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Reply to "Alive and Kicking: Mortality of New Orleans Medicare Enrollees After Hurricane Katrina"

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LINK TO ABSTRACT

In the paper "Does When You Die Depend on Where You Live? Evidence from Hurricane Katrina," published in the November 2020 issue of the *American Economic Review* (Deryugina and Molitor 2020—hereafter "DM"), we used administrative Medicare data to estimate the short- and long-run mortality effects of Hurricane Katrina on elderly and long-term disabled victims who were initially living in New Orleans. We found that despite a substantial mortality increase in the year of the hurricane, 2005, the cumulative probability of dying in the longer run was *lower* among Hurricane Katrina victims than among several comparison groups. This surprising result is apparent in plots of raw mortality rates, difference-in-difference analyses using a variety of comparison groups, estimates from the synthetic control method, and survival model analyses.

To explain why the mortality of Hurricane Katrina victims decreased in the long run, we compared the mortality of victims who moved to higher- versus lower-mortality destination regions. We showed that these movers' ex ante predicted mortality was unrelated to the local mortality rate in the destination region, but their realized mortality was highly correlated with the destination mortality rate, demonstrating that place has a causal effect on life expectancy. Using a back-of-the-

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envelope calculation, we showed that the estimated place effect combined with the average decline in local mortality among the victims can account for most of the mortality decline among the Hurricane Katrina victims.

Robert Kaestner (2021—hereafter "RK") questions both our finding of a mortality decrease among Hurricane Katrina victims and our analysis of mortality among survivors who left New Orleans in the aftermath of the hurricane. While many of the concerns he raises are ex ante valid, most of them have testable implications that were already addressed in the published paper. Below, we respond to RK's concerns, restating and elaborating on our original findings. Additionally, we show that the back-of-the-envelope exercise offered by RK to show that our estimates imply a decrease in mortality among New Orleans stayers is incorrect and does not fit the context.

Non-parallel trends

The first issue raised by RK is that because mortality in New Orleans was higher than other places even prior to Hurricane Katrina, residents of New Orleans in 2004—our primary sample of treated individuals—are those who survived relatively harsh initial conditions and therefore might be positively selected on health relative to control individuals with similar demographics. If so, the treated group would have experienced lower mortality rates even absent the hurricane, relative to control groups who faced more favorable initial conditions. In this case, a difference-in-differences estimate would produce what appears to be a mortality reduction due to the hurricane, even if there were no effect whatsoever. Of course, selection and estimator bias could just as well go the other way. For example, individuals who survived the harsh conditions in New Orleans prior to Katrina may have suffered health scarring, such as through earlier onset and progression of chronic conditions, leaving them *negatively* selected on health relative to others of similar demographics.

We completely agree that correctly estimating the mortality effects of Hurricane Katrina hinges on selecting a comparison group whose mortality rates parallel the counterfactual mortality among New Orleans victims. That is why our AER paper does much to address the validity of various control groups and the plausibility of the parallel-trends assumption. Our preferred specification compares mortality rates among cohorts from New Orleans versus cities that had similar baseline demographic and economic conditions and, it turns out, also had above-average mortality rates pre-Katrina. Concerns about differential mortality trends also motivate our use of pre-2004 cohorts in some specifications: if differential pre-hurricane mortality were a concern, we would expect New Orleans mortality rates to diverge from the control group even prior to the hurricane. Yet we see no evidence of this, even in the 1992 cohort, which we observe for over a decade prior to the hurricane (DM, 3619 Figure 4).

We also, in the Online Appendix to DM (**link**), report results where the control group consists of beneficiaries from the entire United States (DM, A-23 Figure A.9) or is constructed from other high-mortality regions, which would arguably be selected similarly to the New Orleans cohort (DM, A-19 Figure A.5). We see mortality rates that are nearly indistinguishable from New Orleans in both levels and trends prior to the hurricane but diverge following the hurricane, implying mortality decreases that are very similar to what we estimate using our preferred control group. Overall, the evidence in the paper suggests that the type of positive selection posited by RK (or another type of selection that would cause the parallel trends assumption to be violated) is not present in our setting.

Non-random moves

The second issue raised by RK is that movers did not choose where to locate at random and that we "overturn standard economic theory" to "support the argument that moving is random" (RK, 46). In fact, our paper does not claim that those leaving New Orleans chose their destination at random. We explicitly acknowledge that "little systematic information is available on how victims chose where to relocate in the longer run" (DM, 3607). In Section IV.C (DM, 3620–3629), we also explicitly acknowledge the presence of some sorting along dimensions other than the local mortality rate (e.g., local pollution and social capital levels).

Moving to destinations at random is a stronger condition than needed for estimating causal effects of place on a specific outcome, like mortality. As we state in the introduction, "The relationship between local and migrant mortality describes the causal effect of place on individual mortality under the assumption that baseline mortality risk among those who move is uncorrelated with mortality rates in the destination region" (DM, 3604). In support of this assumption, we find that movers' observable risk factors, including demographics, medical spending, and chronic conditions, and predicted mortality based on these factors are largely uncorrelated with mortality rates in the destination region (DM, 3621–3622). Of course, this assumption could still be violated if there are unobservable mortality risk factors that are uncorrelated with the observed factors but are correlated with destination mortality. The scope for such a violation is higher when observables explain a small share of risk. Related to this point, RK speculates that "these characteristics likely explain a small portion of mover mortality (not reported by

DM)" (RK, 46). While reporting the R-squared for a binary outcome like mortality would not be particularly informative, DM (A-28 Figure A.14) does show that the rich set of risk factors we include in our prediction model yields out-of-sample "mortality predictions that are strongly correlated with realized mortality among the New Orleans movers" (DM, 3621).

Movers versus stayers

RK also takes issue with the fact that we never compare the mortality of New Orleans movers to the mortality of New Orleans stayers. The reason we do not do this is because the New Orleans movers are quite different on most of the observable characteristics from both the average 2004 New Orleans Medicare beneficiary (DM, 3609 Table 1) and from survivors of the hurricane who remain in New Orleans (DM, A-35 Table A.7). Thus, even with extensive controls, any direct comparison of movers and stayers is likely to be highly susceptible to the influence of unobservable differences between them.

Instead, our estimate of the share of the net mortality decline that can be explained by moving is based on a calculation that combines the average change in the local mortality among movers with the estimated effect of a change in the local mortality rate on one's own mortality. This is what we mean when we say, in the article abstract, "Migration to lower-mortality regions explains most of this survival increase" (DM, 3602). As we explicitly acknowledge and discuss in the paper, there may be other factors affecting mortality in our sample, among movers, among stayers, or both (DM, 3628).

In an attempt to infer mortality effects among stayers from our estimates, RK presents a back-of-the-envelope calculation and claims that it implies Hurricane Katrina caused a mortality decline among stayers. The calculation begins with equation (1):

$$M_{pre} = \alpha M_{pre}^{stayers} + (1 - \alpha) M_{pre}^{movers},$$

where M_{pre} is the pre-hurricane mortality rate of the New Orleans cohort, α is the share of stayers, and $1 - \alpha$ is the share of movers. The variables $M_{pre}^{stayers}$ and M_{pre}^{movers} represent the pre-hurricane mortality of stayers and movers, respectively.

This equation is ill-defined and unhelpful for evaluating our results. First, the pre-Katrina New Orleans cohort is not composed only of would-be movers and stayers—categories that by their nature require surviving the hurricane—but also includes individuals who died prior to the beginning of 2006 (the point at which we classify victims as movers or stayers, as described on pages 3610 and 3614 of DM). Second, because it is impossible to classify someone who has died prior to or in the immediate aftermath of the hurricane as a mover or a stayer, $M_{pre}^{stayers}$ and M_{pre}^{movers} are both mechanically zero. These issues invalidate the back-of-the-envelope exercise for its intended purpose. In addition, RK's definition of movers (and thus his calculated α) does not match the definition used in our paper (DM, 3614).

Age-by-year fixed effects

To estimate the long-run mortality effects of Hurricane Katrina on its victims, in the paper we measure mortality outcomes of cohorts from New Orleans and compare these to mortality outcomes of control cohorts. Both the treatment and control cohorts age together over time. Because our primary estimating strategy does not include age-by-year fixed effects, RK argues that this approach will "generally fail to account for the fact that people of different ages, races, or sexes will have different probabilities of dying as time goes by" (RK, **abs.**). He states, "The inclusion of age-by-year fixed effects is clearly the appropriate model" (RK, 41). Yet such a model is not clearly superior to our primary specification, nor does the evidence support using age-by-year fixed effects to estimate the mortality effects of Hurricane Katrina.

Age-by-year fixed effects essentially match individuals from the treatment group to individuals of the same age in the comparison group. This may be appropriate in some settings, but if a 65-year-old in New Orleans is in worse health than a 65-year-old in other regions, someone older than 65 might be a better control. Ultimately, a valid control group is not necessarily one with demographics that are statistically identical to the control group but one whose mortality rate would have evolved in parallel with that of the New Orleans cohort absent the hurricane.

RK notes that when we control for all combinations of age, race, sex, and year in the 1992 and 1999 cohorts, mortality prior to Hurricane Katrina rises somewhat faster among the New Orleans cohort compared to the controls. RK appears to misinterpret this positive trend as demonstrating positive selection on health (i.e., that New Orleans residents are healthier than others of the same age, race, and sex). Actually, the positive mortality trend indicates *negative* selection on health: prior to Hurricane Katrina, mortality rates among the New Orleans cohort are increasing somewhat with respect to others of the same age, sex, and race. Thus, in our context, the inclusion of fixed effects for all combinations of age, race, sex, and year appears to worsen the validity of the counterfactual, suggesting that estimates from specifications that do not include such fixed effects are more reliable.

RK also points out that some of the post-Katrina estimates for the 1992 and

1999 cohorts are not statistically significant after controlling for all combinations of age, race, sex, and year. However, the presence of a small but positive pre-trend implies that post-period estimates will be upwardly biased, yielding estimated mortality decreases that are smaller than the true mortality decreases. Additionally, as we note, "The 1992 and 1999 cohorts…may only partially capture Hurricane Katrina's impact on Medicare victims, as about two-thirds (one-third) of individuals in the 1992 (1999) cohort had moved away or died before 2005" (DM, 3612–3613). Furthermore, the elderly in the 1992 (1999) Medicare cohort were at least 77 (70) by the time Hurricane Katrina struck. It should therefore not be surprising that the post-Katrina estimates are noisier for these cohorts, though as Table 2 shows, the 2006–2013 mortality declines are jointly highly significant for the 1999 cohort, even with extensive controls (DM, 3619).

Heterogeneity

Following Hurricane Katrina, residents of New Orleans who were black or under the age of 65 were more likely to move than others. Yet because these groups did not experience significantly larger mortality declines after the disaster, RK takes this as evidence "that moving was not the primary cause of the decline in mortality reported" (RK, 40).

Our paper does not claim to estimate the mortality effect of moving. Rather, we estimate how exposure to the local conditions in a place shape mortality, net of any moving effects. To do so, we compare a mover's mortality to that of *other movers* as a function of the local mortality rate in his or her destination region. Such a comparison "will control for any mortality effects that are common to all migrants" (DM, 3614). For example, it is possible for the long-run mortality of a particular group of movers to increase, on average, as a result of the hurricane even while some of the group members experience a *relative* mortality decline as a result of moving to lower-mortality regions.

How selection into moving among a demographic group maps to subsequent mortality declines also depends on whether there are heterogeneous treatment effects. A demographic group with a greater propensity to move than another demographic group may not experience larger average mortality declines if the additional movers are also those for whom place effects are smallest. Finally, the effect of place itself may vary by demographic group, which RK acknowledges (RK, 40 n.4). As we mention in the paper, "place may have a larger impact for black individuals, who made up a large share of the New Orleans victims and were also disproportionately likely to move after the hurricane, than for other races" (DM, 3629).

Local correlates of movers' mortality

In addition to examining the correlation between movers' mortality rates and destination region mortality, our AER paper reports how movers' mortality correlates with other destination characteristics. RK critiques these estimated correlations as though they are meant to capture a causal relationship between a particular destination characteristic and movers' mortality. He goes on to state "Imbuing estimates of these correlations with causal meaning is inappropriate" (RK, 46).

It is not clear to us what prompted RK to raise this point. Nowhere does the published manuscript claim that these correlations reflect the causal effect of a particular local characteristic. By contrast, as we state in the paper, "we emphasize that the estimate reflects the causal effect of the given characteristic itself only if the characteristic is uncorrelated with any other local attribute that also affects movers' mortality. Because each region is a bundle of many, often correlated, characteristics, these results should be viewed as suggestive of what actually determines place effects" (DM, 3625).

Conclusion

In his comment, RK expresses skepticism about the effects of Hurricane Katrina on the long-run mortality of its victims documented in our AER paper. He raises several potential threats to the validity of our analysis that we previously addressed and reiterates some points of caution that we emphasized in our published paper. In his criticism, RK himself makes several assumptions—such as that hurricane survivors who did not move away from New Orleans must have suffered mortality increases—that seem to be based on strong priors rather than empirical evidence.

While our finding that Hurricane Katrina reduced mortality among New Orleans residents may be surprising, we are not the first to document that Hurricane Katrina improved some outcomes for its victims. Deryugina, Kawano, and Levitt (2018) and Groen, Kutzbach, and Polivka (2020) show that Hurricane Katrina *increased* its victims' earnings in the longer run. Sacerdote (2012) documents improved test scores among students displaced from New Orleans. Beyond Hurricane Katrina, Ruhm (2000) shows that mortality *decreases* during recessions. No study is without its limitations, but we are grateful for the opportunity to reiterate the ways in which our study adds new evidence on how Hurricane Katrina decreased mortality among its elderly and long-term disabled victims and that moving to lower-mortality places played an important role in these dynamics.

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Go to archive of Comments section
Go to March 2021 issue

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