Long-Run Health Dynamics in the Wake of Disaster: Evidence from Hurricane Katrina*

Tatyana Deryugina
University of Illinois at Urbana-Champaign and NBER

David Molitor
University of Illinois at Urbana-Champaign and NBER

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Abstract

Natural disasters devastate infrastructure and displace populations, but little is known about their long-term consequences for victims' health. We follow Medicare cohorts over time to estimate the mortality and dislocation effects of Hurricane Katrina among the elderly and long-term disabled living in New Orleans prior to the storm. We find greatly elevated mortality in the week Hurricane Katrina made landfall, but this immediate effect quickly dissipates and becomes a sustained mortality *reduction*: eight years after the storm, Katrina victims are 1.75 percentage points more likely to be alive. Those initially residing in flooded neighborhoods experience larger cumulative mortality reductions along with a much higher rate of displacement. Among movers, each percentage point increase in the destination's mortality rate predicts a 0.52-0.69 percentage point increase in the migrant's own subsequent mortality rate. Because New Orleans began as one of the highest mortality areas in the country, these results suggest that the relocation of New Orleans residents to areas with better health outcomes may have driven long-run mortality improvements. By contrast, local average Medicare spending is not predictive of movers' subsequent mortality.

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1 Introduction

Natural disasters are increasing in frequency and severity. They are forecast to continue doing so in the foreseeable future as wealth and the number of people living in disaster-prone areas rise and the effects of climate change continue to manifest themselves. While the economic consequences of natural disasters have been studied extensively (e.g., Skidmore and Toya, 2002; Cavallo et al., 2013; Hsiang and Jina, 2014; Deryugina, 2017), little is known about their long-run *health* consequences. Given the large value of health and longevity (Murphy and Topel, 2006), the omission of such impacts could significantly bias conclusions about disasters' welfare effects. Thus, it is critical to understand the factors that shape the longer-run trajectories of mortality and public health in the wake of natural disasters.

We undertake a systematic analysis of the short- and long-run mortality impacts of Hurricane Katrina, which devastated the Gulf Coast in 2005, on one of the most vulnerable subsets of the population: the elderly and long-term disabled who qualify for Medicare. Accounts of the immediate impacts of Hurricane Katrina estimate that about half of the nearly 2,000 people killed by the storm were over the age of 75 (Brunkard, Namulanda and Ratard, 2008). About a fifth of the individuals displaced by the Hurricane were elderly on Medicare (Super and Biles, 2005). The displacement created by Hurricane Katrina could have further worsened the health of the elderly by negatively affecting their living conditions, disrupting the continuity of their care, forcing them to relocate, and (possibly) by creating a generally stressful environment. Anecdotal evidence suggests that the elderly struggled during and after Katrina (e.g., Spiegel, 2006), but rigorous empirical analyses of disasters' long-run consequences are virtually non-existent.

We use administrative data for the entire Medicare population to identify cohorts of elderly and long-term disabled who were living in New Orleans prior to Hurricane Katrina. We identify a control group of individuals who are similar to those residing in New Orleans but who were not directly or indirectly affected by the Hurricane. We then track everyone's mobility and mortality for up to six years before the storm (1999–2004) and for nine years after (2005–2013), regardless of where they move.

We find that the Hurricane raised the 2005 mortality rate of the 2004 New Orleans elderly and disabled by over half a percentage point, which is almost ten percent of that cohort's average annual death rate. Excess mortality in the week of the storm explains the majority of the estimated annual increase. Mortality one and two weeks after the storm is also elevated, but the point estimates in the remaining 62 weeks we consider in our short-run analysis are largely insignificant or significantly negative. This is perhaps surprising, as this population is thought to be particularly vulnerable to disruption in general and relocation in particular. Moreover, parts of New Orleans remained uninhabitable for months, and all major New Orleans hospitals were closed for at least 28 days after Katrina, almost certainly reducing access to health care for people who remained in the city.[1] Because we cannot separate the effect of the Hurricane from the aid response to it, it should be noted that post-Katrina mortality increases could have been substantially higher without such aid.

Consistent with Deryugina, Kawano and Levitt (2018), who study the long-run economic effects of Hurricane Katrina, we estimate that the Hurricane led to a massive and lasting dislocation of the elderly: those living in New Orleans in 2004 were forty percentage points more likely to leave their city of residence than the control group, and over half of those still alive had not returned as of 2013.

We then estimate the effect of Hurricane Katrina on mortality rates in 2006 and beyond and find that the Hurricane led to long-run *reductions* in cumulative mortality: inclusive of the initial mortality increase, a Medicare beneficiary living in New Orleans prior to Hurricane Katrina was 1.75 percentage points more likely to be alive at the end of 2013 than someone from the control group. We show that this finding is not explained by differential pre-trends or mortality displacement and is robust to using the rest of the U.S. Medicare population as a control group.

Next, we consider two possible (and not mutually exclusive) explanations for the long-

[1]As we discuss in the next section, smaller inpatient facilities and several hospitals in nearby cities remained open.

run mortality decline: relocation of the elderly to areas with better health outcomes and improvements in the New Orleans health care system. To that end, we examine the correlation between the subsequent mortality of New Orleans residents who moved between 2004 and 2006 (i.e., left New Orleans after the Hurricane for a non-trivial amount of time) and the underlying mortality rate in the area they moved to. We find a large and highly significant positive correlation between the two. The relationship is highly stable even after we control for a host of fixed effects and pre-existing chronic conditions, suggesting that it is not driven by selection. Moreover, this correlation already exists in 2006, implying that it is not arising because of longer-run lifestyle changes brought about by the move. By contrast, we find no correlation between local average Medicare spending and subsequent mortality of the movers.

Two empirical patterns suggest that the long-run mortality declines were unrelated to any improvements in the health care system in New Orleans, although this evidence is necessarily more circumstantial and limited. First, New Orleans hospital capacity fell sharply in the aftermath of the Hurricane and its recovery in most of the years in our sample did not keep pace with the rebound in population. Second, the New Orleans health care infrastructure was rebuilt gradually. However, when we consider the 2006–2013 mortality of those who remain in the city, we see a flat pattern rather than gradual mortality improvements. Thus, any mortality improvements for those who stayed would either have to begin in 2006 or be exactly offset by stayers' counterfactual mortality trend, both of which seem unlikely.

To our knowledge, we provide the first quasi-experimental estimates of the long-run mortality effects of a natural disaster. The current research on the short- and long-run health impacts of natural disasters is largely limited to looking at birth outcomes and infant health (e.g., Torche, 2011; Currie and Rossin-Slater, 2013; Currie and Schwandt, 2014) or is conducted by surveying a subset of the victims, almost always without a control group. In addition, survey samples are often limited to victims who returned to the area, and their pre-disaster outcomes are not observed.[2] More generally, two data challenges have hampered analyses of the long-run health effects of natural disasters. First, non-random displacement and migration mean that place-based measures of health (such as county-level mortality) provide a biased measure of the health impacts on the *cohort* exposed to the disaster. Second, health measures acquired through surveys are challenged by small samples, non-random attrition, and/or lack of a comparable control group. By contrast, our data track the mortality and location of every Medicare-eligible individual.

Our findings add to a growing literature that uses migration to identify how local conditions affect individual outcomes such as earnings, education, and health care spending (e.g., Chetty, Hendren and Katz, 2016; Finkelstein, Gentzkow and Williams, 2016; Nakamura, Sigurdsson and Steinsson, 2016; Chetty and Hendren, 2017). We contribute to this literature by studying how the long-run mortality outcomes of those displaced by Hurricane Katrina depend on the local mortality rates of the destination region. We find that each percentage point increase in the destination's mortality rate predicts a 0.52-0.69 percentage point increase in the migrant's own subsequent mortality rate. This highly robust finding suggests that local public health conditions are an important determinant of individual health outcomes.

A key question in public health has been whether higher-spending regions generate better health outcomes than lower-spending regions. Although there is a great deal of geographic variation in health care spending, it has little correlation with health outcomes (Fisher et al., 2003*a*,*b*; Baicker and Chandra, 2004; Sirovich et al., 2006; Skinner, 2011). However, regional spending may partly reflect differences in the baseline health of the resident population. Doyle (2011) addresses this limitation by examining hospital outcomes for individuals who experience health emergencies while visiting Florida, and Doyle et al. (2015) use quasi-random assignment of patients to hospitals based on the ambulance provider. Both studies find that patients have better outcomes when treated at higher-spending hospitals. While

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

these analyses focus on the returns to being hospitalized in a high-spending region, our analysis speaks to the returns to *living* in a high-spending region. This distinction is important because higher-quality health systems could reduce the need for hospitalization. In fact, we find no relationship between a mover's subsequent mortality and local health care spending, suggesting that average returns to living in a high-spending region may be low.

The rest of this paper is organized as follows. Section 2 provides an overview of Hurricane Katrina and its known impacts on economic outcomes, as well as of natural disasters and elderly health. In Section 3, we describe our data and how we construct the control group. Section 4 outlines our empirical strategy and Section 5 presents the results. Section 6 concludes.

2 Background

2.1 Overview of Hurricane Katrina

Hurricane Katrina struck New Orleans on August 29th, 2005 as a Category 3 hurricane with sustained winds of 140 miles per hour (Federal Emergency Management Agency, 2015).[3] Even prior to landfall, officials realized that there was a danger of the hurricane overtopping one or more levees protecting the city. Those fears proved well-founded: there were numerous levee and flood wall failures in the aftermath of Hurricane Katrina, resulting in widespread flooding in New Orleans and the nearby St. Bernard Parish. As a result of both the direct impact of the hurricane and the levee failures, hundreds of thousands of homes were damaged or destroyed. Parts of the city remained uninhabitable for months after the storm; rebuilding in some areas took years. It is uncertain exactly how much direct damage the Hurricane caused, but estimates range from $80 to $150 billion.

Despite a mandatory evacuation order and an estimated evacuation rate of 80 percent,

[3] For additional details on Hurricane Katrina, see Deryugina, Kawano and Levitt (2018) and the sources cited therein.

the storm caused an enormous number of fatalities relative to other disasters in the modern United States. Hurricane Katrina's official death toll was 1,833, making it the deadliest natural disaster in the United States since 1928. Moreover, hundreds of people were still listed as missing nearly a year later (Graumann et al., 2005). It was estimated that half of those killed by the immediate impact of the storm were over the age of 75 (Brunkard, Namulanda and Ratard, 2008), and up to 200,000 of those displaced were elderly individuals on Medicare (Super and Biles, 2005). Anecdotally, the health of impacted elderly worsened, resulting in elevated death rates for at least the first few months after Hurricane Katrina (Spiegel, 2006). However, rigorous empirical evidence on whether this was actually the case is lacking.

Figure 1 shows the extent of flooding in New Orleans in the immediate aftermath of Hurricane Katrina. Green and blue hues correspond to flood levels of at least 6 feet, enough to completely cover most individuals. As the map shows, such flooding was not uncommon. Yellow tones correspond to 4-6 feet of water and likewise can be seen in many parts of the city. As the flood waters receded, they left behind uninhabitable homes and in some cases created the risk of harmful mold growth. Officially, individuals in the vast majority of New Orleans zip codes were prohibited from returning to their homes until at least December 9, 2005. On that date, FEMA allowed residents of 10 of the 17 zip codes to return to their homes and stay there; residents in the remaining 7 zip codes could return to their homes during the day but could not spend the night there (see Federal Emergency Management Agency, 2005, for details). Storm victims who could not find suitable living arrangements were given funds to pay for a hotel or apartment or the opportunity to live in specially provided trailers.

The aid response to Hurricane Katrina was considerable. Excluding flood insurance payments and loans, the state of Louisiana received about $50 billion from the federal government.[4] However, the majority of these funds were earmarked for rebuilding infrastructure,

[4]See the Online Appendix to Deryugina, Kawano and Levitt (2018) for sources and for more detailed information about various aid components.

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, repair, rebuilding, and other disaster-related expenses. Between the years of 2006 and 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. Deryugina, Kawano and Levitt (2018) calculate that a reasonable upper bound on the aid spending for the city of New Orleans is \$125,000 per capita, of which about \$17,000 consisted of direct transfers to individuals.

2.2 Natural disasters and elderly health

The elderly and long-term disabled have, on average, more chronic conditions and lower physical and mental capabilities. Thus, they are generally thought to fare poorly in the aftermath of a natural disaster, 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, Centers for Disease Control and Prevention (2004) found that older adults experienced a disruption 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% had at least one chronic condition (Brodie et al., 2006). In the same survey, a similar number reported needing prescription medication.

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). Specifically, post-traumatic stress disorder (PTSD) has been found to be a fairly common occurrence among disaster victims (Galea, Nandi and Vlahov, 2005; Neria, Nandi and Galea, 2008). PTSD is associated with severe anxiety and depression, significantly affecting one's quality of life. In extreme cases, PTSD can increase the risk of suicide.

Finally, the elderly are thought to be particularly prone to "relocation stress syndrome", where individuals' physical and mental health suffer as a result of being transferred from one environment to another (Barnhouse, Brugler and Harkulich, 1992). Given the scale and suddenness of the dislocation created by Hurricane Katrina, the manifestations and consequences of relocation stress syndrome may be non-trivial. More mechanically, the relocation may have made it more difficult for patients to get care they need.

Several studies have tried to quantify the health consequences of Hurricane Katrina specifically, almost exclusively focusing on the short-run (e.g. Brodie et al., 2006; Kessler et al., 2008; Sastry and VanLandingham, 2009; Sastry and Gregory, 2013).[5] Virtually all the studies known to us find at least a short-run deterioration in mental and physical health, but each lacks a control group from outside of New Orleans and thus cannot account for any secular trends. Many also lack pre-Katrina measures of health, except as recalled by study participants.

The economic effects of Hurricane Katrina have also been studied extensively.[6] A surprising conclusion reached by both Groen, Kutzbach and Polivka (2016) and Deryugina, Kawano and Levitt (2018) is that the hurricane led to a long-run *increase* in victims' earnings. Similarly, (Sacerdote, 2012) finds a long-run increase in test scores among students displaced by Katrina. Of course, whether similar long-run gains are possible for health outcomes is ultimately an empirical question.

[5]In the only longer-run study we are aware of, 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.

[6]See, for example, Vigdor (2007); Groen and Polivka (2008*b*,*a*); Gregory (2011); Groen, Kutzbach and Polivka (2016); Deryugina, Kawano and Levitt (2018). For a review of the literature studying the economic effects of natural disasters more generally, see Shabnam (2014).

2.3 New Orleans health care system

Hurricane Katrina devastated the health care infrastructure in New Orleans.[7] Many health care professionals left the city in the aftermath of the storm. In 2005, there were nine large hospitals in the city of New Orleans. All were closed in the immediate aftermath of the storm due to damage and/or flooding. One of them (Touro Infirmary) reopened 28 days after. One more hospital (Tulane Medical Center) reopened in early 2006 and two more (Memorial Medical Center and University Hospital/Interim LSU Hospital) reopened in late 2006. The remaining large hospitals were closed for years or never reopened. In 2015, a new hospital (University Medical Center) was opened, replacing University Hospital and Charity Hospital. Although smaller inpatient facilities and several hospitals in nearby cities continued operating, the closure of so many hospitals undoubtedly reduced health care access for many individuals. In addition, many non-inpatient health care facilities were likely adversely affected, although precise data on them is not available.

DeSalvo, Sachs and Hamm (2008) report that, by 2008, the health care infrastructure in the New Orleans area had recovered somewhat, but problems persisted. The city had returned to 70 percent of its pre-Katrina population and was continuing to grow, increasing the demands on the medical system. At the same time, many hospitals faced staffing and financial problems, resulting in long wait times. Moreover, the permanent closure of Charity Hospital, which served a large number of New Orleans uninsured, resulted in many uninsured seeking care in emergency rooms, placing further strain on hospital resources.

At the same time, DeSalvo, Sachs and Hamm (2008) point out that the New Orleans area had a higher-than-average number of beds per capita prior to Katrina, and that its post-Katrina ratio was closer to the national average. Similarly, although the number of physicians dropped significantly post-Katrina, so did the population, resulting in New Orleans more physicians per capita than the national average. Moreover, community-based primary care clinics funded by various sources sprung up after the hurricane, potentially filling in the void

[7]See Rowland (2007) for additional details.

left by the closure and shrinkage of hospitals.

To assess the state of the health care system in New Orleans quantitatively, we obtained annual hospital-level data on the total number of hospital beds and the number of hospital employees from Centers for Medicare and Medicaid Services. Hospitals and other inpatient facilities that receive Medicare reimbursements are required to provide this information annually.[8] Figure 2 shows the total number of hospital beds and hospital employees in New Orleans in 1997–2010 (red lines). For comparison, we also show the total number of hospital beds averaged across the control cities we utilize later for our individual-level analysis (black lines) and the population of New Orleans (dashed blue lines, right axis).[9]

Figure 2 makes it clear that Hurricane Katrina had a large and immediate effect on hospital capacity in New Orleans. Between 2004 and 2005, the number of beds fell by over a thousand, a drop of about 30 percent. It continued dropping in 2006 and 2007, finally reaching a low point of slightly more than 1,000 beds, about 70 percent lower than the number of beds in 2004. The number of beds rose only slightly in 2008–2010, suggesting that the availability of inpatient care was permanently diminished in New Orleans.

The number of hospital employees in New Orleans follows a similar trajectory: there is approximately a 30 percent drop between 2004 and 2005, followed by another large drop between 2005 and 2006. In 2006, New Orleans inpatient facilities employed 5,076 people, about 33 percent of the 15,249 who worked there in 2004. Meanwhile, the number of hospital beds and employees in the control cities was very stable, demonstrating that the patterns we see are largely due to Hurricane Katrina rather than any national shocks.

At the same time, the bottom two subfigures of Figure 2 show a precipitous decline in health care utilization in New Orleans. Between 2004–2005, the number of hospital discharges in New Orleans nearly halved, and in 2006 the number of discharges was about two thirds lower than in 2004. We observe a similar pattern for the total number of inpatient Medicare days and other hospitalization metrics not shown here.

[8] At the end of 2004, there were 22 such facilities in the city of New Orleans.

[9] The annual population of New Orleans is from the Bureau of Economic Analysis.

The population of New Orleans also fell sharply following Hurricane Katrina, from 494,000 in July of 2005 to 230,000 in July of 2006, likely decreasing the overall demand for health care. Thus, some of the decline in utilization, the number of beds, and the number of employees may be simply due to people leaving the area. However, at least part of the decrease in utilization may be due to the reduced availability of hospital resources: with the exception of hospital employees, the rate at which the population recovers exceeds the recovery rate of the hospital metrics in Figure 2, suggesting that the New Orleans health care system could have experienced increasing strain in the post-Katrina years. If the destruction of hospital capacity reduced health care availability to the point where it significantly impaired health, we may observe that in our analysis of long-run mortality.

We also examined the hospital data for parishes that border New Orleans (Jefferson, St. Bernard, and St. Tammany) to see if there were any spillovers on their health care system (not shown). The number of hospital beds in the nearby parishes did not increase, suggesting that health care systems in these locations did not increase capacity to offset the loss in New Orleans. The number of hospital employees in neighboring parishes was also stable.

Thus, there are both theoretical reasons to expect that Katrina led to persistent health declines and some empirical work suggesting deterioration in victims' mental and physical health in the short run. However, it is also possible that recovery was quick as massive resources were directed at the people affected. Moreover, by being forced to relocate to other areas, the elderly may have found themselves with better care than before.

3 Data and control group

3.1 Data

The primary data for our analysis are Medicare claims and administrative records. As of 2010, over 97 percent of the US population 65 and older was enrolled in Medicare, making these data the most comprehensive record of elderly health in the US. Medicare data have two other features essential for studying health dynamics. First, Medicare records the zip code of residence for each beneficiary over time, allowing us to identify individuals living in a particular place at a certain time (e.g., New Orleans residents prior to Hurricane Katrina) and to track those individuals over time without attrition even if they move. Second, Medicare records each individual's exact date of death, which is subject to verification by the Social Security Administration (SSA) and is thus considered to be very reliable.

The majority of our analysis uses three segments from the 1999-2013 annual Beneficiary Summary Files (BSF). The BSF are generated from claims data by Medicare specifically for researchers and, among other things, contain patient identifiers as well as demographic information obtained from the Social Security Administration (SSA) record system, including 9-digit zip code of residence, race, sex, date of birth, date of death, and an end-stage renal disease indicator. The beneficiary address on file is from the SSA and corresponds to the address where all official SSA communication and any Social Security benefits are mailed. Although Medicare continuously updates beneficiaries' addresses for its own records (based on updates submitted to SSA by the beneficiary), they only provide an annual snapshot of these to researchers. In 1999 and in 2006–2013, the zip code of residence is based on where the beneficiary was living at the end of the year. In the other years, the zip code corresponds to the address as of March 31st of the following year. If a beneficiary dies during a particular year, the zip code of residence corresponds to the last recorded address when he or she was alive.

The Cost and Utilization segment of the BSF reports total patient spending, among other things. The spending measure includes any payments made by Medigap, an optional insurance plan which covers some or all of Medicare's deductible and coinsurance, and Medicaid, which does the same for poor elderly. However, we do not observe total spending for individuals enrolled in "traditional Medicare" (less than 20 percent of our sample). In these cases, Medicare makes a fixed payment to private providers who then handle any claims these individuals have. By contrast, in "fee-for-service" Medicare, payments are made directly to providers on a claim-by-claim basis.

The Chronic Conditions segment infers conditions from medical claims and provides indicators for 27 common conditions of interest, including heart attack, stroke, hypertension, diabetes, cancer, Alzheimer's disease, and depression. We group these into eight broad categories: heart disease and stroke; respiratory disease; blood and kidney disease; cancer; diabetes; musculoskeletal diseases; dementia (including Alzheimer's disease); and "other" (cataracts, glaucoma, hypothyrodism, benign prostatic hyperplasia, and depression). Because the chronic condition indicators are based on claims, they are only available for individuals continuously enrolled in "fee-for-service" Medicare for a long enough time to meet a condition-specific look-back window (usually two years). Chronic condition indicators will be missing for elderly who do not meet this claims availability requirement. For more details on how chronic conditions are determined by Medicare and classified by us, see the Online Appendix.

For heterogeneity analysis, we also utilize Katrina flood depth data from FEMA, aggregating it to the 9-digit zip code level. We also use block-group-level income data from the 2000 Census, interpolating it to the 9-digit zip code.

Each zip code in our data is assigned to a "Hospital Service Area" (HSA). HSAs delineate 3,436 local health care markets in the United States. The New Orleans HSA is nearly identical to the city of New Orleans, the only difference being that the former includes some sparsely populated areas located to the south of the city. Hospital Service Areas are further aggregated into 306 Hospital Referral Regions (HRR), which can be thought of as distinct *regional* health care markets. Throughout, we define our sample based on beneficiaries' HSAs.

3.2 Control group and basic data tabulations

Although the exogeneity of Hurricane Katrina with respect to any other unobservable factor is certain, there are some barriers to estimating its true effect on short- and long-run mortality. Importantly, we must identify a credible counterfactual for the storm victims. New Orleans is a unique city in many ways. It is poorer and blacker than most other metropolitan areas in the United States and it was in relative decline even prior to Hurricane Katrina. While a city's economic conditions may not be as relevant for the elderly, many of whom are retired, New Orleans' health system may also systematically differ from other large cities.

We follow Deryugina, Kawano and Levitt (2018) and restrict our control group to the ten HSAs corresponding to the control cities used in that study (see Figure 3). As we show later, these cities appear to be excellent counterfactuals for the elderly mortality rate in New Orleans.

We consider six cohorts of elderly individuals who were residing in New Orleans or these ten cities, defined based on the year in which they were first present in the Medicare data and living in one of these cities. As we explain in more detail below, we use the cohort of beneficiaries residing in New Orleans or control cities in 1999 to assess any pre-trends in mortality that may be present, while the 2004 New Orleans cohort is the most relevant one for assessing the impact of the hurricane. The 2000-2003 cohorts are used as robustness checks, with most of the results relegated to an Online Appendix.

One of the dimensions of heterogeneity that we are interested in is leaving New Orleans. We consider both annual moves (as an outcome) and individuals who are movers. To capture movers who left the city for an extended period of time, we define "movers" based on beneficiaries' 2006 HSA.[10] This variable is defined only for individuals who survived all of 2005.

Our data also include Medicare-eligible disabled individuals under the age of 65. People with a qualifying disability are automatically enrolled in Medicare after 24 months, unless they are diagnosed with end-stage renal disease, in which case they are immediately eligible. These individuals have fairly high death rates and are potentially even more vulnerable than a typical 65 year old, even though they are younger. For example, looking at our 1999 cohort

[10] Recall that Katrina made landfall in August of 2005. Using the 2006 HSA instead of the 2005 HSA thus ignores relocation that only lasted several months.

in years 1999-2004, those who are 64 at baseline (and thus qualify for Medicare because of a disability) have a 5.2 percent annual mortality rate, while those who are aged 65 have a 2.2 percent annual mortality rate, which is more than 50 percent lower. Similarly, the 64 year-olds in our 2004 cohort have a 4 percent chance of dying in 2004, while those who are 65 and older have only a 1.3 percent mortality rate that year.

Table 1 shows the summary statistics for the 2004 New Orleans/control cities cohort, starting with time-invariant variables (Panel A). We have almost 1.3 million individuals in our sample, of whom over 80 thousand live in New Orleans. About 35 percent of our sample is black, 42 percent is male, and the average age in 2004 is 71. 82 percent of the individuals are 65 and older, implying that about 18 percent are younger individuals who qualify for Medicare because of a disability.

On average, 6.9 percent of the whole sample moves between 2004 and 2006 (the smaller number of observations reflects deaths that happen in 2004 and 2005). However, in the New Orleans sample, over 44 percent of those surviving until 2006 left the city, reflecting the massive displacement created by Katrina. The average New Orleans beneficiary experienced almost 1.8 feet of flooding during the storm, with a standard deviation of 2.6 feet.

Panel B of Table 1 shows summary statistics for the main panel variables we utilize in our analysis. About 5.5 percent of individuals die each year, and 80 percent are enrolled in fee-for-service Medicare (both of these variables are missing for beneficiaries who were not alive in that year). Finally, the annual Medicare spending for the average fee-for-service beneficiary was $11,912 with a standard deviation of $25,579 (we do not observe spending for beneficiaries enrolled in traditional Medicare).

Next, we show some general mortality trends in our data. For ease of viewing, we group HSAs outside of our control group into their respective Hospital Referral Regions (HRRs). Figure 4 plots the raw annual death rates for the 1999 New Orleans HRR Medicare cohort (red), the 1999 cohorts in the ten control cities' HSAs (blue), and the 1999 cohorts in other HRRs (light grey). Prior to Hurricane Katrina, the New Orleans HRR is an outlier, with higher mortality rates than almost all the other HRRs. The ten control HSAs we select also have high mortality rates, largely falling in the top half of the distribution. In the year of Hurricane Katrina, New Orleans had literally the highest mortality rate in the nation. However, something remarkable happens in the second half of our sample: in 2006, New Orleans falls into the middle of the mortality rate distribution and remains there through 2013, the latest year for which we have data. As we discuss below, these decreases are so large that they cannot be explained by "mortality displacement" or "harvesting", where Katrina killed individuals who would have died soon even in the absence of the Hurricane, leading to a mechanical decline in future mortality rates. Patterns for later (2000-2004) cohorts look similar.

While Figure 4 strongly suggests that New Orleans cohorts experienced a long-run decline in mortality following Hurricane Katrina, more rigorous tests are necessary. Although it appears that New Orleans is following a similar trend as the rest of the United States prior to the Hurricane, an explicit evaluation of this is needed. In addition, the raw death rates do not account for differences in demographic characteristics among the areas (i.e., race and age), which could affect the evolution of mortality rates over time.

4 Empirical Framework

4.1 Short-run effects of Hurricane Katrina on mortality

We first estimate the short-run effect of Hurricane Katrina on mortality of the New Orleans Medicare population, using a difference-in-differences event study analysis. We begin by identifying the cohort of all individuals who were alive and eligible for Medicare in a base year prior to the hurricane (e.g. 2004) and who initially resided in New Orleans (the New Orleans cohort) or one of the control cities (control cities cohorts). We then construct a panel data set for these cohorts with observations for each individual i and week t over the 100-week period beginning 35 weeks prior to and ending 64 weeks after Hurricane Katrina.

Using these data, we estimate the following difference-in-differences regression:

$$Died_{it} = \sum_{\substack{\tau=-35, \\ \tau\neq-1}}^{64} \beta_t \mathbf{1}(t=\tau) \times NOLA_i + [week\ FE] + [base\ ZIP5\ FE] + \theta X_{it} + \varepsilon_{it}, \quad (1)$$

where the outcome, $Died_{it}$, is equal to zero if individual i survived through week t, equals one if they died that week, and is missing if the individual died prior to week t. We define a "treatment" indicator $NOLA_i$ to equal one if individual i lived in New Orleans at baseline and to equal zero otherwise. Event week $t = 0$ refers to the week of Monday, August 29, 2005, when Hurricane Katrina struck New Orleans. Fixed effects for the 5-digit ZIP code of an individual's residence in the base year capture baseline geographic differences in mortality rates, while event week fixed effects capture how mortality evolves relative to the reference week ($t = -1$). For robustness, some specifications include additional controls X_{it}, such as differential trends by baseline demographics. Standard errors are clustered by baseline ZIP code.

The key parameters of interest in Equation 1 are β_t, the coefficients on the interaction of event week indicators with the New Orleans indicator $NOLA_i$. Thus, β_t non-parametrically 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 cities cohorts' mortality rates 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 cities 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 35 pre-event weeks when estimating Equation 1.

4.2 Long-run effects of Hurricane Katrina on mortality and relocation

Annual Mortality and Mobility We estimate the long-run effects of Hurricane Katrina on mortality and relocation using a cohort approach very similar to our short-run weekly analysis, except that we define the time dimension of the panel data to be annual and extend our period of analysis to cover up to 8 years after Hurricane Katrina.[11] Specifically, we include observations for each individual i and year t starting from the base year used to define the cohort (e.g. 2004) through 2013. As with the weekly analysis, we omit observations after the year in which the individual dies, if any. We then estimate

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

where the outcome Y_{it} is a measure of either mortality or residing outside one's baseline city of residence. To capture mortality, we define $Died_{it}$ to equal zero if individual i survived through year t and to equal one if they died that year. To capture relocation, we define $LeftHSA_{it}$ to equal zero if the individual resides in their baseline HSA that year and to equal one otherwise. 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 again clustered by baseline ZIP code. 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 the change in the control cities cohorts' mortality rates 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 cities cohorts' mortality rates

[11] We do not estimate weekly effects on relocation because we only have annual measure of individuals' location.

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 for each of the six Medicare cohorts defined by base years 1999-2004. We show results for the 1999 and 2004 cohorts in the main paper, and relegate results for other cohorts to the Online Appendix. The 1999 cohort allows us to test for pre-trends over a long time horizon, but this cohort may not adequately capture the experiences of those affected by Hurricane Katrina, as many individuals in this cohort move away or die before 2005. Furthermore, the elderly in the 1999 Medicare cohort were at least 70 by the time Hurricane Katrina struck. To include younger elderly in our estimates, we need to consider a later cohort. While we cannot estimate pre-Katrina trends for the 2004 Medicare cohort, it consists of the most relevant group of individuals exposed to the Hurricane, and we focus on these as our preferred estimates for drawing conclusions about Hurricane Katrina's effect on health.

Cumulative Mortality The annual mortality results from Equation 2 provide direct estimates to calculate the effect of Hurricane Katrina on changes in cumulative mortality for the New Orleans cohort. Specifically, for each post-Katrina year t between 2005 and 2013, the change in cumulative mortality ΔM_t is given by

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

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

Concise Difference-in-Differences Event study estimates from Equation 2 non-parametrically identify treatment effects over time and can also be used to gauge pre-trends to assess the plausibility of assuming parallel trends in outcomes between the New Orleans and control cohorts. If there are no pre-trends and if the treatment effect is constant over a period of time, a more efficient approach is to partition years into longer periods. To that end, we group years into a pre-treatment reference period (base year – 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 use the following regression specification:

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

Other than the first two terms on the right-hand side, the controls in Equation 4 are the same as those in Equation 2. The coefficients β_{SR} and β_{LR} 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 estimating Equation 2.

Heterogeneous Treatment Effects We estimate heterogeneity in treatment effects with respect to a variety of baseline characteristics, including flooding from Hurricane Katrina in one's 9-digit ZIP code of residence, being 75 or older in 2004, race, residing in a below-median income 9-digit ZIP code in New Orleans, and the presence of various chronic conditions. We allow estimates of treatment effects to vary arbitrarily by a given characteristic by augmenting the event study and concise difference-in-differences specifications (Equations 2 and 4, respectively) to include interactions between the key treatment indicators and the characteristic of interest. Because outcome levels at baseline may differ by the chosen characteristic within New Orleans and between the treatment and control cities, we also control for each characteristic and its interaction with the New Orleans indicator. Furthermore, to allow for differential secular trends, we include interactions between the characteristic and year fixed effects whenever there is variation in the characteristic within the control cohort. However, in some cases we do not include such interactions: for example, there is no flooding from Hurricane Katrina in the control cities, so heterogeneity analysis by flood level of an individual's residence at baseline does not include flood-by-year fixed effects.

4.3 Mechanisms

To explain the changes in survival probability following Hurricane Katrina, we conduct another empirical exercise. Specifically, we restrict our sample to New Orleans individuals who moved HSAs between 2004 and 2006.[12] Thus, individuals had to have survived at least until early 2006 to be included in our sample. We calculate each HSA's empirical mortality rate in 1999-2013, using its 1999 Medicare cohort. We exclude New Orleans from the sample of HSAs because its mortality rate was clearly influenced by Hurricane Katrina. We then estimate the relationship between a New Orleans mover's mortality rate in 2006-2013 and the average mortality rate of the HSA to which he or she moved in 2006 as follows:

$$\begin{aligned} Died_{it} = {} & \gamma_m \times 1[t = 2006 - 2007] \times MDR_{2006HSA} \\ & + \gamma_l \times 1[t = 2008 - 2013] \times MDR_{2006HSA} + \alpha_t + \alpha_z + \varepsilon_{it}. \end{aligned} \tag{5}$$

The variable $MDR_{2006HSA}$ is the mean death rate of the HSA to which an individual

[12]We exclude 2005 from this exercise to avoid capturing short-run relocation following Hurricane Katrina.

moved in 2006, as calculated using that HSA's 1999 cohort. We do a similar estimation for spending, replacing $MDR_{2006HSA}$ with a similarly calculated spending variable. All other variables are as before. Because we do not include non-New-Orleans individuals in this estimation, it is not necessary to have New Orleans indicators in equation (5).

It is important to note that these estimates are not necessarily causal: it is possible that individuals with higher future mortality rates sort into higher-mortality areas. Further tests are necessary to probe this possibility.

5 Results

5.1 Effect of Hurricane Katrina on short-run mortality

Figure 5 reports short-run, weekly effects of Hurricane Katrina on mortality among the 2004 Medicare cohort estimated using Equation 1. 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 Hurricane Katrina made landfall: the New Orleans 2004 Medicare cohort's mortality increased by 0.34 percentage points that week, which accounts for 63 percent of the excess 2005 mortality we identify later in our annual analysis. Relative to average weekly mortality in our sample, the mortality rate more than tripled the week of Katrina. We also see statistically significant increases in mortality for two weeks after landfall. While the magnitudes are about an order of magnitude smaller (0.03 and 0.05 percentage points, respectively), they nonetheless represent large relative mortality increases (a 34 percent and a 45 percent increase, respectively).

In the subsequent 62 weeks, only one of the positive point estimates is significant (at the 10 percent level). The combined total of the coefficients in the week of Katrina and the six weeks after *exceeds* the 2005 increase in mortality we see later in the annual analysis, suggesting the presence of short-run mortality displacement (i.e., the deaths of elderly/disabled who would have died in the near future even absent Katrina). Toward the end of the sample period, there are nine weeks in which the mortality rate is significantly lower than the week before the Hurricane. However, without considering a longer time horizon, we cannot rule out that such patterns are driven by harvesting.

The absence of a more prolonged negative impact on mortality is quite surprising in light of existing literature on elderly health, especially given the scale of Hurricane Katrina's destruction described earlier. On the other hand, about 80 percent of the New Orleans population had evacuated before landfall, and the Hurricane prompted a massive private and public response to help the victims. Because we cannot separate the effects of the aid response from the direct effects of the Hurricane, it is important to note that counterfactual mortality could have been higher absent the aid.

5.2 Effect of Hurricane Katrina on long-run mortality and mobility

5.2.1 Event study

Next, we use annual data to estimate the effect of Hurricane Katrina on elderly and long-term disabled mortality and mobility in the longer run. Figure 6 shows the estimates corresponding to Equation 2 (changes in annual mortality, black lines) as well as Equation 3 (changes in cumulative mortality, red lines).[13] The top graph shows the estimated effect of Hurricane Katrina on the 1999 Medicare cohort, for which we can observe mortality pre-trends relative to the control group. Reassuringly, there are no statistically significant differences in the mortality rate of the New Orleans elderly relative to control elderly from the same cohort. The second graph shows the same results for the 2004 cohort, which most closely corresponds to the set of New Orleans Medicare beneficiaries affected by the Hurricane.

[13] Exact coefficients and standard errors for this and other figures can be found in the Online Appendix.

Both graphs yield similar results in the post-Katrina period. For the 2004 cohort, the mortality rate increases by over half a percentage point in the year of Hurricane Katrina. This increase corresponds to about ten percent of the average annual mortality rate for this cohort, which is particularly large given that these additional deaths all occur in the last four months of the year.

The 2005 increase in the mortality rate for the 1999 New Orleans cohort is even larger: over three-quarters of a percentage point. At first glance, this is perhaps surprising, as some of these individuals are no longer alive by 2005 and others may have left New Orleans. However, the remaining individuals are 69 years old or older in 2005, which, as we show later, makes them more susceptible to negative effects of the disaster than younger Medicare beneficiaries.

What is truly remarkable, however, is how quickly this mortality increase reverses itself: the death rate for both cohorts falls below pre-Katrina levels in 2006 and remains below them for the rest of the sample period (though not all the estimates are statistically significant at the 5 percent level). In almost every year after 2005, the death rate for New Orleans elderly is at least a quarter of a percentage point lower than that of the controls. The initial decrease in death rates is perhaps unsurprising, as it can potentially be explained by Hurricane Katrina killing particularly sick individuals who would have died relatively soon even in the absence of the storm. However, as the red line plotting changes in cumulative mortality shows, harvesting can at best explain two years of subsequent mortality reductions.

Empirically, we see the change in cumulative mortality become negative in 2007–2008 after an initial increase in 2005. It continues to decline throughout the post-Katrina period, reaching about -1.75 percentage points in 2013 for the 2004 cohort. That is, by the end of our sample period, the victims of Hurricane Katrina are 1.75 percentage points *more* likely to be alive than the control group, despite no significant differences in survival probability prior to the Hurricane. About 60 percent of the 2004 cohort survives to 2013. Thus, relative to the average survival rate over this time period, a decrease in cumulative mortality of 1.75

percentage points represents a survival improvement of 2.9 percent.

Finally, we examine the effect of Hurricane Katrina on elderly mobility. We define a variable equal to 1 if an individual is alive and living in his or her 2004 city of residence and 0 if she or he is alive and not living in his or her 2004 city of residence. For the 2004 cohort, the 2004 city of residence will be New Orleans. For the 1999 cohort, it may be something different. However, as long as there are no large pre-Katrina trend differences between the New Orleans beneficiaries and the control group, pre-2005 mobility will not affect our estimates.

The estimated mobility effects are shown in Figure 7. In 2005, Hurricane Katrina displaced about forty percentage points more individuals than leave their city of residence in a typical year, and most of the displaced stayed away in 2006. They slowly began returning in 2007; however, by 2013, New Orleans residents that were alive were still 23-24 percentage points less likely to have returned than the controls. Thus, a large share of New Orleans elderly left the city after Hurricane Katrina and never came back.

5.2.2 Robustness of event study estimates

In the Online Appendix, we replicate Figure 6 using a random 50 percent sample of the entire US cohort of Medicare beneficiaries as a control for each New Orleans cohort (see Figure A.1 and Table A.4). Each cohort consists of over 170 million individual observations (number of individuals times the number of years in which they are alive during the sample period). As with our smaller control group, we see no differential pre-trends in mortality prior to the Hurricane. We obtain qualitatively similar but quantitatively larger and more significant estimates of the post-Katrina reductions in the mortality rate, indicating that the cumulative mortality of New Orleans residents decreased by 2.2-2.7 percentage points by the end of 2013. Because we are more confident in the comparability between New Orleans residents and the residents of the ten cities in our main control group, we continue to use the more restricted control group. However, it is reassuring that our results do not hinge on this particular choice.

We also show mortality estimates for the 2000, 2001, 2002, and 2003 Medicare cohorts in the Online Appendix (see Figure A.2). The pre-trends are always insignificant, which makes us confident that the estimated effects of Hurricane Katrina on the 2004 cohort are not confounded by differences between treatment and control individuals. Our post-Katrina results are also very robust: the estimated effects on annual and cumulative mortality rates are similar across the cohorts.

Finally, we have also probed the robustness of the event study estimates to increasingly stringent controls for beneficiaries' characteristics. The estimates, available upon request, are very similar if we include fixed effects for all possible combinations of age, sex, and race and if we also allow differential time trends by these combinations (i.e., interact year fixed effects with the set of age-sex-race indicators). We further demonstrate the robustness of our estimates to modifying the control variables in the next two sections, where we employ more parsimonious specifications.

5.2.3 Concise difference-in-differences

Table 2 shows the Equation 4 mortality estimates for the 2004 cohort (columns (1)-(3)) and the 1999 cohort (columns (4)-(7)). In addition to our preferred specification (columns (1) and (4), labeled "A" in the table), we also show results where we add fixed effects for all 5-year-age-bins-by-gender-by-race combinations (labeled "B") and where we additionally allow the year fixed effects to vary by each 5-year-age-bin-by-gender-by-race combination (labeled "C"). Finally, for the 1999 cohort, we also add an indicator for New Orleans interacted with an indicator for the years 1999-2001. This last fixed effect allows us to gauge whether differential pre-trends, if any, are affecting these results.

Overall, we find effects that are very similar to each other and to the event study, but more precisely estimated. The immediate (2005) mortality increase for the 2004 cohort ranges from 0.54 to 0.56 percentage points. As in the event study graphs, the increase for the 1999 cohort is larger, 0.74-0.82 percentage points. In 2006-2007, the 2004 New Orleans cohort experiences a significant decline in its mortality rate of 0.25-0.30 percentage points. Given the initial mortality increase, this decline is potentially rationalizable by mortality displacement of very sick individuals. However, the mortality rate of this cohort continues to be 0.29-0.53 percentage points lower over the next six years (2008-2013), which, as we showed in the event study, cannot be explained by harvesting.

The 1999 New Orleans cohort likewise experiences medium- and long-run declines in the mortality rate. In 2006-2007, its mortality rate is 0.39-0.45 percentage points lower than before. In the next six years, it is 0.38-0.50 percentage points lower. All estimates in Table 2 are significant at the 5 percent level or better.

5.2.4 Heterogeneous treatment effects

The analysis above shows that while Hurricane Katrina devastated much of the infrastructure of New Orleans, the Medicare cohorts living in New Orleans were likely to relocate to other cities and to experience long-run reductions in mortality, on average. Of course, Hurricane Katrina hit some parts of New Orleans harder than others. We begin by estimating how the relocation and mortality effects of the storm varied by the degree to which an individual's baseline 9-digit ZIP code of residence was flooded in the aftermath of the Hurricane. To do this, we define a categorical variable indicating either no storm flooding, up to four feet of storm flooding, or over four feet of flooding. We then estimate the controlled event study, allowing treatment effects to vary by the degree of flooding.

Results of this exercise are reported in Figure 8. The short-run mortality effects of the hurricane (in 2005) were similar by flood level (top panel). However, and strikingly, the long-run mortality declines are *larger* in areas that experienced flooding. To try to understand why, we consider heterogeneous relocation effects, reported in the bottom panel of Figure 8. Individuals whose homes were flooded by the hurricane were much more likely to relocate to another city following the storm. Together, these results suggest that responses to the storm such as leaving New Orleans may play a role in long-run survival gains among the cohort exposed to the Hurricane.

Table 3 reports concise estimates of heterogeneous difference-in-differences effects across a larger set of pre-Katrina characteristics. The first row reports concise difference-in-differences estimates for the entire sample (no heterogeneity) for reference. As reported in column (3), average excess mortality among the New Orleans cohort was 0.551 percentage points in the short-run (2005), an increase of more than 10 percent relative to this cohort's mortality in 2004. However, there was an average mortality reduction of 0.360 (column (5)) over the long-run period (2006–2013). Each subsequent row of Table 3 reports heterogeneity in the short-run and long-run effects of the hurricane with respect to the baseline variable is specified by the row.

Building on the heterogeneous event study effects reported in Figure 8, the second row of Table 3 reports concise difference-in-differences effects by whether one's 9-digit zip code of residence in 2004 was subsequently flooded. The short-run impact of the hurricane was the same in flooded and non-flooded regions (column (4)), but long-run mortality reductions were 0.441 percentage points *greater* in flooded regions (column (6)). This striking result points to a stronger recovery response among those whose homes were most damaged by the storm and who were more likely to move to a new city after the storm.

Next, we examine heterogeneity by baseline demographics: age, race (black indicator), sex, and below median income. We observe no statistically significant heterogeneity by sex or race. However, we do find that the short-run impact of the hurricane was concentrated almost entirely among those aged 75 and older in 2004. The long-run mortality reduction was similar for those above or below age 75. Because the younger Medicare beneficiaries experienced a smaller short-run increase, this latter result indicates larger long-run survival gains for younger Medicare beneficiaries. Individuals initially residing in 9-digit ZIP codes with below-median income experienced mortality effects similar to those residing in flooded ZIP codes.

Finally, we examine heterogeneity by chronic conditions present at baseline. There is broad concern that individuals with Alzheimer's disease or dementia may be especially vulnerable to disasters, given their limited memory and reasoning abilities. Supporting this concern, we find that individuals with Alzheimer's disease or dementia were 3.14 percentage points more likely to die in the year of the hurricane. However, there does not appear to be a long-run scarring effect from the hurricane for this group: long-run mortality was reduced (though only marginally significantly) by an additional 1.49 percentage points, on average, over 2006–2013.

Across the other chronic conditions we examine, we find that the short-run risk of death is significantly higher among those with respiratory and musculoskeletal conditions, but lower among those with blood or kidney conditions. In the long-run, those with blood or kidney conditions experience significantly larger mortality improvements, while those with musculoskeletal or "other" chronic conditions experience smaller mortality improvements.

Overall, two takeaway points emerge from this table. First, the long-term mortality gains are not concentrated in any particular segment of the Hurricane Katrina victims, but appear fairly widespread. In particular, even individuals that seem more vulnerable *ex ante*, such as those with lower incomes or chronic conditions, have lower long-run mortality. Second, those who were most likely to lose their homes and be displaced surprisingly experience the largest long-run improvements in survival. To shed light on this counterintitutive pattern, we turn to examining the mechanisms behind our main result.

5.3 Mechanisms

Our results thus far show that Katrina led to non-trivial *declines* in long-run mortality among the elderly. Additionally, one's flooding experience is *negatively* correlated with long-run mortality. Both these facts are highly counterintuitive, as there is no solid theoretical basis for natural disasters to have positive *direct* effects on health. A natural hypothesis, then, is that the mortality improvements as a result of Hurricane Katrina came about indirectly through other effects of the Hurricane. For example, Sacerdote (2012) finds that Katrina and Rita student evacuees experience long-run improvements in test scores, likely because they transferred to better schools. Relatedly, Deryugina, Kawano and Levitt (2018) find long-run increases in New Orleans individuals' earnings brought about because Hurricane Katrina both forced individuals to relocate to stronger labor markets and strengthened the New Orleans labor market. Similarly, it is possible that the mortality improvements came about because Hurricane Katrina forced elderly individuals to relocate to areas more conducive to survival (e.g., because of better health care). Alternatively, or in addition to, Hurricane Katrina may have led to improvements in New Orleans itself (Marsa, 2015).

It is difficult to formally test whether any improvements in the New Orleans health care system are at least responsible for the aggregate mortality improvements we see. One way to do so indirectly is to consider the mortality outcomes of those who stayed in New Orleans separately from those who left. As we show below, the decision of whether to leave or stay was highly correlated with observable characteristics and chronic conditions, so the level differences in mortality between these two groups are not informative. However, if the New Orleans health care system improved during rebuilding, we might expect stayers' mortality to gradually improve as well.

We restrict the sample to individuals from the 2004 cohort who survive until at least 2006 in order to have a meaningful measure of "staying" and estimate an augmented version of equation 2 with separate New-Orleans-by-year interactions for movers and stayers. The reference category is stayers in 2006. The result, shown in Figure 9, makes it clear that stayers' mortality does not improve over time relative to 2006. Thus, unless one is willing to consider the possibility that any long-run improvements in New Orleans were already there in 2006 – which would go against all observational evidence – it is unlikely that the mortality improvements we are seeing are caused by improvements in New Orleans.

Next, we probe the relocation mechanisms. We begin by restricting our sample to New Orleans elderly from the 2004 cohort who moved between 2004 and 2006 (i.e., were not in

New Orleans in 2006). This necessarily limits our sample to people who survived at least until the beginning of 2006. We then calculate the average mortality rate of the 1999 cohorts in each of the 3,436 HSAs. This gives us a measure of the "typical" mortality rate in each area. We then regress each New Orleans' resident's post-2006 mortality outcome on the mean mortality rate in his or her 2006 HSA (see equation (5)). Note that calculating each HSA's mortality rate using its 1999 cohort means that the HSAs' mortality rates will not be mechanically correlated with the mortality rates of the movers. To further ensure that we are not capturing any changes in local mortality brought about by Hurricane Katrina, we exclude the New Orleans HSA and the HSAs in the same Hospital Referral Region from the local mortality rate calculation and the subsequent estimation. That is, 2004 New Orleans residents that move to just outside New Orleans are excluded from this analysis.

The results are shown in Table 4. Remarkably, the mean death rate in the HSA to which a New Orleans individual from the 2004 cohort moves in 2006 is highly predictive of his or her subsequent mortality (Panel A). The estimated coefficients are large: a 1 percentage point increase in one's new HSA's mortality rate is associated with a 0.52-0.65 percentage point increase in one's 2006-2013 mortality rate. These estimates are also highly robust to the inclusion of very flexible controls, such as year fixed effects that vary by zip code and by baseline characteristics (column (3)). They also do not change much if we also include non-New Orleans elderly in the sample (column (4)). Finally, in column (5), we control for 2004 spending and eight types of chronic conditions, as described earlier. This reduces our sample by more than 50 percent, as even many fee-for-service beneficiaries do not have enough claims to reliably infer if they have a particular chronic condition. However, our results hardly change.

In Panel B of Table 4, we additionally estimate the correlation between average medical spending in one's destination HSA and New Orleans movers' subsequent mortality. Prior research has found little statistical relationship between local spending and health outcomes (e.g., Fisher et al., 2003*a*,*b*; Baicker and Chandra, 2004; Sirovich et al., 2006). The relationship may be absent because the additional local spending is largely wasteful (e.g., Fisher, Bynum and Skinner, 2009; Cutler, 2010; Skinner and Fisher, 2010). Alternatively, it may be that additional spending leads to better health *and* that worse population health leads to higher spending; the net correlation may be close to zero even though spending and quality are positively related, all else equal.

An advantage of our context is that the health of the movers cannot be affecting local spending, if the latter is properly defined. We define local average spending as an HSA's 2006 per-patient spending on patients from that HSA's 2004 cohort. In addition to not being contaminated by the movers' health, this spending measure also has the advantage of being highly relevant to the New Orleans cohort.

We find very small and insignificant results: a $1,000 increase in an area's average medical spending is associated with a 0.01 to 0.07 percentage point reduction in movers' mortality.[14] By contrast, the relationship between local mortality and movers' subsequent mortality remains highly significant and similar to that of Panel A.

In Table 5, we extend these results by separating the post-Katrina period into two, 2006–2007 and 2008–2013, and focus on estimates that do not include local spending.[15] Remarkably, the high correlations between local and movers' mortality are observed as early as 2006-2007 (and are even larger in those years), suggesting that the differences in mortality are not due to longer-run effects of lifestyle changes. The correlations are somewhat lower in magnitude in 2008–2013; this is at least partly due to the fact that we are measuring the mean mortality rate in the individual's 2006 HSA of residence and many New Orleans individuals are continuing to move throughout the sample period.[16]

Overall, Tables 4 and 5 make it quite clear that the background mortality rate in places

[14] Considering local spending without also considering local mortality yields very similar results.

[15] Including local spending yields insignificant results similar to Table 4 and does not meaningfully affect the other coefficients.

[16] Letting the local mortality rate change each year is problematic because some individuals die or and others move back to New Orleans after 2006: we would either have to drop them from our sample in those years, which would likely bias the estimates, or use the New Orleans mortality rate, which would be mechanically correlated with individuals' mortality rates and which was clearly affected by Hurricane Katrina.

where people move is strongly predictive of their own mortality. Individuals in our sample are largely dying from cardiovascular disease, cancer, and other chronic conditions, such as respiratory diseases, renal disease, and Alzheimer's. The speed with which individuals' mortality rates converge to the local rate makes it very unlikely that the main reason for this convergence is that individuals become more or less likely to develop these chronic conditions. More plausibly, the quality of the local health care system is affecting both local mortality rates and the mortality rates of new arrivals.

An alternative non-causal explanation is that individuals are choosing locations based on their health: healthier individuals are moving to areas with lower mortality rates and subsequently living longer, while sicker individuals are moving to areas with higher mortality rates, producing the patterns we see here. Perhaps the fact that much of the displacement created by Hurricane Katrina was at least partly involuntary makes this possibility less likely. It is also difficult (but not impossible) to explain why sorting would produce larger coefficients in 2006–2007 compared to 2008–2013. By contrast, the declining correlation is consistent with a causal interpretation, as individuals in our sample continue moving during this time period. Nonetheless, we proceed by examining the extent to which sorting played a role in the moving decisions.

In Table 6, we look at the predictors of leaving New Orleans between 2004 and 2006, restricting our attention on the 2004 cohort of New Orleans individuals who survived at least until the beginning of 2006. It is clear that the decision to move is not as good as random, even along the lines of very basic characteristics: female, black, poorer, and younger elderly individuals are all more likely to move, all else equal. Flood levels are also highly predictive of leaving New Orleans: a one-foot increase in predicted flood levels increases an individual's probability of moving by 1.9 percentage points. Interestingly, many of the chronic conditions we examine are *positively* correlated with moving: beneficiaries with Alzheimer's/dementia, end-stage renal disease, respiratory disease, diabetes, and conditions we group into the "other" category are all more likely to leave the city. In fact, the only chronic condition negatively associated with leaving New Orleans is cancer. One possible explanation for this pattern is that these individuals rely more heavily on continuous medical care and thus had more incentives to leave the New Orleans following the decimation of the health care system.

Next, we look at the correlation between these same characteristics and the local mortality rate of the destination HSA, focusing on New Orleans movers only. Unlike the decision to move, the local mortality rate of the destination HSA is uncorrelated with most characteristics and chronic conditions, with three exceptions. Individuals with Alzheimer's disease/dementia or with heart disease and stroke move to places with slightly higher mortality levels, while individuals with blood or kidney diseases move to places with slightly lower mortality levels. The remaining chronic conditions are not predictive of the mortality rate at the movers' destination.

6 Conclusion

Hurricane Katrina devastated the City of New Orleans and other parts of the Gulf Coast, killing nearly 2,000 people and causing billions dollars' worth of direct damage. However, the Hurricane appears to have come with a silver lining: the elderly and long-term disabled living in New Orleans at the time of the Hurricane experienced non-trivial reductions in long-run mortality. Our analysis suggests that the most likely explanation for these longevity gains is relocation to areas with better health outcomes. By contrast, we see no evidence of long-run improvements in the New Orleans health care system, although our ability to rule this out definitively is limited by lack of appropriate within-city geographic variation.

Two caveats to our conclusion are in order. First, the mortality improvements we find do not mean that individuals' *welfare* increased: the forced relocation and asset destruction may have more than offset any indirect benefits of the Hurricane. Second, the massive aid response to the disaster should also not be overlooked, and our findings would not necessarily generalize to disasters where minimal aid is given. It should be noted, however, that essentially all natural disasters in the US and similarly developed nations are typically accompanied by non-trivial aid.

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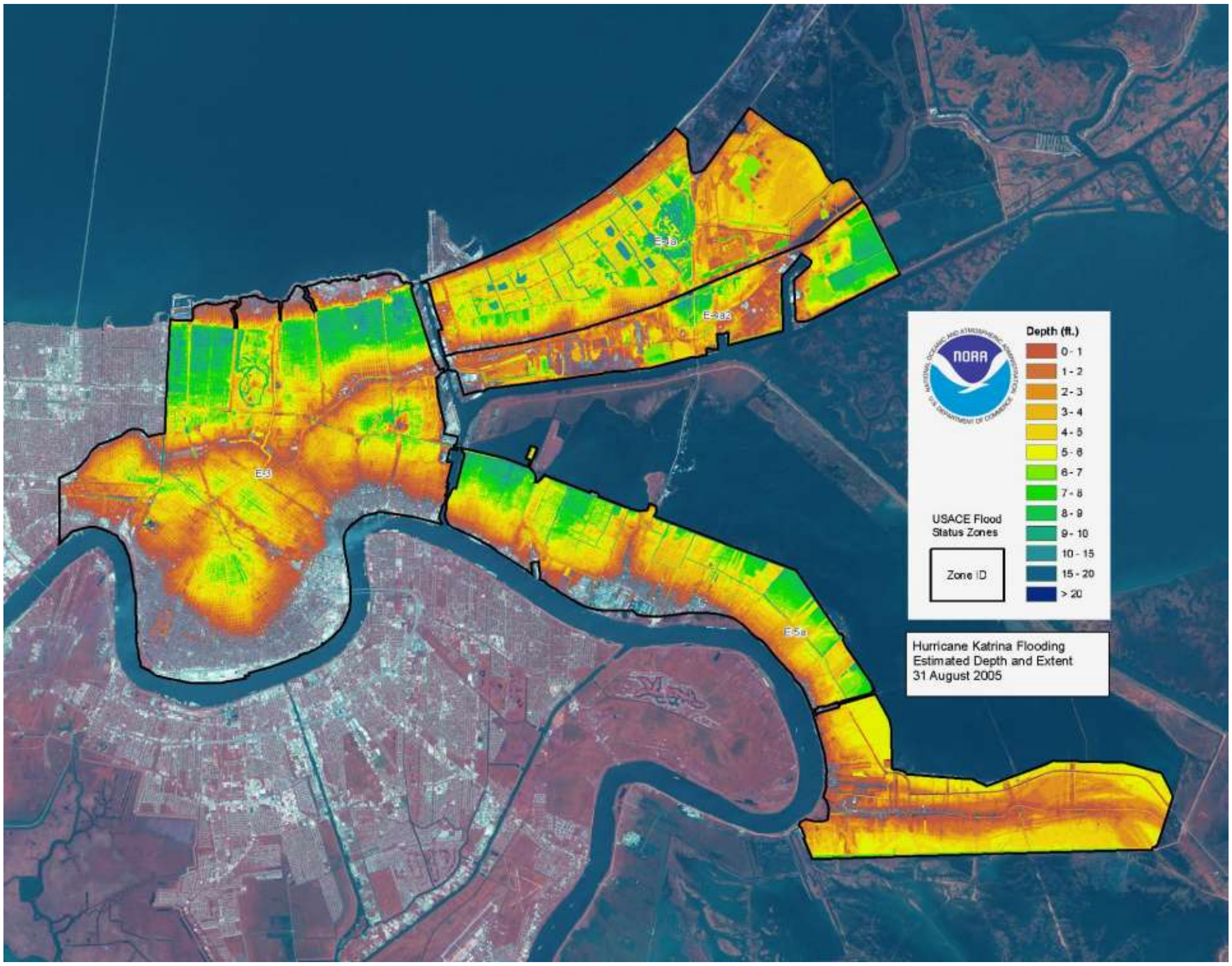
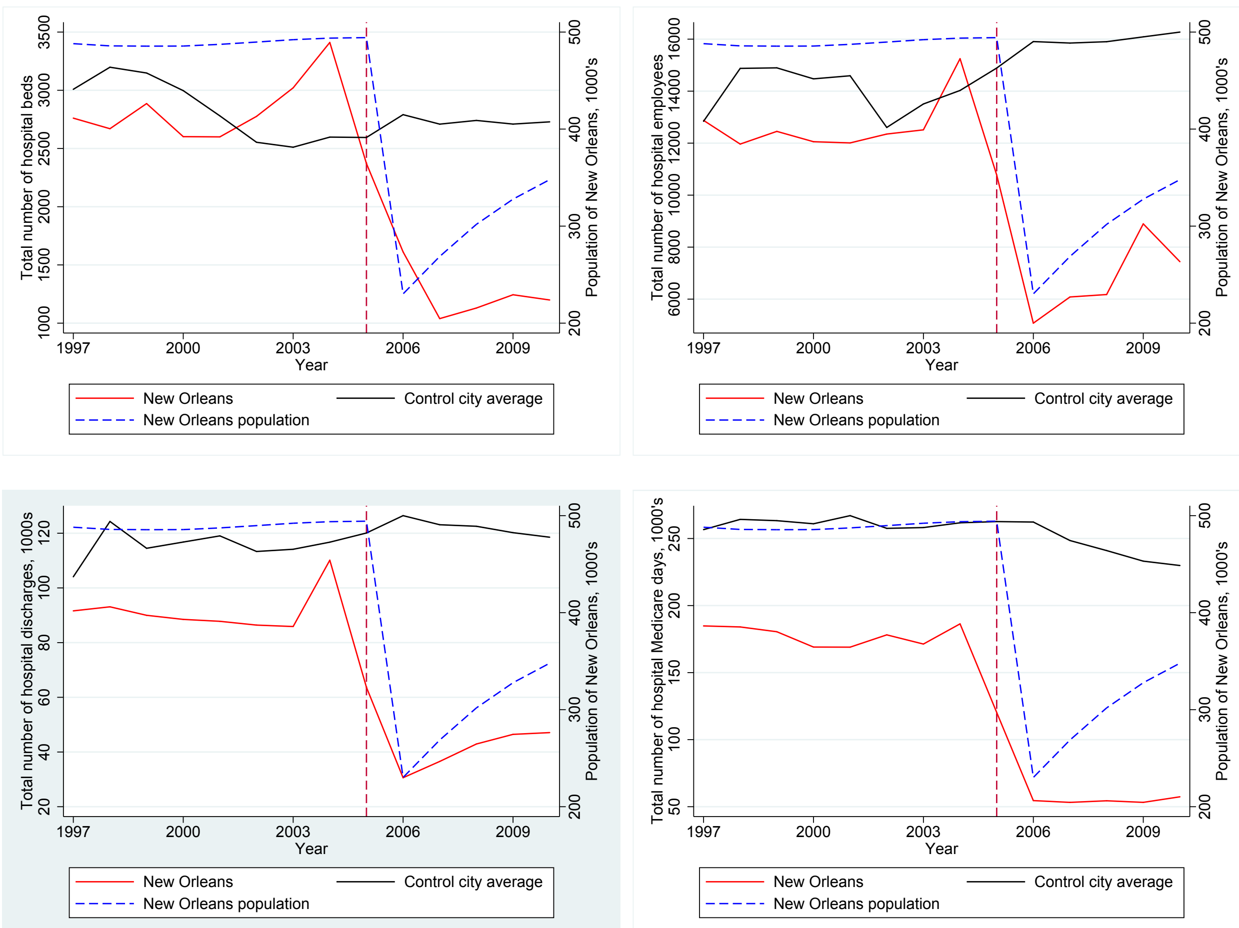
Figures

Figure 1: Hurricane Katrina flood map



Source: National Oceanic and Atmospheric Administration (NOAA).

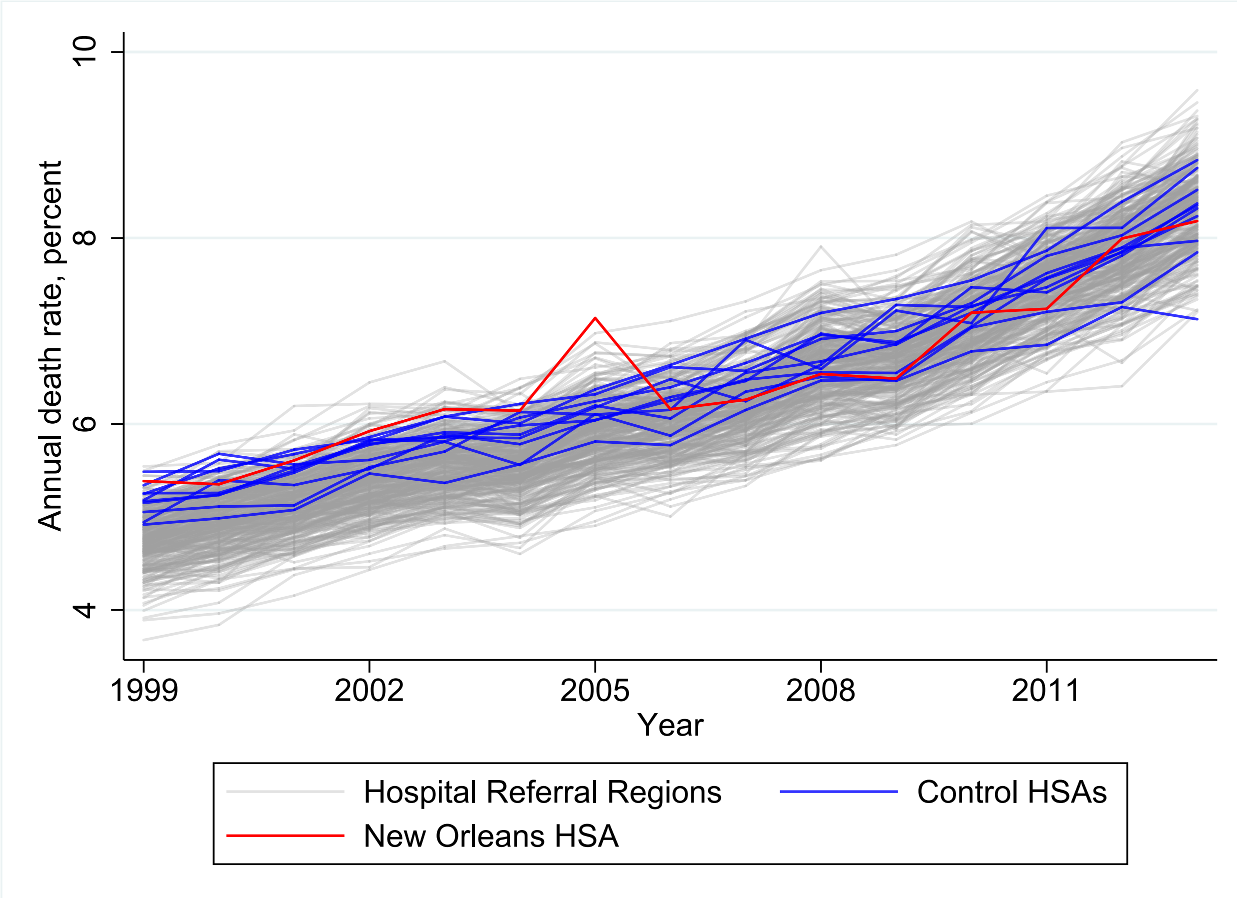
Figure 2: Changes in the capacity of the New Orleans health care system following Hurricane Katrina



Notes: Figure shows the New Orleans population (right axis), the number of hospital beds, the number of hospital employees, the number of hospital discharges, and the number of hospital Medicare days in New Orleans and the control cities we utilize for our individual-level analysis. Sources: Centers for Medicare and Medicaid Services Hospital 2552-96 Cost Report Data file; Bureau of Economic Analysis.

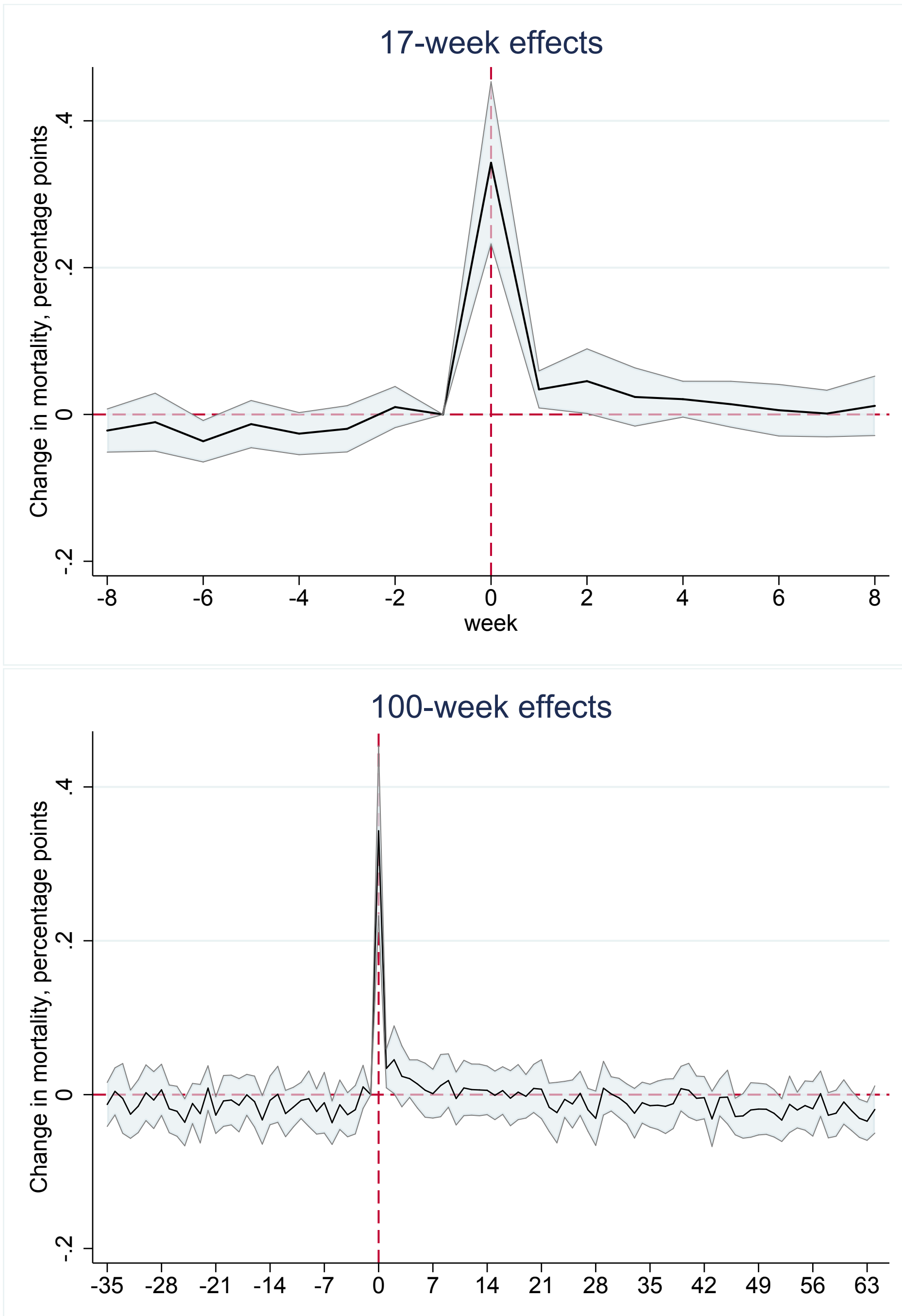
Figure 3: New Orleans and control cities

Figure 4: Death rates in New Orleans versus other areas, 1999 cohort



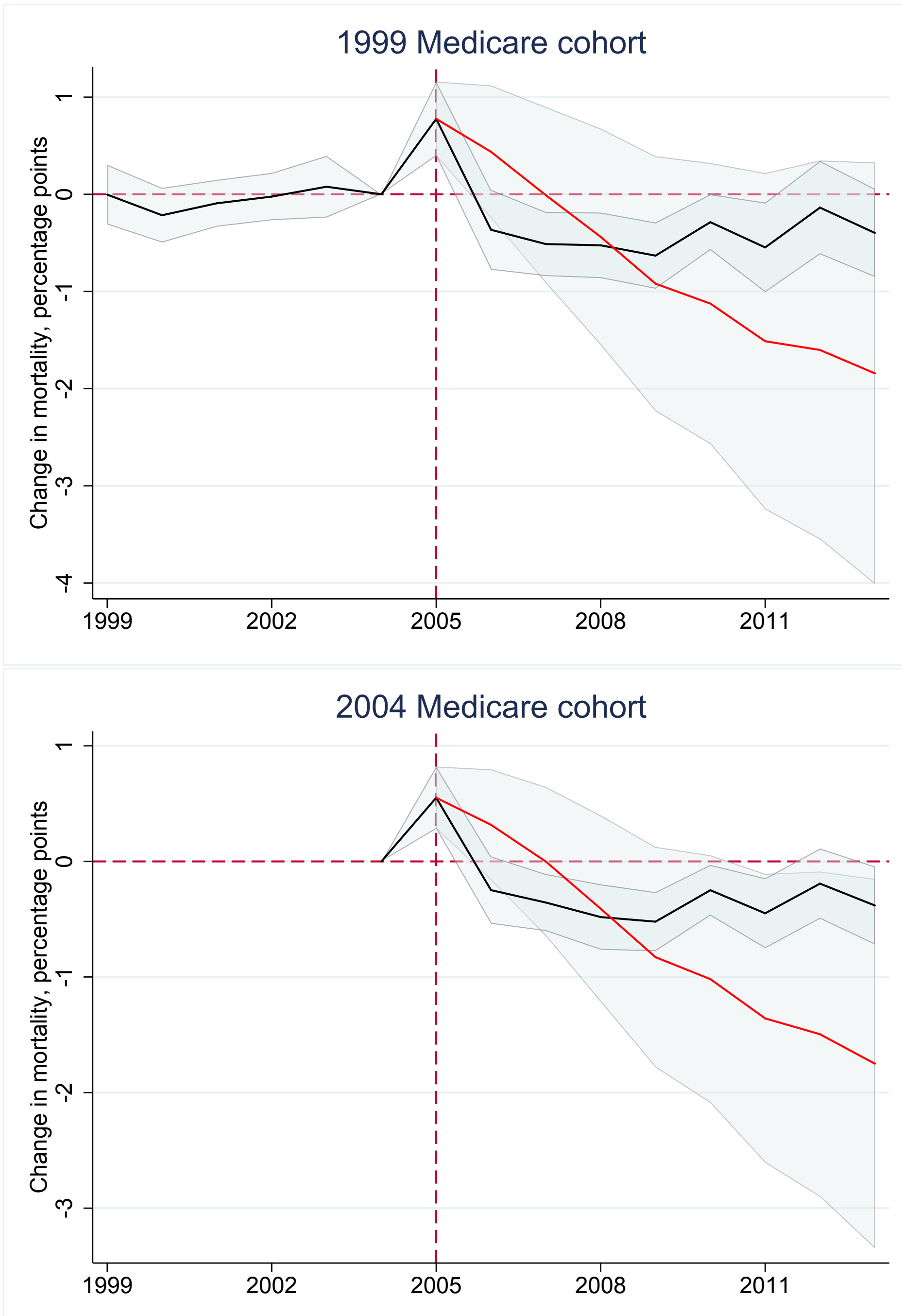
Notes: Figure shows raw annual death rates of the 1999 Medicare cohort in locations indicated by the legend.

Figure 5: Short-run effect of Hurricane Katrina on elderly mortality (2004 Medicare cohort)



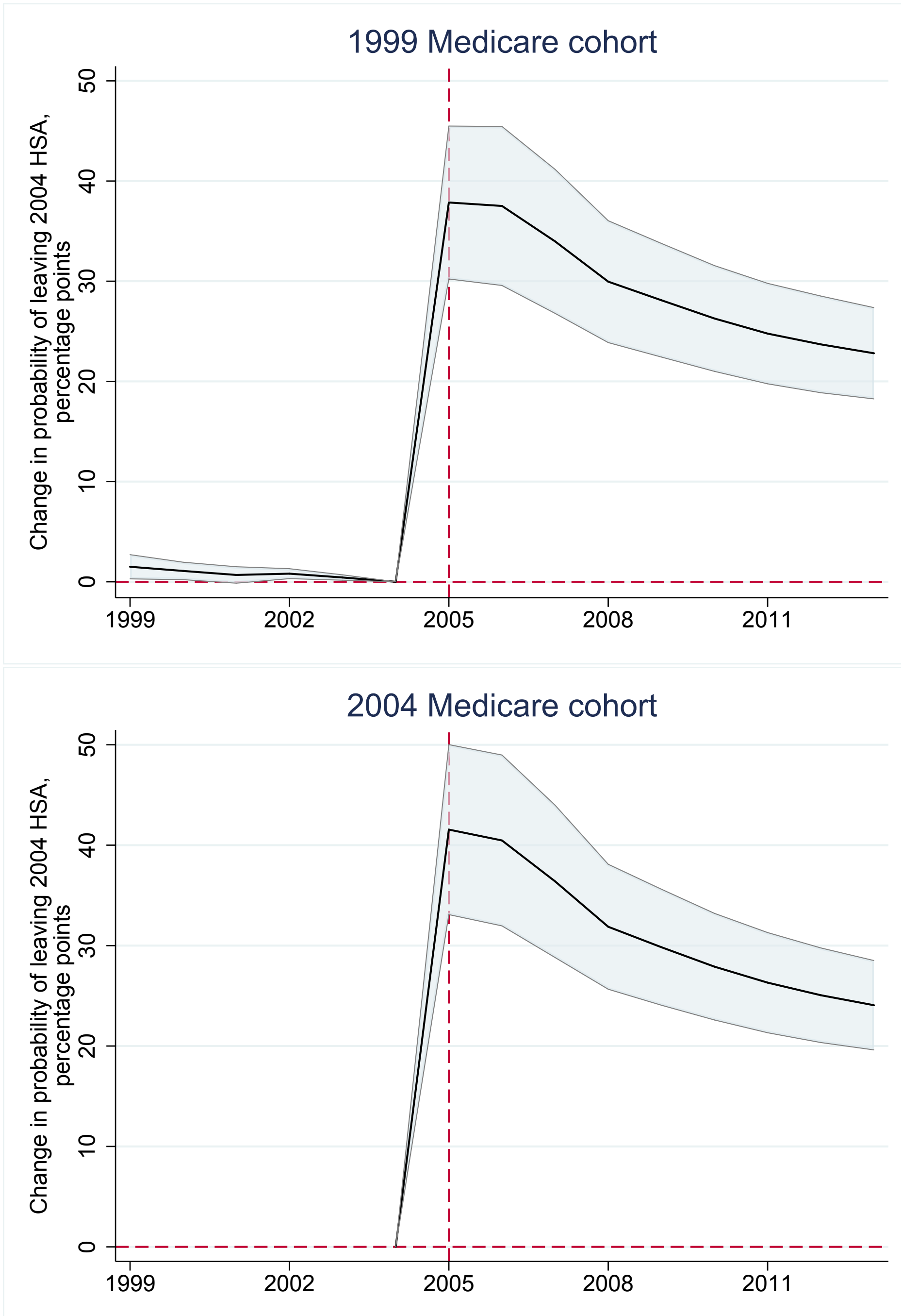
Notes: Figure shows estimates and corresponding 95 percent confidence intervals from Equation 1, estimated over the 35 weeks preceding Hurricane Katrina and the 64 weeks following. Both sets of estimates are from the same regression. Standard errors clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire week and equal to 1 if a beneficiary died in a given week. The week Hurricane Katrina struck New Orleans (Monday, August 29th, 2005) is labeled "0" on the x-axis. Coefficients and confidence intervals have been scaled by 100.

Figure 6: Long-run effect of Hurricane Katrina on elderly mortality



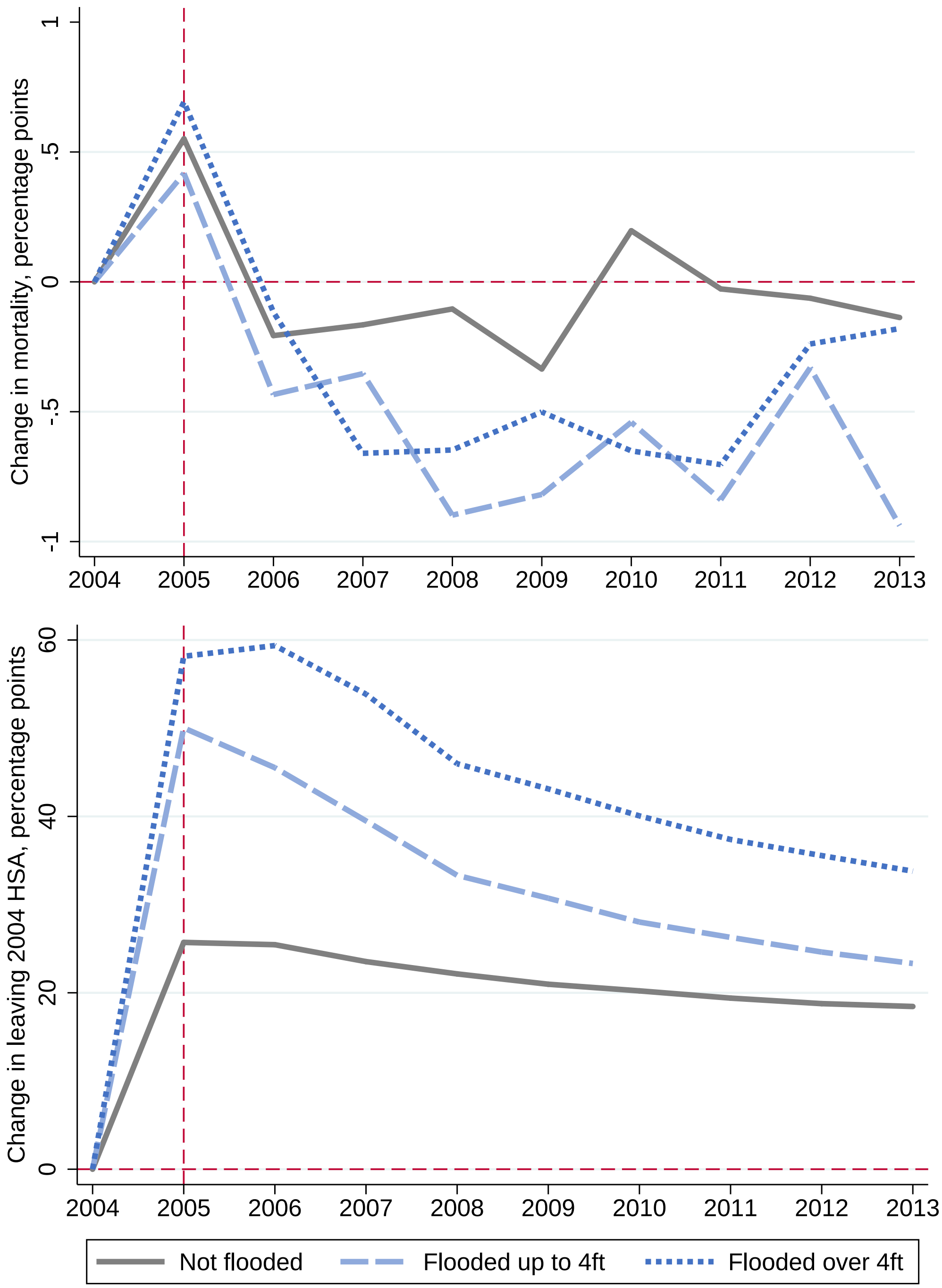
Notes: Black line shows estimates from Equation 2 for the Medicare cohort indicated above each subplot. Red line shows the implied change in cumulative mortality probability (Equation 3). Grey shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and confidence intervals have been scaled by 100.

Figure 7: Effect of Hurricane Katrina on elderly mobility



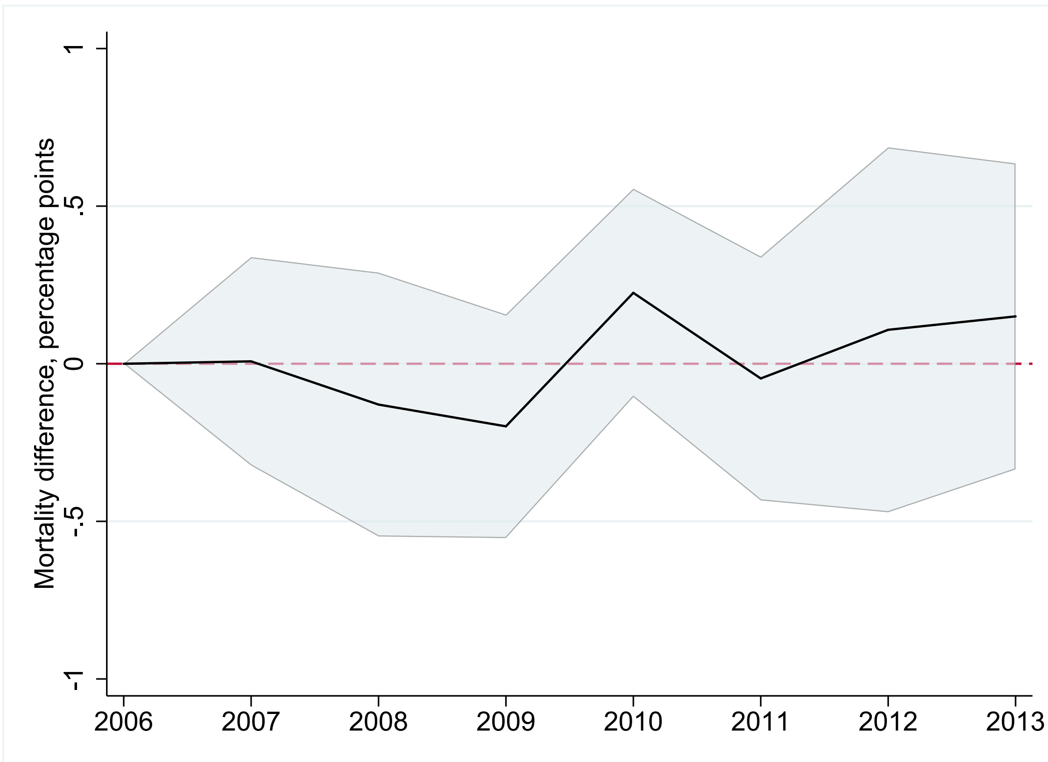
Notes: Figure shows estimates and 95 percent confidence intervals from Equation 2 for Medicare cohort indicated above each subplot. Standard errors clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 1 if a beneficiary is residing in a different HSA compared to 2004 and equal to 0 if the beneficiary is residing in the same HSA as in 2004. Coefficients and confidence intervals have been scaled by 100.

Figure 8: Effect of Hurricane Katrina on elderly mortality and mobility, by level of flooding (2004 Medicare cohort)



Notes: Figure shows estimates of the probability an individual dies or is living in a city other than the city of residence in 2004, by the level of flooding from Hurricane Katrina. Estimates are from an augmented version of Equation 2 where the interactions between calendar year and living in New Orleans at baseline (2004) are fully interacted with a categorical variable indicating whether an individual's baseline 9-digit ZIP code was either (a) not flooded by the hurricane, (b) flooded 0-4 feet, or (c) flooded more than 4 feet. In the top panel, the dependent variable is an indicator equal to 1 if a beneficiary dies that year. In the bottom panel, the dependent variable indicates residing in a different HSA compared to 2004 and equal to 0 if the beneficiary is residing in the same HSA as in 2004. Coefficients have been scaled by 100 to reflect changes in percentage points.

Figure 9: Changes in stayers' mortality rates over time



Notes: Figure shows estimates and 95 percent confidence intervals from an augmented version of Equation 2 for individuals who remained in New Orleans in 2006. Standard errors clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and confidence intervals have been scaled by 100.

Tables

Table 1: Summary statistics for the 2004 cohort

	(1) Mean	(2) Std. dev.	(3) Obs.
Panel A: cross-sectional variables			
Race = black indicator	0.353	0.478	1,279,531
Male indicator	0.422	0.494	1,279,531
Age at baseline	71.107	12.683	1,279,531
Above 64 indicator	0.820	0.385	1,279,531
Moved in 2004-2006	0.069	0.253	998,104
Moved in 2004-2006, New Orleans only	0.444	0.497	62,523
Flood depth during Katrina, feet	1.775	2.607	80,607
Panel B: panel variables			
Died during the year	0.055	0.228	10,174,388
Enrolled in fee-for-service Medicare	0.802	0.399	10,174,388
Annual Medicare spending (fee-for-service only)	11,912	25,579	8,157,260

Table 2: Concise difference-in-diffence mortality estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(3)
	2004 Cohort			1999 Cohort			
2005 x New Orleans	0.55***	0.54***	0.56***	0.82***	0.80***	0.76***	0.74***
	(0.14)	(0.14)	(0.13)	(0.17)	(0.18)	(0.15)	(0.15)
(2006-2007) x New Orleans	-0.30**	-0.29**	-0.25**	-0.39***	-0.39***	-0.45***	-0.45***
	(0.12)	(0.13)	(0.11)	(0.12)	(0.13)	(0.11)	(0.11)
(2008-2013) x New Orleans	-0.39***	-0.43***	-0.29***	-0.38***	-0.44***	-0.45***	-0.50***
	(0.12)	(0.15)	(0.10)	(0.13)	(0.17)	(0.12)	(0.14)
Control for 1999-2001 N.O.	No	No	No	No	No	Yes	Yes
Included controls	A	B	C	A	B	A	B
Dep. var. mean	5.48	5.48	5.48	6.27	6.27	6.27	6.27
Observations	10,174,387	10,174,387	10,174,380	12,843,079	12,843,079	12,843,079	12,843,079

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Table reports estimates of equation (2) from the main text. Standard errors (in parentheses) clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes zip code and year fixed effects; B also includes fixed effects for each 5-year-age-bin, race, and gender combination. C additionally allows the year fixed to differ by each 5-year-age-bin, race, and gender combination. Control for 1999-2001 N.O. row indicates whether a fixed effect for 1999-2001 New Orleans is included (1999 cohort only).

Table 3: Heterogeneous mortality effects of Hurricane Katrina on 2004 New Orleans cohort

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Difference-in-differences estimates				
			Short-run (2005)		Long-run (2006-2013)		
Baseline *var*	Percent $var = 1$ in NOLA, 2004	Mean mortality if $var = 1$ in NOLA, 2004	NOLA × 2005	NOLA × 2005 × *var*	NOLA × (2006-2013)	NOLA × (2006-2013) × *var*	Observations
1	100.0	5.0	0.551***		-0.360***		10,174,388
			(0.138)		(0.113)		
Flooded	56.1	5.2	0.551***	0.000	-0.112	-0.441**	10,174,388
			(0.182)	(0.256)	(0.178)	(0.212)	
Below median income	24.3	5.3	0.639***	-0.361	-0.233*	-0.523**	10,174,388
			(0.154)	(0.269)	(0.121)	(0.220)	
Age 75+	40.7	8.1	0.034	1.303***	-0.384***	0.070	10,174,388
			(0.125)	(0.307)	(0.091)	(0.234)	
Black	51.6	5.0	0.807***	-0.489	-0.104	-0.356	10,174,388
			(0.248)	(0.351)	(0.145)	(0.218)	
Male	43.3	5.4	0.462***	0.210	-0.313**	-0.109	10,174,388
			(0.156)	(0.185)	(0.141)	(0.192)	
End-stage renal disease	1.9	19.9	0.567***	0.170	-0.287**	-1.936	10,174,388
			(0.145)	(1.322)	(0.115)	(1.346)	
Heart disease or stroke	64.8	7.7	0.523**	0.207	-0.695***	-0.373	7,140,694
			(0.251)	(0.345)	(0.161)	(0.331)	
Respiratory disease	12.7	12.0	0.368**	1.274**	-1.021***	-0.055	7,774,624
			(0.164)	(0.529)	(0.169)	(0.512)	
Blood or kidney disease	47.2	8.4	0.999***	-0.737***	-0.536***	-0.913***	7,140,694
			(0.184)	(0.259)	(0.129)	(0.296)	
Cancer	6.9	12.8	0.516***	0.152	-1.005***	-0.272	7,774,624
			(0.165)	(1.037)	(0.167)	(0.653)	
Diabetes	26.8	8.7	0.605***	0.226	-0.796***	-0.420	7,140,694
			(0.186)	(0.291)	(0.142)	(0.261)	
Musculoskeletal disease	28.7	6.3	0.310	1.234**	-1.154***	0.827***	7,140,694
			(0.226)	(0.487)	(0.164)	(0.309)	
Alzheimer's/dementia	12.5	19.9	0.456***	3.137***	-0.785***	-1.491*	6,511,643
			(0.174)	(0.928)	(0.160)	(0.888)	
Other chronic condition	40.4	4.5	0.445**	0.377	-1.212***	0.739***	7,774,624
			(0.217)	(0.287)	(0.182)	(0.216)	

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

Table 4: Correlation between own mortality, local mortality and local spending, movers only

	(1)	(2)	(3)	(4)	(5)
Panel A: local mortality only					
Mean death rate in 2006 HSA	0.55**	0.64***	0.65***	0.52***	0.63**
	(0.24)	(0.22)	(0.22)	(0.14)	(0.29)
Set of controls	A	B	C	C	D
New Orleans sample only	Yes	Yes	Yes	No	Yes
Dep. var. mean	5.95	5.95	5.95	6.31	6.50
Observations	201,863	201,863	201,810	511,906	94,384
R-squared	0.00	0.04	0.05	0.06	0.07
Panel B: local mortality and local spending					
Mean death rate in 2006 HSA	0.60**	0.68***	0.69***	0.53***	0.67**
	(0.24)	(0.24)	(0.23)	(0.15)	(0.30)
Mean spending in 2006 HSA, thousands	-0.07	-0.05	-0.05	-0.01	-0.06
	(0.05)	(0.05)	(0.05)	(0.03)	(0.06)
Set of controls	A	B	C	C	D
New Orleans sample only	Yes	Yes	Yes	No	Yes
Dep. var. mean	5.95	5.95	5.95	6.31	6.50
Observations	201,863	201,863	201,810	511,906	94,384
R-squared	0.00	0.04	0.05	0.06	0.07

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) clustered by beneficiary's 2006 HSA. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes year and baseline zip code fixed effects. B adds 5-year-age-bins-by-gender-by-race fixed effects to the controls in A. C controls for 1-year-age-bins-by-gender-by-race fixed effects, and and allows year fixed effects to vary by all unique 5-year-age-bins-gender-race combinations as well as by baseline zip code. D includes all controls in C, as well as controls for 2004 Medicare spending and chronic conditions.

Table 5: Correlation between own mortality and local mortality over time, movers only

	(1)	(2)	(3)	(4)	(5)
Mean death rate x (2006-2007)	0.79*	0.87*	0.82*	0.42*	0.60
	(0.43)	(0.45)	(0.43)	(0.23)	(0.56)
Mean death rate x (2008-2013)	0.44*	0.55**	0.58***	0.57***	0.65**
	(0.26)	(0.22)	(0.22)	(0.15)	(0.32)
Controls	A	B	C	C	D
New Orleans sample only	Yes	Yes	Yes	No	Yes
Dep. var. mean	5.95	5.95	5.95	6.31	6.50
Observations	201,863	201,863	201,810	511,906	94,384
R-squared	0.00	0.04	0.05	0.06	0.07

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) clustered by beneficiary's 2006 HSA. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. Controls are as follows: A includes year and baseline zip code fixed effects. B adds 5-year-age-bins-by-gender-by-race fixed effects to the controls in A. C controls for 1-year-age-bins-by-gender-by-race fixed effects, and and allows year fixed effects to vary by all unique 5-year-age-bins-gender-race combinations as well as by baseline zip code. D includes all controls in C, as well as controls for 2004 Medicare spending and chronic conditions.

Table 6: Predictors of leaving New Orleans

	(1)	(2)	(3)	(4)
Black	0.071	0.066	0.078*	0.076*
	(0.043)	(0.044)	(0.044)	(0.044)
Male	-0.028***	-0.028***	-0.026***	-0.024***
	(0.004)	(0.004)	(0.005)	(0.005)
75 and older	-0.008	-0.007	-0.029***	-0.026***
	(0.007)	(0.007)	(0.010)	(0.009)
Below median income	0.055***	0.030***	0.033***	0.033***
	(0.007)	(0.008)	(0.008)	(0.008)
Katrina flood level, feet		0.019***	0.019***	0.019***
		(0.003)	(0.003)	(0.003)
Alzheimer's/dementia			0.046***	0.043***
			(0.014)	(0.014)
End-stage renal disease			0.061***	0.062***
			(0.022)	(0.022)
Heart disease and stroke			0.004	-0.002
			(0.004)	(0.004)
Blood and kidney disease			0.007*	0.005
			(0.004)	(0.004)
Musculoskeletal				-0.006
				(0.004)
Respiratory disease				0.046***
				(0.007)
Cancer				-0.037***
				(0.012)
Diabetes				0.011**
				(0.005)
Other				0.008**
				(0.004)
Observations	72,072	72,072	32,471	32,471
R-squared	0.149	0.154	0.162	0.163
Dep. var. mean	0.449	0.449	0.472	0.472

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) clustered by beneficiary's 2006 HSA. Dependent variable is an indicator equal to 0 if the New Orleans beneficiary was not living in his or her 2004 HSA in 2006. All regressions include baseline 5-digit-zip-code fixed effects.

Table 7: Predictors of New Orleans movers' local mortality rates

	(1)	(2)	(3)	(4)
Black	0.017	0.017	0.020	0.019
	(0.027)	(0.027)	(0.027)	(0.027)
Male	0.013***	0.013***	0.004	0.004
	(0.004)	(0.004)	(0.006)	(0.006)
75 and older	-0.004	-0.004	-0.008	-0.007
	(0.006)	(0.006)	(0.008)	(0.008)
Below median income	0.006	0.004	0.002	0.002
	(0.006)	(0.006)	(0.008)	(0.008)
Katrina flood level, feet		0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)
Alzheimer's/dementia			0.032**	0.032**
			(0.014)	(0.014)
End-stage renal disease			0.009	0.007
			(0.019)	(0.019)
Heart disease and stroke			0.013**	0.013**
			(0.006)	(0.006)
Blood and kidney disease			-0.018***	-0.018***
			(0.006)	(0.006)
Musculoskeletal				-0.005
				(0.006)
Respiratory disease				0.001
				(0.008)
Cancer				-0.007
				(0.010)
Diabetes				0.005
				(0.007)
Other				-0.004
				(0.005)
Observations	30,785	30,785	14,671	14,671
R-squared	0.009	0.010	0.014	0.015
Dep. var. mean	6.279	6.279	6.278	6.278

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Standard errors (in parentheses) clustered by beneficiary's 2006 HSA. Dependent variable is the average mortality rate in New Orleans movers' 2006 HSA. All regressions include baseline 5-digit-zip-code fixed effects.

A Online Appendix

Definition of chronic conditions

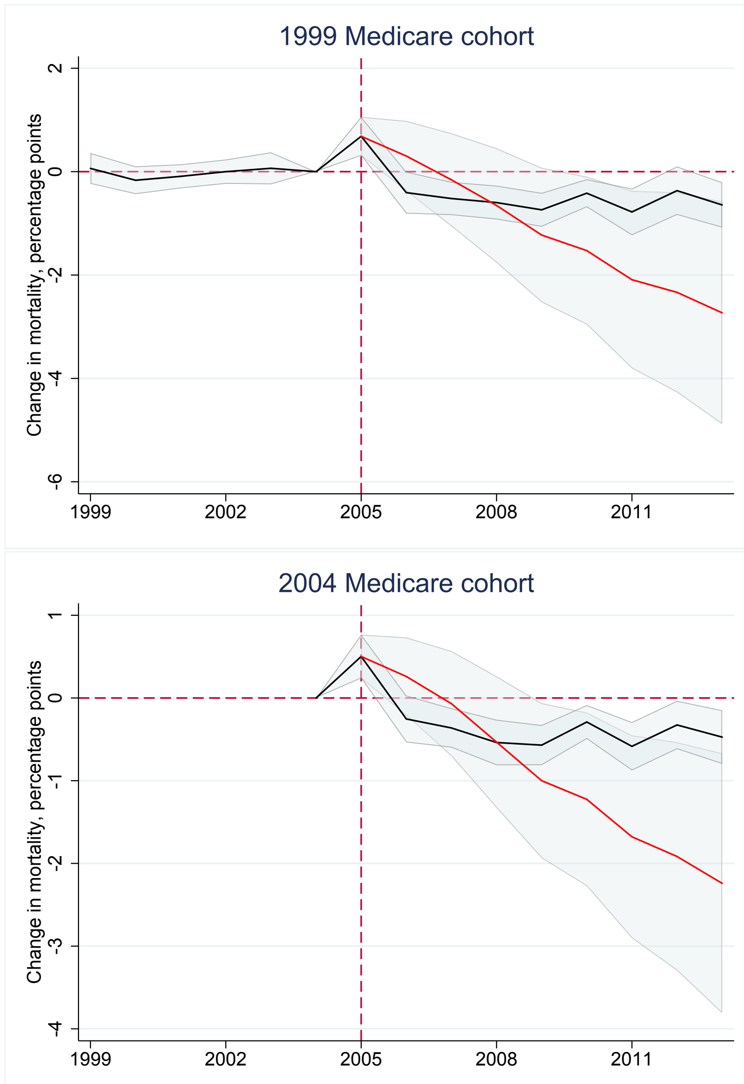
1. Heart disease and stroke: acute myocardial infarction, atrial fibrillation, heart failure, ischemic heart disease, hypertension, stroke/transient ischemic attack
2. Respiratory disease: chronic obstructive pulmonary disease, asthma
3. Blood and kidney disease: chronic kidney disease, anemia, hyperlipidemia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

Throughout, we use the end-of-year flags from 2004 to determine whether an individual has a particular condition. For more details on how the chronic conditions flags are defined, see the CMS Chronic Conditions Data Flags Data Dictionary [17]

[17] Available from https://healthcaredelivery.cancer.gov/seermedicare/medicare/chronic-conditions-flags.pdf.

Appendix Figures and Tables

Figure A.1: Effect of Hurricane Katrina on elderly mortality, using a 50 percent US sample as a control



Notes: Black line shows estimates from Equation 2 for the Medicare cohort indicated above each subplot. Red line shows the implied change in cumulative mortality probability (Equation 3). Grey shaded areas represent 95 percent confidence intervals based on standard errors that are clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and confidence intervals have been scaled by 100.

Figure A.2: The effect of Hurricane Katrina on elderly mortality, other cohorts

Notes: Figure shows estimates and 95 percent confidence intervals from Equation 2 for Medicare cohort indicated above each subplot. Standard errors clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Red line shows the implied change in cumulative mortality probability. Coefficients and confidence intervals have been scaled by 100.

Table A.1: Point estimates for Figure 3

	(1)	(2)	(3)	(4)
	Mortality rate		Cumulative mortality	
	1999 cohort	2004 cohort	1999 cohort	2004 cohort
1999	-0.00			
	(0.16)			
2000	-0.22			
	(0.14)			
2001	-0.09			
	(0.12)			
2002	-0.02			
	(0.12)			
2003	0.08			
	(0.16)			
2005	0.78***	0.55***	0.78***	0.55***
	(0.20)	(0.14)	(0.20)	(0.14)
2006	-0.37*	-0.25*	0.44	0.32
	(0.21)	(0.15)	(0.35)	(0.24)
2007	-0.51***	-0.36***	-0.01	-0.00
	(0.17)	(0.13)	(0.46)	(0.33)
2008	-0.53***	-0.48***	-0.44	-0.41
	(0.17)	(0.14)	(0.57)	(0.41)
2009	-0.63***	-0.52***	-0.92	-0.83*
	(0.17)	(0.13)	(0.67)	(0.49)
2010	-0.29*	-0.25**	-1.12	-1.02*
	(0.15)	(0.11)	(0.74)	(0.55)
2011	-0.55**	-0.45***	-1.51*	-1.36**
	(0.24)	(0.15)	(0.88)	(0.64)
2012	-0.14	-0.19	-1.60	-1.50**
	(0.24)	(0.15)	(1.00)	(0.72)
2013	-0.40*	-0.38**	-1.84*	-1.75**
	(0.23)	(0.17)	(1.11)	(0.82)
Dep. var. mean	6.27	5.48	6.27	5.48
Observations	12,843,079	10,174,387	12,843,079	10,174,387

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Table reports estimates of equation (1) from the main text. Standard errors (in parentheses) clustered by beneficiary's baseline zip code. Dependent variable indicated at top of column. All regressions include zip code and year fixed effects.

Table A.2: Point estimates for Figure 4

	(1) 1999 cohort	(2) 2004 cohort
1999	1.50**	
	(0.62)	
2000	1.08**	
	(0.44)	
2001	0.68	
	(0.42)	
2002	0.81***	
	(0.25)	
2003	0.42***	
	(0.14)	
2005	37.86***	41.55***
	(3.89)	(4.31)
2006	37.51***	40.47***
	(4.04)	(4.33)
2007	33.98***	36.40***
	(3.66)	(3.86)
2008	29.96***	31.88***
	(3.10)	(3.17)
2009	28.10***	29.84***
	(2.89)	(2.94)
2010	26.28***	27.90***
	(2.69)	(2.70)
2011	24.77***	26.31***
	(2.55)	(2.54)
2012	23.70***	25.06***
	(2.46)	(2.40)
2013	22.81***	24.07***
	(2.32)	(2.27)
Dep. var. mean	7.47	8.57
Observations	11,900,373	10,168,746

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Table reports estimates of equation (1) from the main text. Standard errors (in parentheses) clustered by beneficiary's baseline zip code. Dependent variable is an indicator for whether the beneficiary was living in a different HSA in a given year compared to 2004 and is missing for beneficiaries who died prior to that year. All regressions include zip code and year fixed effects.

Table A.3: Point estimates for Figure A1

	(1)	(2)	(3)	(4)
	Mortality rate		Cumulative mortality	
	1999 cohort	2004 cohort	1999 cohort	2004 cohort
1999	0.06			
	(0.15)			
2000	-0.17			
	(0.14)			
2001	-0.09			
	(0.12)			
2002	0.00			
	(0.12)			
2003	0.06			
	(0.16)			
2005	0.68***	0.50***	0.68***	0.50***
	(0.19)	(0.13)	(0.19)	(0.13)
2006	-0.41**	-0.25*	0.30	0.26
	(0.21)	(0.14)	(0.35)	(0.24)
2007	-0.52***	-0.36***	-0.16	-0.07
	(0.16)	(0.12)	(0.46)	(0.32)
2008	-0.60***	-0.54***	-0.65	-0.53
	(0.17)	(0.14)	(0.56)	(0.41)
2009	-0.74***	-0.57***	-1.23*	-1.00**
	(0.17)	(0.12)	(0.66)	(0.48)
2010	-0.42***	-0.29***	-1.53**	-1.23**
	(0.14)	(0.10)	(0.73)	(0.54)
2011	-0.78***	-0.58***	-2.09**	-1.68***
	(0.23)	(0.15)	(0.88)	(0.63)
2012	-0.37	-0.33**	-2.33**	-1.92***
	(0.24)	(0.15)	(0.99)	(0.70)
2013	-0.64***	-0.47***	-2.73**	-2.24***
	(0.22)	(0.17)	(1.10)	(0.80)
Dep. var. mean	5.95	5.16	5.95	5.16
Observations	206,069,664	171,138,064	206,069,664	171,138,064

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Table reports estimates of equation (1) from the main text. Standard errors (in parentheses) clustered by beneficiary's baseline zip code. Dependent variable indicated at top of column. All regressions include zip code and year fixed effects.

Table A.4: Point estimates for Figure A2

	(1) 2000 cohort	(2) 2001 cohort	(3) 2002 cohort	(4) 2003 cohort
2000	-0.22			
	(0.15)			
2001	-0.11	-0.12		
	(0.13)	(0.13)		
2002	-0.07	-0.13	-0.09	
	(0.11)	(0.10)	(0.11)	
2003	0.05	-0.04	-0.03	-0.00
	(0.15)	(0.14)	(0.15)	(0.12)
2005	0.77***	0.70***	0.67***	0.62***
	(0.18)	(0.15)	(0.14)	(0.13)
2006	-0.32*	-0.31*	-0.24	-0.23
	(0.19)	(0.16)	(0.16)	(0.15)
2007	-0.47**	-0.48***	-0.37**	-0.34**
	(0.18)	(0.16)	(0.16)	(0.14)
2008	-0.53***	-0.57***	-0.50***	-0.48***
	(0.16)	(0.14)	(0.16)	(0.15)
2009	-0.58***	-0.58***	-0.56***	-0.53***
	(0.15)	(0.14)	(0.13)	(0.12)
2010	-0.25**	-0.31***	-0.24**	-0.25**
	(0.12)	(0.11)	(0.12)	(0.11)
2011	-0.51***	-0.48***	-0.44**	-0.42***
	(0.19)	(0.18)	(0.18)	(0.15)
2012	-0.16	-0.18	-0.19	-0.18
	(0.22)	(0.19)	(0.19)	(0.18)
2013	-0.43**	-0.46**	-0.39*	-0.32*
	(0.22)	(0.21)	(0.20)	(0.19)
Dep. var. mean	6.11	5.96	5.81	5.65
Observations	12,428,118	11,879,365	11,379,931	10,800,965

Significance levels: * 10 percent, ** 5 percent, *** 1 percent. Table reports estimates of equation (1) from the main text. Standard errors (in parentheses) clustered by beneficiary's baseline zip code. Dependent variable is an indicator equal to 0 if the beneficiary is alive during the entire calendar year and equal to 1 if a beneficiary died in a given year. Coefficients and standard errors have been scaled by 100. All regressions include zip code and year fixed effects.