Retirement Age and Longevity

Mark Borgschulte*

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Abstract

This project investigates the relationship between retirement age and mortality. Building on the cross-country work of the International Social Security Project detailing the implicit retirement incentives in the social security and retirement insurance programs of OECD countries, I analyze the effect of eligibility age reforms on mortality outcomes. The West German reform of 1973 serves as a case study; I also analyze France's 1972, Sweden's 1976 and Denmark's 1979 reforms. Results indicate a positive relationship between retirement eligibility age and subsequent mortality outcomes, i.e. reducing retirement eligibility age decreases mortality in affected cohorts. Program reforms coincided with changes in mortality consistent with a 1 to 4% effect on the base level of age-specific mortality for each year change in retirement eligibility age. I present a simple model of an optimal pension program to demonstrate the role of the semi-elasticity of mortality with respect to retirement age and the magnitude of my results.

*PhD Candidate, Department of Economics, University of California, Berkeley. This projects uses data from the Human Mortality Database. I gratefully acknowledge the support of NIA Grant # T32 AG00246. I would like to thank Emmanuel Saez, David Card, Alan Auerbach, and Ron Lee for their help and encouragement with this project. Evan Brand and Angela Wong provided useful research assistance. **This version is preliminary, please do not cite.**

1 Introduction

As population aging in the OECD progresses, proposals to raise social security retirement eligibility ages present one margin of adjustment for debt-constrained governments. The Congressional Budget Office estimates allowing the normal retirement age to rise to 68, rather than 67 as is currently legislated, would reduce social security outlays by 0.2% of GDP per year in 2040 or 3% of outlays ($29 billion in 2010, or $50 billion in 2040 at 2% GDP growth), and reduce lifetime benefits by approximately 6% for the 1980-2000 cohorts. (Option 26, CBO (2010)). Borsch-Supan (2010) estimates removing implicit taxes in Germany's public pension would result in the average retirement age rising by 3 years, with an annual gain of 1.2% of GDP. In October of 2010, the French Parliament voted to increase retirement ages from 60 to 62 in the face of growing budget deficits. As these reforms move from proposal to law, the effect of retirement age on population health is a central parameter, both in assessing the fiscal impact of these reforms and their welfare implications.

Despite previous attempts to uncover health effects of retirement age and pension income, the current literature does not agree on the magnitude or even sign of the effects, and solid evidence remains elusive. [1] In recent published work, Snyder and Evans (2006) find a negative effect of working on mortality in the 1917-1918 "notch" cohort in the United States, but Handwerker-Weber (2011) shows mortality differences between cohorts predate the "notch" reform. Another line of research uses cross-country statutory retirement ages as an instrument for actual retirement behavior, in an attempt to uncover the causal effect of retirement on various health outcomes (Neuman (2008), Rohwedder and Willis (2010)). While important evidence on the cross-sectional correlations, establishing causality from retirement to health using this strategy rests on the assumption of random assignment of

[1] Coe and Zamarro (2008) and Wise (2010) summarize recent literature. Coe and Zamarro point to Minkler's 1981 survey of early work.

statutory retirement ages across countries. Across Europe, the correlation between statutory retirement age and lifespan is a robust 0.72 (0.16 in Western Europe), suggesting policy makers consider population health in setting the statutory retirement age. Coe and Zamarro (2008) use statutory retirement ages in a regression discontinuity design, and find small positive effects of retirement on health. At the individual level, endogenous selection into retirement complicates micro research designs. The endogeneity problem arises for several reasons: poor health and negative health shocks predict earlier retirement (Wise (2010) and others discuss) and retirement itself may affect health, as I show here. To further complicate the investigation at the individual level, individuals possess private, unobservable information regarding their life expectancy (Finkelstein and McGarry, 2006), which likely impacts the retirement decision.

Following the work of Gruber and Wise and the International Social Security Project (ISSP), this study uses the dates of OECD program reforms to identify the effect of retirement age on mortality both visually and in reduced-form specifications. The Gruber-Wise results provide evidence of a strong first-stage by documenting the effect of the changing retirement incentives on the distribution of retirement ages. I analyze in detail reforms in West Germany in 1973 and France in 1972, both of which appear in the original Gruber and Wise (1999) paper; I also present results from Sweden's 1976 reform, and Denmark's 1979 reform, used by Wise (2010). The results indicate a positive relationship between retirement eligibility age and subsequent mortality outcomes. The magnitude of the estimates imply a one-year decrease in retirement eligibility age decreases subsequent mortality by 1 to 4% each year following the reform. The main effects I find occur for over a decade after the retirement age has been reached, suggesting the primary effect is through health capital, rather than the incidence of health shocks.

This identification strategy rests upon the assumption that reforms do not coincide with other changes in mortality affecting the reform-eligible cohorts. I investigate this assumption using several sources. First, I draw upon narrative evidence on the proximate cause of the reforms. One worrying proximate cause of the reforms would be a positive correlation between increasing generosity of a pension program and underlying economic conditions, in which case the pension reform might spuriously coincide with slackness in the government budget constraint. In most of the cases I analyze, this effect can be ruled out. West Germany and France's reforms in the early 1970's were passed in response to high unemployment and a slowdown in economic growth; lowering the retirement age was hypothesized to stimulate to youth employment (Wise (2010) argues against such a link). Second, I use the mortality effect of the reform for cohorts too old to be eligible for earlier retirement to control in difference-in-differences specifications for concurrent changes. This helps rule out changes, for example in the country's old age health insurance, replacement rate of its pension system, or medical advances affecting all ages, such as the cardiac revolution. Finally, I construct a panel of similar countries to perform difference-in-differences and difference-in-differences-in-differences (hereafter, diff-in-diff and triple difference) analysis, helping rule out confounders affecting reform-eligible cohorts in multiple countries.

To guide and interpret the empirical analysis, I present a simplified models of retirement timing and pension budget balance. The retirement timing model uses the textbook labor supply model, augmented with an endogenous time endowment meant to represent an endogenous lifespan. After presenting the empirical results, I simulate a pension reform using my estimates in order to demonstrate the role of the estimated parameter and the magnitude of the results. I calibrate the model to the current US Social Security system, and follow the actuarial effects of increasing the social security eligibility age by one year. (Note: this section is incomplete, and not included in the current version.)

Section 2 describe the data, and methodology I use to identify reforms.

Section 3 details my empirical strategy, using the West German reform of 1973 as a case study. Section 4 expands these results to three other reforms. Section 5 calibrates a simple model of a US Social Security system reform, using the range of estimates of the semi-elasticity of mortality with respect to retirement age. Section 6 discusses conclusions and future work on this project.

2 A Simple Model of Retirement

In this section I describe a simple model of retirement timing. I use a one-period labor supply model, in which the only decision the worker faces is a labor-leisure trade-off. The innovation in the model is the inclusion of an endogenous time endowment, reflecting endogenous lifespan. Lifespan depends on two terms- the lifetime income of the worker, and the quantity of labor supplied. Many elements of the real-world retirement decision are left out, but I believe we get some guidance in how to think about the estimated effects in the following sections.

2.1 Model

Workers maximize their lifetime utility, $u(c, R)$, where $c = y$ is lifetime income, all of which is consumed, and R is the years of life spent in retirement. Utility is concave in both arguments. I model the worker's lifetime as occurring in a single period. Workers earn a wage w, and have non-labor income I^0. Workers must divide their time allocation, T between labor force participation, L, and retirement, R, where $T = L + R$. $T = T(y, L)$ is a function of lifetime income, $y = wL + I_0$ and years spent working, L. [2]

[2] This model abstracts from several relevant features of the retirement decision. For example, although I allow the labor supply choice to affect lifespan, I simplify the model by excluding T from the lifetime income component of utility. It is easy to modify utility to $u(\frac{c}{T}, R)$, though the expressions become messier. Further improvements would expand the model to include worker heterogeneity, health expenditures and to allow an intensive

Workers face a single choice in this model, namely, how much of their time to devote to work. Their program is

$$max_L \; u(wL + I^0, T(wL + I^0, L) - L),$$

which yields a marginal rate of substitution at the optimum,

$$\frac{U_R}{U_y} = \frac{w}{1 - T_L - wT_y}.$$

This expression differs from the usual MRS condition for labor supply (i.e. in the absence of endogenous lifespan), where we assume $T_L \equiv \frac{\partial T}{\partial L} = 0$ and $T_y \equiv \frac{\partial T}{\partial y} = 0$. Under these assumptions, the worker sets $MRS = w$.

Incorporating endogenous lifespan into the model can increase or decrease the optimal retirement age, depending on the sign and relative magnitudes of the partials of T. The sign of $\frac{\partial T}{\partial y}$ is almost certainly positive, as income can be used to purchase health (Lindahl, 2002). Although some studies have found short-run increases in mortality following income receipt (Evans and Moore, 2009), and a countercyclical trend in mortality relative to the business cycle (Ruhm, 2005), these effects appear to have much shorter durations than the changes in mortality I find here. It will be important to consider how this parameter affects the empirical results below, particularly how the reforms may have changed lifetime income.

The sign and magnitude of $\frac{\partial T}{\partial L}$ is less clear, and the subject of this paper. This effect could be positive, for example, if labor force participation increased worker's social interactions, and social interactions promote health. Along these lines, Willis and Rohwedder (2010) find a negative effect of retirement on cognitive abilities. Alternatively, the effect could be negative, if work is stressful, or physically debilitating, in which case $T_L < 0$. If T_L were positive, estimates of T_L (and associated elasticities) will be confounded

margin in the work choice. Regardless, I hope this simple model provides some intuition for my thinking and I leave these improvements for future versions.

with wT_y, i.e. exogenous changes in L will affect T through both through the direct effect of the change in working years, and indirectly, through income earned or not earned as a result of the reform. If T_L is negative, as is the case in my data, the change in income works to bias the estimated effects towards zero. Due to this bias, my results are likely smaller in magnitude than the truth.

2.2 Pension Systems and Reforms

The simple model I describe does not include a role for a welfare-improving pension system, as there is no market failure or redistributive role for the government. However, it is easy to incorporate the types of reforms considered here in the above model. During this era, and today, the pension systems of many OECD countries feature strong incentives to retire upon reaching benefit eligibility ages. These incentives are the subject of **?** and the International Social Security Project. The clearest example of these incentives are rules requiring workers to retire before beginning to collect a pension, without provisions to increase pension payments if retirement is delayed. This creates a situation where delaying retirement results in a large loss of pension wealth, i.e. an implicit tax on continued work.

I can incorporate this change in the simple model above by imposing a tax τ on work past some age $\bar{L}$. This introduces a kink in the worker's budget constraint at age $\bar{L}$, as the MRS changes discontinuously from

$$\frac{U_R}{U_y} = \frac{w}{1 - T_L - wT_y} \rightarrow \frac{U_R}{U_y} = \frac{w(1-\tau)}{1 - T_L - wT_y}$$

.

Workers with MRS between these two values will choose $\bar{L}$ as their optimal retirement age. Empirically this will appear as bunching at the retirement eligibility age, a feature common to all of the systems I study. In the case of the pension systems considered here, τ is quite large, often above

0.5, and most workers who have not retired by $\bar{L}$ will choose to do so upon reaching this age.

In this model, consider a reform that lowers $\bar{L}$ to $\bar{L}'$. In principle, this will affect worker lifespan through both the labor and income channels. The first-order substitution effect will exogenously lower the retirement age of all workers previously retiring at $\bar{L}$, and those workers who would have chosen to retire between $\bar{L}$ and $\bar{L}'$. There will also be a second-order effect through the income channel, as the increase in tax rates on work between $\bar{L}$ and $\bar{L}'$ has a negative income effect, inducing workers to work more. (This is second order in that it only affects work between $\bar{L}$ and $\bar{L}'$. One possibility not considered in this simple model is that workers may substitute on the intensive margin in the years preceding $\bar{L}'$ to offset the negative income effect, e.g. workers may choose to work longer hours, or exert more effort upon the shortening of their career.

2.3 Ideas for a more complete model

See footnote above for extensions to the static model I use here.

A more complete model would include health capital. An individual's stock of health capital would affect their age-specific mortality rate. Investments would be made in health capital, either through leisure activities such as exercise or cooking at home, and other forms of capital, through work, which may reduce health capital. Retirement would result in a discontinuous change in the price of each type of investment- health investments would now be cheaper, asset investments would become more expensive, and the effect of work on health would be zero.

Grossman's Handbook of Labor Economics chapter is a good starting point. Income Inequality and Medical Expenditures by Serdar Ozkan (2011) is a very nice, more recent, dissertation on the subject.

In such a model, a change in the retirement age would induce adjustment along several margins. Consider a lowering of the retirement age. Workers would face a shortened period of remaining work-life, leading to a substitution towards asset investments, and away from health investments. The result would be a pattern of mortality consistent with my findings- an increase in mortality upon impact, followed by a reversal following retirement.

Such a model is beyond the scope of this paper, as it would ideally require individual data on labor supply, savings, health status, and time use to estimate.

3 Data and Methodology

This paper uses data from the Human Mortality Database (HMD). The HMD contains annual mortality rates disaggregated by year of birth for 37 countries or areas, including historical data for for most European countries. These mortality rates represent outcomes for the entire population of each country; imputations for death rates at higher ages, or divisions into civilian and military death rates contained in the HMD are unnecessary for the countries, years and ages I use in this paper.

Since I explore the effect of retirement age on mortality, and no corresponding cross-national database exists for historical retirement patterns, I focus the majority of this paper on two reforms analyzed closely by Gruber and Wise, the 1973 introduction of early retirement options in West Germany and the 1972 decrease of the French normal retirement eligibility age from 65 to 60 for both men and women. The retirement effects of both of these reforms are carefully documented in Gruber and Wise, and I include several figures from their paper that illustrate the changes in retirement behavior that followed these reforms.

The extension of this analysis to other reforms requires some decisions about the chronological and geographical scope of the investigation. State-run pension systems extend back through the 19th century, however, early systems often featured low coverage, or low benefits, suggesting a weak link to population-level retirement behavior. [3] Following World War II, most of Western Europe expanded state-run pension systems to cover the entire civilian population; unfortunately, reforms immediately after the war are difficult to disentangle at the population-level from the aftermath of the war itself. More recently, reforms in the last two decades have frequently been fazed in over a period of years, or added actuarial adjustments to benefit calculations, meaning that no sharp break in mortality outcomes can be expected. (This is likely the result, at least in part, of the Gruber-Wise study.) Finally, data from reforms in the recent past have not affected enough cohorts to allow for a meaningful sample size.

For these reasons, I limit my investigation to reforms from 1960 through 1990. As well, I limit the geographic scope to Western European countries featured in the Gruber-Wise project, as the project's country-studies provide an important source of information about labor force participation and retirement decisions, even if the specific reforms I locate are not analyzed in the same depth as the French and West German reforms mentioned above. Focusing largely on countries in Western Europe will be helpful as I can employ difference-in-difference and panel techniques as additional controls for underlying mortality trends. Several interesting reforms are excluded from this window, and I hope to include them in a supplemental paper. [4]

To locate candidate reforms, I use two sources. First, the United States Social Security Administration publishes an overview of retirement systems called Social Security Throughout the World. This book (now also an on-line publication) summarizes the key features of state-run pensions in most countries of the world. By comparing the normal and early retirement eligibility ages in the 1958 publication with later publications, I can narrow down the year of reforms to within a two year time frame in most cases. Second, the aforementioned Gruber-Wise country studies provide descriptions of several

[3] On the origins of state-run pensions: http://www.ssa.gov/history/ottob.html

[4] Canada, Taiwan, New Zealand...

reforms located in the above manner.

4 Empirical Strategy and West German Reform of 1973

I employ several methods to document the effect of the reforms. The results appear robust to the different specifications and identifying assumptions. In this section I explain the general method.

4.1 West German Reform of 1973

Since the identification strategy hinges upon the timing of reforms, I attempt to detail the historical context and institutional features of each reform with an eye for concurrent events that may bias the results.[5] In this subsection I use the reform in West Germany in 1973 to present an illustrative case study. The reform introduced an early retirement option and opened unemployment and disability pathways to retirement for younger workers. Within 10 years of the reform, the mean age at retirement had fallen over 4 years. Several features of the pension system and the reform give the West German reform a strong case for both a relevant and plausibly exogenous first-stage relationship between the reform and resulting retirement behavior in the population.

Previous research has established the relevance of the reform to West German retirement behavior (Borsch-Supan and Schnabel, 1997). Before 1973, West Germany's public pension plan allowed retirement at age 65 and through a disability channel for younger workers. The reform of 1973 introduced an early retirement option for men at age 63, conditional on 35 years of service, and women at age 60, conditional primarily on 15 years of service. As well, the reform expanded pathways to retirement through an unemployment

[5]This section draws extensively from Borsch-Supan and Schnabel (1997) and Eric Solsten (1995).

channel, and through an expanded disability channel, which allowed workers to retire if they passed one of several earnings tests. Figure 2, (taken from Gruber Wise 1999) illustrates the mean retirement age following the reform. Gruber and Wise (1999), Borsch-Supan and Schnabel (1997), and many others have argued for a strong causal link between the retirement eligibility age and retirement patterns. Borsch-Supan presents additional evidence that the reform was the primary cause of the decline in the retirement age. All in all, the relevance of the reform seems well-established.

One threat to my identification strategy would arise if reforms that increase the generosity of the pension system tend to be passed during periods of economic booms, which might also accompany improvements in medical care, and a general expansions of other social safety net programs. The circumstances of the reform of 1973 make the timing of its passage an unlikely explanation for the resulting mortality patterns. The reform was enacted by the embattled administration of Karl Schiller in the face of rising unemployment rates in mid-1972; Schiller left office before it was implemented. The post-war boom in West Germany was ending in the early 1970's, amid an international slowdown, and lowering the retirement age was proposed as a stimulus to youth employment. (Though most research has failed to find evidence for these sorts of benefits, some policy-makers still cite this logic when debating changes to the retirement age; see Wise (2010).) Because of the worsening economic conditions at the time of its passage, omitting the economic conditions from the estimation does not appear to be a source of downward bias in the mortality rates for the West German reform of 1973- or for the French reform of 1972, discussed in the next section. Some researchers have proposed a counter-cyclical relationship between mortality and the business cycle (Ruhm, 2005); however, the timing of mortality improvements and long-run effects of the reform argue against such an interpretation.

While narrative evidence can rule out a spurious relationship between the timing of the reform and certain economic conditions, in general, any concurrent reforms that affect old-age health could bias the estimation. I attempt to deal with the problem econometrically in three ways: first, by controlling for period effects using non-reform eligible cohorts within my sample, fixed effects, and by using a panel of Western European countries and a generalized difference-in-differences strategy. The first strategy should address concerns about concurrent changes within West Germany, except those specific to the ages and cohorts affected by the reform in question. The second addresses issues such as age-specific improvements in medical technology or public health that might be common to other European countries. Note that the panel estimation will deliver results biased towards zero, as most of Western Europe was experiencing a decline in both statutory and mean retirement age. Results are detailed below.

4.2 Gompertz Mortality and Cohort Trend

The simplest model I use to illustrate the effect of the reforms is to estimate the age-profile of mortality with a cohort trend, and ask if the cohorts and age groups affected by the reform departed from the pre-existing trend. This model builds on the Gompertz model of mortality, which proposes a linear relationship between the natural log of the mortality rate and age. Figure 1 confirms the near linearity in the age-profiles of mortality for the 1905-1922 cohorts for West German men. Empirically, the Gompertz model provides a good fit in low-mortality countries from age 45 or 50 until age 90, when the mortality curve begins to flatten. (I use ages 51 to 80 to avoid weighting observations by surviving population, as the older ages exhibit more variability and represent a small fraction of the initial population. Results are qualitatively similar when the analysis is extended to older ages and/or weighted by surviving population.) When I estimate the Gompertz model with a linear trend in cohort on West German mortality from 1960-1995, I find an R^2 of 0.945. Adding higher-order age terms further improves the fit in almost all specifications. I report results for the simple model with linear age and cohort terms, and use a cubic in age when a polynomial in age is called for elsewhere. For the complete West German sample, a cubic in age and linear cohort term deliver an R^2 of 0.976.

In the basic specification, I estimate:

$$log(mortality_{c,a}) = \beta_0 + \beta_1 Eligible_{a,c} + \beta_2 c + f(a) + \epsilon_{c,a}, \tag{1}$$

where c indexes cohort, a indexes age, $f(a)$ is linear in age (cubic in later specifications) and $Eligible_{a,c}$ is defined by

$$Eligible_{a,c} = \mathbb{1}\{c \geq \text{Reform cohort \& } y \geq \text{Reform year}\}. \tag{2}$$

Here, eligibility begins in the year of the reform for all cohorts young enough to experience earlier retirement. My intention is to capture the total effect of the reform, including the income effect and substitution behaviors, which begin in the year the reform is passed. (As my data is annual, and anticipation would introduce a downward bias in my results, I ignore anticipation effects.)

To isolate the changes in mortality in the retirement years, I augment the estimation with

$$RetireEligible_{a,c} = \mathbb{1}\{c \geq \text{Reform cohort \& } a \geq \text{Retirement Age}\}. \tag{3}$$

Note that the colinearity of age, cohort and year imply the year condition is met in the $RetireEligible$ equation.

Controlling for an independent period effect is an important element of the model. First, I augment the model with a pre- and post-reform dummy, i.e. an indicator for $y >$ Reform year. This dummy accounts for changes in mortality occurring among all cohorts in the sample, beginning in the year of the reform. In principle, it should account for any concurrent reforms, advances in medical technology or other changes in the disease environment that do not have a differential effect on the cohorts. In particular, it should address the decline in mortality in Europe in the 1970's attributed to the cardiac revolution, as the deaths due to cardiac disease showed similar declines for all ages above 50 Crimmins (1981). Second, I add year and cohort fixed-effects to the model. A joint test of the relevance of year-effects to the model, conditional on the inclusion of a cubic age term and cohort fixed effects, does not reject their inclusion, although many of the year effects are insignificant. The year effects should account for changes in medical technology in a more flexible form, as well as capturing year-to-year changes in the disease environment.

In the case of West Germany's 1973 reform, I use 1909 as the first reform-eligible cohort. The 1909 cohort is age 63 at the start of the year in 1973, meaning qualifying pensioners are immediately eligible for between one and two years of early retirement. The 1908 cohort is eligible for 0 to 1 years of early retirement, and is included with the non-eligibles. I use age 63 as the post-reform retirement age, as this is the new statutory age. Borsch-Supan and Schnabel (1997) reports the effective average retirement age fell from 63 to 58 (see Figure 2), and this transition took place over a decade. As the reform-eligible dummy does not account for the time path of retirement, it may understand the total effect of the reform.

Figure 3 plots the residuals for reform-eligible cohorts, where I estimate the model with pre-reform data and ineligible cohorts. The model is intended to generate a counterfactual, in which cohort-adjusted age-profiles of mortality among the reform-eligible cohorts evolve as the mortality of the cohorts too old to be eligible for earlier retirement. Hence, the identification comes from the convergence of mortality rates at older ages. Not surprisingly, the effect on the first four "intermediate" cohorts is smaller than those young enough to shift their behavior by a larger margin. What is more surprising is that the effect increases for younger cohorts. This suggests a dynamic model of retirement decision-making, in which the exogenous shift in retirement age leads to a new path to lifetime mortality. Younger cohorts can adjust along more margins, for example, by shifting savings, or increasing labor supply on the intensive margin.

Table 1 reports regression results for the linear model and Table 2 extends this analysis to the fixed-effects models with a cubic age term. Residuals are larger in Figure 3 because the model is re-estimated on the entire sample in the regression specifications. The identification in these regressions comes off a level shift in the cohort trend that coincides with the year of reform. Clearly, this does not match the pattern of mortality decline following the reform(s); however, such a strategy is useful for distilling a parameter we can interpret as the total semi-elasticity of mortality with respect to retirement age. Although the age-profile of mortality gain is important, it is the semi-elasticity which enters the steady-state optimization program of the worker and pension planner. Statistical significance of the β_1 coefficient indicates lower age- and cohort-conditional mean mortality rates for the reform-eligible cohorts, beginning in the year of the reform. Mortality rates show 1.2% decline by cohort in Western Europe over this period, and 8% increase by age, giving some guidance on the magnitude of the estimates.

A primary threat to this identification strategy is concurrent reforms that also improve mortality outcomes for the affected cohorts. For example, an expansion of an old-age pay-as-you-go medical insurance system in the same year as the social security reform would confound the effects of the medical insurance reform with the treatment I am trying to estimate. The Gruber-Wise country studies are useful for investigating this sort of confounder, and I have also checked myself for concurrent reforms. The only significant reform I have found is a minor increase in the social security replacement rate in the years following the reform, documented in Borsch-Supan. A simple check on this is the mortality outcomes of the older cohorts who retired before the reform. In general, the post-reform dummy does not alter the model estimates significantly.

Another potential confounder in estimating the effect of retirement age on mortality is the income effect of the reform itself. By focusing on the specifications which include only the *Eligible* term, I lump together these effects, by "turning on" the treatment in the year of the reform. This allows cohorts that have not reached retirement to "experience" the reform starting in the year in which the reform goes into effect, rather than when they reach retirement. This is the right model if these cohorts anticipate the reform, and alter their behavior earlier in life, in light of their now earlier retirement age, and earlier access to their pension. Alternatively, if retirement age has a distinct effect on mortality, we expect the $Eligible + Retired$ effect to be independently significant. A priori, since all the reforms considered here left pensioners the option of taking their previous bundle (by continuing to work beyond early retirement), and longevity is a normal good, we do not expect the total effect to increase mortality at all ages. Despite this prediction, it is possible workers may choose to substitute towards mortality-increasing behaviors, such as increasing intensive margin labor supply, in exchange for lower expected mortality post-retirement. Although the evidence is somewhat mixed, it appears that the retirement effect works in the opposite direction of the reform, which would be consistent with a dynamic model in which workers respond to an earlier retirement age along several margins. Regardless of the pre-retirement effects, the primary effect of the reform occurs in the post-retirement years.

4.3 Panel Estimation

Broadly speaking, the list of possible concurrent developments that threaten identification in the difference specification above could be too long to address individually. For example, improvements in medical technology that differentially affect the newly retired, and arrive in the reform years, could also explain the result.

To investigate one class of potential confounders, I perform the estimation in a panel context. The panel approach refines the within-country identification strategy by comparing mortality gains in the country experiencing the reform to mortality outcomes in similar countries. I perform both diff-in-diff analysis on the affected cohorts, and triple difference analysis, using the mortality of older cohorts as described above. The panel approach has benefits and drawbacks: a panel should address concerns about common shocks to the countries in the panel, such as developments in medical technology, or regional or international economic conditions; however, other Western European countries are experiencing similar reforms, and may have a weak relationship with mortality in a large country such as West Germany. For these reasons, the diff-in-diff and triple difference estimators are likely biased downwards, and it is possible that the within country analysis contains a better estimation of the counterfactual.

The panel approach rests upon the assumption that no reform-country-specific shock coincides with the year of the reform, i.e. that the treated country would have continued on the common trend estimated among the other countries in the panel, absent the reform. To the extent that other countries in the panel pass similar reforms in these years or if the reform-country showed a pre-reform trend not removed by the fixed effects, the estimated effect will be the sum of this trend and the effect of the reform. The pre-reform trends appear to be a problem in the case of the Swedish and Danish reforms, and has led to the inclusion of a country-specific trend in the analysis. Broadly speaking, Western Europe is experiencing a convergence in mortality rates during this period, and I have struggled to find a simple and clear method of addressing this, preferably one without strong parametric assumptions on the convergence process.

In the West German case, I use 1957-1988 mortality data from 14 other Western European countries (Austria, Belgium, Denmark, Finland, France, Ireland, Italy, The Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom) to estimate country, age and cohort effects.

These are all the large countries in Western (non-Communist) Europe. My estimating equation is

$$\begin{aligned}log(mortality_{n,c,a}) = \beta_0 + \quad & \beta_1 \quad \mathbb{1}\{c \geq \text{Reform cohort}\& \\ & \quad a \geq \text{Retirement Age } \& n = \text{Reform Country }\} \\ + \quad & \beta_n + \beta_c + \beta_a + \epsilon_{n,c,a}, \end{aligned} \tag{4}$$

where n indexes the country. Table 3 reports the coefficients. The panel estimates are consistent across specifications, and demonstrate a decline in mortality beginning in the year of the reform, with no pronounced decline in the retirement years. Inclusion of a post-reform dummy (specific to West Germany) has no effect on the estimates. Estimates in the 3-4% range correspond to a two to three years of cohort effect, or being born half of a year later.

4.4 Wald Estimates, LATE

Interpretation of the coefficient of interest raises several issues. To translate the estimated parameter into a semi-elasticity of mortality with respect to retirement, the coefficient of interest needs to be adjusted for the percentage of the population receiving "treatment," i.e. the percentage of the population eligible for earlier retirement after the reform. This the is the "Wald estimator" of treatment on the treated (ToT). Additionally, although I lack micro-level data on heterogeneity in the response to the reform, some discussion of local average treatment effect (LATE) concerns is deserved.

The Wald estimator of the ToT effect divides the average treatment effect, estimated in the regressions above, by the percentage of the population treated by the reform. In this case, there are several ways to think about the percentage of the population receiving the "treatment." The most obvious group receiving treatment are workers, and using male labor force participation in 1970 in the 55-59 age group, 88%, and a 4 year change in retirement age, I arrive at an estimate of the semi-elasticity of mortality with respect to retirement age between .03% and 1.8%. A smaller percentage of workers were covered by the public pension system- approximately 83%- increasing the estimated elasticity slightly. To be clear, this implies that a 1% counterfactual mortality rate would be between .997 and .982 following a one-year change in retirement age.

Local average treatment effects, or LATE, occur when a subset of the population have their behavior differentially affected by reform. For example, changes in pension wealth may not have a large effect on the net worth of the very wealthy, leading them to have a smaller response to the reform than the average worker. As well, the first cohorts eligible for the reform may not have been prepared to respond to the change in eligibility age. Workers who would respond the quickest are likely to be those with both a high demand for retirement and low income. Workers in poor health, or working in industries with poor health conditions plausibly experience the largest treatment effect.

It is possible that other elements of the West German retirement institutions responded to the reform, especially by lowering the normal retirement ages in firm pension schemes. Changes in social norms regarding age of retirement would be expected, as well. In general, the lowering of the average retirement age should affect the optimization problems of reform-eligible cohorts along several dimensions. Workers in their early 50's at the time of the reform would have had their remaining work-year cut in half, in many cases, leading to large shifts in labor supply and savings, with attendant changes on their employers. Together, these mechanisms may explain the increasing effect on later cohorts.

4.5 Female Treatment and Cross-gender effects

While I have focused on the effects of the reform on men, the 1973 West German reform introduced a female retirement age of 60, in addition to widening the disability and unemployment paths to retirement for both men and women. Previous work on this topic has employed female mortality patters as a control group for male mortality, though marriage and other family arrangements imply spillovers may be large.

The estimated effect for the female treatment is smaller than the male treatment, approximately by a factor of one-half, except when the income effect is included. (Results not reported here.) Coefficient estimates range from -0.02 to -0.03 for the base specification, and -0.06 to -0.07 when the retirement effect is estimated separately. Female labor force participation was approximately 35% in the 55-59 age groups in 1970, suggesting a Wald estimator of -.06 to -0.25 for the effect of the reform. Considering the statutory female retirement age was lowered by 5 years, this yields a semi-elasticity around 1%, in line with the male estimates.

One important avenue through which the social security system interacts with retirement decision includes the effect of retirement incentives on the joint retirement decisions of spouses. Pension reforms, especially those affecting men and women differently, offer a unique angle through which to study inter-household risk-sharing arrangements.

5 Other Reforms

5.1 French Reform of 1972

The second reform analyzed in Gruber and Wise (1998) is the 1972 introduction in France of an early retirement age of 60, with the normal retirement age at 65. A 1983 reform established age 60 as the normal retirement age. These reforms led to a dramatic shift in French retirement behavior, with the modal retirement age shifting from 65 to 60 between 1972 and 1986. I use 1972 as the date of the reform in this analysis; when interpreting the results, we should keep in mind the 1983 reform. The political and economic context of the legislation was similar to that in West Germany.

Figure 4 replicates the analysis explained above for French men. As can be seen, the reform coincided with a decline in French mortality in the retirement years, especially among younger cohorts. The cohorts immediately preceding the reform remain near the model's benchmark mortality, and those in the 57-65 age groups experience only modest gains on the model benchmark. Younger cohorts experience much larger gains, and repeat the pattern found in the West German within-country analysis, wherein mortality rose before the retirement age, but fell by more than the rise in the retirement years. Gains are concentrated in the post-retirement ages.

The regression results broadly support the visual evidence. Estimates are generally larger for all specifications, as compared to the West German estimates. The net effect of the reform differs between the linear and year-cohort models, but the pattern of mortality declines in the retirement years is reproduced in all specifications.

5.2 Swedish Reform of 1976

I located the Swedish reform using the Social Security Administration's publication *Social Security Programs Throughout the World*. The reform lowered the normal retirement age from 67 to 65 during the years from 1976-1978. Early retirement and disability channels were also expanded. I have not located previous research exploiting this reform, though preliminary work is in-progress. There is evidence that retirement behavior changed substantially following the reform.

Figure 5 provides visual evidence of the result of the Swedish reform. Mortality declines substantially from the model's counterfactual, with younger cohorts experiencing the largest gains. Table 7 and Table 8 report the regression estimates of the cumulative effect of the reform. Table 9 contains the panel estimates. Sweden's place as the lowest mortality country in Western Europe during a period of convergence is clearly a problem with the common-trends assumption necessary for a panel. Inclusion of country-specific trends reverses the sign of the coefficient.

5.3 Danish Reform of 1979

The Danish reform of 1979 introduced an early retirement option at the age of 60, 7 years before the normal retirement age of 67. Workers with 20 years of service were eligible for the program. Barrell and Genre (1999) details the recessionary economic conditions that coincided with the reform, which was passed in the wake of the second big oil shock of the 1970's. The program was in place until 1992. My sample is composed of the 12 cohorts turning 67 before and after the 1979 reform (24 cohorts total), a cutoff which excludes cohorts eligible for the post-1992 regime. This choice of sample is conservative, as only 5 cohorts are eligible for the entire 7 years of early retirement.

Although I do not have evidence on the change in retirement behavior that resulted from the reform, Wise (2010) uses Denmark's 1979 reform to analyze the effect of retirement-inducing reforms on youth employment.

Figure 6 plots the mortality residuals from the year-cohort model described above. The youngest cohorts experience the largest departure from the model, with almost all mortality gains occurring in retirement. Table 10 and Table 11 repeat the regression analysis. The regression estimates are mixed, possibly as a result of the conservative sample selection. The same concerns regarding cross-country convergence discussed in relation to Sweden apply to Denmark, also a low-mortality country in Western Europe.

6 Simulation (to be added)

7 Conclusion and Future Directions

This project is an broad attempt to estimate population-level responses to changes in retirement age. This strategy has a number of advantages, most notably, the problem of selection into retirement can be abstracted from, since all members of the population are included in the sample. These estimates are useful to pension planners, who typically cannot target subsets of the population with different retirement ages.

Clearly, this strategy also comes with drawbacks. Large-scale pension reforms of the sort analyzed here have become less common, each reform has distinctive characteristics, and more recent reforms have reduced labor-supply distorting incentives in most national pension programs. The results presented here are not uniform, and likely depend on a heterogenous response across and within countries. Such an aggregate analysis raises questions that cannot be answered without additional data. Of particular concern is the industries and occupations that contribute the most workers to the shift in retirement behavior, and the health consequences of these employments. Changes in savings, intensive margin labor supply, and health investments should play important roles in explaining the age-profiles of mortality that result from the reforms. An ideal study would use a regression-discontinuity design in a setting in which retirement age was raised for those born after a certain date. This sort of natural experiment would be particularly informative regarding the proposed increase in retirement age facing many pension systems.

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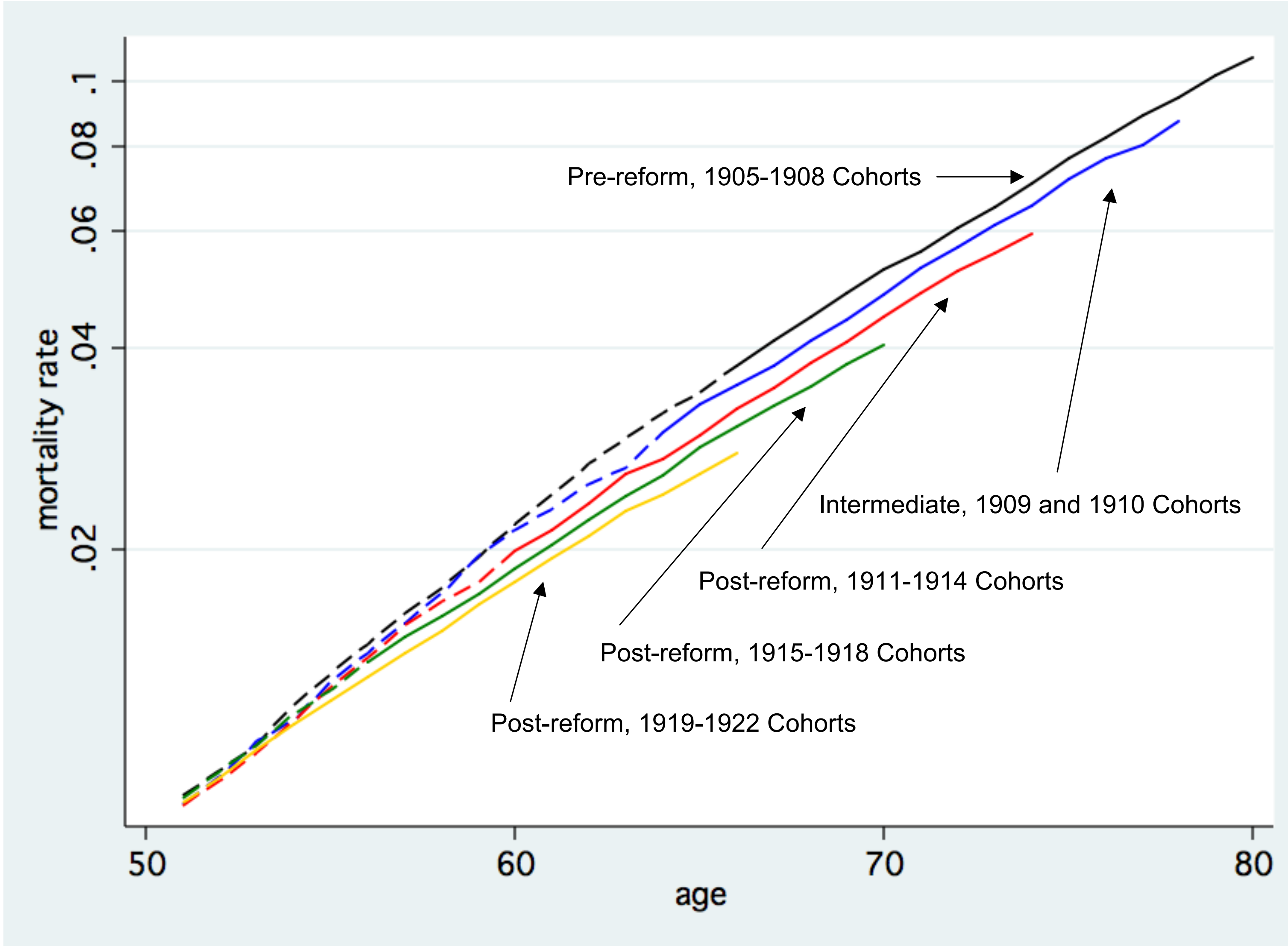

Figure 1: West German Age-Profiles of Mortality, Males

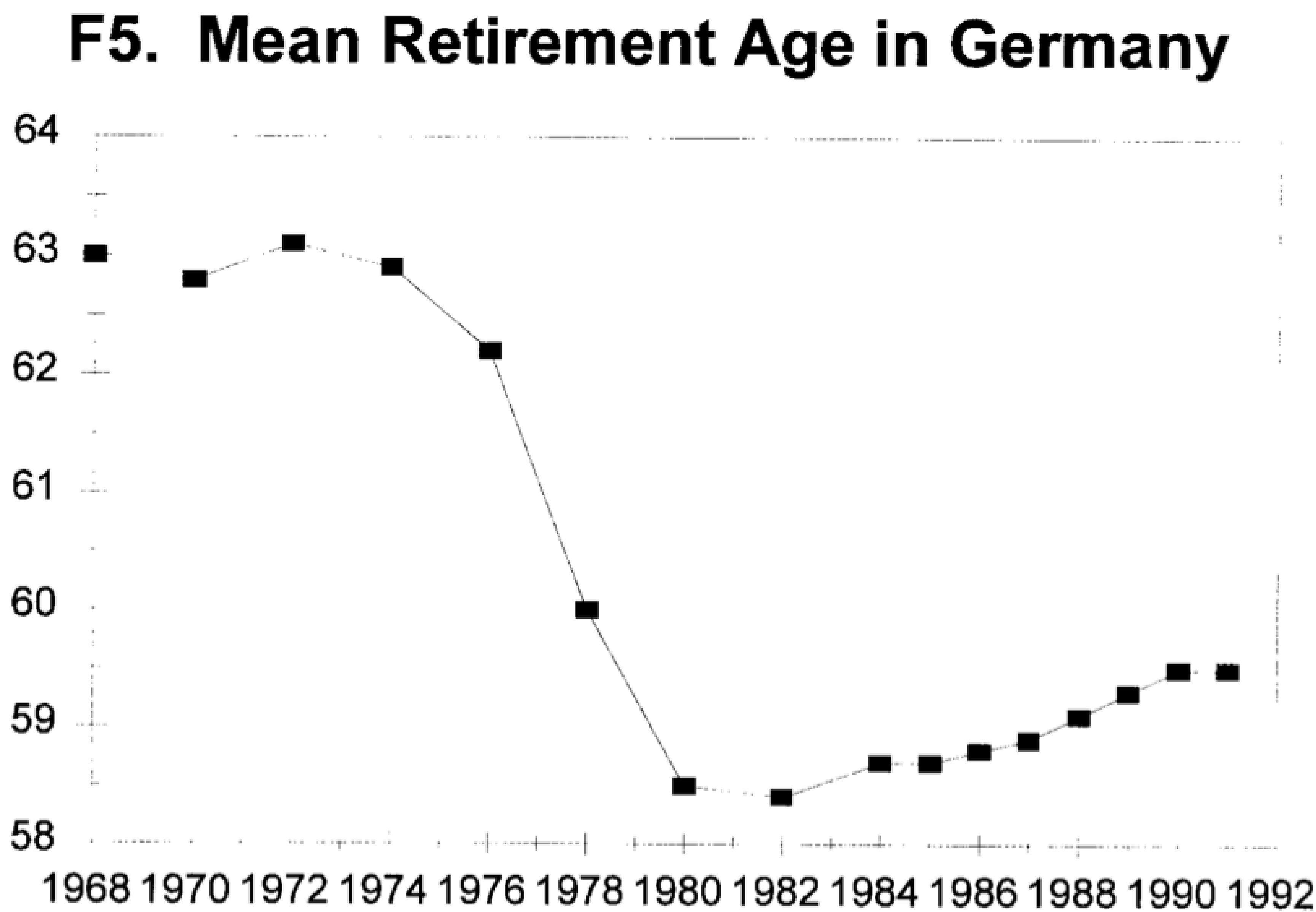

Figure 2: West German Mean Retirement Age, Men, (Gruber and Wise, 1999)

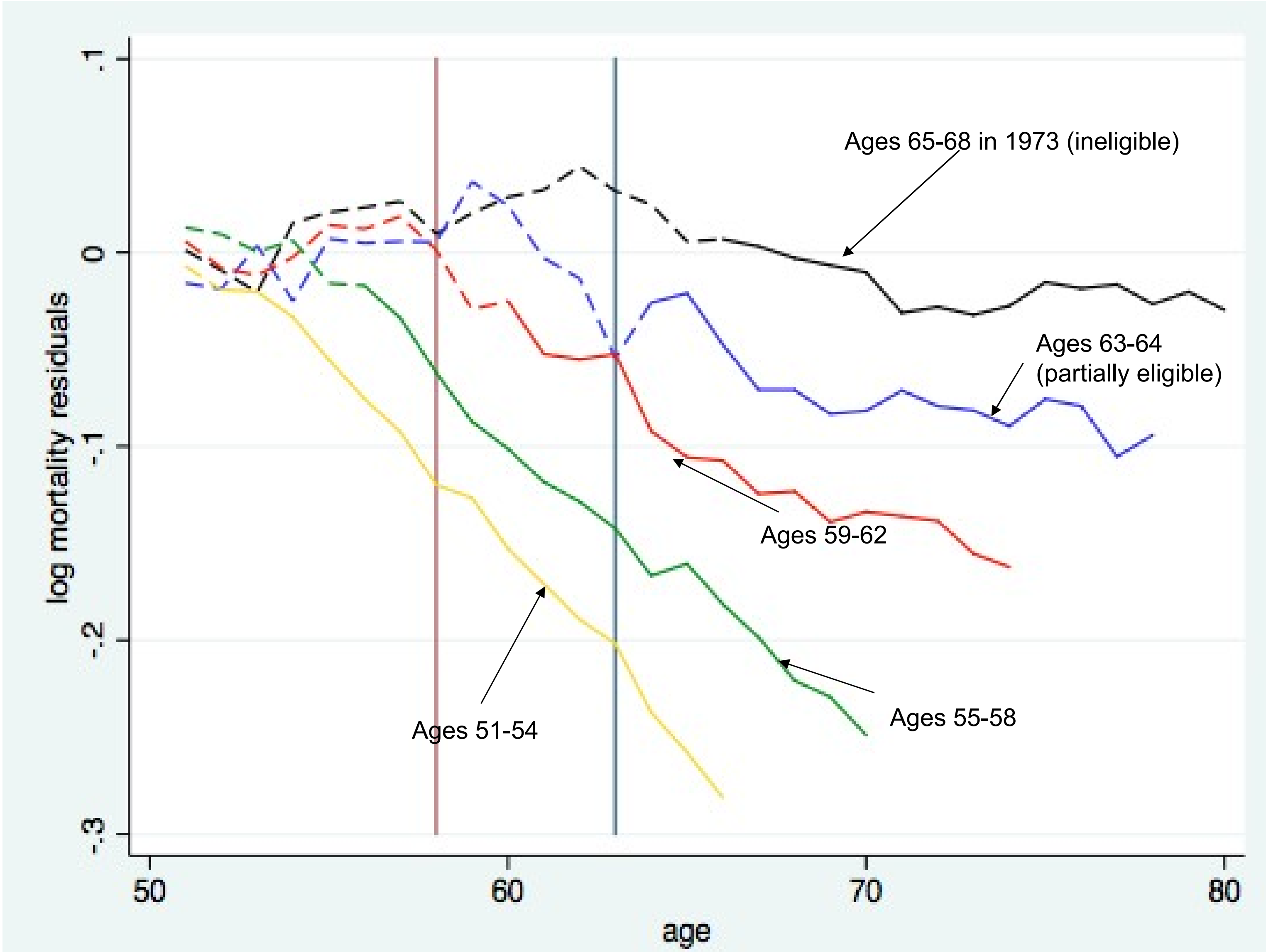

Figure 3: West Germany Residuals: from age cubic, year-cohort fixed effect model estimated on pre-reform period and ineligible cohorts. Dashed lines signify the pre-reform period, and solid lines signify the post-reform period. Sample includes ages 51-80, 1897-1920 cohorts. Younger cohorts depart from the model around the time of the reform, with most mortality gains occurring later in life.

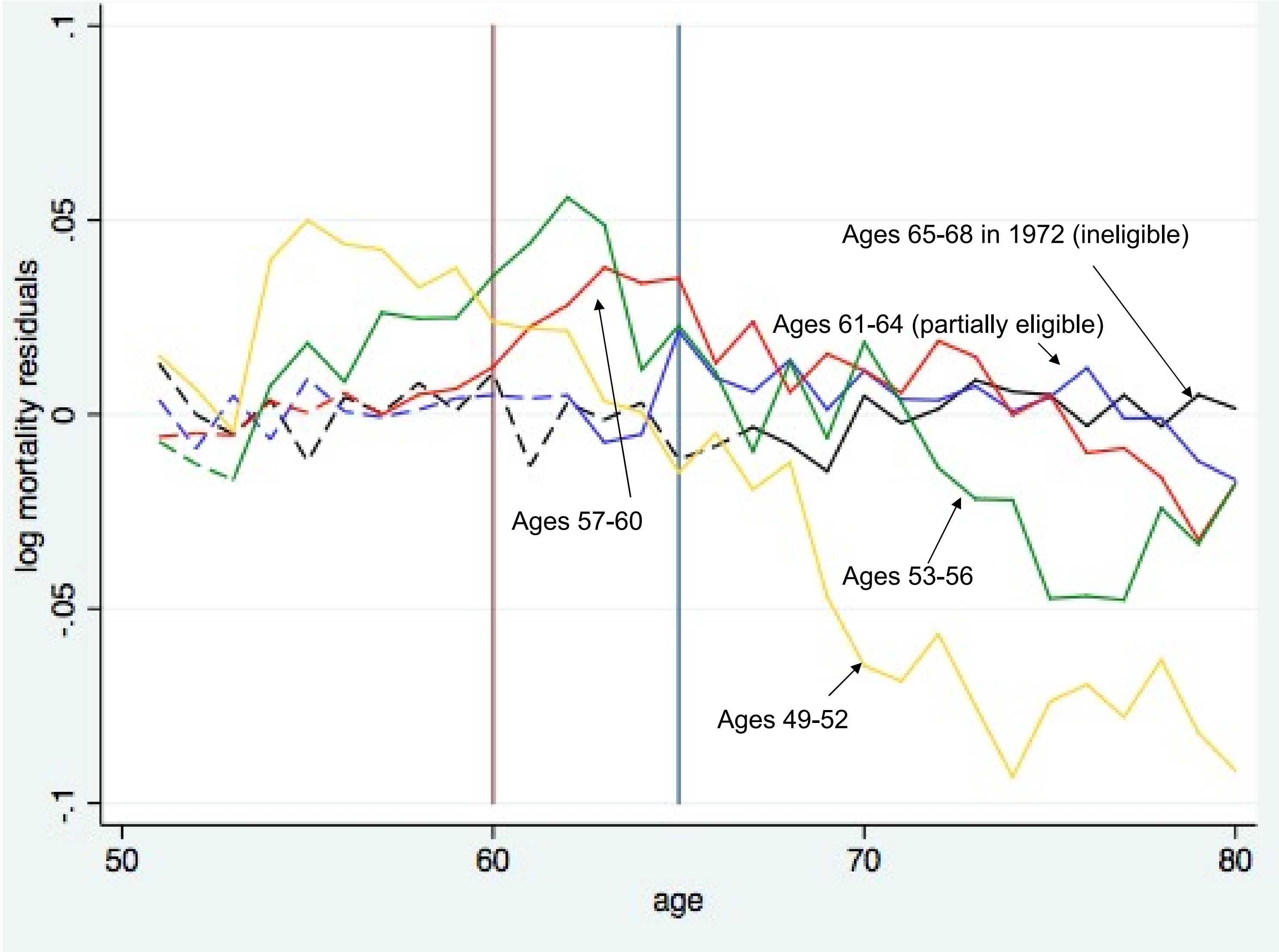

Figure 4: France Mortality Residuals: see Figure 1 for description of model. Figure 2 does not exhibit a pattern of decline on impact, instead rising temporarily, before falling below the model's counterfactual in the post-retirement years. Again, the largest effects are found in younger cohorts, later in life.

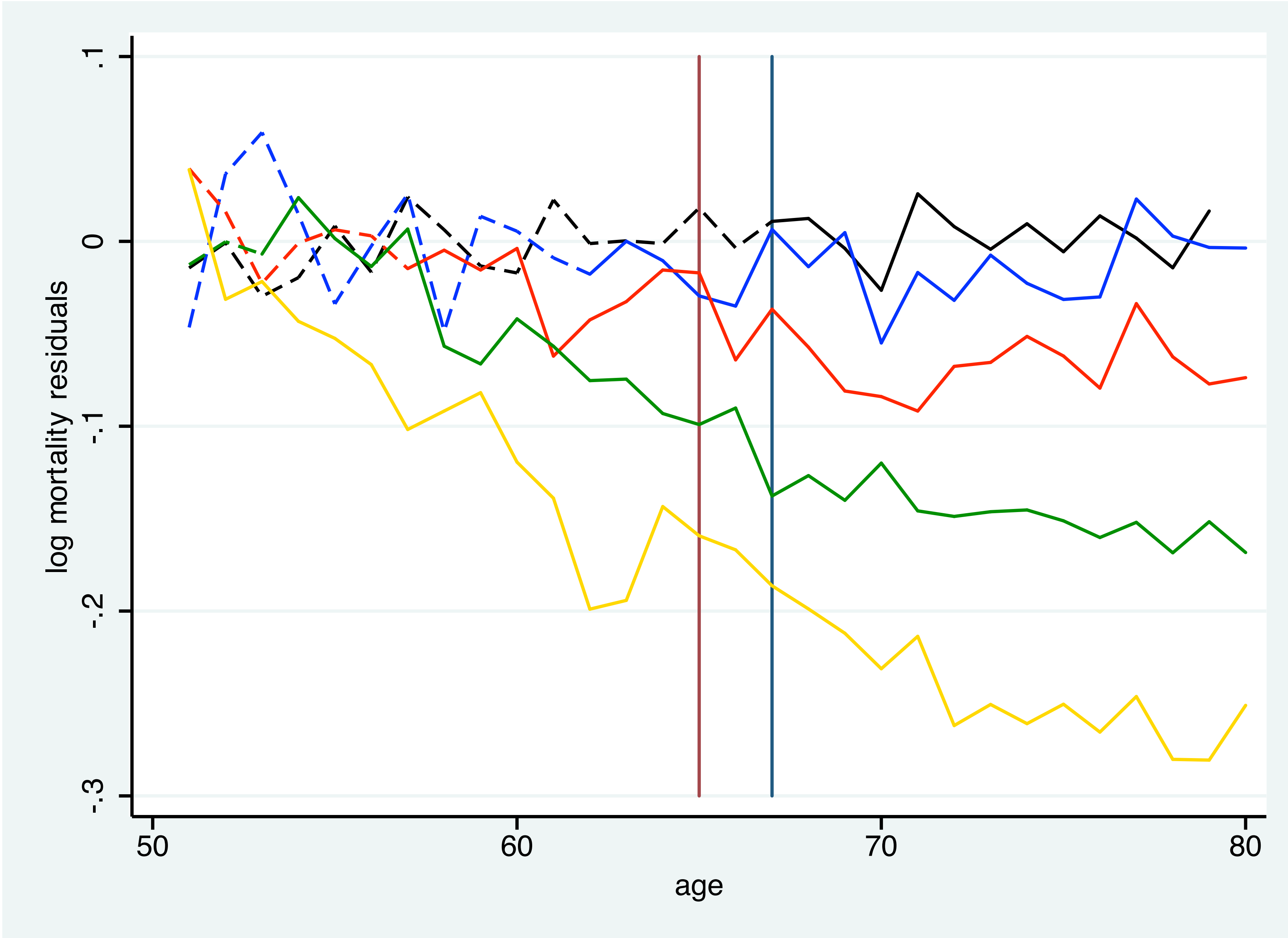

Figure 5: Sweden Mortality Residuals

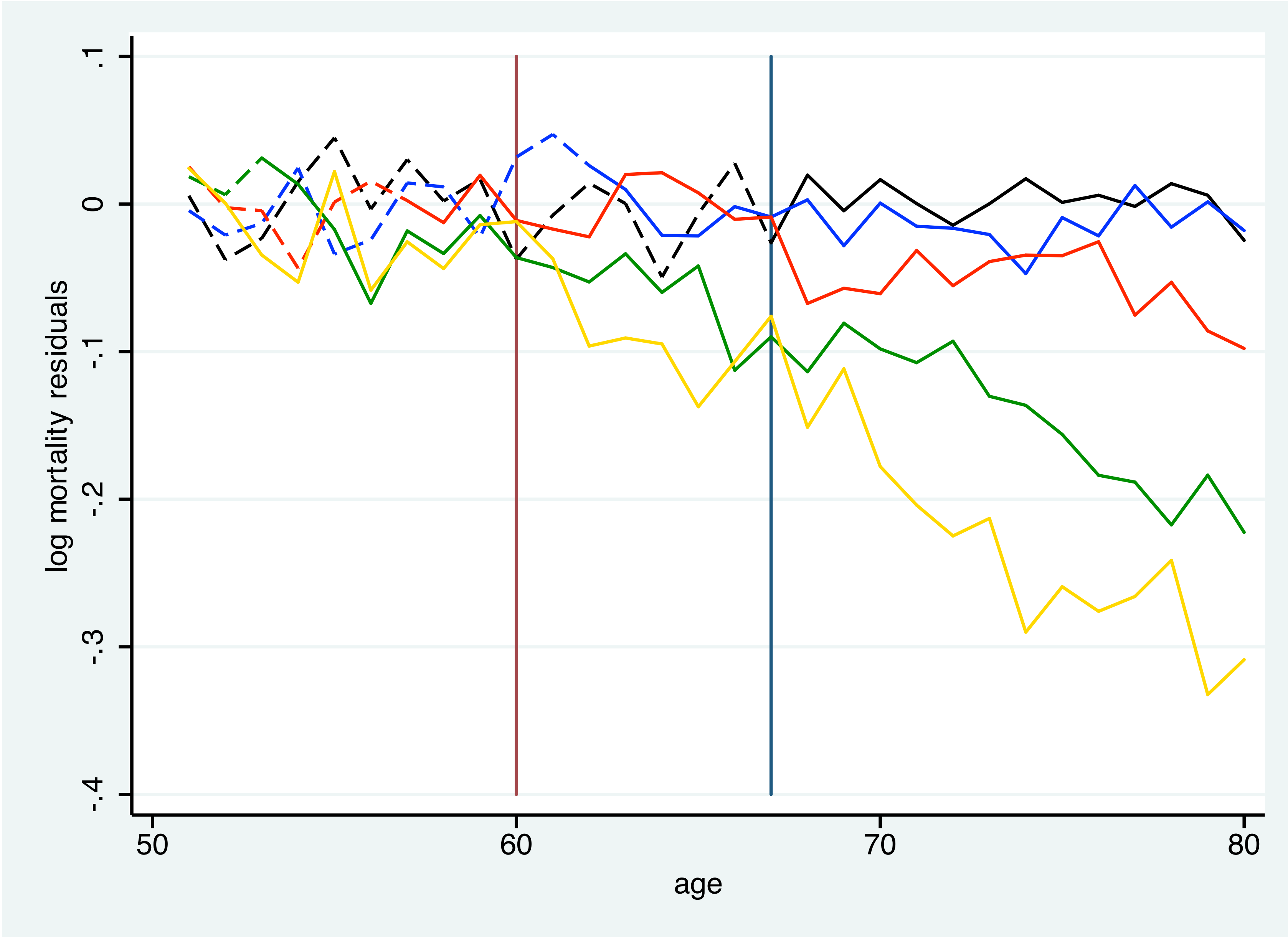

Figure 6: Denmark Mortality Residuals

Table 1: West Germany, 1973 Reform, Within country, Linear Model

VARIABLES	(1) logmale	(2) logmale	(3) logmale
Eligible	-0.00258	0.0281***	0.0359***
	(0.00822)	(0.00791)	(0.00994)
Elig+Retired		-0.0413***	-0.0455***
		(0.00719)	(0.00725)
year>1972			-0.0122
			(0.00856)
age	0.0844***	0.0847***	0.0852***
	(0.000521)	(0.000521)	(0.000527)
cohort	-0.0131***	-0.0132***	-0.0129***
	(0.000767)	(0.000743)	(0.000771)
Constant	16.07***	16.24***	15.65***
	(1.480)	(1.431)	(1.485)
Observations	671	671	671
R-squared	0.995	0.995	0.995

*** p<0.01, ** p<0.05, * p<0.1

Table 2: West Germany, 1973 Reform, Within country, Full Model

VARIABLES	(1) logmale	(2) logmale	(3) logmale	(4) logmale	(5) logmale
Eligible	-0.079***	-0.030**	-0.033**	0.0044	0.010
	(0.013)	(0.011)	(0.012)	(0.0088)	(0.0081)
Elig+Retired		-0.077***	-0.076***		-0.010*
		(0.0095)	(0.012)		(0.0057)
year>1972			0.0028		
			(0.010)		
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	no	no	no	yes	yes
Observations	671	671	671	671	671
R-squared	0.998	0.998	0.998	1.000	1.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: West Germany, 1973 Reform, Panel

VARIABLES	(1) logmale	(2) logmale	(3) logmale	(4) logmale	(5) logmale
Eligible	-0.039***	-0.037***	-0.039***	-0.036***	-0.044***
	(0.0067)	(0.0078)	(0.0073)	(0.0074)	(0.0077)
Elig+Retired		-0.0027	-0.0027	-0.0011	0.0047
		(0.0071)	(0.0071)	(0.0072)	(0.0077)
year>1972, WG			0.0032		0.022**
			(0.0069)		(0.0093)
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	yes	yes	yes	yes	yes
Country Fx	yes	yes	yes	yes	yes
Country Trend	no	no	no	yes	yes
Observations	10,065	10,065	10,065	10,065	10,065
R-squared	0.992	0.992	0.992	0.996	0.996

*** p<0.01, ** p<0.05, * p<0.1

Table 4: France, 1972 Reform, Within-country, Linear Model

	(1)	(2)	(3)
VARIABLES	logmale	logmale	logmale
Eligible	-0.050***	0.039***	-0.00090
	(0.014)	(0.0099)	(0.011)
Elig+Retired		-0.11***	-0.089***
		(0.010)	(0.010)
year>1971			0.061***
			(0.012)
age	0.073***	0.073***	0.072***
	(0.00080)	(0.00057)	(0.00071)
cohort	-0.013***	-0.013***	-0.014***
	(0.00068)	(0.00065)	(0.00061)
Constant	16.5***	16.6***	18.6***
	(1.32)	(1.26)	(1.19)
Observations	1,024	1,024	1,024
R-squared	0.991	0.992	0.992

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: France, 1972 Reform, Within-country, Full Model

VARIABLES	(1) logmale	(2) logmale	(3) logmale	(4) logmale	(5) logmale
Eligible	-0.026*	0.067***	0.024	0.037***	0.044***
	(0.014)	(0.014)	(0.015)	(0.0058)	(0.0049)
Elig+Retired		-0.12***	-0.10***		-0.011**
		(0.013)	(0.014)		(0.0050)
year>1971			0.054***		
			(0.010)		
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	no	no	no	yes	yes
Observations	1,024	1,024	1,024	1,024	1,024
R-squared	0.992	0.993	0.993	0.999	0.999

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: France, 1972 Reform, Panel

VARIABLES	(1) logmale	(2) logmale	(3) logmale	(4) logmale	(5) logmale
Eligbile	-0.16***	0.017*	0.16***	0.13***	0.15***
	(0.0082)	(0.0086)	(0.0081)	(0.013)	(0.0092)
Elig+Retired		-0.21***	-0.21***	-0.11***	-0.12***
		(0.0070)	(0.0071)	(0.0092)	(0.0083)
year>1972, FR			-0.19***		-0.047***
			(0.0085)		(0.0095)
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	yes	yes	yes	yes	yes
Country Fx	yes	yes	yes	yes	yes
Country Trend	no	no	no	yes	yes
Observations	15,174	15,174	15,174	15,174	15,174
R-squared	0.986	0.986	0.986	0.994	0.994

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Sweden, 1976 reform, Within-country, Linear Model

	(1)	(2)	(3)
VARIABLES	logmale	logmale	logmale
Eligible	-0.0471***	0.0320***	0.0241
	(0.0140)	(0.0105)	(0.0156)
Elig+Retired		-0.120***	-0.116***
		(0.0146)	(0.0175)
year>1976			0.0113
			(0.0165)
age	0.0933***	0.0947***	0.0943***
	(0.00119)	(0.000798)	(0.00133)
cohort	-0.00623***	-0.00607***	-0.00622***
	(0.000793)	(0.000855)	(0.000958)
Constant	2.061	1.677	1.987
	(1.563)	(1.657)	(1.881)
Observations	750	750	750
R-squared	0.993	0.994	0.994

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Sweden, 1976 Reform, Within-country, Fixed Effect Model

VARIABLES	(1) logmale	(2) logmale	(3) logmale	(4) logmale	(5) logmale
Eligible	-0.0917***	-0.00133	-0.0392*	0.00316	0.00861
	(0.0199)	(0.0184)	(0.0216)	(0.0153)	(0.0139)
Elig+Retired		-0.143***	-0.124***		-0.0119
		(0.0178)	(0.0184)		(0.0110)
year>1976			0.0404***		
			(0.0127)		
Constant	-12.45***	-6.530***	-7.655***	-11.88***	-11.43***
	(1.662)	(1.012)	(1.203)	(0.755)	(0.904)
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	no	no	no	yes	yes
Observations	750	750	750	750	750
R-squared	0.994	0.996	0.996	0.998	0.998

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Sweden, 1976 Reform, Panel

	(1)	(2)	(3)	(4)	(5)
VARIABLES	logmale	logmale	logmale	logmale	logmale
Eligible	0.091***	0.043***	-0.083***	-0.037***	-0.076***
	(0.0094)	(0.0070)	(0.0057)	(0.0100)	(0.0083)
Elig+Retired		0.065***	0.064***	-0.014	0.010
		(0.0046)	(0.0046)	(0.011)	(0.010)
year>1977, SW			0.17***		0.085***
			(0.0053)		(0.0085)
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	yes	yes	yes	yes	yes
Country Fx	yes	yes	yes	yes	yes
Country Trend	no	no	no	yes	yes
Observations	11,229	11,229	11,229	11,229	11,229
R-squared	0.987	0.987	0.988	0.995	0.995

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: Denmark, 1979 Reform, Within-country, Linear Model

	(1)	(2)	(3)
VARIABLES	logmale	logmale	logmale
Eligible	-0.0182	0.0654***	0.0572**
	(0.0125)	(0.0169)	(0.0255)
Elig+Retired		-0.0887***	-0.0846***
		(0.0202)	(0.0256)
year>1979			0.0116
			(0.0228)
age	0.0912***	0.0915***	0.0911***
	(0.000953)	(0.000887)	(0.00158)
cohort	-0.00172**	-0.00176**	-0.00195**
	(0.000713)	(0.000716)	(0.000939)
Constant	-6.264***	-6.198***	-5.813***
	(1.397)	(1.399)	(1.869)
Observations	720	720	720
R-squared	0.993	0.993	0.993

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Denmark, 1979 Reform, Within-country, Fixed Effect Model

	(1)	(2)	(3)	(4)	(5)
VARIABLES	logmale	logmale	logmale	logmale	logmale
Eligible	-0.0536***	0.0388**	0.00890	0.00875	0.0128
	(0.0147)	(0.0140)	(0.0198)	(0.0131)	(0.0153)
Elig+Retired		-0.100***	-0.0869***		-0.00481
		(0.0132)	(0.0152)		(0.0183)
year>1978			0.0366***		
			(0.0106)		
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	no	no	no	yes	yes
Observations	720	720	720	720	720
R-squared	0.995	0.996	0.996	0.997	0.997

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Denmark, 1979 Reform, Panel

	(1)	(2)	(3)	(4)	(5)
VARIABLES	logmale	logmale	logmale	logmale	logmale
Eligible	0.20***	0.14***	0.038***	0.056***	0.040***
	(0.011)	(0.013)	(0.0097)	(0.0073)	(0.0081)
Elig+Retired		0.061***	0.060***	-0.0088	-0.0026
		(0.013)	(0.013)	(0.011)	(0.011)
year>1979, DM			0.13***		0.031***
			(0.0096)		(0.0067)
Age Cubic	yes	yes	yes	yes	yes
Cohort Fx	yes	yes	yes	yes	yes
Year Fx	yes	yes	yes	yes	yes
Country Fx	yes	yes	yes	yes	yes
Country Trend	no	no	no	yes	yes
Observations	14,314	14,314	14,314	14,314	14,314
R-squared	0.987	0.987	0.987	0.994	0.994

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1