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DOES WHEN YOU DIE DEPEND ON WHERE YOU LIVE? EVIDENCE FROM HURRICANE KATRINA

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ABSTRACT

We follow Medicare cohorts over time and space to estimate Hurricane Katrina's long-run mortality effects on elderly and disabled victims initially living in New Orleans. Inclusive of the initial shock, the hurricane improved survival eight years past the storm by 1.74 percentage points. Migration to lower-mortality regions explains most of this survival increase. Migrants to low- versus high-mortality regions look similar at baseline, but migrants' subsequent mortality is 0.83-0.90 percentage points lower for each percentage-point reduction in local mortality, quantifying causal effects of place on mortality among this population. By contrast, migrants' mortality is unrelated to local Medicare spending.

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1 Introduction

Hurricane Katrina, the costliest tropical cyclone ever to strike the United States mainland, devastated the Gulf Coast in 2005. The immediate impact of the storm killed nearly 2,000 individuals and displaced more than 1 million residents, resulting in the largest migration of U.S. residents since the Dust Bowl in the 1930s (Nigg, Barnshaw and Torres, 2006). While prior studies have evaluated how demographic and economic outcomes evolve in the aftermath of environmental catastrophes (e.g., Hornbeck, 2012; Hornbeck and Naidu, 2014; Nakamura, Sigurdsson and Steinsson, 2017), little is known about the effects of such events on long-run health and longevity, which represent considerable economic value (Murphy and Topel, 2006). In particular, when a disaster displaces large segments of the population from their homes, the regions to which people move may play an important role in shaping long-term health outcomes. Thus, understanding how health outcomes evolved in the wake of Hurricane Katrina provides insight into both the economic impact of this historic event and also the ways in which local conditions affect population health (e.g., Fisher et al., 2003*a*,*b*; Chetty et al., 2016).

We study the short- and long-run mortality impacts of Hurricane Katrina on one of the most vulnerable subsets of the population: the elderly and long-term disabled of New Orleans. Roughly half of those killed by the immediate impact of Hurricane Katrina were over the age of 75 (Brunkard, Namulanda and Ratard, 2008), and about one-fifth of the displaced population were elderly individuals on Medicare (Super and Biles, 2005). While the immediate losses and disruption caused by the disaster may have scarred the health of this vulnerable group, widespread migration out of New Orleans to regions with better economic and health outcomes may have generated health benefits. Yet, quantifying the long-run health impacts of events like Hurricane Katrina has proven difficult, due largely to lack of data that capture pre-disaster outcomes and track individuals post-disaster with minimal attrition.

To overcome this challenge, we use administrative Medicare data, which cover the vast majority of the U.S. elderly and long-term disabled populations over the period 1992–2013. Importantly, these data allow us to follow individuals over time and space and provide exact dates of death. We identify cohorts of Medicare beneficiaries living in New Orleans prior to Hurricane Katrina and track their mobility and mortality rates for eight years after the storm (2005–2013). To identify how outcomes would have evolved in the absence of Hurricane Katrina, we examine mobility and mortality for comparable cohorts of Medicare beneficiaries initially residing in 10 cities that were not directly affected by the hurricane (Deryugina, Kawano and Levitt, 2018). To validate this control group choice, we show that mortality trends in the New Orleans and control city cohorts were very similar prior to Hurricane Katrina going back as far as 1992 (the earliest year for which we have data). We then estimate the causal effects of the hurricane by comparing how the New Orleans cohort's post-hurricane outcomes changed relative to those of the comparison cohort.[1]

We find that Hurricane Katrina caused a substantial short-run increase in the mortality rate of the Medicare cohort that resided in New Orleans as of March 2005. Among this group, mortality increased by over half a percentage point in 2005, which is over 10 percent of the cohort's mortality that year. Most of these excess deaths occurred within a week of the hurricane's landfall, and this immediate effect dissipated over several months.

We then assess the effects of Hurricane Katrina on annual mortality and population migration. In contrast to the short-run mortality increase observed in 2005, we find that Hurricane Katrina led to sustained *reductions* in mortality from 2006 through 2013. This long-run mortality decline is not explained by short-run mortality displacement, or "harvesting": inclusive of the initial increase in mortality, we estimate that Hurricane Katrina increased the probability of surviving eight years past the storm (i.e., through 2013) by 1.74 percentage points, a 2.7 percent increase relative to the overall eight-year survival rate of those residing in New Orleans in early 2005. We also find that the hurricane led to a massive

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[1] As we show, our central findings are robust to using the rest of the United States as the comparison cohort and to using the synthetic control method to conduct inference that accounts for the aggregate nature of the shock created by Hurricane Katrina.

and lasting dislocation of the elderly and long-term disabled, consistent with prior evidence pertaining to the demographic and economic effects of the hurricane (Deryugina, Kawano and Levitt, 2018). Medicare beneficiaries living in New Orleans as of March 2005 were about 40 percentage points more likely to leave their city of residence than members of the control group. Over half of those who left had not returned as of 2013, conditional on being alive.

We consider two possible (and not mutually exclusive) explanations for the long-run mortality decline: improvements in the New Orleans health care system and relocation of the victims to areas with better health outcomes. Two empirical patterns suggest that the long-run mortality decline was unrelated to improvements in the New Orleans health care system. First, hospital capacity in New Orleans fell sharply in the aftermath of the hurricane, and its recovery did not keep pace with the rebound in population. Second, the New Orleans health care infrastructure was rebuilt gradually. However, when we consider the 2006–2013 mortality of those who remain in the city, we see a flat pattern rather than gradual mortality improvements. Thus, any mortality improvements for those who stayed would either have to have occurred in or before 2006 or be exactly offset by stayers' counterfactual trend in mortality, both of which seem unlikely.

Because New Orleans was one of the highest mortality areas in the country prior to Hurricane Katrina, displaced individuals generally relocated to regions with better health outcomes. To the extent that regional health outcomes reflect place-specific factors such as access to quality health care, the decline in mortality among the Hurricane Katrina victims may have been driven by relocation to areas that are more conducive to survival. To directly examine the role of place on health, we examine the mortality patterns among New Orleans residents who moved between March 2005 and March 2006 (i.e., had left New Orleans after the hurricane). We find that hurricane survivors who moved to low-mortality regions subsequently experienced lower mortality than survivors who moved to high-mortality regions. Specifically, each percentage-point increase in the destination region's mortality rate corresponds to a 0.83–0.90 percentage-point increase in the movers' mortality rate. This effect emerges as early as 2006–2007, suggesting it does not arise entirely through slow-moving channels such as lifestyle. The relationship between local and migrant mortality describes the causal effect of place on individual mortality under the assumption that baseline mortality risk among those who move is uncorrelated with mortality rates in the destination region. Supporting this assumption, we find little correlation between destination mortality rates and baseline characteristics of movers, including chronic conditions that are strongly predictive of mortality. In addition, the estimates are highly stable even with rich controls, including pre-existing chronic conditions. We estimate that changes in the local mortality rate experienced by hurricane victims explain about 70 percent of the long-run mortality decline caused by the hurricane.

To the best of our knowledge, we provide the first controlled estimates of the long-run mortality effects of an environmental disaster on adult victims. By contrast, prior research on disasters and health has been largely limited to looking at birth outcomes and infant health (e.g., Torche, 2011; Currie and Rossin-Slater, 2013; Currie and Schwandt, 2016) or conducted by surveying a subset of the victims.[2] These survey approaches, however, generally suffer from non-random sampling, rarely measure pre-existing outcomes, and usually lack a control group. We are able to overcome these limitations in our setting because our data track the mortality and location of every Medicare-eligible individual. Our main finding—that Hurricane Katrina reduced long-run mortality among the elderly and disabled populations by inducing them to relocate—builds on recent evidence that the hurricane indirectly generated other long-run benefits, including higher earnings among the working-age population (Groen, Kutzbach and Polivka, 2016; Deryugina, Kawano and Levitt, 2018) and improved test scores among displaced students (Sacerdote, 2012).

Our findings add to a growing body of literature in economics that uses migration to identify how local conditions affect individual outcomes. For example, Song et al. (2010) and Finkelstein, Gentzkow and Williams (2016) study Medicare patients who move across

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[2] See, for example, Armenian, Melkonian and Hovanesian (1998); Sastry and VanLandingham (2009); Rhodes et al. (2010); Adams et al. (2011); Adeola and Picou (2012); Pietrzak et al. (2012).

regions to identify local determinants of diagnosis rates and medical spending. Black et al. (2015) find that the Great Migration, where African Americans from the rural South moved to mainly urban areas in the North, *increased* their long-run mortality, partly because of increases in smoking and alcohol consumption. Outside of a health setting, movers have been used to study how local conditions affect education and earnings (Chetty, Hendren and Katz, 2016; Nakamura, Sigurdsson and Steinsson, 2017; Chyn, forthcoming; Chetty and Hendren, forthcoming), income reporting in tax filings (Chetty, Friedman and Saez, 2013), and brand preferences (Bronnenberg, Dubé and Gentzkow, 2012). We contribute to this literature by studying how the long-run mortality outcomes of those displaced by Hurricane Katrina depend on the local mortality rates of the destination region. Our finding that a migrant's individual mortality risk corresponds closely to the destination region's mortality rate suggests that local public health conditions are an important determinant of individual health outcomes, at least for the elderly and disabled populations.

Our results also shed light on why life expectancy differs across the United States and on how health capital accumulates over the life cycle. As demonstrated by Chetty et al. (2016) and Dwyer-Lindgren et al. (2017), life expectancy varies significantly across U.S. locations, and these disparities correlate strongly with numerous demographic factors and health behaviors, like income and smoking. Interpreting these correlations causally is hampered by the possibility that areas where populations have low socioeconomic status or engage in unhealthy behaviors may also make fewer investments in dimensions of public health that are difficult to measure. Our results suggest that geographic variation in life expectancy is at least partly driven by contemporaneous location characteristics and not just by differences in demographics or health behaviors that affect mortality only over long time horizons. In addition, the sharp and enduring decline in the mortality rate of Hurricane Katrina victims contrasts with the canonical Grossman (1972) model of health capital, which posits that health capital changes slowly, although it is consistent with a version of the model in which health capital depreciates rapidly, as may be the case with the elderly and disabled.

Finally, a key question in public health is whether higher-spending regions generate better health outcomes than lower-spending regions. Numerous studies have documented widespread geographic variation in health care spending and have shown that higher-spending regions often have little better or even worse health outcomes than lower-spending regions (Fisher et al., 2003*a*,*b*; Baicker and Chandra, 2004; Sirovich et al., 2006; Skinner, 2011). However, regional spending disparities may partly reflect differences in the baseline health of the resident population. Doyle (2011) and Doyle et al. (2015) address this limitation by analyzing quasi-random assignment of patients to hospitals. Both studies find that patients have better outcomes when treated at higher-spending hospitals. While these analyses focus on the returns to being hospitalized in a high-spending region, the returns to *living* in a high-spending region may differ—for example, higher-quality health systems could reduce the need for hospitalization. In our setting, we find no relationship between a mover's subsequent mortality and local health care spending, suggesting that average returns to living in a high-spending region are low.

Section 2 provides an overview of Hurricane Katrina and the health care system of New Orleans. Section 3 describes our data and estimation sample and presents summary statistics. Section 4 outlines our research design, and Section 5 presents the results. Section 6 concludes.

2 Setting

2.1 Overview of Hurricane Katrina

Hurricane Katrina formed as a tropical depression on August 23, 2005 (National Weather Service, 2016). As Katrina's strength and path became apparent, Louisiana officials declared a state of emergency on August 26 and issued a mandatory evacuation order for New Orleans on August 27. The fears proved well-founded: Hurricane Katrina struck New Orleans on August 29 as a Category 3 hurricane with sustained winds of 125 miles per hour, causing numerous levee and flood wall failures that resulted in widespread flooding (see Appendix

Figure A.1). As a result of both the direct impact of the hurricane and the levee failures, thousands of homes in New Orleans were damaged or destroyed. The National Oceanic and Atmospheric Administration (NOAA) estimates that Hurricane Katrina caused $161 billion in direct damages (2017 dollars), making it the costliest U.S. natural disaster on record (National Hurricane Center, 2018; NOAA, 2018).

Despite an estimated evacuation rate of 80–90 percent (Wolshon, 2006), Hurricane Katrina's official death toll was 1,833, making it the deadliest natural disaster in the United States since 1928 (Beven-II et al., 2008). About half of those killed by the immediate impact of the storm were over the age of 75 (Brunkard, Namulanda and Ratard, 2008). Parts of New Orleans remained uninhabitable for months after the storm; rebuilding in some areas took years. Officially, individuals in 17 out of 19 New Orleans ZIP codes were prohibited from returning to their homes until at least December 9, 2005 (Federal Emergency Management Agency, 2005). On that date, the Federal Emergency Management Agency (FEMA) allowed residents of 10 New Orleans ZIP codes to return to their homes and stay ("look-and-stay" ZIP codes); residents in seven other New Orleans ZIP codes could return to their homes during the day but could not spend the night ("look-and-leave" ZIP codes). Storm victims who could not find suitable living arrangements were given funds to pay for a hotel or apartment or the opportunity to live in specially provided trailers. Up to 200,000 of those displaced by Hurricane Katrina were elderly individuals on Medicare (Super and Biles, 2005).

The aid response to Hurricane Katrina was considerable. Excluding flood insurance payments and loans, Louisiana received about $50 billion from the federal government (in nominal dollars).[3] The majority of these funds were earmarked for rebuilding infrastructure rather than given directly to victims. Much of the latter type of aid came through FEMA's Individual Assistance program, which paid out about $2.9 billion to New Orleans residents for temporary housing, repairs, rebuilding, and other disaster-related expenses. In 2006–2013, New Orleans homeowners also received about $4.3 billion through the "Road Home" program

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[3] See Deryugina, Kawano and Levitt (2018) for a detailed description of Hurricane Katrina aid components.

to rebuild or sell their homes. Finally, FEMA also paid about $320 million in Disaster Unemployment Assistance in the state of Louisiana. Deryugina, Kawano and Levitt (2018) calculate that a reasonable upper bound on the aid spending for the city of New Orleans is $125,000 per capita, of which about $17,000 consisted of direct transfers to individuals. This aid may have generated new benefits for the victims (e.g., long-run mortality reductions) or—given the initial destruction created by Hurricane Katrina—it may have merely offered protection from more severe negative effects (e.g., higher mortality increases).

2.2 Health and health care in New Orleans

Hurricane Katrina devastated the health care infrastructure in New Orleans (Rowland, 2007). All nine large hospitals operating there in 2005 were closed in the immediate aftermath of the storm due to damage and/or flooding. One hospital (Touro Infirmary) reopened 28 days later, a second (Tulane Medical Center) reopened in early 2006, and two more (Memorial Medical Center and University Hospital/Interim LSU Hospital) reopened in late 2006. The remaining large hospitals were closed for years or never reopened. Although smaller inpatient facilities and several hospitals in nearby cities continued operating, the closure of so many hospitals reduced health care access for many individuals. Many health care professionals left the city after the storm, likely disrupting access to care across other traditional health care facilities as well. Overall, the number of beds and employees in the 22 inpatient facilities in New Orleans fell by nearly 70 percent between 2004 and 2007 (see Appendix Figure A.2) without any offsetting increase in the nearby parishes. Hospital utilization also declined precipitously, driven at least partly by the enormous decline in the city's population.

By 2008, health care infrastructure in the New Orleans area had begun to recover, although problems persisted (DeSalvo, Sachs and Hamm, 2008). The city had returned to 70 percent of its pre-Katrina population and was continuing to grow, increasing demand for medical services. At the same time, many hospitals faced staffing and financial problems, resulting in long wait times. The permanent closure of Charity Hospital, which served a large number of the uninsured in New Orleans, forced many of the uninsured to seek care in emergency rooms, placing further strain on hospital resources. However, due to the large reduction in population, post-Katrina New Orleans still had about the average number of beds per capita and more physicians per capita than the national average (DeSalvo, Sachs and Hamm, 2008). Moreover, community-based primary care clinics funded by various sources sprung up after the hurricane, potentially filling the void left by hospital closures.

Katrina's large-scale destruction of homes, health care capacity, and general infrastructure likely created a harsh environment for the elderly and long-term disabled, who have, on average, a higher incidence of chronic conditions and less robust physical and mental capabilities. These groups are thought to be more vulnerable to environmental catastrophes than the general population, and emergency managers are often urged to pay special attention to their needs (e.g. Morrow, 1999; Fernandez et al., 2002). Mensah et al. (2005) summarize the many additional challenges that chronic conditions pose during natural disasters, most of which are self-evident. For example, following Hurricane Charley in 2004, the Centers for Disease Control and Prevention (2004) found that many older adults experienced disruptions in treatment for pre-existing conditions, which could have adversely affected their health. In the case of Hurricane Katrina, the evacuees as a whole were not a healthy group: a survey of victims in Houston shelters revealed that 40 percent had at least one chronic condition and a similar fraction reported needing prescription medication (Brodie et al., 2006).

There are several other reasons to expect that Hurricane Katrina led to persistently worse health outcomes among elderly and disabled victims. The elderly are thought to be particularly prone to "relocation stress syndrome," where individuals' physical and mental health suffers as a result of being transferred from one environment to another (Barnhouse, Brugler and Harkulich, 1992). Natural disasters are also thought to lead to a deterioration in mental health (Freedy, Kilpatrick and Resnick, 1993; Norris et al., 2002; Norris, Friedman and Watson, 2002), including increased rates of post-traumatic stress disorder (Galea, Nandi and Vlahov, 2005; Neria, Nandi and Galea, 2008). Additionally, the disruption and displacement caused by the storm may have made it more difficult for patients to get appropriate health care. While several studies have found deteriorated mental and physical health following Hurricane Katrina, these studies generally lack a control group to account for secular trends, most lack outcomes measured pre-Katrina, and almost all have focused on short-run effects.[4]

It is also possible, however, that disaster aid and victims' responses led to a quick recovery. In particular, the significant population displacement brought about by Hurricane Katrina could have improved long-run survival if victims relocated to areas that are more conducive to good health. After we estimate the aggregate effects of Hurricane Katrina on long-run mortality among the elderly and disabled, we return to consider the role of migration and place in shaping the recovery of the hurricane victims.

3 Data and Estimation Sample

3.1 Data

The primary data for our analysis are Medicare administrative records for the universe of Medicare beneficiaries over the period 1992–2013. As of 2010, over 97 percent of the U.S. population aged 65 and older was enrolled in Medicare, making these data the most comprehensive record of elderly health in the United States. Medicare also covers non-elderly, long-term disabled individuals who have received Social Security Disability benefits for 24 months or have either end-stage renal disease or amyotrophic lateral sclerosis.

In addition to their comprehensive coverage of the U.S. elderly and disabled populations, Medicare data offer two features essential for studying health dynamics in our setting. First, Medicare reports in each year the mailing ZIP code of each beneficiary where SSA benefits and official communication are mailed, which we refer to as the "ZIP code of residence." This

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[4] See, for example, Brodie et al. (2006); Kessler et al. (2008); Sastry and VanLandingham (2009); Sastry and Gregory (2013). In the only longer-run study of which we are aware, Paxson et al. (2012) follow 532 low-income mothers who lived in New Orleans during Hurricane Katrina, finding long-lasting increases in post-traumatic stress symptoms and psychological distress.

information allows us to identify individuals living in a particular place at a certain time (e.g., New Orleans residents prior to Hurricane Katrina) and to track those individuals over time without attrition even if they move. Second, Medicare records each individual's exact date of death based on Social Security Administration (SSA) records.

Our analysis relies on four sets of annually recorded Medicare variables, which we summarize here.[5] The first set comes from Medicare eligibility records and contains beneficiary identifiers and demographic information obtained from the SSA record system, including 9-digit ZIP code, race, sex, date of birth, date of death, and an end-stage renal disease indicator. For 1999, 2007, and 2009–2013, ZIP codes correspond to the mailing address on record at the end of the calendar year. In all other years, ZIP codes correspond to the address on record as of March of the following year. Thus, the 2004 ZIP code reflects a beneficiary's address as of March 2005, about five months prior to Hurricane Katrina. The 2005 ZIP code reflects a beneficiary's address as of March 2006, about seven months after the hurricane. For individuals who die prior to the date of the location snapshot, the location variable will reflect their last ZIP code of residence on record.

The second set of Medicare variables we use measure health care spending based on fee-for-service claims. For each beneficiary, we calculate total annual spending as the sum of payments due to institutional or non-institutional providers (e.g. physicians), excluding payments for drugs covered under Medicare Part D. Because spending is based on claims, we do not observe spending for individuals enrolled in Medicare Advantage plans (less than 20 percent of our sample). In these cases, Medicare makes fixed payments to private providers who then handle any claims these individuals have.

The third set of Medicare variables we use include 27 indicators for common chronic conditions inferred from medical claim histories. Measured conditions include heart attack, stroke, hypertension, diabetes, cancer, Alzheimer's disease, and depression. We group the 27 individual conditions into eight broad categories: heart disease and stroke; respiratory

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[5] See the Online Appendix for additional details on these data and the definitions of key variables used in our analysis, including beneficiary location, chronic conditions, and cause of death.

disease; blood and kidney disease; cancer; diabetes; musculoskeletal diseases; dementia (including Alzheimer's disease); and "other" (cataracts, glaucoma, hypothyrodism, benign prostatic hyperplasia, and depression). Because the chronic condition indicators are based on claims, they are available only for individuals who are continuously enrolled in fee-for-service Medicare over a condition-specific look-back window (usually two years).

The fourth set of Medicare variables we use in our analysis of New Orleans movers come from the National Death Index created by the Center for Disease Control and matched to Medicare beneficiaries who died in 1999–2008. We group the ICD-10 codes specifying the cause of death into four major groups: cardiovascular, cancer, other internal causes (e.g., diabetes, influenza), and external causes (e.g., vehicle accidents, suicide).

Our analysis relies on identifying the regions in which a Medicare beneficiary lives, both before and after Hurricane Katrina. Our primary units of geography for this purpose are Hospital Service Areas (HSAs), as defined by the Dartmouth Atlas, which group U.S. ZIP codes into 3,436 local health care markets for hospital care (Wennberg, 1996). In some cases, we consider Hospital Referral Regions (HRRs), which aggregate HSAs into 306 regions. We refer to an HSA by the primary city located in the HSA and use the terms "city" and "HSA" interchangeably, even though the boundary of the HSA may extend beyond the city's political boundary. For example, the New Orleans HSA is very similar to the city of New Orleans, although it also includes several sparsely populated areas located to the south of the city.

Finally, we match Medicare beneficiaries to flooding and income neighborhood characteristics based on their 9-digit ZIP code of residence at baseline, as geocoded by GeoLytics. To measure flooding exposure at the 9-digit ZIP code level, we utilize Hurricane Katrina flood depth data from NOAA at a resolution of five meters and measure average flooding within a 50-meter radius of the 9-digit ZIP code. We classify beneficiaries as "flooded" if their 9-digit ZIP code of residence experienced at least some flooding according to this measure. To measure neighborhood income, we use the median income of households with a head who is at least 65 years old, as reported in the 2000 Census for the block group containing each

9-digit ZIP code. To minimize measurement error, we do not assign these neighborhood characteristics to the 4.4 percent of sample beneficiaries for whom Medicare only reports a 5-digit ZIP code or to the additional 8.2 percent of sample beneficiaries whose 9-digit ZIP codes are not geocoded by GeoLytics.

3.2 Estimation sample and summary statistics

Although Hurricane Katrina generated a credibly exogenous shock to New Orleans residents, identifying the causal effect of the storm on short- and long-run mortality requires estimating counterfactual mortality outcomes for its victims. Our primary approach to estimating counterfactual outcomes relies on examining how outcomes evolve among groups of Medicare beneficiaries initially residing in one of the 10 control cities identified by Deryugina, Kawano and Levitt (2018). From among U.S. cities with a population of at least 100,000, the authors selected 10 that most closely matched New Orleans in the years 2000–2005 along three demographic dimensions: median earnings, the population growth rate, and the percent of population that is black. These cities are: Baltimore, MD; Birmingham, AL; Detroit, MI; Gary, IN; Jackson, MS; Memphis, TN; Newark, NJ; Portsmouth, VA; Richmond, VA; and St. Louis, MO (see Appendix Figure A.3 for a map). While the matching procedure and our finding that mortality pre-trends were similar in New Orleans and these ten cities support the validity of these cities as controls, we show that our main results change little when using the rest of the United States as the control group or when using the synthetic control method.

Because individuals move or die over time, the cohort of individuals who were alive and eligible for Medicare in 2004 (the "2004 cohort") is the most relevant cohort for assessing the impact of the hurricane among Medicare residents of New Orleans. Thus, individuals in the 2004 cohort initially residing in either New Orleans or one of the 10 control cities form our preferred sample for estimating the long-run effects of Hurricane Katrina. Table 1 shows summary statistics for the characteristics of this sample, which contains nearly 1.3 million beneficiaries. About 35 percent of the individuals are black, 42 percent are male, and the average age at baseline is 71. Eighty-two percent are 65 and older at baseline, implying that the rest (18 percent) qualify for Medicare because of a disability. Median household income among householders aged 65 and older in the Census block group corresponding to a beneficiary's baseline residence averages about \$31,000. In 2004, almost 89 percent of the sample was enrolled in fee-for-service Medicare, spending on average slightly more than \$9,000 each. About 4.8 percent of the sample died that year.

Among individuals in the 2004 cohort who survived until January 1, 2005, 5.2 percent moved between March 2005 and March 2006, with moves defined as a change in the HSA of residence reported in the 2004 and 2005 Medicare eligibility files. The average New Orleans beneficiary experienced almost 2.6 feet of flooding during the storm in his or her 9-digit ZIP code, with a standard deviation of 2.8 feet.

A limitation of the 2004 cohort is that it does not enable us to assess annual mortality trends prior to Hurricane Katrina. To do so, we consider cohorts based on Medicare eligibility and residence in 1992 and 1999. Figure 1 plots raw annual death rates for the 1999 Medicare cohort, by initial city of residence. For example, the 2005 mortality rate for New Orleans is calculated as the 2005 mortality rate among Medicare beneficiaries in the 1999 cohort who survived past 2004 and initially lived in New Orleans, regardless of where they lived in 2005. Mortality rates for the New Orleans cohort are plotted in black, and mortality rates for cohorts from each of the 10 control cities are plotted in blue. To see how New Orleans compares with the rest of the United States, the light gray lines plot mortality rates for the cohorts initially residing in each HRR except the one containing New Orleans.

The raw data plotted in Figure 1 reveal one of the key findings we formally estimate below. Prior to Hurricane Katrina, the New Orleans cohort had one of the highest regional mortality rates in the United States. Cohorts from the 10 control cities also had high mortality rates, falling largely in the top half of the national distribution and trending similarly to the New Orleans cohort. In 2005, the year of Hurricane Katrina, the mortality rate of the New Orleans cohort spiked and became higher than the mortality rate of any other regional cohort in the nation. Yet, remarkably, mortality among the New Orleans cohort fell to the middle of the mortality rate distribution in 2006 and remained there through 2013, the latest year for which we have data. This pattern suggests that Hurricane Katrina led to a long-run decline in mortality among the New Orleans cohort. As we estimate formally below, these decreases are so large that they cannot fully be explained by mortality displacement, or "harvesting," as would occur if Hurricane Katrina killed individuals who would have died soon even in the absence of the hurricane, thereby depressing future mortality rates.

4 Research Design

4.1 Short-run effects of Hurricane Katrina

We estimate the short-run effects of Hurricane Katrina on the mortality of the New Orleans Medicare population using a difference-in-differences event study analysis of the 2004 New Orleans and control city cohorts. We define event week $t = 0$ as the seven-day period beginning on Monday, August 29, 2005, the day Hurricane Katrina struck New Orleans. We then construct a panel data set with observations for each individual i and week t over the 100-week period beginning 34 weeks prior to and ending 65 weeks after Hurricane Katrina, corresponding to weeks starting on January 3, 2005 and on November 27, 2006, respectively. We then estimate

$$Died_{it} = \sum_{\substack{\tau=-34,\\ \tau\neq-1}}^{65} \beta_t \mathbf{1}(t=\tau) \times NOLA_i + [week\ FE] + [base\ ZIP5\ FE] + \varepsilon_{it}, \qquad (1)$$

where the outcome, $Died_{it}$, equals zero if individual i survived through week t and equals one if he or she died that week. If the individual died prior to week t, $Died_{it}$ is missing and the observation is dropped from the regression. As a result, beneficiaries from the 2004 cohort have to survive until January 3, 2005 to be included in this regression. We define a "treatment" indicator $NOLA_i$ as equal to one if individual i lived in New Orleans at baseline and equal to zero otherwise. Fixed effects for the 5-digit ZIP code of an individual's residence in the base year capture baseline geographic differences in mortality rates, while event-week fixed effects capture how mortality evolves over time for the sample as a whole. For this and subsequent regression analyses, standard errors are clustered by baseline ZIP code.

The key parameters of interest in equation (1) are β_t, the coefficients on the interaction of event-week indicators with the New Orleans indicator $NOLA_i$. Thus, β_t nonparametrically captures how the change in the New Orleans cohort's mortality between the reference week and week t differs from the change in the control city cohorts' mortality over the same period. β_t identifies the causal effect of Hurricane Katrina on the New Orleans cohort's mortality rate under the assumption that the mortality rate among the New Orleans cohort would have paralleled the control city cohorts' mortality rates in the absence of the hurricane. The plausibility of this assumption can be assessed by testing for parallel trends in the weeks prior to the storm (i.e. $\beta_t = 0$ for $t < 0$), which motivates the inclusion of the 34 pre-event week indicators in equation (1). Finally, to minimize sensitivity of the results to the choice of reference week, we calculate and report adjusted estimates $b_t = \beta_t - \bar{\beta}_{pre}$, where $\bar{\beta}_{pre}$ is the average value of β_t for $t < 0$ (including β_{-1}, which is mechanically zero). Thus, b_t reflects Hurricane Katrina's mortality effect in week t, relative to average mortality rate differences in the 34 weeks prior to the hurricane.

4.2 Long-run effects of Hurricane Katrina

Annual Mortality and Mobility We estimate the long-run effects of Hurricane Katrina on mortality and relocation using a cohort approach very similar to our short-run weekly analysis, except that we define the time dimension of the panel data to be annual and extend our period of analysis to cover up to eight years after 2005, the year of Hurricane Katrina. Specifically, we include observations for each individual i and year t starting from the base year used to define the cohort (1992, 1999, or 2004) through 2013, omitting any observations after the year in which the individual dies. We then estimate

$$Y_{it} = \sum_{\substack{\tau = Base\,Year, \\ \tau \neq 2004}}^{2013} \beta_t \mathbf{1}(t = \tau) \times NOLA_i + [year\ FE] + [base\ ZIP5\ FE] + \theta X_{it} + \varepsilon_{it}, \qquad (2)$$

where the outcome Y_{it} is a measure of either mortality or residing outside one's baseline city of residence. To capture mortality, we define $Died_{it}$ as equal to zero if individual i survived through year t and equal to one if he or she died that year. To capture relocation, we define $LeftHSA_{it}$ as equal to zero if the individual resided in their baseline HSA in year t and equal to one if he or she was alive and living in another HSA. For simplicity, our preferred specification does not control for baseline demographics beyond the baseline ZIP code fixed effects. Because the residual demographic balance between treatment and control group cohorts may shift over time and thereby influence mortality trends through a change in cohort composition, we also report event study results that include fixed effects X_{it} for all combinations of baseline age (in 5-year bins), race, and sex. All other variables are defined as in equation (1) except that the time period t reflects years instead of weeks and we thus include year fixed effects instead of week fixed effects. Standard errors are clustered by baseline ZIP code, although for robustness we also carry out inference using the synthetic control method with permutation tests conducted at the HSA and HRR levels.

We use 2004, the year prior to Hurricane Katrina, as the reference period so that β_t captures how the change in the New Orleans cohort's mortality between 2004 and year t differs from changes in the control city cohorts' mortality over the same period. As with the weekly analysis, β_t identifies the causal effect of Hurricane Katrina on the New Orleans cohort's mortality rate in a given year under the assumption that the New Orleans cohort's mortality would have paralleled the control city cohorts' mortality rates in the absence of the hurricane. The plausibility of this assumption can be assessed by testing for parallel trends in the years prior to the storm (i.e. $\beta_t = 0$ for $t < 2004$), which can be done when estimating equation (2) for cohorts formed in base years prior to 2004. We estimate equation (2) separately for the 1992, 1999, and 2004 Medicare cohorts. The 1992 and 1999 cohorts allow us to test for pre-trends over a long time horizon, but these cohorts may not adequately capture the experiences of those affected by Hurricane Katrina, as about two-thirds (one-third) of individuals in the 1992 (1999) cohort had moved away or died before 2005.[6] Furthermore, the elderly in the 1992 (1999) Medicare cohort were at least 77 (70) by the time Hurricane Katrina struck. While we cannot estimate pre-Katrina trends for the 2004 Medicare cohort, that cohort includes the most relevant group of Medicare beneficiaries exposed to the hurricane, including younger elderly. Thus, we use the 2004 Medicare cohort to calculate our preferred estimates of the magnitude of Hurricane Katrina's mortality effect.

Cumulative Mortality The annual mortality results obtained from equation (2) can be used to calculate the effect of Hurricane Katrina on changes in cumulative mortality for the New Orleans cohort. Specifically, for each post-Katrina year t between 2005 and 2013, the change in cumulative mortality ΔM_t is given by

$$\Delta M_t = \sum_{\tau=2005}^{t} S_\tau \beta_\tau, \tag{3}$$

where β_τ are the annual mortality effects of Hurricane Katrina and S_τ is the empirical fraction of the New Orleans cohort who are alive at the start of 2005 and survive to the start of year τ. We estimate ΔM_t and its standard error using the estimates $\widehat{\beta}_t$ from equation (2). The term S_τ in equation (3) is a "discount factor" reflecting the impact of a mortality rate change β_τ at time τ on the cumulative mortality of those who are alive in 2005. Note that $S_{2005} = 1$, and thus $\Delta M_{2005} = \beta_{2005}$, i.e., the cumulative mortality effect equals the effect on the mortality rate in the first year. Because survival decreases weakly over time, changes in the mortality rate later in time matter less than earlier changes, holding all else equal. For example, a percentage-point increase in the mortality rate this year followed by a

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[6] By 2004, 7,376 of the 83,677 individuals (8.8%) from the 1992 New Orleans cohort were living outside of New Orleans and 45,912 (54.9%) had died. Among the 1999 New Orleans cohort, 6,612 of the 81,690 individuals (8.1%) were living outside of New Orleans and 20,758 (25.4%) had died by 2004.

percentage-point decrease in the mortality rate next year results in a cumulative mortality *increase* because individuals are more likely to experience the increase than to experience the decrease.

Concise Difference-in-Differences Event study estimates from equation (2) nonparametrically identify treatment effects over time and can also be used to gauge pre-trends to assess the plausibility of assuming parallel trends in outcomes between the New Orleans and control cohorts. If there are no pre-trends and if the treatment effect is constant over a period of time, a more efficient approach is to combine years into longer periods. To that end, we group years into a pre-treatment reference period (base year through 2004), the year of treatment (2005) for capturing short-run effects, and a post-treatment period (2006–2013) for estimating long-run effects. Specifically, we estimate

$$\begin{aligned} Y_{it} = {} & \beta_{SR}\mathbf{1}(t = 2005) \times NOLA_i + \beta_{LR}\mathbf{1}(t \geq 2006) \times NOLA_i \\ & + [\mathit{year\ FE}] + [\mathit{base\ ZIP5\ FE}] + \theta X_{it} + \varepsilon_{it}. \end{aligned} \tag{4}$$

The indicators $\mathbf{1}(t = 2005)$ and $\mathbf{1}(t \geq 2006)$ denote whether the year of observation is 2005 or falls within the period 2006–2013, respectively. As with equation (2), we include year and baseline ZIP code fixed effects. For robustness, some specifications include additional controls X_{it}, such as baseline demographics. The coefficients β_{SR} and β_{LR} thus describe the average short-run (2005) and long-run (2006–2013) causal effects, respectively, of Hurricane Katrina on mortality among the New Orleans cohort under the same identification assumption required for interpreting equation (2) estimates as causal.

Heterogeneous Treatment Effects We estimate heterogeneity in treatment effects with respect to a variety of baseline characteristics, including flooding from Hurricane Katrina in one's 9-digit ZIP code of residence, age, race, income, and the presence of various chronic conditions. To do so, we augment the event study or concise difference-in-differences specification (equations (2) and (4), respectively) to include interactions between the treatment indicators and an indicator for the characteristic of interest. For example, we augment equation (4) as follows:

$$\begin{aligned} Y_{it} = {} & \beta_{SR}\mathbf{1}(t = 2005) \times NOLA_i + \beta_{SR}\mathbf{1}(t = 2005) \times NOLA_i \times H_i \\ & + \beta_{LR}\mathbf{1}(t \geq 2006) \times NOLA_i + \beta_{LR}\mathbf{1}(t \geq 2006) \times NOLA_i \times H_i \\ & + \gamma NOLA_i \times H_i + [\textit{year-by-}H_i \textit{ FE}] + [\textit{base ZIP5 FE}] + \varepsilon_{it}, \end{aligned} \tag{5}$$

where H_i indicates whether individual i has the characteristic of interest at baseline. Because outcome levels at baseline may differ by the chosen characteristic within New Orleans and between New Orleans and control cities, we also control for each characteristic and its interaction with the New Orleans indicator ($NOLA_i \times H_i$). Furthermore, to allow for differential secular trends, we include full interactions between the characteristic and year fixed effects whenever there is variation in the characteristic within the control cohort, with one exception. Because there was no flooding from Hurricane Katrina in the control cities, heterogeneity analysis by the flood level of an individual's residence at baseline includes year and flood level fixed effects rather than flood-by-year fixed effects.

4.3 Mechanisms: migration and place

To examine the role of relocation in determining mortality risk following Hurricane Katrina, we estimate how mortality outcomes of individuals displaced by the hurricane depend on characteristics of the region to which they moved. To do so, we restrict our sample to individuals in the 2004 New Orleans cohort who survived through 2005 and moved to another HSA at some point between March 2005 and March 2006. Plausibly, most of these migrants left New Orleans in the aftermath of Hurricane Katrina. To avoid conflating local characteristics with Hurricane Katrina's impact in the vicinity of New Orleans, we further exclude from the migrant sample individuals who moved to an HSA in the same Hospital Referral

Region as the New Orleans HSA.

We estimate the relationship between a New Orleans mover's post-Katrina (2006–2013) annual mortality rate and the average annual post-Katrina mortality rate of the HSA in which mover i resided in 2006, which we denote by $MDR_{2006HSA(i)}$.[7] To avoid a mechanical relationship between migrant mortality outcomes and our measure of destination mortality, we calculate $MDR_{2006HSA(i)}$ as the annual mortality rate of the HSA's 2004 Medicare cohort (i.e., of Medicare beneficiaries who lived in that HSA as of March 2005) averaged over 2006–2013. We then estimate

$$Died_{it} = \gamma MDR_{2006HSA(i)} + [year\ FE] + [base\ ZIP5\ FE] + \theta X_{it} + \varepsilon_{it}. \tag{6}$$

Because we do not include non-New-Orleans individuals in this empirical exercise, it is not necessary to have New Orleans indicators in equation (6). All remaining control variables are defined as before. The coefficient γ describes the causal effect of place, as captured by local mortality, on migrant mortality under the assumption that migrants do not sort to high- or low-mortality regions based on unobserved mortality risk. When we present the results, we evaluate the plausibility of this assumption by assessing the degree of sorting along observable risk factors, as well as sensitivity of estimates of γ to the inclusion of rich controls, including baseline demographics, medical spending, and chronic conditions.

We also estimate how migrant mortality varies by average medical spending in the destination region by replacing $MDR_{2006HSA(i)}$ in equation (6) with the average local Medicare spending in each mover's 2006 destination HSA. Analogous to how we defined local mortality rates, we define spending in an HSA as the average annual Medicare spending per fee-for-service beneficiary in 2006–2013, using the HSA's 2004 Medicare cohort. As with local mortality, the estimated relationship between local medical spending and migrants'

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[7] In principle, we could let the local mortality rate $MDR_{2006HSA}$ change each year for individuals who continue moving. However, in our setting this is problematic because a non-trivial share of our movers return to New Orleans in the longer run. As a result, we would either have to drop those individuals from our sample in those years—which would likely bias the estimates—or use the New Orleans mortality rate, which was clearly affected by Hurricane Katrina.

mortality captures the causal effect of living in a low- or high-spending place under the assumption that migrants are not differentially selected into low- and high-spending areas based on unobserved mortality risk.

5 Results

5.1 Short-run effects of Hurricane Katrina

Figure 2a reports raw weekly mortality rates for the 2004 New Orleans and control city cohorts. Figure 2b reports the corresponding weekly difference-in-differences mortality effects of Hurricane Katrina from equation (1), adjusted such that the reference period is the 34 weeks prior to the hurricane (as described in Section 4.1).[8] The gray dashed line 14 weeks after the hurricane indicates the week of FEMA's "look-and-leave"/"look-and-stay" announcement on December 9, 2005; prior to this date, most New Orleans residents were formally prohibited from returning to their homes. The lack of differential trends in mortality prior to Hurricane Katrina supports interpreting the post-Katrina estimates as causal effects of the hurricane on mortality rather than pre-existing differences between treatment and control individuals.

Perhaps unsurprisingly, the mortality increase is heavily concentrated in the week of Hurricane Katrina. That week, the New Orleans cohort's mortality increased by 5.2 deaths per thousand (0.52 percentage points), which accounts for 95 percent of the excess 2005 mortality we identify later in our annual analysis. Relative to the average of 1.37 deaths per thousand beneficiaries in the sample we use for this analysis, the mortality rate nearly quadrupled during the week of Katrina. We also see statistically significant increases in mortality for as long as 8 weeks after landfall. While the estimates are about an order of magnitude smaller (0.011–0.075 percentage points), they nonetheless represent large relative mortality increases (8–55 percent). In the subsequent 55 weeks, none of the positive point

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[8] Numerical values for a subset of the estimates plotted in Figure 2 are reported in Appendix Table A.1.

estimates is significant, even at the 10 percent level, while two of the negative estimates are significant at the 5 percent level.

5.2 Long-run effects of Hurricane Katrina

5.2.1 Annual mortality and mobility

Next, we use annual data to estimate the longer-run effect of Hurricane Katrina on the mortality and mobility of the elderly and long-term disabled. Figure 3 shows estimated changes in annual mortality and mobility from equation (2) (solid black lines) as well as changes in cumulative mortality given by equation (3) (dashed lines) for the 2004 Medicare cohort.[9] The top graph shows the estimated effect of Hurricane Katrina on mortality. In the year of Hurricane Katrina, the mortality rate of New Orleans residents increased by over half a percentage point. This increase corresponds to about 10 percent of the average annual mortality rate for this cohort, which is particularly large given that these additional deaths occurred in the last four months of the year.

Remarkably, this mortality increase quickly reversed and became a mortality *reduction*: the death rate fell below pre-Katrina levels in 2006 and remained below them for the rest of the sample period (although not all the estimates are statistically significant at the 5 percent level). In almost every year after 2005, the death rate for New Orleans elderly is at least a quarter of a percentage point lower than that for the controls.

After an initial increase in 2005, changes in cumulative mortality (dashed lines) fell and became negative beginning in 2008. Thus, mortality displacement or harvesting can explain at most two years of post-Katrina mortality reductions. The change in cumulative mortality became increasingly negative throughout the post-Katrina period, reaching -1.74 percentage points in 2013 for the 2004 cohort. That is, by the end of our sample period, this cohort of Hurricane Katrina victims was 1.74 percentage points *more* likely to be alive than members

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[9] Event study results are similar if we include fixed effects for all combinations of baseline age (in 5-year bins), race, and sex (see Appendix Figure A.4). Numerical values of the point estimates and standard errors shown in Figure 3 can be found in Appendix Table A.2.

of the control group, despite no significant differences in survival probability prior to the hurricane. About 63 percent of the 2004 cohort survived through 2013. Thus, relative to the average survival rate over this time period, a decrease in cumulative mortality of 1.74 percentage points represents a survival improvement of 2.7 percent.

Using a value of $100,000 per life year (Cutler, 2004) and a discount rate of 3 percent (Siegel, 1992), we calculate the net present value of the changes in cumulative mortality brought about by Hurricane Katrina over the period 2005–2013. The discounted value of the cumulative mortality effects plotted in Figure 3 is $4,853 per capita ($6,010 per capita without discounting). Because the cumulative mortality reduction likely persisted beyond 2013, this figure plausibly provides a lower bound on the value of the mortality reduction. For our sample of 76,436 elderly and disabled victims from the 2004 New Orleans cohort who were alive as of January 1, 2005, the implied aggregate value of the mortality reduction over the period 2005–2013 is about $371 million ($459 million without discounting). Because Hurricane Katrina had many negative consequences that are not captured by mortality outcomes, these estimates do not imply that victims' aggregate welfare increased as a result of the hurricane.

Finally, the bottom graph in Figure 3 shows the effect of Hurricane Katrina on Medicare beneficiaries' mobility. In 2005, Hurricane Katrina displaced about 42 percentage points more individuals than would have otherwise left, and most of the displaced stayed away in 2006. They began returning slowly in 2007; however, by 2013, those from the New Orleans cohort who were alive were still about 24 percentage points less likely to be living in their baseline city than were individuals from the control city cohorts. Thus, a large share of New Orleans elderly and disabled left the city after Hurricane Katrina and never returned.

5.2.2 Robustness

Individuals in the 2004 Medicare cohort must have been alive on January 1, 2004 to be included in our sample. Thus, we must use earlier Medicare cohorts to compare pre-Katrina mortality trends of New Orleans and the control city cohorts. In Figure 4, we re-estimate equation (2) for the mortality rate of the 1992 and 1999 Medicare cohorts.[10] For both cohorts, mortality trends are very similar between the New Orleans and control city cohorts, with no statistically significant differences. The post-Katrina differences in mortality rates are also broadly similar across the cohorts.[11]

In the Online Appendix, we replicate the mortality results displayed in Figure 3 using the entire 1992, 1999, and 2004 U.S. cohorts of Medicare beneficiaries as a control for each New Orleans cohort (see Appendix Figure A.5 and Appendix Table A.5). Each regression includes at least 340 million observations (the number of individuals times the number of years in which they were alive during the sample period). As with our smaller control group, we see no differential pre-trends in mortality prior to the hurricane, even with the 1992 cohort, which we observe for 13 years prior to the hurricane. We obtain qualitatively similar but quantitatively larger and more significant estimates of the post-Katrina reductions in the mortality rate, indicating that the cumulative mortality of the New Orleans cohorts decreased by 2.1–3.6 percentage points by the end of 2013. The similarity of our baseline results to those obtained from using the rest of the United States as the control group demonstrates that our results do not hinge on the particular choice of 10 cities as the main controls.

Finally, we probe the robustness of our baseline method of inference, which allows for clustering at the ZIP code level. Because the entire city of New Orleans was affected by Hurricane Katrina, our setting could reasonably be viewed as a case with only one treated unit, which presents a challenge for reliable inference. Test statistics based on cluster-robust standard errors will over-reject when there is only a single treated group (Conley and Taber, 2011), while those based on the wild cluster bootstrap can either over- or under-reject (MacK-

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[10] Numerical values of the mortality event study point estimates and standard errors shown in Figure 4 can be found in Appendix Table A.3. Mobility event study estimates for the 1999 cohort are shown in Appendix Figure A.6.

[11] The 2005 increases in the mortality rate for the 1992 and 1999 New Orleans cohorts are even larger than that of the 2004 cohort. This is likely due to elderly individuals in the former cohorts being at least 77 and 70 years old, respectively, at the time of Hurricane Katrina, which may have made them more susceptible to the short-run negative effects of the disaster than younger Medicare beneficiaries.

innon and Webb, 2017). As described in more detail in Appendix Section A.2, we address these challenges by re-estimating the mortality effects of Hurricane Katrina for the 1999 cohort using the synthetic control method of Abadie, Diamond and Hainmueller (2010, 2015), which yields point estimates similar to those of our regression approach. Following Abadie, Diamond and Hainmueller (2015), we use permutation inference to assess the statistical significance of the results by comparing the synthetic control estimate for New Orleans to the distribution of "placebo" estimates obtained from assigning treatment status to each of the units (treatment or control). As in our baseline event study analysis, the estimated treatment effects for New Orleans are almost always outliers at the 5% level or better, leading to similar inference as when we cluster our regression standard errors at the ZIP code level.

5.2.3 Concise difference-in-differences

Table 2 shows mortality estimates from equation (4) for the 2004 cohort (columns (1)–(3)) and the 1999 cohort (columns (4)–(6)). In addition to our preferred specification (columns (1) and (4), labeled "A" in the table), we also show results we obtained by adding fixed effects for all 5-year-age-bins-by-gender-by-race combinations (labeled "B") and where we additionally allow the year fixed effects to vary by each 5-year-age-bin-by-gender-by-race combination (labeled "C").

Overall, the point estimates remain stable across control specifications and are similar in magnitude to those obtained in the event study, but more precisely estimated. The estimated immediate (2005) mortality increase for the 2004 cohort ranges from 0.54 to 0.56 percentage points. In 2006–2013, the 2004 New Orleans cohort experienced a significant decline in its mortality rate of 0.28–0.39 percentage points. The 1999 New Orleans cohort likewise experienced a short-run mortality rate increase, with an estimated magnitude ranging from 0.80 to 0.88 percentage points across control specifications. In the longer run, annual mortality declined by 0.20–0.43 percentage points.

5.2.4 Heterogeneous treatment effects

The aggregated results presented above could mask heterogeneity in the mortality effects of Hurricane Katrina across subpopulations of victims. We use equation (5) to estimate heterogeneous treatment effects along various dimensions, including the extent of flooding in one's neighborhood, baseline demographics, and pre-existing chronic conditions. Appendix Table A.4 presents the complete set of findings, which we summarize here.

We find that short-run mortality rates increased more among individuals initially living in flooded parts of New Orleans compared to those living in non-flooded areas, but long-run mortality rates also decreased more for individuals in flooded areas, although this difference is not statistically significant. At the same time, individuals living in flooded neighborhoods were much more likely to relocate away from New Orleans.[12] Together, these patterns suggest that the severity of flooding in an individual's neighborhood may have indirectly generated long-run health benefits by leading people to move to places that cause better health outcomes. To examine this possibility directly, we estimate the mortality effects of location in the next section.

When we look at heterogeneity by age, we find that the short-run increase in mortality is concentrated among Medicare beneficiaries aged 65 and older at baseline, while the mortality of those who were younger than 65 was unaffected in 2005.[13] However, the 2006–2013 mortality gains are similar for these two populations, supporting our decision to combine them in our main analyses. A general takeaway from examining other dimensions of heterogeneity is that long-term mortality reductions following Hurricane Katrina do not appear to be limited to narrow subsets of New Orleans victims. Even individuals that seem more vulnerable *ex ante*, such as those with chronic conditions, did not experience increases in long-run mortality, and the long-run survival gains for low-income individuals are statistically *larger* that those for higher-income individuals. Finally, there is suggestive evidence

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[12] See Appendix Figure A.7 for event studies of mortality and mobility by flooding.

[13] Appendix Figure A.4 shows mortality event study results from estimating equation (2) separately for individuals who were either younger than 65 or at least age 65 at baseline.

that blacks experience lower immediate mortality increases and larger subsequent mortality decreases, although the differences are not statistically significant.

5.3 Mechanisms

Thus far, we have shown that Hurricane Katrina led to significant declines in long-run mortality among the elderly and disabled. This result, in isolation, is counterintuitive, as natural disasters are unlikely to have positive *direct* effects on health. A natural hypothesis, then, is that the mortality improvements following Hurricane Katrina came about indirectly, through other effects of the hurricane. Such indirect benefits have been demonstrated in other contexts. For example, Sacerdote (2012) finds that Katrina and Rita student evacuees experienced long-run improvements in test scores, likely because they transferred to better schools. Relatedly, Deryugina, Kawano and Levitt (2018) find long-run increases in New Orleans victims' earnings brought about because Hurricane Katrina both forced individuals to relocate to stronger labor markets and strengthened the New Orleans labor market. In our context, Hurricane Katrina may have increased long-run survival rates by causing elderly and disabled individuals to move to areas more conducive to better health or by generating health care quality improvements in New Orleans itself (e.g., Marsa, 2015).

5.3.1 The importance of rebuilding in New Orleans

We first consider whether improvements in New Orleans following Hurricane Katrina help explain the aggregate mortality improvements we estimate. There are two key challenges for directly testing how the hurricane affected mortality among individuals who remained in New Orleans. First, because the decision to move is observed only for individuals who survived the initial shock of the hurricane, we cannot estimate stayer-specific difference-in-differences mortality effects using pre-Katrina as a reference period. Second, differences in mortality levels between stayers and movers are also unlikely to be informative of the relative effect of staying in New Orleans since, as we show later, the decision regarding whether to leave or stay was highly correlated with observable predictors of mortality risk.

New Orleans infrastructure was devastated following Hurricane Katrina, however, and therefore it is likely that any health improvements accruing to New Orleans stayers would have developed over time during rebuilding. To empirically test this intuition, we restrict the sample to individuals from the 2004 cohort who survived until at least the beginning of 2006. We then estimate equation (2) with only "stayers" (defined by individuals' location as of March 2006). Because survival until 2006 is necessary for inclusion, the reference category is 2006. Figure A.9 plots the results, which show that stayers' mortality did not improve over time relative to 2006. This finding suggests it is unlikely that the cohort-level mortality declines among hurricane victims reflect health improvements from remaining in New Orleans.

5.3.2 The importance of place: improving survival through migration

Determinants of Migration Next, we consider whether migration to other regions contributed to the long-run mortality declines among New Orleans hurricane victims. The elderly and disabled mortality rate in New Orleans was among the highest in the country prior to Hurricane Katrina, so individuals displaced by the storm generally relocated to places with better health outcomes. To the extent that regional mortality differences reflect causal effects of place, migrant health may have improved as a result of the move. To examine the role of place in mortality outcomes, we focus on individuals who were displaced by the hurricane and relate their subsequent mortality outcomes to the local mortality rate and local average Medicare spending of the region to which they moved, as outlined in Section 4.3 and captured by equation (6). This relationship describes the causal effect of place, as captured by mortality rates and local spending, on individual mortality under the assumption that baseline mortality risk among those who move is uncorrelated with mortality rates and local spending in the destination region. A primary threat to this identification assumption is that migrants with lower latent mortality risk may sort to destination regions with systematically different mortality rates or local spending.

We assess the scope for this kind of sorting in two ways. As a first test of differential sorting, we directly estimate how migrants' baseline observable risk factors—demographics and chronic conditions—vary with the characteristics of the region into which these individuals move. As shown in columns (1)–(2) of Table 3, individuals who moved out of New Orleans differed substantially from those who stayed along many of the observable risk factors we consider: Medicare beneficiaries who were black, female, disabled, and those who had below median income, greater storm flooding, higher medical spending in 2004, or respiratory disease were all more likely to move, holding all else equal, while individuals with cancer were less likely to leave the city. However, because this test focuses only on the decision to leave New Orleans and not on sorting among those who moved, it neither validates nor invalidates the identification assumption of our movers exercise.

Columns (3)–(4) of Table 3 provide a direct test of sorting to regions with relatively higher or lower mortality. Column (3) contains predictors that are available for all New Orleans movers with a geocoded 9-digit ZIP code, while column (4) contains additional chronic condition variables that are available only for a subset of individuals enrolled in traditional Medicare. In contrast to the strong selection observed for the decision to move out of New Orleans, we observe few differences in observable characteristics between those who moved to regions with higher or lower mortality. In column (3), where we do not control for chronic conditions, the estimates indicate that being male and having end-stage renal disease are associated with moving to a higher-mortality area. When we add chronic conditions in column (4), we see that individuals with higher medical spending at baseline and those with Alzheimer's disease or dementia moved to places with slightly higher mortality, while the other 14 characteristics are insignificant. The p-value of the F-statistic for all the variables in our most comprehensive specification displayed in column (4) is 0.11, indicating little overall correlation between destination mortality rates and baseline characteristics of movers. Similarly, columns (5)–(6) of Table 3 show that only one of the 16 baseline characteristics we examine for migrants is an individually significant predictor of local spending in the area to which they moved (individuals with respiratory disease move to lower-spending areas). The p-value of the F-statistic for all the variables in the comprehensive specification displayed in column (6) is 0.47, further suggesting that sorting appears not to be an important confounder in this setting.[14]

Local Mortality, Local Spending, and Movers' Mortality We begin by examining how movers' mortality rates depend on mortality rates in the destination HSA. As shown in column (1) of Table 4, Panel A, destination mortality rates are strongly associated with movers' own mortality rates for 2006–2013: each percentage-point reduction in the destination region's 2004 cohort's 2006–2013 mortality rate corresponds to a 0.83 percentage-point reduction in the 2006–2013 mortality rate of individuals from New Orleans who move to that region.

As a second test for whether selection may be driving the relationship between mover mortality and destination region mortality rates, we re-estimate equation (6) using increasingly comprehensive controls, including indicators for each possible interaction between the eight groups of chronic conditions described earlier. Whenever we control for chronic conditions, which restricts us to a subset of fee-for-service beneficiaries, we also control for ten deciles of 2004 Medicare spending. As reported in columns (2)–(5) of Table 4, Panel A, the estimated coefficient on destination mortality changes little, ranging from 0.83 to 0.90 across these specifications. The estimate of 0.86 reported in column (5)—obtained by controlling for baseline ZIP code fixed effects, ten deciles of baseline Medicare spending, all possible interactions of the eight chronic conditions groups, and separate year fixed effects for all combinations of 5-year age bins, gender, and race—is very similar to the estimate of 0.83 reported in column (1) with only year and baseline ZIP code fixed effects. The stability of this estimate across the various sets of controls further suggests that significant migrant sorting on latent mortality risk is unlikely in this context.

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[14] Results that include baseline ZIP code fixed effects are similar (see Appendix Table A.6).

Figure 5a shows a graphical representation of the regression in column (3) of Table 4. Specifically, we residualize the mortality of the destination HSA by the remaining control variables and fixed effects, group the resulting residuals into ten deciles, and show the mean residualized mover mortality for each decile, with the sample means added for convenience. The resulting relationship between local mortality and movers' subsequent mortality looks approximately linear, validating that the findings in Table 4 are not driven by a few outliers.

In Appendix Table A.7, we extend these results by separating the post-Katrina years into two periods: 2006–2007 and 2008–2013. We find a strong relationship between local mortality and movers' mortality as early as 2006–2007, suggesting that migrant mortality rates are not shaped solely by slow-moving channels such as lifestyle changes. More generally, the speed with which individuals' mortality rates converge to the local rate makes it unlikely that this convergence is primarily due to individuals' becoming more or less likely to develop chronic conditions. Rather, faster-moving channels such as the quality of the local health care system or other environmental factors appear to be driving both local mortality rates and the mortality rates of new arrivals.

In Appendix Table A.8, we further extend these results by considering how movers' mortality rates from specific causes of death relate to destination mortality rates. We divide causes of death into four comprehensive categories: cardiovascular, cancer, other internal, and external. For reference, column (1) reproduces the all-cause estimate from column (3) of Table 4, Panel A. We find that cardiovascular, cancer, and other internal causes of death are all significantly associated with the destination mortality rate, but we find no relationship with respect to external causes of death. The largest response occurs among other internal causes, although our standard errors are sufficiently large such that we cannot reject equality of the three significant coefficients. These results indicate broad mortality benefits of moving to a lower-mortality place that are not limited to mortality from a narrow set of causes.

Next, we examine how movers' mortality rates depend on average Medicare spending per beneficiary in the destination HSA. Numerous studies have found that regional spending levels are not associated with better health outcomes, including mortality (e.g., Fisher et al., 2003*a*,*b*; Baicker and Chandra, 2004; Sirovich et al., 2006), suggesting that the returns to additional medical spending may be low (e.g., Fisher, Bynum and Skinner, 2009; Cutler, 2010; Skinner and Fisher, 2010). Likewise, in our setting there is a wide distribution of local mortality rates and local spending in the movers' destination regions, with a positive but small and statistically insignificant association between the two (see Appendix Figure A.8). However, worse population health may lead to higher spending, which could result in a net zero or even positive correlation between local spending and mortality even if the returns to living in a higher spending region are positive. The mass migration caused by Hurricane Katrina provides a novel opportunity to estimate the returns to living in a higher- or a lower-spending region, using measures of regional spending that are separate from the migrants' health and spending.

We perform this test by replacing the local mortality rate in equation (6) with local average spending in the destination region. As reported in Panel B of Table 4, we estimate small effects: a $1,000 increase in an area's average medical spending is associated with a (statistically insignificant) 0.02 to 0.06 percentage-point reduction in movers' mortality. Figure 5b shows a graphical representation of column (3), confirming the absence of any relationship. In Appendix Table A.9, we show that considering local mortality and local spending jointly yields similar results as when considering them separately.

In Appendix Table A.10, we show the results of estimating the relationship between movers' own mortality and the mortality in their destination HSA using individuals' locations from the 2006 Medicare eligibility files (i.e., locations as of March 2007). This allows us to exclude any short-term moves as well as moves that may have not been reported by the beneficiaries until later in 2006. The coefficients on local mortality are slightly lower than, but similar to, the baseline estimates. In Appendix Table A.11, we further demonstrate that our results are robust to controlling for each of the 27 chronic condition indicators separately, excluding Houston and Baton Rouge (the two most common destinations for

New Orleans movers), and dropping individuals with Alzheimer's (who exhibit the largest amount of sorting and who may not be making their own moving decisions).

Taken together, these results demonstrate the importance of place—as characterized by local mortality rates but not by local Medicare spending—in shaping health outcomes even (or especially) later in life. We emphasize two important considerations regarding the interpretation of this finding. First, because almost half of the beneficiaries who leave New Orleans after Hurricane Katrina and survive eventually return (Figure 3b), our estimates of the relationship between local mortality and movers' subsequent mortality are likely lower than they would be if the relocation was permanent, and therefore they plausibly provide a lower bound on the long-run effect of place.[15] Second, we are largely agnostic as to the ultimate factors behind our finding that place has a causal effect on mortality. In principle, we could incorporate additional local destination characteristics into the movers analysis, such as pollution levels, climate, crime rates, or health care quality metrics. However, attributing causality to any of these local characteristics requires that they are not correlated with any other local factors that themselves affect mortality outcomes but are excluded from the estimating equation. Such an exercise requires carefully addressing the possibility of omitted variable bias, for example by jointly examining a multitude of local characteristics. We therefore leave the exploration of the specific causal channels to future research.

Finally, we perform a back-of-the-envelope evaluation of the extent to which migration to lower-mortality regions can account for the average mortality decline among the New Orleans cohort over the period 2006–2013 (Table 2). For individuals in the 2004 New Orleans cohort surviving past 2005, we first identify their HSA of residence as of March 2006. To measure the changes in mortality exposure following Hurricane Katrina, we then calculate the difference in mortality between each individual's 2006 HSA and the New Orleans HSA, using the 2004 mortality rate of each area's 2004 cohort.[16] Calculated in this way, the

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[15] Movers to higher-mortality areas are no more or less likely to return to New Orleans than movers to lower-mortality areas (see Appendix Table A.12).

[16] Calculating mortality rate differences over the period 2006–2013 instead of 2004 would adhere most closely to our movers' regression framework, but that figure for New Orleans would be confounded by the

local mortality change experienced by Hurricane Katrina victims averages -0.23 percentage points (including individuals who remained in New Orleans, for whom the difference is zero). Using our most carefully controlled estimates of the 2006–2013 mortality reduction brought about by Hurricane Katrina (-0.28, column (3) of Table 2) and of the relationship between local mortality rates and Katrina victims' own subsequent mortality (0.86, column (5) of Table 4), these changes in local mortality explain 71 percent ($= 0.86 * 0.23/0.28$) of the average long-run mortality decline caused by the hurricane.

There are a number of factors that may explain the remaining 29 percent of the 2006–2013 mortality decline. First, the initial mortality decline may reflect mortality displacement, although our cumulative mortality results show that harvesting cannot explain the persistent mortality reduction. Second, the mortality decline may be due in part to effects that were uncorrelated with whether or where victims moved. For example, the disaster may have increased resilience among the elderly (Adams et al., 2011). Third, it is possible that earnings gains experienced by Hurricane Katrina victims (Groen, Kutzbach and Polivka, 2016; Deryugina, Kawano and Levitt, 2018) contributed to mortality improvements, although it should be noted that our sample consists mainly of retired and disabled individuals, making this channel less likely. Fourth, elderly and disabled victims may have become more likely to move in with or closer to relatives. This mechanism is difficult to evaluate with existing data, but to the extent that the propensity to move in with relatives is uncorrelated with local mortality, its presence should not affect our conclusion.[17] Finally, some of the long-run mortality decline following Hurricane Katrina may be driven by where people move, but based on local factors that are uncorrelated with a region's mortality rate or local spending. In this case, our estimate of the share of the mortality decline that can be attributed to moving is a lower bound on the effect of place on mortality.

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effects of the hurricane. If counterfactual cohort mortality rates trend in parallel across regions, differences in 2004 mortality rates provide an unbiased, although perhaps less precise, estimate of longer-run differences.

[17] Between October 2005 and October 2006, the Current Population Survey collected information on Hurricane Katrina evacuees and whom they were living with. However, the sample size of elderly evacuees (58 individuals in the March 2006 survey) is too small for meaningful statistical inference.

6 Conclusion

Hurricane Katrina devastated the City of New Orleans and other parts of the Gulf Coast, causing billions of dollars' worth of direct damage and displacing over one million individuals from their homes. However, the hurricane appears to have come with a silver lining: the elderly and long-term disabled living in New Orleans at the time of the hurricane experienced non-trivial reductions in long-run mortality. We do not find evidence that these reductions are due to improvements in New Orleans itself. Instead, our analysis suggests that relocation to areas with better mortality outcomes can explain about 70 percent of the post-Katrina mortality decline among the elderly and disabled. By contrast, regional differences in health care spending largely fail to explain the observed mortality reductions.

While we find that Hurricane Katrina reduced long-run mortality rates, these effects do not necessarily imply that individuals' *welfare* increased, as the destruction of physical assets and lost utility due to displacement may have more than offset any indirect benefits of the hurricane. We estimate that changes in mortality due to the hurricane—inclusive of the initial mortality shock—are worth almost \$4,900 per capita. Given that moving costs have been estimated to be as high as \$300,000 for some populations (Kennan and Walker, 2010), New Orleans residents may not have voluntarily relocated for these mortality benefits alone.

To the best of our knowledge, our paper is the first to show that one's location of residence has a causal effect on mortality, adding to a growing body of work on the importance of place for shaping individual choices and well-being. The speed with which movers' mortality rates respond to the local mortality rate also suggests that health capital may be able to accumulate or depreciate more rapidly than the canonical model of Grossman (1972) implies, at least for the population in our study. Finally, we are able to convincingly estimate the effect of a natural disaster on long-run mortality, something that data challenges have hindered in the past. Our conclusion that Hurricane Katrina reduced mortality by inducing relocation demonstrates the importance of accounting for local public health conditions when projecting the long-run impacts of disasters.

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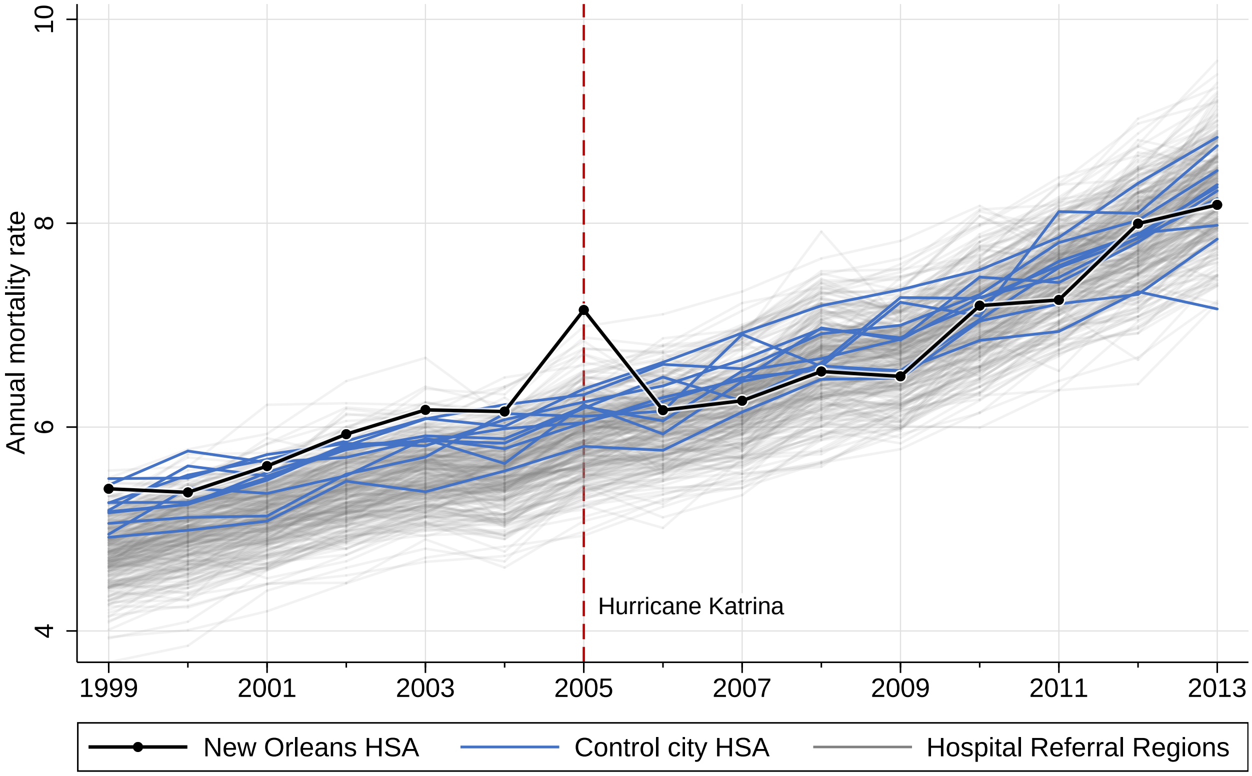
Figures

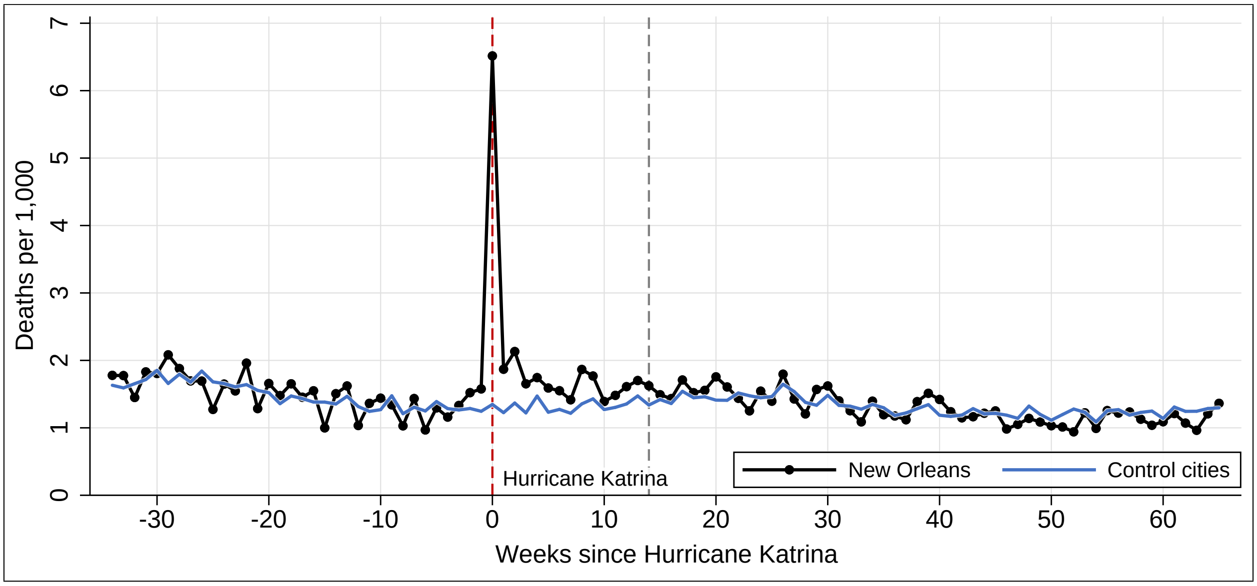
Figure 1: Mortality rates in New Orleans versus other areas (1999 Medicare cohort)



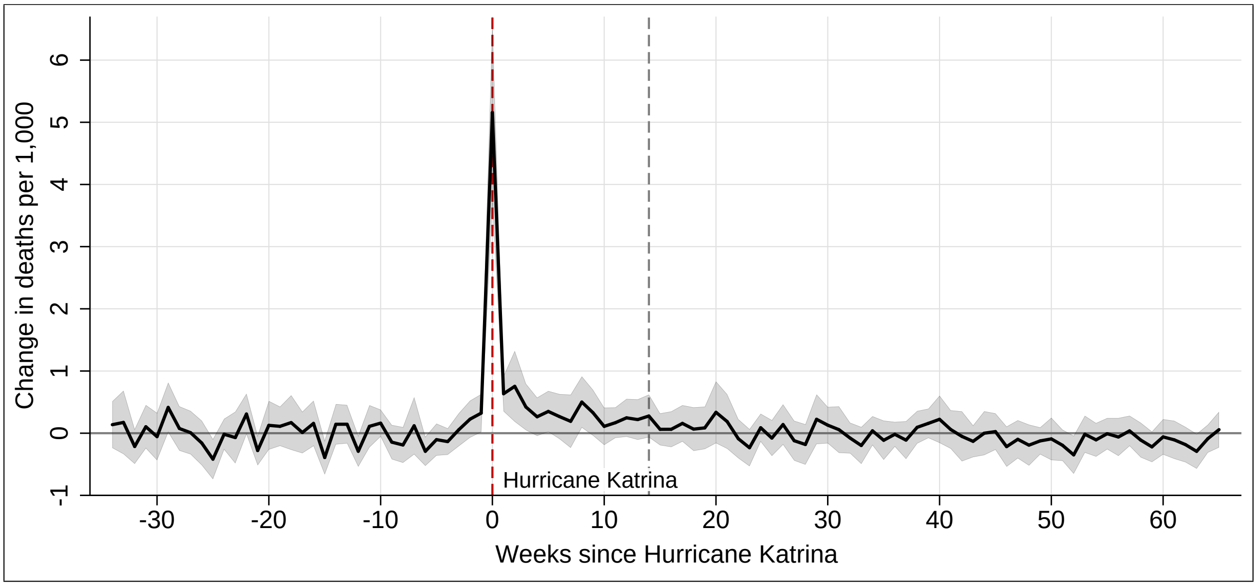
Notes: The figure shows raw annual death rates of 1999 Medicare cohorts by initial region of residence. Mortality rates for the New Orleans cohort ("New Orleans HSA") are plotted in black, and mortality rates for 1999 cohorts from each of the 10 control cities ("Control HSAs") are plotted in blue. The light gray lines plot mortality rates for the cohorts initially residing in each U.S. Hospital Referral Region except the one containing New Orleans.

Figure 2: Short-run effects of Hurricane Katrina (2004 Medicare cohort)

(a) Raw weekly mortality rates



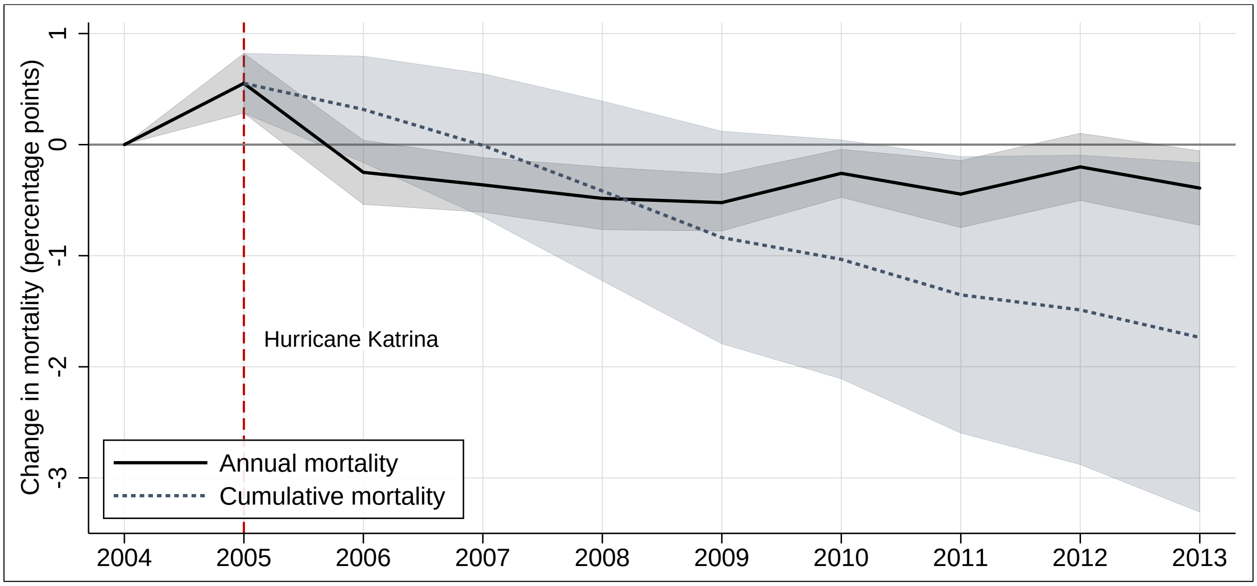
(b) Difference-in-differences event study



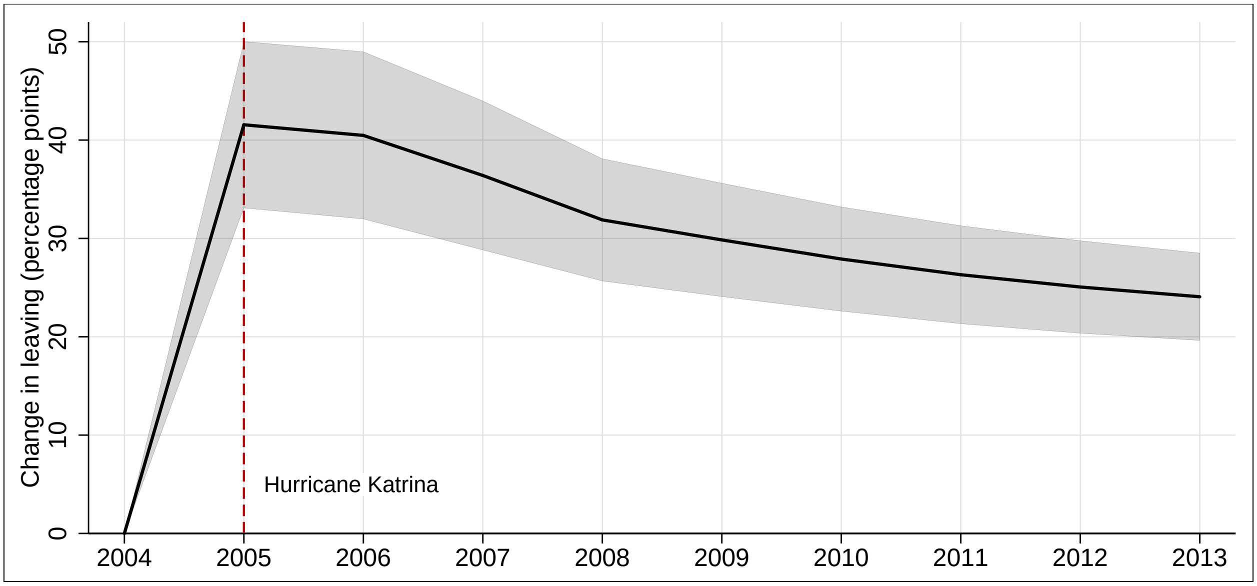
Notes: Panel (a) shows raw weekly mortality rates over the 34 weeks preceding and the 65 weeks following Hurricane Katrina for the New Orleans cohort (black line) and for the control cities cohort (blue line). Panel (b) shows difference-in-differences estimates and corresponding 95 percent confidence intervals from equation (1), adjusted such that the reference period is the average of the 34 weeks prior to the hurricane. The dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire week and equal to 1 if the beneficiary died in a given week. The week in which Hurricane Katrina struck New Orleans is labeled "0" on the horizontal axis (this week begins on Monday, August 29, 2005). The gray dashed line indicates the week of FEMA's "look-and-leave"/"look-and-stay" announcement date (December 9, 2005). Standard errors are clustered by beneficiary baseline ZIP codes. Coefficients and confidence intervals have been scaled by 1,000 to reflect changes in deaths per thousand beneficiaries. See Appendix Table A.1 for numerical values of a subset of the statistics plotted here.

Figure 3: Long-run effects of Hurricane Katrina (2004 Medicare cohort)

(a) Annual and cumulative mortality



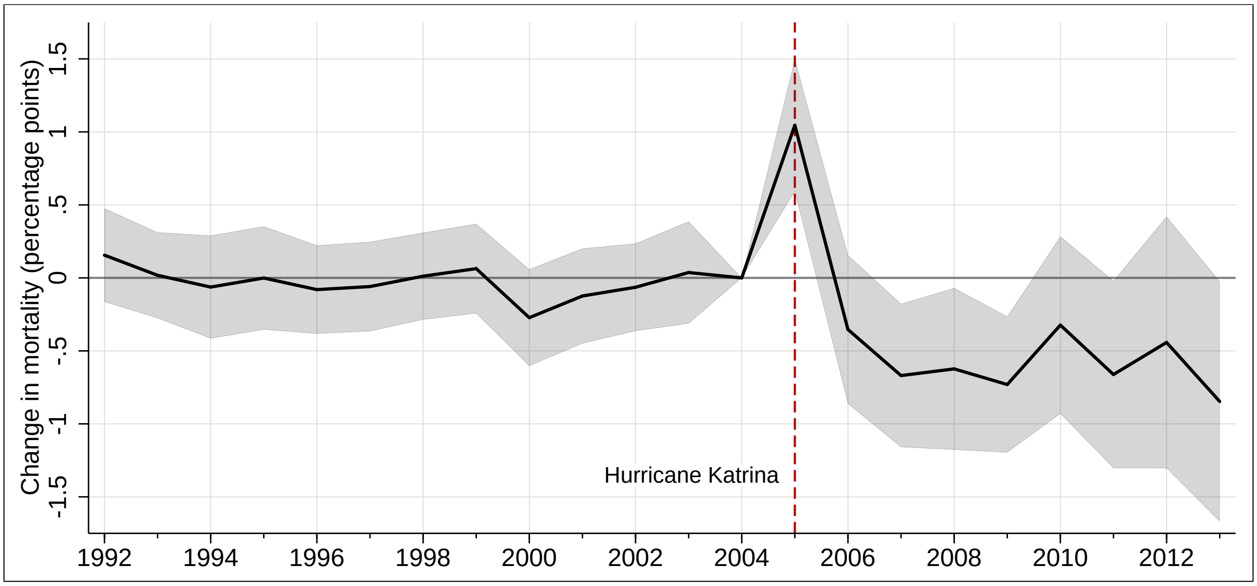
(b) Relocation



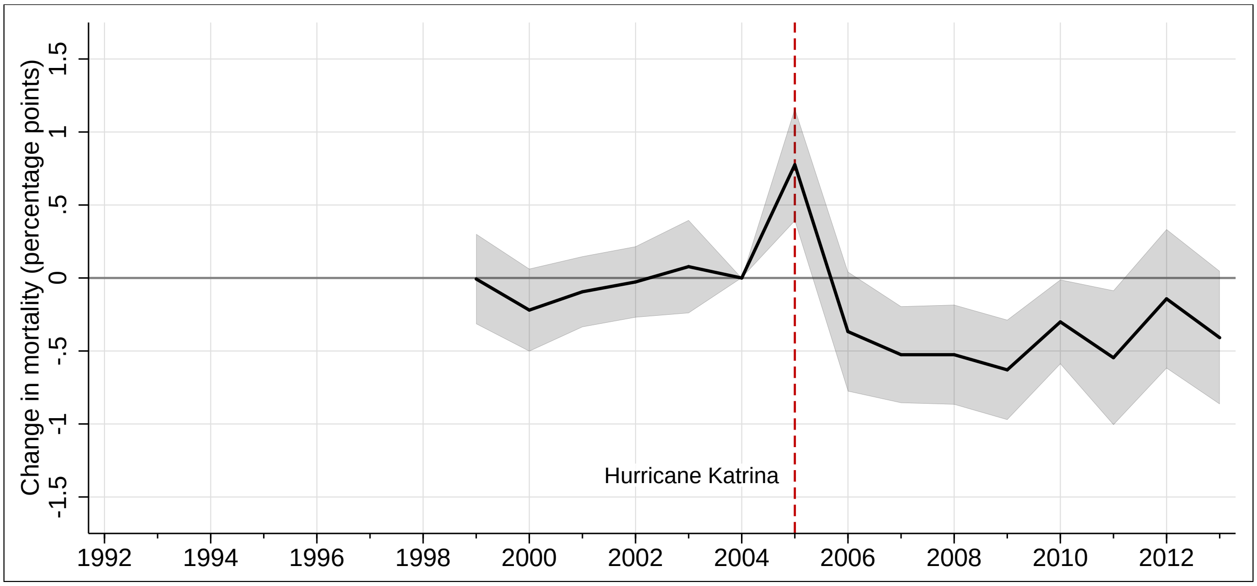
Notes: The figure shows estimates of changes in the probability that an individual dies (panel (a)) or is living in a city other than the city of residence in 2004 (panel (b)). The black solid lines reflect estimates from equation (2). The dashed line in panel (a) tracks the implied changes in cumulative mortality probability (equation (3)). The gray shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary baseline ZIP codes. See Section 4 for definitions of the dependent variables. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points. Appendix Table A.2 reports numerical values of these point estimates and their standard errors, along with the empirical survival rate of the 2004 New Orleans cohort.

Figure 4: Long-run mortality effects of Hurricane Katrina (earlier Medicare cohorts)

(a) 1992 Medicare cohort



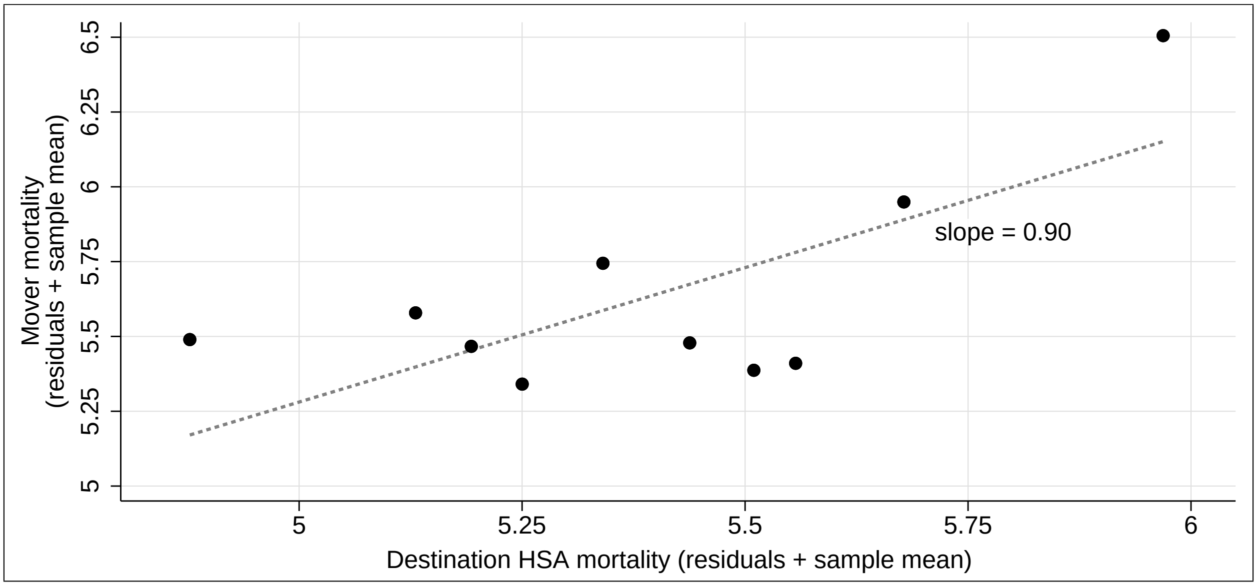
(b) 1999 Medicare cohort



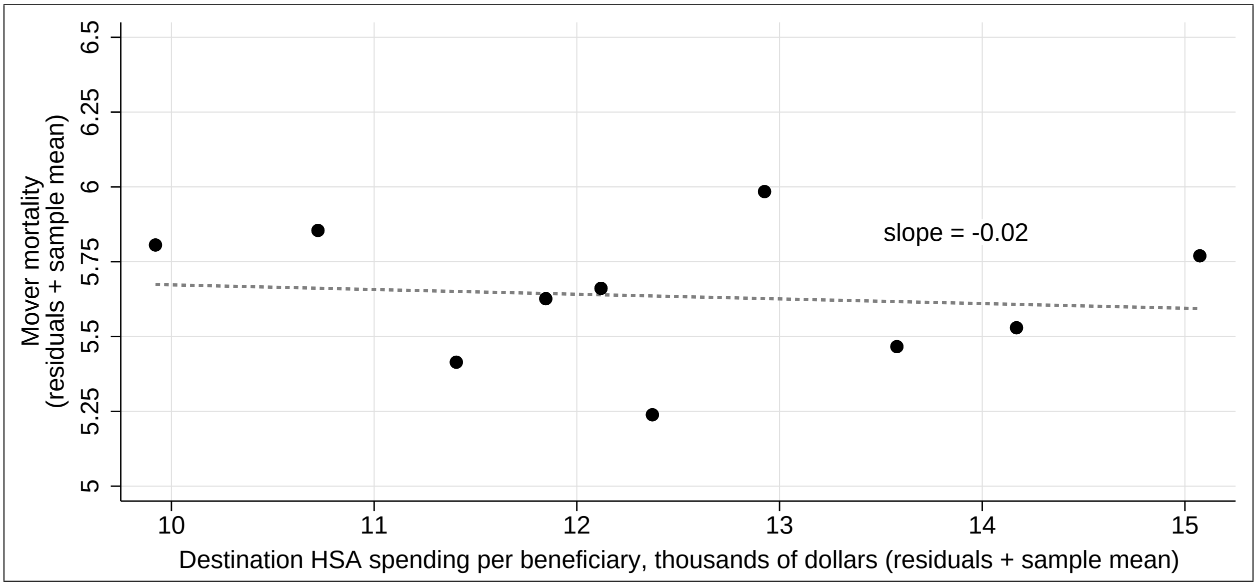
Notes: The figure shows estimates and 95 percent confidence intervals from equation (2) for the Medicare cohort indicated above each panel. The dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire year and equal to 1 if the beneficiary died in a given year. Standard errors are clustered by beneficiary baseline ZIP codes. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points. Appendix Table A.3 reports numerical values of these point estimates and their standard errors.

Figure 5: Mover mortality by destination characteristic

(a) Destination mortality



(b) Destination average Medicare spending



Notes: The figure shows the relationship between mover mortality rates and characteristics of the regions to which they moved. The sample includes one observation for each individual who moved out of New Orleans following Hurricane Katrina and for each post-move year to which the individual survived. Mover mortality rates and destination region characteristics in this figure refer to recentered residuals, constructed as the residuals from regressing the variable on ZIP code and 5-year-age-bins-by-gender-by-race-by-year fixed effects plus the sample mean of the variable. The scatter plot in each panel shows average mover mortality residuals and the destination characteristic residuals by decile of the destination HSA characteristic residuals. The dashed trendline in each panel shows the fitted values from a regression of mover mortality residuals on the destination characteristic residuals. The slopes of the trendlines shown here in panels (a) and (b) are identical to the regression coefficients reported in panels A and B, respectively, of Table 4, column (3).

Tables

Table 1: Summary statistics for the 2004 cohort

	(1)	(2)	(3)
	Mean	Std. dev.	Obs.
Race = black indicator	0.353	0.478	1,278,250
Male indicator	0.422	0.494	1,278,250
Age at baseline	71.110	12.686	1,278,250
65 and older at baseline	0.820	0.384	1,278,250
Median block group income, householders aged 65+	31,004	17,187	1,116,654
Enrolled in fee-for-service Medicare in 2004	0.886	0.318	1,278,250
2004 Medicare spending	9,041	20,728	1,132,209
Died in 2004	0.048	0.214	1,278,250
Moved in 2005–2006	0.052	0.222	1,216,791
Flood depth during Hurricane Katrina, feet	2.552	2.803	70,119

Sources: Centers for Medicare and Medicaid Services, 2000 U.S. Census, National Oceanic and Atmospheric Administration. There is one observation for each Medicare beneficiary in the baseline sample. Medicare spending is available only for beneficiaries enrolled in fee-for-service Medicare. Flood depth summary statistics are based on the New Orleans cohort only.

Table 2: Concise difference-in-differences mortality estimates

	(1)	(2)	(3)	(4)	(5)	(6)
		2004 Cohort			1999 Cohort	
2005 x New Orleans	0.55***	0.54***	0.56***	0.83***	0.80***	0.88***
	(0.14)	(0.14)	(0.13)	(0.17)	(0.18)	(0.15)
(2008-2013) x New Orleans	-0.36***	-0.39***	-0.28***	-0.39***	-0.43***	-0.20**
	(0.11)	(0.13)	(0.10)	(0.12)	(0.15)	(0.08)
Included controls	A	B	C	A	B	C
Dep. var. mean	5.49	5.49	5.49	6.28	6.28	6.28
Observations	10,162,395	10,162,395	10,162,357	12,820,286	12,820,286	12,820,256

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports difference-in-differences estimates of equation (4) based on the 2004 cohort (columns (1)–(3)) and the 1999 cohort (columns (4)–(6)). Standard errors (in parentheses) are clustered by beneficiary baseline ZIP codes. Dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes baseline ZIP code and year fixed effects; B also includes fixed effects for each 5-year-age-bin, race, and gender combination. C additionally allows the year fixed effects to differ by each 5-year-age-bin, race, and gender combination.

Table 3: Predictors of leaving New Orleans and of destination characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Whether moved		Local mortality rate		Local Medicare spending	
Black	0.161***	0.153***	-0.0081	-0.0101	203	188
	(0.048)	(0.043)	(0.0269)	(0.0270)	(209)	(213)
Male	-0.036***	-0.040***	0.0124***	0.0054	-24	-25
	(0.005)	(0.005)	(0.0042)	(0.0057)	(25)	(29)
64 and younger	0.115***	0.111***	-0.0060	-0.0057	53	-1
	(0.008)	(0.008)	(0.0087)	(0.0096)	(62)	(71)
75 and older	0.018	0.007	-0.0023	-0.0107	9	-33
	(0.011)	(0.009)	(0.0054)	(0.0084)	(31)	(38)
Below median income	0.062***	0.073***	0.0082	0.0092	29	35
	(0.017)	(0.016)	(0.0068)	(0.0083)	(35)	(47)
Katrina flood level, feet	0.048***	0.050***	-0.0007	-0.0004	6	3
	(0.006)	(0.005)	(0.0016)	(0.0019)	(8)	(9)
End-stage renal disease	0.020*	-0.031*	0.0239*	-0.0112	61	-44
	(0.010)	(0.016)	(0.0143)	(0.0189)	(62)	(98)
2004 medical spending, thousands		0.001***		0.0004**		1
		(0.000)		(0.0002)		(1)
Alzheimer's/dementia		-0.002		0.0443***		20
		(0.013)		(0.0153)		(64)
Respiratory disease		0.043***		-0.0125		-90**
		(0.006)		(0.0095)		(44)
Heart disease and stroke		0.013**		0.0076		24
		(0.005)		(0.0064)		(30)
Blood and kidney disease		0.003		-0.0078		-22
		(0.007)		(0.0073)		(38)
Diabetes		0.002		0.0073		65
		(0.006)		(0.0072)		(42)
Musculoskeletal		0.002		-0.0050		-4
		(0.005)		(0.0073)		(30)
Cancer		-0.042***		-0.0179*		-57
		(0.011)		(0.0102)		(55)
Other		0.000		-0.0019		10
		(0.004)		(0.0061)		(35)
p-value of joint F-test	< 0.001	< 0.001	0.086	0.108	0.425	0.468
Observations	62,690	28,061	27,678	12,844	27,678	12,844

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) are clustered by beneficiary 2006 HSAs. The dependent variable is specified at the top of each column.

Table 4: Migrant mortality, by destination mortality and spending (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
		Panel A: Local mortality			
Mean death rate in 2006 HSA	0.83***	0.88***	0.90***	0.84***	0.86***
	(0.25)	(0.22)	(0.23)	(0.29)	(0.29)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	None	None	None	Grouped	Interactions
Dep. var. mean	5.63	5.63	5.63	6.22	6.21
Observations	213,697	213,697	213,679	97,927	97,918
R-squared	0.00	0.04	0.04	0.06	0.07
		Panel B: Local spending			
Mean spending in 2006 HSA, thousands	-0.06	-0.02	-0.02	-0.04	-0.04
	(0.06)	(0.03)	(0.04)	(0.05)	(0.05)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	None	None	None	Grouped	Interactions
Dep. var. mean	5.63	5.63	5.63	6.22	6.21
Observations	213,697	213,697	213,679	97,927	97,918
R-squared	0.00	0.04	0.04	0.06	0.07

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equation (6). Standard errors (in parentheses) are clustered by a beneficiary's 2006 HSA. Dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes baseline ZIP code and year fixed effects; B also includes fixed effects for each 5-year-age-bin, race, and gender combination. C additionally allows the year fixed effects to differ by each 5-year-age-bin, race, and gender combination.

A Online Appendix

A.1 Data

A.1.1 Medicare beneficiary location

Medicare eligibility files provide the ZIP code of each beneficiary's mailing address, which is maintained by the Social Security Administration (SSA). This is the address that is used to deliver cash benefits to the beneficiary (such as Social Security) and is also used by the Centers for Medicare and Medicaid Services (CMS) for premium billing. While the address from SSA may be updated by the beneficiary at any time, the Medicare eligibility files that are made available to researchers only report beneficiary ZIP codes as of a given date in each year.

The format of Medicare's eligibility files have changed over time, including the date on which the location variable is "frozen."[1] For some file years, the location variable reflects a beneficiary's ZIP code as of March of the following calendar year (in these cases, CMS does not specify the exact day in March on which records were frozen). In the remaining file years, the location variable reflects a beneficiary's ZIP code as of December 31 of that calendar year. If the beneficiary dies prior to the date on which the location variable is frozen for the Medicare eligibility file, the location variable will reflect the beneficiary's last location on record prior to his or her death.

The Medicare eligibility files we use, which we access through the National Bureau of Economic Research, span eligibility file formats. The chart below summarizes when the location variable is frozen for each eligibility file that we use.

Medicare eligibility file year t	ZIP code reflects residence as of...
$t \in \{1999, 2007, [2009, 2013]\}$	min(date of death; December 31, t)
$t \in \{[1992, 1998], [2000, 2006], 2008\}$	min(date of death; March $t + 1$)

We illustrate how the structure of the Medicare eligibility files affects observations in our sample with two examples. Suppose a beneficiary moves in November 2005 and dies on January 10, 2006. Because she survived until January 1, 2006, she will appear in the 2006 Medicare eligibility file. In both the 2005 and 2006 eligibility files, her ZIP code will correspond to where she moved in November 2005. Suppose another beneficiary who turned 65 in the year 2000 moves in January 2005 and remains alive through 2013. He will appear in each of the 2000–2013 Medicare eligibility files. In the 2000–2003 files, his ZIP code will

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[1] For a description of how Medicare's beneficiary eligibility and enrollment files have changed over time, see `https://www.resdac.org/resconnect/articles/138` (accessed on June 19, 2018).

correspond to his location prior to the move. In the 2004–2013 files, his ZIP code will reflect his new location.

A.1.2 Chronic conditions

We use end-of-year chronic condition flags from the 2004 Medicare beneficiary summary file to determine whether an individual in the 2004 Medicare cohort has a particular condition at baseline. These flags are based on patterns of services that the beneficiary has received, and serve as a proxy for whether the beneficiary is receiving treatment for a particular condition.[2] Because patterns of services are only available for Medicare beneficiaries enrolled in traditional fee-for-service Medicare, our chronic condition flags are only defined for beneficiaries who have been continuously enrolled in fee-for-service for the condition-specific look-back window used to construct the condition flag.

The Medicare data we use include 27 chronic condition flags, which we group into eight categories as follows:

1. **Heart disease and stroke:** acute myocardial infarction, atrial fibrillation, heart failure, ischemic heart disease, hypertension, stroke/transient ischemic attack
2. **Respiratory disease:** chronic obstructive pulmonary disease, asthma
3. **Blood and kidney disease:** chronic kidney disease, anemia, hyperlipidemia
4. **Cancer:** breast cancer, colorectal cancer, prostate cancer, lung cancer, endometrial cancer
5. **Diabetes:** own category
6. **Musculoskeletal:** hip fracture, osteoporosis, rheumatoid arthritis/osteoarthritis
7. **Alzheimer's/dementia:** Alzheimer's disease, dementia
8. **Other:** cataracts, glaucoma, hypothyrodism, benign prostatic hyperplasia, and depression

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[2] The CMS Chronic Conditions Data Flags Data Dictionary provides details on how each flag is defined, available from `https://healthcaredelivery.cancer.gov/seermedicare/medicare/chronic-conditions-flags.pdf` (accessed June 20, 2018).

A.1.3 Cause of death

We use cause of death information, which is available for all Medicare beneficiaries from 1999–2008. For beneficiaries who die during this period, Medicare provides the ICD-10 cause of death code from the National Death Index (NDI), a centralized database of death record information compiled from state vital statistics offices and maintained by the National Center for Health Statistics (NCHS).[3]

We first categorize ICD-10 cause of death codes into 39 groups based on the NCHS's list of 39 selected causes of death.[4] For use in our analysis, we further categorize these 39 causes of death into four groups as follows:

1. **Cardiovascular deaths:** hypertensive heart disease with or without renal disease, ischemic heart disease, other diseases of the heart, essential (primary) hypertension and hypertensive renal disease, cerebrovascular diseases, atherosclerosis, other diseases of circulatory system
2. **Cancer deaths:** stomach cancer, colon cancer, pancreatic cancer, lung cancer, breast cancer, ovarian and uterine cancer, prostate cancer, bladder cancer, non-Hodgkin's lymphoma, leukemia, other cancer
3. **Other internal causes of death:** tuberculosis; syphilis; HIV; diabetes; Alzheimer's disease; influenza and pneumonia; chronic lower respiratory disease; peptic ulcer; chronic liver disease and cirrhosis; nephritis; pregnancy, childbirth and the puerperium; perinatal conditions; congenital abnormalities; SIDS; abnormal clinical findings; all other diseases
4. **External causes of death:** Motor vehicle accidents, suicide, homicide, other accidents, other external causes

A.2 Estimation via synthetic control

We estimate the effect of Hurricane Katrina on mortality using individual-level data and regression analysis. An alternative approach is to treat the New Orleans cohort as a single unit and compare its survival prospects to that of cohorts from other areas using the synthetic control method (Abadie, Diamond and Hainmueller, 2010, 2015). Because the synthetic control method is not easily adaptable to individual-level controls and is not appropriate for

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[3] For more information about the NDI, see `https://www.cdc.gov/nchs/ndi.htm` (accessed June 20, 2018).

[4] The list of 39 selected causes of death and the ranges of ICD-10 codes that comprise each cause are available at `https://www.cdc.gov/nchs/data/dvs/im9_2002.pdf.pdf` [*sic*] (accessed June 20, 2018).

studying how movers' subsequent mortality is related to local mortality, we do not adopt it as our primary specification. However, we demonstrate that our main annual event study findings are very similar if we use this approach.

We focus on the 1999 cohort, as this ensures that we have enough pre-Katrina data to form a reliable "synthetic control" unit. To enable meaningful permutation inference, which would be difficult to do with only 10 control cities, our control units consist either of all Hospital Service Areas with a baseline population of over 50,000 (122 HSAs, including New Orleans) or of all Hospital Referral Regions (306 HRRs, including New Orleans). We exclude smaller HSAs because their annual death rates are inherently more variable, making them less reliable counterfactuals for New Orleans. When we consider the sample of HSAs, our treated unit is the New Orleans HSA. When we consider the sample of HRRs, our treated unit is the New Orleans HRR, which is only slightly larger than the HSA.

Because we are primarily interested in *changes* in the death rate, our outcome variable is an HSA's/HRR's death rate minus its 1999–2004 average death rate. To form the synthetic treatment unit, i.e., the counterfactual, we use this demeaned death rate in each year between 1999 and 2004. The synthetic control algorithm then assigns weights to the control units such that the difference between the demeaned death rate of the treated unit (New Orleans HSA/HRR) and the weighted average of the control units is minimized in the pre-treatment period. The weighted difference in the post-period is then the estimated treatment effect.

The synthetic control method does not directly produce standard errors. To conduct statistical inference, Abadie, Diamond and Hainmueller (2015) suggest "in-space placebos" tests, where treatment status is assigned to each of the control units, one at a time. The researcher applies the same synthetic control methodology to each case and then estimates where the true treated unit falls in the distribution of estimated treatment effects. We follow this procedure, fixing the timing of the treatment in 2005.[5]

The results for the annual mortality rate are shown in Appendix Figure A.10. Here, we plot the estimated treatment effect for the New Orleans cohort (black line) as well as the estimated treatment effects under the assumption that each of the other HSAs/HRRs is the treated unit. It is immediately clear that post-2005 New Orleans is an outlier, both with respect to the initial mortality increase in 2005 and with respect to the subsequent mortality decreases. Specifically, the 2005 treatment effect is always the largest for New Orleans, the true treated unit. In almost each year between 2006 and 2013, the estimated change in mortality for New Orleans is smaller than 114–120 of the other 120 HSAs for

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[5] During the permutation exercise, the synthetic control method fails to converge for one of the HSAs and three of the HRRs. These permutations are thus omitted from the sample, leaving us with 120 non-New Orleans HSAs and 302 non-New Orleans HRRs.

which we estimate a placebo treatment effect and 294–302 of the other 302 HRRs.[6] The implied p-values thus generally fall between 0 (in cases where the New Orleans mortality change is literally the largest/smallest) and 0.058 (the year when New Orleans has the seventh smallest mortality change in the sample of HSAs). The magnitudes of the estimated treatment effects in 2006–2013 are similar but somewhat larger than in our main regression event study, ranging from -0.32 percentage points to -0.74 percentage points.

Using the estimated change in the mortality rate in each period and the empirical survival probability of each HSAs/HRRs cohort, we next calculate the changes in the cumulative survival probability implied by the treatment effects (equation (3)). We focus on the probability of surviving until 2013 and plot the density of these estimates in Appendix Figure A.11. The red line indicates where the true treatment effect (i.e., that of New Orleans) falls in the distribution. Only 4 HSAs and 7 HRRs have larger falls in the cumulative mortality rate than New Orleans, implying p-values of 0.04 and 0.03, respectively. As with annual mortality, our estimates of cumulative mortality changes are slightly larger in absolute value than those using regression methods: -2.4 percentage points in both sets of synthetic control analyses.

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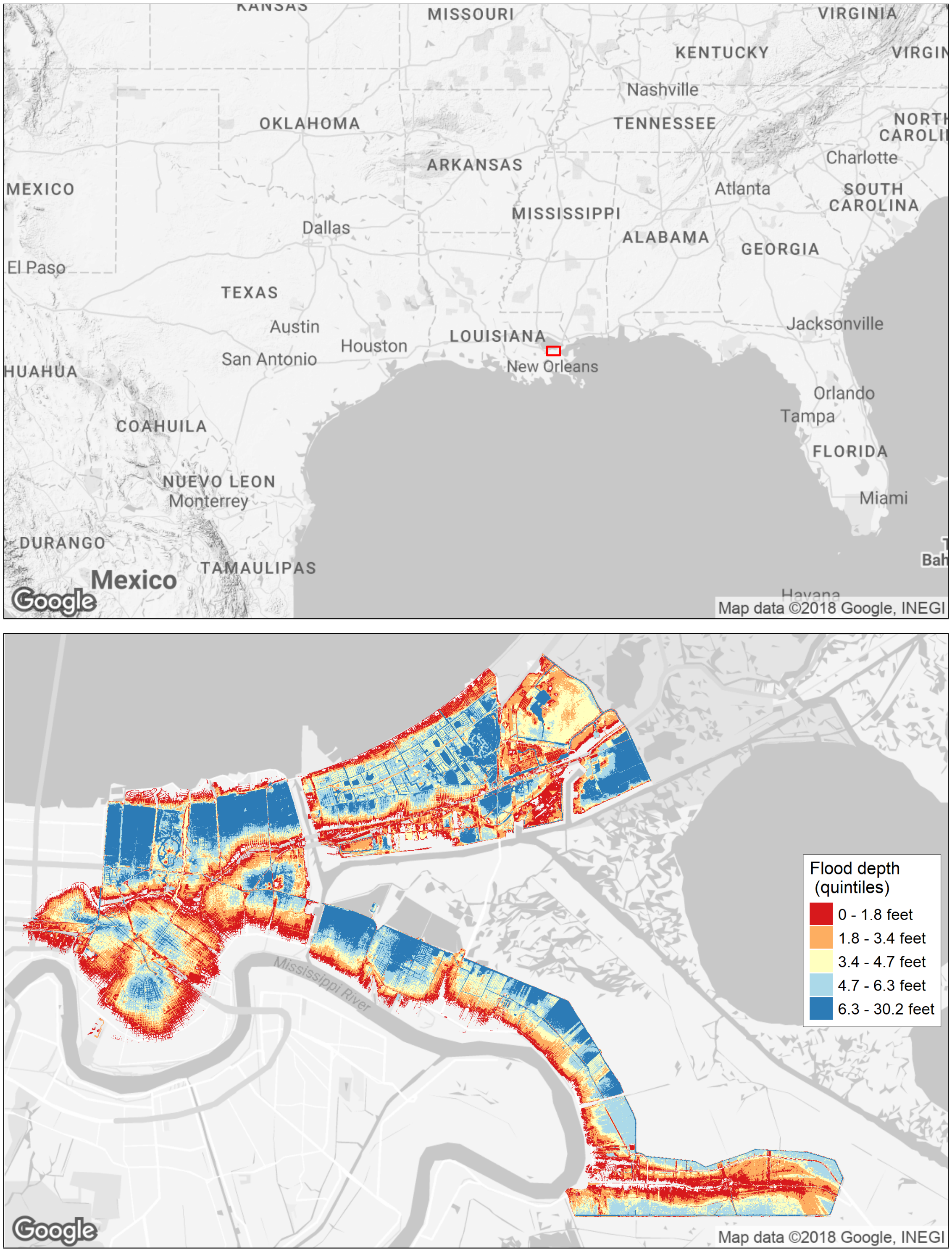
[6] The exception is 2012, when 95 of the 120 HSAs and 289 of the 302 HRRs have larger changes in mortality.

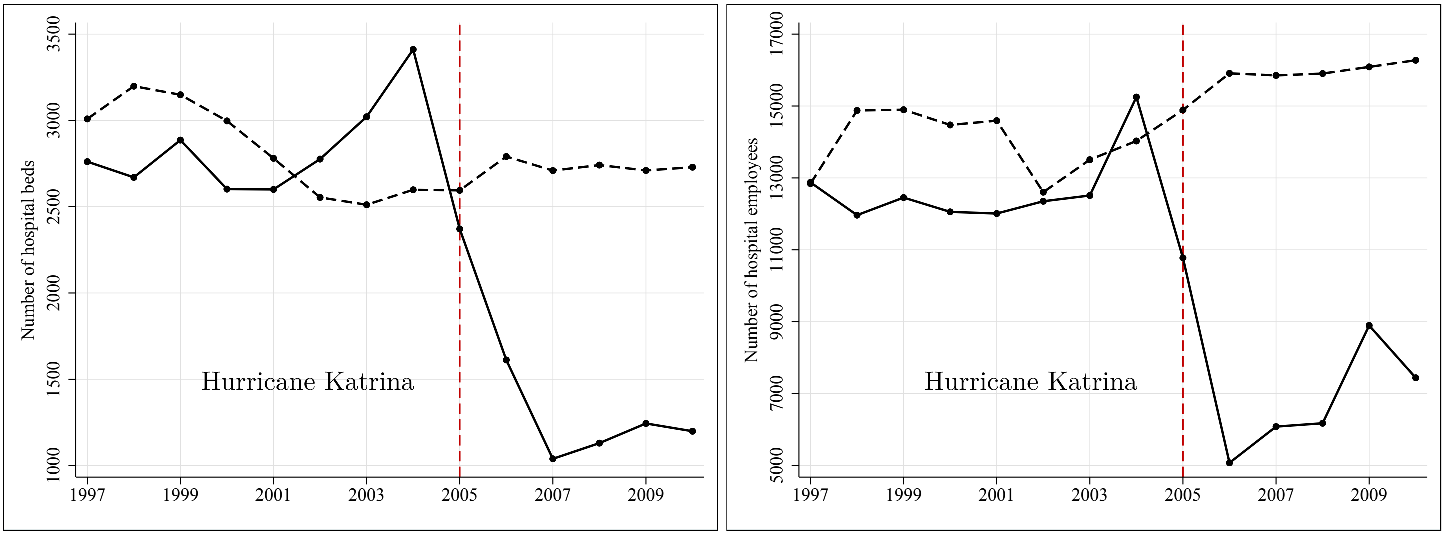
Appendix Figures and Tables

Figure A.1: New Orleans Hurricane Katrina flood map



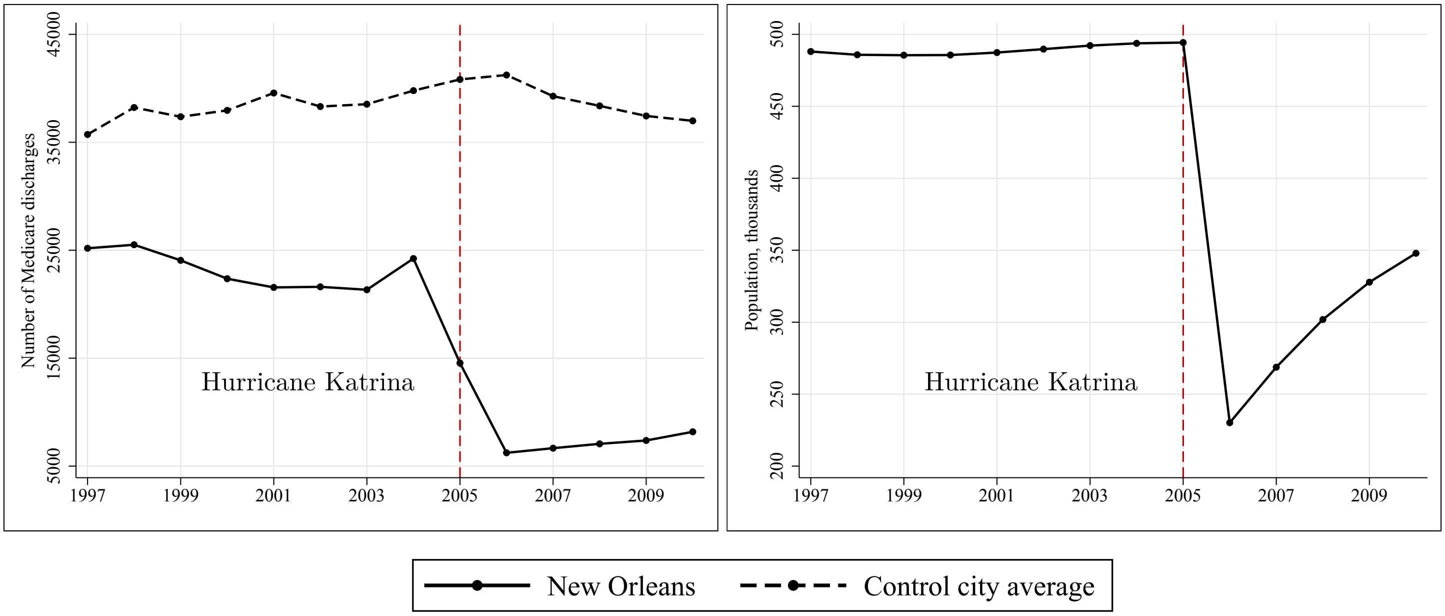
Notes: The figure shows the location of New Orleans (top panel) and Hurricane Katrina flood depth estimates at a resolution of five meters (bottom panel). Flood data come from the National Oceanic and Atmospheric Administration (NOAA).

Figure A.2: Capacity and utilization of the New Orleans health care system following Hurricane Katrina



(a) Hospital beds

(b) Hospital employees



(c) Medicare discharges

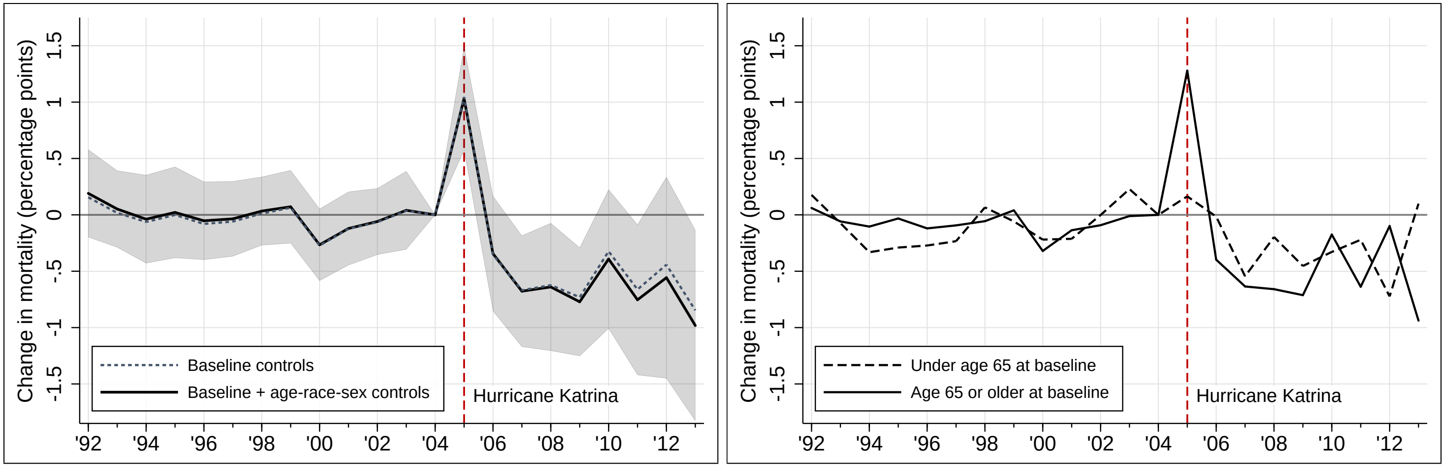
(d) New Orleans population

Notes: The figure shows the number of hospital beds (panel (a)), the number of hospital employees (panel (b)), and the number of Medicare discharges (panel (c)) in New Orleans and the 10 control cities we utilize for our baseline difference-in-differences analysis. Panel (d) shows the New Orleans population. The vertical dashed red lines indicate the year of Hurricane Katrina (2005). Sources: Centers for Medicare and Medicaid Services Hospital 2552-96 Cost Report Data file; Bureau of Economic Analysis.

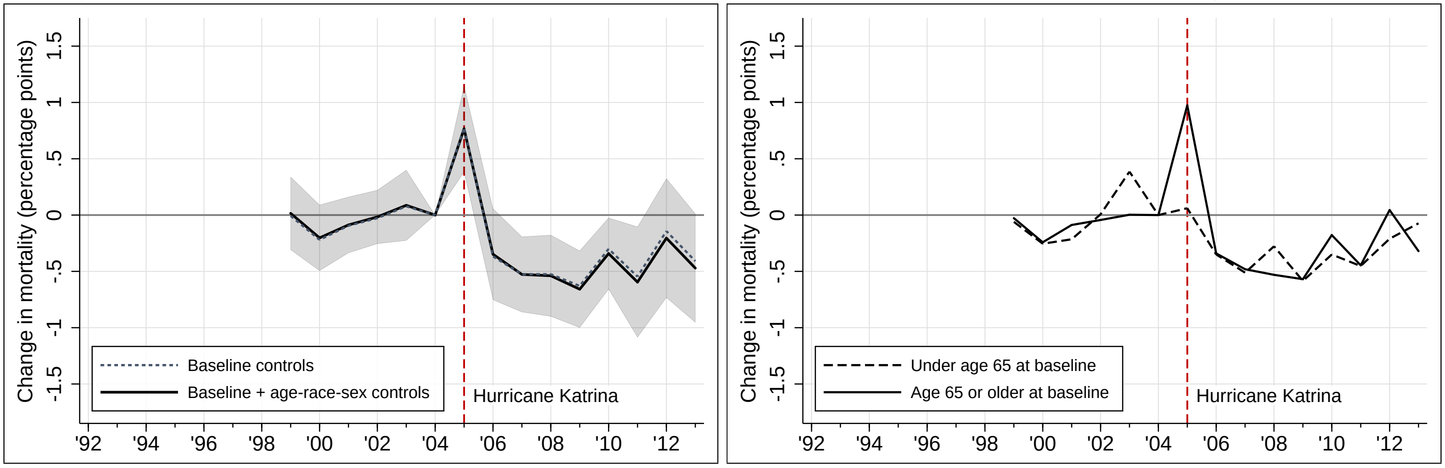
Figure A.3: New Orleans and control cities

Notes: The figure shows the location of New Orleans and each of the 10 control cities used to construct comparison cohorts for identifying the effects of Hurricane Katrina on Medicare beneficiaries initially residing in New Orleans.

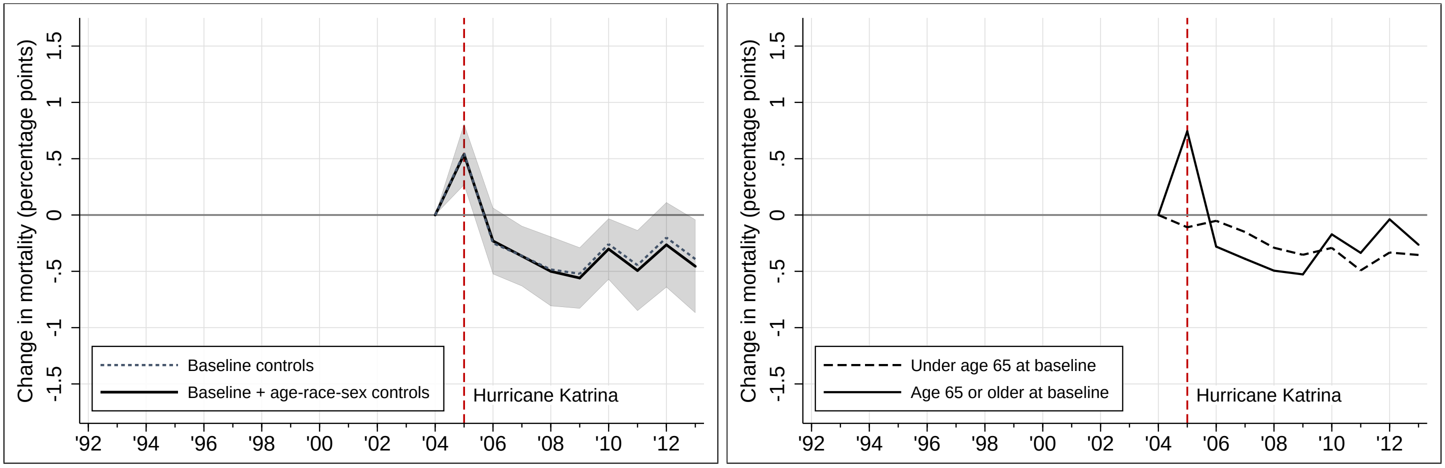
Figure A.4: Annual mortality event studies with demographic controls and with the under/over age 65 populations separated


(a) 1992 Medicare cohort: demographic controls

(b) 1992 Medicare cohort: under/over age 65


(c) 1999 Medicare cohort: demographic controls

(d) 1999 Medicare cohort: under/over age 65

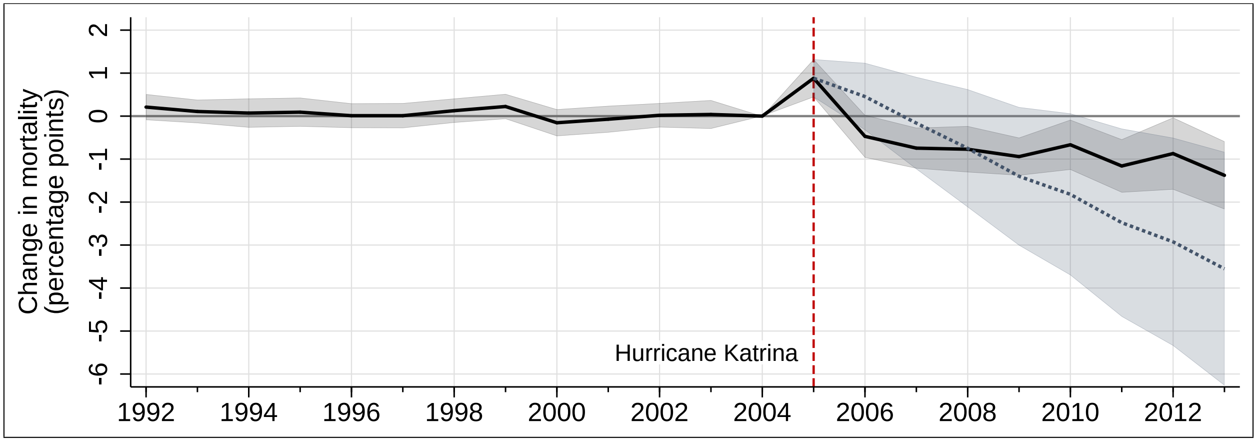

(e) 2004 Medicare cohort: demographic controls

(f) 2004 Medicare cohort: under/over age 65

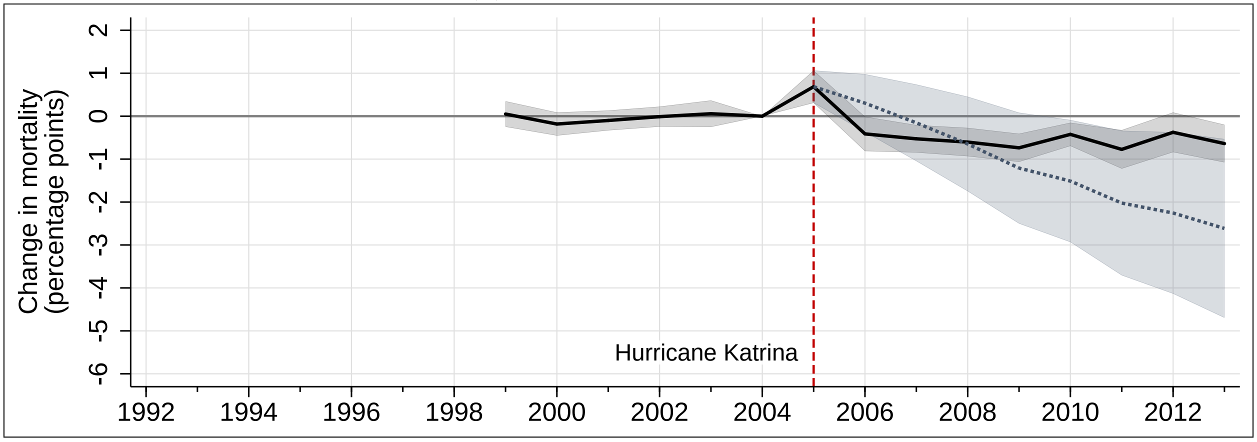
Notes: The black lines plot difference-in-differences event study estimates from equation (2) for the Medicare cohort indicated above each panel. Panels in the left column show the annual mortality effect estimates reported in Figures 3 and 4 (dashed lines) as well as estimates and their corresponding 95 percent confidence intervals that also control for all combinations of baseline gender, race, and 5-year age bins (solid lines and shaded regions, respectively). Panels in the right column report estimates from the baseline specification, but estimated separately for individuals who were either younger than 65 (dashed line) or at least age 65 (solid line) at baseline.

Figure A.5: Long-run mortality effects of Hurricane Katrina using the United States (except New Orleans) as the control group

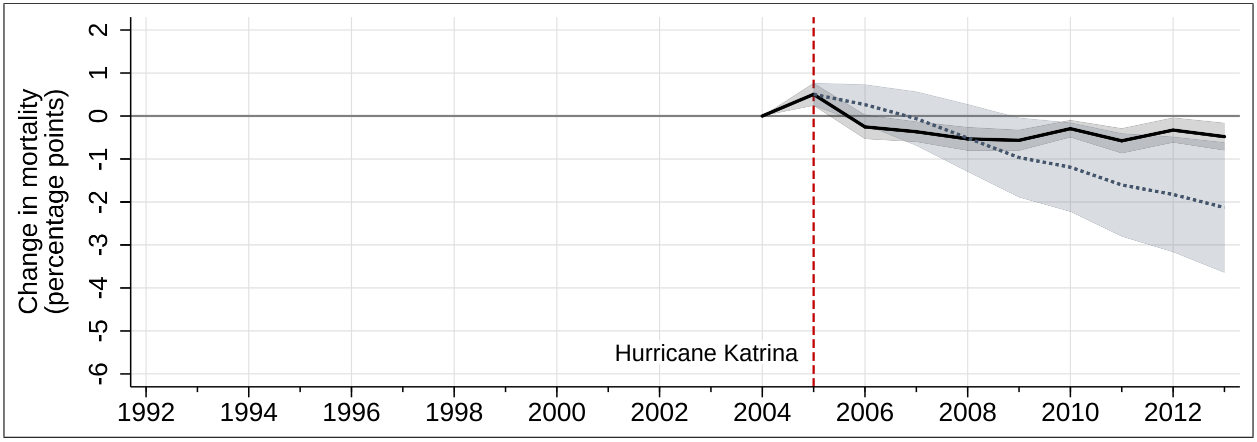
(a) 1992 Medicare cohort



(b) 1999 Medicare cohort

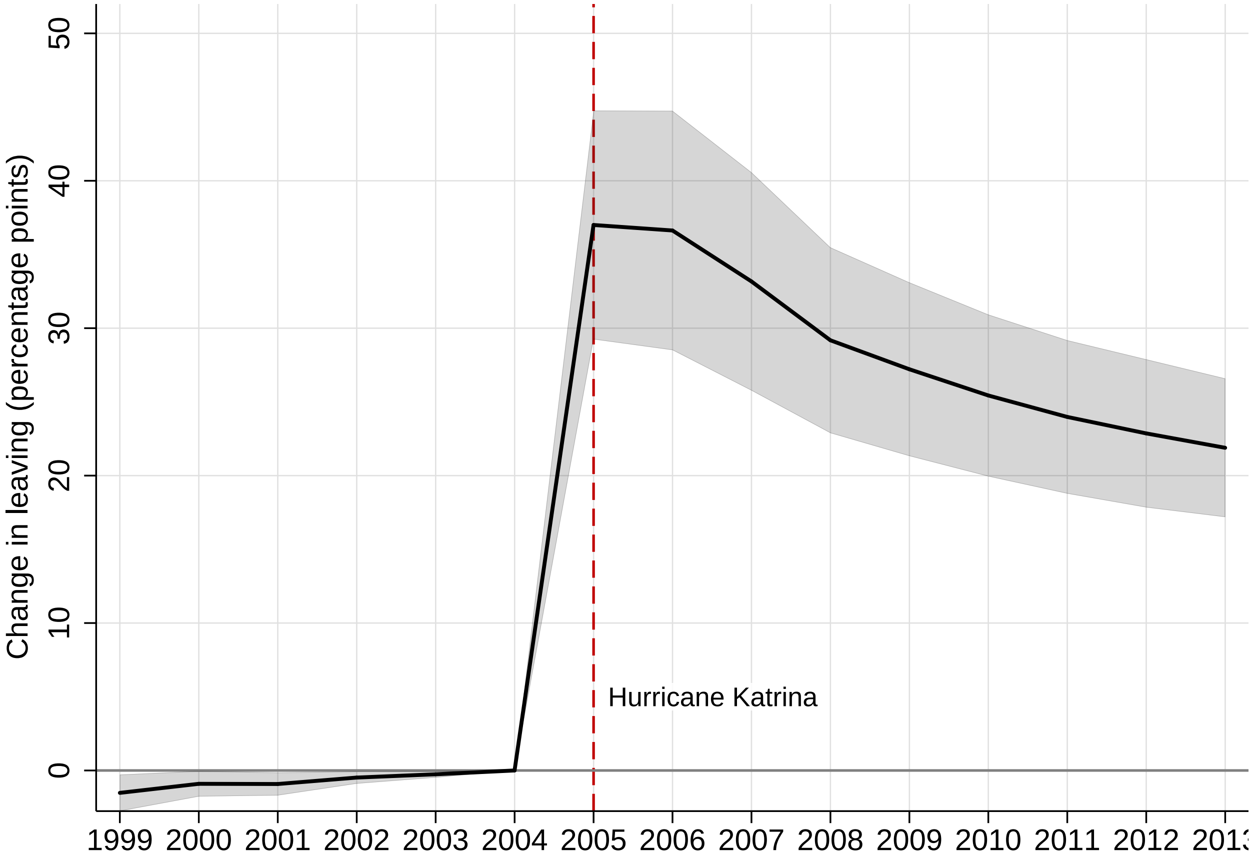


(c) 2004 Medicare cohort



Notes: The black lines plot difference-in-differences event study estimates from equation (2) for the Medicare cohort indicated above each panel. "Treated" beneficiaries are those initially living in New Orleans, and "control" beneficiaries are those initially living in any other part of the United States. The dependent variable is an indicator equal to 1 if the beneficiary died in a given calendar year and equal to 0 if a beneficiary survived that year. The dashed blue line tracks the implied changes in cumulative mortality probability (equation (3)). The shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by a beneficiary's baseline ZIP code. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points. Appendix Table A.5 reports numerical values of these point estimates and their standard errors.

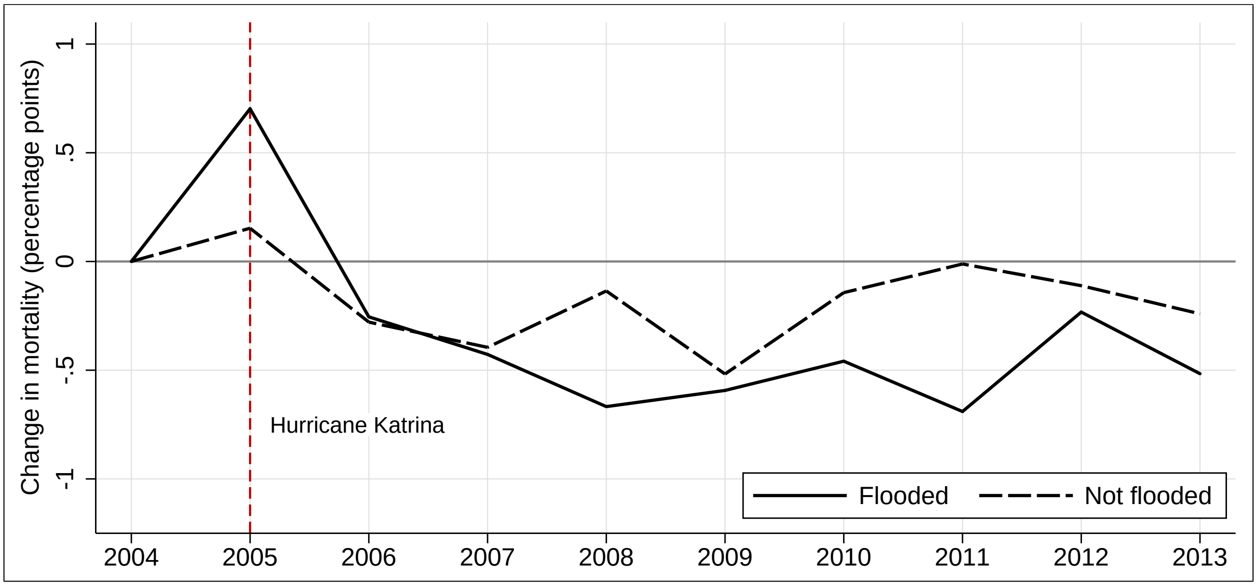
Figure A.6: Long-run mobility effects of Hurricane Katrina (1999 Medicare cohort)



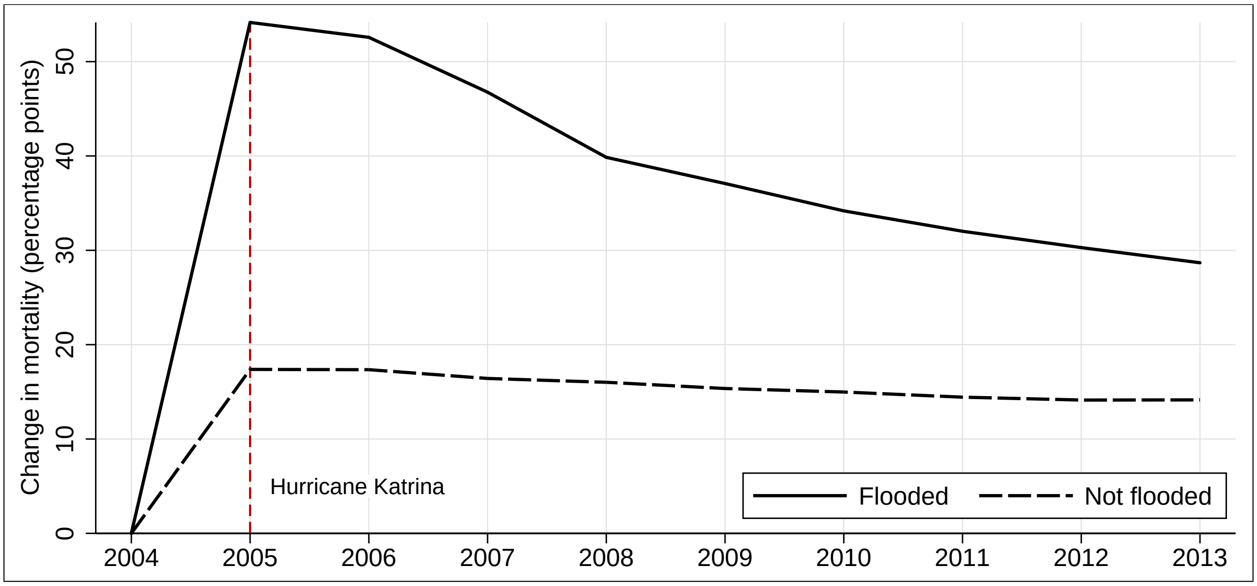
Notes: The figure shows estimates of changes in the probability that an individual is living in a city other than their city of residence in 1999. The black line plots estimates from equation (2) where the dependent variable is an indicator equal to 0 if a beneficiary was living in his or her 1999 HSA of residence in that year and equal to 1 if the beneficiary was living in a different HSA. The gray shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary baseline ZIP codes. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points. Appendix Table A.3 reports numerical values of these point estimates and their standard errors.

Figure A.7: Long-run effects of Hurricane Katrina, by flood level (2004 Medicare cohort)

(a) Annual mortality

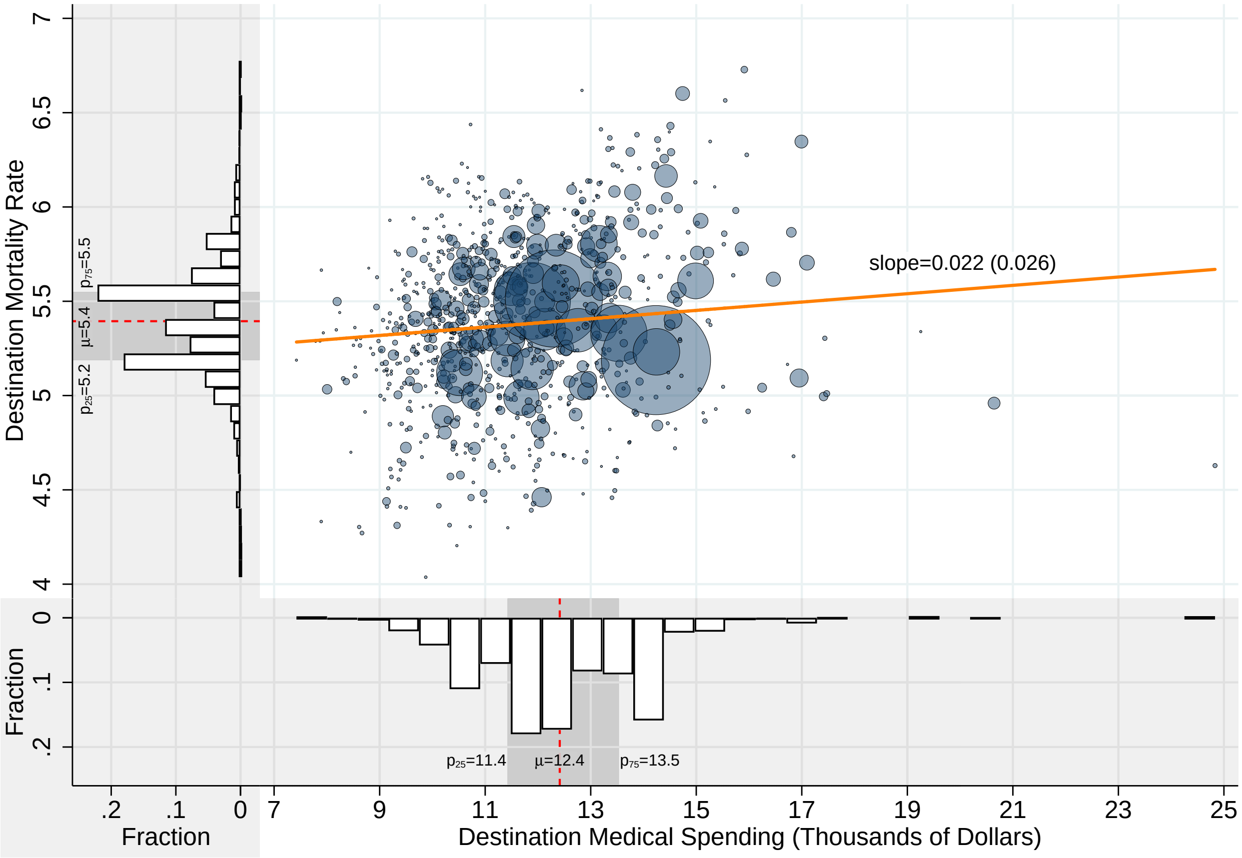


(b) Relocation



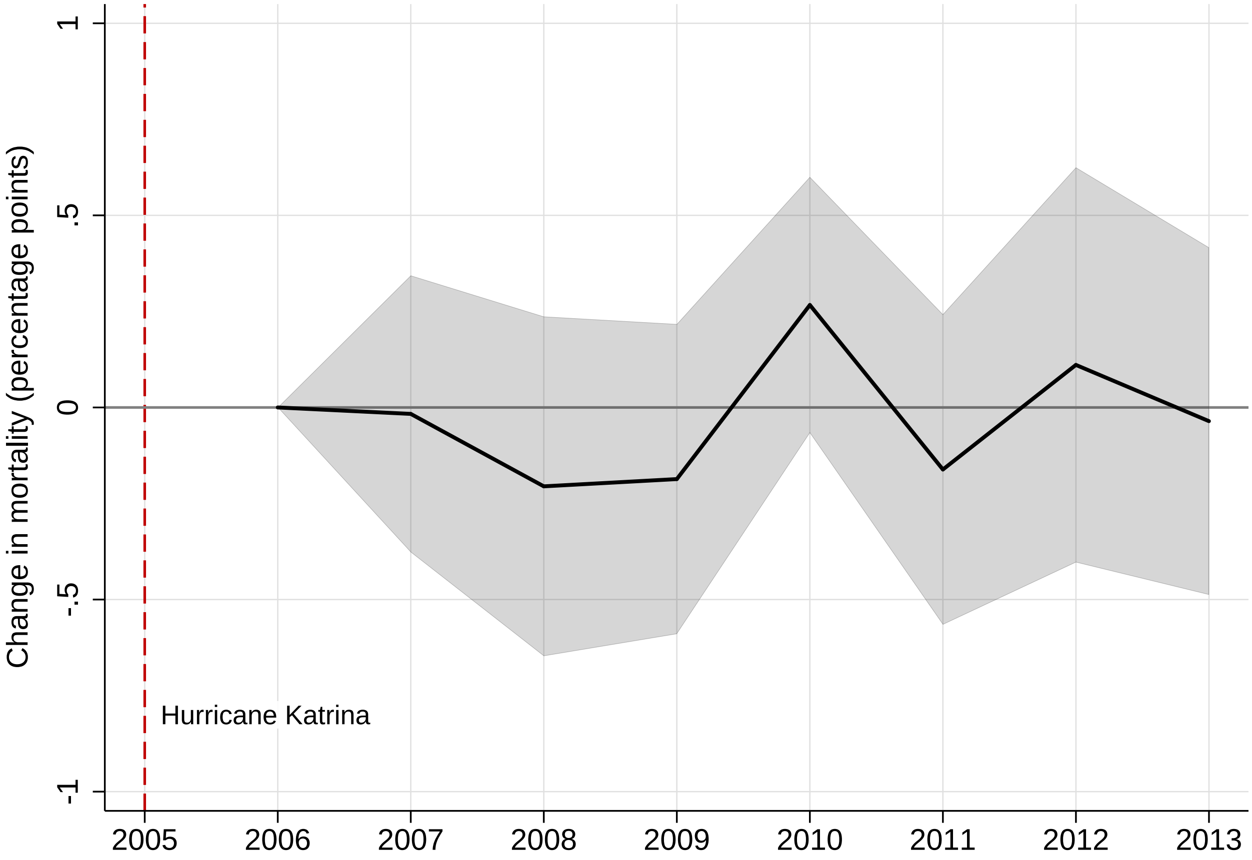
Notes: The figure shows estimates of changes in the probability that an individual dies (panel (a)) or is living in a city other than the city of residence in 2004 (panel (b)), by whether or not his or her 9-digit ZIP code of residence at baseline experienced flooding from Hurricane Katrina. Estimates are from an augmented version of equation (2) where the interactions between calendar years and living in New Orleans at baseline (2004) are fully interacted with a categorical variable indicating whether an individual's baseline 9-digit ZIP code had experienced at least some flooding due to the hurricane. See Section 4 for definitions of the dependent variables. Coefficients have been scaled by 100 to reflect changes in percentage points.

Figure A.8: Local mortality and Medicare spending in migrants' destination cities (HSAs)



Notes: The figure shows the annual Medicare spending per fee-for-service beneficiary and annual mortality rates in the cities (HSAs) to which people displaced from New Orleans after Hurricane Katrina moved in 2006. Local annual Medicare spending and local annual mortality rates are calculated based on the cohort of individuals living in each city in 2004, averaged over the post-Katrina period 2006–2013. Each observation in the scatter plot corresponds to a destination city and the size of each circle is proportional to the number of migrants to that city. Only destinations with 11 or more migrants, which cover over 90 percent of all migrants, are shown. The orange trendline is from regressing local mortality on local spending over all destination cities in the sample, including cities receiving fewer than 11 migrants, and weighting by the number of migrants to each city. The slope of this trendline is denoted in the figure, as is the heteroskedasticity-robust standard error of the slope estimate (in parentheses). The histograms show the migrant-weighted distribution of local spending and mortality among destination cities shown in the scatter plot. The dashed red lines and darkly shaded regions behind the histograms show the migrant-weighted mean (μ) and interquartile range ($p_{25} - p_{75}$), respectively, for the corresponding characteristic across all destination cities in the sample.

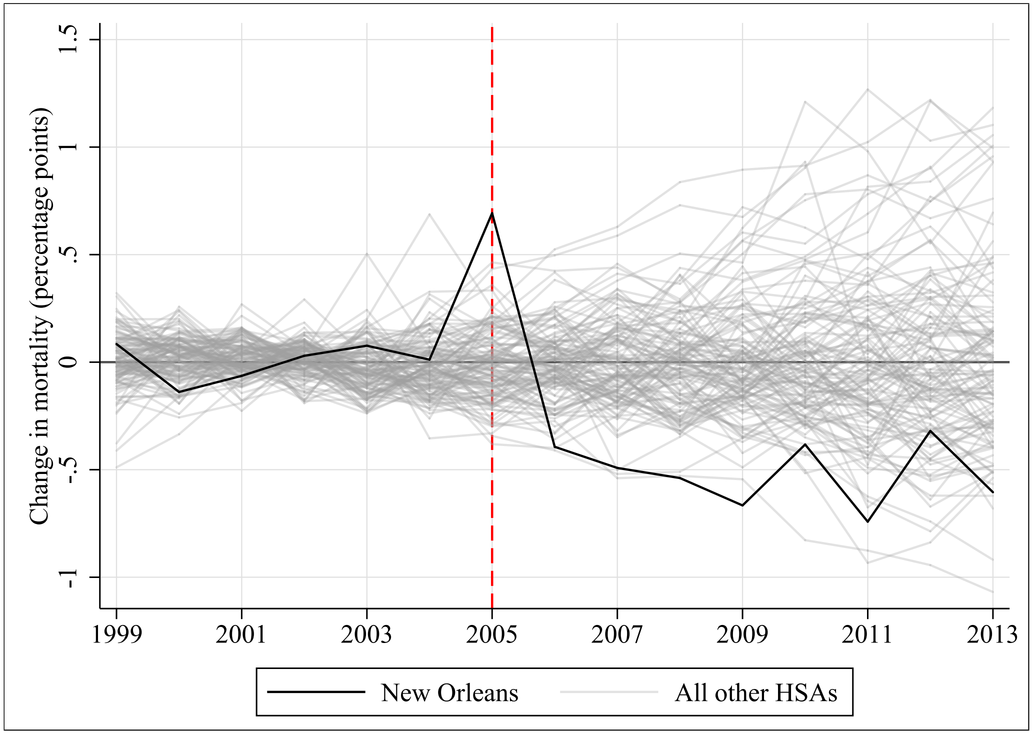
Figure A.9: Changes in stayers' mortality rates over time



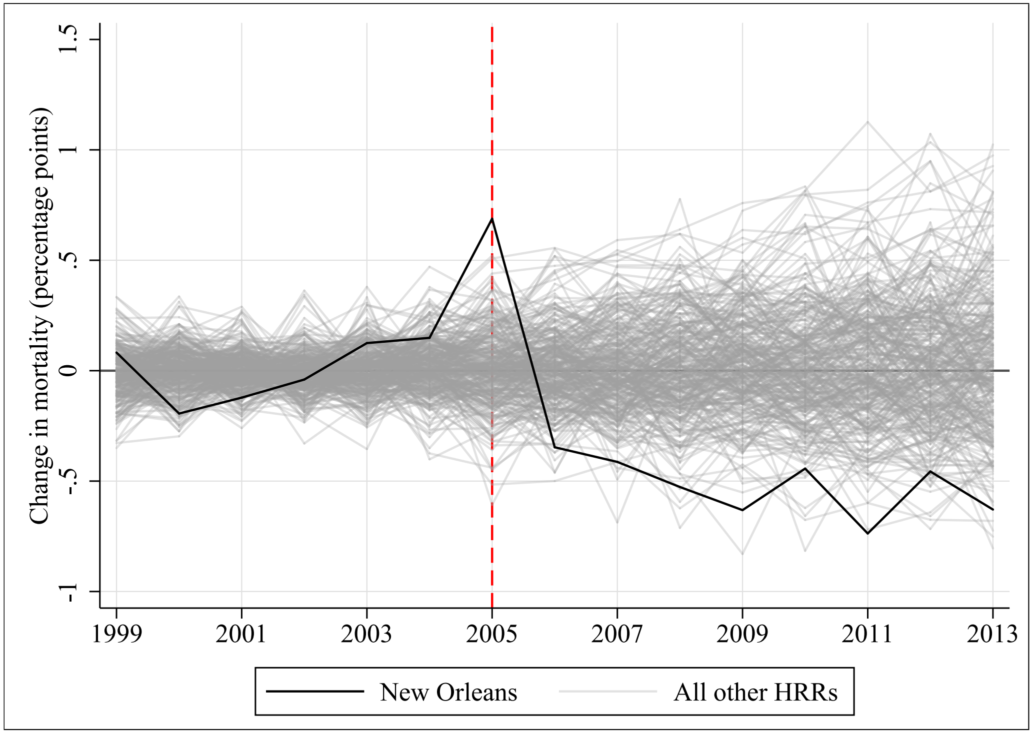
Notes: The figure shows estimates and 95 percent confidence intervals from estimating equation (2) using individuals whose residence, as of March 2006, was located in their baseline (2004) HSA of residence. The dependent variable is an indicator equal to 0 if a beneficiary is alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Standard errors are clustered by beneficiary baseline ZIP codes. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points.

Figure A.10: Long-run mortality effects of Hurricane Katrina, synthetic control method

(a) HSAs with 50,000 or more beneficiaries at baseline



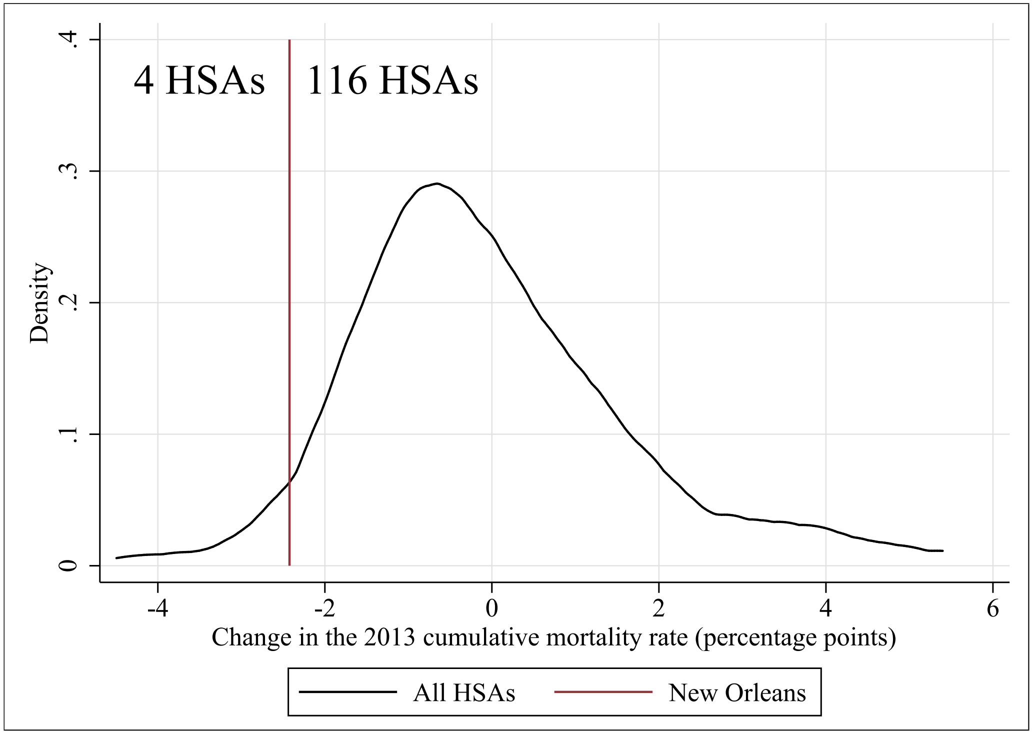
(b) All HRRs



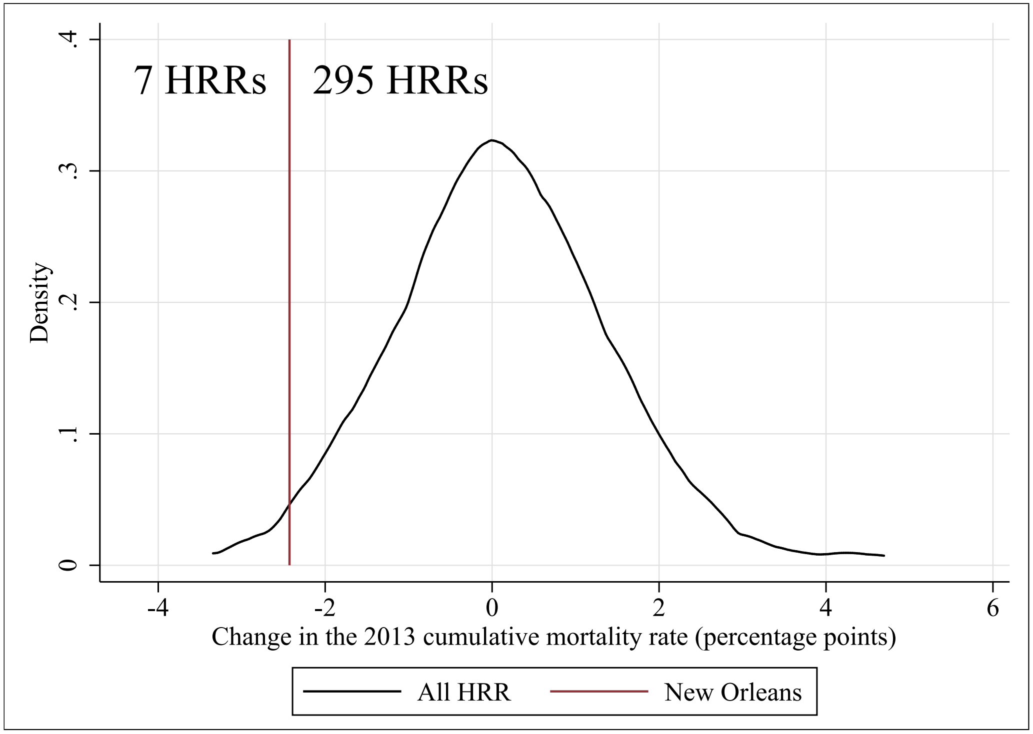
Notes: The black lines plot the effect of Hurricane Katrina on the annual mortality rate of the New Orleans cohort, estimated using the synthetic control method. The gray lines plot the "effect" of a 2005 event for each non-New Orleans HSA/HRR in the sample, also estimated using the synthetic control method. The sample in panel (a) contains 121 HSAs, including New Orleans. The sample in panel (b) contains 303 HRRs, including New Orleans. Estimates have been scaled by 100 to reflect changes in percentage points.

Figure A.11: Cumulative mortality effects of Hurricane Katrina, synthetic control method

(a) HSAs with 50,000 or more beneficiaries at baseline



(b) All HRRs



Notes: The black lines plot the density of the effects of a 2005 event on the 2013 cumulative mortality rates of HSAs/HRRs in our sample, estimated using the synthetic control method. The red line indicates where the New Orleans HSA/HRR falls in that distribution. The added text indicates how many HSAs/HRRs have a cumulative mortality change that is larger/smaller than the cumulative mortality change in New Orleans. The sample in panel (a) contains 121 HSAs, including New Orleans. The sample in panel (b) contains 303 HRRs, including New Orleans.

Table A.1: Estimates for Figure 2

	(1)	(2)	(3)
Event week	Deaths per thousand (New Orleans)	Deaths per thousand (control)	Effect on death rate per thousand people
-8	1.03	1.21	-0.19
			(0.15)
-7	1.43	1.31	0.12
			(0.23)
-6	0.97	1.25	-0.29**
			(0.12)
-5	1.30	1.39	-0.10
			(0.13)
-4	1.16	1.28	-0.14
			(0.11)
-3	1.33	1.27	0.06
			(0.13)
-2	1.52	1.29	0.23
			(0.15)
-1	1.58	1.25	0.32**
			(0.16)
0	6.52	1.35	5.16***
			(0.74)
1	1.87	1.23	0.63***
			(0.14)
2	2.13	1.37	0.75***
			(0.29)
3	1.65	1.22	0.42**
			(0.19)
4	1.74	1.47	0.26*
			(0.16)
5	1.59	1.23	0.35**
			(0.17)
6	1.55	1.27	0.27
			(0.18)
7	1.42	1.22	0.19
			(0.22)
8	1.87	1.35	0.50**
			(0.21)
9	1.77	1.43	0.33*
			(0.19)
10	1.39	1.27	0.11
			(0.15)
Dep. var. mean			1.37
Observations	5,354,225	78,872,928	84,227,152

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Table reports raw weekly mortality rates of the 2004 New Orleans cohort (column (1)), the 2004 control cohorts (column (2)), and estimates of equation (1) from the main text (column (3)). Estimates prior to 8 weeks before Hurricane Katrina and more than 10 weeks after Hurricane Katrina are omitted for space reasons. Mortality rates have been scaled by 1000, implying that each coefficient corresponds to the change in the number of deaths per thousand people. Standard errors (in parentheses) are clustered by beneficiary baseline ZIP codes. Regression includes baseline ZIP code and year fixed effects.

Table A.2: Point estimates for Figure 3

	(1)	(2)	(3)	(4)
Year	Effect on annual mortality rate	Effect on cumulative mortality	$S_\tau \times 100$	Effect on Pr(leaving 2004 HSA)
2005	0.55***	0.55***	100.00	41.6***
	(0.14)	(0.14)		(4.3)
2006	-0.25*	0.32	94.20	40.5***
	(0.15)	(0.24)		(4.3)
2007	-0.36***	-0.01	89.38	36.4***
	(0.13)	(0.33)		(3.9)
2008	-0.48***	-0.42	84.79	31.9***
	(0.14)	(0.41)		(3.2)
2009	-0.52***	-0.84*	80.35	29.9***
	(0.13)	(0.49)		(2.9)
2010	-0.26**	-1.03*	76.14	27.9***
	(0.11)	(0.55)		(2.7)
2011	-0.45***	-1.35**	71.74	26.3***
	(0.15)	(0.63)		(2.5)
2012	-0.20	-1.49**	67.60	25.1***
	(0.15)	(0.71)		(2.4)
2013	-0.39**	-1.74**	63.37	24.1***
	(0.17)	(0.80)		(2.3)
Dep. var. mean	5.49			8.6
New Orleans individuals surviving until 2005			76,436	
Observations	10,162,397	10,162,397		10,162,397

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Columns (1), (2), and (4) report estimates of equations (2) and (3) from the main text. Column (3) reports the empirical survival of the 2004 New Orleans cohort used for cumulative mortality estimates (S_τ). Coefficients and standard errors have been scaled by 100. Standard errors (in parentheses) are clustered by beneficiary baseline ZIP codes. Outcome variables are indicated at top of each column. All regressions include baseline ZIP code and year fixed effects.

Table A.3: Point estimates for Figure 4 and Appendix Figure A.6

	(1)	(2)	(3)
Year	Effect on annual mortality rate (1992 cohort)	Effect on annual mortality rate (1999 cohort)	Effect on Pr(leaving 2004 HSA) (1999 cohort)
1992	0.16		
	(0.16)		
1993	0.02		
	(0.15)		
1994	-0.06		
	(0.18)		
1995	-0.00		
	(0.18)		
1996	-0.08		
	(0.15)		
1997	-0.06		
	(0.16)		
1998	0.01		
	(0.15)		
1999	0.06	-0.01	-1.52**
	(0.16)	(0.16)	(0.63)
2000	-0.27	-0.22	-0.91**
	(0.17)	(0.14)	(0.44)
2001	-0.12	-0.09	-0.92**
	(0.17)	(0.12)	(0.40)
2002	-0.06	-0.03	-0.48**
	(0.15)	(0.12)	(0.21)
2003	0.04	0.08	-0.26**
	(0.18)	(0.16)	(0.11)
2005	1.05***	0.78***	37.00***
	(0.23)	(0.20)	(3.95)
2006	-0.35	-0.37*	36.63***
	(0.26)	(0.21)	(4.13)
2007	-0.67***	-0.53***	33.17***
	(0.25)	(0.17)	(3.77)
2008	-0.62**	-0.53***	29.18***
	(0.28)	(0.17)	(3.21)
2009	-0.73***	-0.63***	27.22***
	(0.24)	(0.17)	(3.00)
2010	-0.32	-0.30**	25.44***
	(0.31)	(0.15)	(2.80)
2011	-0.66**	-0.55**	23.98***
	(0.33)	(0.23)	(2.65)
2012	-0.44	-0.14	22.86***
	(0.44)	(0.24)	(2.56)
2013	-0.85**	-0.41*	21.89***
	(0.42)	(0.23)	(2.40)
Dep. var. mean	0.07	0.06	0.10
Observations	14,473,864	12,820,287	12,820,287

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equation (2) from the main text. Coefficients and standard errors have been scaled by 100. Standard errors (in parentheses) are clustered by beneficiary baseline ZIP codes. The dependent variable is indicated at the top of each column. All regressions include ZIP code and year fixed effects.

Table A.4: Heterogeneous mortality effects of Hurricane Katrina (2004 Medicare cohort)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Difference-in-differences estimates						
	Short-run (2005)		Long-run (2006-2013)				
Baseline var	NOLA x 2005	NOLA x 2005 x var	NOLA x (2006-2013)	NOLA x (2006-2013) x var	Percent var=1 in NOLA, 2004	Mean mortality if var=1 in NOLA, 2004	Observations
1	0.55***		-0.36***		100.0		10,162,396
	(0.14)		(0.11)				
Flooded	0.15	0.55**	-0.24	-0.24	64.4	5.2	8,886,088
	(0.16)	(0.21)	(0.20)	(0.22)			
Below median income	0.60***	-0.23	-0.14	-0.55**	49.7	5.4	8,886,088
	(0.22)	(0.29)	(0.16)	(0.23)			
64 or younger at baseline	0.74***	-0.85***	-0.32**	0.04	21.2	2.9	10,162,395
	(0.16)	(0.29)	(0.14)	(0.23)			
75 or older at baseline	0.03	1.31***	-0.39***	0.06	40.7	8.1	10,162,395
	(0.13)	(0.31)	(0.09)	(0.23)			
Black	0.81***	-0.49	-0.11	-0.35	51.7	5.0	10,162,395
	(0.25)	(0.35)	(0.14)	(0.22)			
Male	0.46***	0.21	-0.32**	-0.11	43.2	5.4	10,162,395
	(0.16)	(0.19)	(0.14)	(0.19)			
End-stage renal disease	0.57***	0.16	-0.29**	-1.92	1.9	19.9	10,162,395
	(0.14)	(1.32)	(0.12)	(1.35)			
Heart disease and stroke	0.53**	0.21	-0.71***	-0.36	64.8	7.7	7,133,489
	(0.25)	(0.35)	(0.16)	(0.33)			
Respiratory disease	0.37**	1.27**	-1.03***	-0.06	12.8	12.0	7,766,417
	(0.16)	(0.53)	(0.17)	(0.51)			
Blood and kidney disease	1.00***	-0.74***	-0.54***	-0.91***	47.2	8.4	7,133,489
	(0.18)	(0.26)	(0.13)	(0.30)			
Cancer	0.52***	0.15	-1.01***	-0.27	6.9	12.8	7,766,417
	(0.17)	(1.04)	(0.17)	(0.66)			
Diabetes	0.61***	0.22	-0.80***	-0.42	26.8	8.7	7,133,489
	(0.19)	(0.29)	(0.14)	(0.26)			
Musculoskeletal	0.31	1.24**	-1.16***	0.83***	28.7	6.3	7,133,489
	(0.23)	(0.49)	(0.16)	(0.31)			
Alzheimer's/dementia	0.46***	3.14***	-0.79***	-1.47*	12.5	19.9	6,505,419
	(0.17)	(0.93)	(0.16)	(0.89)			
Other chronic condition	0.45**	0.38	-1.22***	0.74***	40.4	4.5	7,766,417
	(0.22)	(0.29)	(0.18)	(0.22)			

Each row reports summary statistics along with short-run (2005) and long-run (2006-2013) mortality effects estimated from a difference-in-differences model where the effect may vary by the individual, baseline characteristic, var, specified by the row. Observations are at the individual-year level, and include all Medicare beneficiaries living in New Orleans or one of the 10 control cities in 2004 and who were alive at the beginning of the year of observation. The outcome in each regression is an indicator for whether an individual died that year. All regressions control for baseline ZIP code and calendar year fixed effects. For characteristics that vary within the control cities, regressions further include interactions between the characteristic and calendar-year fixed effects. Standard errors clustered by baseline ZIP code are reported in parentheses. A */**/*** indicates significance at the 10%/5%/1% levels, respectively.

Table A.5: Estimates for Appendix Figure A.5

	(1)	(2)	(3)	(4)	(5)	(6)
Year	Mortality rate			Cumulative mortality		
	1992 cohort	1999 cohort	2004 cohort	1992 cohort	1999 cohort	2004 cohort
1992	0.21					
	(0.15)					
1993	0.11					
	(0.14)					
1994	0.07					
	(0.17)					
1995	0.09					
	(0.17)					
1996	0.01					
	(0.15)					
1997	0.01					
	(0.15)					
1998	0.13					
	(0.14)					
1999	0.22	0.05				
	(0.15)	(0.15)				
2000	-0.15	-0.18				
	(0.16)	(0.14)				
2001	-0.07	-0.10				
	(0.16)	(0.12)				
2002	0.02	-0.01				
	(0.14)	(0.12)				
2003	0.04	0.06				
	(0.17)	(0.16)				
2005	0.88***	0.69***	0.50***	0.88***	0.69***	0.50***
	(0.22)	(0.19)	(0.13)	(0.22)	(0.19)	(0.13)
2006	-0.47*	-0.41**	-0.25*	0.46	0.30	0.27
	(0.25)	(0.21)	(0.14)	(0.40)	(0.34)	(0.24)
2007	-0.75***	-0.53***	-0.37***	-0.16	-0.15	-0.06
	(0.24)	(0.16)	(0.12)	(0.55)	(0.46)	(0.32)
2008	-0.77***	-0.60***	-0.53***	-0.75	-0.65	-0.51
	(0.27)	(0.17)	(0.14)	(0.70)	(0.56)	(0.40)
2009	-0.94***	-0.74***	-0.57***	-1.40*	-1.21*	-0.97**
	(0.22)	(0.17)	(0.12)	(0.82)	(0.66)	(0.47)
2010	-0.67**	-0.42***	-0.29***	-1.82*	-1.51**	-1.19**
	(0.30)	(0.14)	(0.10)	(0.96)	(0.73)	(0.53)
2011	-1.16***	-0.77***	-0.58***	-2.48**	-2.02**	-1.60***
	(0.32)	(0.23)	(0.15)	(1.12)	(0.86)	(0.61)
2012	-0.87**	-0.38	-0.33**	-2.92**	-2.25**	-1.82***
	(0.43)	(0.24)	(0.15)	(1.23)	(0.96)	(0.69)
2013	-1.38***	-0.64***	-0.48***	-3.55**	-2.61**	-2.13***
	(0.40)	(0.22)	(0.16)	(1.39)	(1.06)	(0.77)
Dep. var. mean	6.79	5.96	5.17			
Observations	435,790,496	409,852,288	340,827,168	435,790,496	409,852,288	340,827,168

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equations (2) and (3) from the main text. Standard errors (in parentheses) are clustered by beneficiary baseline ZIP codes. Dependent variables are indicated at top of each column. All regressions include baseline ZIP code and year fixed effects.

Table A.6: Predictors of leaving New Orleans and of destination characteristics, with ZIP code fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Whether moved		Local mortality rate		Local Medicare spending	
Black	0.122***	0.123***	-0.0055	-0.0095	203	175
	(0.038)	(0.037)	(0.0258)	(0.0261)	(201)	(209)
Male	-0.038***	-0.038***	0.0124***	0.0055	-21	-21
	(0.004)	(0.004)	(0.0041)	(0.0057)	(24)	(28)
64 and younger	0.114***	0.115***	-0.0061	-0.0056	55	-7
	(0.007)	(0.007)	(0.0087)	(0.0094)	(61)	(70)
75 and older	0.015*	0.005	-0.0025	-0.0101	10	-31
	(0.008)	(0.006)	(0.0053)	(0.0082)	(29)	(37)
Below median income	0.042***	0.057***	0.0067	0.0118	25	12
	(0.008)	(0.009)	(0.0062)	(0.0077)	(30)	(39)
Katrina flood level, feet	0.019***	0.021***	0.0018	0.0016	-6	-3
	(0.003)	(0.003)	(0.0013)	(0.0016)	(7)	(9)
End-stage renal disease	0.019**	-0.023	0.0234	-0.0106	58	-40
	(0.009)	(0.016)	(0.0143)	(0.0189)	(61)	(97)
2004 medical spending, thousands		0.001***		0.0004**		1
		(0.000)		(0.0002)		(1)
Alzheimer's/dementia		0.004		0.0392***		19
		(0.016)		(0.0146)		(61)
Respiratory disease		0.041***		-0.0142		-89**
		(0.005)		(0.0095)		(44)
Heart disease and stroke		0.012**		0.0080		23
		(0.005)		(0.0065)		(30)
Blood and kidney disease		0.008		-0.0092		-19
		(0.006)		(0.0072)		(37)
Diabetes		0.002		0.0071		65
		(0.006)		(0.0071)		(41)
Musculoskeletal		-0.002		-0.0022		-3
		(0.005)		(0.0072)		(30)
Cancer		-0.042***		-0.0168		-57
		(0.011)		(0.0102)		(54)
Other		0.005		-0.0026		16
		(0.004)		(0.0060)		(35)
p-value of joint F-test	< 0.001	< 0.001	0.028	0.085	0.752	0.491
Observations	62,690	28,061	27,678	12,844	27,678	12,844

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) are clustered by beneficiary 2006 HSAs. The dependent variable is specified at the top of each column. All regressions include baseline 5-digit ZIP code fixed effects.

Table A.7: Migrant mortality over time, by destination mortality (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
Mean death rate x (2006-2007)	1.15***	1.15***	1.14***	0.93*	0.88*
	(0.38)	(0.40)	(0.38)	(0.52)	(0.51)
Mean death rate x (2008-2013)	0.69**	0.77***	0.79***	0.80**	0.85**
	(0.28)	(0.23)	(0.24)	(0.33)	(0.33)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	None	None	None	Grouped	Interactions
Dep. var. mean	5.63	5.63	5.63	6.22	6.21
Observations	213,697	213,697	213,679	97,927	97,918
R-squared	0.00	0.04	0.04	0.06	0.07

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equation (6). Standard errors (in parentheses) are clustered by a beneficiary's 2006 HSA. The dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes baseline ZIP code and year fixed effects; B also includes fixed effects for each 5-year-age-bin, race, and gender combination. C additionally allows the year fixed effects to differ by each 5-year-age-bin, race, and gender combination.

Table A.8: Migrant mortality by cause of death, by destination mortality (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
	All causes	Cardiovascular	Cancer	Other internal causes	External causes
Mean death rate in 2006 HSA	0.90*** (0.23)	0.31** (0.15)	0.24** (0.12)	0.55*** (0.20)	-0.005 (0.034)
Dep. var. mean	5.63	1.95	1.12	2.05	0.130
Observations	213,679	91,558	91,558	91,558	91,558

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equation (6). Standard errors (in parentheses) are clustered by a beneficiary's 2006 HSA. Dependent variable is an indicator equal to 1 if the beneficiary died in a given year from the cause of death specified in the column and equal to 0 if a beneficiary was alive during the entire calendar year or died that year from a different cause. Coefficients and standard errors have been scaled by 100. All specifications include 5-year-age-bins-by-gender-by-race-by-year fixed effects and baseline ZIP code fixed effects.

Table A.9: Migrant mortality, by destination mortality and spending jointly (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
Mean death rate in 2006 HSA	0.88***	0.91***	0.92***	0.87***	0.89***
	(0.24)	(0.22)	(0.23)	(0.30)	(0.30)
Mean spending in 2006 HSA, thousands	-0.08*	-0.04	-0.04	-0.05	-0.06
	(0.05)	(0.03)	(0.03)	(0.05)	(0.05)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	None	None	None	Grouped	Interactions
Dep. var. mean	5.63	5.63	5.63	6.22	6.21
Observations	213,697	213,697	213,679	97,927	97,918
R-squared	0.00	0.04	0.04	0.06	0.07

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equation (6). Standard errors (in parentheses) are clustered by a beneficiary's 2006 HSA. The dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes baseline ZIP code and year fixed effects; B also includes fixed effects for each 5-year-age-bin, race, and gender combination. C additionally allows the year fixed effects to differ by each 5-year-age-bin, race, and gender combination.

Table A.10: Migrant mortality, by destination mortality and spending (2005–2007 New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
Panel A: Local mortality					
Mean death rate in 2007 HSA	0.69*** (0.25)	0.70*** (0.24)	0.71*** (0.24)	0.60* (0.32)	0.57* (0.33)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	None	None	None	Grouped	Interactions
Dep. var. mean	5.95	5.95	5.95	6.51	6.50
Observations	201,659	201,659	201,635	94,349	94,339
R-squared	0.00	0.04	0.04	0.06	0.07
Panel B: Local spending					
Mean spending in 2007 HSA, thousands	-0.05 (0.05)	-0.02 (0.04)	-0.02 (0.04)	-0.03 (0.06)	-0.03 (0.06)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	None	None	None	Grouped	Interactions
Dep. var. mean	5.95	5.95	5.95	6.51	6.50
Observations	201,659	201,659	201,635	94,349	94,339
R-squared	0.00	0.04	0.04	0.06	0.07

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) are clustered by a beneficiary's 2007 HSA. The dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes baseline ZIP code and year fixed effects; B also includes fixed effects for each 5-year-age-bin, race, and gender combination. C additionally allows the year fixed effects to differ by each 5-year-age-bin, race, and gender combination.

Table A.11: Migrant mortality, by destination mortality and spending (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)	(6)
Mean death rate in destination HSA	0.77*** (0.29)	0.60* (0.32)	0.85** (0.36)	0.91*** (0.35)	0.71** (0.28)	0.73*** (0.28)
Chronic conditions controls	Individual	Individual	Grouped	Interactions	Grouped	Interactions
Sample	All 2006 movers	2007 movers	No Houston/B.R.	No Houston/B.R.	No ind. with Alzh.	No ind. with Alzh.
Dep. var. mean	6.22	6.51	6.22	6.21	5.58	5.58
Observations	97,927	94,349	76,919	76,849	91,904	91,900
R-squared	0.07	0.07	0.06	0.07	0.05	0.06

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. The table reports estimates of equation (6). Standard errors (in parentheses) are clustered by a beneficiary's destination HSA in the year of the move. Dependent variable is an indicator equal to 0 if a beneficiary was alive during the entire calendar year and equal to 1 if the beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. All specifications include 5-year-age-bins-by-gender-by-race-by-year fixed effects and baseline ZIP code fixed effects.

Table A.12: Predictors of returning to New Orleans

	(1)	(2)	(3)	(4)	(5)	(6)
	Returned by March of 2007			Returned by December 31, 2010		
Mean death rate in 2006 HSA	-0.140	0.180	0.016	-1.470	-1.388	-1.294
	(1.707)	(1.402)	(1.752)	(3.469)	(2.476)	(2.721)
Black		0.072***	0.065***		0.229***	0.241***
		(0.009)	(0.011)		(0.014)	(0.015)
Male		0.011***	0.006		0.005	0.006
		(0.004)	(0.007)		(0.007)	(0.011)
64 and younger		-0.036***	-0.031***		-0.059***	-0.059***
		(0.007)	(0.009)		(0.010)	(0.011)
75 and older		-0.044***	-0.029***		-0.044***	-0.042***
		(0.005)	(0.008)		(0.009)	(0.013)
Below median income		-0.014***	0.002		-0.019***	-0.017
		(0.005)	(0.007)		(0.007)	(0.011)
Katrina flood level, feet		-0.013***	-0.013***		-0.009***	-0.009***
		(0.001)	(0.002)		(0.002)	(0.003)
End-stage renal disease		-0.049***	-0.034		-0.025	-0.026
		(0.014)	(0.029)		(0.027)	(0.041)
2004 medical spending, thousands			-0.000			-0.000
			(0.000)			(0.000)
Alzheimer’s/dementia			-0.033***			-0.078***
			(0.011)			(0.019)
Respiratory disease			-0.021**			-0.016
			(0.009)			(0.018)
Heart disease and stroke			-0.005			-0.007
			(0.009)			(0.012)
Blood and kidney disease			-0.005			-0.002
			(0.008)			(0.016)
Diabetes			-0.014			-0.005
			(0.010)			(0.013)
Musculoskeletal			-0.002			0.002
			(0.008)			(0.010)
Cancer			0.015			0.021
			(0.015)			(0.020)
Other			-0.014**			-0.026**
			(0.006)			(0.011)
Observations	32,213	27,678	12,844	25,864	22,207	10,042
R-squared	0.000	0.017	0.018	0.000	0.047	0.059

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) are clustered by beneficiary 2006 HSAs. The dependent variable is specified at the top of each column.