NBER WORKING PAPER SERIES

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Working Paper 29622
http://www.nber.org/papers/w29622

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2021

We are grateful for insights and suggestions from Joe Aldy, Glenn Blomquist, Greg Caetano, Tatyana Deryugina, Catie Hausman, Jonathan Ketcham, Edson Severnini, Aradhya Sood, and seminar audiences at the ASSA Annual Meetings, the UEA North American Meeting, the AERE Annual Meeting, and the AREUEA Annual Meeting. This study analyzed secondary data on human subjects under Arizona State University IRB ID STUDY00001990. This study was financed in part by the Coordenacao de Aperfeicoamento de Pessoal de Nivel Superior - Brasil (CAPES) - Finance Code 001. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed additional relationships of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w29622.ack

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NBER Working Paper No. 29622
December 2021
JEL No. H0,I0,Q0,R0

ABSTRACT

We provide the first evidence that spatial variation in all-cause mortality risk is capitalized into US housing prices. Using a hedonic framework, we recover the annual implicit cost of a 0.1 percentage-point reduction in mortality risk among older Americans and find that this figure is both relatively low and decreasing in age, from $1,346 for a 67 year old to $246 for an 87 year old. These estimates are one-fifth of the size of comparable estimates found in the labor market, suggesting that the housing market provides an alternative, substantially cheaper channel to reducing mortality risk.

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1 Introduction

Recent studies of the US Medicare population show that where an individual lives after the age of 65 causally affects their remaining life expectancy (Deryugina and Molitor, 2020; Finkelstein et al., 2021). While the precise mechanisms are not fully understood, the idea that mortality risk varies causally across residential locations, combined with the fact that individuals are free to move across locations, implies that mortality risk may be capitalized into residential property values. However, any potential capitalization effects may be dampened by frictions associated with the availability of information and the costs associated with migration (Chetty et al., 2020). The rate of capitalization answers an important question: How much would an older American need to pay via the housing market to reduce their mortality risk? We answer this question using national data describing the US Medicare population (including migration and mortality), the stock of houses, and location-specific amenities to estimate the implicit cost of reducing mortality risk.

We estimate the rate at which housing markets capitalize location-based mortality risk using a modern version of hedonic property-value techniques developed in Rosen (1974) and Roback (1982). We start by constructing our variable of interest following Finkelstein et al. (2021). Specifically, we apply the identifying assumptions and econometric methods from that paper to Medicare data (describing 7.2 million older Americans aged 65 and above) to recover estimates of age- and location-specific mortality risk. Following Finkelstein et al. (2021), we refer to the resulting estimates as measures of "causal" mortality risk. We merge these causal mortality-risk estimates with Census data describing house values and characteristics and with multiple datasets describing an extensive set of location-based amenities.

Our hedonic estimation strategy addresses two key challenges in identifying the capitalization rate of causal mortality risk. The first challenge is that location-specific causal mortality risk may be measured with error. To address this, we employ an Instrumental-Variables (IV) estimation approach and instrument for causal mortality risk using the raw, population-based measures of location-specific mortality reported by the U.S. Centers for

Disease Control and Prevention. As these measures are simple population statistics, they are unlikely to embed measurement error.

The second challenge is the potential for correlation between mortality and unobserved determinants of housing prices. In particular, location-specific amenities that are associated with mortality, such as environmental quality, may affect the quality of life and be directly capitalized into housing prices. The difficulty in fully observing and controlling for amenities may therefore lead to confounding through capitalization of amenities that households observe but researchers do not. To overcome this, we narrow our focus to within-state variation in mortality risk and housing prices and control for an extensive set of location-specific amenities. Thus, our estimates of the implicit costs of reducing mortality risk are purged of differences in housing prices that may be explained by observed physical housing characteristics and location-specific amenities that affect housing prices directly. The amenity controls include 18 variables measuring climate, air pollution, crime, school quality, transportation, and cultural amenities. These controls absorb approximately two-thirds of the variation in causal mortality risk. The residual variation in causal mortality risk that we use for identification may be attributed to location features that are less likely to be capitalized into housing prices through channels other than mortality risk (Muehlenbachs et al., 2015; Ma, 2019; Hausman and Stolper, 2020; Christensen and Timmins, 2021). Finally, we use the insights from Altonji et al. (2005), Nevo and Rosen (2012), Altonji et al. (2015), and Oster (2019) and include an analysis that assesses the direction of any remaining potential biases that could arise from residential amenities that buyers and sellers observe, but that researchers do not.

We find that causal mortality risk is indeed capitalized into housing prices. Our IV estimates suggest that the marginal cost to reduce annual mortality risk by 0.1 percentage points (pp) is approximately $575 (year 2010 dollars) at age 77. A 0.1pp reduction in annual mortality risk is approximately equal to the within-state standard deviation of mortality risk across locations. We find this implicit cost to be declining in age, from a high of $1,346 at age 67 to a low of $246 at age 87. This is a result of the fact that the spatial variation in mortality risk is increasing in age while the spatial variation in housing prices is not varying in age, making the implicit cost of mortality-risk reduction lowest among the oldest individuals. As discussed below, this implicit cost is comparable to that found in medical-spending-based estimates but only one-fifth of typical wage-based estimates.

A key identifying assumption is that, conditional on our extensive set of controls, the factors generating the variation in our instrument do not affect housing prices through channels other than causal mortality risk. As we cannot test this exclusion restriction directly, we build on the techniques from Altonji et al. (2005), Nevo and Rosen (2012), Altonji et al. (2015), and Oster (2019) to predict the sign of the potential bias under the assumption that selection on observables is informative about selection on unobservables. Specifically, we examine how our IV estimates evolve as we incrementally add each of 18 individual covariates describing location-specific amenities in random order, generating 262,144 different specifications of our model. The results from this analysis suggest that any remaining bias in our estimate of the cost of mortality risk reduction is likely to be positively signed and small in magnitude.

Our findings add to three distinct literatures. First, our study is related to the hedonic property-value literature on amenity capitalization. Portney (1981) was the first to estimate the housing-market capitalization of mortality risk using a hedonic model in a study of the impacts of air pollution in Pennsylvania. Subsequent studies have estimated the capitalization effects of other specific mortality risks in specific areas, such as lead exposure in North Carolina (Billings and Schnepel, 2017), violent conflict in Northern Ireland (Besley and Mueller, 2012), and pediatric leukemia in Nevada (Davis, 2004). Our study adds to this literature by providing the first nationwide analysis of mortality-risk capitalization, the first analysis of mortality risk among senior citizens, and the first analysis to focus on a broadly inclusive measure of causal mortality.

Second, our focus on location-based mortality risk relates to the literature on how an individual's residential location affects the evolution of their health, wealth, and human capital (Cutler and Glaeser, 1997; Kahn, 2004; Kling et al., 2007; Bayer et al., 2008; Graff Zivin and

Neidell, 2013; Deschenes, 2014; Barreca et al., 2015; Chetty and Hendren, 2018a,b; Aliprantis and Richter, 2020; Deryugina and Molitor, 2020; Finkelstein et al., 2021; Caetano and Macartney, 2021; Couillard et al., 2021; Deryugina and Molitor, 2021). Two important studies in this literature, Deryugina and Molitor (2020) and Finkelstein et al. (2021), conclude that exposure to different residential environments after age 65 causes disparities in seniors' remaining life expectancies. We add to this literature by estimating the implicit cost of reducing mortality risk that older Americans face when choosing where to live.

Third, our study relates to the literature recovering the costs associated with reducing one's mortality risk, in which estimates are typically scaled to be interpreted as the marginal cost of avoiding one premature death from a statistical standpoint. One branch of this literature recovers the wage compensation for undertaking a higher risk of on-the-job death (Viscusi and Aldy, 2003; Costa and Kahn, 2004; Cropper et al., 2011; Kniesner et al., 2012; Lee and Taylor, 2019; Evans and Taylor, 2020; Banzhaf, 2021), with prevailing estimates exceeding \$6 million (year-2010 dollars) among workers aged 60 to 65 (Smith et al., 2004; Aldy and Viscusi, 2008). Another branch of this literature recovers the medical spending required to reduce mortality risk, with estimates of avoiding one statistical death mostly near or below \$1 million (year-2010 dollars) among individuals over the age of 65 (Hall and Jones, 2007; Doyle et al., 2015; Huh and Reif, 2017; Ketcham et al., 2020). In order to compare our findings with these figures, we rescale our estimates to measure the aggregate housing expenditure required to avoid one statistical death and find the implied figure to be approximately \$1.3 million at age 67. This figure is about one-fifth of wage-based estimates but similar in magnitude to medical-spending-based estimates. And, given the results of our incremental-addition-of-covariates analysis, these ratios may even be interpreted as upper bounds, further highlighting how much lower the implicit cost of reducing mortality is in the housing market, relative to the labor market.

It is important to note that our paper makes no claims about the welfare implications of capitalization effects. Specifically, we refrain from presenting a revealed-preference interpretation of capitalization effects as it is unclear what, exactly, households believe about their ability to adjust their own mortality risk through their choice of location. We leave questions regarding how capitalization effects relate to private and social benefits of reducing mortality risk to future research.

2 Data

2.1 Locations

We begin by defining location at the level of commuting zones (CZs), as defined by the U.S. Economic Research Service using 2000 Census data, for the 48 coterminous US states. Each CZ is a cluster of counties that approximate a local labor market and is similar in size to a metropolitan area. There are 709 distinct CZs in our data. To deal with the issue of sparsely-populated CZs, we follow the procedure in Finkelstein et al. (2021) (FGW) which aggregates CZs into 536 aggregate CZs (which we continue to refer to as CZs for simplicity).[1]

This CZ definition of location is well suited to measuring the housing-market capitalization of causal mortality risk.[2] A finer resolution of geography (e.g., a Census tract) would exacerbate any potential for measurement error, as individuals would likely spend a considerable amount of time outside of their home location, while a coarser resolution of geography (e.g., a state) would limit the scope to describe individuals' ability to adjust their mortality risk by moving reasonably short distances.

2.2 Location-Specific Empirical Mortality Rates

We use the "Multiple Cause of Death" data produced by the U.S. Centers for Disease Control and Prevention (CDC) to measure spatial variation in all-cause mortality. These data include

[1]The number of aggregate CZs in our study, 536, is slightly smaller than the 563 defined by Finkelstein et al. (2021) because we apply their methodology to a smaller (20%) sample of Medicare beneficiaries and we drop rural CZs where we do not observe any movers. The cumulative population of the rural CZs that we drop is less than fifty thousand people, or 0.01% of the US population.

[2]National hedonic studies typically partition the country into similar geographies such as metropolitan areas (Bayer et al. (2009)), counties (Blomquist et al. (1988)), or public-use microdata areas (Albouy et al. (2016)).

annual county-level mortality rates for each integer age from 65 to 84. For ages 85 and over, the data is right-censored. The underlying data on deaths are derived from the population of death certificates, while county population sizes are derived from Census data.

We refer to the location-specific mortality rates as "empirical" as they simply describe the mortality rates that are observed within locations. Panel (a) of Figure (1) shows the spatial variation in empirical mortality at age 77, which is approximately the average age of the over-65 population. The variation across locations is substantial. For example, moving from the 10th percentile to the 90th percentile in the distribution is associated with a 1.85pp increase in mortality risk over one year (a 63% percent increase).

To construct our empirical-mortality instrument, we synthesize these population-based empirical mortality data into a single instrumental variable, which we construct by taking a weighed average over a CZ's empirical mortality rates at each year, county, and integer age, weighting by Census-based measures for U.S. population shares by age.[3] We prefer this construction of the instrument because it combines all of the available CDC data, but we also show in Appendix D that our results are essentially unchanged if we replace our preferred instrument with CDC mortality rates observed at a single integer age (e.g., 65, 75, or 85+).

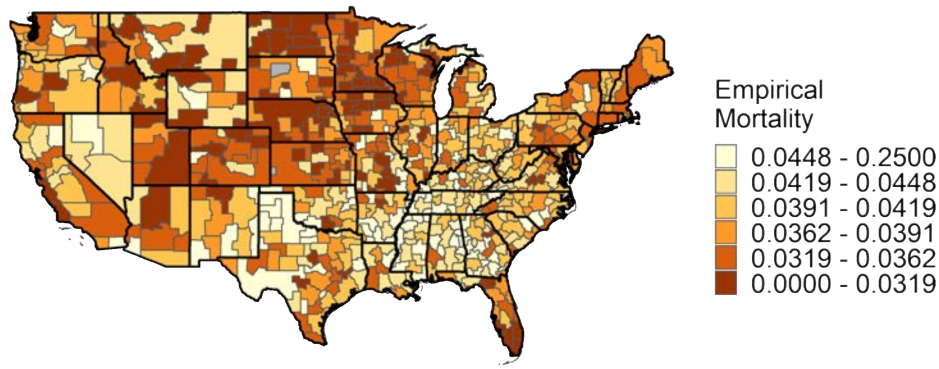
Importantly, the variation in empirical mortality rates cannot be interpreted as causal. While some of the variation may be caused by placed-based amenities that affect mortality risk, some of the variation may arise from healthier and wealthier people sorting themselves into higher amenity areas and living longer for reasons independent of their residential locations.

2.3 Location-Specific Causal Mortality Rates

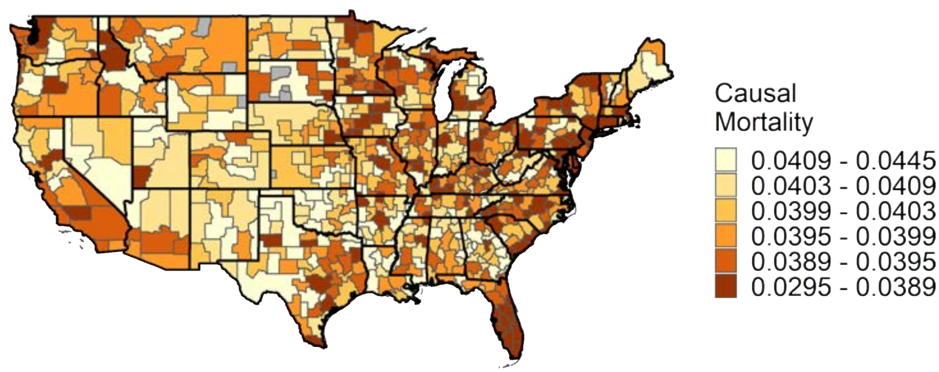
To disentangle location-based inputs to mortality risk from residential sorting, we follow FGW both in applying the estimator developed in that paper and in describing the resulting measures as providing the "causal" effect of location on mortality. The analysis in that paper

[3]Applying the same national weights to each CZ avoids compositional bias that would be introduced if we were to instead weight by CZ-specific population shares (e.g. higher mortality rates in CZs where people tend to survive to older ages).

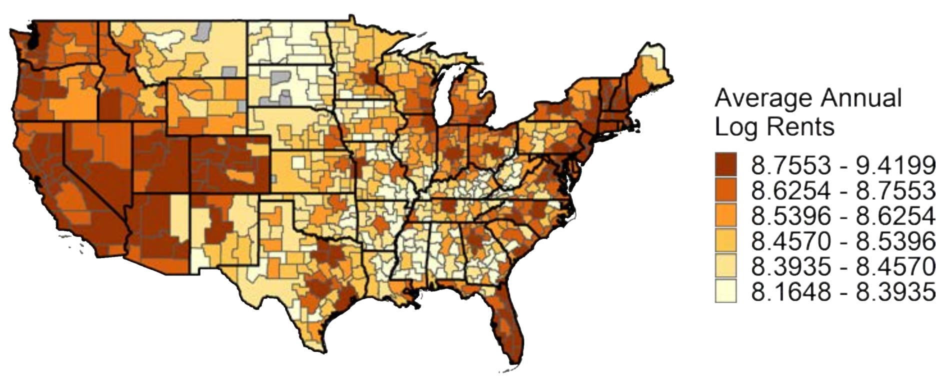
Figure 1: Spatial Variation in Empirical Mortality, Casual Mortality, and Housing Rents



(a) Empirical Annual Mortality Rates at Age 77 by Commuting Zone



(b) Causal Annual Mortality Rates at Age 77 by Commuting Zone



(c) Average Annual Log Rents by Commuting Zone uses Medicare data describing people over the age of 65 to recover CZ-specific mortality risks while controlling for differences in age, race, gender, medical spending, and clinical diagnoses of chronic medical conditions. The identification strategy leverages the variation in survival rates among movers who originate from the same location, but who move to different locations. A selection-correction procedure is used to account for any sorting based on unobserved health factors. We replicate the procedures described in that paper using administrative records for a 20% random sample of Medicare beneficiaries (7.2 million people) that we observe from 1999 through 2013 to estimate causal mortality for seniors aged 67 and older. Our results are very similar to the estimates reported in FGW. Details are provided in Appendix A.

Panel (b) of Figure (1) shows the spatial variation in causal mortality at age 77. There is less spatial variation in causal mortality rates than in empirical mortality rates, speaking to the role of residential sorting; our estimates imply that moving from the 10th percentile to the 90th percentile in the distribution across locations causes a 0.28pp increase in causal mortality risk over one year, compared with a 1.85pp increase in empirical mortality risk. The standard deviation of mortality risk across locations is 0.13pp.

We find substantial within-state variation in causal mortality rates across locations. For example, regressing age-77, location-specific causal mortality rates on a vector of state dummies yields an R^2 of 0.25 and a residual within-state standard deviation of mortality risk of 0.11pp. These results highlight the scope for differences in location-specific mortality rates to be capitalized into property values, and, importantly, may inform policy based on the ability of older Americans' to lower their mortality risk through within-state moves.

2.4 Housing Characteristics and Prices

Data describing housing characteristics and prices come from the 2000 Census Integrated Public Use Microdata Series (IPUMS) 5% sample. In this sample, locations are defined as Public Use Microdata Areas (PUMAs), which are contiguous areas comprised of approximately 100,000 individuals each. The data contain a total of 2,071 PUMAs which we aggregate into CZs.

For each of the 5.1 million properties in our sample, we observe the number of rooms, the number of bedrooms, the number of units in the structure, whether the property has a kitchen and indoor plumbing, and the age of the structure. We additionally observe the value of the property, if owner-occupied, or the gross rent, if renter-occupied.[4] Table B.1 reports summary statistics.

Panel (c) of Figure (1) shows the spatial variation in the log of annual gross rents across locations. Visual inspection of the three panels in Figure (1) suggests that mortality and log rents are correlated across space. More formally, the unweighted correlation coefficient between causal mortality at age 77 and log rents is -0.22. This unconditional negative correlation may be causal or may be driven by amenities that households value for reasons apart from mortality. For example, air pollution and extreme climates may reduce the quality of life in ways that are distinct from their effects on mortality. Thus, we compile data describing a large set of location-specific amenities.

2.5 Additional Location-Specific Amenities

We compile a rich set of location-specific amenities from several sources. We begin by following Diamond (2016) in defining a broad set of amenities at the county level. In that paper, the variables describe urban counties. For our analysis, we measure amenity variables for rural counties, too, and include additional climate variables for all counties. We aggregate all county-level amenities to the level of the CZ, weighting by population.

Our final set of 18 amenities is comprised of measures of summer temperature, winter temperature, precipitation, air pollution ($PM_{2.5}$), ozone concentrations, violent crime, property crime, student-teacher ratios in local public schools, interstate highway mileage, urban arterial mileage, number of urban rail stops, unemployment rate, share of residents with college degrees, and per-capita measures of government spending on parks, government spending on schools, number of movie theaters, number of restaurants and bars, and number

[4] We restrict the sample to those properties with at least one resident aged 20 or older who reports these figures.

of apparel stores. Appendix C documents the data sources and aggregation procedures and Table C.1 reports summary statistics.

3 Econometric Model

3.1 A Hedonic Price Function of Mortality Risk

Equation (1) specifies a hedonic price function where the dependent variable, $\log p_{hj}$, describes the log price associated with occupying house h in location j. We follow Bayer et al. (2007) in pooling data on rents and property values by measuring price, p_{hj}, as annual gross rent, if renter-occupied, and property value, if owner-occupied. Controlling for owner-occupancy with the indicator own_h allows the coefficients on house characteristics to be interpreted as annual measures of their implicit prices.[5] We partition house characteristics into three categories: x_h, a vector of physical characteristics, x_j, a vector of location-specific amenities, and m_j, location-specific causal mortality risk measured at an arbitrary reference age.

$$\log p_{hj} = \alpha_1 + \alpha_2 own_h + \alpha_3 x_h + \alpha_4 x_j + \beta m_j + \varepsilon_{hj}. \tag{1}$$

The reference age at which we measure mortality risk can be chosen without loss of generality, conditional on employing the Gompertz specification used in FGW. The Gompertz specification allows mortality risk to vary with location, age, and other characteristics:

$$m(age, j) = \exp\left(\delta \cdot age + \bar{\theta} + \gamma_j\right). \tag{2}$$

In this specification, δ is the estimated scaling parameter on age and $\bar{\theta}$ is the estimated index of other individual characteristics (race, gender, medical expenditures, and diagnoses of chronic medical conditions) that we scale to its national average value. γ_j is a CZ fixed effect that captures how location contributes to mortality risk. It is the only component of $m(age, j)$ that varies across locations. m_j is then simply defined as $m(age, j)$ evaluated at

[5] Our main econometric specification allows α_1 and α_2 to differ by US state.

the arbitrary reference age.

When embedded within the hedonic price function, the Gompertz specification has two important implications for estimating the implicit cost of reducing mortality risk. The first implication is that causal mortality for any given age is simply a scaled version of causal mortality for any other age: $m(age, j) = m(age', j) \cdot exp(\delta \cdot (age - age'))$. This means that the price regression needs to be estimated only once using mortality risk for an arbitrary reference age, as shown in Equation (1). The coefficient on mortality risk can then be scaled to recover the coefficient for any other age. For example, if β is defined for mortality risk at age 77, then the coefficient on mortality risk at age 67 is $\beta_{67} = \beta \cdot exp(\delta \cdot (77 - 67))$.

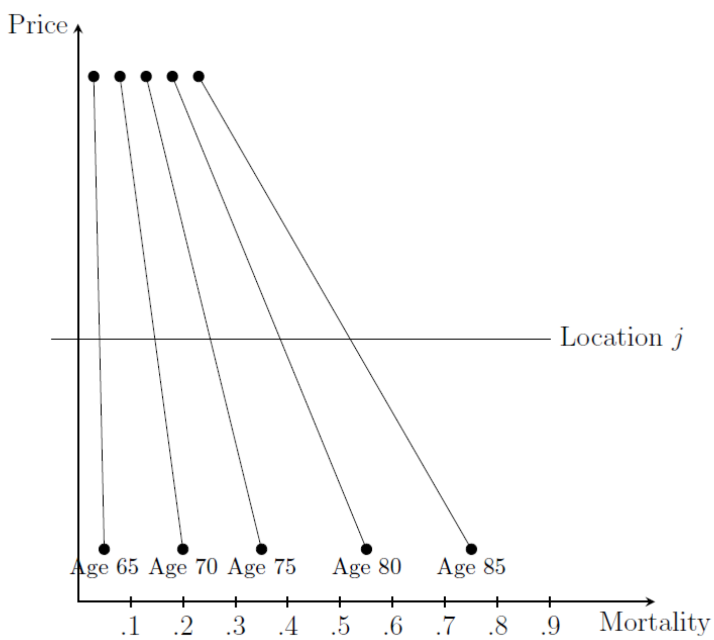


Figure 2: The Age-Mortality Curtain

The second implication of the Gompertz specification is that the marginal cost of reducing mortality risk will decrease in age if mortality risk increases in age (i.e., $\delta > 0$) and housing price decreases in mortality risk (i.e., $\beta < 0$). Figure (2) illustrates these mechanics. Each age line represents an available menu of mortality risk and associated implicit costs for a given age. For each age, a lower mortality risk is associated with a higher housing price, but the lines flatten as age increases so that they collectively trace out an age-mortality "curtain" in the price function as predicted by Portney (1981). This figure foreshadows our findings.[6] Intuitively, the marginal cost of reducing mortality risk decreases in age; while spatial variation in mortality risk increases in age, all individuals face the same menu of housing prices, regardless of age.

The marginal cost of mortality risk reduction (MCMRR) at a given age can be written in terms of hedonic parameters as:

$$MCMRR_{age} = -\frac{\beta_{age} \cdot \tilde{p}}{s}. \tag{3}$$

The numerator is the derivative of the log-linear price function in Equation (1) with respect to mortality risk, evaluated at an age-specific mortality risk and the mean annual housing cost over all houses, $\tilde{p}$, where $\tilde{p} = 1/H \sum_{h=1}^{H} p_{hj}/exp(\alpha_2 \cdot own_h)$.[7] The negative sign defines the MCMRR in terms of the cost of reducing mortality risk. While mortality risk is measured at the individual level, housing price is measured at the household level. Therefore, we follow Davis (2004) in dividing by the average household size, $s = 2.59$, in order to define MCMRR at the individual level. The formula in Equation (3) highlights that the main challenge in identifying the MCMRR is to develop a consistent estimator for β.

3.2 Identifying the Marginal Cost of Mortality Risk Reduction

Our estimation approach addresses two key threats to identifying the hedonic parameter on mortality risk, β. The first threat to identification is that our estimates of location-specific mortality effects are likely to be measured with error. Formally, this may be written as: $\hat{m}_j = m_j + \xi_j$, where $\hat{m}_j$ denotes our measure for location-specific causal mortality risk derived using the FGW estimator. The second threat to identification is that our estimates of location-specific mortality effects may be driven, in part, by unobserved amenities that are

[6] As expected, our estimate of δ is positive with $\hat{\delta} = 0.0977$ and our estimates for β are universally negative across numerous specifications.

[7] Equation (3) can be evaluated at any housing price. For example, if we were to evaluate it at the price that the average individual pays for housing, conditional on age, that measure would embed the effects of sorting along the price function; e.g., older households choosing to locate in more expensive locations where life expectancy is higher.

directly capitalized into housing prices because they simultaneously affect both the quality and the quantity of life. Formally, the set of location-specific amenities can be partitioned as: $x_j = [x_j^o, x_j^u]$, where o and u denote the subsets of observable and unobservable amenities, respectively. Analogously, $\alpha_4 = [\alpha_4^o, \alpha_4^u]$. Equation (4) rewrites Equation (1) to highlight these two potential sources of endogeneity.

$$\begin{aligned} \log p_{hj} &= \alpha_1 + \alpha_2 own_h + \alpha_3 x_h + \alpha_4^o x_j^o + \beta \hat{m}_j + \nu_{hj}, \\ &\text{where } \ \nu_{hj} = \varepsilon_{hj} + \alpha_4^u x_j^u - \beta \xi_j. \end{aligned} \tag{4}$$

We interpret ε_{hj} as idiosyncratic noise and focus on potential threats to identification from omitted variables, x_j^u, and measurement error, ξ_j.[8]

We address the omitted-variable and measurement-error threats separately. First, we use a population-level measure of empirical mortality, z_j, to instrument for causal mortality. Our endogenous variable of interest, causal mortality, is effectively a selection-corrected version of empirical mortality. We assume that any measurement error is introduced through the inevitable modeling assumptions used in the selection-correction procedure of the causal-mortality estimator. In contrast, as the population-based instrument is simply a population statistic, we have no reason to expect it to embed measurement error and we assume that it is uncorrelated with the measurement error in causal mortality:[9]

$$cov(z_j, \xi_j) = 0. \tag{5}$$

This IV strategy addresses measurement error, but remains vulnerable to confounding from x_j^u. The concern is that empirical mortality may be correlated with the composite error if important amenities that determine price cannot be observed and are correlated with empirical mortality, so that $cov(z_j, x_j^u) \neq 0$. Specifically, if unobserved amenities are

[8] ε_{hj} captures the standard modeling errors that are often thought to be relatively innocuous in hedonic price function estimation, such as measurement error in prices, functional form mis-specification, and omitted architectural details.

[9] A violation of this assumption would require that the measurement error in the estimates of m_j is systematic, which would violate the identifying assumptions in FGW.

correlated with the instrument, we would expect this correlation to be negative, i.e.,

$$cov(z_j, x_j^u) < 0. \tag{6}$$

Intuitively, amenities that increase the quality of life (and therefore housing prices) are also likely to increase the quantity of life. This mechanism will be reinforced if healthier and wealthier people sort themselves into locations with more desirable amenities and survive longer—either because of those amenities or independently of them.

Equation (6) is not directly testable, but it is supported by residential sorting literature (Banzhaf and Walsh, 2008; Kuminoff et al., 2013). Further, following Altonji et al. (2005) and Oster (2019), Equation (6) can be indirectly tested under the assumption that the impact on $\hat{\beta}$ from omitting unobserved amenities has the same sign as the impact of omitting observed amenities:

$$\frac{\hat{\beta}(W, x_j) - \hat{\beta}(W, x_j^o)}{\hat{\beta}(W, x_j^o) - \hat{\beta}(W)} \geq 0, \tag{7}$$

where $W = [p_{hj}, own_h, x_h, z_j]$. The denominator is the measurable effect on $\hat{\beta}$ from adding x_j^o as covariates in the hedonic regression. The numerator is the unmeasurable effect of adding x_j^u. It equals zero in the special case where all relevant amenities are observed: $x_j^o = x_j$.

Taken together, Assumptions (5) and (7) allow us to sign the direction of any inconsistency caused by omitted amenities and apply the imperfect IV strategy from Nevo and Rosen (2012) to identify a bound on β. This result can then be used to bound the MCMRR. In summary, our strategy for addressing potential confounding by unobserved location-specific amenities is to control for an extensive set of observable amenities and to interpret our MCMRR estimates based, in part, on how they evolve as we expand the set of amenity controls.

4 Results

4.1 Estimates of the Marginal Cost of Mortality Risk Reduction

Table 1 reports our estimates for the MCMRR from five different specifications of the price function. For each specification, we report estimates of MCMRR for ages 67, 72, 77, 82 and 87 followed by robust standard errors based on clustering at the level of CZ.[10] All five specifications control for physical house characteristics, but differ in the steps taken to mitigate potential confounding.

The estimates in Column (1) are from an OLS regression that excludes amenities. We scale the coefficients and report the MCMRR for a 0.1pp reduction in the annual probability of death among a given age group. This reduction is roughly equivalent to the within-state standard deviation of causal mortality risk across locations that we observe for people at age 77. The results show that the marginal reduction in mortality risk among 77-year-olds is associated with a $244 increase in annual housing costs. This associative measure of the MCMRR declines with age from $572 among 67-year-olds to $104 among 87-year-olds. We use the $244 value for the 77-year old group as a benchmark when comparing to other specifications as 77 is close to the mean age among the 65-and-over population. The results in Column (1) do not have a causal interpretation as they embed potential biases from measurement errors in mortality risk and omitted amenities, and the net direction of these biases is ambiguous. We address these issues incrementally.

To address measurement error in casual mortality risk we use our population-based empirical mortality variable as an instrument in Column (2). The results show that, all else constant, moving from OLS to IV increases the estimates by a factor of 6, yielding a MCMRR of $1,550 at age 77. This increase captures the combined effect of the instrument reducing the scope for bias due to measurement error in casual mortality and increasing the scope for bias due to healthier people sorting into higher-amenity areas and/or higher-amenity areas causing people to live longer. The next two columns take steps to disentangle these

[10] We report estimates for intermediate integer ages in Appendix Table D.1.

Table 1: The Marginal Cost of Mortality Risk Reduction

	(1)	(2)	(3)	(4)	(5)
Marginal cost of mortality risk reduction, age 67	572 (198)	3,632 (1,026)	2,912 (565)	1,346 (266)	1,183 (248)
Marginal cost of mortality risk reduction, age 72	375 (129)	2,373 (670)	1,903 (370)	880 (175)	772 (162)
Marginal cost of mortality risk reduction, age 77	244 (84)	1,550 (438)	1,244 (241)	575 (114)	505 (106)
Marginal cost of mortality risk reduction, age 82	160 (54)	1,013 (286)	813 (157)	376 (75)	331 (70)
Marginal cost of mortality risk reduction, age 87	104 (35)	662 (187)	531 (103)	246 (48)	215 (46)
1st stage coefficient on instrument		0.079 (0.019)	0.084 (0.013)	0.072 (0.011)	0.072 (0.011)
1st stage F-statistic		17.1	42.3	43.0	39.6
state dummies			x	x	x
amenity covariates				x	x
recent moves only (last 5 years)					x
clustering (number of CZs)	536	536	536	536	536
number of houses	5,118,669	5,118,669	5,118,669	5,118,669	2,413,218

Note: The table reports estimates for the annual housing price of reducing the annual risk of death by one tenth of a percentage point for each age group. Estimates are reported in 2010 dollars. All specifications include housing covariates. Robust standard errors are clustered by CZ.

mechanisms.

In Column (3) we narrow our focus to within-state variation in causal mortality by adding state dummies along with interactions of the state dummies and the owner-occupancy indicator. This reduces the implied MCMRR at age 77 to $1,244. Adding state dummies sharpens the identification strategy in three ways. First, the state dummies absorb the price effects of between-state variation in omitted amenities that are correlated with mortality risk. Second, the interactions terms absorb any between-state variation in the user-cost of housing that is correlated with mortality risk (Poterba, 1984). Finally, focusing on within-state variation in prices and mortality risk reduces the potential concern that moving costs and information frictions may limit the extent to which housing markets capitalize spatial variation in mortality risks.

The estimator in Column (3) remains vulnerable to confounding from within-state sorting on amenities. For example, wealthier people with longer life expectancies may tend to locate in higher-amenity locations within their home states. This sorting behavior would impart an upward bias to our MCMRR estimator as we would expect omitted amenities to be positively correlated with housing prices while negatively correlated with empirical mortality.

We address this threat to identification by augmenting the IV estimator from Column (3) to add the 18 amenity covariates based on Diamond (2016). Adding these covariates reduces the age-77 MCMRR to $575. This decline is consistent with the intuition that people live longer in locations where housing is more expensive due to amenity capitalization. We consider these results, shown in Column (4), to be our main MCMRR estimates.

4.2 Instrument Validity

The results in Column (4) of Table 1 can be interpreted as consistent point estimates under the assumption that our amenity covariates span the set of amenities that matter for housing prices and are correlated with empirical mortality. However, if there remain latent amenities that are positively correlated with both longevity and housing prices then the estimates in Column (4) can be interpreted as upper bounds under Assumptions (5) and (7). In order to distinguish between these two scenarios, we examine how our MCMRR estimates evolve as we add amenity covariates incrementally. This exercise adapts the techniques from Altonji et al. (2005), Nevo and Rosen (2012), Altonji et al. (2015), and Oster (2019).

We show the results of this analysis in Figure 3. The solid curve shows how our MCMRR estimates at age 77 change as we incrementally add amenity covariates to move from the specification in Column (3) that excludes amenity covariates (denoted by the square) to the specification in Column (4) that includes all 18 amenity covariates (denoted by the triangle). To avoid sensitivity to the order in which amenities are added, we estimate models for all 262,144 possible combinations of the 18 amenity covariates. Each point on the solid curve

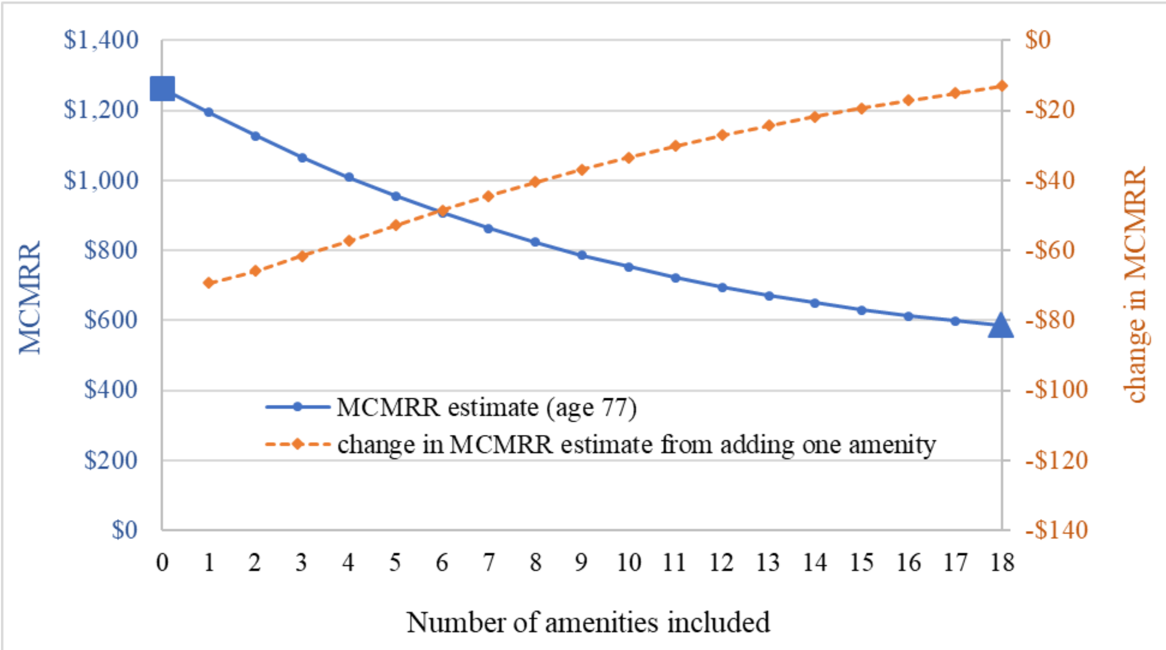


Figure 3: Sensitivity of Results to Amenity Covariates across 262,144 IV Regressions

shows the mean MCMRR (measured on the left vertical axis) estimated over all models that used the number of amenity covariates shown on the horizontal axis. Thus, the curve shows how our estimates evolve, on average, as amenity covariates are added.

The mean MCMRR declines monotonically in the number of amenities, as expected. Moreover, it declines at a decreasing rate; the slope is close to zero by the time we add the final amenity. To further illustrate this point, the dashed curve plots the change in mean MCMRR as we incrementally add more amenities, i.e., the gradient of the solid curve. The size of the change is measured on the right horizontal axis. As we randomly add the first amenity to the model the MCMRR declines by $69 on average. However, adding the last amenity reduces the MCMRR by only $13 on average. Overall, Figure 3 provides evidence that any bias in our main MCMRR estimates is likely to be positively signed and small in magnitude.

4.3 Interpretation of Results

To compare our results with existing evidence on the cost of reducing mortality risk in markets for labor and health care, we multiply our MCMRR estimates by 1,000 to measure the capitalization associated with a one-unit reduction in the probability of a death. In other words, we measure the annual housing expenditures needed to avoid one premature statistical death in expectation.[11] These expenditures decline from approximately $1.3 million at age 67 to $0.2 million at age 87. This range overlaps with prior estimates for the marginal medical cost of avoiding a statistical death among subpopulations over age 65 (Hall and Jones, 2007; Doyle et al., 2015; Huh and Reif, 2017; Ketcham et al., 2020). By contrast, our largest estimate (i.e., MCMRR_{67}) is roughly one-seventh the size of estimates for the marginal cost of avoiding one death on the job among the general worker population (Viscusi and Aldy, 2003; Costa and Kahn, 2004; Cropper et al., 2011; Kniesner et al., 2012; Lee and Taylor, 2019; Evans and Taylor, 2020) and one-fifth the size of estimates for subpopulations of workers aged 60 to 65 (Smith et al., 2004; Aldy and Viscusi, 2008). Further, these comparisons are reinforced by the likely positive sign of any bias in our estimator for capitalization effects.

4.4 Sensitivity Analysis

We take a systematic approach to analyzing the sensitivity of our main estimates to using alternative estimation samples, alternative instruments, and different sets of covariates. Appendix D reports our findings, which we briefly summarize here. First, we consider four alternate ways of defining the empirical mortality instrument: (i) CDC mortality at age 65, (ii) CDC mortality at age 75, (iii) CDC mortality at age 85+, and (iv) a weighted-average measure of empirical mortality from the same CMS sample that we use to estimate causal mortality following the same procedure as for our preferred instrument. The first three instruments use less information than our preferred instrument, but they avoid the need to aggregate over multiple ages. While the final instrument may embed measurement error due

[11] As noted above, we do not interpret this result as a welfare measure. Our lack of revealed preference interpretation differentiates our approach from wage hedonic studies that typically interpret cost estimates as measures for the "value of statistical life".

to working with a 20% sample of the population, it has the advantage of using the same data to measure both causal and empirical mortality. We use each of these four instruments, plus our preferred instrument, to estimate models that differ in the following characteristics: (a) whether the model includes amenity covariates, (b) whether the model includes state dummies, Census division dummies, or no geographic dummies, and (c) whether the model uses the full estimation sample or limits the sample to houses that had changed occupant within the previous five years.

With five alternative instruments, two samples, two sets of covariates, and three ways of modeling spatial dummies, we estimate a total of 60 models. We find, unsurprisingly, that when amenity covariates are excluded, the MCMRR estimates decline substantially when spatial dummies are added. When amenity covariates are included, the MCMRR estimates are relatively insensitive to other modeling decisions, entirely falling between \$391 and \$703 at age 77.

Appendix D reports our findings in greater detail. However, we highlight one important sensitivity check here where we limit our estimation sample to houses that had changed occupant within the previous five years. This addresses any potential concerns that self-reported house prices are likely reported with greater accuracy by recent movers. Results of this sensitivity check are presented in Column (5) of Table 1 and show that point estimates decline by about 12%, but are statistically indistinguishable from our main results.

5 Conclusion

The literature has shown that an individual's location affects the evolution of their human capital, wealth, health, and life expectancy. Using national data describing houses, locations, and the US Medicare population, we provide the first evidence that all-cause location-specific mortality risk in capitalized into housing prices. Specifically, we find the marginal cost of a mortality reduction of 0.1pp is \$1,060 among people in their late 60s and \$190 among people in their late 80s. These measures coming from the housing market are similar in magnitude to existing estimates coming from medical spending among sub-populations over age 65, but less than one-fifth of the existing estimates coming from the labor market among workers near retirement age.

One hypothesis for the discrepancy between the housing and labor markets is that people may be better informed about job-related mortality risks than about location-based mortality risks. While the association between residential location and life expectancy is often noted by the popular press (e.g., Ferrari (2017)) causal evidence supporting this link has only emerged recently. Another hypothesis is that the discrepancy reflects life-cycle heterogeneity in the willingness to pay for mortality-risk reduction. Testing these hypotheses would require knowledge about households' beliefs about future spatial variation in mortality risk (Bishop and Murphy, 2019) and how these beliefs affect their migration decisions (Mathes, 2021). Developing this knowledge is an important area for further research.

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6 Supplemental Online Appendices

Appendix A Estimating Causal Mortality

A.1 Overview

We use the model developed in Finkelstein et al. (2021) [henceforth FGW] to estimate the causal effect that each commuting zone (CZ) has on the probability of death. The model starts from the assumption that causal mortality rates may differ from empirical mortality rates for numerous reasons, including differences in the compositions of local populations due to sorting on health, wealth, and preferences. Intuitively, causal mortality rates are identified by following observationally equivalent people who move from the same origin location to different destinations. In addition to conditioning on a rich set of observable measures of individual demographics and health, FGW use a selection-correction procedure to control for sorting on unobserved health. We outline our data cuts and the econometric procedures below and direct readers to FGW for a fuller description of the method.

A.2 Data

We derive causal measures of spatial variation in mortality risk from administrative records on senior citizens maintained by the U.S. Centers for Medicare and Medicaid Services (CMS). The Medicare program provides universal health insurance for people over age 65. Beneficiaries can choose between HMO-style Medicare Advantage plans and the more traditional form of coverage, commonly known as fee-for-service (FFS) Medicare, in which the government pays health care providers a fixed fee for each service they perform and beneficiaries pay the remainder. Our data span the period 1999 to 2013.

We start with a random, 20-percent sample of all 65-and-older Medicare enrollees from 1999 to 2013. These data describe approximately 17 million people and contain each person's birth date, race, sex, and death date (if they died before the end of 2013). Each year we observe their residential locations (as ZIP+4 codes), their diagnoses of 27 chronic medical conditions, and their gross expenditures on health care services.[12] The spending measures cover inpatient care, skilled nursing facilities, hospice, lab tests, surgery, home health care, outpatient services, durable medical equipment, home health care and some types of preventative care. The annual measures of spending and chronic conditions are derived from FFS claims processed by Medicare. Unfortunately, we cannot observe these variables for Medicare Advantage patients. This is a well known limitation of CMS data for our study period and we follow prior studies in focusing primarily on those enrolled in FFS in their second year, or in the year before they move (Finkelstein et al., 2021; Deryugina et al., 2019). Fortunately, this restriction affects a relatively small share of our sample because more than 75% of seniors enrolled exclusively in FFS Medicare in any given year during our study period.

Prior to estimation, we implement the main data cuts from FGW. First, we drop individuals who move across CZs more than once between 1999 and 2013. Second, we drop all movers who were enrolled in Medicare Advantage during their pre-move year and all nonmovers who were enrolled in Medicare Advantage during their first year in the sample, which is treated as their counterfactual pre-move year in the estimation.

We also follow FGW in aggregating sparsely populated CZs within each state, which takes us from 709 CZs to 536. The number of CZs in our study, 536, is slightly smaller than the 563 CZs defined by Finkelstein et al. (2021) because we apply their methodology to a smaller (20%) sample of Medicare beneficiaries and we drop rural CZs where we do not observe any movers. The cumulative population of the rural CZs that we drop is less than fifty thousand people, or one one-hundredth of one percent of the U.S. population.

[12] Specifically, we observe medical diagnoses of: Acute myocardial infarction, Alzheimer's disease, all-cause dementia, atrial fibrillation, cataract, congestive heart failure, chronic kidney disease, chronic-obstructive pulmonary disease, diabetes, glaucoma, hip fracture, ischemic heart disease, depression, osteoporosis, rheumatoid arthritis and osteoarthritis, stroke or transient ischemic attack, breast cancer, colorectal cancer, prostate cancer, lung cancer, endometrial cancer, anemia, asthma, hyperlipidemia, hypertension, hyperplasia, hypothyroidism.

Table A.1: Summary Statistics: Year 2006 Comparison Sample

	(1) Movers	(2) Non-movers
2006 comparison sample (# of individuals)	47,459	47,459
Age:		
65-74	0.50	0.52
75-84	0.33	0.37
85+	0.16	0.12
Female	0.60	0.58
White	0.87	0.84
Region:		
Northeast	0.14	0.21
South	0.49	0.37
Midwest	0.19	0.27
West	0.19	0.15
On Medicaid	0.10	0.07
Avg. # of chronic conditions	3.34	1.61
One year mortality	0.08	0.05
Four year mortality	0.28	0.20

A.3 Summary Statistics

Table A.1 reports average characteristics of all 47,549 people in our estimation sample who moved in 2006 alongside average characteristics of a randomly selected comparison sample of non-movers during that same year. We condition on a single year in order to compare the characteristics of movers and non-movers, and we choose the year 2006 in order to enable comparison to the analogous table in FGW (their Table 1). Despite working with a smaller random sample of data, our summary statistics and qualitatively and quantitatively very similar to FGW. Like FGW, we observe that movers tend to be diagnosed with more chronic conditions and are more likely to be older, female, white and Medicaid recipients.

Table A.2 summarizes our full estimation sample by reporting mean characteristics of movers and non-movers. We use 6.7 million observations on non-movers and 536 thousand observations on movers. The mean characteristics of each group are very similar to statistics

Table A.2: Summary Statistics: Estimation Sample

	(1) Movers	(2) Non-movers
Age:		
65-74	0.48	0.73
75-84	0.32	0.20
85+	0.20	0.07
Female	0.60	0.56
White	0.87	0.83
Region:		
Northeast	0.14	0.21
South	0.47	0.37
Midwest	0.19	0.26
West	0.20	0.16
On Medicaid	0.10	0.08
Avg. # of chronic conditions	3.27	1.64
One year mortality	0.09	0.04
Four year mortality	0.26	0.14
Number of individuals	536,407	6,702,668

reported in Table A.3 of FGW.

Table A.3 reports quantiles of the distribution of sample sizes of movers received per CZ. The median CZ in our data receives 356 movers compared to a median of 1,522 in FGW (Table A.2). This is because we start from a smaller sample of CMS administrative records. Working with a smaller sample may be expected to increase measurement error in our estimates. This measurement error concern is a central part of our motivation for selecting an instrumental variable for causal mortality.

A.4 Estimation of Causal Mortality Effects

To estimate the effect that a CZ has on individual mortality, we begin with Equation A.1 where individual mortality m_i is regressed on age, demographics X_i, health h_i, and CZ fixed effects for movers and nonmovers. The binary outcome variable is coded as 0 for survival and

Table A.3: Number of Movers Received by Commuting Zone (CZ). 536 CZs in Total.

Statistics	# of Movers to CZ
Minimum	37
10th Percentile	86
25th Percentile	167
Median	356
75th Percentile	1,058
90th Percentile	2,561
Maximum	12,780

1 for death. The unit of observation is a person-year. In years after death, the individual is no longer observed.

$$\log(m_i) = \beta\ age_i + \psi\ X_i + \lambda\ h_i + \tau_j^o\ \mathbb{I}_{j,orig} + \tau_j^d\ \mathbb{I}_{j,orig} + \tau_j^n\ \mathbb{I}_{j,dest} + \eta_i \tag{A.1}$$

Demographic covariates X_i include gender, race, and a gender-by-race interaction. Health covariates h_i include a vector of indicators for the presence of chronic medical conditions, and the log of health care utilization in the pre-move year (i.e. the log of expenditures). Log utilization in the pre-move year is interacted with a set of dummies for the number of months that the individual was enrolled in Medicare in the pre-move year. The fixed effects τ_j^o, τ_j^d, τ_j^n capture the location specific mortality effects of each CZ j. τ_j^n captures the CZ-specific variation for non-movers, and τ_j^o and τ_j^n for the origin and destination CZs of movers. The youngest enrollees in our Medicare data are 65. However, as the estimation approach requires individuals to survive for two years to observe baseline location and health, the youngest age used to estimate causal mortality is 67

The CZ specific effects on mortality τ_j^d are biased estimators of location-specific causal mortality if movers sort into locations based on unobserved health. To address this concern, Equation A.2 shows how $\hat{\tau}_j^d$ is corrected for spatial sorting on health, under the assumption that selection on unobserved health can be approximated by selection on observed health.

The unit of observation is this estimation is a person.

$$\hat{h}_i = \beta^h \; age_i + \psi^h \; x_i + h_j^o \; \mathbb{I}_{j,orig} + h_j^d \; \mathbb{I}_{j,dest} + \eta_i^h \tag{A.2}$$

The fitted health stock from Equation A.1, $\hat{h}_i = \hat{\lambda} h_i$, is then regressed on age, demographics, a set of dummies for the number of months that the individual was enrolled in Medicare in the premove year, and location specific fixed effects h^o and h^d. $\hat{\tau}_j^d$ is then corrected by the estimated health-sorting effect $\hat{h}_j^d$. The causal place-specific mortality effect $\hat{\gamma}_j$ is thus estimated as

$$\hat{\gamma}_j = \hat{\tau}_j^d - \frac{\hat{sd}(\hat{\tau}_j^o)}{\hat{sd}(\hat{h}_j^o)} \; \hat{h}_j^d \tag{A.3}$$

$\hat{sd}(\hat{\tau}_j^o)$ and $\hat{sd}(\hat{h}_j^o)$ are estimated as the standard deviations of τ_j^o and h_j^o in a split-sample bootstrap. Finally, the $\hat{\gamma}_j$ estimates are corrected for noise with an Empirical Bayes adjustment with the following equations:

$$\hat{\gamma}_j^{EB} = \frac{\chi^2}{\chi^2 + s_j^2} \; \hat{\gamma}_j, \quad \chi^2 = Var(\hat{\gamma}_j) - E(s_j^2) \tag{A.4}$$

This correction involves bootstrapping ($b = 200$) the variance s_j^2 of the $\hat{\gamma}_j$ estimates.

A.5 Calculating Mortality Treatment Effects

To calculate the local annual average mortality as a function of age, while controlling for selection, the estimate for γ_j is used with the population average value of health capital, $\bar{\theta}$ in the following version of the Gompertz mortality function:

$$m_j(a) = exp(\delta \cdot age + \gamma_j + \bar{\theta}) \tag{A.5}$$

where θ_j captures the average health capital of residents in location j and is estimated as the mean of $\hat{\psi} X_i$ across all non-movers in j and where $\bar{\theta}$ is the population average value of θ_j.

Appendix B IPUMS - Data Description

To estimate the relationship between the price of housing and location-specific mortality effects, we use micro data of the Census 2000 from IPUMS USA. We include both renter-occupied and owner-occupied properties. We use all variables that contain information on the characteristics of the house that the individuals inhabit, such as the number of rooms, bedrooms, the age of the structure, the type of the structure, and the presence of kitchen and plumbing facilities. Table B.1 reports summary statistics.

Mapping PUMAs to CZs, requires the crosswalk from PUMAs to FIPS county codes comes from the Geocorr 2000 Geographic Correspondence Engine (Version 1.3.3) provided by the Missouri Census Data Center. Approximately one-third of PUMAs intersect CZ borders. In these cases we integrated over the uncertainty in assigning houses to CZs using the relative population sizes of the PUMA-CZ intersections as probability weights. We implemented two tests to examine the scope for our assignment procedure to affect our results. First, we repeated the estimation after assigning everyone living in PUMAs that intersected CZ borders to the single CZ with the largest population of intersection. This produced nearly identical results to our main specification (e.g. \$1.2 million at age 77). Second, we repeated the estimation after dropping all PUMAs that intersect CZs. Dropping this third of PUMAs only produced a small change in our age 77 estimate for the housing cost of mortality reduction (\$1.4 million compared to our main estimate of \$1.2 million).

Table B.1: Census Data Summary Stats

	N	Mean	SD	5th perc	95th perc
Home Value (USD 2000)	3,604,445	149,870	142,139	22,500	350,000
Gross Rent (USD 2000)	1,515,093	657.61	361.24	190.00	1303.00
Indicator Home Ownership	5,119,538	0.68	0.47	0.00	1.00
Number of Rooms	5,119,538	5.47	1.96	2.00	9.00
Indicator Kitchen	5,119,538	0.99	0.08	1.00	1.00
Indicator Indoor Plumbing	5,119,538	0.99	0.08	1.00	1.00
Decade Built	5,119,538	1962.92	20.03	1930.00	1990.00
Number Adults in Hhld	5,119,538	1.84	0.78	1.00	3.00
Number Workers in Hhld	5,119,538	1.22	0.91	0.00	3.00
Youngest Person in Hhld	5,119,538	44.22	17.51	21.00	78.00
Oldest Person in Hhld	5,119,538	50.85	17.33	26.00	82.00

Notes: Summary statistics are taken across houses, using the household weights of the resident households. The sample covers houses in 2,057 PUMAs, which represents 99.3 percent of all PUMAs. Home value is calculated for owner-occupied houses, gross rents for renter-occupied houses. The number of rooms, home values and gross rents are topcoded. The decade in which the structure was built is left-censored in 1930.

Appendix C Amenities

To build amenity data we started by downloading datasets provided in the supplemental online appendix to Diamond (2016). Since the data in Diamond (2016) are limited to metropolitan areas, we returned to the original sources to collect information for rural areas or, when such data did not exist, we collected national data on the closest available substitute amenity. The resulting amenity data cover 93 percent of U.S. counties and 99 percent of PUMAs.

Data on the number of apparel stores, dining places, and movie theaters per capita come from County Business Patterns (2000). The number of reported violent crimes and property crimes come from the FBI crime reports (ICPSR, 2006). The student-teacher ratio, government expenditures per student, and government expenditures on parks come from the 1997 Census of Governments. Average concentrations of fine particulate matter and ozone come from the U.S. EPA Air Quality System. Data on interstate highway mileage, urban arterial mileage, and the number of urban rail stops come from the National Atlas of the U.S. Geological Survey. Finally, we derived the climate measures by aggregating data from 9,959 NOAA weather stations that reported continuously from 1999 through 2012.[13] Summer temperature is measured by the highest within-month average of daily maximum temperatures in a given year, averaged per station from 1999 to 2001. Winter temperature is measured as the lowest within-month average of daily maximum temperatures, and precipitation is measured as average daily rainfall. We calculate summer temperature, winter temperature, and precipitation per PUMA as the weighted average across weather stations, weighted by squared inverse distance of each weather station to the population weighted geographic centroid. Further information on data sources are provided in Diamond (2016).

Table C.1 reports summary statistics by PUMA for the 18 amenities that we include as covariates in our main econometric specifications.

[13]Retrieving and processing NOAA weather station data was made possible by Chamberlain (2020).

Table C.1: Amenity Data Summary Stats

	Mean	SD	5th perc	95th perc
Average Daily Maximum Temperature, Summer (C)	30.61	3.06	26.21	36.36
Average Daily Maximum Temperature, Winter (C)	7.02	6.88	-2.22	19.36
Average Daily Precipitation (mm per m2)	2.57	0.89	0.82	3.76
Apparel Stores per 1,000 Residents (log)	1.12	0.14	0.89	1.39
Eating and Drinking Places per 1,000 Residents (log)	1.74	0.15	1.51	1.95
Movie Theaters per 1,000 Residents (log)	0.02	0.01	0.01	0.03
Violent Crimes Reported per 1,000 Residents (log)	1.56	0.65	0.39	2.49
Property Crimes Reported per 1,000 Residents (log)	3.34	0.79	1.53	4.18
Student-Teacher Ratio (log)	2.19	0.25	1.82	2.51
Local Government Expenditure per K-12 Student (USD 2004, log)	8.74	1.79	5.97	11.84
Local Government Expenditure on Parks, Recreation and Natural Resources per Capita (USD 2004, log)	1.36	2.07	-2.48	4.11
Average Concentration of PM 2.5 (μg per m^3, log)	2.66	0.24	2.24	3.02
Average Concentration of Ozone (parts per billion, log)	1.38	1.37	0.02	3.43
Miles of Interstate per square mile (log)	0.08	0.08	0.00	0.23
Miles of Urban Arterials per square mile (log)	0.22	0.29	0.00	0.71
No. of Urban Rail Stops (log)	1.02	1.65	0.00	4.53
Share Unemployed (log)	1.42	0.32	0.95	2.03
Share of College Degree Holders (log)	3.22	0.48	2.47	4.03

Notes: Summary statistics are taken across 2,057 Census PUMAs.

Appendix D Additional Results

D.1 Housing Cost of Mortality Risk Reduction by Integer Age

Table D.1 reports hedonic property value estimates for the marginal cost of mortality risk reduction from age 67 to age 87. The estimates correspond to the specifications in Table 1. Estimates for the annual capitalization effects of a 0.1 percentage point reduction in annual mortality risk are reported in thousand 2010 dollars. All specifications include housing covariates. Robust standard errors are clustered by CZ. The table is continued on the next page.

Table D.1: The Marginal Cost of Mortality Risk Reduction, Full Results

	(1)	(2)	(3)	(4)	(5)
Marginal cost of mortality risk reduction, age 67	572	3,632	2,912	1,346	1,183
	(198)	(1,026)	(565)	(266)	(248)
Marginal cost of mortality risk reduction, age 68	527	3,335	2,676	1,236	1,086
	(181)	(942)	(519)	(244)	(228)
Marginal cost of mortality risk reduction, age 69	484	3,063	2,457	1,136	998
	(166)	(865)	(476)	(225)	(209)
Marginal cost of mortality risk reduction, age 70	443	2,812	2,257	1,043	917
	(153)	(795)	(438)	(206)	(192)
Marginal cost of mortality risk reduction, age 71	408	2,583	2,072	957	842
	(141)	(729)	(401)	(190)	(177)
Marginal cost of mortality risk reduction, age 72	375	2,373	1,903	880	772
	(129)	(670)	(370)	(175)	(162)
Marginal cost of mortality risk reduction, age 73	344	2,179	1,747	808	710
	(118)	(615)	(339)	(160)	(149)
Marginal cost of mortality risk reduction, age 74	315	2,001	1,606	742	652
	(109)	(565)	(312)	(147)	(137)
Marginal cost of mortality risk reduction, age 75	290	1,839	1,474	681	599
	(100)	(519)	(286)	(135)	(125)
Marginal cost of mortality risk reduction, age 76	266	1,688	1,354	626	550
	(91)	(477)	(262)	(124)	(115)
Marginal cost of mortality risk reduction, age 77	244	1,550	1,244	575	505
	(84)	(438)	(241)	(114)	(106)
1st stage coefficient on instrument		0.079	0.084	0.072	0.072
		(0.019)	(0.013)	(0.011)	(0.011)
1st stage F-statistic		17.1	42.3	43.0	39.6
state dummies			x	x	x
amenity covariates				x	x
recent moves only (last 5 years)					x
clustering (number of CZs)	536	536	536	536	536
number of houses	5,118,669	5,118,669	5,118,669	5,118,669	2,413,218

Note: The table is continued on the following page.

Table D.1: The Marginal Cost of Mortality Risk Reduction, Full Results Continued

	(1)	(2)	(3)	(4)	(5)
Marginal cost of mortality risk reduction, age 78	224 (77)	1,423 (403)	1,142 (222)	528 (105)	463 (98)
Marginal cost of mortality risk reduction, age 79	206 (71)	1,308 (370)	1,048 (204)	485 (96)	425 (90)
Marginal cost of mortality risk reduction, age 80	190 (65)	1,200 (339)	964 (187)	446 (89)	391 (82)
Marginal cost of mortality risk reduction, age 81	173 (60)	1,103 (312)	885 (172)	409 (81)	360 (76)
Marginal cost of mortality risk reduction, age 82	160 (54)	1,013 (286)	813 (157)	376 (75)	331 (70)
Marginal cost of mortality risk reduction, age 83	147 (51)	931 (263)	746 (144)	344 (68)	303 (63)
Marginal cost of mortality risk reduction, age 84	134 (47)	855 (242)	685 (133)	317 (63)	279 (58)
Marginal cost of mortality risk reduction, age 85	124 (43)	785 (222)	629 (122)	291 (57)	256 (53)
Marginal cost of mortality risk reduction, age 86	114 (39)	721 (204)	579 (113)	267 (53)	234 (49)
Marginal cost of mortality risk reduction, age 87	104 (35)	662 (187)	531 (103)	246 (48)	215 (46)
1st stage coefficient on instrument		0.079 (0.019)	0.085 (0.013)	0.072 (0.011)	0.072 (0.011)
1st stage F-statistic		17.1	43.1	43.1	39.6
state dummies			x	x	x
amenity covariates				x	x
recent moves only (last 5 years)					x
clustering (number of CZs)	536	536	536	536	536
number of houses	5,119,538	5,119,538	5,119,538	5,119,538	2,413,257

Note: The table reports estimates for the housing cost of reducing the annual risk of death by one tenth of a percentage point by age group. Estimates are reported in 2010 dollars. All specifications include housing covariates. Robust standard errors are clustered by CZ.

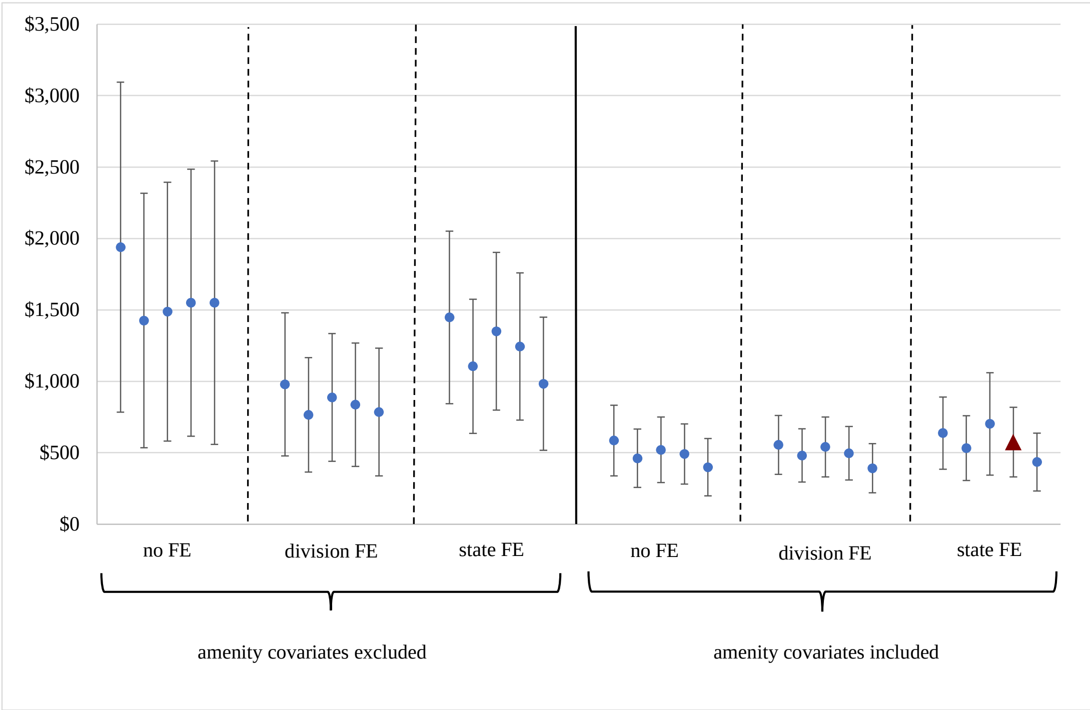
D.2 Sensitivity Analysis: MCMRR Estimates

Figure D.1 shows the sensitivity of our main estimates for the marginal cost of mortality risk reduction (MCMRR) at age 77 to sixty alternative IV specifications. For reference, the results from our main specification in columns (4) and (5) of Table 1 are shown as triangles. Each dot in the figure reports an estimate and its 95% confidence interval from a specification that differs from our main specification in one or more of the following dimensions: (i) the instrumental variable; (ii) the estimation sample: all houses or only houses lived in by recent movers, (iii) the treatment of amenity covariates: included or excluded, and (iv) the treatment of fixed effects: none, Census divisions, or U.S. states. Within each delineated sub-panel the five dots correspond to the following instrumental variables ordered from left to right: (i) CDC empirical mortality for people age 65, (ii) CDC empirical mortality for people age 75, (iii) CDC empirical mortality for people aged 85 and above, (iv) our preferred CDC measure of empirical mortality, (V) an equivalent measure of empirical mortality calculated from the same CMS data that we use to estimate causal mortality.

Comparing Figure D.1a to Figure D.1b shows that the results are robust to limiting the sample to people who moved to their current dwellings within the last five years and, therefore, may more accurately assess the market values of their houses. The three sub-panels in the left half of each sub-figure show that when amenity covariates are excluded from the model adding fixed effects for Census divisions or U.S. states reduces the MCMRR. Comparing the left half of each sub-figure to the right half shows that adding amenity covariates further reduces the estimated MCMRR. Finally, the right three sub-panels within each sub-figure show that when amenity covariates are included the MCMRR estimates are relatively robust to using alternative fixed effects and/or alternative empirical mortality instruments. The estimates all fall within a range from \$339 to \$703.

Figure D.1: Marginal Cost of Mortality Risk Reduction, Sensitivity Analysis

(a) Marginal Cost of Mortality Risk Reduction at Age 77 (all households)



(b) Marginal Cost of Mortality Risk Reduction at Age 77 (recent movers only)