THE IMPACT OF RETIREMENT ON MORTALITY:
EVIDENCE FROM MALE AMERICAN AIRPLANE PILOTS

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Abstract

Despite the significant change in lifestyle that occurs at retirement, relatively little is known about the impact of retiring on health. This is not surprising because retirement may be driven by health concerns, so for many this is a simultaneous decision, making it difficult to isolate the impact of one variable on another. This study estimates the influence of forced retirement on mortality, exploiting legislation that requires commercial airline pilots to retire at age 60. We use data from the 1990 and 2000 Census Five-Percent Public Use Micro Samples and a regression discontinuity design to demonstrate that, among males that report airline pilot as their occupation, there is a sharp drop in the fraction that are in the labor force at age 60. There is no such change in a comparison sample of highly educated males. Mortality rates by age, cohort, and occupation are estimated using the Multiple Cause of Death data set from the U.S. National Center for Health Statistics and Census data. We analyze the slope of the Gompertz curve, which relates the natural log of mortality rates to age, in both our pilot group and the selected comparison sample. We find suggestive evidence of an increase in the slope of the Gompertz curve in the pilot group after age 60 relative to what we find in the control group, though our estimates fall short of statistical significance.

I. Introduction

Retirement is a relatively new concept. Prior to the late 1800s, when life expectancy was much lower, most workers were employed until death. As life expectancies surged at the end of the 19th century, the concept of retirement was introduced. Today, the vast majority of Americans say that they plan to retire at some point in their lives and, according to 2011 U.S. Census data, about 84% of all Americans age 65 or older are no longer in the work force, almost all of whom are permanently retired. Though it is difficult to say with certainty how many retirees there are in the United States, the 40 million retired workers receiving Social Security payments gives us a rough sense of the scope of retirement in the US.

Retirement is associated with a profound change in lifestyle. Data from the American Time Use Survey shows that the increase in free time associated with retirement means that retired workers spend more time eating, sleeping, shopping, reading and socializing
than their working counterparts. Though referred to colloquially as the golden years, there is surprisingly little conclusive empirical evidence of the effect that retirement has on health and mortality. Ekerdt et al. (1983) was the first major modern study of the retirement-health link, and their results suggested no significant difference in physical health, as measured by a series of medical exams, between retirees and workers of the same age. While many studies have echoed this result in the years since, including Latif’s (2013) study of the effect of retirement on mental health in Canada, others have yielded conflicting results. Dave et al. (2006) found that retirement was associated with declines in both mental and physical function and increases in the prevalence of illnesses, a result also found by Moon et al. (2012). However, Jokela et al. (2010) found in a study of British civil servants that retirement was associated with a statistically significant increase in both mental and physical function. Likewise, Oksanen et al. (2011) found a statistically significant decrease in antidepressant use among Finnish public sector retirees. In sum, previous empirical evidence gives a very cloudy picture of the true relationship between retirement and health status.

The ambiguity in results in these observational studies should be no surprise, as anecdotal evidence also seems conflicted. It seems likely that retirement might be correlated with lower stress levels, which would tend to suggest better health outcomes through decreases in the rates of cardiovascular disease, certain cancers, diabetes, Alzheimer’s Disease, and other stress-related disorders within the retired population. In addition, those working in jobs associated with some sort of inherent workplace risk would likely no longer be subject to potential workplace accidents and injuries. Retirement, however, may also be associated with lower levels of both physical and mental activity.
This might cause many of the same diseases that decreased stress levels can prevent. More importantly, retirement may be brought on by the onset of a health shock suggesting that some may be retired precisely because they are in poor health. The net result is that in single-equation models that regress health on retirement status, it is highly likely the results are biased and we have little sense of whether the results are biased up or down.

The determination of the impact of retirement is particularly important in today’s political and economic context. Retirement is changing dramatically as a result of longer life expectancies, a changing economy, the rise of defined-contribution pension plans, and shifting demographic balances in the US. US citizens are increasingly postponing retirement or deciding not to voluntarily retire at all. By analyzing U.S. Census Five-Percent Public Use Micro Samples, we estimate that the proportion of men between age 65 and 69 who are in the labor force increased from 27.9% in 1990 to 35.8% in 2010, and an even greater change is observed in women. It is difficult to analyze how this trend might impact health, health care, and life expectancies without understanding the link between retirement and health.

In this paper, we study the impact of retirement on mortality rates in male American airplane pilots and navigators between 1989 and 1998, using a federal law that requires mandatory retirement for this occupation at age 60 as a quasi-experiment. Data from the 1990 and 2000 Census Five-Percent Public Use Micro Samples and a regression discontinuity design model demonstrate that among male pilots there is a sharp drop in the fraction of individuals who are in the labor force at age 60. We use the same data to construct a sample of higher-educated and well-compensated males to form a comparison sample. In this group, there is no such discontinuity in work at age 60.
Using data from a consistent sample of states that report occupations in the Multiple Cause of Death data files from 1989 to 1998, we calculate mortality rates for cohorts between the ages of 50 and 69 for both a treatment group of pilots and a comparison group. To test whether forced retirement alters mortality we utilize the well-established Gompertz curve, which notes that the natural log of mortality rates are linear in age. We test whether there is a break in the Gompertz line trend after age 60 for pilots who are forced to retire at age 60 as a result of a US federal regulation. This strategy of utilizing a legislated mandatory retirement age to assess the impact of retirement is, to knowledge, a new approach that has not previously been used in the literature. The utilization of pilots and their unique retirement profile conveniently solves a number of selection bias issues that have plagued earlier attempts to isolate the impact of retirement from other unobserved covariates.

In the comparison sample, we find no structural break in the Gompertz curve at age 60. We do, however, find suggestive evidence that retirement is associated with a sizeable increase in mortality rates in the treatment population, though our results fall short of statistical significance. Our results, though in agreement with some previous literature that suggests a decrease in health as a result of retirement, provide significantly larger and more noteworthy estimates of the detrimental effects of retirement on health and mortality than any previous study.

It is worth noting that there is evidence that pilots may be exposed to certain risks that normal populations are not. It might seem obvious that pilots have been shown to have a significantly higher risk of mortality from aircraft accidents. According to a study of Canadian pilots by Band et al. (1996), pilots are about 25 times as likely to die from aircraft accidents.
accidents as the general population. While this is an extremely high figure, the extraordinarily low rates at which the general population dies in aircraft accidents makes it such that this is highly unlikely to influence our estimates. Pilots are also exposed to higher levels of radiation than a normal populace due to the amount of time they spend in the upper atmosphere. This has led many to suggest that pilots may be at higher risk for certain cancers. Band et al.’s study of Canadian pilots found a significantly increased risk of Acute Myeloid Leukemia (AML) in pilots, suggesting that they have the disease at just under three times the normal rate. AML, however, is a relatively rare disease, and even if the study’s result is to be believed, the scope of the increase in risk is so small that it would have no significant impact on the results of our study. Though the model used should have relatively strong internal validity, there are potential limitations with its external validity. First, pilots may not be wholly representative of the average individual. Pilots are, on average, wealthier than the typical American and they perform a very specific job function, which may make their transition into retired life different than that of someone in another line of work. One may be able to more easily extrapolate our results to populations that are in relatively sedentary but mentally demanding jobs. The results may also not be representative of the effect of retirement on mortality rates in women, who were not included in the study due to the extreme concentration of males among airplane pilots. The study is also limited to those who retire at age 60, and caution should be exercised in extrapolating these results to retirees of other ages. This result, however, remains useful as it demonstrates an impact on those who retire slightly before the average retirement age of 62¹. Finally, the data used measures mortality in pilots between the years of 1989 and

¹ Based on U.S. Census Bureau Calculations
1998. It may be the case that changes in lifestyle, health care, or otherwise may make our results less applicable to mortality rates today. It is worthy of note that the mandatory retirement age for pilots was increased to age 65 in 2007. Another study might exploit more recent health and mortality data in order to examine a similar phenomenon in a more recent population, though such data was not immediately available.

Section II of this paper will discuss the data used in this study and the general econometric model. Section III will provide the estimation strategy employed, and Section IV will provide the results of our model. Section V will give conclusions from the study.

II. Data and Econometrics Model

a. The impact of mandatory retirement on labor supply

In order to study the impact of mandatory retirement on mortality, we analyze a population of male “Airplane Pilots and Navigators”, a class within the Bureau of Labor Statistics’ Standard Occupational Classification System. Within this occupation, we further restrict our study to Airplane Pilots and Navigators who work within the “Air Transportation” Industry, an industry classification that is also in line with standard census breakdowns. Airplane pilots and navigators, as a group, allow for a convenient study of retirement effects because of federal legislation that mandated, until 2007, that pilots of large aircraft retire before the age of 60. This legislation creates an exogenous shock of retirement and solves a significant selection bias problem that plagues many studies of retirement’s impact on health.

Retirement is highly correlated with variables like wealth, health status, and personal perceived life expectancy, all of which are also correlated with mortality. This
creates an omitted variables bias in any study that observes a population that freely chooses their age of retirement. Though studies generally try to control for factors that co-vary with retirement and health outcomes, it is difficult or impossible to know if omitted variables bias accounts for the results of prior studies that observe populations that choose their own retirement age, as it is impossible to control for all factors that impact the retirement decision. Mandatory retirement at age of 60 eliminates self-selection, and forces a certain proportion of pilots into retirement regardless of their health, wealth or other factors that would normally play a role in determining a person’s age of retirement.

The relevant legislation was part of the Aeronautics and Space section of the Code of Federal Regulations, and was found at Title 14, Part 121.383c, which stated that “No [Federal Aviation Administration] certificate holder may use the services of any person as a pilot on an airplane engaged in operations...if that person has reached his 60th birthday”. This statute, colloquially referred to as the “Age 60 Rule”, applied to all pilots who worked for cargo transporters engaged in “air commerce” or for airlines flying “civil airplanes with a seat configuration of 20 or more passengers, or a maximum payload capacity of 6,000 pounds or more”. This rule effectively barred all pilots over the age of 60 from flying commercial aircraft. It is unlikely that many effected pilots continued to work in the air transportation industry after their 60th birthday, as it would likely be very difficult for pilots to find employment opportunities that would not violate the Age 60 Rule. The Age 60 Rule was published on December 5, 1959 and became effective on March 15, 1960. The law was repealed in 2007, at which time a similar “Age 65 Rule” replaced it. By restricting our study to pilots within the Air Transportation industry, we rid our study of people in

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2 Selections taken from the 2005 edition of the Code of Federal Regulations, Title 14
such sub-occupations as crop dusting, pilot education, and aviation tourism, in which pilots might not be subject to the mandatory retirement legislation due to the absence of the passengers and large aircraft that exist in the Air Transportation industry.

In order to determine the empirical effects of the Age 60 Rule on retirement status in pilots, we utilize the US Census Five-Percent Public Use Micro Samples (PUMS). The PUMS data set contains, among many things, information on whether an individual is part of the labor force—meaning that they are currently working or are searching for a job. This is a reasonable proxy for their retirement status within the age range studied, as the vast majority of those who are not part of the labor force are retired in this age group. The PUMS data set also documents a person’s occupation, or their associated former occupation in the case of a retiree, using the Standard Occupational Classification System. In order to capture as large a sample as possible, we look at the 1990 and 2000 PUMS data sets, which each surveyed five percent of the U.S. population. By looking at both the data from 1990 and from 2000, we essentially “bookend” the years of our 1989-1998 study by observing population data at the beginning and just after the end of our years of study. Any significant changes in the effect of the Age 60 Rule on pilot retirement rates over the course of the study would likely be apparent in the two PUMS data sets.

As we outline below, we use a difference-in-difference framework within a regression discontinuity design model to isolate the impact of the Age 60 Rule on retirement for commercial pilots. We would expect that, in general, changes in labor supply should occur smoothly as a cohort ages. However, we anticipate a discrete change in employment for pilots at age 60. To hedge against the chance that there is a structural change in labor supply at age 60 for this group anyway, we examine comparison samples
that contain individuals surveyed in each of the two census years who are non-pilot males but have similar characteristics to pilots. In order to control for the higher education status of pilots relative to the general populace, we restricted the control groups by years of education. As many airline pilots are required to have a college degree, we selected for the control only those individuals with between two and five years of college education. In order to match the pilot and comparison groups used in this first-stage estimate with the mortality data used later in this study to construct mortality rates, which was only reported continuously between 1989 and 1998 by 16 states, we restrict the pilot and control groups from the 1990 and 2000 PUMSs to only those in the relevant states. These states and the mortality data are discussed in greater detail later. Summary statistics of the treatment and the control group are listed in Table 1.

Table 1 shows that pilots earn about thirty-five thousand dollars more per year in average income than our comparison group. While this is certainly not ideal for our study, it is difficult to find a group with similarly high income without looking at groups with significantly more education, as pilots are paid much more than most professions requiring so little education on average. Pilots also have less education on average than the control group by 1.34 years. This gap is also unfortunately large, but rather unavoidable. There are also likely to be some pilots within our sample who, despite our restrictions that try to isolate those affected by the Age 60 Rule, are not subject to the legislation. Since there are generally stringent educational requirements in the airline industry, which is the group most likely to be affected by the legislation, it is possible that the true underlying population affected by the Age 60 Rule may have higher educational attainment than is suggested by our estimates. This might make the treatment and control groups more
TABLE 1
Sample Characteristics for Airplane Pilots & Navigators and Control

<table>
<thead>
<tr>
<th></th>
<th>Pilots</th>
<th>Control</th>
<th>P-Value Pilot=Control</th>
</tr>
</thead>
<tbody>
<tr>
<td>Observations</td>
<td>1472</td>
<td>52082</td>
<td></td>
</tr>
<tr>
<td>Average Reported Annual Income ($)</td>
<td>90857</td>
<td>55093</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(1531)</td>
<td>(206)</td>
<td></td>
</tr>
<tr>
<td>Average Years of Education</td>
<td>14.76</td>
<td>16.1</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.0042)</td>
<td></td>
</tr>
<tr>
<td>Proportion Veterans</td>
<td>0.8845</td>
<td>0.718</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(0.0083)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>Proportion Currently Married, Living With spouse</td>
<td>0.8573</td>
<td>0.8588</td>
<td>0.879</td>
</tr>
<tr>
<td></td>
<td>(0.0091)</td>
<td>(0.0015)</td>
<td></td>
</tr>
<tr>
<td>White (Including Hispanic)</td>
<td>0.983</td>
<td>0.94</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(0.0033)</td>
<td>(0.001)</td>
<td></td>
</tr>
</tbody>
</table>

similar in education than our estimates suggest, though there is no way to test this using our data sets. The treatment and control groups also differ in veteran status, as pilots are significantly more likely to be military veterans. This result is expected since a large proportion of civil pilots serve in the armed forces, where they learn to fly before working for airlines or other air transport companies. The two groups appear very similar in terms of marriage status, which might seem to have an obvious impact on retirement lifestyle. The groups seem to be different in their ethnic makeup, as a lower proportion of the control population identify as White or Hispanic in the CPS data. This result seems be
unavoidable, but is likely of little significance to our later estimates of mortality rates. This control group is also used in our later analysis of mortality data.

In order to obtain an estimate of the total impact of the Age 60 Rule on retirement rates, we run a regression for person \( i \) with age \( a \) from group \( g \) (either pilots or comparison sample) of the form:

\[
R_{iag} = \alpha + P_g \pi + D60_a \beta + D60_a P_g \gamma + \sum_{j=1}^{2} \left( D60_a(a-60)^j + (1-D60_a)(a-60)^j \right) + \varepsilon_{iag}
\]

where \( R \) is a dummy variable that equals 1 if the individual is not in the labor force, \( P \) is a dummy variable for being a pilot, and \( D60 \) is a dummy variable for being age 60 or greater. The terms \((a-60)\) are indexes that capture the smooth movement of retirement above and below the age 60 cutoff and this term is entered as a quadratic term in the regression. In order to avoid including idiosyncratic data in the tail ends of the age spectrum, we restrict our regression to those between ages 50 and 69 in both the treatment and control—ten years on either side of the cut off. This construction is essentially two regression discontinuity models—one for the treatment pilot group and one for the control—that serve as inputs into a difference-in-differences model. Our coefficient of interest, the interaction between the “age 60 or greater” dummy variable and the pilot dummy variable, gives the difference between the two regression discontinuity estimates, each of which estimates the break at age 60 in the regression of the retired proportion of the group. This analysis is performed separately on the 1990 and 2000 data to give two different estimates of the effect of the Age 60 Rule on the pilot population at two different times. Because the outcome is a dummy variable, the equation described by (1) is a linear probability model.
The final two columns of Table 2 provide estimates for the above regression in 1990 and in 2000, respectively, using the PUMS 5% survey data. The result of note is the interaction between the dummy variable for being a pilot and the dummy variable for being age 60 or greater, which yields our estimate of the percentage of the pilot population that retired at age 60 as a result of the Age 60 Rule. The regression of the 1990 data suggests a statistically significant jump in the percentage of pilots retired at age 60 of 29.4 percentage points more for pilots than would otherwise be expected. The same regression using the 2000 survey data yields a 32.0 percentage point increase in retirement rates at age 60 when compared to the control sample. The similarity of these two results, which are not statistically significantly different, suggests that the proportion of pilots that retired on their 60th birthday held relatively constant over the course of our study. The dummy variable for being over age 60 in non-pilots yields a result that is not significantly different from zero, which is to be expected given that normal populations generally would not be expected to retire at age 60 with any significantly different frequency than at 59 or 61. The dummy variable for being a pilot in the 1990 data did, however, yield a statistically significant result, as pilots were overall about 7.5 percentage points more likely to be retired at a given age. This result makes sense given that pilots are generally wealthier than those in the control sample, and thus may have more opportunity to retire at younger ages than their less wealthy counterparts. This result, however, disappears in the 2000 data, where we find a coefficient of essentially zero on the pilot dummy variable.

We find significant coefficients on age in both the 1990 and 2000 data. Before age 60—or rather between age 50 and 59—an additional year increases the proportion of individuals not in the labor force by about 5.0 percentage points in the 1990 data and
Table 2

First Stage Model:
Linear Probability Model Estimates of Not in the Labor Force Equation,
Male Pilots and Comparison Sample, Ages 50-69,
1990 and 2000 Census 5% PUMS

<table>
<thead>
<tr>
<th></th>
<th>1990 PUMS</th>
<th>2000 PUMS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pilot</td>
<td>0.07443 (0.0259)</td>
<td>0.00775 (0.0355)</td>
</tr>
<tr>
<td>(1-D60a)(A-60)</td>
<td>0.0497 (0.0201)</td>
<td>0.0364 (0.0275)</td>
</tr>
<tr>
<td>(1-D60a)(A-60)^2</td>
<td>0.0025 (0.0018)</td>
<td>0.00168 (0.0024)</td>
</tr>
<tr>
<td>D60a(A-60)</td>
<td>0.0252 (0.0166)</td>
<td>0.0225 (0.0166)</td>
</tr>
<tr>
<td>D60a(A-60)^2</td>
<td>0.0001 (0.0018)</td>
<td>-0.0020 (0.0024)</td>
</tr>
<tr>
<td>Dummy for Over Age 60</td>
<td>0.00539 (0.0607)</td>
<td>0.0687 (0.0832)</td>
</tr>
<tr>
<td>Pilot * Over Age 60</td>
<td>0.294 (0.0366)</td>
<td>0.320 (0.0502)</td>
</tr>
</tbody>
</table>

about 3.6 percentage points in the 2000 data, both reasonable figures given the age range in consideration. Second order effects are also measured to be positive in both data sets, suggesting that between ages 50 and 59 retirement occurs at an increasing rate. This is, again, a sensible number given the age range in question. Between ages 60 and 69 we again find positive first order estimates of the effect of age on retirement rates, as the 1990 data yields a 2.5% increase in the proportion of individuals retired for each additional year.
FIGURE 1
of age and a 2.3% increase in the 2000 data. Second order effects are statistically insignificant but negative in the 2000 data, which might be expected for a population in their 60's, and essentially zero in 1990.

Figure 1 contains graphs of the percentage of individuals who are not currently in the labor force for both the treatment and control groups by age in 1990 and 2000. Though the control groups in both 1990 and 2000 have smoothly increasing proportions of individuals not in the labor force across all ages, there is an obvious jump for pilots at age 60 explained by the Age 60 Rule. This jump is, as was suggested by our difference-in-difference model earlier, similar in magnitude across the two census data sets. This confirms that the legislation has indeed delivered an exogenous shock of retirement to the population of pilots, which we can exploit to determine if retirement has a significant impact on mortality rates.

b. The impact of mandatory retirement on mortality

In order to observe mortality data for the treatment and control samples, we use the Multiple Cause of Death Data Set (MCD) from the National Center for Health Statistics. The MCD data set records every certified death that occurs in the United States along with a number of characteristics of the deceased. These include age, gender, educational attainment, state of residence, and a number of other specifics regarding the circumstances of an individual death. In addition, from 1985 until 1999 states were encouraged but not required to record the associated occupation and industry of the deceased. Information was obtained post mortem and was gathered from family members or others who knew the deceased well. These people were asked what the deceased's “usual occupation” was,
even if the individual had been retired from that occupation at the time of death, and what their “kind of business/industry” was. These occupations and industries were recorded using the same Bureau of Labor Statistics Occupational and Industry classification systems used in the Census PUMS used in the previous section. This allows us to properly match the underlying populations studied in our first order estimates and in our mortality estimates.

Between 1985 and 1999, twenty-six states reported the previous occupation of the deceased in their state, though many of these states did not record for the entirety of the 15-year period. Many states began recording a few years into the program and many exited prior to 1999. In order to prevent fluctuations in the reporting states from influencing our data, we observe only a set of 16 states that reported the occupations of the deceased over a continuous set of years from 1989 to 1998. There is an inherent trade-off in choosing this set of states, as we could expand the years during which we observe mortality data, but only at the cost of observing data from fewer states over that time horizon. The states chosen were judged to be an optimal choice of the “most population for the most years”. These states are shown in Figure 2, and are generally a good nationally representative set. The reporting states tend to be clustered together geographically. Georgia, North Carolina, and South Carolina reported, but none of their surrounding states did, as was the case with Nevada, Utah, Idaho, Colorado, New Mexico, and Kansas. However, because these clusters of states are spread across the country, it gives a broad geographical mix.

A limitation of using the MCD data set is that it does not capture as large a population of pilots as is ideal. The 16 states that we use are on average smaller states, and none of the
We extract raw death counts from the 16 states shown above for all years between 1989 and 1998. We then use the occupational and educational variables associated with each death in order to construct two groups that mirror the control and pilot groups used in our first-stage estimates. As before, both groups are limited to males and the control is restricted by educational status to approximate the educational attainment of the pilots. These data sets give the number of deaths in each group for any given age in any given year from our 16 selected states.
Because the MCD data gives us only raw death counts for the pilot and control groups, where we are interested in mortality rates, we turn our attention to estimating the size of the underlying populations from which these deaths derive. This in effect will give us the denominator of our mortality rate. We again turn to the Census PUMS, where we use the 1990 5% survey in order to estimate the size of each cohort of relevance to our study—i.e. any cohort that, between 1989 and 1998, was between the ages of 50 and 69 at any point. For both the control group and the treatment pilot group, we find the number of individuals in each cohort that appear in the CPS survey, and estimate the total number of underlying individuals by simply multiplying each number by \( \frac{1}{0.05} = 20 \).

\[ (2) \quad \text{PUMS respondents}_{gc} \times \frac{1}{0.05} = \text{Total underlying 1990 population}_{gc} \]

Once estimates of the 1990 underlying populations are found, we use the MCD data to add those who died in 1989 in each group-cohort combination to find the underlying 1989 populations. We then subtract the 1990 deaths from the 1990 underlying populations to find populations for 1991, and subtract the 1991 deaths from the 1991 underlying populations to find the 1992 underlying populations, and so forth. In this way, we are able to obtain estimates of the underlying populations for every relevant age-year population for each of the treatment and control groups.

Using the raw death counts and the estimated underlying population counts we then estimate the mortality rates by dividing the raw death counts by the relevant underlying population size. These estimated mortality rates are the basis for our investigation of potential mortality rate differentials between pilots and our control group occurring after the age-60 cutoff.
III. Main Estimation Strategy

In order to analyze mortality rates in our treatment and control groups, we utilize the concept of the Gompertz Law of Mortality, a theory first posited by Samuel Gompertz in an 1825 article. The law takes the form of

\[ MR(x) = \alpha e^{\beta x} \]

where \( MR(x) \) predicts the mortality rate of a cohort of age \( x \), and \( \alpha \) and \( \beta \) are constants. The Gompertz equation is well known to model mortality rates in humans accurately, provided that age \( x \) is neither too low—the equation tends to fail below about age 30—nor too high—it also tends to fail as age approaches 90. Given our age range between 50 and 69, the Gompertz curve should provide a good model with which to analyze mortality rates in our population.

A natural mathematical result of the Gompertz law is that \( \ln(MR(x)) \) is a linear function of age of the form

\[ \ln[MR(x)] = \ln(\alpha) + \beta x \]

If there were a significant impact of our exogenous shock of retirement on our pilot population, we would expect to see a change in mortality rates starting at age 60. It is unlikely that the any change in mortality rates will occur abruptly at age 60 so we cannot estimate a regression discontinuity model as we did in the previous section. It is likely that, were retirement to increase or decrease the risk of death, changes in mortality rates would gradually accumulate, and hence there might be a departure of the mortality rates from their expected “Gompertz curve”—the linear logarithmic pattern—over a period of several years. This differential would likely exist for some time following the age 60 cutoff, as we see in Figure 1 that retirement rates remain elevated relative to what might be expected of
the population if there were no legislated retirement age in the treatment group. It would seem that, as a result of the mandatory retirement age, some fraction of the pilot population retires—some 30% or so if our first stage estimates are to be believed. This fraction likely remains retired for the remainder of their lifetimes. Those who did not retire at age 60, either because they were not subject to the Age 60 Rule or because they decided to take up employment in some other capacity following their departure from the Air Transportation industry, likely retired at normal rates for the remainder of their lifetimes. This result should give us a consistently larger retirement population across our age 60-69 period of treatment observation. In effect, what we would expect to see in this population if retirement impacts mortality is a movement onto a “new” Gompertz curve, still linear logarithmic but with a different slope than was present prior to age 60.

We now analyze mortality rates for our comparison group and our pilot treatment group. We run a regression of the form

\[(5) \ln(MR_{cy}) = B_0 + age1_{cy} B_1 + age2_{cy} B_2 + \lambda_c + u_{ay}\]

where \(MR_{cy}\) is the mortality rate for cohort \(c\) in year \(y\). The variable \(age1\) is defined to be equal to age until age 59 and equal to 59 for every age greater than 59. \(Age2\) is equal to 0 if age is less than 60, and is equal to age-59 for all ages 60 or greater. These variables allow us to form two separate linear relationships with \(\ln(MR_{cy})\) above and below age 60. We expect both \(B_1\) and \(B_2\) to be positive in accordance with the Gompertz curve. Note that by construction \(age1 + age2 = age\) and hence if there is no shift in mortality at age 60, then \(B_1 = B_2\). However, if mortality begins to rise after age 60 because of mandatory retirement then \(B_1 < B_2\) and if retirement decreases mortality rates then \(B_1 > B_2\). The variable \(\lambda_c\) are cohort fixed effects. There is presumably a secular trend in mortality over the time period
studied, as mortality rates likely shift for a given age group over the years. Dummy variables for cohort control for any such trend, and allow us to isolate the effect of age across the 10-year observation period without any interference from inherent differences between those who were a given age in two different years.

Given the size of the comparison group, each calculated mortality value is a non-zero positive and hence we are able to run this model as an OLS regression. Since there are far fewer pilots, the annual cohort-specific mortality rate is sometimes zero and. Since ln(0) = -∞, such regression cannot be done. Instead, for the pilot population we utilize a Poisson regression in order to capture the same logarithmic regression without the need to use a direct logarithm. Within a Poisson regression, we assume mortality counts are generated by a Poisson distribution. In this case, let \( y_{cy} \) be counts of deaths for cohort \( c \) in year \( y \) and let \( \text{POP}_{cy} \) be the underlying population that survives until that year. By definition

\[
\text{MR}_{cy} = \frac{y_{cy}}{\text{POP}_{cy}}.
\]

In the Poisson specification \( E[y_{cy}] = \exp(B_0 + age1_{cy} B_1 + age2_{cy} B_2 + \lambda_c + \ln(\text{POP}_{cp})) \) which means that \( \ln E[y_{cy}] = B_0 + age1_{cy} B_1 + age2_{cy} B_2 + \lambda_c + \ln(\text{POP}_{cp}) \) and hence \( \ln \{ E[y_{cy}] / \ln(\text{POP}_{cp}) \} = B_0 + age1_{cy} B_1 + age2_{cy} B_2 + \lambda_c \). The coefficients in this regression can be interpreted in exactly the same manner as the coefficients in the OLS regression of \( \ln(\text{mortality rate}) \) used in our comparison group.

An alternative specification would be a Negative Binominal regression, which allows for “over-dispersion” in the data—where variance/mean is not equal to 1, as is true by definition in a Poisson regression. The Negative Binominal model allows for the estimation of an over-dispersion parameter \( \alpha \) where the variance to mean ratio is defined as \( 1 + \alpha \). In our pilot population, we find that we are unable to reject the null hypothesis
that $\alpha=0$. Thus, since we cannot show that mean $\neq$ variance, we utilize a Poisson regression rather than a Negative Binomial.

The standard Ordinary Least Squares regression of the form shown in equation (5) and the equivalent Poisson regression that we use to analyze the pilot population will both yield unbiased estimates of the percent change in mortality rate associated with an additional year of age below and above the age 60 cutoff.

We also run the same regressions—OLS logarithmic for the control sample and Poisson for the pilots—without the double regression. That is to say, we regress mortality rates on raw age and cohort effects, without the cutoff at age 60. This allows us, particularly in the case of the control group, to assess the goodness-of-fit of the Gompertz curve to our data.

**IV. Results**

Table 3 gives the results of our mortality rate regressions. Column (1) gives the OLS regression of $\ln(MR)$ on raw age—without a break at age 60—and cohort effects in our control sample. We estimate a coefficient of 0.097 on the age variable, suggesting an increase in mortality rates of 9.7% with every additional year of age across the entire age range from 50-69. This result is somewhat remarkable in that it exactly matches a result from a frequently cited 1973 article by RL Prentice and A El Shaawari in Applied Statistics that used mortality data from Ontario in order to assess the accuracy of the Gompertz Law. Prentice and Shaawari break down their population by gender and by age range. The result of their Gompertz curve fitting for men between the ages of 30 and 70—the age
range that they felt fit the curve most accurately—was precisely a 9.7% increase in mortality per year of age.

The $R^2$-value on the column (1) regression, which is in excess of 0.99, further corroborates the goodness of fit of the Gompertz curve in our control group. This suggests that, over our age range from 50-69, there is indeed a linear relationship between $\ln(MR)$ and age in our control group. A similar regression of mortality on age and cohort effects is shown in column (3) for the pilot population. This is of less consequence because we have reason to believe that this population is not “smooth” across the age range of 50-69 due to the notch in retirement at 60. Nonetheless, we estimate the coefficient on the age variable

**TABLE 3**

<table>
<thead>
<tr>
<th></th>
<th>Control Single Regression</th>
<th>Control Double Regression</th>
<th>Pilots Single Regression</th>
<th>Pilots Double Regression</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Age</td>
<td>0.0970 (0.0015)</td>
<td>0.0871 (0.020)</td>
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<td></td>
</tr>
<tr>
<td>Age, Through 59 (A)</td>
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<td>0.0980 (0.0025)</td>
<td>0.0621 (0.034)</td>
<td></td>
</tr>
<tr>
<td>Age, 60 and Above (B)</td>
<td></td>
<td>0.0960 (0.0023)</td>
<td>0.103 (0.027)</td>
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<tr>
<td>Test that A=B, P-Value</td>
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<td>0.381</td>
<td></td>
</tr>
<tr>
<td>Statistical Model</td>
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<td>OLS</td>
<td>Poisson</td>
<td>Poisson</td>
</tr>
<tr>
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<td>0.994</td>
<td>0.416</td>
<td>0.417</td>
</tr>
<tr>
<td>Observations</td>
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<td>202,274</td>
<td>449</td>
<td>449</td>
</tr>
</tbody>
</table>
to be 0.087, suggesting that in the pilot population an additional year of age increased mortality rates by 8.7%. While this is slightly lower than our estimate for the control population, the two estimates are not statistically significantly different, leading us to believe that the control group still serves as a good comparison to our treatment group of pilots. The $R^2$ values on the pilot regression are much lower than those on the control regression, as would be expected given the much smaller observable population from which we are extracting mortality data. Further, a kink in the Gompertz curve such as that which we are looking for at age 60 would naturally cause less “goodness of fit” in the pilot population, since we are trying to fit a single line on what may be a piecewise function.

Column (2) in Table 3 gives the results of our two-part regression of $\ln(MR)$ on $age_1$, our regression prior to age 60, $age_2$, our regression for ages 60 and above, and cohort effects. We estimate a coefficient on $age_1$ of 0.0980 and a coefficient of 0.0960 on $age_2$. These estimates are remarkably close, and give very strong evidence that there is no differential in the growth of mortality rates above and below age 60 in our control population. Despite relatively precise estimates as a result of a large sample size for the control, the coefficients are not significantly different from each other. In order to show this, we run a simple t-test to determine whether the coefficient on $age_1$ is equal to that on $age_2$. The result of the test is shown in Table 3 and yields a P-value of 0.579, suggesting that there is no evidence that the Gompertz curve shows any change in slope before and after age 60. This regression, not surprisingly, also shows remarkable “goodness-of-fit”, with an $R^2$ of, like before, greater than 0.99. Given the similarity of the control group to our pilot treatment group, we should expect that, if retirement were to have no bearing on
mortality rates, we should find the same absence of a differential in the rates of change of mortality rates above and below age 60 in our pilot population.

Column (4) of Table 3 gives the result of the two-part regression of MR on $age_1$, $age_2$, and dummy variables for cohort effects in our pilot group. As was stated before, in order to circumnavigate statistical problems resulting from the presence of zero values in our pilot mortality data, we utilize a Poisson regression in this analysis. The numbers estimated, however, should be equivalent to those of the OLS regression used in the control population. We estimate the coefficient on $age_1$ to be 0.0621, suggesting that, prior to age 60, mortality rates in pilots are rising at a rate of 6.21% per year. This estimate is below its equivalent in the control population, suggesting that there may be some fundamental difference between the two groups, because of retirement rates or otherwise. We estimate the coefficient on $age_2$ to be 0.103, suggesting that an additional year of age, between ages 60 and 69, increases mortality rates by 10.3% per year.

This appears to be a rather strong result. According to our estimates, mortality increases by a rate that is over 4 percentage points higher after age 60 than before in our pilot population. This result would suggest that retirement, at least in a population of male pilots, may increase mortality rates significantly. It is difficult to explain such a change in the Gompertz curve otherwise as, even if there were significant fundamental differences between the control and treatment populations, it would be unlikely that there should be a kink in the Gompertz curve unless there were a significant and sudden change in the population at the age in question. Even differentials in variables like income, health care access, and social status would more likely cause a difference in slope of the entire Gompertz curve rather than a kink.
Our results also must be considered in light of the fact that only about 30% of pilots seem to have retired as a direct result of the Age 60 Rule. The remaining 70% were already retired, were not subject to the rule, or found other work after they were made to leave their jobs as pilots in the Air Transportation industry. Our results are essentially only an intent-to-treat result, for which it is difficult to estimate a final treatment effect. This would suggest that, if indeed the difference in the slope of the Gompertz curve found in our regressions is the direct result of the retirement notch, then the actual impact of retirement would be estimated to be even larger.

Unfortunately, our results, though intriguing and certainly suggestive of an effect, fall short of statistical significance. Again, we employ a simple t-test to determine if the coefficient on \( age_1 \) is equal to that on \( age_2 \). The result of this test is a P-value of 0.381, suggesting that we cannot statistically reject the hypothesis that \( age_1 - age_2 = 0 \). This is largely the result of an undersized data set. As mentioned before, we were only able to collect data on pilots that died in 16 U.S. states over the course of our study. These states, further, tended to be smaller states on average, and the largest six states in the nation, which comprise over 40% of the U.S. population, did not report. In the end, the size of the standard errors on our estimates, not the magnitude of the difference between the values on the \( age_1 \) and \( age_2 \) coefficients, prevented statistical significance. A larger sample size would be necessary in order to obtain sufficiently accurate results.

V. Conclusions

Though our results do not emphatically prove a statistically significant impact of retirement on mortality, they do provide strong suggestive evidence that mandatory
retirement may increase the risk of death, and give strong reason to further investigate this connection. Reasons for this relationship are not clear, but it seems that a frontrunner might be the difference between the levels of mental and physical activity in individuals that are working and those that are retired. Higher mental and physical activity levels tend to be correlated with better health and lower mortality rates, though it is easy to see how this relationship might not be wholly causal. It seems that there is likely a decrease, on average, in mental activity after retirement, though there does not seem to be well-established evidence of this link in the literature. Physical activity may or may not decrease with retirement, as a 2009 study by Chung et al. suggested that physical activity might in fact increase at retirement in those with certain sedentary jobs. Nonetheless, a decrease in consistent activity at retirement may be one of the leading candidates for the fundamental reason behind an increase in mortality rates after retirement.

Despite the difficulty in obtaining significant estimates with our particular data set, the use of legislated mandatory retirement ages as a quasi-experiment still seems a potentially fruitful way of studying the impact of retirement on health or other outcomes without the interference of selection bias. The age-60 retirement notch in pilots could be more accurately studied if Social Security records of deceased pilots could be obtained, allowing for mortality data to be collected from all 50 states, rather than only the 16 we were able to use. This would also allow for the ability to study years outside of the period from 1985-1999 when the National Center for Health Statistics collected occupational information with death records. It may also be useful to study the age-60 notch in pilot retirement by observing other health characteristics like blood pressure levels, cortisol levels, or other biomarkers associated with the types of diseases and disorders thought to be impacted by
the lifestyle change that generally occurs with retirement. By using health statistics other
than mortality numbers, a study could observe a much larger group of pilots around the age
60 cut off. Because Americans generally do not die at particularly high rates at age 60, the
use of mortality as an outcome of choice significantly reduces the amount of useable data
available. It is not, however, immediately apparent what data sets in existence might be
useable to link occupation with biomarkers, as few data sets note the occupation of the
subjects, especially when the individual retires.

It is worth noting that the Age 60 Rule was abolished in 2007 and replaced with the
Age 65 Rule, which is currently the law of the land. As mortality rates are higher at age 65
than they are at 60, this may present a more viable notch to study than the notch used in our
study. Further, as the average retirement age continues to increase, the age 65 increasingly
looks like the better of the two ages at which to study an exogenous shock of retirement, as
the results of such a study might be more accurately applicable to a larger segment of the
American public.

While our results shed a bit of light on the causal relationship between retirement
and mortality, it is vitally important to further investigate this link. Understanding the
relationship between retirement status and health is crucial to making decisions regarding a
number of policy actions—perhaps most importantly the question of Medicare cost
structures in a time of rapidly changing retirement rates—as well as personal retirement
decisions.
References


