Does When You Die Depend on Where You Live? Evidence from Hurricane Katrina*

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Abstract

We follow Medicare cohorts to estimate Hurricane Katrina's long-run mortality effects on victims initially living in New Orleans. Including the initial shock, the hurricane *improved* eight-year survival by 2.07 percentage points. Migration to lower-mortality regions explains most of this survival increase. Those migrating to low- versus high-mortality regions look similar at baseline, but their subsequent mortality is 0.83–1.01 percentage points lower per percentage-point reduction in local mortality, quantifying causal effects of place on mortality among this population. Migrants' mortality is also lower in destinations with healthier behaviors and higher incomes but is unrelated to local medical spending and quality.

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1 Introduction

Hurricane Katrina, the costliest storm in US history, devastated the Gulf Coast in 2005. The immediate impact of the storm killed nearly 2,000 individuals and displaced more than one million residents, resulting in the largest migration of US residents since the Dust Bowl of the 1930s (Nigg, Barnshaw and Torres, 2006). Climate models predict that such extreme weather events will increase in frequency and severity (Field et al., 2012), yet little is known about the effects of environmental catastrophes on long-run health and longevity, which represent considerable economic value (Murphy and Topel, 2006).

The disruption induced by extreme weather events can be used to illuminate factors that affect the accumulation or depreciation of health capital (Grossman, 1972). For example, when a disaster displaces people from their homes, the regions they move to may play an important role in shaping long-term health outcomes. While life expectancy varies significantly across US locations (Chetty et al., 2016; Dwyer-Lindgren et al., 2017), little is known about the extent to which this variation reflects the causal effect of place on health. Because Hurricane Katrina displaced many survivors, it can serve as a natural experiment of how place affects long-term health. If the geographic variation in life expectancy reflects causal effects of place, then the widespread migration out of New Orleans to regions with better economic and health conditions may have generated health benefits.

This paper has two primary aims. The first is to estimate the short- and long-run mortality impacts of Hurricane Katrina on the elderly and the long-term disabled of New Orleans. Prior studies have evaluated how demographic and economic outcomes evolve after environmental catastrophes (e.g., Hornbeck, 2012; Hornbeck and Naidu, 2014; Nakamura, Sigurdsson and Steinsson, 2017), but little is known about the long-run health effects of these events, especially among adults. The second aim is to estimate how the mortality of displaced survivors was shaped by their destinations.

Quantifying the long-run health impacts of events like Hurricane Katrina has proven difficult, largely due to lack of data that capture pre-disaster outcomes and track individuals post-disaster with minimal attrition. To overcome this challenge, we use Medicare administrative data from 1992–2013 that allow us to follow elderly and long-term disabled individuals over time and space and to provide exact dates of death. We identify Medicare cohorts living in New Orleans just prior to Hurricane Katrina and trace out their mortality rates up to eight years after the storm, regardless of whether or where they move.

To identify how outcomes would have evolved in the absence of Hurricane Katrina, we measure mortality for comparable Medicare cohorts initially residing in ten cities that were not directly affected by the hurricane, following Deryugina, Kawano and Levitt (2018). To validate this control group, we show that mortality trends in the New Orleans and control city cohorts were similar prior to Hurricane Katrina as far back 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 (i.e., a cohort-level difference-in-differences event study analysis).[1]

We find that in 2005, the year of the storm, Hurricane Katrina increased mortality by 0.56 percentage points (over 10 percent of the mean). Most of these excess deaths occurred within a week of the hurricane's landfall, and this immediate effect dissipated over several months. In contrast to the short-run mortality increase, we find that Hurricane Katrina led to sustained *reductions* in mortality from 2006 to 2013. This long-run mortality decline is not explained by short-run mortality displacement, or "harvesting." Inclusive of the initial increase in mortality, Hurricane Katrina increased the probability of surviving through 2013, eight years past the storm, by 2.07 percentage points, a 3.2 percent increase relative to the overall eight-year survival rate of the 2005 New Orleans cohort.

We also find that the hurricane led to a massive and lasting dislocation of the elderly and long-term disabled, consistent with prior evidence on the demographic effects of the hurricane (Deryugina, Kawano and Levitt, 2018; Eyer et al., 2018). Medicare beneficiaries

[1] As we show, our central findings are robust to using the entire United States (except New Orleans) or the top 20 mortality regions to define 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.

living in New Orleans as of March 2005 were nearly 50 percentage points more likely to leave their commuting zone of residence than members of the control group. Over half of those who left had not returned as of 2013, conditional on being alive. New Orleans was one of the highest mortality areas in the country prior to Hurricane Katrina, and displaced individuals generally moved to lower-mortality regions. To the extent that local mortality outcomes reflect place-specific determinants of health, relocation to lower-mortality regions may have driven the decline in mortality among Hurricane Katrina victims.

To directly examine the role of place on health, we examine the mortality patterns among New Orleans residents who had moved away by March 2006. 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 0.83–1.01 percentage-point increase in the movers' mortality rate. This effect emerges quickly after the move, 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 movers' ex ante predicted mortality, which we construct from detailed data on demographic characteristics, past health care utilization, and pre-existing chronic conditions. In addition, the estimated relationship between movers' mortality and destination mortality is highly stable, even with rich controls, including variables that are strongly predictive of mortality. Changes in the local mortality rate experienced by hurricane victims can explain over 70 percent of the long-run mortality decline caused by the hurricane.

Next, we examine how other local attributes correlate with movers' mortality. We find that movers' mortality increases with local rates of obesity or smoking and decreases with average income, home values, and urban population share. Movers' mortality is not statistically significantly correlated with local physician or hospital bed supply or a hospital quality index. We also do not find a relationship between movers' mortality and local health care spending, shedding new light on a long-standing question of whether higher-spending regions generate better health outcomes than lower-spending regions and pointing to low returns to living in a high-spending region.[2]

Our study adds to a growing body of literature that uses migration to identify how local conditions affect individual outcomes. Song et al. (2010), Finkelstein, Gentzkow and Williams (2016), and Molitor (2018) study movers in Medicare to identify local determinants of diagnosis rates, medical spending, and physician practice styles, respectively. Movers have also been used to study how local conditions affect education and earnings (Chetty, Hendren and Katz, 2016; Nakamura, Sigurdsson and Steinsson, 2017; Chyn, 2018; Chetty and Hendren, 2018*a*), income reporting in tax filings (Chetty, Friedman and Saez, 2013), and brand preferences (Bronnenberg, Dubé and Gentzkow, 2012). Black et al. (2015) and Johnson and Taylor (2019) estimate mortality effects of migration but do not study how migrant outcomes depend on destination characteristics.

We contribute to this literature by studying how the long-run mortality outcomes of those displaced by Hurricane Katrina depend on the destination region. Our finding that migrants' realized mortality outcomes correspond closely to their destination region's mortality rate suggests that local conditions are an important determinant of individual health outcomes. Our study complements contemporaneous work by Finkelstein, Gentzkow and Williams (2019), who estimate the mortality effects of place among Medicare movers using a novel method to account for potentially endogenous sorting on unobservables. They also find that current location matters greatly for life expectancy, although the correlation they

[2]Higher spending regions often have no 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), but the direction of causality is not clear from these correlations alone. Doyle (2011) and Doyle et al. (2015) address this limitation by analyzing quasi-random assignment of patients to hospitals and find that patients have better outcomes when treated at higher-spending hospitals. Yet the returns to being hospitalized in a high-spending region may differ from the returns to *living* in a high-spending region (e.g., higher-quality health systems could reduce the need for hospitalization).

find between the effect of place on mortality and local mortality is smaller than what we estimate. Because Hurricane Katrina displaced many people who would not have otherwise moved, this pattern suggests that place may matter less for the typical mover than for the typical stayer, analogous to evidence of negative selection on children's economic gains among movers to a better neighborhood Chyn (2018).

Our results also shed light on why life expectancy differs across the United States and on how health capital accumulates over the life cycle. Regional differences in life expectancy correlate strongly with numerous demographic factors and health behaviors, like income and smoking (Chetty et al., 2016; Dwyer-Lindgren et al., 2017). 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 long-term disabled.

We also provide the longest-run controlled estimates of the mortality effects of a disaster on adult victims. Prior research on disasters and health has been largely limited to considering birth outcomes and infant health (e.g., Torche, 2011; Currie and Rossin-Slater, 2013; Currie and Schwandt, 2016), immediate post-disaster mortality (e.g., Kahn, 2005), or survey-based measures of longer-run health for a subset of the victims.[3] Survey approaches, however, generally suffer from non-random sampling, rarely measure pre-existing outcomes, and usually lack a control group. By contrast, our data track the mortality and location of every Medicare-eligible individual. Our finding that Hurricane Katrina reduced long-run mortality among the elderly and long-term disabled populations builds on recent evidence that the hurricane indirectly generated other long-run benefits, including higher earnings

[3]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).

among the working-age population (Groen, Kutzbach and Polivka, 2016; Deryugina, Kawano and Levitt, 2018) and improved test scores among displaced students (Sacerdote, 2012).

The paper proceeds as follows. Section 2 provides an overview of Hurricane Katrina. Section 3 describes the data and estimation sample. 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, resulting in an 80–90 percent evacuation rate (Wolshon, 2006). Hurricane Katrina struck the city on August 29 as a Category 3 hurricane with sustained winds of 125 miles per hour. The storm surge caused numerous levee and flood wall failures, resulting in widespread flooding (see Appendix Figure A.1). The hurricane winds and flooding together severely damaged thousands of homes and other infrastructure throughout New Orleans. According to the National Oceanic and Atmospheric Administration (NOAA), Hurricane Katrina caused $161 billion in direct damages (2017 dollars), making it the costliest US natural disaster on record (National Hurricane Center, 2018; NOAA, 2018).

Hurricane Katrina's official death toll was 1,833, also making it the deadliest US natural disaster since the 1928 Okeechobee hurricane (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). The storm further displaced an estimated one million individuals living in its path (Nigg, Barnshaw and Torres, 2006), including up to 200,000 Medicare beneficiaries (Super and Biles, 2005). The Federal Emergency Management Agency (FEMA) prohibited most New Orleans residents from returning home for months.[4] Because of the widespread damages across Louisiana, many of the displaced were transported to other states. Texas received about 200,000 evacuees, and Arkansas received about 50,000, but refugees were also evacuated to at least 32 other states (Grier, 2005). Storm victims unable to find suitable living arrangements were given housing assistance, but little systematic information is available on how victims chose where to relocate in the longer run.

The aid response to Hurricane Katrina was considerable.[5] The federal government provided about $50 billion in disaster aid to Louisiana, excluding flood insurance payments and loans. Most 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 to rebuild or sell their homes. Finally, FEMA also paid about $320 million in Disaster Unemployment Assistance in the state of Louisiana. In total, aid spending for the city of New Orleans was as much as $125,000 per capita, of which about $17,000 consisted of direct transfers to individuals (Deryugina, Kawano and Levitt, 2018). Absent the massive aid response, the long-term impacts of Hurricane Katrina could have been much worse than what occurred.

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

[4] Residents in 17 out of 19 New Orleans ZIP codes were not allowed to return to their homes before December 9, 2005 (FEMA, 2005). On that date, residents of 10 of the 17 ZIP codes could return to their homes and stay ("look-and-stay" ZIP codes); residents in the other 7 ZIP codes could visit their homes but only during the day ("look-and-leave" ZIP codes).

[5] Deryugina, Kawano and Levitt (2018) provide a detailed description of Hurricane Katrina aid components.

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 New Orleans 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, sprang 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 long-term 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.[6]

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 were more conducive to good health. After we estimate the aggregate effects of Hurricane Katrina on long-run mortality among the elderly and long-term disabled, we return to consider the role of migration and place in shaping the recovery of the hurricane victims.

[6]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.

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 US 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 (ESRD) or amyotrophic lateral sclerosis.

Medicare data offer two features essential for studying health dynamics in our setting. First, Medicare reports in each year the ZIP code of each beneficiary where Social Security Administration (SSA) benefits and official communication are mailed, which we refer to as the "ZIP code of residence." This 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.[7] Second, Medicare records each individual's exact date of death based on SSA records.

Our analysis relies on four sets of annually recorded Medicare variables.[8] The first set comes from Medicare eligibility records and contains beneficiary identifiers and demographic information obtained from SSA records, including nine-digit ZIP code, race, sex, birth and death dates, and an ESRD 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,

[7]When constructing our panel, we drop individuals who disappear from the annual beneficiary summary file prior to a recorded death or who have gaps in enrollment. For the 2004 cohort, these restrictions drop 0.8 percent of the full sample.

[8]See Appendix A.1 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.

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 measures 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 includes 27 indicators for common chronic conditions inferred from medical claim histories. We group the 27 individual conditions into eight broad categories: heart disease and stroke, respiratory disease, blood and kidney disease, cancer, diabetes, musculoskeletal diseases, Alzheimer's/dementia, and other (cataracts, glaucoma, hypothyrodism, benign prostatic hyperplasia, and depression). These claims-based chronic condition indicators 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 comes 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 counties. In some cases, we consider commuting zones (CZs), as defined by the Economic Research Service of the US Department of Agriculture for the year 2000. CZs aggregate counties into 709 regions based on work commute patterns. We refer to a county by the primary city located in the county and use the terms "city" and "county" interchangeably, even though the boundary of the county may extend beyond the city's political boundary. For New Orleans, a consolidated city-county, the county and city boundaries are identical.

We match Medicare beneficiaries to flooding and income neighborhood characteristics based on their nine-digit ZIP code of residence at baseline, as geocoded by GeoLytics.[9] We calculate Hurricane Katrina flood depth for each nine-digit ZIP code as the average flood depth within a 50-meter radius of the centroid, using NOAA flood data at a resolution of five meters. We classify beneficiaries as "flooded" if their nine-digit ZIP code of residence experienced two or more feet of 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 nine-digit ZIP code. We classify beneficiaries as "below median income" if the neighborhood income measure is below the median neighborhood income in New Orleans at baseline.

Finally, to shed light on the relationship between mover mortality and specific local characteristics, we obtain several county-level attributes related to public health, the environment, and economic conditions, including average Medicare spending, the number of physicians per capita, smoking and obesity rates, fine particulate matter (PM 2.5) levels, frequency of temperature extremes, the crime rate, local government expenditure, and per-capita income. The construction of these variables is detailed in Appendix Section A.1.4.

3.2 Estimation Sample

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 approach to estimating counterfactual outcomes relies on examining how outcomes evolve among groups of Medicare beneficiaries initially residing in other regions. Our primary control regions are the ten US cities with

[9] 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 five-digit ZIP code or to the additional 8.2 percent of sample beneficiaries whose nine-digit ZIP codes are not geocoded by GeoLytics.

a population of at least 100,000 chosen by Deryugina, Kawano and Levitt (2018) to most closely matched New Orleans in the years 2000–2005 along three demographic dimensions: median earnings, the population growth rate, and the percent of the 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). We discuss the robustness of our results to the choice of control group in Section 5.2.2.

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 one 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 ("treatment group") or one of the ten control cities ("control group") form our preferred sample for estimating the long-run effects of Hurricane Katrina. Table 1 summarizes the baseline (2004) characteristics of this sample, which contains 65,457 beneficiaries from New Orleans (column (1)) and 941,685 beneficiaries from the ten control cities (column (2)). The gender and age distributions and average incomes of New Orleans and control city beneficiaries are similar. However, about 61 percent of the New Orleans individuals are black, compared to 39 percent of the control city beneficiaries. Seventy-eight percent of New Orleans beneficiaries are 65 and older at baseline, implying that the rest (22 percent) qualify for Medicare because of a disability. Fifty-seven percent of New Orleans beneficiaries lived in nine-digit ZIP codes that experienced two or more feet of flooding during Hurricane Katrina.

Sixty-nine percent of New Orleans beneficiaries were enrolled in fee-for-service Medicare in 2004, compared to 93 percent of control city beneficiaries. The 2004 Medicare spending of fee-for-service beneficiaries is slightly higher for treatment than for control individuals ($10,460 and $9,640, respectively). New Orleans beneficiaries are slightly more likely to have ESRD but are somewhat less likely to have each of the other eight chronic condition groups, such as diabetes, blood and kidney disease, and heart disease/stroke.

Table 1 also summarizes the characteristics of the 26,467 New Orleans beneficiaries that left the New Orleans CZ between March 2005 and March 2006 ("movers," column (3)). Compared to the average New Orleans beneficiary, movers are younger, poorer, more likely to be black (76 percent versus 61 percent), and more likely to have experienced at least two feet of flooding. They are slightly less likely to have some chronic conditions (e.g., heart disease and cancer) but are more likely to have others (e.g., diabetes and respiratory disease).

Appendix Table A.2 shows the top 20 destination counties for the New Orleans movers. These destinations account for slightly over 50 percent of all movers, but only two destinations have more than 5 percent of migrants moving there: Harris County, Texas, which contains the city of Houston (14.2 percent of migrants), and East Baton Rouge, Louisiana, which contains the city of Baton Rouge (9.6 percent of migrants). Most of the other top 20 destinations attracted 1–2 percent of movers. The table also implies that nearly half of the movers relocated to destinations that attracted less than 1 percent of the movers.

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 region of residence.[10] 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 ten 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 CZ 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

[10]Appendix Table A.1 lists the raw annual mortality rates of the 1992, 1999, and 2004 New Orleans cohorts and the corresponding control city cohorts. Appendix Figure A.4 shows annual death rates for the 1999 Medicare cohort that are adjusted for all combinations of beneficiaries' current age (in one-year bins), race, and sex.

rates in the United States. Cohorts from the ten 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.

Mortality decreases after 2005 among the New Orleans cohort are also not explained by regression to the mean, which could occur if regions with relatively high cohort mortality rates in 1999–2005 tend to experience relatively lower cohort mortality rates in later years. In Appendix Figure A.5, we partition non-New Orleans commuting zones into groups based on the average mortality rate of their 1999 cohort over the "pre-period" 1999–2005 and plot cohort mortality for each group in 1999–2013. Panel (a) plots each group's annual mortality rates, while panel (b) plots cumulative mortality rates. Annual cohort mortality trends roughly in parallel across CZ groups, indicating that cohorts with relatively high mortality rates from 1999–2005 do not experience relatively lower mortality in 2006–2013. Prior to 2005, cumulative mortality rates for the New Orleans county cohort are similar to the average cumulative mortality of other CZ cohorts that had the 20 highest pre-period mortality rates. Following Hurricane Katrina, however, these cumulative mortality paths diverge. By 2013, cumulative mortality in the New Orleans cohort is 2.35 percentage points lower than it is in the other top 20 pre-period mortality CZ cohorts. This difference in cumulative mortality is similar to the main cumulative mortality effect we estimate formally below.

4 Research Design

4.1 Short-Run Effects of Hurricane Katrina

We estimate the short-run mortality effects of Hurricane Katrina on 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 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, which corresponds to weeks starting on January 3, 2005, and on November 27, 2006, respectively. We 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, then $Died_{it}$ 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 five-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. Standard errors are clustered by baseline ZIP code.

The focal parameters in equation (1) are β_t, the coefficients on the interactions of event week and treatment indicators. 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). 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 Relocation 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

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

where the outcome Y_{it} measures either mortality or relocation. The mortality outcome, $Died_{it}$, equals zero if individual i survived through year t and equals one if he or she died that year. The relocation outcome, $LeftCZ_{it}$, equals zero if the individual resided in their baseline CZ in year t and equals one if he or she was living in another CZ. Both outcomes are set to missing in years after death.

For simplicity, we first estimate equation (2) with no demographic controls beyond 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 one-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 county and CZ 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 examine pre-trends over a long time horizon, but these cohorts may only partially capture Hurricane Katrina's impact on Medicare victims, as about two-thirds (one-third) of individuals in the 1992 (1999) cohort had moved away or died before 2005.[11] 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.

[11] By 2004, 6,600 of the 71,433 individuals (9.2 percent) from the 1992 New Orleans cohort were living outside of New Orleans and 39,500 (55.3 percent) had died. Among the 1999 New Orleans cohort, 5,310 of the 67,649 individuals (7.8 percent) were living outside of New Orleans and 17,434 (25.8 percent) had died by 2004.

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.[12] Specifically, for each post-Katrina year t between 2005 and 2013, the change in cumulative mortality probability ΔM_t is given by

$$\Delta M_t = \prod_{\tau=2005}^{t} (1 - m_\tau + \beta_\tau) - \prod_{\tau=2005}^{t} (1 - m_\tau), \tag{3}$$

where β_τ are the annual mortality effects of Hurricane Katrina and m_τ is the empirical fraction of the New Orleans cohort who die in year τ.[13] We estimate ΔM_t and its standard error using the estimates $\hat{\beta}_t$ from equation (2).

Concise Difference-in-Differences Event study estimates from equation (2) nonparametrically identify treatment effects over time and also help to assess the plausibility of the parallel trends assumption. If there are no pre-trends, and if the treatment effect is constant over a period of time, then 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

[12]Because cumulative mortality converges to one for each cohort, any differences in baseline annual mortality rates between the treatment and control cohorts imply that the cumulative mortality rates would not have moved in parallel had treatment not occurred. By contrast, annual mortality risk need not converge or diverge over time. For this reason, we estimate annual mortality effects and use the results to infer changes in cumulative mortality.

[13]To derive equation (3), note that $\Delta M_t = (1 - S_t^O) - (1 - S_t^C) = S_t^C - S_t^O$, where $S^O = \prod_{\tau=2005}^{t}(1 - m_\tau)$ is the cohort's observed survival rate and $S_t^C = \prod_{\tau=2005}^{t}(1 - m_\tau + \beta_\tau)$ is the counterfactual survival rate.

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 age, race, income, flooding from Hurricane Katrina in one's nine-digit ZIP code of residence, and the presence of various chronic conditions. To do so, we augment equation (4) above to include interactions between the treatment indicators and an indicator for the characteristic of interest:

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

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, which occurs for all characteristics we examine but one. 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 Migration and Place Effects

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 area they moved to. To do so, we restrict our sample to individuals in the 2004 New Orleans cohort who survived through 2005 and moved to another county 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 movers sample individuals who moved to a county in the same CZ as New Orleans.

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 county in which mover i resided in 2006, which we denote by $MDR_{2006C(i)}$.[14] To avoid a mechanical relationship between migrant mortality outcomes and our measure of destination mortality, we calculate $MDR_{2006C(i)}$ as the annual mortality rate of the county's 2004 Medicare cohort (i.e., of Medicare beneficiaries who lived in that county as of March 2005) averaged over 2006–2013. We then estimate

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

Because only New Orleans movers are included in this empirical exercise, it is not necessary to have New Orleans indicators in equation (6). All remaining control variables are defined as before. Note that year fixed effects will control for any mortality effects that are common to all migrants, such as the effect of moving in with relatives. The coefficient γ describes the causal effect of place, as captured by local mortality, on migrant mortality under the

[14] In principle, we could let the local mortality rate MDR_{2006C} 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 these 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.

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 the sensitivity of estimates of γ to the inclusion of rich controls, including baseline demographics, medical spending, and chronic conditions.

We also estimate other local correlates of migrant mortality by replacing $MDR_{2006C(i)}$ in equation (6) with other attributes of each mover's 2006 destination county, such as health behaviors, income, and medical spending and quality. To avoid capturing the outcomes of Hurricane Katrina movers in the destination characteristics, we measure Medicare-derived characteristics (mortality and medical spending) using the 2004 cohort from each region and, when possible, all other characteristics before 2005 (see Appendix Section A.1.4).[15] As with local mortality, the estimated relationship captures the causal effect of living in a low- or high-attribute place only if unobserved mortality risk is uncorrelated with that attribute. Additionally, for an estimate to reflect the causal effect of *that attribute* on mortality, there must be no other unobserved local characteristic that is both correlated with the attribute of interest and affects movers' mortality.

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).[16] The gray dashed line 14 weeks after the hurricane indicates the week of FEMA's "look-and-leave"/"look-and-stay"

[15] We measure environmental characteristics over the time period 2006–2013 because these are unlikely to be affected by Hurricane Katrina movers.

[16] Numerical values for a subset of the estimates plotted in Figure 2 are reported in Appendix Table A.3.

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 6.01 deaths per thousand (0.61 percentage points), which can fully account for the excess 2005 mortality we identify later in our annual analysis. Relative to the average of 1.39 deaths per thousand beneficiaries in the sample we use for this analysis, the mortality rate more than quadrupled during the week of Katrina. We also see statistically significant increases in mortality for as long as nine weeks after landfall. While the estimates are about an order of magnitude smaller (0.011–0.076 percentage points), they nonetheless represent large relative mortality increases (8–55 percent). In the subsequent 55 weeks, only one of the positive point estimates is significant at the 10 percent level (week 11), 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 Relocation

Figure 3a shows estimated effects of Hurricane Katrina on annual mortality (equation (2), solid black lines) as well as on cumulative mortality (equation (3), dashed lines) for the 2004 Medicare cohort.[17] The hurricane increased mortality in 2005 by 0.56 percentage points (over 10 percent of the mean), which is particularly large given that these additional deaths occurred in the last four months of the year. Remarkably, this initial mortality increase quickly reversed and became a mortality *reduction*: in 2006, the death rate fell below pre-

[17]Event study results are similar if we include fixed effects for all combinations of baseline age (in one-year bins), race, and sex (see Appendix Figure A.7). Numerical values of the point estimates and standard errors shown in Figure 3 can be found in Appendix Table A.4.

Katrina levels and remained depressed by at least 0.25 percentage points each year through 2013 (all estimates after 2006 are statistically significant at the 5 percent level). The decrease in the mortality rate is approximately constant over time in absolute terms, corresponding to a declining relative effect as a cohort's mortality rate grows (see Figure 1).

After an initial increase in 2005, changes in cumulative mortality (dashed lines) fell and became negative by 2007, indicating that mortality displacement can explain, at most, two years of post-Katrina mortality reductions. The change in cumulative mortality became increasingly negative throughout the post-Katrina period: the cumulative share of the 2004 New Orleans cohort dying by 2013, the end of the sample period, was 2.07 percentage points *lower* than if their mortality rates had trended in parallel with the control group. Relative to the 64 percent survival rate of the 2004 cohort over this time period, a decrease in cumulative mortality of 2.07 percentage points represents a survival improvement of 3.2 percent.

Using a value of $100,000 per life-year (Cutler, 2004) and a discount rate of 3 percent (Siegel, 1992), we calculate that the net present value of the changes in cumulative mortality brought about by Hurricane Katrina over the period 2005–2013 is $6,743 per capita ($8,230 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 62,094 elderly and long-term disabled victims from the 2004 New Orleans cohort who were alive as of January 1, 2005, the implied aggregate value of the mortality changes over the period 2005–2013 is about $419 million ($511 million without discounting). Because Hurricane Katrina had other negative consequences, improved mortality outcomes do not imply that the storm increased victims' aggregate welfare.

Finally, Figure 3b shows the effect of Hurricane Katrina on Medicare beneficiaries' long-run relocation. In 2005, Hurricane Katrina displaced about 48 percentage points more New Orleans victims 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 remained about 25 percentage points less likely to be living in their baseline CZ than were individuals from the control city cohorts. Thus, a large share of New Orleans elderly and long-term disabled left the city after Hurricane Katrina and never returned. These estimates of cohort-level migration responses to Hurricane Katrina over time complement recent evidence on the effect of natural disasters on county-level net migration (Strobl, 2011; Deryugina, 2017; Boustan et al., 2017).

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.[18] For both cohorts, mortality trends are 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.[19]

Appendix Figure A.7 shows the sensitivity of our event study estimates to adding detailed demographic controls. Controlling for every possible combination of one-year age bins, sex, and race has little impact on either the pre- or post-Katrina estimates for any of the three cohorts. Allowing the year fixed effects to vary by every possible combination of age, race, and sex has a larger effect on the estimates, but the vast majority of them fall within the 95 percent confidence intervals of the original estimates.

We also estimate a proportional hazard model version of equation (2), which allows demographic controls to shift mortality rates proportionally rather than additively. Appendix Figure A.8 reports the estimated hazard ratios for the interactions between year and New Orleans indicators, analogous to the linear model estimates reported in Figures 3–4. Esti-

[18] Appendix Table A.5 reports numerical values of the mortality event study point estimates and standard errors shown in Figure 4. Appendix Figure A.6 reports relocation event study estimates for the 1999 cohort.

[19] 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 disaster's short-run negative effects than younger Medicare beneficiaries.

mates that control for all combinations of one-year age bins, race, and sex (right panels) are similar to those that include no demographic controls (left panels), and both cases reflect effects similar to those estimated by the linearly additive model.

To assess the sensitivity of the event study estimates to our choice of control group, we replicate the mortality results displayed in Figures 3–4 using the entire United States except New Orleans—not just the ten control cities—to construct control cohorts (see Appendix Figure A.9). 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 the ten control cities, we see no differential mortality pre-trends with either the 1999 or 1992 cohorts. We obtain similar, but slightly larger, estimates of the post-Katrina reductions in the mortality rate, indicating that the cumulative mortality of the New Orleans cohorts decreased by 2.3–3.3 percentage points by 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 ten 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 (MacKinnon and Webb, 2017). As detailed 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). The synthetic control treatment effects for New Orleans are almost always outliers at the 5 percent level or better, leading to similar inference as in our baseline regression analysis where we cluster 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"), we also show results that additionally control for all combinations of one-year age bins, sex, and race (labeled "B") and where we further allow the year fixed effects to vary by each one-year age bin, sex, and 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 are more precisely estimated. The estimated initial (2005) mortality increase for the 2004 cohort is 0.55–0.56 percentage points. In 2006–2013, the 2004 New Orleans cohort experienced a statistically significant decline in its mortality rate of 0.36–0.49 percentage points. The 1999 New Orleans cohort likewise experienced a short-run mortality rate increase, with an estimated magnitude ranging from 0.91 to 1.00 percentage points across control specifications. In the longer run, annual mortality declined by 0.23–0.49 percentage points.

5.2.4 Heterogeneous Treatment Effects

The cohort-level mortality effects presented above could mask heterogeneity in Hurricane Katrina's effects across subpopulations of victims. To investigate potential heterogeneity, we estimate the annual event study specification in equation (2) separately by gender (male or female), race (black or not black), and age (65 and older or 64 and under at baseline). The results for the 1992 and 2004 cohorts are shown in Appendix Figure A.10. We see no evidence of differential pre-Katrina mortality trends for any group, further reinforcing the validity of our counterfactual. The initial effect of Hurricane Katrina on mortality is larger for men, non-black individuals, and those aged 65 and older. Only those who are 64 or younger at baseline do not experience a statistically significant mortality increase in 2005, suggesting that, among these characteristics, age is the most important predictor of short-run vulnerability to this disaster. Despite these initial differences, the mortality declines we observe in 2006–2013 are similar for each group, supporting our decision to combine them in our main analyses.

We also use equation (5) to more concisely estimate heterogeneous treatment effects along these and other dimensions, including the extent of flooding in one's neighborhood and pre-existing chronic conditions. Appendix Table A.6 presents the complete set of results. We find that short-run mortality effects are larger, but not statistically significantly so, among individuals initially living in parts of New Orleans that experienced two or more feet of flooding. However, long-run mortality effects are similar for these two groups. 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* than those for higher-income individuals. Finally, there is suggestive evidence that black individuals experience lower initial mortality increases and larger subsequent mortality decreases, although the latter difference is not statistically significant.

5.3 Migration and Place Effects

Thus far, we have shown that Hurricane Katrina led to significant declines in long-run mortality among the elderly and long-term 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 of Hurricane Katrina have been demonstrated in other contexts including higher earnings among the working-age population (Deryugina, Kawano and Levitt, 2018) and improved test scores among displaced students (Sacerdote, 2012). In this section, we explore the hypothesis that Hurricane Katrina may have increased long-run survival rates by causing elderly and long-term disabled individuals to move to areas more conducive to good health.[20]

The elderly and long-term disabled mortality rate in New Orleans was among the highest in the country prior to Hurricane Katrina, and 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 effect of place on mortality outcomes, we focus on individuals who were displaced by the hurricane and relate their mortality outcomes to the local mortality rate of the region they moved to, as outlined in Section 4.3 and captured by equation (6).

5.3.1 Determinants of Migration

The relationship in equation (6) reflects the causal effect of place, as captured by mortality rates, on individual mortality under the assumption that baseline mortality risk among those who move is uncorrelated with mortality rates in the destination region. This identification assumption would be violated if migrants with lower latent mortality risk systematically sort to destinations with different mortality rates.

As a direct test of differential sorting, we estimate how migrants' predicted mortality risk varies with the local mortality of the county to which they move. To predict mortality risk, we model the relationship between mortality and baseline (2004) characteristics $X_{i,2004}$ as

$$Died_{it} = X_{i,2004}\beta + \epsilon_{it}.$$

We estimate this model using the 2004 control county cohorts over the period 2006–2013. The

[20] Hurricane Katrina may have also generated health benefits through other channels, including quality improvements in New Orleans itself (e.g., Marsa, 2015). In Appendix Section A.3, we demonstrate that there is no evidence of improvements over time in the health of those who stayed in New Orleans.

fitted model is then used to generate out-of-sample mortality predictions, $PredictedMortality_i$, for individuals in the 2004 New Orleans cohort, providing a time-invariant index of an individual's ex ante mortality risk over the period 2006–2013. Our preferred set of predictors $X_{i,2004}$, described below, yields mortality predictions that are strongly correlated with realized mortality among the New Orleans movers (see Appendix Figure A.13).

To test whether migrants' predicted mortality risk is correlated with the local mortality of the county they move to, we estimate the following regression:

$$MDR_{2006C(i)} = \beta PredictedMortality_i + [base\ ZIP5\ FE] + \epsilon_i, \tag{7}$$

where $MDR_{2006C(i)}$ is the mortality rate in migrant i's destination county, as in equation (6). We estimate equation (7) using one observation per mover from the 2004 New Orleans cohort.

Table 3 reports the results of estimating equation (7) using increasingly rich sets of baseline characteristics $X_{i,2004}$ to generate migrants' predicted mortality risk. When mortality risk is predicted using demographics alone (all possible one-year age, race, and sex combinations), the relationship between predicted mortality and destination mortality is a small and statistically insignificant (column (1)). However, when augmenting the mortality predictors to also include indicators for ESRD, eight chronic condition groups, and ventiles of health care spending at baseline (our preferred set of predictors), the coefficient on predicted mortality remains small but becomes statistically significant at the 5 percent level.

Appendix Table A.8, which reports the relationship between destination mortality and $X_{i,2004}$ directly (i.e., without combining the characteristics into a mortality risk measure), suggests that this relationship is driven by a small number of individuals with Alzheimer's/dementia who have high predicted mortality risk and who move to higher-mortality areas. When we exclude these individuals from the sample (column (3)), the estimated relationship between predicted mortality and destination mortality ceases to be statistically significant and the magnitude of the coefficient falls nearly to zero. For this sample, the coefficient remains small and statistically insignificant even when enriching mortality predictors to include all two-way interactions between chronic condition groups (column (4)) or when replacing grouped chronic conditions and spending ventiles with all available 27 chronic condition indicators and centiles of baseline spending (column (5)).[21]

The limited degree of sorting on observable mortality risk supports the assumption that the underlying mortality risk of New Orleans migrants was unrelated to destination mortality, especially among those without Alzheimer's/dementia. As a second test of this assumption, we report in the next section how estimates of the relationship between mover mortality and destination region mortality rates change when using increasingly comprehensive controls and when restricting the movers sample to those without Alzheimer's/dementia.

5.3.2 Movers' Mortality and Local Mortality

Table 4 reports how movers' 2006–2013 mortality varies with the mortality rate in their destination county (equation (6)). Column (1) reports effects for the full movers sample when controlling only for baseline ZIP code and year fixed effects. The results show that each percentage-point reduction in the destination mortality rate corresponds to a 0.85 percentage-point reduction in the mortality rate of New Orleans migrants who had relocated to that region by 2006.

Columns (2)–(6) of Table 4 show the results of estimating equation (6) using increasingly comprehensive controls for baseline demographics and chronic conditions. Whenever we control for chronic conditions, which restricts us to a subset of fee-for-service beneficiaries, we also include fixed effects for centiles of beneficiaries' 2004 Medicare spending. The estimated coefficient on destination mortality changes little across these specifications, ranging from 0.83 to 1.01. Importantly, excluding beneficiaries with Alzheimer's/dementia (columns (5) and (6)) yields similar results to estimates that include those individuals (column (4)). The stability of this estimate across the various sets of controls further suggests that significant

[21] By contrast, Appendix Table A.7 shows that there is a consistent and strong negative relationship between predicted mortality and the probability of leaving New Orleans in 2005–2006.

migrant sorting on latent mortality risk is unlikely in our context.

Finally, as a placebo test to complement Table 3, column (7) reports the estimated "effect" of destination mortality on predicted mortality, yielding a small and statistically insignificant coefficient. Because many beneficiaries who left New Orleans after Hurricane Katrina eventually returned (Figure 3b), the relationship between local mortality and movers' subsequent mortality should be interpreted as an intent-to-treat estimate.[22] Additionally, even if local mortality is a good proxy for the effect of place, the magnitude of the relationship between local mortality and migrant mortality could reflect other factors, such as differences between the demographics of the movers and those of local residents.

Figure 5 shows a graphical representation of the results in columns (5) and (7) of Table 4. Specifically, we residualize destination county mortality by the fixed effects included in each regression and bin observations by centile of the resulting residuals, yielding 100 groups. For each centile, we plot movers' mean residualized mortality (black circles) and mean residualized predicted mortality (green squares), with the sample means added for interpretability. The resulting relationship between local mortality and movers' subsequent mortality reflects a general upward trend, demonstrating that the findings in Table 4 are not driven by a few outliers. The relationship between destination mortality and movers' ex ante predicted mortality appears to be flat, supporting the absence of mover sorting on this dimension.

In Appendix Table A.10, 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 changes in migrant mortality rates are not shaped solely by slow-moving channels such as lifestyle changes. More generally, the speed with which individuals' mortality rates reflect the local rate makes it unlikely that this relationship is primarily due to them becoming more or less likely to develop chronic conditions. Rather, faster-moving channels, such as the quality of the local

[22] A potential difficulty with interpreting the intent-to-treat effect is that the probability of returning to New Orleans could differ across high- and low-mortality destinations. However, Appendix Table A.9 indicates that this was not the case.

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.11, we consider how movers' mortality from specific causes varies with destination mortality. We divide causes of death into four comprehensive categories: cardiovascular, cancer, other internal causes, and external causes. For reference, column (1) shows the all causes estimate for the years 2006–2008, as cause of death information is not available for later years. We find that cardiovascular and other internal causes of death are each statistically significantly associated with the destination mortality rate, but we estimate small and statistically insignificant relationships with respect to cancer and external causes.

Appendix Table A.12 shows that a mover's mortality is more correlated with the local mortality of beneficiaries of his/her race (black or not black) and with the local mortality of his/her age group (65 or older versus 64 or younger). Our gender-specific estimates are inconclusive: coefficients on both own-gender and other-gender local mortality rates are sizable and positive, but neither is statistically significant when we estimate them jointly, possibly because of a high correlation between the two mortality rates.[23]

Appendix Table A.13 performs a similar exercise. with destination mortality defined at the movers' 2006 ZIP code, county, or CZ level. Destination ZIP code mortality is strongly related to movers' mortality (column (2)), but the magnitude is about half of our preferred county-level mortality estimate (column (1)). Both ZIP- and county-level mortality are individually significant when included jointly (column (3)). CZ mortality is also significantly correlated with movers' subsequent mortality, with a coefficient of 1.03 (column (3)). However, we lose power when we estimate a specification that includes both county and CZ mortality (column (5)), likely due to the high correlation between county mortality and CZ mortality (0.80 in our sample).[24]

Appendix Table A.14 estimates the relationship between the destination mortality rate

[23] The correlation between the local male and female mortality rates is 0.75 in our sample. By contrast, the correlations between local black and non-black mortality rates and between local mortality rates for those under 65 and those over 65 are much smaller (0.37 and 0.06, respectively).

[24] The correlation between ZIP- and county-level mortality is smaller (0.41 in our sample).

and the probability that a mover has died by the end of 2013. Each percentage-point increase in the destination mortality rate increases this probability by 3.3–4.0 percentage points or 9–11 percent of the average cumulative mortality rate in the movers' sample. This is comparable to our preferred estimate that considers annual mortality (column (3) of Table 4), which implies that a 1 percentage-point increase in the destination mortality rate increases a movers annual mortality rate by about 15.5 percent of its mean.

In Appendix Table A.15, we estimate the relationship between movers' own mortality and the mortality in their destination county using individuals' locations from the 2006 Medicare eligibility files (i.e., locations as of March 2007). This should exclude any short-term moves and include moves that had not been reported by the beneficiary until later in 2006. The coefficients on local mortality are slightly lower than, but similar to, the baseline estimates. In Appendix Table A.16, we further demonstrate that our results are robust to controlling for each of the 27 chronic condition indicators separately, controlling for distance between New Orleans and the destination county, or excluding Houston and Baton Rouge (the two most common destinations for New Orleans movers). Taken together, these results demonstrate the importance of place in shaping health outcomes, even later in life.

5.3.3 Movers' Mortality and Other Local Characteristics

Next, we examine how movers' mortality rates vary with other attributes of the destination county, including health behaviors, health care supply and quality, the environment, income and income mobility, crime, urbanicity, and social capital. The full set of 21 attributes is summarized in Appendix Table A.17, where we display the median, 10th percentile, and 90th percentile of each destination characteristic, as measured in the 2006 cross-section of New Orleans movers.

To estimate the correlation between movers' mortality and each of these characteristics, we simply replace the local mortality rate in equation (6) with the local characteristic of interest. The correlation reflects the causal mortality effect of place, as captured by the local characteristic, under the assumption that migrants' baseline mortality risk is uncorrelated with the destination characteristic. Even if this assumption holds, we emphasize that the estimates reflect the causal effect of the given characteristic itself only if the characteristic is uncorrelated with any other local attribute that also affects movers' mortality. Because each region is a bundle of many, often correlated, characteristics, these results should be viewed as suggestive of what actually determines place effects.

Figure 6 reports the results for the no-Alzheimer's/dementia sample, controlling for ZIP code and year-by-age-by-race-by-sex fixed effects as well as for eight chronic condition indicators and centiles of baseline spending. Black markers indicate effects when the outcome is $Died_{it}$, an indicator for realized mortality. Green markers indicate effects when the outcome is $PredictedMortality_i$, which evaluates the extent of differential sorting to regions with high or low values of the characteristic by ex ante predicted mortality risk. In the figure, we report standardized mortality effects and 95 percent confidence intervals, constructed by multiplying coefficients and confidence intervals by the interdecile range of each characteristic.[25] Thus, each estimate can be interpreted as the change in actual or predicted mortality when moving from a place in the 10th to a place in the 90th percentile of a characteristic's distribution in the movers sample.

Across all local health characteristics we consider (Figure 6a), the estimated standardized mortality effect is largest for the local mortality rate: moving to a region in the 10th versus the 90th percentile of local mortality corresponds to a 0.76 percentage-point decrease in realized mortality. The estimate is similar when we adjust the local mortality rate to account for the demographics of local residents, which we construct by demeaning raw mortality by interactions of one-year age, race, and sex. Migrants' mortality is also higher in places with higher rates of smoking, higher rates of obesity, or lower rates of exercise. We find small and statistically insignificant relationships between movers' mortality and the number of hospital

[25] Appendix Table A.17 reports the interdecile range of each local characteristic among the movers sample. Appendix Tables A.18 and A.19 report the numerical values of coefficients and standard errors graphed in Figure 6, along with the correlation between each local characteristic and the local mortality rate.

beds per capita, the frequency of extremely hot days, the number of MDs per capita, an index of hospital quality, or average medical spending. Surprisingly, moving to areas with more air pollution, as measured by PM 2.5, is associated with a lower subsequent mortality rate. However, in contrast with the other local health characteristics, this correlation can largely be explained by sorting of ex ante healthier individuals into more polluted areas.

Our analysis of movers' mortality and local health care spending sheds new light on a long-standing question of whether higher-spending regions generate better health outcomes than lower-spending regions. Higher spending regions often have no better or even worse health outcomes than lower-spending regions, suggesting that the returns to additional medical spending may be low (e.g., Fisher, Bynum and Skinner, 2009; Cutler, 2010; Skinner and Fisher, 2010). However, higher spending could be partly due to worse population health, which could result in a net zero or even positive correlation between local spending and local mortality even if the returns to living in a higher spending region are positive. Our focus on movers allows us to sidestep this concern, and our results indicate that the return to living in a high-spending region is low.

Low returns to *living* in a high-spending area need not imply low returns to additional medical spending: it depends on the extent to which differences in local health care spending reflect differences in spending among the migrants themselves. We therefore estimate the relationship between the average spending in destination counties and movers' own subsequent spending. For this analysis, we restrict the sample to New Orleans residents who were enrolled in fee-for-service Medicare for all 12 months of 2004 and only consider post-2006 spending in years in which they were enrolled in fee-for-service for all 12 months. As reported in Appendix Table A.20, we find that a mover's spending increases by \$0.61–\$0.93 for each \$1 increase in average local spending, estimates that are broadly consistent with those of Finkelstein, Gentzkow and Williams (2016).

Our estimates of the spending and mortality effects of moving to a higher-spending region can be combined in a back-of-the-envelope fashion to derive an implied cost of saving one life-year. Because we do not find a statistically significant relationship between destination spending and movers' mortality, we focus on deriving the lowest cost that is not ruled out by our estimates. We start with our smallest estimate of the effect of an interdecile range increase in destination medical spending (about a \$4,000 increase) on a mover's own spending ($0.61 \times \$4,000$). We divide the result by the lower bound of the 95 percent confidence interval of the estimated annual mortality effect ($-0.21 - 1.96 \times 0.19$ percentage points). This calculation implies that we can rule out costs of less than \$420,000 to save one life-year.

Figure 6b reports how migrant mortality varies with local economic conditions. Migrants to higher-income areas, areas with higher housing prices, more urban areas, or areas with higher local government spending per capita subsequently experience lower mortality. Perhaps counterintuitively, movers to areas with higher social capital experience significantly higher mortality, although this could partly reflect ex ante sorting on mortality risk. We do not find statistically significant differences in mortality with respect to local crime, poverty, upward income mobility, or income segregation. While some of these economic characteristics correlate with migrants' mortality, all of the estimated standardized mortality effects are smaller than that of the local mortality rate.

5.3.4 Discussion

We perform a back-of-the-envelope evaluation of the extent to which migration can account for the average mortality decline among the New Orleans cohort in 2006–2013 (Table 2). The 2005–2006 local mortality change experienced by surviving Hurricane Katrina victims averages -0.31 percentage points (-0.39 of the interquartile range), including individuals who remained in New Orleans, for whom the difference is zero.[26] Combining our most carefully

[26]To measure the changes in local mortality following Hurricane Katrina, we calculate the difference in mortality between each individual's 2006 county and the New Orleans county using the 2004 mortality rate of each area's 2004 cohort. Calculating mortality rate differences over the period 2006–2013 instead of 2004 would adhere most closely to our movers' regression framework, but the 2006–2013 mortality rate for New Orleans would be confounded by the effects of the hurricane. If counterfactual cohort mortality rates trend in parallel across regions, then differences in 2004 mortality rates provide an unbiased, although perhaps less precise, estimate of longer-run differences.

controlled estimates of the 2006–2013 mortality reduction (-0.36, column (3) of Table 2) with the normalized relationship between local mortality rates and Katrina movers' own subsequent mortality (0.67, column (2) of Table A.18), we conclude that changes in victims' local mortality explain 73 percent $\left(\frac{0.67\times0.39}{0.36}\right)$ of the average long-run mortality decline caused by the hurricane.

There are a number of factors that may explain the remaining 27 percent of the 2006–2013 mortality decline. First, some of the long-run mortality decline following Hurricane Katrina may be driven by variation in other destination characteristics that is orthogonal to the local mortality rate. Appendix Table A.21 shows the results of simultaneously regressing movers' mortality on the local mortality rate and other local characteristics that, according to Appendix Tables A.18 and A.19, are individually large and significant predictors of movers' mortality (e.g., the local smoking rate). To preserve power, we add one other characteristic at a time, reporting coefficients that are normalized by the interdecile range of the given characteristic in the sample of movers (columns (1)–(6)). Nonetheless, jointly considering both the local mortality rate and another local characteristic causes the coefficient on the latter to cease to be statistically significant, likely due to the high correlation between local mortality and these characteristics (see column (3) of Appendix Tables A.18 and A.19).

Column (7) shows the average 2005–2006 change in each local characteristic for the sample of Hurricane Katrina victims who survived past 2005 as a share of the aforementioned interdecile range. The local smoking rate and the median housing values of Hurricane Katrina victims did not change meaningfully, but the local obesity rate and the percent of population living in an urban area decreased, while the local per-capita income and the percent of population exercising increased. For each specification, we also calculate the share of the -0.36 mortality increase that the two variables in question can explain. The answer ranges from 61 percent (when considering local mortality and smoking) to 85 percent (when considering local mortality and exercise rates).

Second, some of the mortality decline may reflect mortality displacement, although our cumulative mortality results show that harvesting cannot explain the persistent mortality reduction. Third, 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 and long-term disabled (Adams et al., 2011). Fourth, 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.

Fifth, elderly and long-term 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.[27] Finally, some of the long-run mortality decline following Hurricane Katrina may be driven by where people move but may be based on local factors that are uncorrelated with the attributes we considered in our analysis.

The finding that destination mortality explains much of the estimated mortality effect among Medicare victims of Hurricane Katrina suggests that the mortality declines in Figures 1 and 3a would have been even larger had more victims left New Orleans or had fewer returned. Because the hurricane disproportionately displaced vulnerable individuals, including those with higher medical spending or living in low-income neighborhoods (Appendix Table A.8), one possible explanation for this large effect is that place effects may be larger for this subpopulation than for healthier individuals. A related possibility is that place may have a larger impact for black individuals, who make up a large share of the New Orleans victims and were also disproportionately likely to move after the hurricane, than for other races. Finally, it may be that place effects are particularly large in our sample of destinations, which, for example, tend to be more urban than the average US location.

[27] Between October 2005 and October 2006, the Current Population Survey collected information on Hurricane Katrina evacuees and on 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 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 reductions in long-run mortality. 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 long-term disabled. Migrants' mortality is also lower in destinations with healthier behaviors and higher incomes but is unrelated to local medical spending and quality.

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 about \$6,700 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.

Our paper provides novel evidence 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 accumulate or depreciate more rapidly than the canonical model of Grossman (1972) implies, at least for the population in our study. Finally, we 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 migration and local conditions when projecting the long-run impacts of disasters.

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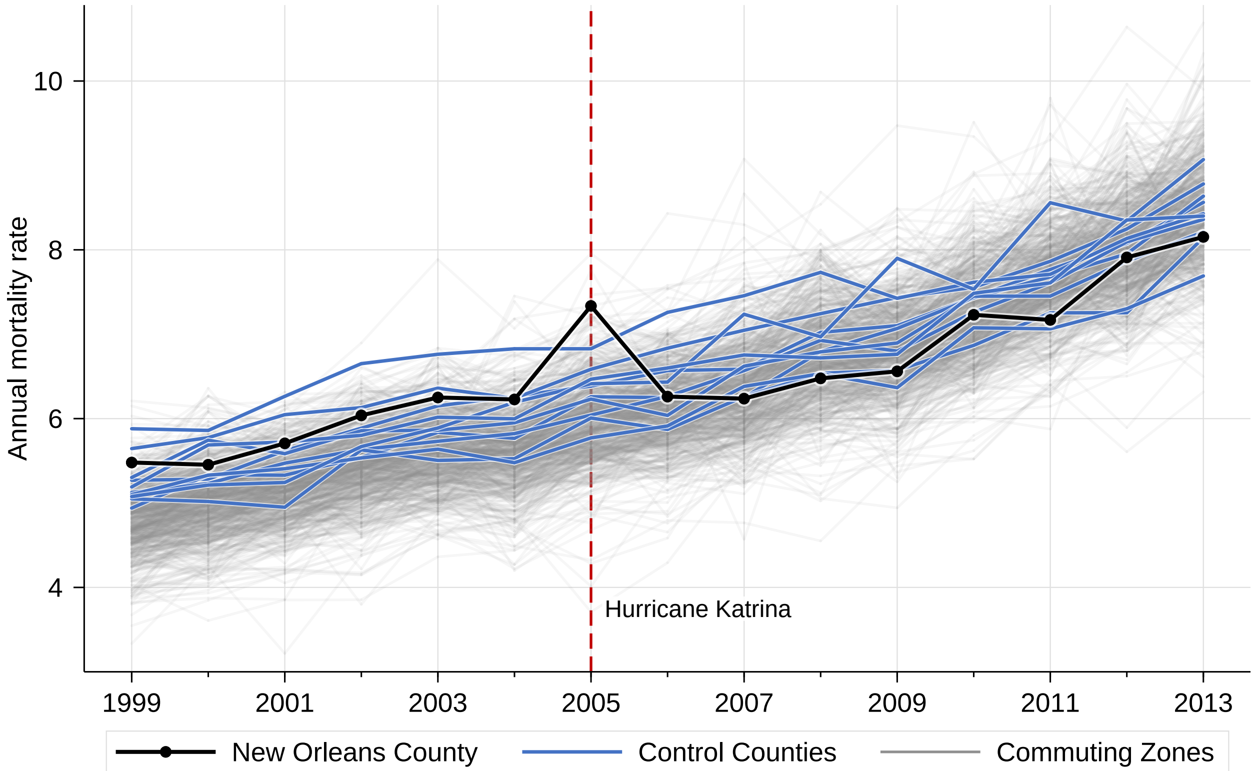
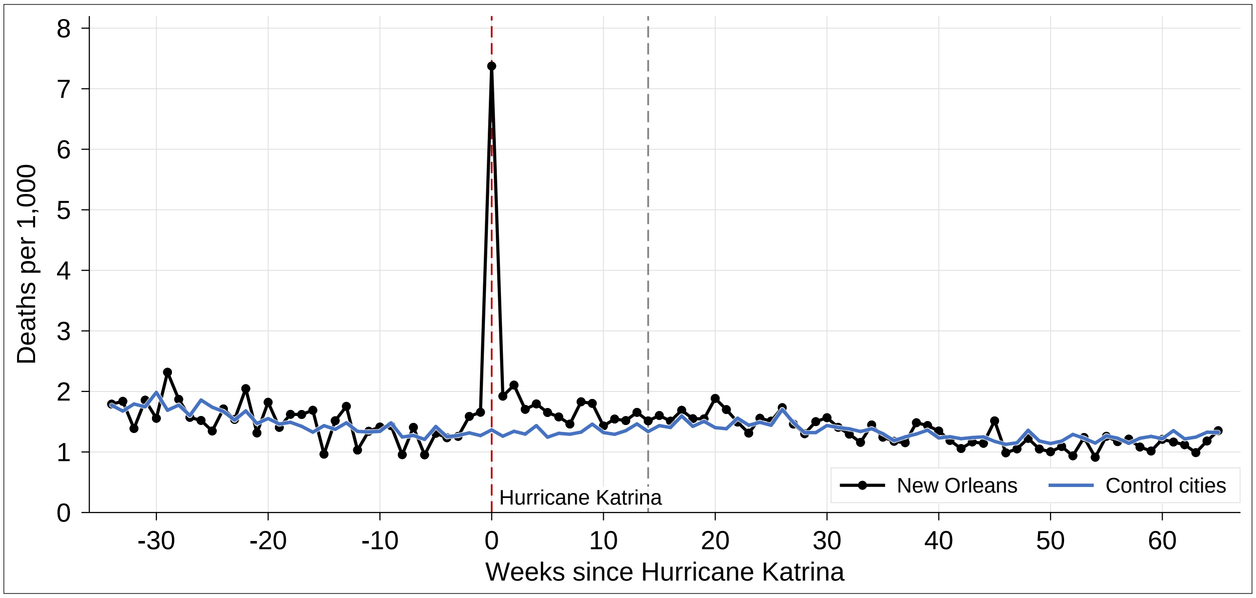
Figures

Figure 1: Cohort annual mortality rates for New Orleans versus other areas

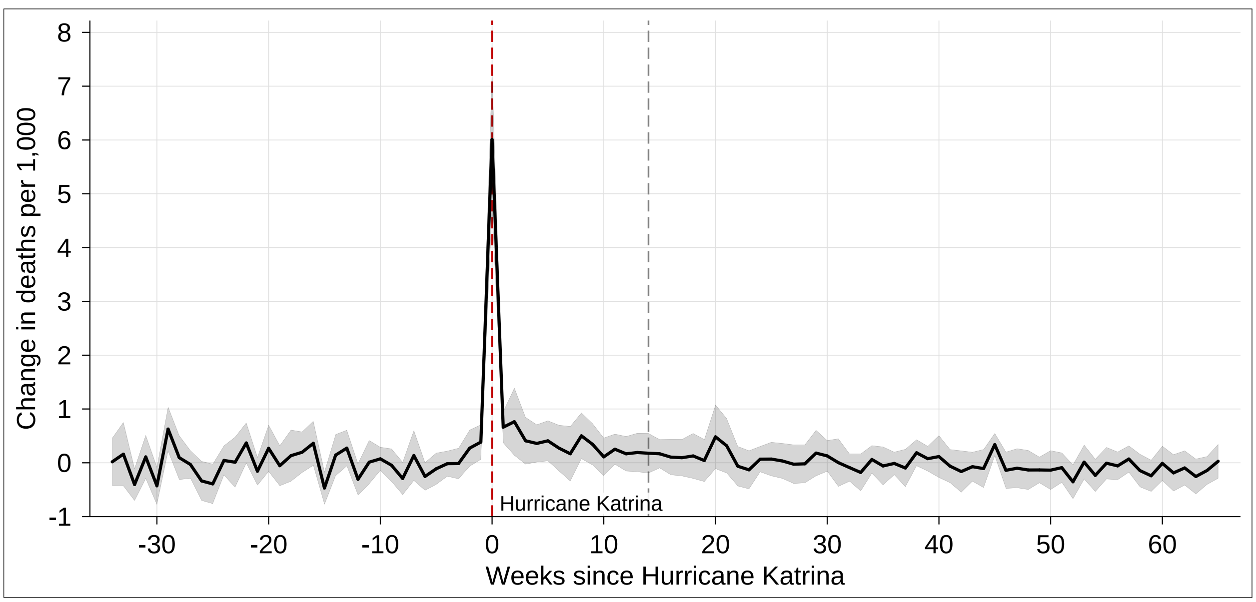


Notes: The figure shows raw annual death rates for the 1999 Medicare cohort, by initial region of residence. Mortality rates for the New Orleans county cohort are plotted in black, and mortality rates for the ten control county cohorts are plotted in blue. The light gray lines plot mortality rates for each US commuting zone cohort with at least 1,000 beneficiaries, except for the New Orleans commuting zone.

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



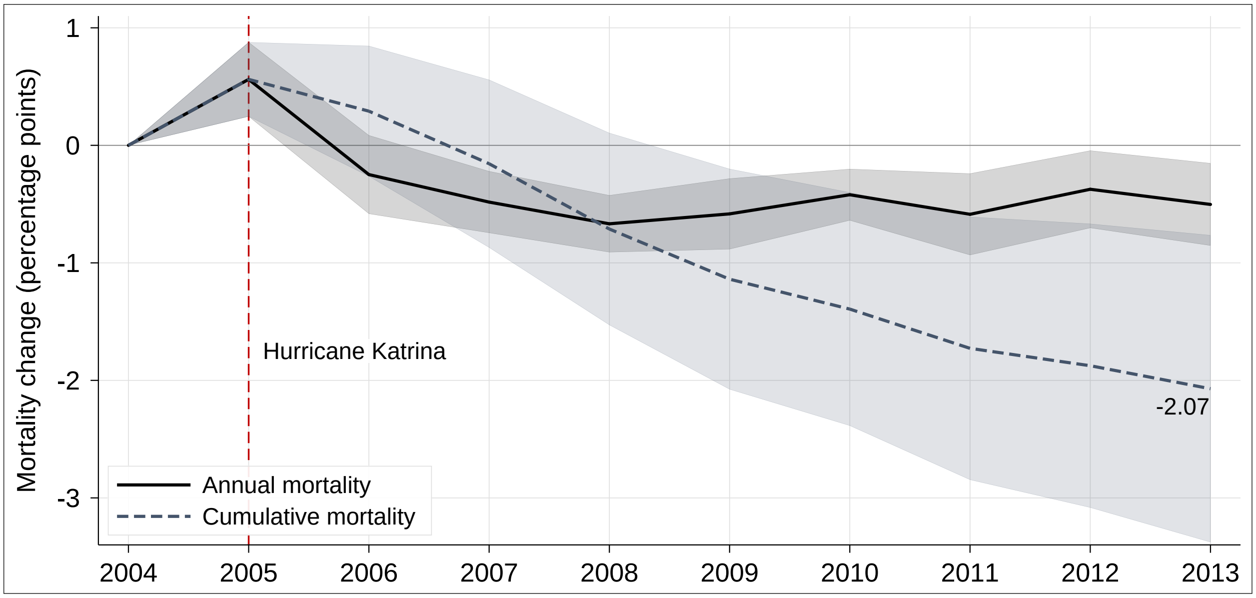
(a) Raw weekly mortality rates



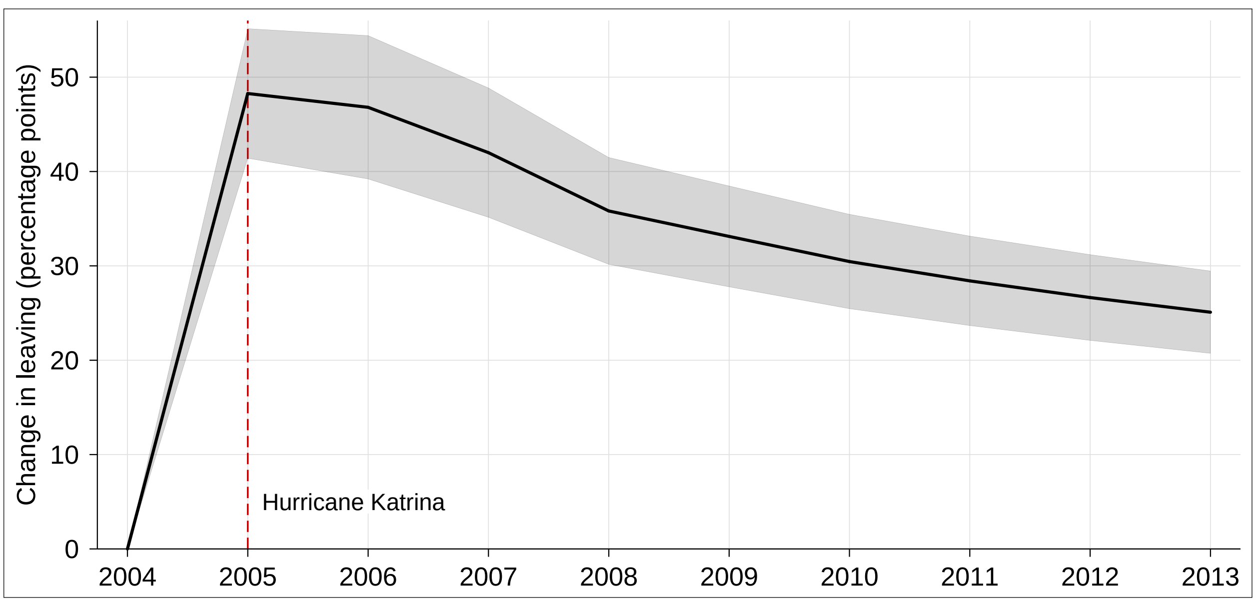
(b) Difference-in-differences event study

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 a mortality indicator equal to zero if a beneficiary was alive during the entire week and is equal to one 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 code. Coefficients and confidence intervals have been scaled by 1,000 to reflect changes in deaths per thousand beneficiaries. See Appendix Table A.3 for numerical values of a subset of the statistics plotted here.

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



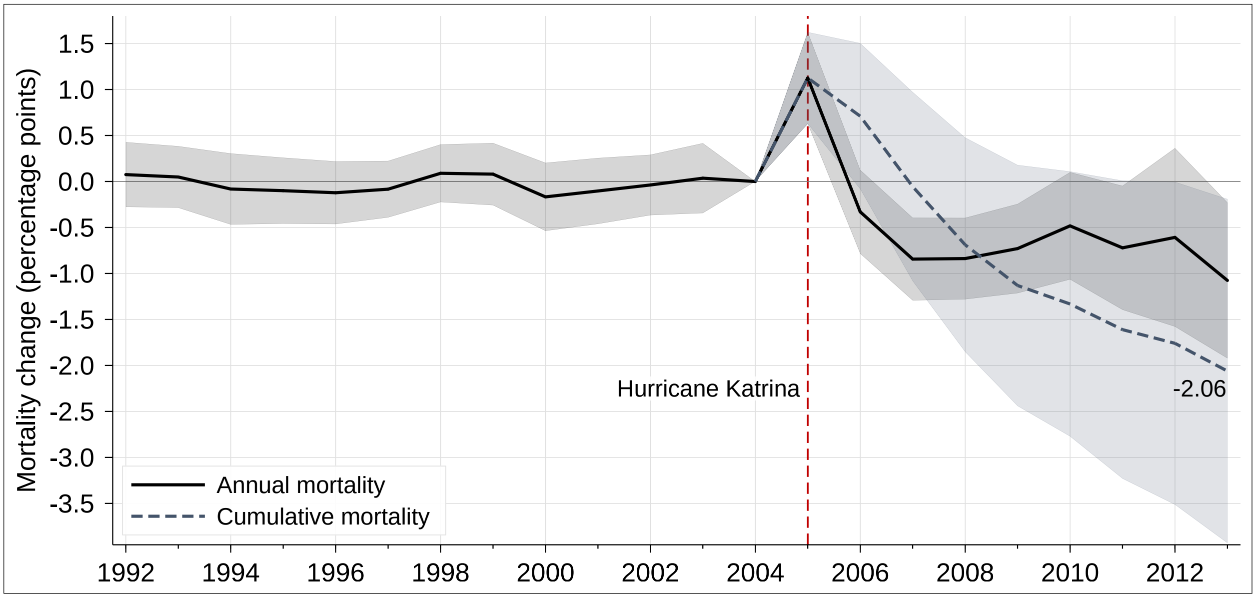
(a) Annual and cumulative mortality



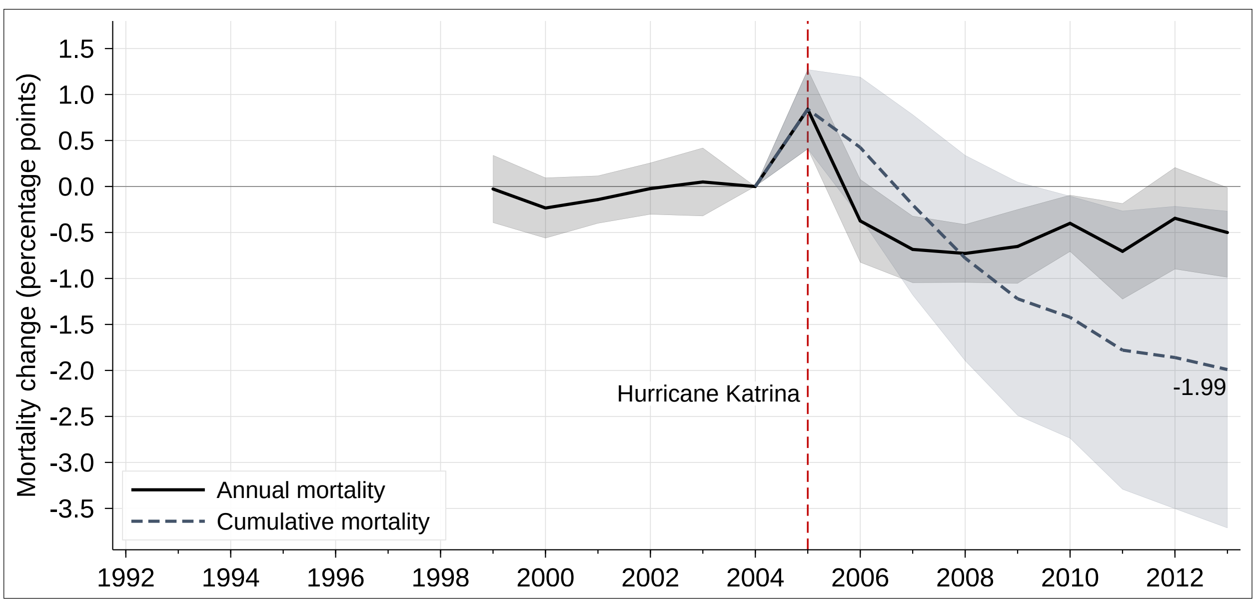
(b) Relocation

Notes: The figure shows estimates of changes in the probability that an individual dies (panel (a)) or is living outside of their 2004 CZ of residence (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 code. Section 4.2 defines the dependent variables. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points. Appendix Table A.4 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)



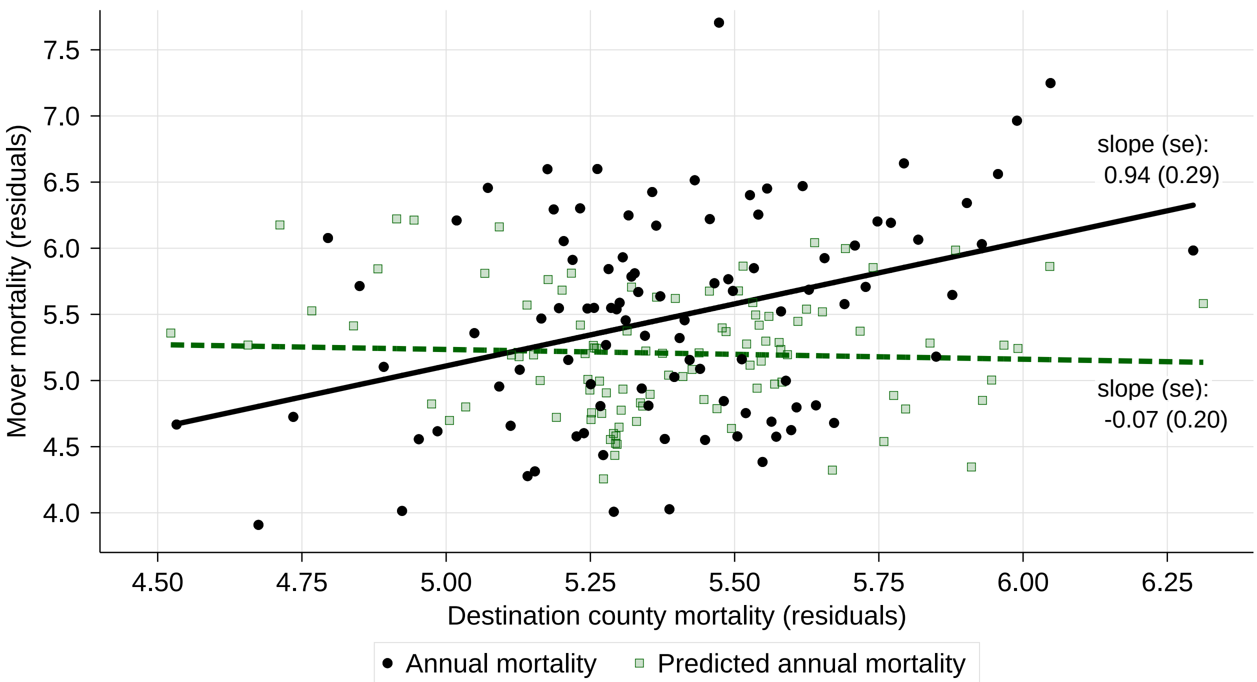
(a) 1992 Medicare cohort



(b) 1999 Medicare cohort

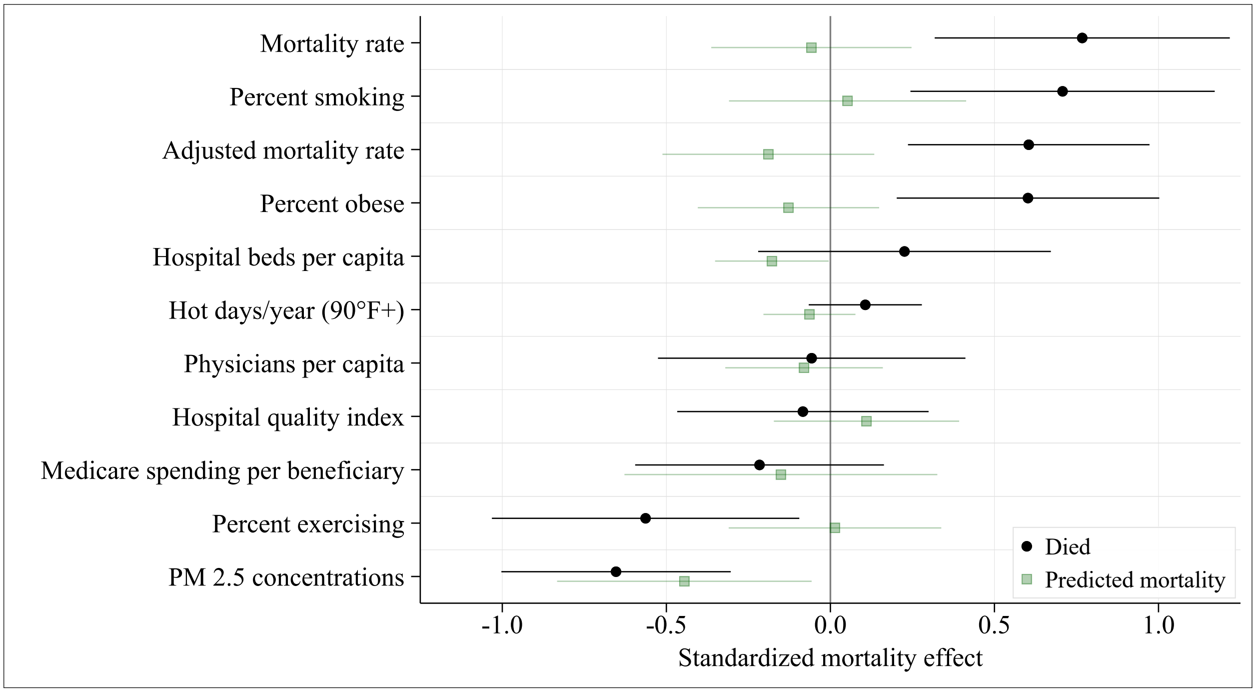
Notes: The figure shows estimates and 95 percent confidence intervals from equation (2) for the Medicare cohort indicated below each panel. The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire year and is equal to one if the beneficiary died in a given year. Standard errors are clustered by beneficiary 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 5: Mover mortality by destination mortality

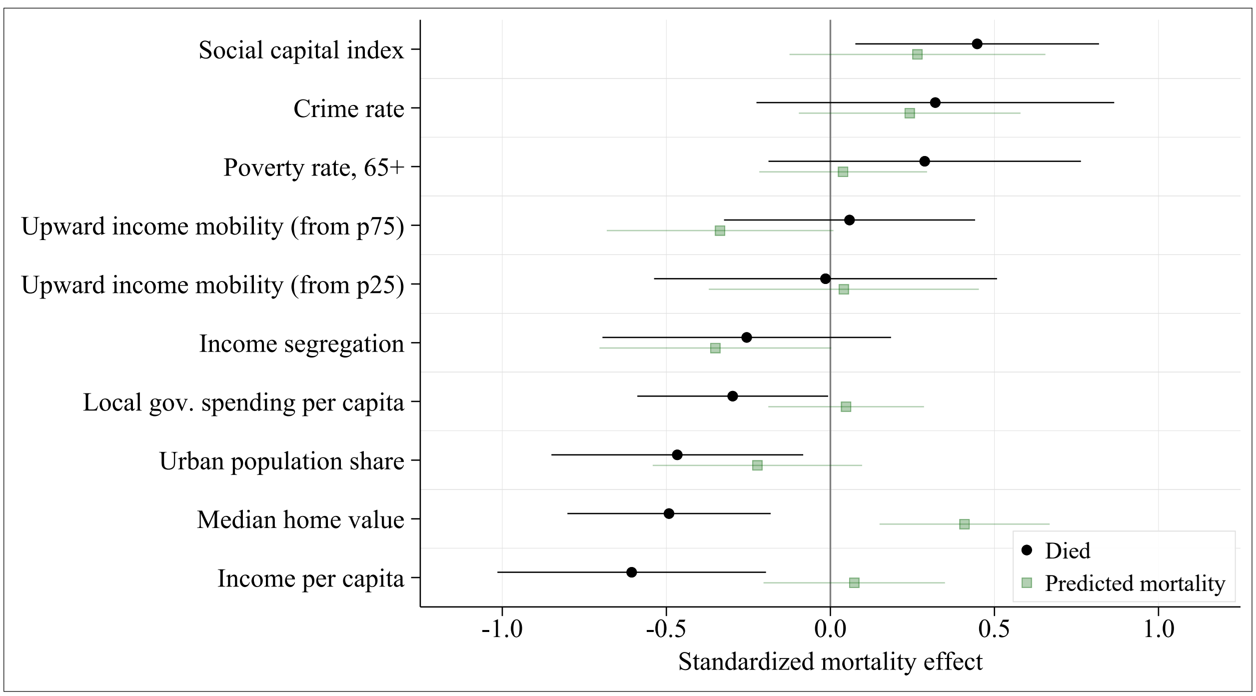


Notes: The figure shows a graphical representation of the results in columns (5) and (7) of Table 4, which describe, respectively, how movers' realized and ex ante predicted mortality rates relate to the local mortality of their destination county. For each outcome, both the outcome and destination county mortality are residualized by the fixed effects included in their respective regression, with the sample means added for interpretability. For each centile of residualized destination county mortality, the plot shows movers' mean residualized mortality (black circles) and mean residualized predicted mortality (green squares).

Figure 6: Mover mortality by destination characteristic



(a) Destination health characteristics



(b) Destination economic characteristics

Notes: The figure shows estimates and 95 percent confidence intervals of how realized and ex ante predicted mortality rates of movers relate to the local characteristics of the counties to which they moved. The standardized mortality effect reflects the effect of an interdecile range change in the local characteristic reported in the row, based on the 10th and 90th percentiles of the local characteristic exposure among movers. Numerical values of the point estimates and standard errors for the "died" and "predicted mortality" outcomes are reported in columns (1) and (4), respectively, of Appendix Tables A.18–A.19. Appendix Table A.17 reports summary statistics for each of the destination characteristics. See notes to Appendix Table A.18 for additional details about the sample and controls.

Tables

Table 1: Summary statistics (2004 Medicare cohort)

	(1) NOLA	(2) 10 Control cities	(3) NOLA movers
Percent male	43.0	41.9	41.4
Percent black	60.5	39.2	75.6
Age	70.1	71.0	67.2
Percent 65+	77.7	81.2	69.5
Percent 75+	40.7	42.2	32.9
Income for 65+ year-olds in 9-digit ZIP	28,230	30,962	25,005
Percent below median income	49.9	34.8	55.4
Percent who experienced 2+ feet of flooding during Katrina	56.5	N/A	67.1
Percent enrolled in fee-for-service Medicare in 2004	68.6	92.5	69.7
2004 Medicare spending (fee-for-service only)	10,460	9,640	8,857
Percent with end-stage renal disease	2.1	1.7	2.0
Percent with blood and kidney disease	46.2	52.7	43.6
Percent with heart disease and stroke	64.8	69.4	63.2
Percent with diabetes	27.4	28.5	28.9
Percent with musculoskeletal condition	28.6	30.4	27.0
Percent with respiratory disease	12.8	14.0	12.8
Percent with cancer	6.8	8.3	4.9
Percent with Alzheimer's/dementia	12.4	12.6	8.5
Percent with other chronic condition	39.9	41.1	38.9
Number of individuals	65,457	941,685	26,467

Notes: The table summarizes baseline (2004) characteristics of the 2004 Medicare cohort. The unit of observation is a beneficiary. Income and flood variables are available only for individuals with valid nine-digit ZIP codes (57,314 New Orleans individuals, 23,295 New Orleans movers, and 847,509 individuals from the ten control cities). Medicare spending is available only for individuals enrolled in fee-for-service Medicare (44,913 New Orleans individuals, 18,695 New Orleans movers, and 871,007 individuals from the 10 control cities). Chronic condition variables are only available for beneficiaries who have been continuously enrolled in fee-for-service Medicare over a condition-specific look-back window, typically two years. Chronic conditions sample sizes range from 31,027 to 37,944 for New Orleans individuals, from 12,532 to 15,797 for New Orleans movers, and from 656,230 to 769,430 for individuals from the ten control cities.

Table 2: Concise mortality difference-in-differences estimates

	(1)	(2) 2004 Cohort	(3)	(4)	(5) 1999 Cohort	(6)
2005 x New Orleans	0.56***	0.55***	0.56***	0.91***	0.91***	1.00***
	(0.16)	(0.16)	(0.16)	(0.18)	(0.19)	(0.15)
(2006–2013) x New Orleans	−0.48***	−0.49***	−0.36***	−0.49***	−0.48***	−0.23**
	(0.12)	(0.14)	(0.11)	(0.14)	(0.17)	(0.09)
Included controls	A	B	C	A	B	C
Dep. var. mean	5.55	5.55	5.55	6.31	6.31	6.31
Observations	7,987,100	7,987,100	7,986,926	10,470,949	10,470,949	10,470,692

Notes: 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)). The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Coefficients, standard errors (in parentheses), and the dependent variable mean 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 age (one-year bins), race, and sex combination. C additionally controls for age-race-sex effects by year. Standard errors are clustered by beneficiary baseline ZIP code. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Correlation between ex ante predicted mortality and destination mortality (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
Predicted mortality	0.0007	0.0016**	0.0004	0.0004	0.0006
	(0.0009)	(0.0008)	(0.0009)	(0.0008)	(0.0008)
Alzheimer's/dementia dropped	No	No	Yes	Yes	Yes
Chronic conditions predictors	None	Grouped	Grouped	Two-way interactions	Individual
Baseline spending predictors	None	Ventiles	Ventiles	Ventiles	Centiles
Dep. var. mean	5.40	5.40	5.39	5.39	5.39
Observations	26,467	12,319	11,268	11,268	11,268

Notes: The table reports the results of estimating equation (7) using increasingly rich sets of baseline characteristics to generate migrants' predicted mortality risk. The dependent variable is the average 2006–2013 mortality rate of the destination county's 2004 cohort. All regressions control for baseline ZIP code fixed effects. Both predicted mortality and destination mortality are expressed in percentage points. Standard errors (in parentheses) are clustered by each beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Died indicator				Pred. mort.
Mean death rate in 2006 county	0.85***	0.83***	0.86***	1.01***	0.94***	0.98***	−0.07
	(0.29)	(0.23)	(0.23)	(0.30)	(0.29)	(0.29)	(0.20)
Set of fixed effects	A	B	C	C	C	C	A
Alzheimer's/dementia dropped	No	No	No	No	Yes	Yes	Yes
Chronic conditions controls	No	No	No	Gr.	Gr.	All int.	No
Dep. var. mean	5.55	5.55	5.54	6.11	5.48	5.47	5.21
Observations	175,936	175,936	175,821	80,084	75,142	75,137	75,286
R-squared	0.00	0.04	0.05	0.09	0.08	0.08	0.04

Notes: The table reports estimates of equation (6). The dependent variable in columns (1)–(6) is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. The dependent variable in column (7) is the beneficiary's ex ante predicted mortality. Coefficients, standard errors (in parentheses), and dependent variable mean 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 age (one-year bins), race, and sex combination. C additionally controls for age-race-sex effects by year. Gr. means that indicators for eight groups of chronic conditions are included; All int. means that indicators for each possible interaction of the eight groups of chronic conditions are included. All specifications that include chronic condition controls also include fixed effects for centiles of baseline Medicare spending. Standard errors are clustered by each beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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, then 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

[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, hypothyroidism, benign prostatic hyperplasia, depression

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

[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).

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.1.4 Destination Characteristics

For our analysis of the effect of place on migrant mortality, we relate migrant outcomes to 21 destination county characteristics. The characteristics are derived from various sources and are intended to capture a broad range of environmental, economic, and public health conditions. Summary statistics for these characteristics among the sample of New Orleans movers are shown in Appendix Table A.17. Below, we list all of the characteristics, organized by data source. We then describe how each variable was constructed.

[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).

- Medicare administrative records
  - Mortality rate
  - Adjusted mortality rate
  - Medicare spending per beneficiary
- Census
  - Income per capita
  - Poverty rate, 65+
  - Median home value
  - Urban population share
- Area Resources Files
  - Physicians per capita
  - Hospital beds per capita
- CMS Hospital Compare
  - Hospital quality index
- Behavioral Risk Factor Surveillance System
  - Percent obese
  - Percent smoking
  - Percent exercising
- Chetty and Hendren (2018)
  - Upward income mobility (from p25)
  - Upward income mobility (from p75)
  - Social capital index
  - Crime rate
  - Local government spending per capita
  - Income segregation
- Climate
  - PM 2.5 concentrations
  - Hot days/year (90°F+)

Medicare administrative records We calculate mortality rates and medical spending using the annual Medicare beneficiary summary files. We calculate the raw mortality rate for each county as the annual mortality rate of the county's 2004 Medicare cohort (i.e., of Medicare beneficiaries who lived in that county as of March 2005) averaged over 2006–2013. We construct adjusted mortality rates for each county by demeaning raw mortality by interactions of one-year age bins, race, and sex, using the demographics of the county's 2004 cohort.

We measure local spending analogously to how we measure the raw mortality rate: by averaging the post-2006 Medicare spending across fee-for-service beneficiaries in the county's 2004 Medicare cohort. We exclude beneficiary-years where the beneficiary was not enrolled in fee-for-service Medicare for all twelve months.

Census We measure income, poverty, home values, urban population share, and total population for each county using 2000 Decennial Census data, which we obtain from the IPUMS National Historical Geographic Information System (NHGIS) (Manson et al., 2017). The table and dataset names we refer to below are from the NHGIS.

We measure income as per capita income in 1999 (table *NP082A* of dataset *2000_SF3a*). We measure the poverty share among the 65+ population as the number of individuals aged 65 or older with income in 1999 below the poverty level (table *NP087C* of dataset *2000_SF3a*) as a share of the 65+ population for whom poverty status can be determined (table *NP087C* of dataset *2000_SF3a*). We measure median home values as the median value of owner-occupied housing units (table *NH085A* of dataset *2000_SF3a*). Finally, we use the total population of a county (table *NP001A* of dataset *2000_SF1a*) as the denominator for physicians and hospital beds per capita.

Area Resources File (ARF) We obtain the number of physicians and hospital beds for each county in 2004 from the ARF. For the number of doctors, we use variable *F12129-04*, the total number of active MDs (federal and non-federal) in 2004, taken from the 2005 release of the ARF (sourced from the AMA Physician Master File). The variable *F08921-04*, taken from the 2009 release of the ARF (sourced from the AHA Survey Database), reports the total number of hospital beds in 2004.

We calculate the number of physicians per capita by dividing the total number of active MDs by the total population in the county (from census data, described above). Likewise, we calculate hospital beds per capita by dividing the total number of hospital beds by the county population.

Behavioral Risk Factor Surveillance System (BRFSS) We measure obesity, smoking, and exercise behavior using the BRFSS, a telephone survey that collects information on health-related behaviors and chronic conditions. We pool survey responses for the period 1995–2004.

We calculate percent smoking in each county as the percent of survey respondents for whom the reported smoking status is either "current, daily" or "current, other than daily". We calculate percent obese in each county as the percent of survey respondents who report a body mass index of 30 or greater. We calculate percent exercising in each county as the percent of survey respondents who report participating in any physical activities or exercises other than their regular job in the past month.

CMS Hospital Compare We measure hospital quality within each county using data from the CMS Hospital Compare Process of Care Scores for 2004, which we obtain from Sacarny (2018). We focus on process of care measures for heart attack (AMI), heart failure (HF), and pneumonia (PN), and restrict to metrics that are reported in at least 1,750 counties. This restriction selects a total of 13 metrics, consisting of four AMI metrics (*ami1_share*, *ami2_share*, *ami5_share*, *ami6_share*), three HF metrics (*hf1_share*, *hf2_share*, *hf3_share*), and six PN metrics (*pn1_share*, *pn2_share*, *pn3_share*, *pn4_share*, *pn5_share*, *pn6_share*).

For each process of care metric, we calculate the share of patients in each county who receive appropriate care according to that metric, among hospitals for whom the metric is reported. We combine these 13 process of care metrics into a single hospital quality index, defined as the county-level mean across all metrics (this mean will be missing if any of the underlying metrics are missing for that county). Thus, this hospital quality index can be loosely interpreted as the share of AMI/HF/PN patients receiving appropriate care in the county.

Chetty and Hendren (2018) We obtain county-level measures of upward income mobility, social capital, crime, local government spending, and income segregation from Chetty and Hendren (2018*b*). For measuring upward income mobility, we use the variables *pct_causal_p25_kr26* and *pct_causal_p75_kr26* from Online Data Table 2, "Preferred Estimates of Causal Place Effects by County."[5] The measures of upward income mobility capture the percentage change in income at age 26 from spending one more year of childhood in the county, for children whose parents were at the 25th or 75th percentiles, respectively, of the

[5]A description of the variables in Online Data Table 2 can be found at https://opportunityinsights.org/wp-content/uploads/2018/04/online_table2-2.pdf (accessed October 1, 2019).

US household income distribution.

The measures of social capital, crime, local government spending, and income segregation come from Online Data Table 4, "Complete County-Level Dataset: Causal Effects and Covariates." Specifically, we use the variables *scap_ski90pcm*, *crime_total*, *subcty_total_expend_pc*, and *cs00_seg_inc*.[6]

Climate We measure fine particulate (PM 2.5) air pollution concentrations and the number of extremely hot days using data recorded by ground monitor stations. We measure the average PM 2.5 concentration in a county for the period 2006–2013. We obtain PM 2.5 air pollution data from EPA's Air Quality System database, which provides hourly data at the pollution-monitor level for pollutants that are regulated by the Clean Air Act. We aggregate monitor readings to the daily level by averaging across hourly observations and then construct daily ZIP code level pollution measures by calculating the inverse distance-weighted average across all monitors located within 20 miles of the ZIP code centroid. We then average these daily values over the period 2006–2013. Finally, we aggregate ZIP code level average pollution concentrations to the county level by averaging across all ZIP codes matched to a county based on the county recorded for the plurality of Medicare beneficiaries living in that ZIP code.

Our source for daily temperature variables is the Global Historical Climatology Network GHCN-Daily database, which provides weather measurements from land surface stations across the United States. For the period 2006–2013, we calculate daily high and low temperatures for each ZIP code as the inverse distance-weighted average of all available daily maximum and minimum temperatures, respectively, for GHCN stations within a 20-mile radius of the ZIP code centroid. The daily average temperature for a ZIP code is calculated as the midpoint of the daily high and low temperatures. We calculate the number of days per year in which the average daily temperature exceeded 90°F in a ZIP code, and then aggregate to the county level using the same ZIP code to county crosswalk used to construct the pollution measure.

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 those of cohorts from other areas using the synthetic control method (Abadie, Diamond and Hainmueller, 2010, 2015). Because the synthetic

[6] A description of the variables in Online Data Table 4 can be found at https://opportunityinsights.org/wp-content/uploads/2018/04/online_table4-2.pdf (accessed October 1, 2019).

control method is not easily adaptable to individual-level controls and is not appropriate for studying how movers' subsequent mortality is related to local mortality, we do not adopt it as our primary specification. Here, we demonstrate that our main annual event study findings are similar if we use this approach.

We focus on the 1999 cohort, as this provides multiple years of pre-Katrina data to form the synthetic control unit. Because meaningful permutation inference would be difficult to do with only ten control cities, our set of potential control units consists either of all counties with a baseline beneficiary population of over 50,000 (yielding 157 counties, including New Orleans) or the 400 commuting zones (CZs) closest in log Medicare population to the city of New Orleans in 1999.[7] We exclude smaller counties and CZs because their annual death rates are inherently more variable, making them less reliable counterfactuals for New Orleans. Similarly, the largest CZs are less plausible counterfactuals. Whether implementing synthetic controls using control counties or CZs, we always use the city (not CZ) for the New Orleans region.

Because we are primarily interested in *changes* in the death rate, our outcome variable is a county's/CZ's annual death rate minus its 1999–2004 average death rate. We use this demeaned death rate in each year between 1999 and 2004 to form the synthetic treatment unit, whose outcomes are used as the counterfactual. The synthetic control algorithm assigns weights to the control units such that the difference between the demeaned death rate of the treated unit (the city of New Orleans) 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 produce standard errors directly. 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 same synthetic control methodology is applied in each case to produce a distribution of estimated treatment effects. Statistical inference is based on how often the "effect" for a control unit is more extreme than that of the actually treated unit (in this case, New Orleans). We follow this procedure, fixing the timing of the treatment in 2005.[8]

The results for the annual mortality rate are shown in Appendix Figure A.11. We plot the estimated treatment effect for the New Orleans cohort (black line) as well as the estimated

[7] Our results are similar when the set of potential control units consists of 460 counties that are at least as populous as the smallest of our ten control cities (16,469 beneficiaries at baseline); 89 counties with baseline populations of 50,000–100,000; or the 300 CZs that are closest in log number of beneficiaries to the city of New Orleans.

[8] The synthetic control method fails to converge for four of the county permutations. These are thus omitted from the sample, leaving us with 152 permutations where New Orleans is not the treated county.

"effects" when assigning each of the other counties/CZs to be the treated unit. The post-2005 New Orleans is an outlier, both with respect to the initial mortality increase in 2005 and the subsequent mortality decreases. Specifically, the 2005 treatment effect is always the largest for New Orleans, the true treated unit. In each year between 2006 and 2013, the estimated change in mortality for New Orleans is smaller than 141–152 of the other 152 counties for which we estimate a placebo treatment effect and 368–399 of the other 400 CZs. The implied p-values thus generally fall between 0 (in cases where the New Orleans mortality change is literally the largest/smallest) and 0.080 (the year when New Orleans has the 33rd smallest mortality change in the sample of CZs). The magnitudes of the estimated treatment effects in 2006–2013 are slightly larger than in our main regression event study, ranging from -0.31 percentage points to -0.93 percentage points.

Using the estimated change in the mortality rate in each period and the empirical survival probability of each county's/CZ's cohort, we calculate the implied change in cumulative mortality by 2013 (equation (3)). We plot the density of these estimates in Appendix Figure A.12. The red line indicates where the true treatment effect (i.e., that of New Orleans) falls in the distribution. We estimate that Hurricane Katrina caused 2013 cumulative mortality among victims initially living in New Orleans to decline by -2.13 (-2.30) percentage points when using CZs (counties) as synthetic controls. Only five counties and three CZs have larger falls in the 2013 cumulative mortality rate than New Orleans, implying p-values of 0.039 and 0.010, respectively.

A.3 The Importance of Rebuilding in New Orleans

In this section, we briefly 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 in the paper, the decision regarding whether to leave or stay was highly correlated with observable predictors of mortality risk.

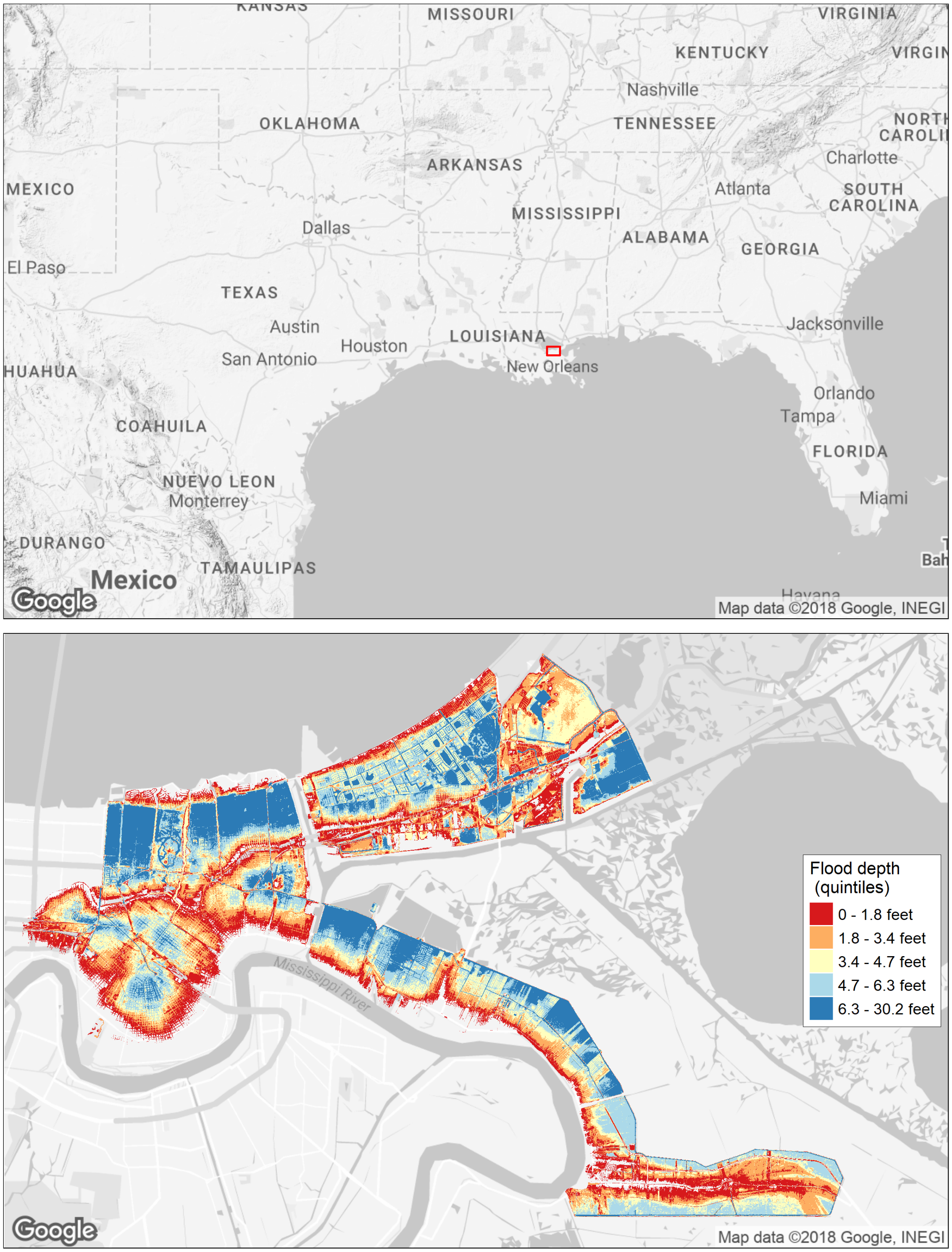
New Orleans infrastructure was devastated following Hurricane Katrina, however, and therefore it is likely that any health improvements accruing to New Orleans stayers would have developed over time during rebuilding. To empirically test this intuition, we restrict the sample to individuals from the 2004 cohort who survived until at least the beginning of 2006.

We then estimate equation (2) with only individuals who were still living in their baseline CZ as of March 2006 (“stayers”). Because survival until 2006 is necessary for inclusion, the reference category is 2006. Figure A.14a plots the results, which show that stayers’ mortality did not improve over time relative to 2006.[9] This pattern suggests it is unlikely that the cohort-level mortality declines among hurricane victims reflect health improvements from remaining in New Orleans.

[9] Appendix Figure A.14b shows the results of an analogous exercise for movers.

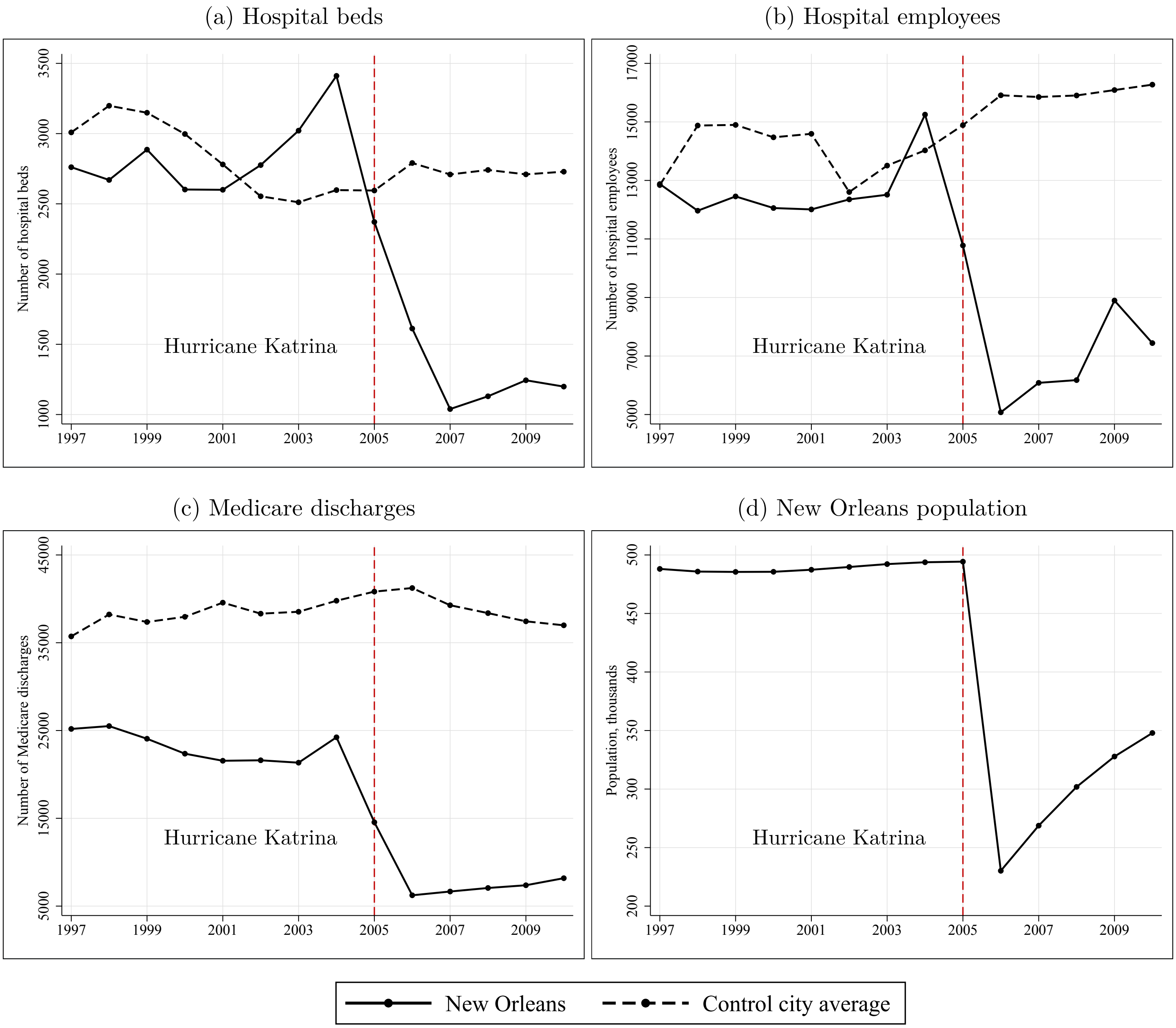
Appendix Figures and Tables

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

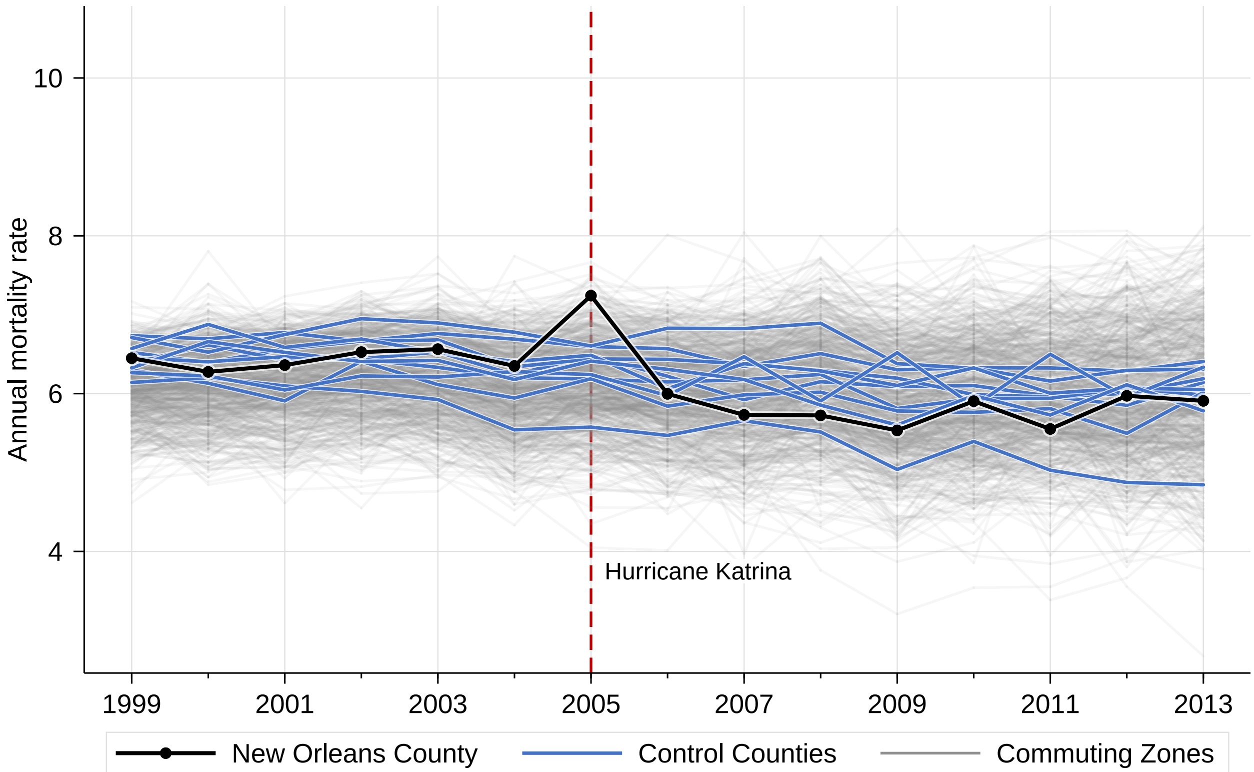


Notes: The figure shows the number of hospital beds (panel (a)), the number of hospital employees (panel (b)), and the number of Medicare discharges (panel (c)) in New Orleans and the ten control cities we use for our baseline difference-in-differences analysis. Panel (d) shows the New Orleans population. 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

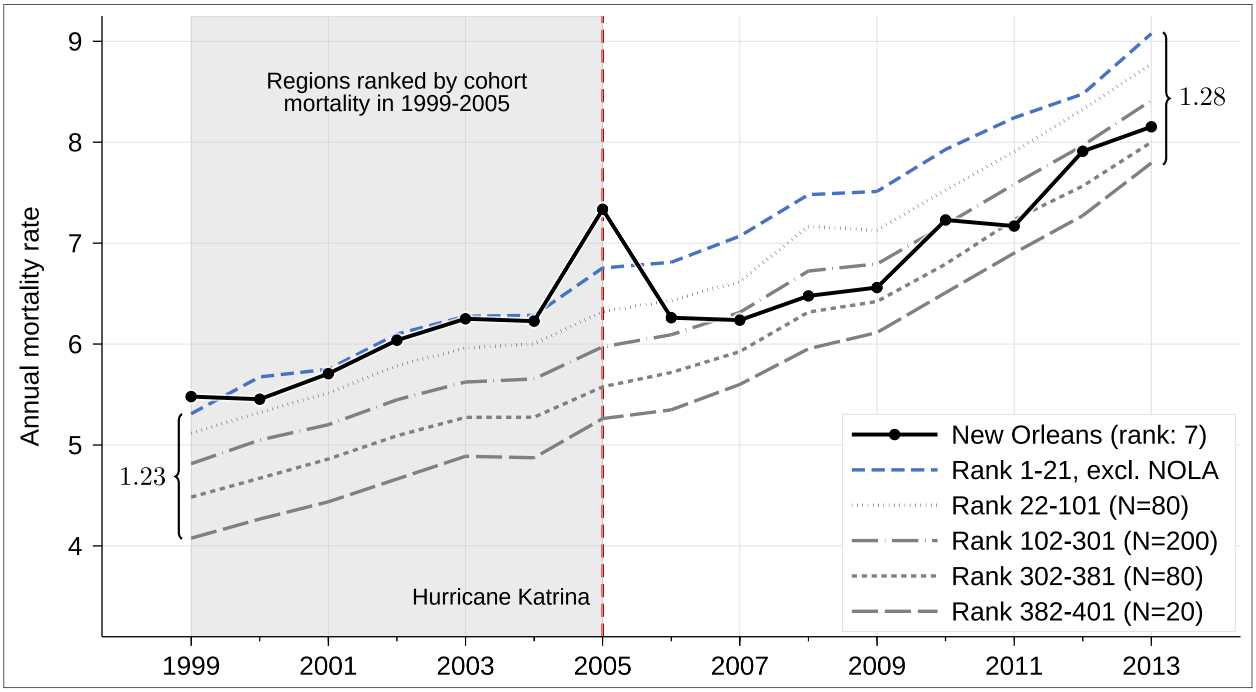
Notes: The figure shows the location of New Orleans and each of the ten control cities used to construct comparison cohorts for identifying the effects of Hurricane Katrina on Medicare beneficiaries initially residing in New Orleans.

Figure A.4: Cohort annual adjusted mortality rates for New Orleans versus other areas

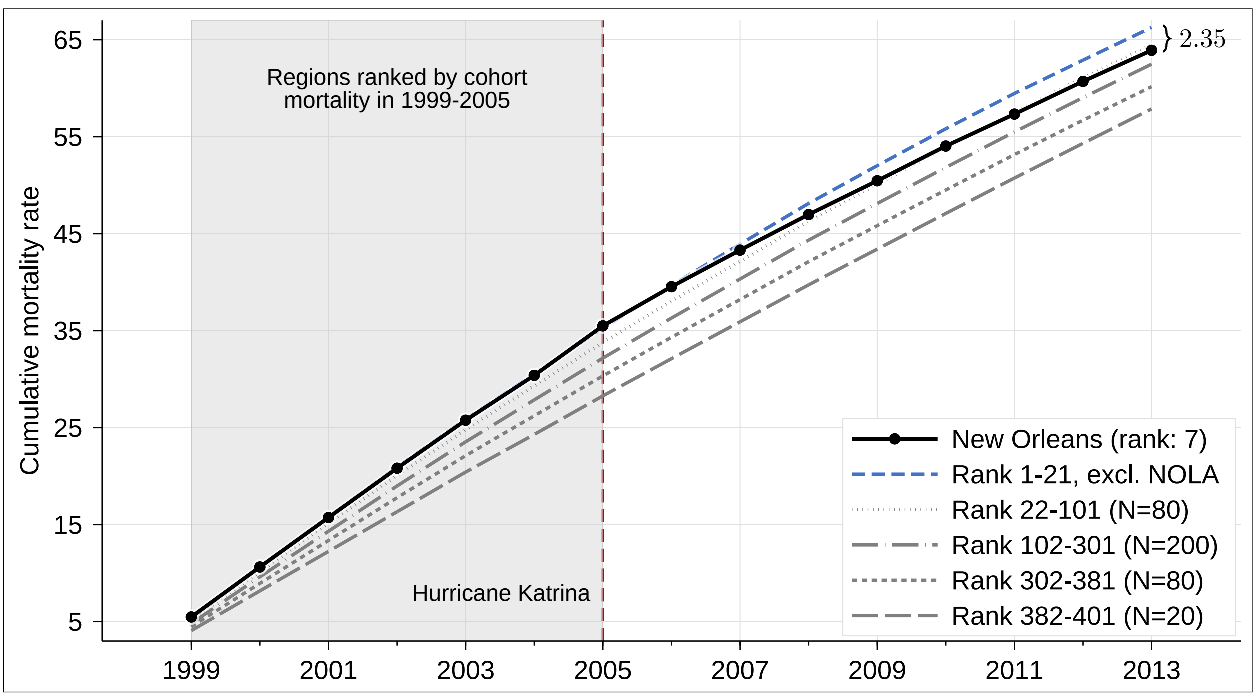


Notes: The figure shows annual death rates for the 1999 Medicare cohort, by initial region of residence, after adjusting for all combinations of beneficiaries' current age (in one-year bins), race, and sex and adding the overall mean mortality rate to the results. Mortality rates for the New Orleans county cohort are plotted in black, and mortality rates for the ten control county cohorts are plotted in blue. The light gray lines plot mortality rates for each US commuting zone cohort with at least 1,000 beneficiaries, except for the New Orleans commuting zone.

Figure A.5: Mortality rates by regional cohort mortality rank in 1999–2005



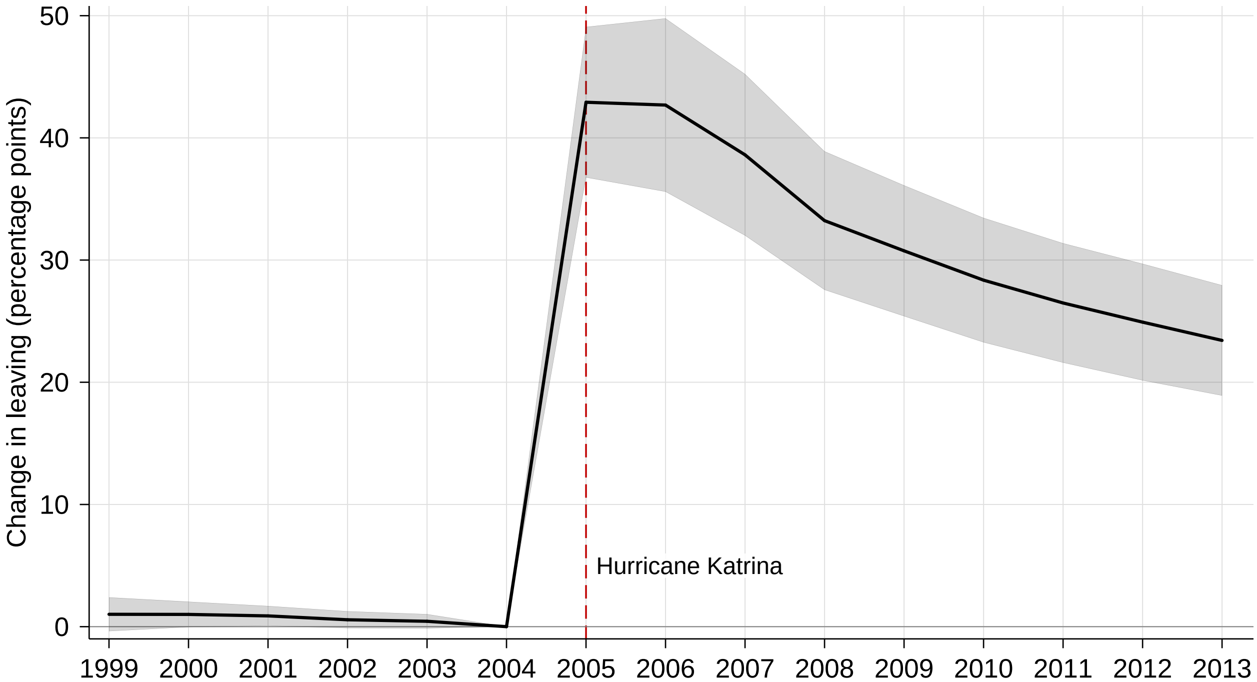
(a) Annual mortality rates



(b) Cumulative mortality rates

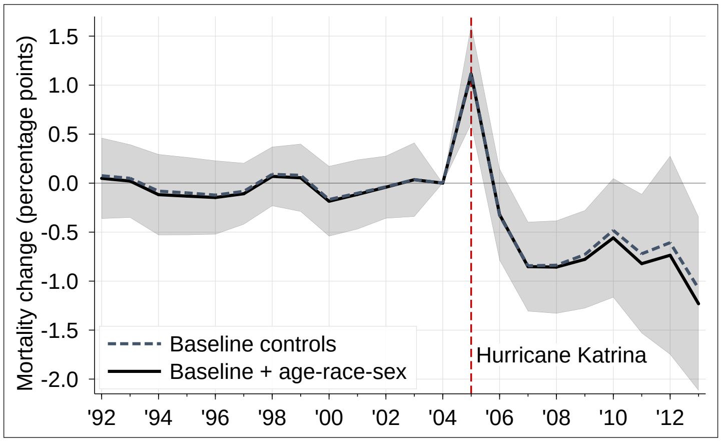
Notes: The 400 commuting zones closest to New Orleans county in 1999 Medicare population are ranked and grouped by the average mortality rate of their 1999 cohort over the period 1999–2005. New Orleans county is reported as its own group (solid black line). The next 20 highest mortality regions—those of rank 1–21, excluding New Orleans—correspond to the dashed blue line. Panel (a) shows average mortality across cohorts in each group, by year. Panel (b) shows cumulative mortality across cohorts in each group, by year.

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

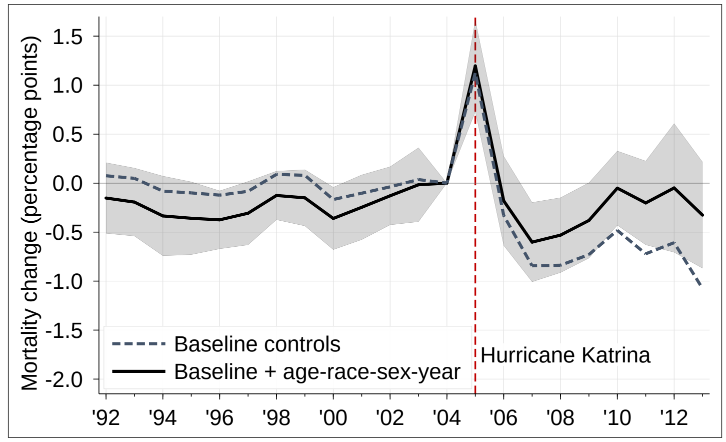


Notes: The figure shows estimates of changes in the probability that an individual is living outside of their 1999 CZ of residence. The black line plots estimates from equation (2), where the dependent variable is a relocation indicator equal to zero if a beneficiary was living in his or her 1999 CZ of residence in that year and is equal to one if the beneficiary was living in a different CZ. The gray shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary 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.

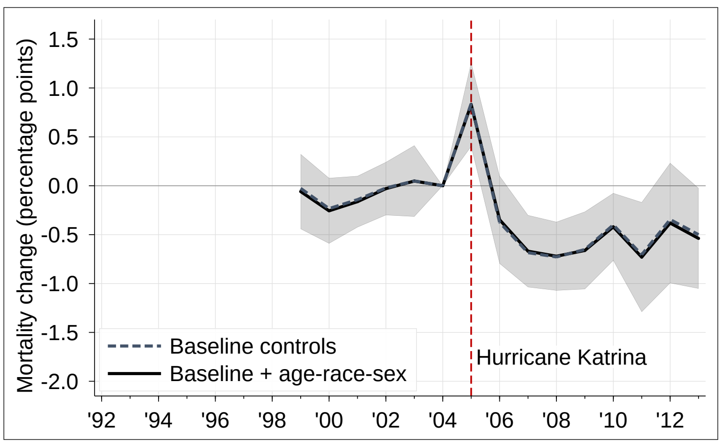
Figure A.7: Annual mortality event studies with demographic controls



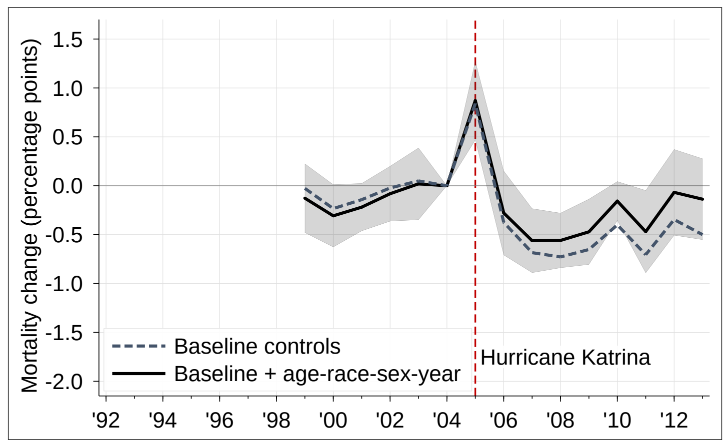
(a) 1992 Medicare cohort: age-race-sex controls



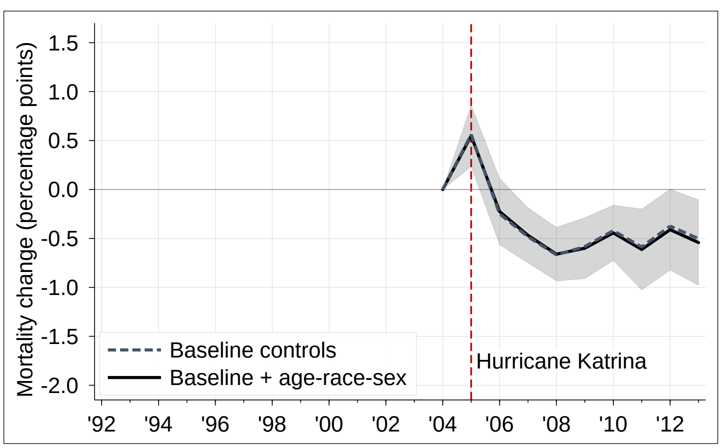
(b) 1992 Medicare cohort: age-race-sex-year controls



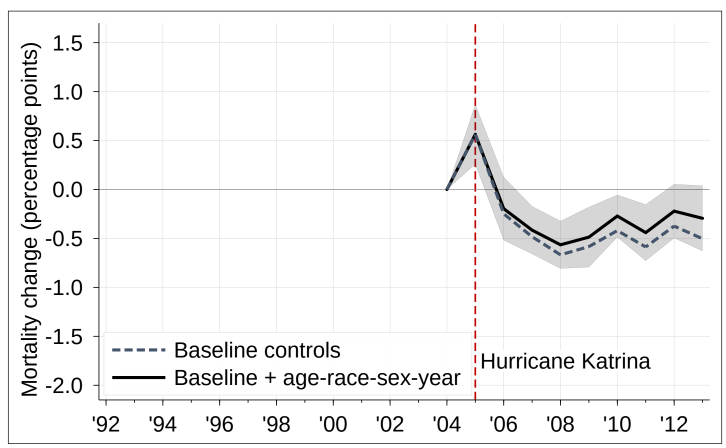
(c) 1999 Medicare cohort: age-race-sex controls



(d) 1999 Medicare cohort: age-race-sex-year controls



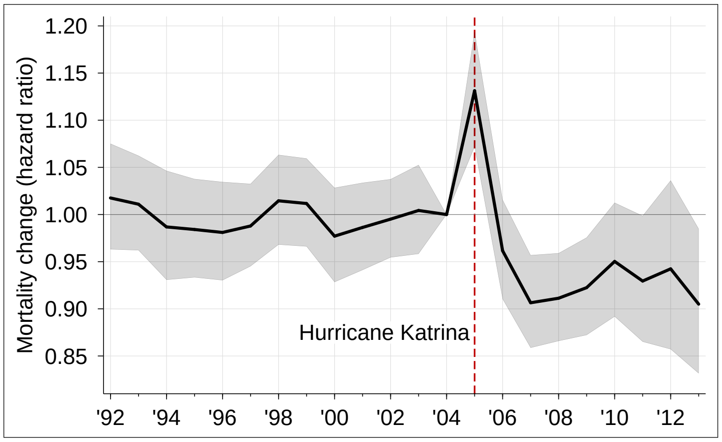
(e) 2004 Medicare cohort: age-race-sex controls



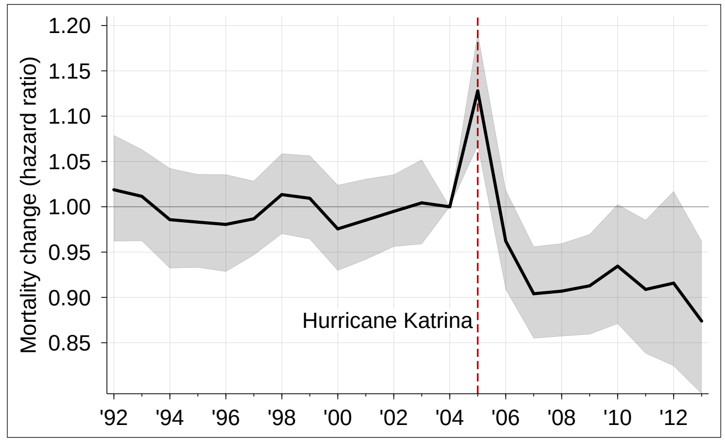
(f) 2004 Medicare cohort: age-race-sex-year controls

Notes: The black lines plot difference-in-differences event study estimates from equation (2) but is modified to include demographic controls. Panels in the left column control for all combinations of baseline age (one-year bins), race, and sex. Panels in the right column further control for age-race-sex effects by year. The gray shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary baseline ZIP code. For reference, the dashed lines show the baseline estimates reported in Figures 3 and 4.

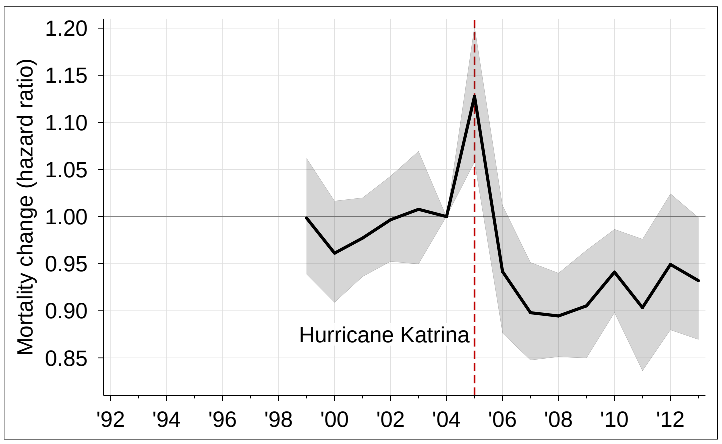
Figure A.8: Survival analysis of Hurricane Katrina



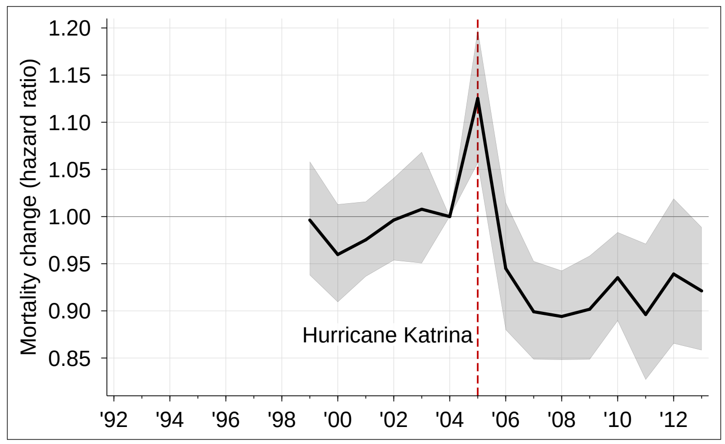
(a) No demographic controls (1992 cohort)



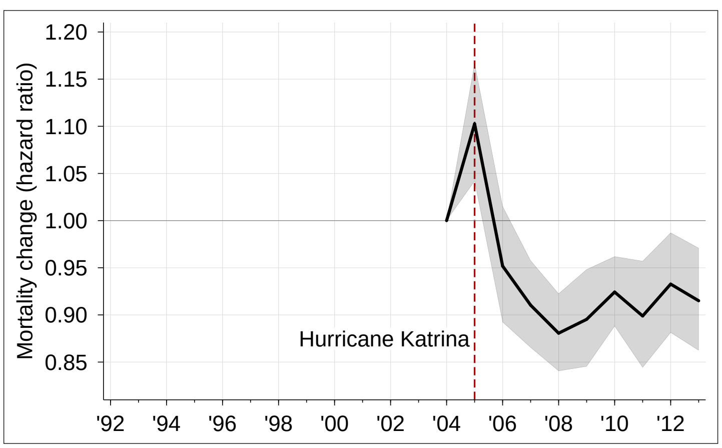
(b) Baseline age-race-sex controls (1992 cohort)



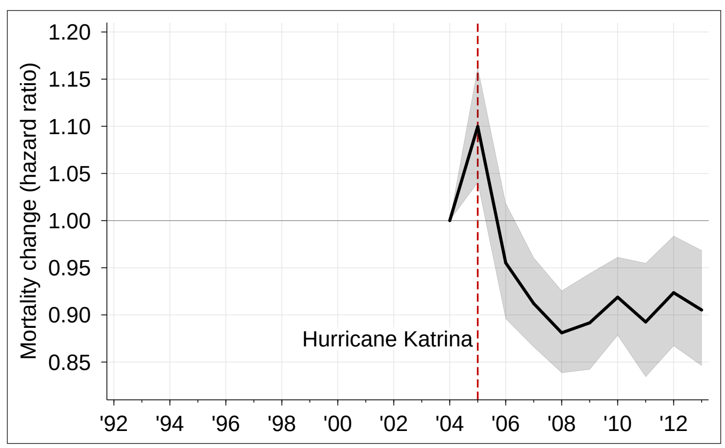
(c) No demographic controls (1999 cohort)



(d) Baseline age-race-sex controls (1999 cohort)



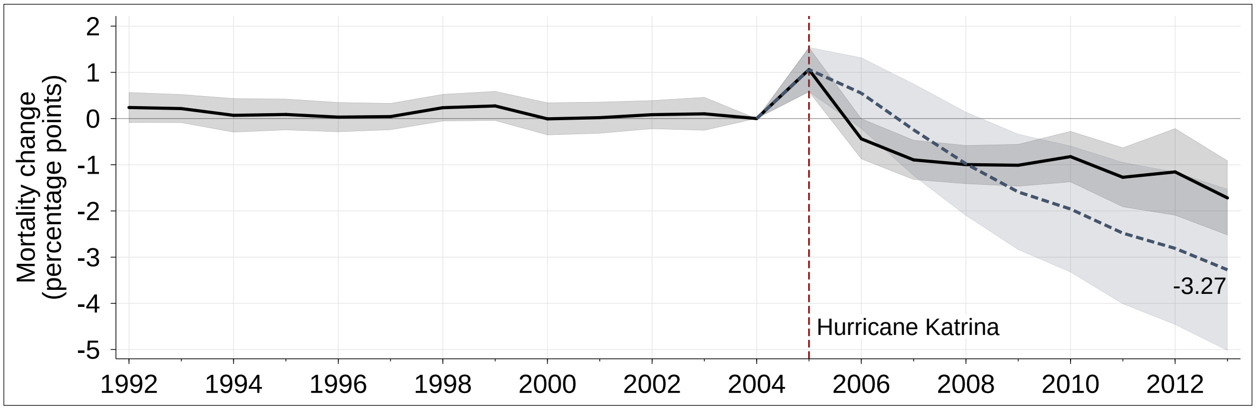
(e) No demographic controls (2004 cohort)



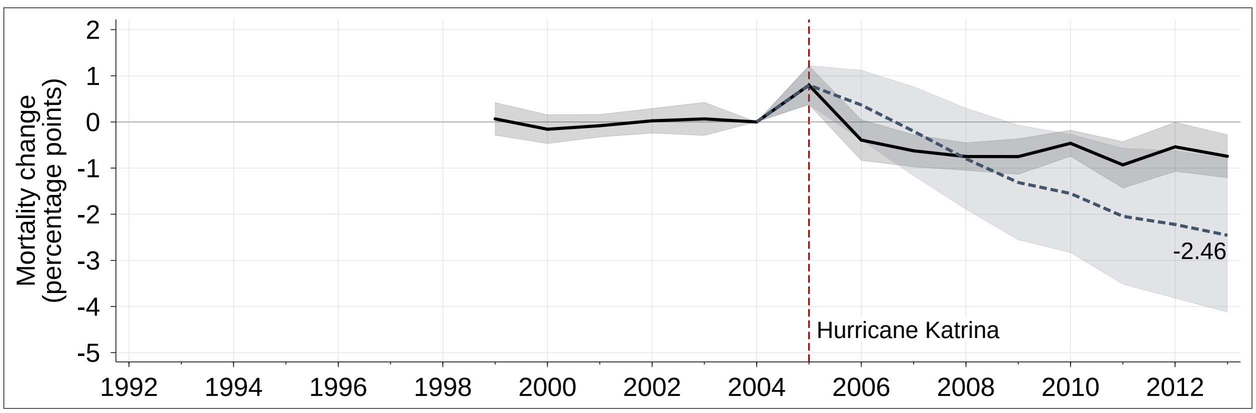
(f) Baseline age-race-sex controls (2004 cohort)

Notes: Each panel reports results from a discrete time (annual) survival analysis of Hurricane Katrina. We fit proportional hazard models that include interactions of year and New Orleans fixed effects, allowing the hazard rate to vary arbitrarily over time for New Orleans and control counties. The panels report estimated hazard ratios (exponentiated coefficients) for the year and New Orleans interactions using 2004 as the reference year, analogous to the difference-in-differences estimates reported in Figures 3–4. Proportional hazard models with no other controls (reported in the left column) are equivalent to the linearly additive hazard model in equation (2) with no demographic controls. Proportional hazard models that also control for all combinations of baseline age (one-year bins), race, and sex are reported in the right column. The gray shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary baseline ZIP code. Estimates are provided for the 1992, 1999, and 2004 Medicare cohorts, as indicated in the caption to each panel.

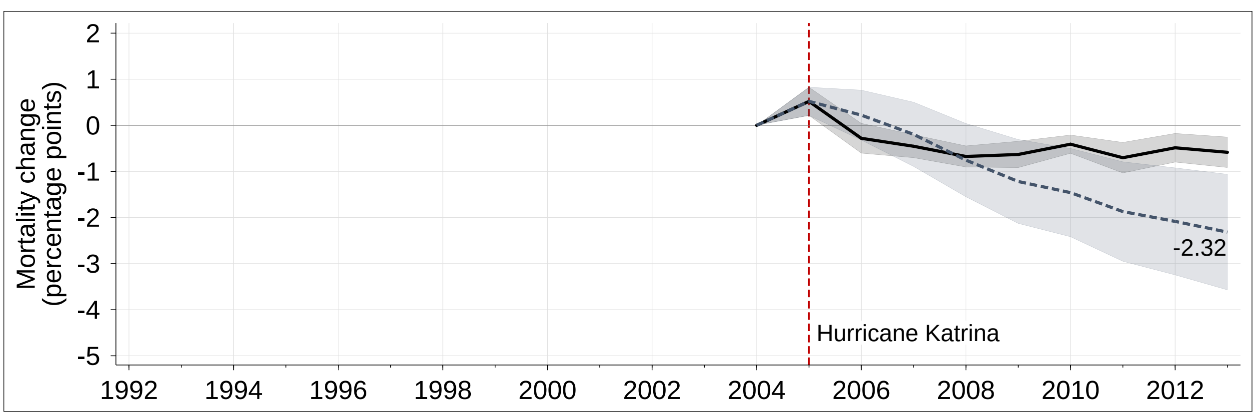
Figure A.9: Long-run mortality effects of Hurricane Katrina using the United States (except New Orleans) as the control group



(a) 1992 Medicare cohort



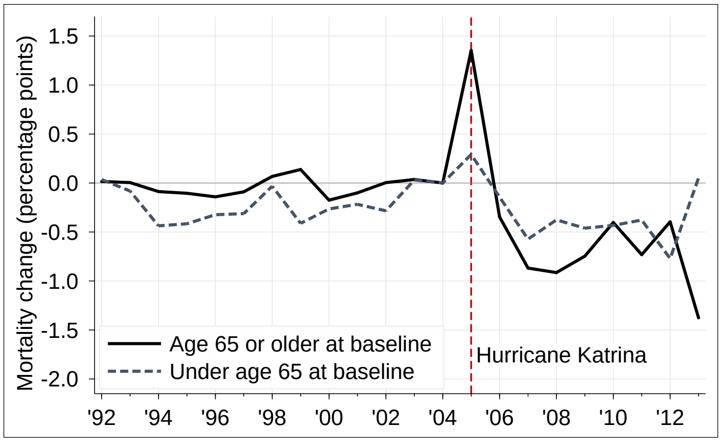
(b) 1999 Medicare cohort



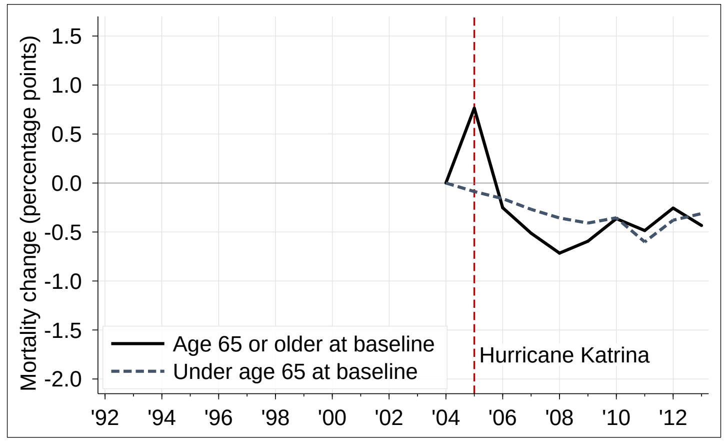
(c) 2004 Medicare cohort

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 one if the beneficiary died in a given calendar year and is equal to zero 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.

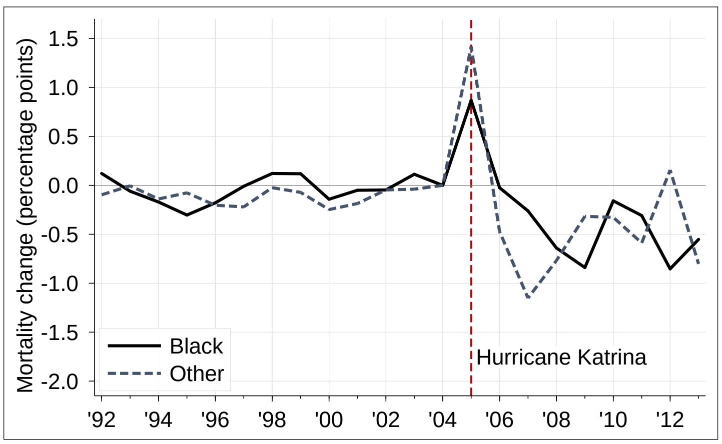
Figure A.10: Annual mortality event studies by baseline demographics



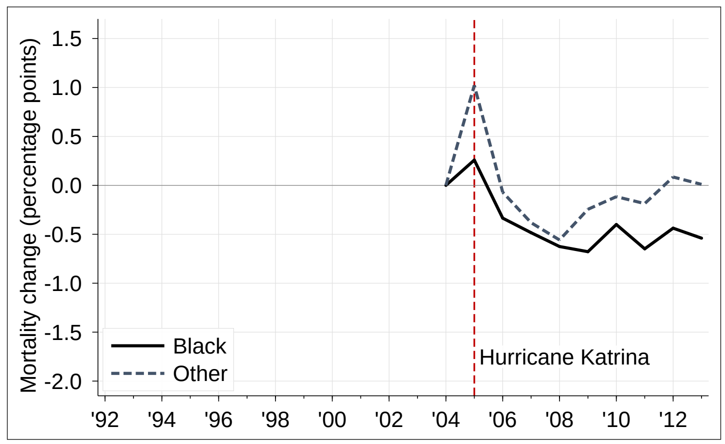
(a) Baseline age (1992 cohort)



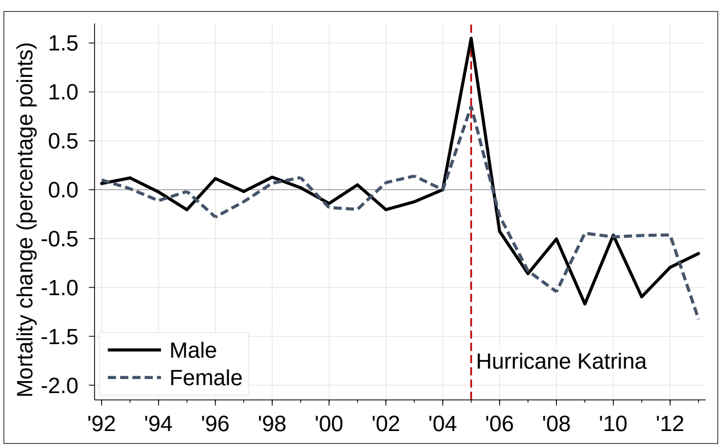
(b) Baseline age (2004 cohort)



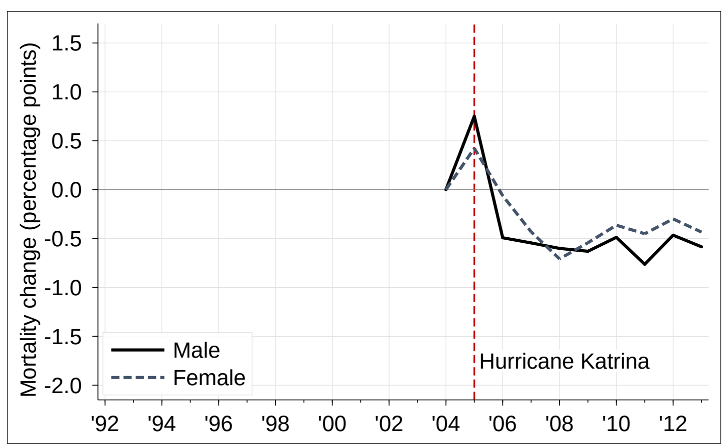
(c) Race (1992 cohort)



(d) Race (2004 cohort)



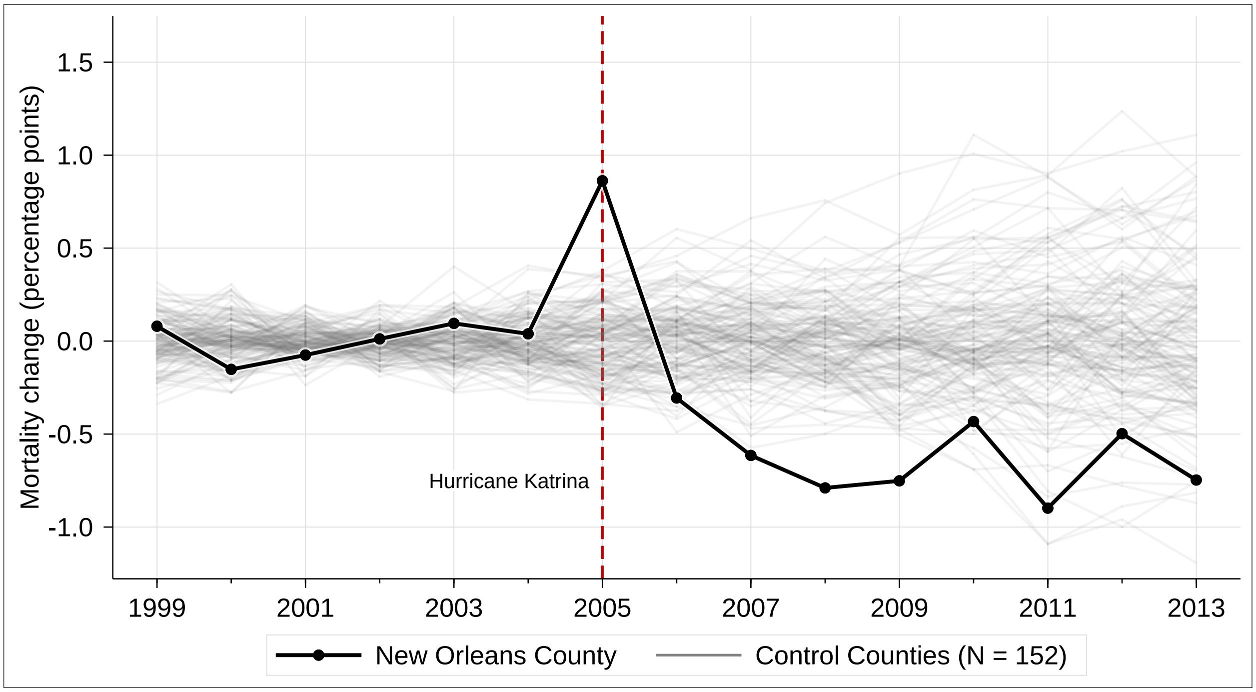
(e) Sex (1992 cohort)



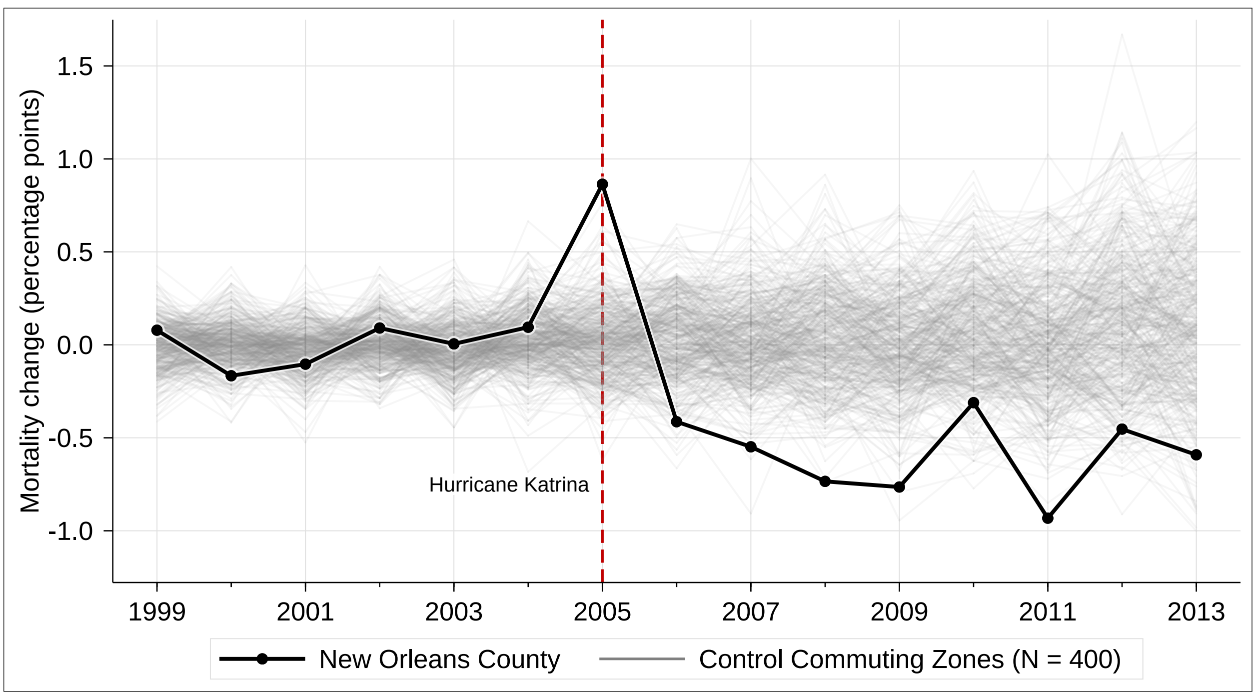
(f) Sex (2004 cohort)

Notes: Each panel reports difference-in-differences event study estimates from equation (2), but they are estimated separately by the baseline demographic group indicated by the legend. Estimates are provided for both the 1992 and 2004 Medicare cohorts, as indicated in the caption to each panel.

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



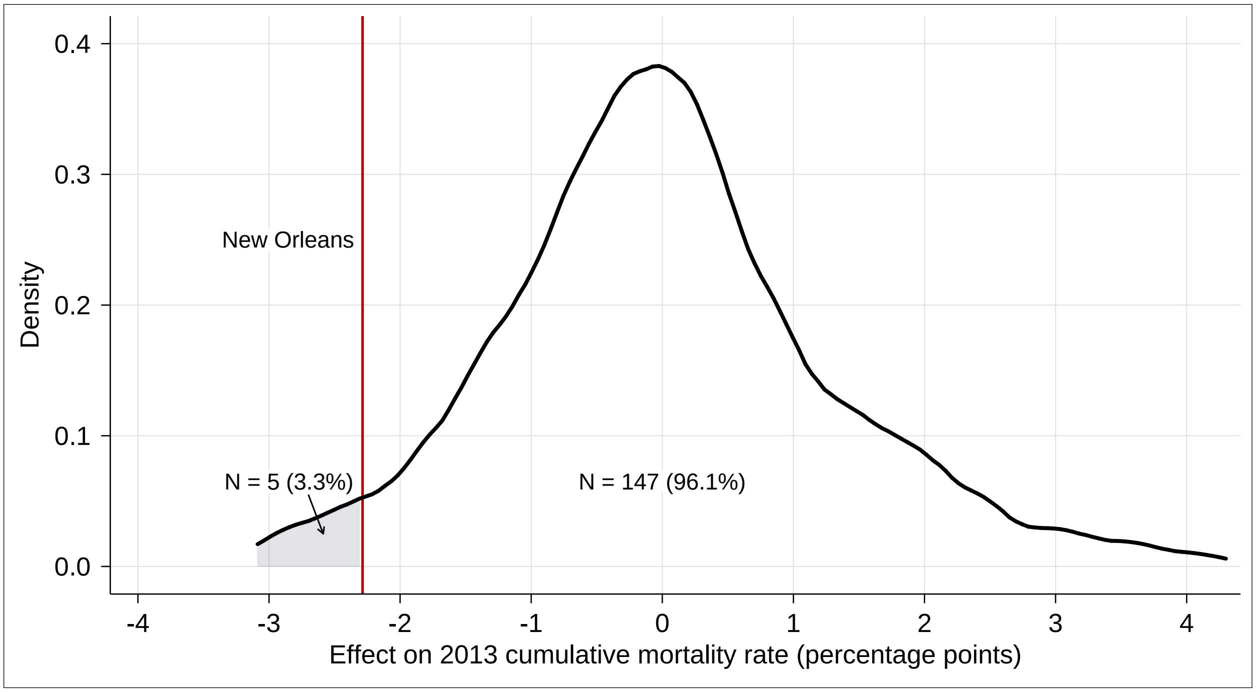
(a) Control units: Counties with 50,000 or more beneficiaries at baseline



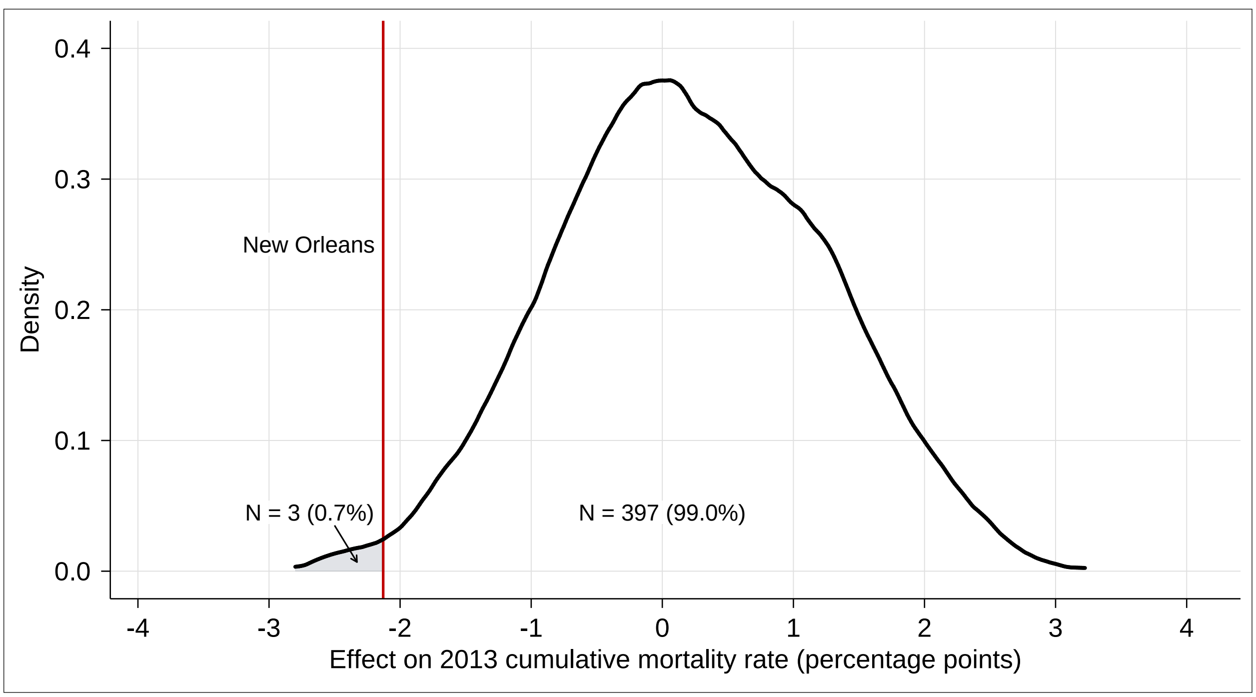
(b) Control units: Commuting zones closest to New Orleans county in baseline population

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 county/commuting zone in the sample, also estimated using the synthetic control method. Estimates have been scaled by 100 to reflect changes in percentage points.

Figure A.12: Cumulative mortality effects of Hurricane Katrina, synthetic control method



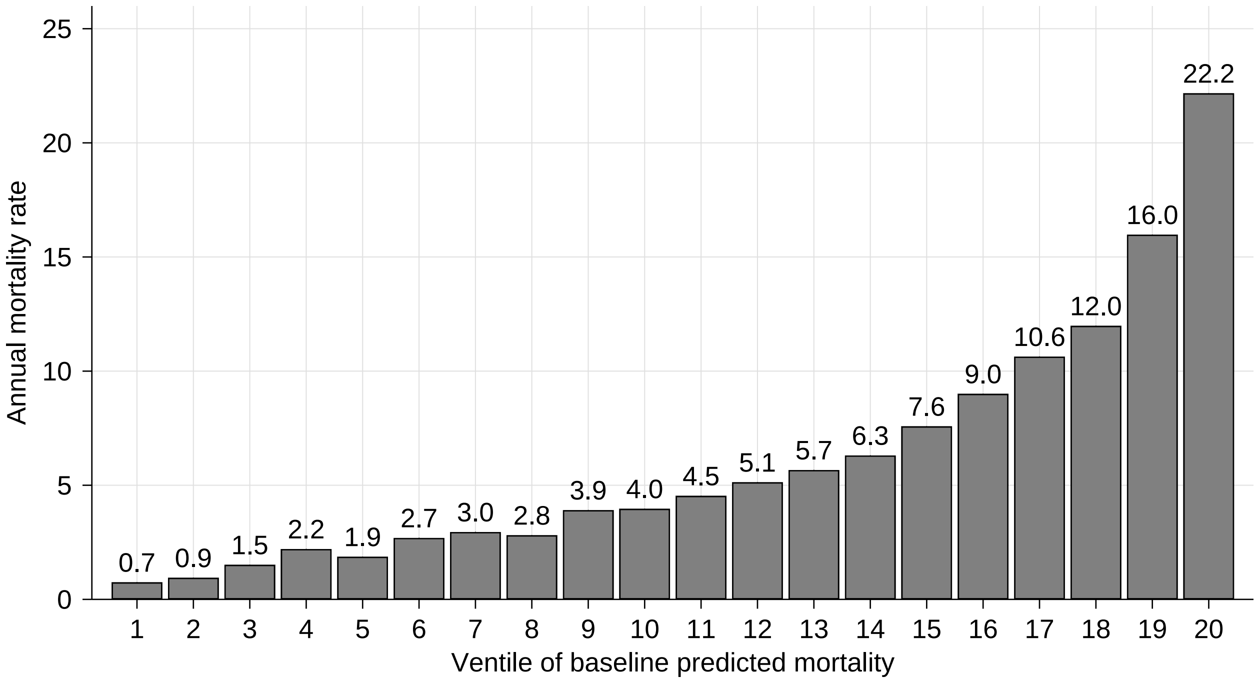
(a) Control units: Counties with 50,000 or more beneficiaries at baseline



(b) Control units: 400 commuting zones closest to New Orleans county in baseline population

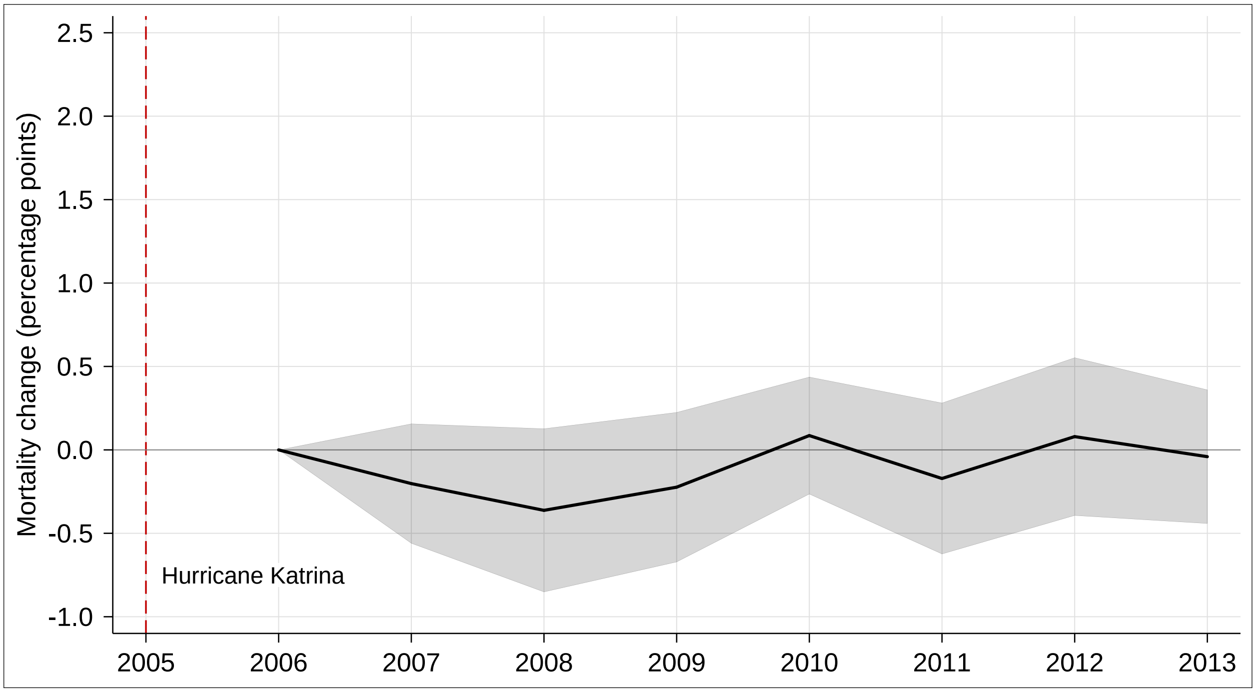
Notes: The black lines plot the density of the effects of a 2005 event on the 2013 cumulative mortality rates of regions in our sample, estimated using the synthetic control method. The red line indicates where the New Orleans county effect falls in that distribution. Text labels describe how many control regions have a cumulative mortality effect that is less or greater than the cumulative mortality effect in New Orleans.

Figure A.13: Movers' realized mortality by ex ante predicted mortality

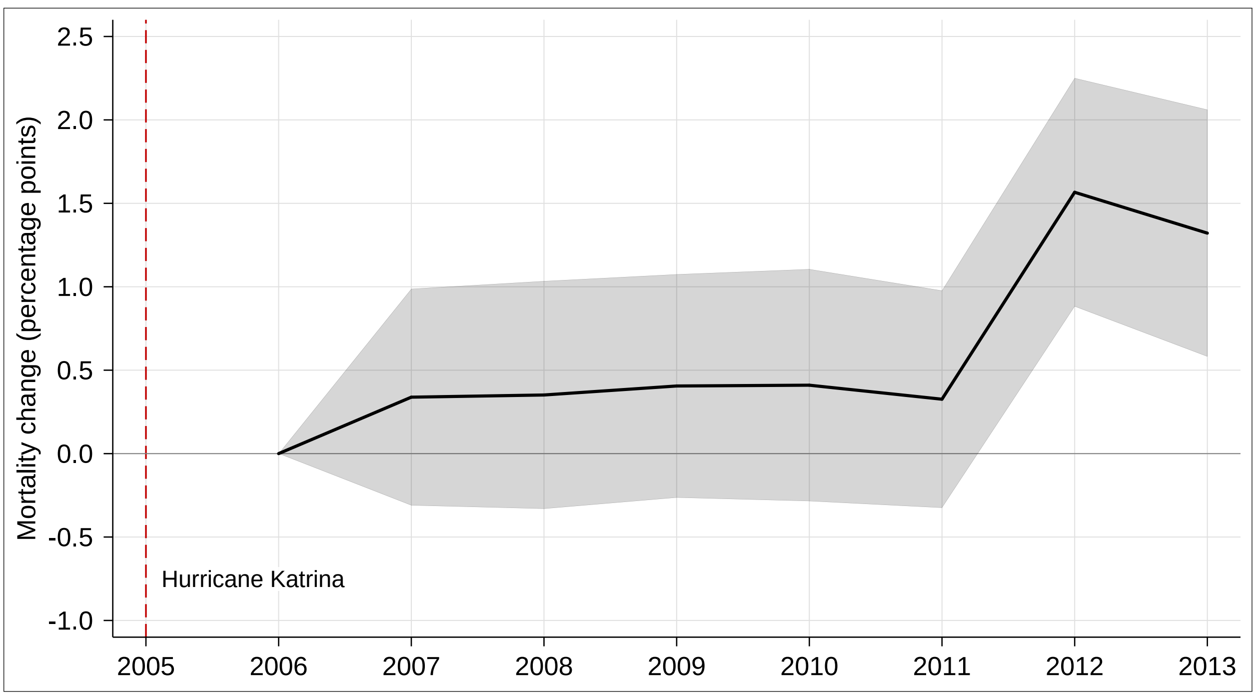


Notes: The figure shows realized annual mortality in 2006–2013 among Hurricane Katrina movers by ventile of ex ante predicted mortality. The mortality prediction model, described in Section 5.3.1, is estimated using the 2004 control county cohorts over the period 2006–2013. The fitted model is then used to generate out-of-sample mortality predictions for individuals in the 2004 New Orleans cohort, providing a time-invariant index of an individual's ex ante mortality risk over the period 2006–2013.

Figure A.14: Post-Katrina changes in stayers' and movers' mortality over time



(a) Post-Katrina stayers



(b) Post-Katrina movers

Notes: The figure shows estimates and 95 percent confidence intervals from estimating equation (2) over the period 2006–2013, using 2006 as the reference year. Panel (a) shows results estimated for the sample of stayers, i.e., beneficiaries who, as of March 2006, were living in their baseline (2004) CZ of residence. Panel (b) shows results estimated for the sample of movers, i.e., beneficiaries who, as of March 2006, were living outside of their baseline (2004) CZ of residence. The dependent variable is a mortality indicator equal to zero if a beneficiary is alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Standard errors are clustered by beneficiary baseline ZIP code. Coefficients and confidence intervals have been scaled by 100 to reflect changes in percentage points.

Table A.1: Annual mortality rates, New Orleans and control cohorts

	(1)	(2)	(3)	(4)	(5)	(6)
	1992 Cohorts		1999 Cohorts		2004 Cohorts	
	New Orleans	Control cities	New Orleans	Control cities	New Orleans	Control cities
1992	5.07	4.92				
1993	5.44	5.32				
1994	5.45	5.46				
1995	5.72	5.75				
1996	5.90	5.94				
1997	6.15	6.15				
1998	6.64	6.46				
1999	7.09	6.93	5.48	5.25		
2000	7.06	7.14	5.45	5.43		
2001	7.37	7.39	5.71	5.59		
2002	7.80	7.74	6.04	5.79		
2003	8.10	7.97	6.25	5.93		
2004	8.10	8.00	6.23	5.96	5.14	4.84
2005	9.67	8.44	7.34	6.22	5.92	5.06
2006	8.38	8.61	6.26	6.36	5.19	5.14
2007	8.28	9.02	6.24	6.64	5.16	5.34
2008	8.77	9.51	6.48	6.93	5.20	5.56
2009	8.91	9.54	6.56	6.93	5.29	5.57
2010	9.72	10.10	7.23	7.35	5.78	5.89
2011	9.93	10.55	7.17	7.59	5.76	6.04
2012	10.59	11.10	7.91	7.97	6.21	6.27
2013	10.70	11.69	8.15	8.37	6.43	6.62
Cohort size	71,433	973,938	67,649	967,062	65,457	941,685

Notes: The table reports the annual mortality rate (in percentage points) of the cohort specified at the top of each column.

Table A.2: Top 20 destinations for New Orleans movers

	(1) Number of movers	(2) Percent of movers	(3) Dest. mort. rate	(4) Dest. Medicare spending
Harris, Texas	3,754	14.18	5.27	14,399
East Baton Rouge, Louisiana	2,552	9.64	5.55	12,210
Dallas, Texas	1,211	4.58	5.32	13,509
Bexar, Texas	604	2.28	5.17	12,045
Tarrant, Texas	554	2.09	5.46	13,030
Lafayette, Louisiana	435	1.64	5.35	12,062
Fulton, Georgia	429	1.62	5.36	10,909
Tangipahoa, Louisiana	403	1.52	5.66	15,246
DeKalb, Georgia	391	1.48	5.13	10,592
Travis, Texas	365	1.38	5.01	11,757
Cobb, Georgia	316	1.19	5.1	10,844
Ascension, Louisiana	314	1.19	5.28	12,981
Caddo, Louisiana	311	1.18	5.95	13,227
Shelby, Tennessee	302	1.14	5.64	11,429
Los Angeles, California	292	1.1	4.93	14,495
St. Landry, Louisiana	286	1.08	5.53	13,327
Rapides, Louisiana	282	1.07	5.49	12,014
Hinds, Mississippi	266	1.01	5.35	11,604
Fort Bend, Texas	264	1	4.64	12,701
Gwinnett, Georgia	214	0.81	4.86	10,061
Total	13,545	51.18		

Notes: The table reports the number and percent of movers to each of the top 20 destination counties for New Orleans migrants as well as the destination mortality rate and per-beneficiary Medicare spending. Percentages are relative to the total number of movers.

Table A.3: Weekly mortality values for Figure 2

Event week	(1) Deaths per thousand (New Orleans)	(2) Deaths per thousand (control)	(3) Effect on death rate per thousand people
−8	0.96	1.25	−0.29*
			(0.15)
−7	1.41	1.27	0.14
			(0.24)
−6	0.95	1.21	−0.25*
			(0.13)
−5	1.31	1.42	−0.11
			(0.15)
−4	1.24	1.25	−0.02
			(0.12)
−3	1.26	1.27	−0.01
			(0.15)
−2	1.59	1.32	0.27
			(0.17)
−1	1.66	1.27	0.39**
			(0.16)
0	7.37	1.37	6.01***
			(0.68)
1	1.92	1.26	0.66***
			(0.15)
2	2.11	1.34	0.76**
			(0.32)
3	1.70	1.30	0.41*
			(0.22)
4	1.79	1.44	0.36**
			(0.18)
5	1.65	1.24	0.41**
			(0.19)
6	1.58	1.31	0.27
			(0.22)
7	1.46	1.29	0.17
			(0.26)
8	1.83	1.33	0.50**
			(0.22)
9	1.80	1.46	0.34*
			(0.20)
10	1.43	1.32	0.11
			(0.18)
Dep. var. mean			1.39
Observations	4,356,235	61,734,576	66,090,811

Notes: Columns (1) and (2) report raw weekly mortality rates for the 2004 New Orleans and control city cohorts, respectively. Column (3) reports estimates of equation (1). Estimates prior to eight weeks before Hurricane Katrina and more than ten weeks after Hurricane Katrina are omitted for space, but estimates for all weeks are plotted in Figure 2. Mortality rates have been scaled by 1,000, 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 code. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.4: Point estimates and cumulative survival statistics for Figure 3

	(1) Effect on annual mortality rate	(2) Effect on cumulative mortality	(3) Cumulative survival to beginning of year	(4) Effect on Pr(leaving 2004 CZ)
2005	0.56***	0.56***	100.00	48.3***
	(0.16)	(0.16)		(3.5)
2006	−0.25	0.29	94.86	46.8***
	(0.17)	(0.28)		(3.9)
2007	−0.48***	−0.16	89.24	42.0***
	(0.13)	(0.36)		(3.5)
2008	−0.67***	−0.71*	84.61	35.8***
	(0.12)	(0.42)		(2.9)
2009	−0.58***	−1.14**	80.24	33.1***
	(0.15)	(0.48)		(2.7)
2010	−0.42***	−1.39***	76.07	30.5***
	(0.11)	(0.51)		(2.6)
2011	−0.59***	−1.73***	72.05	28.4***
	(0.18)	(0.57)		(2.4)
2012	−0.37**	−1.88***	67.89	26.6***
	(0.17)	(0.62)		(2.3)
2013	−0.50***	−2.07***	63.98	25.1***
	(0.18)	(0.67)		(2.2)
Dep. var. mean	5.55			10.4
New Orleans individuals surviving until 2005			65,457	
Observations	7,987,100	7,987,100		7,982,773

Notes: Columns (1), (2), and (4) report estimates of equations (2) and (3). Column (3) reports the empirical survival of the 2004 New Orleans cohort. All regressions include baseline ZIP code and year fixed effects. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. Standard errors are clustered by beneficiary baseline ZIP code. Outcome variables are indicated at top of each column. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5: Point estimates for Figure 4 and Appendix Figure A.6

	(1) Effect on annual mortality rate (1992 cohort)	(2) Effect on annual mortality rate (1999 cohort)	(3) Effect on Pr(leaving 2004 CZ) (1999 cohort)
1992	0.08		
	(0.18)		
1993	0.05		
	(0.17)		
1994	−0.08		
	(0.20)		
1995	−0.10		
	(0.18)		
1996	−0.12		
	(0.17)		
1997	−0.08		
	(0.16)		
1998	0.09		
	(0.16)		
1999	0.08	−0.03	1.01
	(0.17)	(0.19)	(0.70)
2000	−0.17	−0.23	1.00*
	(0.19)	(0.17)	(0.53)
2001	−0.10	−0.14	0.88**
	(0.18)	(0.13)	(0.41)
2002	−0.04	−0.02	0.57
	(0.17)	(0.14)	(0.35)
2003	0.04	0.05	0.44
	(0.19)	(0.19)	(0.30)
2005	1.13***	0.84***	42.92***
	(0.25)	(0.22)	(3.14)
2006	−0.33	−0.37	42.69***
	(0.23)	(0.23)	(3.61)
2007	−0.84***	−0.68***	38.61***
	(0.23)	(0.18)	(3.36)
2008	−0.84***	−0.73***	33.23***
	(0.23)	(0.16)	(2.89)
2009	−0.73***	−0.65***	30.76***
	(0.25)	(0.20)	(2.73)
2010	−0.48	−0.40**	28.36***
	(0.30)	(0.16)	(2.59)
2011	−0.72**	−0.71***	26.49***
	(0.34)	(0.27)	(2.49)
2012	−0.61	−0.35	24.92***
	(0.49)	(0.28)	(2.43)
2013	−1.08**	−0.50**	23.42***
	(0.43)	(0.25)	(2.30)
Dep. var. mean	0.07	0.06	0.13
Observations	12,446,594	10,470,951	10,463,469

Notes: The table reports estimates of equation (2) from the main text. The dependent variable is indicated at the top of each column. All regressions include ZIP code and year fixed effects. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. Standard errors are clustered by beneficiary baseline ZIP code. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.6: 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
All	0.56***		−0.48***		100.0		7,987,100
	(0.16)		(0.12)				
Experienced 2+ feet of flooding	0.29	0.48	−0.42*	0.04	56.5	5.1	7,183,178
	(0.22)	(0.31)	(0.23)	(0.31)			
Below median income	0.83***	−0.58*	−0.13	−0.61**	49.9	5.6	7,183,178
	(0.25)	(0.31)	(0.17)	(0.25)			
64 or younger at baseline	0.76***	−0.85**	−0.45***	0.10	22.3	3.0	7,987,100
	(0.19)	(0.34)	(0.16)	(0.27)			
75 or older at baseline	0.06	1.29***	−0.45***	−0.02	40.7	8.2	7,987,100
	(0.15)	(0.37)	(0.09)	(0.25)			
Black	1.02***	−0.76*	−0.21	−0.30	60.5	5.0	7,987,100
	(0.32)	(0.42)	(0.19)	(0.26)			
Male	0.42**	0.33*	−0.41**	−0.16	43.0	5.5	7,987,100
	(0.19)	(0.19)	(0.16)	(0.22)			
End-stage renal disease	0.58***	−0.09	−0.40***	−2.10	2.1	19.7	7,987,100
	(0.17)	(1.40)	(0.12)	(1.45)			
Heart disease and stroke	0.69***	−0.03	−0.88***	−0.24	64.8	7.8	5,788,235
	(0.25)	(0.36)	(0.18)	(0.36)			
Respiratory disease	0.37**	1.39**	−1.15***	0.06	12.8	12.0	6,300,486
	(0.19)	(0.59)	(0.19)	(0.58)			
Blood and kidney disease	1.07***	−0.88***	−0.63***	−1.01***	46.2	8.7	5,788,235
	(0.19)	(0.28)	(0.14)	(0.32)			
Cancer	0.52***	0.58	−1.12***	0.02	6.8	12.8	6,300,486
	(0.19)	(1.19)	(0.19)	(0.73)			
Diabetes	0.70***	−0.03	−0.86***	−0.48*	27.4	8.8	5,788,235
	(0.21)	(0.29)	(0.16)	(0.27)			
Musculoskeletal	0.43*	0.92	−1.23***	0.84**	28.6	6.5	5,788,235
	(0.26)	(0.56)	(0.19)	(0.35)			
Alzheimer's/dementia	0.51***	3.09***	−0.92***	−0.85	12.4	19.9	5,256,708
	(0.18)	(1.08)	(0.16)	(0.96)			
Other chronic condition	0.53**	0.18	−1.43***	0.98***	39.9	4.5	6,300,486
	(0.23)	(0.31)	(0.21)	(0.25)			

Notes: Each row reports summary statistics along with short-run (2005) and long-run (2006–2013) mortality effects estimated from the difference-in-differences model given by equation (5) 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 ten control cities in 2004 and who were alive at the beginning of the year of observation. The outcome in each regression is a mortality 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7: Correlation between predicted mortality and leaving New Orleans

	(1)	(2)	(3)	(4)	(5)
Predicted mortality	−0.62***	−0.31***	−0.47***	−0.46***	−0.40***
	(0.13)	(0.09)	(0.11)	(0.11)	(0.10)
Alzheimer's/dementia dropped	No	No	Yes	Yes	Yes
Chronic conditions predictors	None	Grouped	Grouped	Two-way interactions	Individual
Baseline spending predictors	None	Ventiles	Ventiles	Ventiles	Centiles
Dep. var. mean	45.32	45.79	45.88	45.88	45.88
Observations	58,403	26,906	24,560	24,560	24,560

Notes: The table reports shows the relationship between ex ante predicted mortality and leaving New Orleans in 2005–2006 among the 2004 New Orleans cohort. The estimating equation is the same as equation (7), except the dependent variable is a relocation indicator equal to one if a 2004 New Orleans beneficiary was alive but not residing in the New Orleans commuting zone as of March 2006 and is equal to zero if she or he was alive and residing in New Orleans commuting zone as of March 2006. All regressions control for baseline ZIP code fixed effects. Both predicted mortality and the moving decision are expressed in percentage points. Standard errors (in parentheses) are clustered by each beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.8: Predictors of leaving New Orleans and of destination mortality

	(1)	(2)	(3)	(4)	(5)	(6)
	Whether moved			Local mortality rate		
Black	21.70***	21.38***	20.02***	−0.001	−0.011	−0.012
	(6.74)	(6.37)	(6.08)	(0.018)	(0.018)	(0.018)
Male	−3.68***	−3.91***	−3.93***	0.016***	0.007	0.007
	(0.62)	(0.67)	(0.66)	(0.005)	(0.008)	(0.007)
64 and younger	12.33***	12.53***	12.30***	−0.014*	−0.016*	−0.015*
	(1.29)	(1.41)	(1.32)	(0.007)	(0.009)	(0.009)
75 and older	0.00	−1.04	−0.73	−0.003	−0.012	−0.013
	(0.55)	(0.78)	(0.83)	(0.007)	(0.010)	(0.010)
Below median income	4.52***	6.45***	5.37***	0.007	0.012	0.014*
	(0.86)	(1.18)	(0.97)	(0.007)	(0.008)	(0.008)
Katrina flood level, feet	2.54***	2.53***	1.16**	0.000	−0.001	−0.001
	(0.73)	(0.83)	(0.56)	(0.001)	(0.002)	(0.002)
End-stage renal disease	0.76	−2.42	−1.61	0.026*	−0.007	−0.007
	(1.25)	(2.19)	(2.12)	(0.015)	(0.019)	(0.019)
2004 medical spending, thousands		0.06***	0.05***		0.000	0.000
		(0.02)	(0.02)		(0.000)	(0.000)
Alzheimer's/dementia		1.14	1.47		0.044***	0.042***
		(1.95)	(2.12)		(0.016)	(0.016)
Respiratory disease		3.49***	3.34***		−0.009	−0.009
		(1.09)	(1.05)		(0.011)	(0.011)
Heart disease and stroke		1.09	1.00		0.005	0.006
		(0.67)	(0.65)		(0.007)	(0.008)
Blood and kidney disease		−0.62	−0.42		−0.004	−0.004
		(0.81)	(0.82)		(0.008)	(0.008)
Diabetes		0.42	0.26		0.010	0.010
		(0.68)	(0.69)		(0.009)	(0.009)
Musculoskeletal		−0.01	−0.10		−0.011	−0.009
		(0.66)	(0.64)		(0.009)	(0.008)
Cancer		−3.51***	−3.53***		−0.024*	−0.024*
		(1.00)	(1.01)		(0.013)	(0.013)
Other		0.79	1.29*		−0.006	−0.007
		(0.71)	(0.68)		(0.007)	(0.007)
Baseline zip code fixed effects	No	No	Yes	No	No	Yes
Dep. var. mean	44.97	45.35	45.35	5.403	5.399	5.399
p-value of joint F-test	<0.001	<0.001	<0.001	0.008	0.047	0.047
Observations	51,104	23,362	23,362	22,981	10,594	10,594

Notes: The table reports how relocation of survivors (columns (1)–(3)) and local mortality in movers' destinations (columns (4)–(6)) relate to baseline (2004) characteristics of the 2004 New Orleans cohort. Local mortality and relocation are expressed in percentage points. Standard errors (in parentheses) are clustered by each beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.9: 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 county (MDR)	0.48	0.55	0.66	0.04	0.21	1.54
	(1.33)	(1.63)	(2.26)	(2.38)	(2.69)	(3.61)
Predicted mortality (PM)		−0.37***	−0.29		−0.54***	0.68
		(0.07)	(0.87)		(0.12)	(1.76)
PM x MDR			−1.39			−22.56
			(15.70)			(31.77)
Dep. var. mean	20.43	18.50	18.50	43.18	41.08	41.08
Observations	26,467	12,319	12,319	21,300	9,671	9,671
R-squared	0.00	0.00	0.00	0.00	0.00	0.00

Notes: The table reports how returning to New Orleans among the movers sample relates to local mortality in movers' initial (2006) destinations. The dependent variable is specified at the top of each column. Predicted mortality, destination mortality, and the decision to return are expressed in percentage points. Standard errors (in parentheses) are clustered by each beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.10: Migrant mortality over time, by destination mortality (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
Mean death rate x (2006–2007)	1.36***	1.28***	1.26***	1.27**	1.23**
	(0.43)	(0.40)	(0.40)	(0.54)	(0.54)
Mean death rate x (2008–2013)	0.63**	0.64***	0.68***	0.89***	0.97***
	(0.31)	(0.24)	(0.24)	(0.33)	(0.34)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	No	No	No	Gr.	All int.
Dep. var. mean	5.55	5.55	5.54	6.11	6.10
Observations	175,936	175,936	175,821	80,084	80,075
R-squared	0.00	0.04	0.05	0.09	0.09

Notes: The table reports estimates of a version of equation (6), augmented to allow for separate effects in the post-Katrina periods 2006–2007 and 2008–2013. The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Coefficients, standard errors (in parentheses), and the dependent variable mean 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 age (one-year bins), race, and sex combination. C additionally controls for age-race-sex effects by year. Gr. means that indicators for eight groups of chronic conditions are included; All int. means that indicators for each possible interaction of the eight groups of chronic conditions are included. All specifications that include chronic condition controls also include fixed effects for centiles of baseline Medicare spending. Standard errors are clustered by a beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.11: Migrant mortality by cause of death, by destination mortality (New Orleans movers)

	(1) All causes	(2) Cardiovascular	(3) Cancer	(4) Other internal causes	(5) External causes
Mean death rate in 2006 county	1.39***	0.40**	0.08	0.86***	0.021
	(0.32)	(0.17)	(0.12)	(0.22)	(0.035)
Dep. var. mean	5.28	1.97	1.11	1.96	0.133
Observations	75,215	75,215	75,215	75,215	75,215

Notes: The table reports estimates of equation (6) for specific causes of death. The dependent variable is an indicator equal to one if the beneficiary died in a given year from the cause of death specified in the column and is equal to zero if a beneficiary was alive during the entire calendar year or died that year from a different cause. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. All specifications include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. Standard errors are clustered by a beneficiary's 2006 county.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.12: Migrant mortality, by own-group and other-group mortality (New Orleans movers)

	(1)	(2)	(3)
Own-gender mortality	0.48		
	(0.32)		
Other-gender mortality	0.37		
	(0.31)		
Own-race mortality		0.27**	
		(0.13)	
Other-race mortality		0.15	
		(0.12)	
Own-age-group mortality			0.49***
			(0.16)
Other-age-group mortality			0.17
			(0.14)
Dep. var. mean	5.54	5.55	5.54
Observations	175,821	175,770	175,821
R-squared	0.05	0.05	0.05

Notes: The table reports estimates of equation (6) augmented to include both own- and other-group mortality rates. Race groups are black or not black. Age groups are (1) 64 or younger and (2) 65 or older. The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. Controls include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. Standard errors are clustered by a beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.13: Migrant mortality, by more and less local mortality measures (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
2006 ZIP code mortality		0.37***		0.28***	
		(0.11)		(0.10)	
2006 county mortality	0.86***			0.57**	0.57*
	(0.23)			(0.23)	(0.30)
2006 commuting zone mortality			1.03***		0.46
			(0.30)		(0.40)
Dep. var. mean	5.54	5.54	5.54	5.54	5.54
Observations	175,821	175,245	175,821	175,245	175,821
R-squared	0.05	0.05	0.05	0.05	0.05

Notes: The table reports estimates of equation (6) with mortality rates calculated at the ZIP, county, or commuting zone level. The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. Controls include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. Standard errors are clustered by a beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.14: Cumulative migrant mortality by destination mortality (New Orleans movers)

	(1)	(2)	(3)	(4)	(5)	(6)
Mean death rate in 2006 county	4.01***	3.31***	3.52**	3.52***	3.50**	3.58**
	(1.49)	(1.06)	(1.40)	(1.33)	(1.49)	(1.40)
Set of fixed effects	A	B	B	B	B	B
Alzheimer's/dementia dropped	No	No	No	No	Yes	Yes
Chronic conditions controls	No	No	Gr.	All int.	Gr.	All int.
Dep. var. mean	36.91	36.92	39.94	39.77	36.74	36.64
Observations	26,461	26,451	12,313	12,254	11,258	11,227
R-squared	0.01	0.19	0.24	0.29	0.21	0.26

Notes: The table reports estimates of the correlation between movers' probability of dying before the end of 2013 and the average mortality rate in their 2006 destination. The dependent variable is a mortality indicator equal to zero if a beneficiary was alive at the end of 2013 and is equal to one if the beneficiary died prior to that date. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. Controls are as follows: A includes baseline ZIP code fixed effects; B also includes fixed effects for each age (one-year bins), race, and sex combination. Gr. means that indicators for eight groups of chronic conditions are included; All int. means that indicators for each possible interaction of the eight groups of chronic conditions are included. All specifications that include chronic condition controls also include fixed effects for centiles of baseline Medicare spending. Standard errors are clustered by a beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.15: Migrant mortality by destination mortality (2005–2007 New Orleans movers)

	(1)	(2)	(3)	(4)	(5)
Mean death rate in 2007 county	0.73**	0.61***	0.63***	0.71**	0.84***
	(0.31)	(0.22)	(0.22)	(0.30)	(0.31)
Set of fixed effects	A	B	C	C	C
Chronic conditions controls	No	No	No	Gr.	All int.
Dep. var. mean	5.92	5.92	5.91	6.43	6.41
Observations	129,669	129,667	129,581	60,102	60,052
R-squared	0.00	0.04	0.06	0.09	0.10

Notes: The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Coefficients, standard errors (in parentheses), and the dependent variable mean 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 age (one-year bins), race, and sex combination. C additionally controls for age-race-sex effects by year. Gr. means that indicators for eight groups of chronic conditions are included; All int. means that indicators for each possible interaction of the eight groups of chronic conditions are included. All specifications that include chronic condition controls also include fixed effects for centiles of baseline Medicare spending. Standard errors are clustered by a beneficiary's 2007 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.16: Migrant mortality by destination mortality (New Orleans movers)

Specification	Chronic condition controls	Estimate	Obs.
All 2006 movers	Individual	0.961***	80,084
		(0.303)	
All 2007 movers	Individual	0.816***	60,058
		(0.300)	
Distance controls	None	1.000***	175,821
		(0.249)	
Distance controls	Interactions	1.104***	80,075
		(0.321)	
Distance controls	Grouped	1.054***	80,084
		(0.325)	
No Houston/B.R.	None	0.932***	118,929
		(0.252)	
No Houston/B.R.	Interactions	1.124***	54,079
		(0.331)	
No Houston/B.R.	Grouped	1.047***	54,142
		(0.336)	

Notes: The table reports estimates of equation (6). The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Coefficients, standard errors (in parentheses), and the dependent variable mean have been scaled by 100. All specifications include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. All specifications that include chronic condition controls also include fixed effects for centiles of baseline Medicare spending. Specifications with distance controls additionally include indicators for deciles of distance between the centroids of New Orleans and of the destination county. Standard errors are clustered by a beneficiary's destination county in the year of the move. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.17: Summary statistics for destination characteristics

	(1) 10th pctile	(2) Median	(3) 90th pctile
Mortality rate	0.050	0.053	0.058
Adjusted mortality rate	-0.001	0.002	0.008
Income per capita (1,000s)	14.31	20.05	25.40
Poverty rate, 65+ (percent)	7.342	12.19	19.65
Income segregation	0.017	0.091	0.125
Median home value (1,000s)	58.90	87.59	145.3
Upward income mobility (from p25)	-0.778	-0.242	0.178
Upward income mobility (from p75)	-0.191	0.039	0.236
Urban population share	0.469	0.928	0.991
Crime rate (per 1,000)	4.566	8.551	16.64
Social capital index	-1.869	-1.001	0.028
Local gov. spending per capita (1,000s)	1.585	2.343	3.226
Percent exercising	65.62	73.48	78.50
Percent obese	17.48	21.00	27.03
Percent smoking	17.85	20.65	26.72
Medicare spending per beneficiary (1,000s)	10.39	12.20	14.39
Physicians (per 1,000)	0.848	2.975	4.637
Hospital beds (per 1,000)	1.455	3.902	6.304
Hospital quality index	0.723	0.766	0.819
PM 2.5 concentrations ($\mu g/m^3$)	9.020	11.04	12.62
Hot days/year (90° F+)	0	0.239	5.451

Notes: The table shows the 10th, 50th, and 90th percentiles of the given characteristic, as measured in the sample of New Orleans movers in 2006.

Table A.18: Migrant mortality by destination characteristics (New Orleans movers)

	(1)	(2)	(3) Local mortality rate × 100	(4) Predicted mortality rate × 100
	Died indicator × 100			
Mortality rate	0.76***	0.67***	.	-0.05
	(0.22)	(0.18)		(0.15)
	[75137]	[175821]		[75286]
Percent smoking	0.70***	0.42**	0.34***	0.05
	(0.23)	(0.19)	(0.06)	(0.18)
	[69327]	[160990]	[24209]	[69474]
Adjusted mortality rate	0.60***	0.49***	0.53***	-0.18
	(0.18)	(0.15)	(0.03)	(0.16)
	[75137]	[175821]	[26467]	[75286]
Percent obese	0.60***	0.40**	0.35***	-0.12
	(0.20)	(0.16)	(0.06)	(0.14)
	[69327]	[160990]	[24209]	[69474]
Hospital beds per capita	0.22	0.13	0.14***	-0.17**
	(0.22)	(0.13)	(0.03)	(0.08)
	[74808]	[175095]	[26355]	[74957]
Hot days/year (90° F+)	0.10	0.06	-0.03**	-0.06
	(0.08)	(0.06)	(0.01)	(0.07)
	[75113]	[175739]	[26455]	[75262]
Physicians per capita	-0.05	0.02	-0.13**	-0.08
	(0.23)	(0.16)	(0.05)	(0.12)
	[74808]	[175095]	[26355]	[74957]
Hospital quality index	-0.08	-0.05	-0.01	0.11
	(0.19)	(0.16)	(0.04)	(0.14)
	[66817]	[154840]	[23286]	[66960]
Medicare spending per beneficiary	-0.21	-0.04	0.09	-0.15
	(0.19)	(0.15)	(0.09)	(0.24)
	[75137]	[175821]	[26467]	[75286]
Percent exercising	-0.56**	-0.32	-0.48***	0.01
	(0.23)	(0.20)	(0.04)	(0.16)
	[69217]	[160775]	[24178]	[69365]
PM 2.5 concentrations	-0.65***	-0.31**	-0.05	-0.44**
	(0.17)	(0.14)	(0.06)	(0.19)
	[69333]	[161096]	[24243]	[69489]

Notes: Columns (1) and (2) report estimates of equation (6) with the independent variable listed in each row. The dependent variable in columns (1) and (2) is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Controls in columns (1) and (2) include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. Column (1) additionally controls for indicators for each possible combination of eight chronic condition groups and indicators for centiles of baseline Medicare spending. Column (3) shows the correlation between the local characteristic specified in the row and the mean death rate in movers' 2006 county, with one observation per mover. Column (4) shows the correlation between the local characteristic specified in the row and the movers' predicted mortality, using a model of mortality that includes demographic characteristics, all two-way interactions of eight baseline chronic condition group indicators, and ventiles of baseline spending. Individuals with Alzheimer's/dementia at baseline are excluded from columns (1) and (4). Standard errors (in parentheses) are clustered by a beneficiary's 2006 county. The number of observations is in square brackets. Coefficients and standard errors in columns have been scaled by 100 and by the difference between the 90th and 10th percentiles of the relevant local characteristic. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.19: Migrant mortality by destination characteristics (New Orleans movers)

	(1)	(2)	(3) Local mortality rate × 100	(4) Predicted mortality rate × 100
	Died indicator × 100			
Social capital index	0.44**	0.29**	0.16***	0.26
	(0.18)	(0.13)	(0.05)	(0.19)
	[75116]	[175774]	[26460]	[75265]
Crime rate	0.31	0.29	0.32***	0.24
	(0.27)	(0.22)	(0.05)	(0.17)
	[72813]	[170173]	[25598]	[72954]
Poverty rate, 65+	0.28	0.14	0.40***	0.03
	(0.24)	(0.18)	(0.04)	(0.13)
	[75137]	[175821]	[26467]	[75286]
Upward income mobility (from p75)	0.05	-0.04	0.20***	-0.33*
	(0.19)	(0.15)	(0.06)	(0.17)
	[75005]	[175507]	[26415]	[75154]
Upward income mobility (from p25)	-0.01	0.03	-0.22***	0.04
	(0.26)	(0.18)	(0.05)	(0.20)
	[75005]	[175507]	[26415]	[75154]
Income segregation	-0.25	-0.00	-0.17**	-0.35*
	(0.22)	(0.18)	(0.08)	(0.17)
	[75137]	[175821]	[26467]	[75286]
Local gov. spending per capita	-0.29**	-0.12	-0.18***	0.04
	(0.14)	(0.11)	(0.05)	(0.12)
	[75137]	[175821]	[26467]	[75286]
Urban population share	-0.46**	-0.15	-0.27***	-0.22
	(0.19)	(0.15)	(0.03)	(0.16)
	[75137]	[175821]	[26467]	[75286]
Median home value	-0.49***	-0.35***	-0.36***	0.40***
	(0.15)	(0.13)	(0.04)	(0.13)
	[75137]	[175821]	[26467]	[75286]
Income per capita	-0.60***	-0.41**	-0.45***	0.07
	(0.20)	(0.16)	(0.04)	(0.14)
	[75137]	[175821]	[26467]	[75286]

Notes: Columns (1) and (2) report estimates of equation (6) with the independent variable listed in each row. The dependent variable in columns (1) and (2) is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Controls in columns (1) and (2) include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. Column (1) additionally controls for indicators for each possible combination of eight chronic condition groups and indicators for centiles of baseline Medicare spending. Column (3) shows the correlation between the local characteristic specified in the row and the mean death rate in movers' 2006 county, with one observation per mover. Column (4) shows the correlation between the local characteristic specified in the row and the movers' predicted mortality, using a model of mortality that includes demographic characteristics, all two-way interactions of eight baseline chronic condition group indicators, and ventiles of baseline spending. Individuals with Alzheimer's/dementia at baseline are excluded from columns (1) and (4). Standard errors (in parentheses) are clustered by a beneficiary's 2006 county. The number of observations is in square brackets. Coefficients and standard errors in columns have been scaled by 100 and by the difference between the 90th and 10th percentiles of the relevant local characteristic. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.20: Migrant spending, by destination spending (New Orleans fee-for-service movers)

	(1)	(2)	(3)	(4)	(5)	(6)
Average medical spending in 2006 county	0.93***	0.87***	0.87***	0.76***	0.66***	0.61***
	(0.11)	(0.10)	(0.10)	(0.13)	(0.13)	(0.11)
Set of fixed effects	A	B	C	C	C	C
Alzheimer's/dementia dropped	No	No	No	No	Yes	Yes
Chronic conditions controls	No	No	No	Gr.	Gr.	All int.
Dep. var. mean	14,616	14,616	14,620	15,969	15,357	15,353
Observations	101,675	101,675	101,544	67,694	63,184	63,178
R-squared	0.01	0.03	0.04	0.16	0.17	0.20

Notes: The table reports estimates of equation (6) with local spending as the independent variable. The dependent variable is the total spending by a beneficiary in a calendar year. Sets of fixed effects are as follows: A includes baseline ZIP code and year fixed effects; B also includes fixed effects for each age (one-year bins), race, and sex combination. C additionally controls for age-race-sex effects by year. Gr. means that indicators for eight groups of chronic conditions are included; All int. means that indicators for each possible interaction of the eight groups of chronic conditions are included. All specifications that include chronic condition controls also include fixed effects for centiles of baseline Medicare spending. Standard errors (in parentheses) are clustered by a beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.21: Migrant mortality by multiple destination characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mortality rate	0.57*	0.59**	0.75**	0.57**	0.59**	0.66**	-0.39
	(0.31)	(0.30)	(0.37)	(0.28)	(0.29)	(0.26)	
Percent smoking	0.48						0.01
	(0.30)						
Percent obese		0.32					-0.13
		(0.25)					
Percent exercising			-0.07				0.15
			(0.33)				
Median home value				-0.25			0.06
				(0.18)			
Median household income, 65+					-0.24		0.14
					(0.25)		
Urban population share						-0.19	-0.16
						(0.21)	
Pct. of 2006–2013 decline explained	60.78	76.29	85.23	66.24	74.13	63.44	
Dep. var. mean	5.47	5.47	5.47	5.48	5.48	5.48	
Observations	69,332	69,332	69,222	75,142	75,142	75,142	

Notes: Columns (1)–(7) report estimates of equation (6). The dependent variable is a mortality indicator equal to zero if a beneficiary was alive during the entire calendar year and is equal to one if the beneficiary died in a given year. Controls include fixed effects for baseline ZIP code and all combinations of year, age (one-year bins), race, and sex. Coefficients and standard errors (in parentheses) have been scaled by 100 and by the difference between the 90th and 10th percentiles of the relevant local characteristic. The dependent variable mean has been scaled by 100. Column (8) reports the mean change in the given local characteristic for New Orleans beneficiaries who survived until the beginning of 2006, as a share of the difference between the 90th and 10th percentiles of the relevant local characteristic. Standard errors are clustered by each beneficiary's 2006 county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.