pattern of earnings loss and recovery across all pre-storm earnings subgroups. As such, we find no indication that our results are driven by differential financial exposure before the storms, risk-mitigation efforts that may be tied to pre-storm earnings, or by divergent earnings paths by pre-storm earnings strata. Regarding steady employment in the two years before the storms, Table A9 breaks out results for various levels of attachment for our sample of workers who were employed at the time of the storms (see Section 9.1). Using the LEHD quarterly earnings data, we define indicators for whether a worker was employed in all eight pre-storm quarters and at the same employer in all eight of those quarters. Given the employment requirement for our sample, it is not surprising that 78 percent are steadily employed and 62 percent have at least two years of tenure. Our results for subgroups defined by these two indicators, as well as a combination of the two, indicate that the less attached have lower short-term losses and higher long-term gains.

In terms of differences by demographic groups (Table A10), our estimates of short-term earnings losses are larger for those who had college degrees (-6.2%) than for those who had less education (close to zero for those with high school or less). In addition, those with less education had stronger earnings gains in the medium and long term. For instance, workers with less than a high school education at the time of the storms experienced a long-term earnings gain of 14.2 percent.

Our estimates by gender indicate that the earnings effects of the storms were worse for women than men. In particular, short-term earnings losses were larger for women (-5.5%, compared to -1.6% for men) and long-term earnings gains were smaller for women (3.9%, compared to 10.5% for men). Our estimates by race indicate that the earnings effects of the storms were worse for blacks than whites, especially in the short term. The short-term effects were -6.8 percent for blacks and -2.2 percent for whites. Further, although blacks experienced earnings gains in the medium and long term, whites gained more. The long-term effects were 6.0 percent for blacks and 8.5 percent for whites.

9.6. Job Separations and Migration

Figure 4 presents results on how earnings effects vary by job-separation status in the first year after the storms. Here, we present estimates showing how damage to workplaces and residences affected the likelihood of job separation and migration, as well as estimates of how earnings outcomes vary by job separation and migration status. Note that short-term job separations are not one-in-the-same with migration; of short-term movers (to another commuting zone), only 38.8 percent separated; of separators, only 28.1 percent moved.[20]

We first present additional results on job separation. Recall that the earnings losses over the first year after the storms are primarily the result of reductions in earnings due to shifts from employment to non-employment. Therefore, we investigate specifically the earnings effects for those who separated from their pre-storm employer. For individuals in the treatment sample, we define a job separation as the loss of earnings from one's main, pre-storm employer for at least the first four quarters after the storms (though one could have earnings from other secondary or new jobs).[21] Based on a regression of an indicator variable for job separation on controls for demographic variables, we find that those whose employer experienced workplace damage were more likely to separate (Table A6).[22]

When we split the treatment sample into separators and non-separators and estimate earnings effects relative to the control sample, we find that separators experienced much larger earnings losses in the short term (Figure 4 and Table A7). The estimated earnings losses for the separators lasted through the third year after the storms, but the earnings of separators and non-separators converged; by the seventh year after the storms the separators experienced earnings gains that are similar to those of the non-separators. The larger short-term losses for separators may reflect loss of specific skills or difficulty finding new employment. Notably, the earnings losses of separators do not last as long as the losses typically experienced by displaced workers (five years or more) (Jacobson et al., 1993; Fallick, 1996). This faster recovery could reflect that unlike typical displaced workers many of those who were separated due to the storms lost their jobs for reasons unrelated to a declining demand for their specific job skills and the economic activity that arose in the wake of the storms provided opportunities not available to typical displaced workers.

We also investigate how the earnings effects of the storms vary with migration status over the first year after the storms. We make this distinction because the effects could be different for those who migrate and those who remain in storm-affected areas. Conceptually, examining earnings effects by migration status is potentially more complicated than examining earnings effects by demographic characteristics because migration itself can be considered a response to differences in post-storm relative earnings between regions of the country. Rather than examining migration and earnings jointly over the entire time period of our study, we keep our focus on earnings as the outcome of interest and define migration based on the initial response to the storms. As with job separation, migration in the immediate aftermath of the storms is more likely to be a direct result of the storms and is less likely to be an endogenous response to differences in earnings potential.

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[20] For the treatment sample, 3.1 percent both moved and separated, 4.9 percent only moved, 7.9 percent only separated, and 84.1 percent neither moved nor separated.

[21] The four-quarter requirement avoids counting near-term recalls and seasonal jobs as separations. The separation rate in the treatment sample was 11.9 percent.

[22] Separately, Jarmin and Miranda (2009) found a greater decline in payroll in areas with more workplace damage and that this decline was largely explained by business closures.

Table A3 presents summary statistics on migration that confirms the well-known movement of people away from storm-affected areas. Making use of the longitudinal place-of-residence data, we measure residential mobility (or the migration rate) as the share of each sample (treatment and control) living in a different commuting zone in a given year than in 2005.[23] For Table A3, we limit the sample to workers with an observed residence location at the county level or better in each year from 2003 to 2010, which reduces the sample by about 10 percent. Prior to the storms, the individuals in the matched control sample had a slightly larger propensity to migrate, with 4.5 percent residing in a different commuting zone in 2004 and 2005, compared to 3.0 percent in the treatment sample. After the storms, migration was greater for the treatment sample. The share of the treatment sample that changed locations between 2005 and 2006 was over twice the share of the control sample that did so. However, after 2006 the relative excess in the migration rate for the treatment sample diminishes; this easing coincides with return migration among some of those in the treatment sample that moved away from their 2005 locations in the aftermath of the storms (Groen and Polivka, 2010) as well as a higher baseline migration rate (both in- and out-migration) in the control area. The patterns are qualitatively similar using states or counties instead of commuting zones to measure locations. In terms of the treatment area overall, as of 2010, 10.8 percent of the treatment sample had left the treatment area entirely.[24]

For our regression analysis, we define short-term migration as relocating to a different commuting zone from 2005 to 2006. Note that the non-mover group contains individuals who may have temporarily evacuated from their 2005 location after the storms but returned as of 2006. Among movers, 23.1 percent had major residence damage, compared with 4.0 percent among non-movers. Looked at another way, residence damage appears to be a strong factor in the decision to migrate from the affected area. Similar to the case of job separation being associated with workplace damage (see Table A6), we find that those who experienced greater residence damage were more likely to move between 2005 and 2006. Specifically, those who experienced major residence damage were 21 percentage points more likely to move between 2005 and 2006 than were those who experienced no residence damage.

We split the treatment sample into movers and non-movers and estimate earnings effects by comparing each group to the control sample as a whole. Our estimates of earnings effects (Figure A8 and Table A7) indicate that movers experienced much larger earnings losses in the short term, potentially due to difficulty adjusting to their new areas. Over the first year after the storms, the estimated earnings losses for movers are about $1,565 per quarter (-15.8%). Larger earnings losses for movers are consistent with prior research on Katrina evacuees that compared those who relocated over the first year after the storm with those who did not (Vigdor, 2007; Groen and Polivka, 2008b).[25] After the short term, the earnings of movers and non-movers converged; in the long term, we estimate that both movers and non-movers experienced earnings gains. Over the entire post-storm period aggregated, movers experienced essentially no net

[23] Commuting zones are sets of counties that are related by commuting ties. They encompass all metropolitan and nonmetropolitan areas in the United States, and they are sensible units for defining local labor markets (Tolbert and Sizer, 1996; Autor, Dorn, and Hanson, 2013). We use the commuting zones based on the 2000 Census.

[24] Of those in the treatment sample, the share residing outside of the treatment area was 2.9 percent in 2004, 0 percent in 2005 (by definition), 7.1 percent in 2006, 9.4 percent in 2008, and 10.8 percent in 2010.

[25] The substantial amount of time it took for movers to experience an increase in earnings is consistent with Vidgor's (2007) prediction that if people were going to obtain greater economic prosperity by moving from New Orleans the process would take several years.

change in earnings (-$13.6/quarter [-0.1%]) whereas non-movers experienced a net increase ($454/quarter [4.6%]).

9.7. Employment, Wages, and Other Measures in Local Labor Markets

We construct estimates of quarterly employment totals (overall and by industry sector) for the treatment and control areas following the tabulation methods used in the Quarterly Workforce Indicators (QWI), a Census Bureau public-use data product that is derived from LEHD data. QWI includes local labor-market indicators of employment, earnings, hires, separations, turnover, and net employment growth. Confidentiality-protection methods, described in Abowd et al. (2009), allow the Census Bureau to release these data in cells defined by employer industry, ownership, and location and by worker characteristics with minimal suppression. Our study makes use of beginning-of-quarter employment, a point-in-time indicator of the count of jobs that had earnings records in two consecutive quarters. The logic of this employment measure is that a worker holding a job in both quarters was most likely employed there at the seam of the quarters (e.g., April 1 is the seam between the first and second quarters). In contrast, an employment measure that included all jobs held in a quarter would over-estimate employment at a point in time because some jobs are held one after the other.

Although it would be possible to construct aggregations of employment for the sets of counties in the treatment and control areas using the pubic-use QWI, there would be some undercount of employment due to suppression of some cells that do not meet Census Bureau publication standards. The undercount would be due to individual counties (or county-by-industry cells) having fewer than three persons or establishments. In addition, the noise infusion for some small cells may result in excessive distortion.

Therefore, to provide a more-accurate representation of aggregate employment in treatment and control areas, we produce custom QWI tabulations where the suppression and distortion issues are not binding. We produce quarterly tabulations of employment in the treatment and control areas using confidentiality protection and suppression rules identical to those used in the QWI. By aggregating the county lists of the two areas, each as a single cell, we avoid the small-cell issues that can occur in single-county tabulations.

Our estimates of average hourly wages in the treatment and control areas over time are derived from the Occupational Employment Statistics (OES) survey. The OES survey, which is a cooperative effort between BLS and the State Workforce Agencies, is a semiannual mail survey measuring occupational employment and wage rates for wage-and-salary workers in nonfarm establishments. In the survey, establishments classify their employment by occupation and wage category. OES estimates are constructed from a sample of about 1.2 million establishments.

Each year, survey forms are mailed to two semiannual panels of approximately 200,000 sampled establishments, one panel in May and the other in November. Estimates for a given reference month are based on data collected from six semiannual panels over a three-year period ending in that month. In order to have wage estimates reflect current conditions, wages in the five previous panels are updated to the reference month using movements in occupational wages over time as measured by the BLS Employment Cost Index.

The starting point for our OES analysis is public-use estimates of average wages by metropolitan area for May 2005, May 2008, and May 2012. Estimates are available for each of 22 major occupation groups (e.g., management, sales, and production) and the total over all occupations. The May 2005 estimates are based on data collected between November 2002 and

May 2005. The May 2008 estimates are based on data collected from November 2005 to May 2008. The May 2012 estimates are based on data collected from November 2009 to May 2012. We use the Consumer Price Index to put all estimates of average wages in 2005:2 dollars.

We use the metropolitan-area estimates to construct estimates for the treatment and control areas. According to the definitions of metropolitan areas (MSAs), 31 of the treatment counties and 92 of the control counties are in metropolitan areas. There are 11 MSAs containing at least one treatment county and 49 MSAs containing at least one control county. These counties represent a large share of employment in the treatment and control areas. In 2004, the 31 treatment counties in the OES analysis account for 80 percent of employment in the 63 treatment counties. The 92 control counties in the OES analysis account for 77 percent of employment in the 287 control counties.

When we aggregate estimates at the MSA level to estimates for treatment and control areas, we weight by MSA employment in the treatment/control counties. The OES estimates provide employment counts for the entire MSA (by occupation), and we rescale these counts by the share of employment in each MSA that is in treatment/control counties. We derive these shares using county employment from the QCEW for the calendar year preceding each OES reference month (e.g., calendar 2004 in QCEW for May 2005 in OES). QCEW employment for a given year is defined for this analysis as the average of employment for March, June, September, and December.

These procedures provide estimates of average wages by occupation for the treatment and control areas over time. To construct estimates of average wages by industry for the treatment and control areas, we make use of OES national estimates of employment by industry and occupation for each of the three time periods. These estimates allow us to construct, for each time period and industry sector, the share of employment that is in each occupation. We then use these shares as weights for the occupational wage estimates in order to construct industry wage estimates. Specifically, the industry wage for a given area (treatment or control) is a weighted average of the occupational wage estimates, with the weights being the share of industry employment in each occupation. Figure A9 presents, by industry sector, the correspondence of OES industry wage growth in the treatment area, relative to the control area, from 2005 to 2012 with estimates from our individual-level analysis of earnings growth for the treatment sample, relative to the control sample, from 2005 to 2012.

One indicator of demand for construction work is the issuance of residential building permits. The Census Bureau creates statistics on residential building permits, including annual totals by county for buildings, units, and value. We focus on the quantity of units, which is likely to apply equally to urban, suburban, and rural areas (building sizes may differ). We use data from 1995 to 2013. The Census Bureau surveys local authorities on permit activity for new construction and renovations and imputes data based on local trends in the event of non-response in a particular year. Because some counties have never responded or do not issue permits, we focus on longitudinal changes among counties in the treatment and control areas that had permit estimates in every year (including all counties in the treatment area and all but seven in the control area). Permitting rose in both the treatment and control areas from 1995 to 2004 (including the core years of the nationwide housing boom) but leveled off in the treatment area in 2005, the year of the storms. From 2006 onward, as permitting declined nationally, the treatment area outpaced the control area with a more moderate decline—approximately two-thirds that of the control area.

As noted by Basker and Miranda (2018), passenger-arrival data from the Bureau of Transportation Statistics provide some indication of changes in tourism demand.[26] All airports in the affected region (New Orleans, Gulfport-Biloxi, Lake Charles, and Beaumont) experienced large drops in traffic in the months following the storms, with October 2005 arrivals in New Orleans and Gulfport-Biloxi down 79 percent and 38 percent, respectively, compared to a year earlier (T-100 domestic-market passenger totals, all U.S. and foreign carriers, domestic and international arrivals). Although traffic at Gulfport-Biloxi and the other smaller airports recovered quickly, arrivals at New Orleans remained 18 percent lower in 2008 (compared to 2004) and 11 percent lower in 2012. Overall U.S. arrivals at major airports grew by 7 percent in 2008 and 10 percent in 2012, compared to 2004.

An indicator of local demand for services is the number of students enrolled in public elementary and secondary schools. The National Center for Education Statistics provides the annual count of students enrolled in each public elementary and secondary school in the Common Core of Data. We use the data for 2002 to 2012, aggregate to the county level, and summarize for the treatment and control areas. The number of students enrolled fell by over 10 percent in the treatment area from 2004 to 2005 (whereas enrollment increased slightly in the control area). Enrollment at schools in the treatment area gradually recovered after 2005, but enrollment in 2012 as a percent of 2004 enrollment was lower in the treatment area by 3.4 percentage points.

9.8. Alternate Control Samples

Although the matched control sample is very similar to the treatment sample in terms of worker characteristics and local economic conditions before the storms, we consider alternate control samples to gauge the robustness of our main results. The alternate control samples have some desirable features, though they are less similar to the treatment sample (along those dimensions) than is the matched control sample. Each of the three alternate control samples is composed of individuals who resided in particular geographic areas in 2005 and had a job that spanned July 1, 2005. The geographic areas used to define the three alternate control samples are shown in Figure A10. Table A11 provides summary statistics on the alternate control samples and Table A12 provides measures of divergence between each alternate control sample and the treatment sample.

Our first alternate control sample is defined using a region along the Atlantic Coastal Plain. We use a definition of coastal counties developed by the National Oceanic and Atmospheric Administration (2013) to designate a region of 117 counties (or county equivalents) in the Atlantic watershed in Virginia, North Carolina, Georgia, South Carolina, and Florida. A desirable attribute of the Coastal Plain, as a control area, is its susceptibility to hurricanes (though it experienced no major storms during our analysis period).[27] Being in the South and consisting of low-lying coastal plains, the area also has demographic and economic characteristics that are broadly similar to those of the treatment area. The Coastal Plain sample includes 229,000 workers.

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[26] See BTS TranStats, https://www.transtats.bts.gov/DL_SelectFields.asp?Table_ID=258 (accessed April 17, 2017).

[27] Notable hurricanes that struck the southern Atlantic coast during the 2003–2012 analysis period were Isabel (2003), Charley (2004), Irene (2009), and Sandy (2012). For the Gulf Coast, notable hurricanes that struck the areas affected by Katrina and Rita were Ivan (2004), Dennis (2005), Gustav (2008), Ike (2008), and Isaac (2012).

The second alternate control sample we construct is formed by individuals whose 2005 residence was in Oklahoma, Arkansas, and Tennessee, which together form a region adjacent to the states that contain the treatment areas. We refer to this control sample as the Upland South sample, following the term for the geographical region that includes these three states. The Upland South is used as a control area because, being adjacent to states that contain the treatment area, it is anticipated that this region would have a relatively similar economy. The Upland South sample includes 438,000 workers.

The third alternate control sample is based on a set of economically weak metropolitan areas identified in a Brookings Institution report (Vey, 2007). These metropolitan areas consist mostly of older industrial cities that had low performance on a set of eight economic indicators (including employment growth from 1990 to 2000 and per-capita income in 2000). The Brookings report identified 65 cities that were weak according to the indicators, and the vast majority (46) of these cities were situated in metropolitan areas that were also considered weak (see page 18 of the report). This set of metropolitan areas, which is the starting point for this control area, includes two areas that were affected by Katrina or Rita: New Orleans and Beaumont-Port Arthur, Texas.[28] Use of a Weak City control area will reflect economies that presumably were on a similar trajectory as these two metropolitan areas in the treatment area. When forming this control sample, we first exclude these two metropolitan areas and then refine the list by excluding areas in any of the states used to define our treatment sample or other alternate control samples. The list used to define this alternate control sample contains 95 counties that include 30 weak cities. As shown in Figure A10, these counties are primarily in the Midwest and Northeast. The Weak Cities sample includes 1,120,000 workers.

According to the summary statistics in Table A11, each alternate control sample is similar to the treatment sample in terms of some characteristics, but overall the alternate control samples are not as close to the treatment sample as is the matched control sample (see Section 9.4 and Table A12). Figure A11 shows estimates of effects on earnings using the alternate control samples; for comparison, the figure also includes estimates using the matched control sample (from Figure A6). The time pattern of estimates we obtain with the alternate control samples is qualitatively similar to pattern obtained with the matched control sample. With the alternate controls, the estimates of short-term earnings losses are in the range of $200–$300 per quarter and the estimates of long-term earnings gains are in the range of $450–$850 per quarter.

[28] The Brookings list of "weak city" metropolitan areas was used as a basis of comparison for New Orleans in terms of its post-Katrina trends on a number of economic and social indicators by the New Orleans Community Data Center (Plyer et al., 2013).

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Table A1. Propensity-Score Model for Constructing the Matched Control Sample

Variable	Coef.	Std. Err.
White, not Hispanic	0.000	0.026
Black, not Hispanic	0.089*	0.026
Hispanic or (Other race, not Hispanic)	--	
Less than high school	0.213*	0.089
High school or Some college	--	
College	-0.071	0.063
Agriculture	-0.195	0.266
Natural resources	0.139	0.103
Construction	0.560*	0.130
Manufacturing	-0.003	0.073
Leisure, Accommodations	0.227*	0.078
Healthcare	0.187*	0.081
Professional services	-0.029	0.087
Local services	-0.033	0.096
Trade, Transport, Utilities	0.039	0.100
Public, Education	--	
Earnings 2003:3	4.738*	1.110
Earnings 2003:4	-1.597*	0.809
Earnings 2004:1	-1.621	1.045
Earnings 2004:2	-1.675	0.957
Earnings 2004:3	-2.356*	1.109
Earnings 2004:4	0.184	0.545
Earnings 2005:1	1.676*	0.688
Earnings 2005:2	1.615	0.900
Percent employed, 2005:2	-0.283*	0.065
Unemployment rate, 2004	-0.026	0.187
Housing-price change, 2000:2–2005:2	-0.168*	0.037
Population change, 2000–2005	0.044	0.044
Constant term	-1.915	5.893

Note: Table shows estimated coefficients and standard errors from a logit model with the dependent variable being an indicator for a county being a treatment county. Number of observations is 2,456. Counties are weighted by the sum of the sample weights across individuals employed in 2005:2. For the model, the variables for race, education, industry, and share employed are percentages (0 to 100) and the earnings variables are coded in thousands of dollars ($2005:2). Housing-price change and population change are rates of change (see Section 9.3 for definitions).
* $p<0.05$.

Table A2. Damage Incidence by Residence and Workplace (in percent)

Type of Damage	Residence	Workplace
Major	5.6	7.1
Minor	12.2	18.3
Imprecise address in surveyed county (Uncertain)	10.8	3.5
County not surveyed (Uncertain)	29.9	19.3
No damage for precise address in surveyed county (None)	41.6	29.1
Outside treatment area	N.A.	22.9

Note: See Section 9.2. Residence and workplace determined by 2005 locations. Residence location is from the linked CPR address. Workplace location is from the Employer Characteristics File, linked to the earnings record at the time of the storms in the Employment History File. Damage labels in parentheses correspond to the labels in Table A5. See Section 9.2 for a description of the FEMA (2005) damage data.

Table A3. Migration Outcomes (percent in different location than 2005)

	County			Commuting Zone			State		
Year	T	C	T – C	T	C	T – C	T	C	T – C
2004	5.8	7.3	-1.5	3.0	4.5	-1.5	1.7	2.9	-1.2
2005	0	0	0	0	0	0	0	0	0
2006	11.3	6.0	5.3	8.0	3.5	4.5	5.6	2.2	3.4
2008	15.9	12.9	3.0	10.4	8.1	2.3	7.0	5.1	1.9
2010	18.3	16.7	1.6	11.8	10.6	1.2	7.7	6.8	0.9

Note: T=treatment sample, C=control sample. Migration is defined as having a residence (per the CPR address) in a different location (county, commuting zone, or state) in the given year than in 2005. We use year 2000 commuting zones (Tolbert and Sizer, 1996). For 2004, values indicate the percent in-migration to a 2005 residence. For 2006, 2008, and 2010, values indicate the percent out-migrating from a 2005 residence. Sample is limited to records with a linked residence location of at least county-level precision for all years 2003-2010 (about 90% of the treatment and control samples).

Table A4. Decomposition of Effects of Earnings

	Effects by Time Period after the Storms			
Channel	Short	Medium	Long	Full
Total	-298.4	343.0*	792.3*	403.8*
	(191.4)	(125.4)	(141.8)	(127.9)
	[-3.0]	[3.5]	[8.0]	[4.1]
Within employment	-10.0	441.9*	666.3*	435.4*
	(137.4)	(136.2)	(182.0)	(145.1)
	[-0.1]	[4.5]	[6.7]	[4.4]
To non-employment	-288.4*	-99.0	126.0	-31.6
	(108.4)	(85.2)	(92.4)	(84.8)
	[-2.9]	[-1.0]	[1.3]	[-0.3]

Note: The estimates for each row are based on separate regressions. Numbers in parentheses are standard errors clustered at the county level (based on 2005 residence location). Numbers in brackets are effects as a percent of average earnings in 2005:2 for the treatment sample as a whole. Short term is 2005:4–2006:3 (k=1–4), medium term is 2007:4–2008:3 (k=9–12), long term is 2011:4–2012:3 (k=25–28), and full is 2005:4–2012:3 (k=1–28).
* $p<0.05$.

Table A5. Effects on Earnings, Overall and by Damage Type

	Effects by Time Period after the Storms			
Model	Short	Medium	Long	Full
Overall	-298.4	343.0*	792.3*	403.8*
	(191.4)	(125.4)	(141.8)	(127.9)
	[-3.0]	[3.5]	[8.0]	[4.1]
Residence Damage				
Major	-1,710.3*	-302.6	535.2*	-295.8*
	(322.2)	(159.8)	(181.0)	(141.7)
	[-17.2]	[-3.1]	[5.4]	[-3.0]
Minor	-631.9*	295.0*	772.1*	258.8
	(167.9)	(120.3)	(258.9)	(134.9)
	[-6.4]	[3.0]	[7.8]	[2.6]
Uncertain	-59.1	433.1*	822.7*	505.5*
	(122.3)	(114.9)	(159.1)	(118.8)
	[-0.6]	[4.4]	[8.3]	[5.1]
None	-245.4	355.5*	802.9*	440.6*
	(210.0)	(157.2)	(158.3)	(149.2)
	[-2.5]	[3.6]	[8.1]	[4.4]
Workplace Damage				
Major	-1,444.0*	-2.7	835.9*	41.4
	(252.9)	(121.9)	(226.5)	(129.1)
	[-14.6]	[0.0]	[8.4]	[0.4]
Minor	-755.3*	286.1	754.7*	218.7
	(298.2)	(171.9)	(189.6)	(181.6)
	[-7.6]	[2.9]	[7.6]	[2.2]
Uncertain	-40.9	432.0*	928.6*	573.6*
	(124.7)	(123.6)	(172.7)	(124.4)
	[-0.4]	[4.4]	[9.4]	[5.8]
None	-214.8	232.4	629.0*	323.6*
	(192.1)	(174.8)	(157.7)	(135.7)
	[-2.2]	[2.3]	[6.3]	[3.3]
Outside treatment area	57.5	546.8*	880.8*	596.4
	(144.5)	(163.2)	(171.3)	(149.8)
	[0.6]	[5.5]	[8.9]	[6.0]

Note: See Section 9.2. The estimates for overall treatment effects, residence damage, and workplace damage are based on separate regressions. Numbers in parentheses are standard errors clustered at the county level (based on 2005 residence location). Numbers in brackets are effects as a percent of average earnings in 2005:2 for the treatment sample as a whole. Short term is 2005:4–2006:3 (k=1–4), medium term is 2007:4–2008:3 (k=9–12), long term is 2011:4–2012:3 (k=25–28), and full is 2005:4–2012:3 (k=1–28).
* $p<0.05$.

Table A6. Effect of Damage on Migration and Job Separations

	Migration			Job Separations		
	(1)	(2)	(3)	(4)	(5)	(6)
Residence Damage						
Major	0.2418*		0.2142*	0.1232*		0.0879*
	(0.0105)		(0.0109)	(0.0086)		(0.0089)
Minor	0.0375*		0.0220*	0.0390*		0.0180*
	(0.0059)		(0.0061)	(0.0059)		(0.0059)
Uncertain	-0.0260*		-0.0176*	-0.0185*		-0.0028
	(0.0046)		(0.0049)	(0.0044)		(0.0048)
None	--		--	--		--
Workplace Damage						
Major		0.1318*	0.0854*		0.1210*	0.1016*
		(0.0115)	(0.0073)		(0.0199)	(0.0076)
Minor		0.0878*	0.0623*		0.0795*	0.0680*
		(0.0110)	(0.0052)		(0.0299)	(0.0050)
Uncertain		-0.0127*	0.0019		-0.0166*	-0.0124*
		(0.0047)	(0.0045)		(0.0050)	(0.0054)
None		--	--		--	--
Outside treatment area		0.0350*	0.0405*		0.0095	0.0111*
		(0.0044)	(0.0045)		(0.0051)	(0.0048)
Demographic controls	X	X	X	X	X	X
Job controls				X	X	X
Individuals	123,000	123,000	123,000	138,000	138,000	138,000
R-squared	0.0772	0.0561	0.0875	0.0964	0.1023	0.1059
Mean of Dep. Var.	0.0797	0.0797	0.0797	0.119	0.119	0.119

Note: Estimation sample is individuals in the treatment sample. Each column comes from a separate regression. Dependent variable for columns 1–3 is an indicator for living in a different commuting zone in 2005 and 2006; dependent variable for columns 4–6 is an indicator for not working for the pre-storm employer in the first four quarters after the storms. Standard errors, in parentheses, account for clustering by residence block (columns 1, 3, 4, and 6) or workplace block (columns 2 and 5). Demographic controls: age, sex, and race/ethnicity. Job controls: industry, employer size, and employee tenure. The sample size for columns 1–3 is smaller than the sample size for columns 4–6 because the sample for the migration regressions is limited to records with a linked residence location of at least county-level precision for all years 2003-2010 (about 90% of the treatment and control samples).
* $p<0.05$.

Table A7. Effects on Earnings by Subgroups based on Migration or Job Separation

	Effects by Time Period after the Storms			
	Short	Medium	Long	Full
Migration				
All	-307.4	368.8*	805.6*	417.1*
	(198.4)	(132.2)	(149.7)	(135.0)
	[-3.1]	[3.7]	[8.1]	[4.2]
Movers	-1,564.7*	6.0	784.2*	-13.6
	(444.5)	(245.2)	(254.3)	(269.2)
	[-15.8]	[0.1]	[7.9]	[-0.1]
Non-movers	-198.5	400.2*	807.4*	454.4*
	(165.0)	(128.6)	(152.3)	(129.2)
	[-2.0]	[4.0]	[8.1]	[4.6]
Job Separation				
All	-298.4	343.0*	792.3*	403.8*
	(191.4)	(125.4)	(141.8)	(127.9)
	[-3.0]	[3.5]	[8.0]	[4.1]
Separators	-2,083.6*	-268.8*	713.3*	-237.6
	(255.0)	(136.3)	(187.0)	(173.0)
	[-21.0]	[-2.7]	[7.2]	[-2.4]
Non-separators	-57.4	425.6*	803.0*	490.4*
	(158.6)	(121.1)	(146.2)	(120.8)
	[-0.6]	[4.3]	[8.1]	[4.9]

Note: Movers are those in the treatment sample who were in a different commuting zone in 2005 and 2006; non-movers are the remainder of the treatment sample. Separators are those in the treatment sample who were not working for their pre-storm employer in the first four quarters after the storms; non-separators are the remainder of the treatment sample. Numbers in parentheses are standard errors clustered at the county level (based on 2005 residence location). Numbers in brackets are effects as a percent of average earnings in 2005:2 for the treatment sample as a whole. Short term is 2005:4–2006:3 (k=1–4), medium term is 2007:4–2008:3 (k=9–12), long term is 2011:4–2012:3 (k=25–28), and full is 2005:4–2012:3 (k=1–28).
* $p<0.05$.

Table A8. Distribution of Treatment and Control Samples across Industries

	Treatment				Control			
Industry	2005:2	2006:2	2008:2	2012:2	2005:2	2006:2	2008:2	2012:2
Agriculture and resources	2.97	2.51	2.58	2.27	1.81	1.58	1.57	1.41
Construction	6.78	5.53	5.31	4.47	5.96	4.93	4.44	3.27
Manufacturing	12.70	10.98	10.59	8.94	13.84	12.39	11.04	8.92
Leisure, Accommodations	7.52	4.69	4.57	4.19	6.18	4.59	4.22	3.61
Healthcare	14.45	11.58	11.42	10.94	13.77	12.07	11.55	10.81
Professional services	12.73	10.48	10.37	9.41	14.15	12.63	11.87	10.42
Local services	17.00	13.22	12.57	10.96	17.39	13.87	12.85	11.45
Trade, Transportation, Utilities	9.63	7.91	7.99	7.17	10.62	9.42	8.96	7.87
Public, Education	16.22	13.38	13.00	12.19	16.28	14.62	14.58	12.93
Not employed		19.72	21.62	29.46		13.90	18.92	29.31

	Difference (Treatment – Control)			
Industry	2005:2	2006:2	2008:2	2012:2
Agriculture and resources	1.16	0.93	1.01	0.86
Construction	0.82	0.60	0.87	1.20
Manufacturing	-1.14	-1.41	-0.45	0.02
Leisure, Accommodations	1.34	0.10	0.35	0.58
Healthcare	0.68	-0.49	-0.13	0.13
Professional services	-1.42	-2.15	-1.50	-1.01
Local services	-0.39	-0.65	-0.28	-0.49
Trade, Transportation, Utilities	-0.99	-1.51	-0.97	-0.70
Public, Education	-0.06	-1.24	-1.58	-0.74
Not employed		5.82	2.70	0.15

Note: Columns in the upper panel provide the distribution (in percentages) across industry sectors of the treatment and matched control samples at the beginning of each quarter listed. Industry assignments are for the highest-earning job in that quarter, among those held in the listed quarter and in the following quarter (referred to in QWI as an end-of-quarter job). The lower panel provides differences between the industry distributions of the treatment and matched control samples in each quarter.

Table A9. Effects on Earnings by Subgroup based on Pre-Storm Earnings and Attachment to Employment

Dimension	Category	Pct.	Pre-storm earnings	Effects by Time Period after the Storms			
				Short	Medium	Long	Full
	All	100	9,916	-298.4	343.0*	792.3*	403.8*
				(191.4)	(125.4)	(141.8)	(127.9)
				[-3.0]	[3.5]	[8.0]	[4.1]
Annual earnings	Low (< $23,000)	36.7	4,197	-154.0	219.9*	403.5*	222.2*
				(115.6)	(53.3)	(83.1)	(58.1)
				[-3.7]	[5.2]	[9.6]	[5.3]
	Middle ($23,000–$43,499)	33.5	8,597	-145.8	394.2*	740.4*	461.2*
				(172.9)	(107.0)	(110.6)	(101.5)
				[-1.7]	[4.6]	[8.6]	[5.4]
	High (≥ $43,500)	29.8	18,449	-666.5	402.2	1,269.2*	522.7
				(375.1)	(306.7)	(386.1)	(323.9)
				[-3.6]	[2.2]	[6.9]	[2.8]
Employed in all 8 pre-storm quarters	Yes	78.2	10,640	-358.1	316.9*	753.1*	371.4*
				(208.2)	(142.5)	(153.8)	(146.5)
				[-3.4]	[3.0]	[7.1]	[3.5]
	No	21.8	7,322	-81.5	435.2*	926.5*	517.9*
				(148.9)	(103.5)	(152.7)	(97.5)
				[-1.1]	[5.9]	[12.7]	[7.1]
Same employer in all 8 pre-storm quarters	Yes	62.2	10,935	-377.1	289.2	773.9*	366.5*
				(219.5)	(149.9)	(140.6)	(147.4)
				[-3.4]	[2.6]	[7.1]	[3.4]
	No	37.8	8,237	-169.4	433.8*	829.3*	468.2*
				(152.9)	(111.5)	(168.2)	(117.3)
				[-2.1]	[5.3]	[10.1]	[5.7]
Combination of employed and tenure	Employed all 8 and same employer	62.2	10,935	-377.1	289.2	773.9*	366.5*
				(219.5)	(149.9)	(140.6)	(147.4)
				[-3.4]	[2.6]	[7.1]	[3.4]
	Employed all 8 and different employer	16.0	9,487	-282.8	438.0*	704.9*	406.8*
				(173.8)	(152.5)	(243.0)	(168.9)
				[-3.0]	[4.6]	[7.4]	[4.3]
	Not employed all 8	21.8	7,322	-81.5	435.2*	926.5*	517.9*
				(148.9)	(103.5)	(152.7)	(97.5)
				[-1.1]	[5.9]	[12.7]	[7.1]

Note: See Section 9.5. The estimates in each row are based on a separate regression. Numbers in column labeled "Pct." are the percent of the treatment sample in each group. Standard errors are in parentheses. Pre-storm earnings are average earnings in 2005:2 for the treatment sample. Numbers in brackets are effects as a percent of average pre-storm earnings for each group. For the earnings categories, annual earnings are based on the eight quarters before the storms, 2003:3–2005:2. Short term is 2005:4–2006:3 (k=1–4), medium term is 2007:4–2008:3 (k=9–12), long term is 2011:4–2012:3 (k=25–28), and full is 2005:4–2012:3 (k=1–28).
* p<0.05.

Table A10. Effects on Earnings by Subgroup based on Demographic Characteristics

Dimension	Category	Pre-storm earnings	Effects by Time Period after the Storms			
			Short	Medium	Long	Full
	All	9,916	-298.4	343.0*	792.3*	403.8*
			(191.4)	(125.4)	(141.8)	(127.9)
			[-3.0]	[3.5]	[8.0]	[4.1]
Education	Less than high school	6,848	-54.1	682.1*	969.0*	652.6*
			(146.9)	(103.5)	(121.6)	(102.3)
			[-0.8]	[10.0]	[14.2]	[9.5]
	High school	8,274	13.6	586.3*	809.0*	589.9*
			(124.7)	(87.6)	(111.7)	(89.4)
			[0.2]	[7.1]	[9.8]	[7.1]
	Some college	9,462	-192.8	341.3*	714.6*	405.8*
			(148.4)	(105.4)	(98.6)	(97.9)
			[-2.0]	[3.6]	[7.6]	[4.3]
	College	14,676	-910.1*	-70.9	915.4*	127.3
			(411.5)	(324.2)	(447.7)	(344.4)
			[-6.2]	[-0.5]	[6.2]	[0.9]
Age in 2005	25–29	7,352	12.4	98.0	588.6	249.0
			(138.7)	(174.2)	(432.1)	(224.7)
			[0.2]	[1.3]	[8.0]	[3.4]
	30–39	9,304	-326.7	430.3*	776.3*	449.8*
			(211.8)	(159.8)	(210.8)	(172.1)
			[-3.5]	[4.6]	[8.3]	[4.8]
	40–49	10,558	-394.7	410.6*	855.7*	439.8*
			(206.0)	(123.9)	(133.5)	(122.2)
			[-3.7]	[3.9]	[8.1]	[4.2]
	50–59	11,144	-292.8	275.1	841.3*	384.2*
			(215.6)	(146.6)	(188.1)	(133.0)
			[-2.6]	[2.5]	[7.6]	[3.4]
Sex	Female	7,417	-410.8*	56.2	290.9*	92.5
			(200.4)	(108.5)	(77.4)	(98.4)
			[-5.5]	[0.8]	[3.9]	[1.2]
	Male	12,409	-194.8	625.6*	1,299.0*	715.5*
			(208.2)	(178.9)	(235.3)	(185.0)
			[-1.6]	[5.0]	[10.5]	[5.8]
Race/ Ethnicity	White, not Hispanic	11,135	-244.8	399.2*	951.7*	489.3*
			(180.9)	(146.2)	(180.4)	(145.5)
			[-2.2]	[3.6]	[8.5]	[4.4]
	Black, not Hispanic	7,099	-481.1	204.7	428.3*	188.4
			(312.4)	(143.8)	(130.7)	(151.6)
			[-6.8]	[2.9]	[6.0]	[2.7]
	Hispanic + Other race/NH	9,627	-106.0	363.6	738.6*	446.5*
			(176.9)	(199.2)	(289.3)	(178.0)
			[-1.1]	[3.8]	[7.7]	[4.6]

Note: The estimates in each row are based on a separate regression. Pre-storm earnings are average earnings in 2005:2 for the treatment sample. Numbers in parentheses are standard errors clustered at the county level (based on 2005 residence location). Numbers in brackets are effects as a percent of average pre-storm earnings for each group. Short term is 2005:4–2006:3 (k=1–4), medium term is 2007:4–2008:3 (k=9–12), long term is 2011:4–2012:3 (k=25–28), and full is 2005:4–2012:3 (k=1–28).
* $p<0.05$.

Table A11. Summary Statistics for Alternate Control Samples

			Alternate Control Samples		
Variable	Treatment	Matched Control	Coastal Plain	Upland South	Weak Cities
Male	50.1	49.0	47.0	50.0	50.8
Female	49.9	51.0	53.0	50.0	49.2
Age 25–29	12.9	12.5	12.6	12.0	14.1
Age 30–39	29.4	30.3	28.9	30.0	29.3
Age 40–49	33.6	33.1	33.6	33.2	32.7
Age 50–59	24.1	24.1	25.0	24.8	23.9
White, not Hispanic	64.7	64.9	59.7	76.4	67.0
Black, not Hispanic	27.1	26.9	32.6	13.7	10.7
Hispanic	5.3	4.5	3.5	3.6	15.4
Other race, not Hispanic	2.9	3.7	4.2	6.3	6.8
Less than high school	12.4	11.0	10.6	10.8	10.6
High school	32.3	31.3	31.0	33.1	26.3
Some college	32.9	32.0	34.3	31.6	32.0
College	22.3	25.7	24.1	24.5	31.1
Annual earnings < $23,000	36.7	34.8	37.3	34.7	28.5
Annual earnings $23,000–$43,499	33.5	35.6	37.1	39.0	33.9
Annual earnings ≥ $43,500	29.8	29.7	25.6	26.3	37.6
Agriculture and resources	3.0	1.8	0.9	1.8	0.5
Construction	6.8	6.0	5.7	4.7	4.7
Manufacturing	12.7	13.8	13.3	18.5	14.4
Leisure, Accommodations	7.5	6.2	6.4	4.7	5.5
Healthcare	14.5	13.8	14.0	13.8	13.9
Professional services	12.7	14.1	14.4	13.2	18.1
Local services	17.0	17.4	18.5	16.2	17.3
Trade, Transport, Utilities	9.6	10.6	8.6	10.3	9.8
Public, Education	16.2	16.3	18.1	16.7	15.9
Earnings 2003:3	8,706	9,214	8,204	8,484	10,240
Earnings 2003:4	9,428	10,001	9,010	9,423	11,306
Earnings 2004:1	9,104	9,821	8,548	8,941	10,920
Earnings 2004:2	9,040	9,620	8,598	9,041	10,769
Earnings 2004:3	9,161	9,701	8,722	9,012	10,814
Earnings 2004:4	10,015	10,618	9,653	10,053	12,146
Earnings 2005:1	9,698	10,361	8,870	9,317	11,352
Earnings 2005:2	9,916	10,388	9,406	9,722	11,624
Percent employed, 2005:2	50.7	53.3	52.4	54.1	59.2
Unemployment rate, 2004	6.2	6.1	5.6	5.4	5.9
Housing-price change, 2000:2–2005:2	23.8	22.7	38.4	21.5	43.0
Population change, 2000–2005	3.7	5.2	6.2	4.0	1.3
Observations	138,000	406,000	229,000	438,000	1,120,000

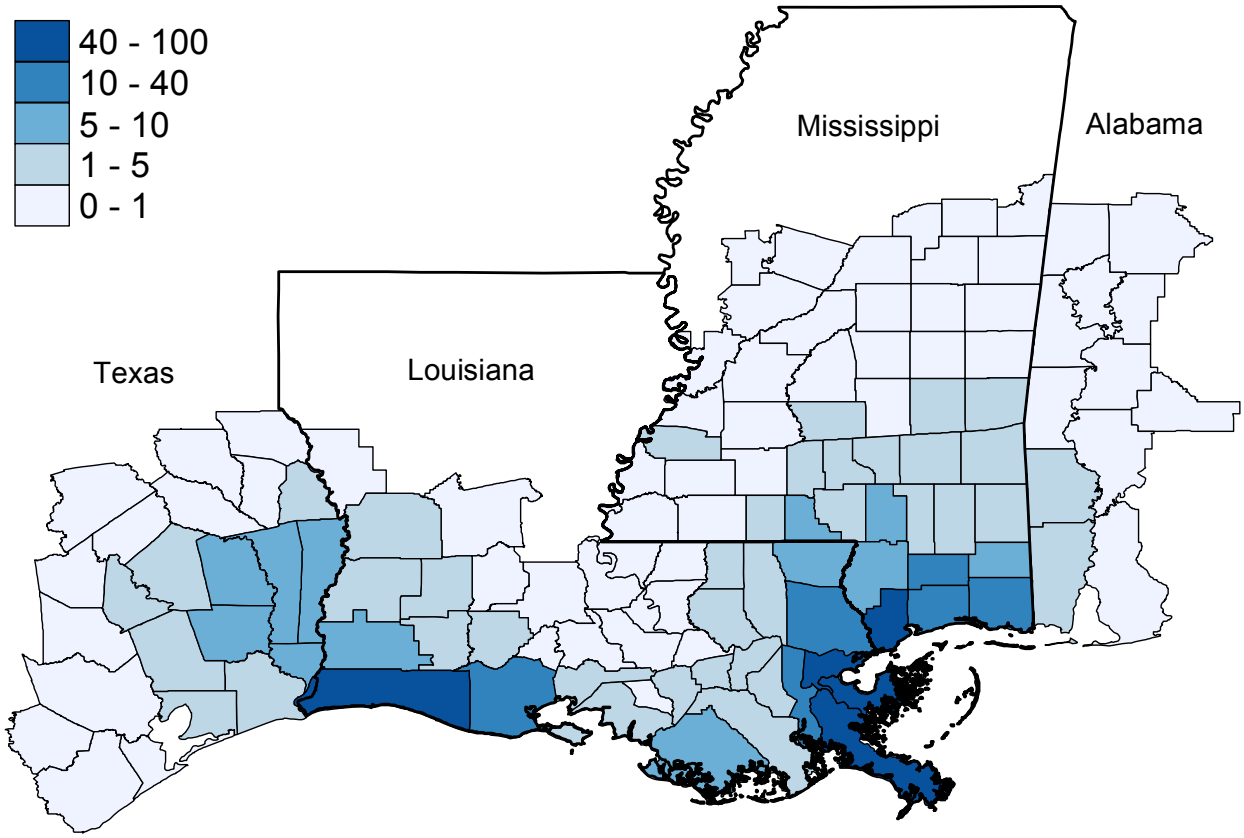
Note: See notes to Table 1.

Table A12. Index of Standardized Differences of Control Samples from Treatment Sample

Characteristic	Variable type	Categories / variables	Potential Control	Matched Control	Alternate Control Samples: Coastal Plain	Alternate Control Samples: Upland South	Alternate Control Samples: Weak Cities
Integrated index (RIMSE)	All	35	25.82	7.57	18.72	16.45	32.51
Age	Categorical	4	1.91	1.20	1.33	1.78	2.06
Sex	Categorical	2	1.70	2.07	6.22	0.19	1.57
Race/ethnicity	Categorical	4	27.87	3.00	9.73	23.21	28.80
Educational attainment	Categorical	4	11.39	4.76	4.12	3.89	12.36
Quarterly earnings (2003:3-2005:2)	Continuous	8	4.94	1.70	1.91	0.64	5.12
Industry (2005:3)	Categorical	9	7.25	3.95	6.14	7.84	9.26
Housing-price change (2000:2-2005:2)	Continuous	1	35.25	7.05	51.23	19.57	41.57
Percent highly attached	Continuous	1	64.70	19.44	15.33	33.27	85.12
Population change (2000-2005)	Continuous	1	0.11	8.62	14.90	2.12	19.80
Unemployment rate (2004)	Continuous	1	15.67	3.90	15.15	24.35	10.02

Note: See Section 9.4. Each characteristic gives the RMSE of the control sample compared to the treatment sample, where standardized differences serve as the error measure. Each characteristic consists of a categorical variable defined as a set of indicators or as one or more continuous variables, with an equal weight on each variable. The integrated index, or RIMSE, integrates the divergence measures across all characteristics, with an equal weight on each characteristic. See Table 1 and Table A11 for the complete list of sample means.

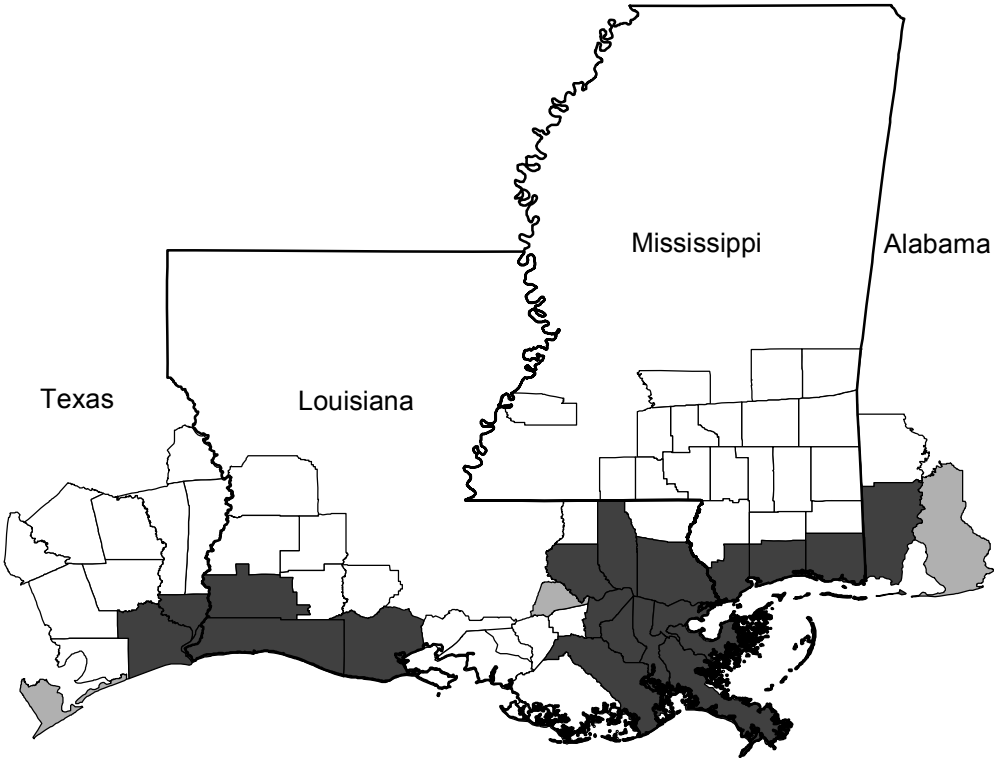
Figure A1. County-Level Damage



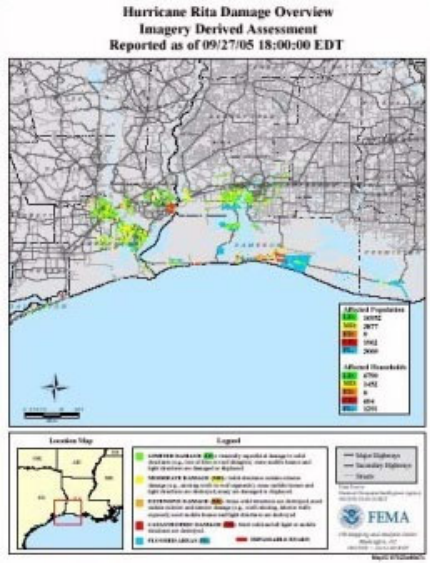
Source: FEMA damage data from HUD (2006).
Note: Legend shows the share of housing units in a county with damage in excess of $5,200. The map shows 122 counties in Texas, Louisiana, Mississippi, and Alabama. Hurricane Katrina made landfall in the east and Hurricane Rita made landfall in the west.

Figure A2. Treatment Counties, County Damage Level, and Sub-County Damage Data

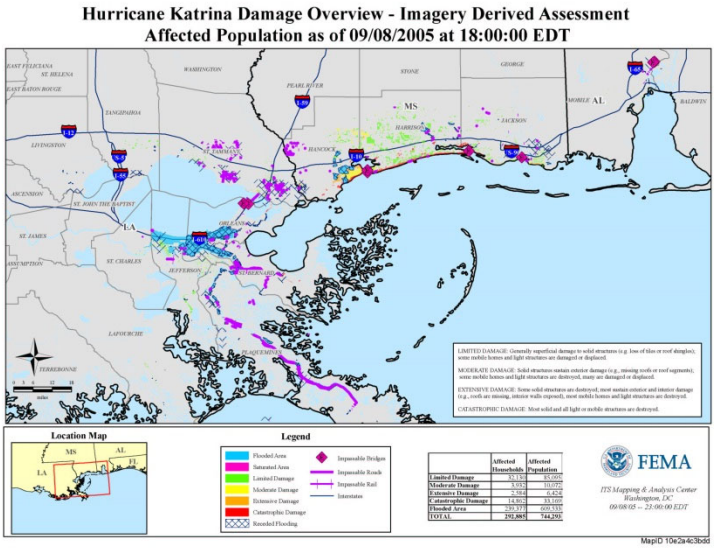
Panel A. Selected Counties



Panel B. FEMA Map: Rita Damage



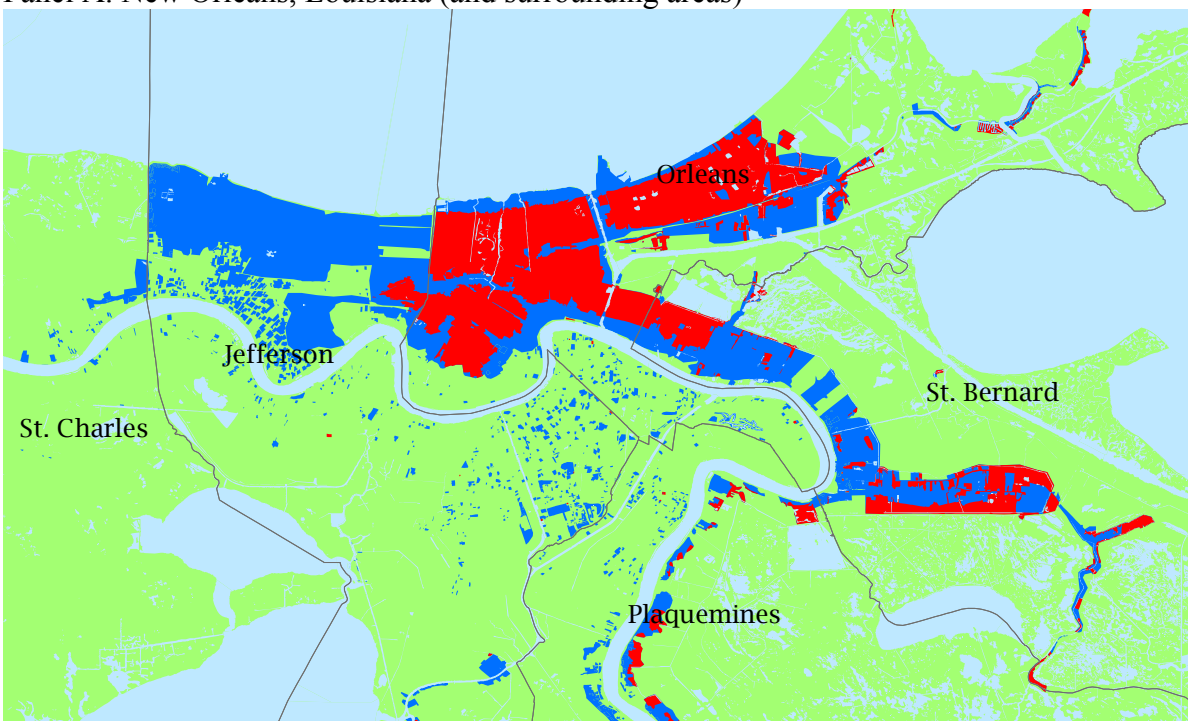
Panel C. FEMA Map: Katrina Damage



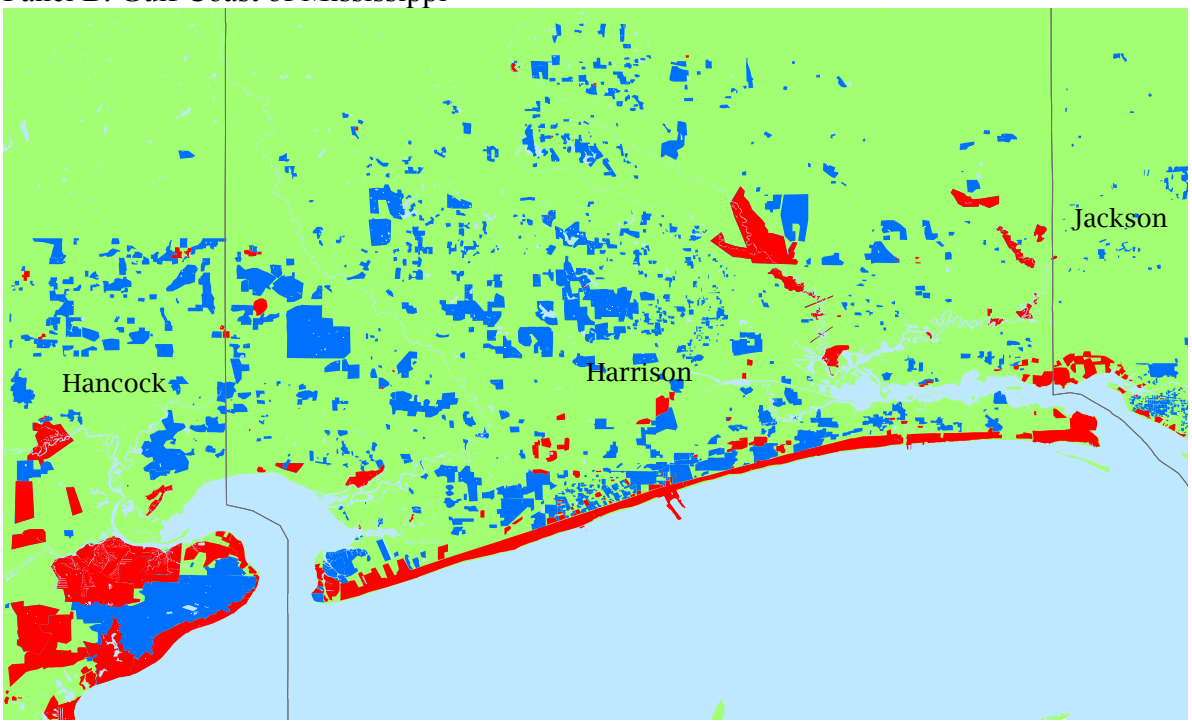
Note: Panel A breaks down the 63 treatment counties into three types. The 41 counties shown in white have at least 1 percent of housing units with damage in excess of $5,200 (HUD, 2006) but do not have sub-county damage data (FEMA, 2005). The 19 counties shown in dark gray have at least 1 percent of housing units with damage in excess of $5,200 and have sub-county damage data. The 3 counties shown in light gray have less than 1 percent of housing units with damage in excess of $5,200 but have sub-county damage data. Panels B and C display original mapping reports of damage for Hurricanes Rita and Katrina, respectively, distributed by FEMA (2005).

Figure A3. Major and Minor Damage

Panel A. New Orleans, Louisiana (and surrounding areas)



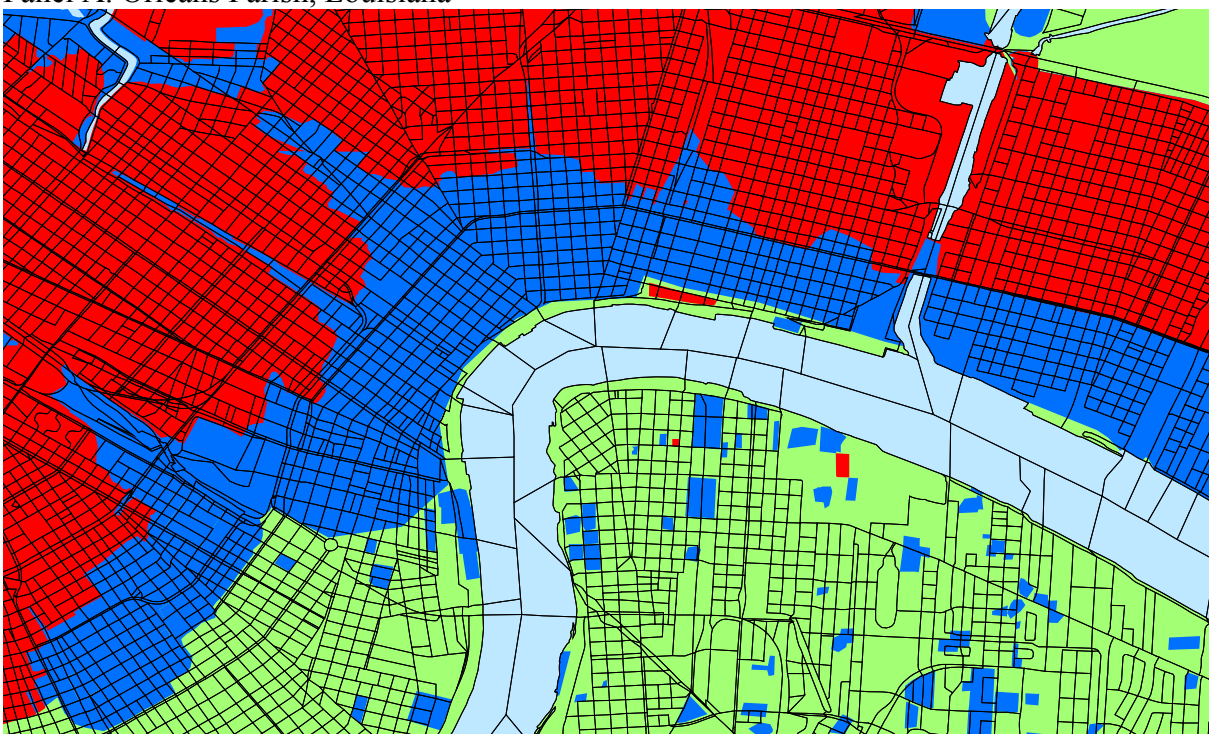
Panel B. Gulf Coast of Mississippi



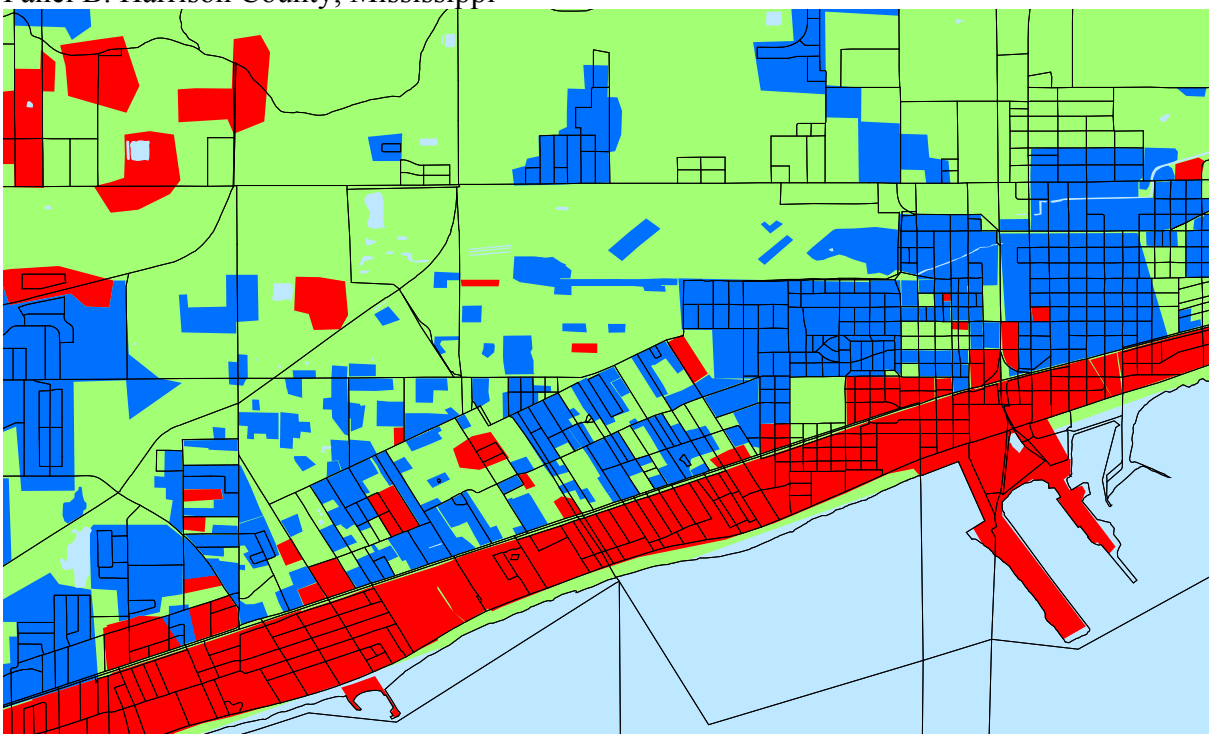
Source: Damage information from FEMA (2005).
Note: Panels A and B depict damage from Hurricane Katrina, along with county names and boundaries. Red indicates major damage, dark blue indicates minor damage, green indicates undamaged land area, and light blue indicates bodies of water. Both maps are to the same scale and depict an area approximately 40 miles wide.

Figure A4. Major and Minor Damage Overlaid with Census Blocks

Panel A. Orleans Parish, Louisiana

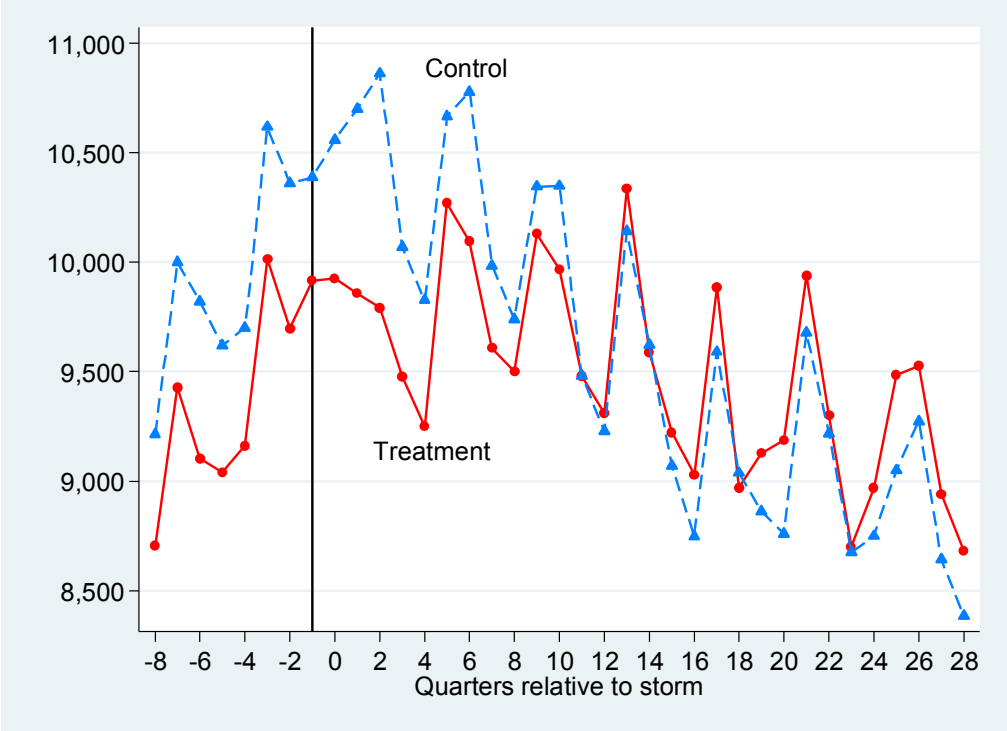


Panel B. Harrison County, Mississippi



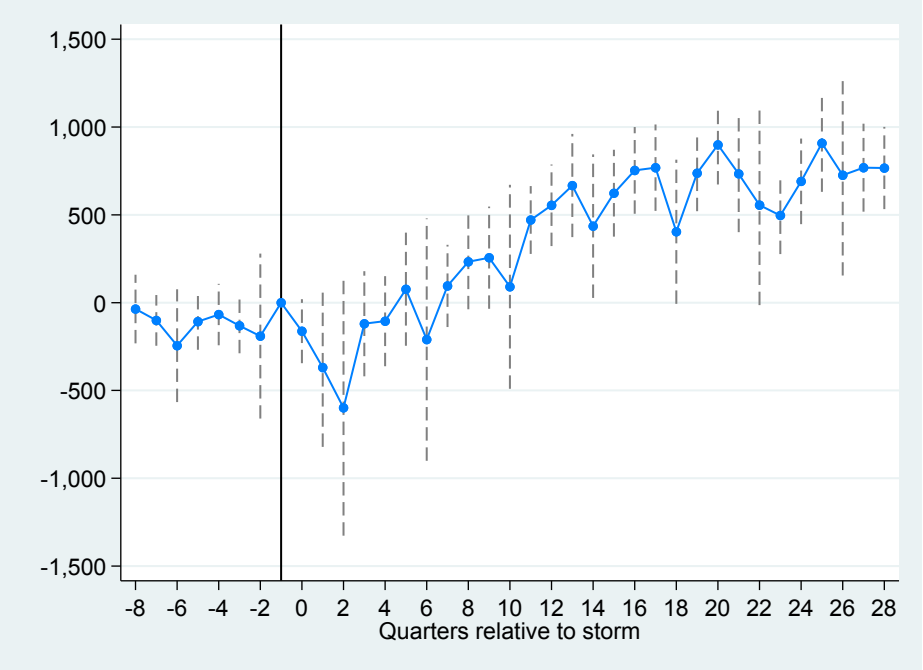
Source: Damage information from FEMA (2005).
Note: Panels A and B depict damage from Hurricane Katrina, along with boundaries of Census blocks. Red indicates major damage, dark blue indicates minor damage, green indicates undamaged land area, and light blue indicates bodies of water. Both maps are to the same scale and depict an area approximately 5.5 miles wide.

Figure A5. Average Earnings in Treatment and Control Samples



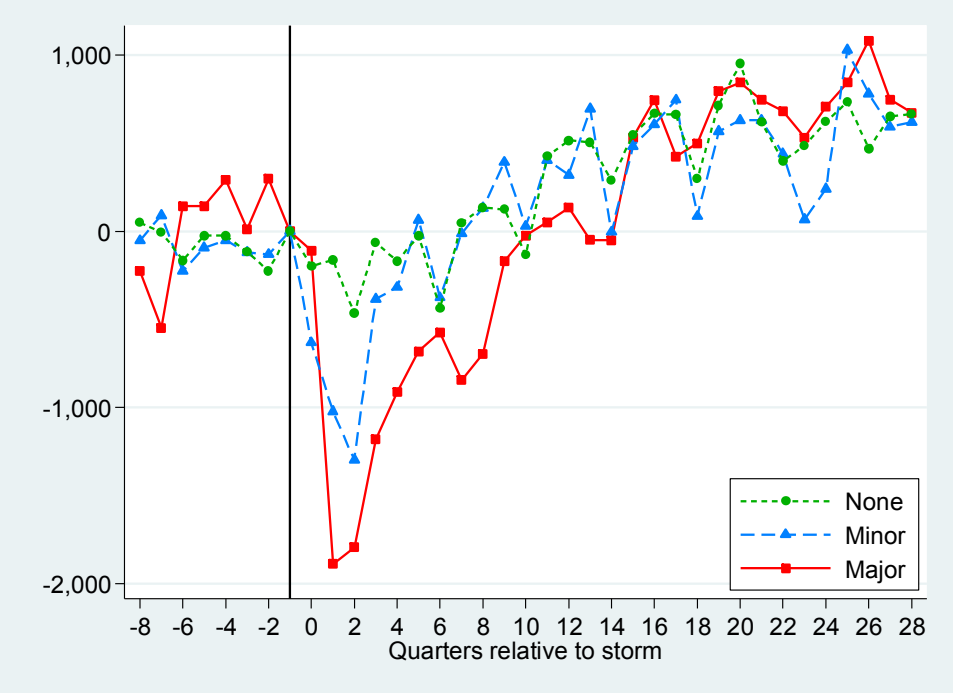
Note: See Figure 2 for description of earnings data. The vertical line marks 2005:2 (k = -1), which is the reference quarter in the earnings regressions.

Figure A6. Effects on Earnings



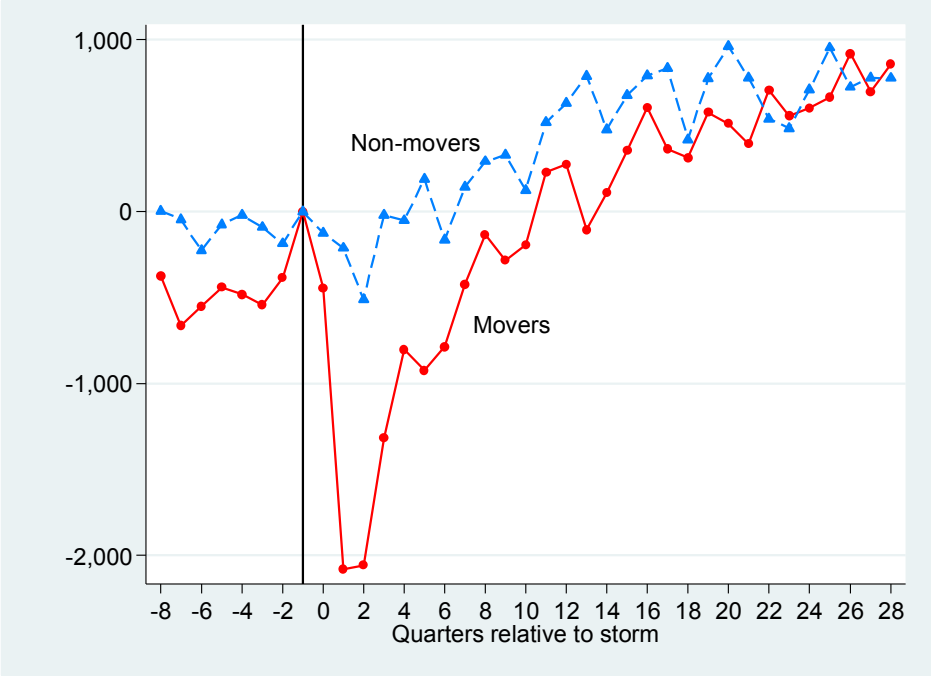
Note: See Figure 2 for description of earnings data. Equation (1) provides the model specification. Estimates capture the earnings difference between individuals in the treatment and control samples in each quarter before/after the storms, relative to this difference in the first quarter before the storms (2005:2). Because 2005:2 (k = -1) is the reference quarter, the D_{ik} variable for this quarter is excluded from the regression and this quarter is marked by the vertical line. Dashed lines show 95% confidence intervals, which are based on standard errors clustered at the county level (based on 2005 residence location).

Figure A7. Effects by Type of Damage to a Worker's Workplace



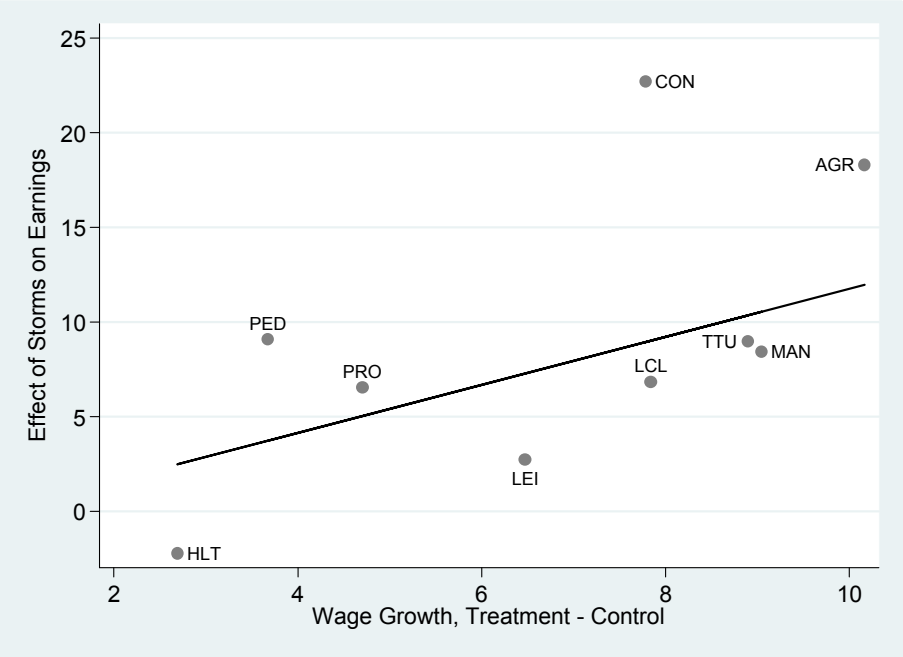
Note: See Figure 2 for description of the earnings data. The reference quarter in the model is 2005:2 (k = -1); this quarter is marked by the vertical line. For simplicity, the figure does not display estimates for uncertain damage (expected to be of lower frequency and intensity) and for working outside of the treatment area (in the workplace-damage model). See Table A2 for distribution of damage in the treatment sample.

Figure A8. Effects on Earnings by Subgroups based on Migration



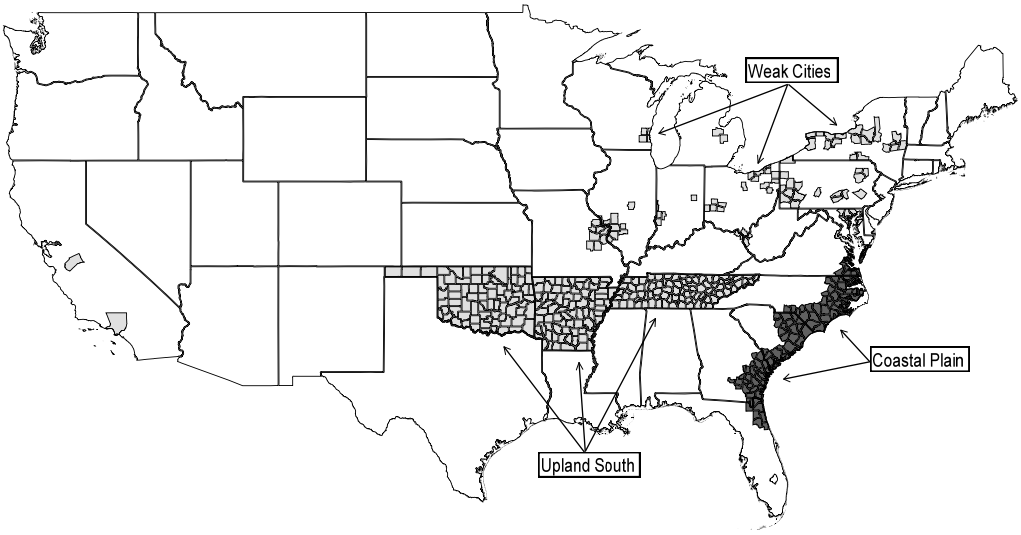
Note: See Figure 2 for description of the earnings data and Table A3 for description of the migration sample. The reference quarter in the model is 2005:2 (k = -1); this quarter is marked by the vertical line. Movers are those in the treatment sample who were in a different commuting zone in 2005 and 2006; non-movers are the remainder of the treatment sample.

Figure A9. Wage Change in Local Areas and Earnings Effect of Storms, by Industry Sector



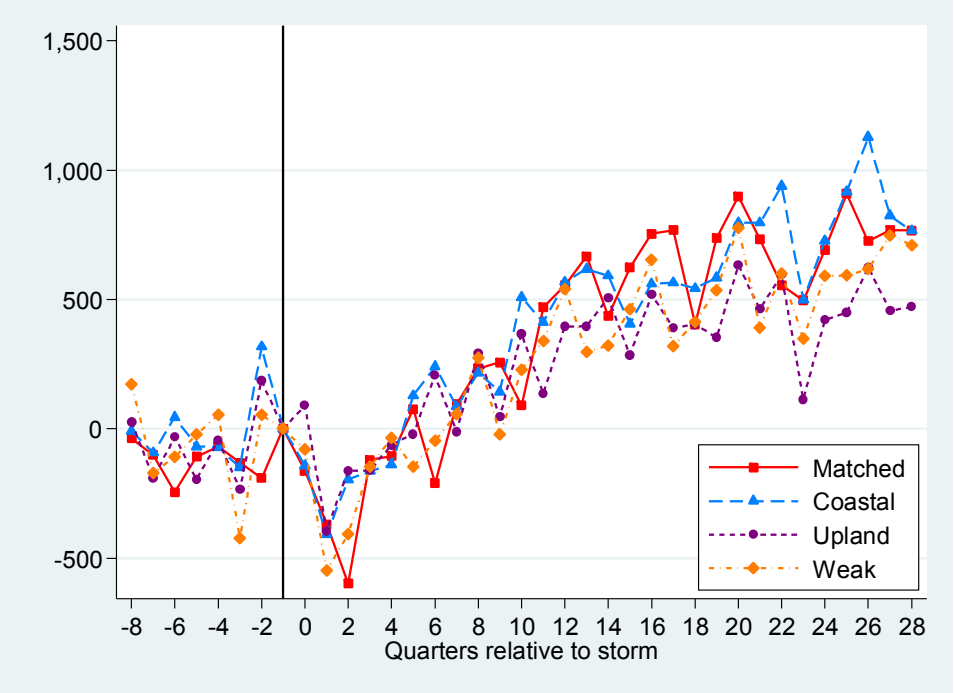
Note: "Wage Growth, Treatment - Control" is based on estimates from the Occupational Employment Statistics survey and is defined as [%change in average wage (2005 to 2012), relative to pre-storm, in treatment] – [%change in average wage (2005 to 2012), relative to pre-storm, in control]. "Effect of Storms on Earnings" is the long-term effect of the storms on earnings as a percent of average pre-storm earnings (taken from Table 3). Sectors: agriculture and natural resources (AGR); construction (CON); manufacturing (MAN); leisure and accommodations (LEI); healthcare (HLT); professional services (PRO); local services (LCL); trade, transportation, and utilities (TTU); and public and education (PED). The regression line is estimated by weighted least squares with the sector share of total employment before the storms in the treatment area as the weight.

Figure A10. Alternate Control Areas



Note: Map depicts the residence location of workers in the alternate control samples. The Coastal Plain control (darker shading) is in Florida, Georgia, South Carolina, North Carolina, and Virginia. The Upland South control (lighter shading) is in Oklahoma, Arkansas, and Tennessee. The Weak Cities control (lighter shading) is in California, Missouri, Illinois, Wisconsin, Indiana, Michigan, Ohio, Kentucky, West Virginia, Pennsylvania, Maryland, New Jersey, and New York.

Figure A11. Effects on Earnings with Alternate Control Samples



Note: See Figure 2 for description of earnings data. Equation (1) provides the model specification. The sample for this analysis is the treatment sample paired with the either the matched, Coastal Plain, Upland South, or Weak Cities control sample. See Table A11 for sample sizes.