Can Income Buy Health? Evidence from Social Security Benefit Discontinuities and Medicare Claims*

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Abstract

Income is a powerful predictor of health among the elderly, but existing research has struggled to identify a causal link. In this paper, I estimate the causal effect of Social Security income on health care utilization and health outcomes among elderly men. Using Medicare administrative records and a regression discontinuity design, I exploit several changes in the Social Security benefit formula that vary abruptly by date of birth – variation which has been overlooked by previous research. I find that increases in Social Security income reduces Medicare utilization across a variety of health care settings. I estimate a 1% increase in Social Security income causes a 0.9% decline in payments for Medicare covered services. I also find declines in diagnoses for chronic conditions and mortality. These results provide evidence that increasing Social Security benefits has positive social and fiscal externalities. More generous benefits can improve population-level health and reduce Medicare expenditures.

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1 Introduction

Although income is widely held to be a key determinant of health among the elderly, income from Social Security and health benefits from Medicare are often studied separately. A causal link from income to health would imply that changes in Social Security benefits could affect Medicare spending, but whether such a relationship exists is unknown. Federal spending for these two programs will soon exceed 10% of GDP, so even modest effects could have sizable budget implications.

The effects of income on health and health care use may seem intuitive, but existing research has struggled to identify a causal link. Factors that influence income – like education or family background – also influence health, and disentangling the direct causal effect of income is challenging. Quasi-experimental research has attempted to solve these endogeneity problems, but results have been inconsistent. The challenge is finding a research design which simultaneously has policy-relevant variation, a representative population, and causal identification. A feature of the Social Security benefit formula provides just such an opportunity.

In this paper, I estimate the effect of income on health outcomes by exploiting an overlooked technical quirk: the generosity of Social Security income varies abruptly by exact date of birth, but Medicare benefits do not. As a consequence of this policy, workers born one day apart receive different income. Assuming workers born near in time are similar along other dimensions, differences in Social Security income near the date of birth cutoff are as good as random. Because nearly all elderly Americans receive Social Security and Medicare, this provides a setting to examine the causal effect of income on Medicare-related health outcomes.

I start by showing that the generosity of monthly Social Security income changes abruptly for workers born after January 2 among all recent birth cohorts. In my sample, the amount ranges from -2% to 4.5% depending on the particular cohort. Unlike Social Security changes studied in prior research (e.g. the 1917 Notch), workers are unlikely to know these changes exist. The calculations to identify the changes are complex, and in fact, this paper is the first to describe them explicitly. In my context, the lack of salience is useful because it minimizes the extent to which labor income offsets the change in Social Security income. Additionally, Social Security reforms have proposed benefit changes of similar magnitude, so this setting has direct policy relevance.[1]

The primary outcomes are spending, diagnoses for chronic conditions, and mortality. I measure these using a 100% extract of restricted access administrative records from the full Medicare pop-

[1] For example, see Fiscal Year 2014 Budget Request (Office of Management and Budget) or Options for Reducing the Deficit: 2019 to 2028 (Congressional Budget Office)

ulation. Because the files cover essential health services across inpatient and outpatient settings (Medicare Part A and B), I can measure substitution effects between different types of care (Chandra, Gruber and McKnight, 2010). The files also provide detailed procedure and diagnosis codes which are useful for constructing measures of physician and hospital quality. To measure mortality, I link individuals across time to calculate what percent of my sample from 2011 is still alive by the end of 2017. The Medicare files do not record benefit amounts, so I use Social Security public use benefit data to validate the income differences across cohorts.

My estimation sample consists of men with an average age of 75. Thus, I observe them 13 years after the income shock becomes binding at age 62. While previous work has focused on the short-term effects of liquidity shocks (Evans and Moore, 2012) or labor force exit (Fitzpatrick and Moore, 2018), the panel structure of my data enable me to study outcomes long after the shock occurs. This helps detect effects that a short-term analysis would otherwise miss. I focus on men because the date-of-birth cutoff is linked to wage earners and men are the primary wage earners for these cohorts.

My empirical strategy relies on a regression discontinuity design with date-of-birth as the assignment variable. There are ten discontinuities corresponding to the ten birth cohorts in my sample. Compared to a single discontinuity, working with multiple discontinuities provides a valuable opportunity for specification and falsification tests. I test if effects are symmetric between positive and negative shocks, and that effects are null for placebo dates. Because some of the discontinuities are modest, I consider several techniques for stacking the discontinuities to maximize statistical efficiency.

I find that increases in Social Security income reduce health care spending and mortality. Specifically, I estimate a 1% increase in Social Security income causes a 0.93% decline in payments for Medicare covered services, and a 0.98% decline in mortality over a 6 year period. I also find some evidence of declines in diagnoses for chronic conditions. My findings suggest that 37% of the per-capita cost of increased Social Security benefits would be offset by lower Medicare spending. Because there is a positive externality, retirement insurance models that ignore spillover effects onto Medicare will underestimate the optimal Social Security benefit level. Overall, the results highlight the importance of examining health outcomes when evaluating the costs and benefits of social insurance programs.

Although there is a literature studying how Social Security impacts health, this paper provides the first evidence on Medicare outcomes. The most closely related research studies "the

Notch" – a 7% decline in Social Security income for retirees with dates of birth after January 2, 1917.[2] Researchers have focused on outcomes such as household structure (Engelhardt, Gruber and Perry, 2005), prescription drug utilization (Moran and Simon, 2006), mortality (Snyder and Evans, 2006), weight (Cawley, Moran and Simon, 2010), long-term care (Goda, Golberstein and Grabowski, 2011), home health (Tsai, 2015), mental health (Golberstein, 2015), cognitive function (Ayyagari and Frisvold, 2016), and earnings (Gelber, Isen and Song, 2016). Although these papers generally find a positive relationship between income and health care utilization, they disagree on the health impacts. The papers find a negative effect (Snyder and Evans, 2006), no effect (Goda, Golberstein and Grabowski, 2011; Cawley, Moran and Simon, 2010), or a positive effect (Moran and Simon, 2006; Golberstein, 2015; Ayyagari and Frisvold, 2016) depending on the outcome and estimation sample. The disagreement may also be explained by empirical specifications that violate the necessary exclusion restrictions. Because these papers rely on survey data that report birth dates (at best) at the quarterly level, Handwerker (2011) argues the variation between cohorts overwhelms the variation in benefit amounts. I avoid these challenges by using administrative data with exact date of birth.[3]

While my analysis focuses on the long-run effect of retirement income, a handful of papers examine the health impact of Social Security programs in other contexts. In the Social Security disability program, Gelber, Moore and Strand (2018) argue higher income reduces mortality. Using a regression kink design, they find an elasticity about -0.6, a magnitude similar to my estimate. Conversely, Fitzpatrick and Moore (2018) show that eligibility for retirement benefits at age 62 leads to a large spike in male mortality. They argue lifestyle changes associated with leaving the labor force explain the negative effect. Their result matches other work finding liquidity shocks have negative short run-effects (Dobkin and Puller, 2007; Evans and Moore, 2012; Gross and Tobacman, 2014).

Outside of the Social Security context, these results build on a large literature on the impacts of income on health and health care utilization (Philipson and Becker, 1998; Grossman, 2000; Cutler, Deaton and Lleras-Muney, 2006; Hall and Jones, 2007; Chetty et al., 2016). Applied research on older adults has focused on wealth shocks due to changes in equity markets (Schwandt, 2018; McInerney, Mellor and Nicholas, 2013), inheritance shocks (Meer, Miller and Rosen, 2003; Kim and Ruhm, 2012; Van Kippersluis and Galama, 2014) or lottery winnings (Lindahl, 2005; Gardner

[2] I cannot examine the effect of the Notch on Medicare spending because the relevant cohort would be 83 years old in 1999, the first year Medicare claims are available.

[3] An exception is Gelber, Isen and Song (2016) which studies labor force outcomes using exact date of birth.

and Oswald, 2007; Apouey and Clark, 2015; Cesarini et al., 2016). Summarizing this research is difficult. The effect can vary from positive to zero to negative depending on the empirical strategy and institutional details. Most effects are modest, but results from the United States do find declines in income or wealth have clear negative effects on mental health. Within developed countries, the literature suggests any positive income effects are likely to arise from behavioral or environmental factors rather than greater consumption of medical care. My result that both utilization and mortality decline supports this view.

This paper also contributes to literature studying spillovers between social insurance programs. Previous work on Social Security spillovers has studied the effect of changing the retirement age on disability applications (Duggan, Singleton and Song, 2007; Li and Maestas, 2008), substitution between disability insurance and unemployment insurance (Lindner, 2016; Mueller, Rothstein and von Wachter, 2016), disability and welfare assistance (Borghans, Gielen and Luttmer, 2014), or disability and Medicaid (Burns and Dague, 2017). The effect of Medicare on other programs has mostly been studied in the context of Medicaid, such as conflicting incentives for long-term care (Grabowski, 2007), or on physician supply constraints (Carey, Miller and Wherry, 2018). Compared to other program combinations, Social Security and Medicare are unique in terms of the size of their expenditures and the scope of their coverage.[4]

The remainder of the paper is structured as follows. Section 2 describes my source of income variation in the context of Social Security and Medicare program rules. Section 3 describes the Medicare administrative data, and section 4 discusses my identification strategy. Section 5 provides results for spending, chronic conditions, and mortality. Section 6 concludes.

2 Institutional Setting

Nearly all elderly Americans receive retirement income from Social Security and health insurance from Medicare.[5] The generosity of Social Security income varies abruptly by exact date of birth, but Medicare benefits do not. This provides a setting to examine how quasi-random income variation affects health care utilization and mortality outcomes for a large population.

[4]See also Zhao (2014) for a discussion of Social Security and Medicare in the context of an overlapping generation general equilibrium model.

[5]Workers and their spouse are eligible for both programs if the worker has at least 10 years of creditable labor market earnings. 97% of adults over age 65 meet this threshold. Infrequent workers with disabilities, late-arriving immigrants, and certain government employees account for the remaining 3% (SSA, 2015).

2.1 Social Security Income

Social Security provides monthly retirement benefits to qualified retirees and their spouses. As a social insurance program, it insures the labor market income of workers against old-age risks. Workers pay into the program through mandatory payroll deductions and receive benefits as a function of their contributions. The benefit formula is progressive. Higher income workers receive higher benefits, but the marginal replacement rate declines with income. Workers can start claiming at age 62, or they can receive bonus benefits by delaying up to age 70.

Because workers make contributions over several decades, implementing the formula requires adjusting wages and benefits for inflation. The modern benefit formula indexes wages and prices separately. Nominal wage histories before entitlement are adjusted using the Average Wage Index (AWI), a time series the Internal Revenue Service computes using administrative records. Nominal benefit amounts after entitlement are adjusted using the Consumer Price Index (CPI), a time series the Bureau of Labor Statistics computes using survey data. The CPI and AWI base years vary based on a worker's date of birth. For wage indexation, the base year is the calendar year a worker turns 60. For benefit indexation, the base year is the calendar year a worker turns 62. These features interact in a way that means two workers with identical nominal earnings histories will receive different benefits depending on their date of birth.

Consider a worker born in January of year b compared to an identical worker born a month before in December of $b-1$. The worker with a January birthday has a base year for wage indexation of $b+60$, and does not receive a CPI adjustment during his first year. The worker with a December birthday has a base year for wage indexation of $b+59$, but does receive a CPI adjustment during his first year. The percent change in benefits for January birthdays is the difference between the percentage growth in AWI at age 60 minus the growth in the CPI at age 61

$$\%\Delta Benefits \approx \%\Delta AWI_{b+60} - \%\Delta CPI_{b+61} \tag{1}$$

Appendix C derives this equation from the benefit formula. There are four features of the discontinuities to highlight. First, they affect every cohort born after 1917. Appendix Figure 1 shows this includes future retirees who are not yet entitled. The relevant discontinuity occurs at January 2 instead of January 1 because under Social Security regulations an individual attains a particular age on the day preceding the anniversary of their birth.[6] Second, they are the same

[6] POMS Regulation GN 00302.400

in percentage terms regardless of income level or claiming age. Consider a pair of workers with identical nominal earnings histories born on either side of the cutoff for a given cohort. The percentage difference comparing two low-income earners claiming at age 62, and the percentage difference comparing two high-income earners claiming at age 65 are the same.[7] This is because the wage index changes average indexed monthly earnings and the thresholds in marginal replacement rates by the same amount. Finally, recipients are unlikely to be aware of these discontinuities. The calculations involved are opaque. They are not described on the Social Security website or in previous academic research. While other benefit changes like the Notch or changes in the Full Retirement Age are highly salient, these changes are effectively invisible. A beneficiary would only be aware of the shock if he calculated how his benefits would change under different hypothetical dates of birth.[8]

Table 1 summarizes the size of the income shock for the relevant cohort discontinuities. The benefit changes range from 4.5% to -1.9%. These are comparable in magnitude to changes studied in prior research. On the low end, Deshpande, Fadlon and Gray (2020) studies 2 month increases in the Full Retirement Age which cut benefits by 1.1%. On the high end, Gelber, Isen and Song (2016) studies the 1917 Notch which cut benefits by 7%. In my setting, the differences in the monthly dollar amount ranges from $59 to -$24. For context, the average premium for Part D prescription drug insurance during this period was $30 per month.

Most changes are positive. Equation (1) predicts this will occur if nominal wages rise faster than prices – a pattern we expect when there is long-run productivity growth. In some years (1993, for example) negative changes will occur when macroeconomic shocks cause prices to grow more quickly than wages. By chance, the wage and price parameters for the 1927/1928 and 1933/1934 cohorts nearly exactly offset each other, so I consider these two cohorts to be placebos.

2.2 Health Coverage from Medicare

Social Security beneficiaries receive health insurance from Medicare. Non-disabled beneficiaries become eligible for Medicare at age 65 and those already claiming retirement benefits are automatically enrolled.[9] Most individuals enroll in traditional Fee-For-Service (FFS) Medicare. FFS

[7]Because half of the cohorts in the sample are affected by a 0.5% change in the delayed retirement credit, there are slight differences that arise when claiming after 65. Appendix C shows these changes are too small to threaten identification.

[8]If beneficiaries were aware of the shock, it would be "realized" when the Social Security Administration publishes the AWI and CPI computations in the Federal Register. This occurs in late October of the year a cohort turns 61.

[9]Receiving Social Security is a sufficient but not necessary condition to receive Medicare. Those who are entitled to Medicare but not Social Security OASI benefits include some Supplemental Security Income (SSI) recipients, Railroad

Medicare consists of Part A which covers inpatient hospital services, skilled nursing, hospice, and home health; Part B which covers physician services, outpatient services, and preventive services; and Part D which covers outpatient prescription drug benefits. Nearly all providers accept FFS Medicare patients and referrals are not required. Alternatively, roughly 30% of beneficiaries enroll in a privately run Medicare Advantage plan. Under this option, Medicare pays private insurers to provide benefits through a managed care regime. These plans provide lower out-of-pocket costs by restricting access to a narrower network of providers. In addition, they sometimes provide vision and dental benefits which traditional Medicare does not.

FFS Medicare has substantial cost-sharing. Patients are responsible for a $1,364 Part A deductible, 20% co-insurance for nearly all Part B services. About 80% of FFS beneficiaries have these expenses paid by secondary insurance plans which cover some or all of the beneficiary's cost-sharing liability. The majority of secondary plans are provided by firms for their retired employees, but consumers can also purchase them directly (MedPAC, 2018). Low-income beneficiaries covered by Medicaid generally are exempt from cost sharing. Most out-of-pocket health expenditures are for services not covered by Part A and B such as prescription drug cost-sharing, nursing, long-term care, and non-medical services (Fahle, McGarry and Skinner, 2016).

3 Data

To examine the first-stage of the income discontinuity, I use Social Security public use files. I measure health care utilization, chronic conditions, and mortality using Medicare administrative files.

3.1 Public Use Social Security Files

Benefit amounts are unobserved in the Medicare data, so I rely on the Public Use Benefits File to estimate a first stage. This 1% anonymized extract from Social Security administrative records provides year of birth, sex, benefit amount for the year 2004, and annual wages from 1951 to 2003. I compute nominal benefit amounts over the 2006 to 2011 period by adjusting the 2004 amount using the SSA Cost-Of-Living Adjustment time series.

To examine income heterogeneity I also use SSA data on mean benefit amounts by zipcode.[10]

Retirement Board beneficiaries, certain government workers, and individuals over 65 with less than 40 quarters of covered earnings.

[10] See OASDI Beneficiaries by State and ZIP Code, 2011.

Unlike the American Community Survey which also aggregates incomes at the zipcode level, the SSA data is derived from administrative records covering the universe of elderly beneficiaries.

3.2 Restricted Access Medicare Administrative Files

My primary dataset is derived from Medicare administrative files. I use the 100% full population panel from 2006 to 2011 as well as 2017.[11] Every individual enrolled in Medicare during these years is included. The dataset has four parts. First, the Master Base Summary File provides demographic data such as exact date of birth, sex, race, and most recent zipcode. When beneficiaries die, their exact date of death is recorded in that year's file and they are deleted from the next year's file. I assume that beneficiaries who appear in the 2011 file but not the 2017 file died during the interim.[12] The cause of death is unobserved. There is also an entitlement code reported directly from the Social Security Administration. The code measures if someone claims benefits based on their own wage history, a spouse's wage history, as a survivor, or through another type of entitlement. On the enrollment side, I observe months of coverage in Part A, Part B, Part D, Medicare Advantage, and Medicaid.

Second, the Cost and Utilization segment provides health expenditure and utilization data. This file measures 11 categories of service (e.g. inpatient hospitalization, evaluation and management, imaging) and includes Medicare payments, cost-sharing liability, and visit counts. Each observation summarizes a beneficiary's utilization over the calendar year. Third, the Chronic Condition segment measures if a beneficiary has received treatment for any of 22 chronic conditions (e.g. hypertension, heart failure, or depression). Medicare constructs this file using a special algorithm developed by professional medical coders. The algorithm searches all recent Part A and B claim records to see if providers billed Medicare under a diagnosis code associated with a given chronic condition. They are imperfect measures of underlying health because those who fail to seek treatment are excluded.

Fourth, the MEDPAR segment provides procedure and diagnosis billing codes for inpatient claims. I use the file to measure avoidable hospitalizations and hospital quality. Following previous literature, I define an avoidable hospitalization using the Prevention Quality Indicators. These identify patterns of billing codes for admissions which might have been avoided through access to high-quality outpatient care.[13]

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[11] Although this period includes the implementation of the Affordable Care Act, the law had only minor effects on Medicare. See CRS Report R41196.

[12] Technically, a beneficiary could leave the panel by voluntarily terminating both their Medicare and Social Security benefits, but this is extremely rare.

[13] The MedPAR files are only available for 2009 to 2011. I use the composite PQI 90 which includes admission

The data has two key limitations. First, the utilization data exclude Medicare Advantage beneficiaries. Medicare has demographic and enrollment data for these individuals, but the underlying claims are unavailable because they are processed by private insurers. Second, payments made by supplemental insurers are unobserved. Although Medicare records the cost-sharing due, it does not record whether that payment was made directly by the consumer, or by an insurer on behalf of the consumer. Thus, I cannot observe the true out-of-pocket cost.

3.3 Survey Data

To provide descriptive evidence on the correlation between income and health expenditures, I rely on the Medical Expenditure Panel Survey. Appendix B describes these results in detail. Although other datasets could provide useful evidence on mechanisms, it is challenging to estimate a reliable first-stage on benefits in survey data. This is due in part to sample sizes (which are two orders of magnitude smaller) and in part to large, non-classical measurement error in reporting of retirement income (Bee and Mitchell, 2017).

3.4 Sample Construction and Summary Statistics

The 100 percent Master Beneficiary Summary File from 2006 to 2011 consists of about 60 million unique beneficiaries. I make several restrictions to create the estimation sample. Most importantly, I focus only on men. I do so for three reasons. First, for women at the margin of claiming on their record or as a spouse, the income shock near the January 2 cutoff may induce them to switch. Allowing women to pick the date of birth at which earnings are computed violates a key identification assumption. Since almost no men from these cohorts would have higher benefits by claiming on their spouse's wage history, focusing only on men ensures that wage-earner birth date remains fixed.[14] Second, combining wage-earning women with all men would not create a sample representative of a general population. This would limit the external validity of the results. Finally, the links between income and health differ by sex. Fitzpatrick and Moore (2018) show that the effect of Social Security income on mortality is much larger for men than women. Focusing only on men maximizes the chance that an effect can be detected, and helps focus the discussion of causal mechanisms.

with diagnoses such as uncontrolled diabetes, bacterial pneumonia, or urinary tract infections. See AHRQ Prevention Quality Indicators Technical Specifications for more details.

[14] About 0.6% of male Medicare beneficiaries receive Social Security benefits as an "aged husband" or "aged widower."

My core sample includes cohorts born between 1927 and 1937. I exclude cohorts born after 1937 because they are subject to changes in the Full Retirement Age (FRA) cohorts prior to 1927 are also excluded because binding maximum taxable earnings thresholds complicate the interpretation of the income shock.[15] I also exclude persons who were originally entitled to Medicare before 65 due to disability, and those who do not receive Social Security Old-Age retirement benefits based on their own wage history.[16] Because my primary outcome is Medicare spending, I exclude those without full Parts A and B coverage as well as those who ever enrolled in Medicare Advantage. To ensure a balanced panel, I exclude people who die within the observation period.

In most specifications I also exclude persons born on January 1 and January 2 – a so-called donut hole RD design (Barreca, Lindo and Waddell, 2016). The density of birth dates spikes on January 1 raising concerns about manipulation of the running variable. However, Kopczuk and Song (2008) argue this is the result of clerical errors by the Social Security Administration, not manipulation on the part of beneficiaries. Persons born on January 2 also may be selected because Social Security rules interact in a peculiar way that allow them to retire one month earlier than normal.[17]

Table 2 reports summary statistics for the 3,068,496 unique beneficiaries in the estimation sample. The majority (87%) are white and not enrolled in a Part D prescription drug plan. The mean age is about 75 years old – 10 years after Medicare eligibility and 13 years after Social Security eligibility. On a monthly basis, mean Social Security income is $1,246 and mean payments for Medicare services is $648. Medicare paid providers directly for 84% of these services. Although the remainder was paid primarily by supplement insurers, some beneficiaries pay directly out-of-pocket. 35% of the baseline sample alive at the end of 2011 is dead by the end of 2017.[18]

4 Empirical Strategy

Because my setting does not match a classic regression discontinuity (RD) design, I consider techniques for "stacking" multiple discontinuities. A classic RD assigns a binary treatment when a

[15]The mean age of my sample is already 75 and excluding older cohorts avoids pushing it even higher.

[16]See Appendix C for a discussion of how these discontinuities apply to disability benefits.

[17]Dropping January 2 birthdates also allows for symmetry around the cutoff. That is, I drop one birthdate on the left and right of the cutoff. See Kopczuk and Song (2008) for a discussion of these issues in the context of Social Security administrative records. Appendix Table 10 shows that including these birth dates makes no difference to the main results. For more recent cohorts, Jacobson, Kogelnik and Royer (2020) show that the use of Cesarean sections leads to "missing" births near major US holidays.

[18]The mortality rate matches data from life tables which predict 35.4% mortality for men from these cohorts (United States Mortality Database, UC Berkeley).

single running variable exceeds a known cutoff. As RD techniques have grown in popularity, researchers have explored how RD tools can be applied in non-classical settings. Examples include stacked RD designs, which collapse discontinuities from multiple cutoffs (Cattaneo et al., 2016); regression kink designs, which test for discontinuities in slopes (Card et al., 2015; Gelber, Moore and Strand, 2018); and difference-in-discontinuities designs, which test for changes in discontinuities over time (Duggan, Gupta and Jackson, 2019; Persson, 2020). My setting does not match any of these categories. Because it includes placebo discontinuities as well as multiple cutoffs, it combines difference-in-discontinuities with stacked, ordered treatments. For this reason, I consider different specifications for aggregating the discontinuities. The goal in each case is to measure a single average elasticity across all cohorts.

4.1 Estimating Several Regression Discontinuities Separately

I first run the classic regression discontinuity design separately for each of the 10 cohorts. Following Gelman and Imbens (2019), I estimate a specification with varying linear trends in date of birth:

$$\log(Y_i) = \beta_0 + \beta_1 D_i + \beta_2 DOB_i + \beta_3 (DOB_i * D_i) + e_i \tag{2}$$

where i indexes date of birth, Y_i denotes the outcome of interest, DOB_i is a linear trend normalized as the distance in days from the cutoff, and D_i is a dummy for the January 2 cutoff. The coefficient of interest is β_1 which estimates the percentage change in the mean of the outcome at the cohort boundary. The unit of observation is an average collapsed within a date of birth cell and regressions are weighted by the number of individuals in each cell. I take this approach to be consistent with Gelber, Isen and Song (2016). Using aggregate data estimates standard errors which are more conservative and accounts for correlated shocks at the date of birth level (Angrist and Pischke, 2009). Working with averages also ensures all observations are positive. This allows me to consistently use logarithmic specifications.

The β_1 coefficient identifies a causal effect of the benefit shock on the outcome if two assumptions are satisfied: (i) beneficiaries cannot precisely manipulate their date of birth around the cutoff, and (ii) no potential confounders are also changing discontinuously at the cutoff.

This specification provides a unique β_1 for each of the 10 cohorts in my sample. One technique for summarizing these results would be to compute the average coefficients for positive and negative treatments and to rescale by average treatment size. However, this method discards valuable information about the ordering of the shocks. Within the group of positive shocks, the effect size should increase with the treatment size. To take advantage of this ordering, I plot the β_1 coefficients from equation (2) for each cohort against the income shock prediction from equation (1). This provides a clear visual summary if the pattern of discontinuities in the data match the pattern of discontinuities from the benefit formula. In particular, it allows us to visually inspect if the effects are symmetric around zero and scale linearly.

4.2 Stacked and Scaled Regression Discontinuity

Extending the intuition of plotting coefficients from separate regressions, I next consider a specification which estimates multiple discontinuities in a single equation. By "stacking" multiple cohorts on top of each other I can maximize the efficiency of the estimator. Specifically, I estimate

$$\log(Y_i) = \beta_1(D_i * S_c) + \sum_{c\in C} \beta_{2,c} DOB_i + \sum_{c\in C} \beta_{3,c}(DOB_i * D_i) + \beta_c + e_i \tag{3}$$

where C is the set of cohorts over which the relevant discontinuities occur, β_c is a cohort fixed effect, and the summation signs permit slopes to vary by cohort on either side of the cutoff. S_c denotes the cohort-specific income shock. Because the outcome is measured in log points, I adjust the scale to be interpreted as 1% shock. For example, under a 4% benefit shock $S_c = 0.04$ for all observations within the cohort. The coefficient of interest is again β_1. If the outcome Y_i is measured as the dollar amount of Medicare payments then β_1 estimates the elasticity of Medicare payments with respect to Social Security income.

In addition to the assumptions described above, this specification also assumes the elasticity is equal for all cohorts. That is, if a positive 4% income shock leads to a 4% spending decline, then a negative 1% income shock leads to a 1% spending increase. This is equivalent to plotting the coefficients against the income shock and constraining the linear fit to pass through the origin.[19] Appendix A1 describes a specification in the spirit of a difference-in-discontinuities design that allows the intercept to vary.

[19] Individuals are unaware they have been "treated," so effects should be symmetric around zero. Non-symmetric effects would only arise from behavioral frictions. The complexity of the shock makes this unlikely.

4.3 Bandwidth Selection

In my primary specifications I use a 30-day bandwidth. This is a natural bandwidth for several reasons. First, the beginning dates of Medicare and Social Security eligibility vary discontinuously for persons with birth dates on the second day every month.[20] Keeping the whole sample within a month of the cutoff avoids adding more discontinuities. Second, fertility patterns differ systematically by season (Buckles and Hungerman, 2013). Although these trends are smooth, they are not necessarily linear over the course of several months. Using a narrow 30-day bandwidth avoids modeling this seasonality and allows for a transparent, linear specification. Finally, the density of birth dates increases slightly at the first of the month. A 30-day bandwidth drops individuals born on December 1 and February 1 who may be somewhat selected.

In a standard RD setting, the Calonico, Cattaneo and Titiunik (2014) procedure is a popular method for bandwidth selection. Although this tool was not designed for multiple ordered treatments of different signs, as a robustness check I explore how it could be adapted for my setting. These results as well as other bandwidth selection procedures are described in Appendix A3.

5 Results

I present results in five subsections. I start by validating that Social Security income across cohorts differ exactly as predicted by equation (1). Next, I look at each cohort separately and provide graphical evidence that income shocks lead to declines in Medicare spending. I then combine all cohorts using a stacked RD specification to estimate a baseline elasticity. I validate the main result using several specification and placebo tests. Finally, I examine health directly by examining chronic conditions and mortality. The results for chronic conditions are imprecise, but I find clear evidence of a decline in mortality. Overall, the results suggest income can reduce health care spending and mortality.

5.1 Evidence of the Income Discontinuity

Benefits amounts are not recorded in the Medicare data, so I use the Public Use Benefits File to confirm that benefit levels in the data follow the pattern predicted by equation (1). The absence of a date-of-birth variable in the public use data makes the preferred specification infeasible. As

[20]For most individuals who qualify by age, Medicare eligibility begins on the first day of the month an individual turns 65. For individuals with a date of birth on the 1st of the month, coverage starts the first day of the prior month.

an alternative, I compare the difference in monthly benefits by year of birth. In the language of regression discontinuity designs, the specification includes no linear trends and a 12 month bandwidth.[21]

Figure 1 shows the pattern in benefit differences matches the pattern predicted by equation (1). The slope of the fit line is almost exactly one. The principle insurance amount (the full entitlement amount before any early claiming penalties) follows the same pattern. This suggests the shocks do not affect claiming behavior. Another question is if the benefit shocks were offset by changes in labor market earnings. Figure 2 shows the pattern in benefit differences have no relationship to total earnings, or labor force participation. This suggests the shocks for total income are similar to the shocks for Social Security income.[22]

The result that labor force outcomes do not change is consistent with recent work exploring how behavioral frictions influence retirement decisions. Factors such as incomplete information (Liebman and Luttmer, 2015), framing effects (Brown, Kapteyn and Mitchell, 2016), and cognitive biases (Brown et al., 2019) have large impacts on Social Security claiming choices. These results are incompatible with standard life-cycle expected utility models. In my setting, beneficiaries are unaware they have been "treated." The shocks have no salience, so it is not surprising that short-run labor force outcomes do not adjust. In contrast, when beneficiaries receive a highly salient "treatment," they are more likely to change their behavior (Deshpande, Fadlon and Gray, 2020; Gelber, Isen and Song, 2016).

5.2 Graphical Evidence on Expenditures from Separate Cohorts

To inspect the pattern in the underlying data, I plot total Medicare spending around the date of birth cutoff for negative, null, and positive treatments separately. To provide a consistent comparison across all 10 cohorts, I use equation (2) to residualize away the cohort-specific means and trends on either side of the cutoff. Figure 3 plots spending residuals after this normalization. The negative income shocks are smaller and less frequent, but there appears to be evidence of a symmetric effect: spending increases for negative shocks, remains unchanged for null shocks, and declines for positive shocks.[23]

[21] This specification is only possible because Social Security benefits do not have a strong age gradient. This is not the case for health spending.

[22] Table 5 in Gelber, Isen and Song (2016) presents additional evidence that labor force outcomes are unchanged. Using SSA and IRS administrative data, they find no discontinuous changes in earnings around January 2 dates of birth for 1928, 1930, 1932, 1934, 1936 cohorts.

[23] A similar pattern is visible in Appendix Figure 3 which shows the same plots without normalization.

To visually examine the cohorts separately, I plot the β_1 coefficients from equation (2) for each cohort against the predicted income shock. Figure 4 depicts these coefficients and their associated confidence intervals.[24] Several patterns emerge. First, the results are consistent with negative shocks leading to increases in Medicare spending, while positive shocks having the opposite effect. The slope of the OLS fit line – a measure of the elasticity – is -0.95 (standard error 0.43). Appendix Figure 2 shows a nearly identical pattern structure when the bandwidth is selected using the Calonico, Cattaneo and Titiunik (2014) non-parametric estimator and robust bias-corrected inference. Second, the fit suggests the assumptions imposed by equation (3) are reasonable. The line intersects the origin which indicates there is, on average, no change for the placebo treatments. There is also no evidence of non-linear treatment effects. Finally, because the estimate for any single cohort is imprecise, it is necessary to estimate all cohorts simultaneously.

5.3 Regression Evidence on Expenditures from Stacked Cohorts

Table 3 presents estimates from the stacked equation (3) with the log sum of all spending from 2006 to 2011 as the dependent variable. Column (1) presents the preferred specification with no controls. The elasticity estimates are centered around -0.9 implying that a 1 percent increase in Social Security benefits reduces total Medicare payments by 0.9 percent.

The result is similar across different specifications. Column (2) includes controls for race which may help account for racial differences in fertility patterns. I do not consider other potential demographic controls (Medicaid enrollment, zipcode characteristics) because these are likely to be affected by the benefit shock. Following previous literature I estimate robust standard errors at the date-of-birth level, but I also compute standard errors using day-month clustering (column 3) and jackknife sampling (column 4).[25] While the previous specifications include all 10 cohorts, columns (5) and (6) consider using only the positive treatments, or only the non-zero treatments. This is equivalent to estimating the slope in figure 1 by omitting the four non-positive treatments, or the two null treatments. The coefficient remains unchanged suggesting particular cohorts are not driving the variation.

Table 4 shows the elasticities are similar for direct provider reimbursement payments and cost-sharing. In terms of levels, if Social Security income increases by \$100, then payments from Medicare decline by \$37 and cost-sharing payments decline by \$6.

[24] Appendix Figure 4 plots the raw data for all 10 cohorts individually.

[25] As an additional robustness check, Appendix Table 5 presents results in levels using both the individual level microdata, and the date-of-birth collapsed file. The standard errors are similar in all cases.

If the income elasticity of spending is negative, does this imply Medicare expenditures are an inferior good? Two pieces of background are useful for interpreting the result. First, preventive care accounts for a small share of Medicare spending. Services like immunization or cancer screenings are inexpensive. The majority of Medicare spending goes to acute care and managing chronic conditions.[26] In this sense, high levels of Medicare expenditures are best interpreted as an indicator of poor health. To the extent that Medicare expenditures are a type of health investment, they function asymmetrically. For people who are sick with many chronic conditions, medical spending mitigates rapid declines in the health capital stock. On the other hand, for people who are in good health, additional medical spending will do little to raise the stock of health capital. Once they are up to date on their immunizations and cancer screenings, productive health investment occurs mostly beyond the scope of Part A and B services through other medical services (prescription drugs, long-term care) or lifestyle changes (diet, exercise, stress, environmental factors).[27]

Second, the price at point of service for many beneficiaries is zero. Although beneficiaries can face substantial out-of-pocket costs for uncovered services (for example, long-term care and prescription drugs), supplement insurance pays for most Part A and B cost-sharing. The administrative files do not record the final payer, but data from the National Health Expenditure Accounts suggest only 4% of the cost of Part A and B services is paid out-of-pocket.[28] Within the context of Part A and B services, beneficiaries are minimally exposed to prices. Table 4 provides further evidence by comparing elasticities for Medicare provider reimbursement with Medicare cost-sharing payments. If cost-sharing was binding these elasticities should differ, but the results indicate they are similar. Because beneficiaries have near full insurance, health status plays a larger role in consumption decisions than income.

5.3.1 Validating the Regression Discontinuity Design

The main specification uses linear trends to be consistent with previous work, but I also consider including quadratic terms. Appendix Table 4 compares four specifications: a baseline linear model, linear with controls, a global quadratic on either side of the cutoff, and cohort-specific quadratic terms on either side of the cutoff. The linear specification with no controls minimizes the Akaike

[26] Reid, Damberg and Friedberg (2019) show that even if preventative care is defined broadly (evaluation and management visits, preventive visits, care transition or coordination services, and in-office preventive services, screening, and counseling), it still accounts for only 2% of FFS Medicare spending.

[27] Appendix D sketches a model to formalize this intuition.

[28] Tables 8 and 12 from Age and Gender files National Health Expenditure Data, 2010. Part A roughly corresponds to Hospital Care and Part B roughly corresponds to Physician and Clinical Services.

Information Criterion (AIC) and Bayes Information Criterion (BIC), so I use it as the baseline.

Although manipulation of the date-of-birth variable could threaten identification, the risk that beneficiaries deliberately use a fraudulent birth date is low. Not only are birth records difficult to falsify, but the complexity of the benefit formula makes it unlikely beneficiaries are aware these discontinuities exist. Appendix Figure 5 plots the histogram for each treatment type and provides no evidence of manipulation. Appendix Table 3 examines this by running regressions with the log count of observations as the dependent variable. Although January birth dates are more common than December birth dates, the pattern holds for all cohorts and does not vary by the sign of the treatment.

A related concern arises from endogenous enrollment into Medicare Advantage. If higher income reduces FFS enrollment this would create a selection bias because the sample would systematically differ on either side of the discontinuity. Although selection into Medicare Advantage would create bunching, I can also test for this directly using the enrollment file. Appendix Table 1 presents OLS results from equation (3) with log share of the population as the dependent variable. I do not find evidence the shocks induce changes in coverage in Part A, Part B, or Medicare Advantage.[29]

A final threat to identification is discontinuous changes in other policies around the cutoff date. Appendix A describes how to test this assumption using a variation on the difference-in-discontinuities design. By assuming linear effects across cohorts, we can test if the pattern of treatment effects is consistent with a null effect for a null treatment. The constant term in Appendix Table 6 is not different from zero, suggesting other policies are not changing around the cutoff.

5.3.2 Sensitivity and Placebo Tests

Given the importance of bandwidth selection to regression discontinuity designs, it is necessary to test how estimates vary under different assumptions. I rerun the regression above for various bandwidth lengths and plot the coefficients with their 95% confidence intervals in Figure 5. The coefficient consistently hovers around -1.0 regardless of bandwidth choice and is statistically significant at the 5% level for most bandwidths in the 10 to 50 day window.

As a placebo test, I rerun the regression with the predicted income shocks at placebo cutoff dates. I consider all dates between the months of February and November on either side of the cutoff. With a 30-day bandwidth, this will exclude any sample with the true January 2 cutoff.

[29]Another potential concern relates to selection around the discontinuity due to mortality prior to the observation period. This is not necessarily a threat to identification, but does change the interpretation of the treatment effect. I postpone a discussion of these issues until section 5.5 where I present mortality results.

Figure 6 shows the histogram of these coefficients with the solid vertical line denoting the estimate from the baseline specification, and the dotted lines denoting 95% confidence intervals. The baseline coefficient is less than the placebo coefficients 98.8% of the time.

5.3.3 Dynamics and Age Heterogeneity

An open question in the literature is how treatment effects evolve over time. For example, increased spending on preventive care could lead to decreased spending on acute care several years later. To test this, Appendix Figure 7 plots the elasticities and their associated confidence intervals for each of the six years in the data. Although we can rule out large positive effects in the earlier years, there does not appear to be a clear pattern. Appendix Section A3 presents another technique for measuring age heterogeneity using a date-of-birth by sample-year level regression. Appendix Table 7 does not find an effect, but the results are imprecise. I do not observe the sample until several years after the benefit shock, so data from ages closer to 65 may tell a different story.

5.3.4 Composition of Spending

To explore the mechanisms behind the decline in Medicare spending, I decompose aggregate spending into its subcomponents.[30] Figure 7 shows how each category contributes to the share of the variation in total spending. The percentages are normalized to sum to negative one. Nearly every category of spending declines with hospitals accounting for almost half of the total effect. This is consistent with income causing improvements in health and reducing the demand for acute care.

Spending on physician-administered drugs (Part B drugs) also shows large declines. In contrast to normal Part D prescription drugs, Part B drugs include vaccinations, chemotherapy drugs, injections for rheumatoid arthritis, and biologics for autoimmune conditions. Although part of this decline can be explained by health improvements, supply side factors may also contribute. Medicare reimburses physicians for these drugs through a formula that provides incentives for prescribing expensive treatments (Chandra and Garthwaite, 2019). If providers for low-income patients emphasize profitability while providers for high-income patients emphasize clinical need, then provider switching would cause declines in spending. This would be consistent with evidence from randomized trials showing that patients who are assigned to higher-quality physicians receive less expensive treatment (Doyle, Ewer and Wagner, 2010). Similarly, hospitals in higher income

[30]The categories do not correspond neatly to definitions of preventive care, but evaluation/management and physician office visits are the closest.

regions are more likely to rapidly adopt cost-effective innovations (Skinner and Staiger, 2015).

5.3.5 Income Heterogeneity

The elasticity of spending with respect to income is likely to differ across the income distribution. To examine treatment effect heterogeneity by income, I split the sample into income quintiles based on beneficiary zipcode. I assign income quintiles using the mean Social Security benefits by zipcode and run equation (3) separately for each of the five subsamples.[31] Appendix Figure 7 shows the effect is largest for the beneficiaries living in middle-income areas, while high- and low-income areas have smaller elasticities. The result for high-income beneficiaries is unsurprising given they have the fewest constraints on health investment. For low-income beneficiaries, the result may reflect the impact of additional safety net programs.

Beneficiaries near the federal poverty line are eligible for Medicare Savings Programs which exempt them from cost-sharing, Part B premiums, and many Medicare Part D costs.[32] This may eliminate a key barrier to care. The lowest-income beneficiaries are additionally eligible for full Medicaid benefits which includes services Medicare does not normally cover like nursing home and dental care. They also may be eligible for Supplemental Security Income (SSI) which effectively bounds the minimum benefit amount at 75% of the poverty line.[33] Appendix Table splits the sample by Medicaid enrollment and provides evidence the changes are largest for those not enrolled in Medicaid. Finally, because the benefit shock is the same in percentage terms across the income distribution, the lowest-income across also have lowest level change in the dollar value of benefits.

5.3.6 Quantile Treatment Effects

Focusing only on the mean treatment effect may overlook large changes in the distribution of health care spending. In particular, the spending distribution has a long right tail, so most of the effect could be driven by a small share of the sample. To compute quantile treatment effects, I estimate equation (3) replacing mean spending in a date-of-birth cell with the decile of spending in a cell. Figure 8 plots the coefficients for each spending decile and its associated confidence intervals. Although some estimates are noisy, there is a clear pattern that effects are largest at the top of the

[31] One caveat to the results from this disaggregation is that the income shock may lead to sorting around the discontinuity if beneficiaries respond by moving neighborhoods.

[32] Eligibility depends on income, assets, and state of residence. See Data Book: Beneficiaries Dually Eligible for Medicare and Medicaid (MACPAC) for details. Appendix Table 2 does not find evidence the income shock influences Medicaid enrollment.

[33] 3% of OASI recipients over 65 also receive SSI payments (SSA Statistical Supplement 2010, Table 3.C5)

spending distribution.

The results is consistent with two explanations. First, the sickest patients with highest spending may have the most to gain from additional income. This follow from a concave health production function where productive investments are made outside of spending on Medicare A and B services. For example, if an income shock reduces mental stress, the value of lower stress would be greater for someone in worse health. Second, higher income may induce patients to use more efficient providers. As may be the case for Part B drugs, if providers differ in their supply elasticities, an income shock that induces patients to shift away from the most expensive providers would cause a large decline in spending.

5.3.7 Hospital Visits and Quality

To better assess how income affects the demand for hospital care, I use the hospital claims data to construct measures of provider quality and service intensity. Table 5 presents the elasticity for total acute stays as well as readmissions and avoidable admissions, two commonly used proxies for quality-of-care.[34] The elasticity for total acute stays is precisely estimated and shows a clear decline. Additionally, there is some evidence that readmissions and avoidable admissions decline as well. This is consistent with higher income beneficiaries receiving higher quality care in both inpatient and outpatient settings.

To investigate hospital quality directly, I decompose the change in hospital spending into high- and low-quality hospitals. I define a high-quality hospital as those with an above average risk-adjusted mortality rates. These ratings were developed by CMS and have been validated by (Doyle, Graves and Gruber, 2018). Specifically, they use quasi-random assignment of ambulances to hospitals to show the CMS ratings are causally associated with clinical outcomes. Columns (4-5) of Table 5 shows results of the quality decomposition. Although the point estimate for high-quality hospitals is closer to zero than the point estimate for low-quality hospital, the confidence intervals for both categories are large and we cannot reject they are equal. One challenge to estimating hospital quality is that claims data are available only for 3 years instead of the 6 years for more aggregate utilization measures.

[34] Following guidelines from AHRQ, I define "avoidable admissions" as admissions for one of 16 ambulatory care sensitive conditions using the PQI 90.

5.4 Regression Evidence on Diagnoses for Chronic Conditions

To examine more directly if income improves health, I consider diagnoses for chronic conditions as another outcome variable. To do this, I replace mean spending in equation (3) with the mean number chronic conditions in a date-of-birth cell. This provides a simple summary measure of underlying health. For consistency, I use the same specification with a 30-day bandwidth. Table 6 estimates the elasticity of the count in chronic conditions with respect to income is about -0.35. This suggests the declines in spending are in due at least in part to improvements in health.

To disaggregate the 22 conditions in the underlying data, I use categories developed by Medicare's Chronic Conditions Data Warehouse. Figure 9 panel (a) plots the coefficient for each category. To interpret the coefficient, a 1% change in Social Security income reduces the fraction of the sample with a cardiovascular condition by 0.59%. Although nearly all of the point estimates are negative, the elasticities are imprecise. I also test the top five most common conditions across any category. Examining particular conditions may reveal an effect that aggregate categories obscure. Figure 9 panel (b) shows a similar pattern. The coefficients are negative, but not statistically significant. One reason effects for chronic conditions are difficult to measure is the data include only a few years. The reference period extends at most back to 2009 while data for spending extend back to 2006. Following the sample over more years would provide more precise estimates.[35]

5.5 Graphical and Regression Evidence on Mortality

The final outcome I consider is an unambiguous measure of health: mortality. Unlike chronic conditions, which are only measured if beneficiaries seeks treatment, mortality is measured for the entire sample. To be consistent, the main mortality results use the same baseline sample as the spending results. I measure mortality as either disappearance from the panel between 2012 and 2016, or a recorded death during 2017. For context, the cumulative mortality rate during this 6 year period is 35%.

To visually examine the cohorts separately, Figure 10 plots the β_1 coefficients from equation (2) for each cohort against the predicted income shock where the dependent variable of interest is the log fraction of the sample that has died by the end of 2017. The pattern for mortality matches the pattern in spending: larger income shocks are associated with larger declines in mortality. Appendix Figure 8 shows an identical pattern when using Calonico, Cattaneo and Titiunik (2014)

[35] Using separate CCT bandwidths for each cohort finds similar results.

bandwidth selection and robust bias-corrected inference.

Table 7 presents regression results under a variety of specifications. Columns (1-2) use the same specification as the main spending results, and column (3) considers a difference-in-discontinuity approach described in Appendix A. Columns (4-6) consider the same specifications under the Calonico, Cattaneo and Titiunik (2014) bandwidth selection procedure. Throughout all specifications, the elasticity is around -1.0. That is, a 1% increase in Social Security income reduces the mortality rate by 1%.

My finding that mortality declines is consistent with results from Gelber, Moore and Strand (2018) on Social Security Disability Insurance, but inconsistent with results from Snyder and Evans (2006) on the 1917 Notch. The discrepancy with Snyder and Evans (2006) can be explained by two factors. First, they use Census data with quarter of birth rather than exact date of birth. As Handwerker (2011) discusses in detail, this may threaten the exclusion restriction because variation between cohorts overwhelms variation from the instrument. Furthermore, in Appendix Figure 8 of Gelber, Isen and Song (2016) the authors are unable replicate the Snyder and Evans result using full population Social Security records with exact date of birth.

Second, while the 1917 Notch changes both income and labor force participation, my setting only affects income. Gelber, Isen and Song (2016) show the Notch was highly salient. Beneficiaries responded by increasing labor force participation and postponing retirement. Given that early retirement can have adverse health impacts, the negative effect of the benefit cut offsets a positive effect from increased labor force participation (Fitzpatrick and Moore, 2018; Kuhn et al., 2019). Conversely, for my policy change which has no salience, I find that labor force outcomes are unchanged. Thus, the positive effect from income is not offset by a negative effect from retirement.

The mortality elasticity is large, but still within the range of estimates from other settings. The paper closest in methods and setting is Gelber, Moore and Strand (2018). They study the effect of income on mortality among Social Security Disability Insurance beneficiaries, another high-mortality, low-income group. Using administrative records and a regression kink design, they estimate an elasticity of 0.56 (s.e. 0.09). My estimate has a wide confidence interval, so I cannot reject that the two are different. Estimates from other contexts include -0.94 for pension recipients in Russia (Jensen and Richter, 2004), -0.57 for Union Army pensions in the United States (Salm, 2011), and -0.18 for elderly recipients of conditional cash transfers in Mexico (Barham and Rowberry, 2013).

(I have ordered 4 more enrollment files for various years starting in 1999. These files should

arrive in September and will allow me to measure mortality at younger ages. In particular, this will help me to discuss concerns about selection due to mortality before the observation period.)

6 Mechanisms and Policy Implications

6.1 Mechanisms

Although my setting provides an opportunity to estimate causal effects, it is less well-suited to determining the mechanisms behind these effects. The key challenge is that many health inputs are unobserved in the Medicare data. Because my results rely on a narrow 30-day window around the date-of-birth cutoff, it is not possible to investigate other health inputs using survey data.[36] Nevertheless, examining my results within the context of existing research suggests there are several potential channels. Because the income shock occurs starting at age 62, early-life factors such as education or family background can be ruled out. Similarly, the results in section 5.1 suggest labor force decisions also do not play a role.

One potential channel may be greater consumption of unobserved health services. This could be through reduced price sensitivity to cost-sharing for prescription drugs. Previous work has found that the income elasticity of prescription drug use is above one (Moran and Simon, 2006) and that greater drug utilization generates large offsetting declines in hospital spending (Chandra, Gruber and McKnight, 2010). More income may also improve health by allowing for greater flexibility to choose between formal home care, informal care, and nursing services (Goda, Golberstein and Grabowski, 2011; Tsai, 2015). My finding that elasticities are lower for Medicaid beneficiaries supports this view.

Second, income is likely to improve mental and emotional health. Evidence suggests less financial strain can reduce stress, improve decision making ability, and reduce the burden of physical disease (Ridley et al., 2020). Given that the elderly suffer from high rates anxiety, mood disorders, and depression, additional income may help ease the psychic burdens of aging (Golberstein, 2015).

Another potential mechanism is residential location. Evidence from Chetty et al. (2016) suggest that neighborhoods are powerful determinants of longevity, and several papers studying focusing on Medicare beneficiaries reach similar conclusions. Deryugina and Molitor (2018) find that Hurricane

[36] In addition to a narrow bandwidth, allowing the slope of the age gradient to differ by cohort is key to identification. This is not possible using public use data with anonymized birth dates. Although information on exact date of birth is available in restricted access versions of datasets like the HRS or NHIS, my power calculations suggest the sample sizes would be at least an order of magnitude too small.

Katrina reduced long-run mortality by encouraging migration low-mortality regions. Finkelstein, Gentzkow and Williams (2019) show similar evidence of a causal effect of place using Medicare movers across the entire country.

One neighborhood-level factor particularly relevant for the elderly is pollution. Using Medicare claims, Deryugina et al. (2019) find exogenous changes in airborne particulate matter lead to increased hospitalizations and mortality. Similarly, Bishop, Ketcham and Kuminoff (2018) provide evidence that pollution can cause greater risk of Alzheimer's disease and related dementias. Another important environmental factor is temperature. The elderly are vulnerable to extreme hot and cold temperatures, so better housing or migration to temperate climates may also be a factor (Deschênes and Moretti, 2009).

(I will have more to say about geography once I receive the earlier enrollment files. This will allow me to track location over longer periods of time.)

6.2 Policy Implications

How would a 1% increase in Social Security benefits affect total federal outlays? Beyond the direct effect of paying Social Security benefits, the estimates in this paper suggest there are two indirect effects. First, per-capita Medicare spending on Part A and B services would decline. Second, life expectancy would increase slightly, the number of beneficiaries would go up, and spending on both Social Security and Medicare would grow.

Quantifying these effects requires several assumptions. To start, it is necessary to model how treatment effects vary over time. The baseline estimate of a 0.9% decline in spending is for cohorts that have, on average, been treated for 10 years. To be conservative, I assume that treatment effects are zero at age 65, increase linearly until the period we observe in the data, and then remain constant. We assume the same pattern of dynamic treatment effects for mortality. To model base levels of spending and mortality over the life-cycle, I use the 2010 distribution of average Medicare spending by age and the 2010 SSA Life Tables. I also assume that those who are on the margin of dying – that is, those whose deaths are postponed due to the extra income from higher benefits – have the same Medicare spending as those who are always alive. In practice, they are likely to have higher spending since they are in worse than average health, but quantifying how much is challenging.

Table 8 summarizes the costs relative to baseline Social Security spending. The fiscal effect of lower Medicare spending is offset by higher spending for more beneficiaries. On net, increasing Social Security benefits by $100 would increase total expenditures across both programs by $91. Several important caveats apply. First, these estimates apply only to men. Evidence from Fitzpatrick and Moore (2018) suggest that causal effects for women may be lower. Second, these only apply for individuals in FFS Medicare plans. The extent to which they apply to Medicare Advantage plans depends on how much health investment is unobserved. Third, they assume no change in labor force participation. Although they may be relevant to policy proposals that change benefits gradually (i.e. by converting to chained price indexes for inflation), more salient policy changes that affect retirement decisions are likely to have different effects.

From the perspective of optimal policy design, cost-benefit analysis should not be limited to the federal budget. A more complete accounting would include the value of quality-adjusted longevity, the insurance value of annuitization, and deadweight losses associated with tax financing. A final caveat is that my results represent a partial equilibrium effect. Predicting the effect of income shocks large enough to induce changes in technology or aggregate supply is beyond the scope of this paper.

7 Conclusion

I have argued that Social Security income reduces health care spending and mortality for elderly men. My evidence is based on comparing health outcomes for individuals born just before versus just after cutoffs which lead to discontinuous changes in Social Security income. I showed these income changes are in fact binding, and provide evidence that their lack of salience leads to no change offsetting changes in labor force income. Next, I showed cohorts with positive income shocks experience declines in spending on Medicare covered services. I also found reductions in chronic conditions and mortality, supporting the view that underlying health is improving.

Although my ability to identify the mechanism behind the effect is limited, the heterogeneity results indicate which channels might be at work. First, spending declines across nearly all settings for all years in the data. This suggests that income generates health investment mainly outside Medicare Part A and B services. Furthermore, the effect appears to be largest for those in the middle of the income distribution and those not covered by Medicaid. This suggests that Medicaid and subsidized Part D drug coverage protect beneficiaries from some of the challenges associated with low-income. Given that take-up rates for these programs are low, policymakers may consider simplifying eligibility rules or expanding auto-enrollment. Additionally, the income effect is largest for the sickest and most expensive patients. Targeted income transfers on those with the worst health may be an effective strategy for reducing the growth rate of health spending.

Future research could explore these mechanisms by using a similar research design and other large databases. The holy grail would be to link the universe of Medicare claims with the universe of Social Security benefits and earnings records. Such a dataset would provide extraordinary opportunities to study the relationship between income and health using quasi-experimental research designs.[37] A less ambitious extension would be to explore how the income shock affects Medicaid, Medicare Advantage, or Medicare Part D claims. Because these programs cover other health services under unique cost-sharing structures, the income effect may differ. Another extension could use credit bureau data to provide evidence on how income affects household finances. A richer understanding of these mechanisms would help design more effective social insurance programs.

[37]For example, one obvious extension would be to apply the regression kink design of Gelber, Moore and Strand (2018) with health spending as an outcome. Although these linkages are available in some survey data, to my knowledge, SSA and CMS have never merged these datasets at a large scale.

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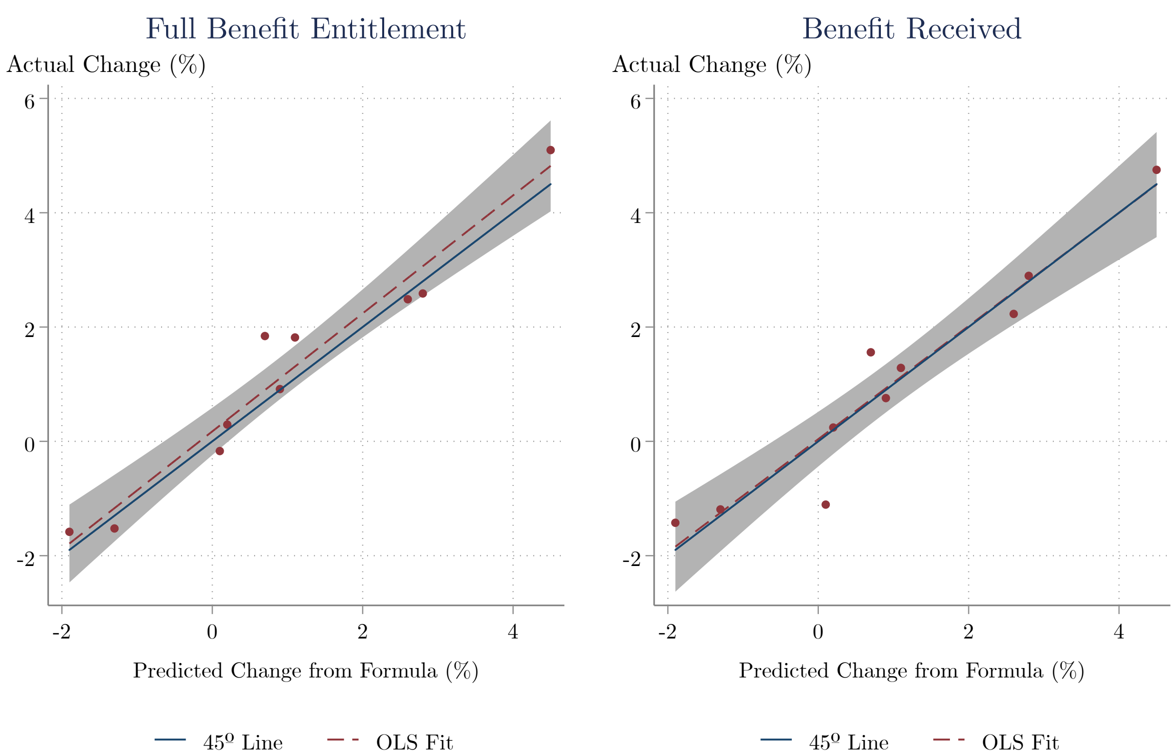
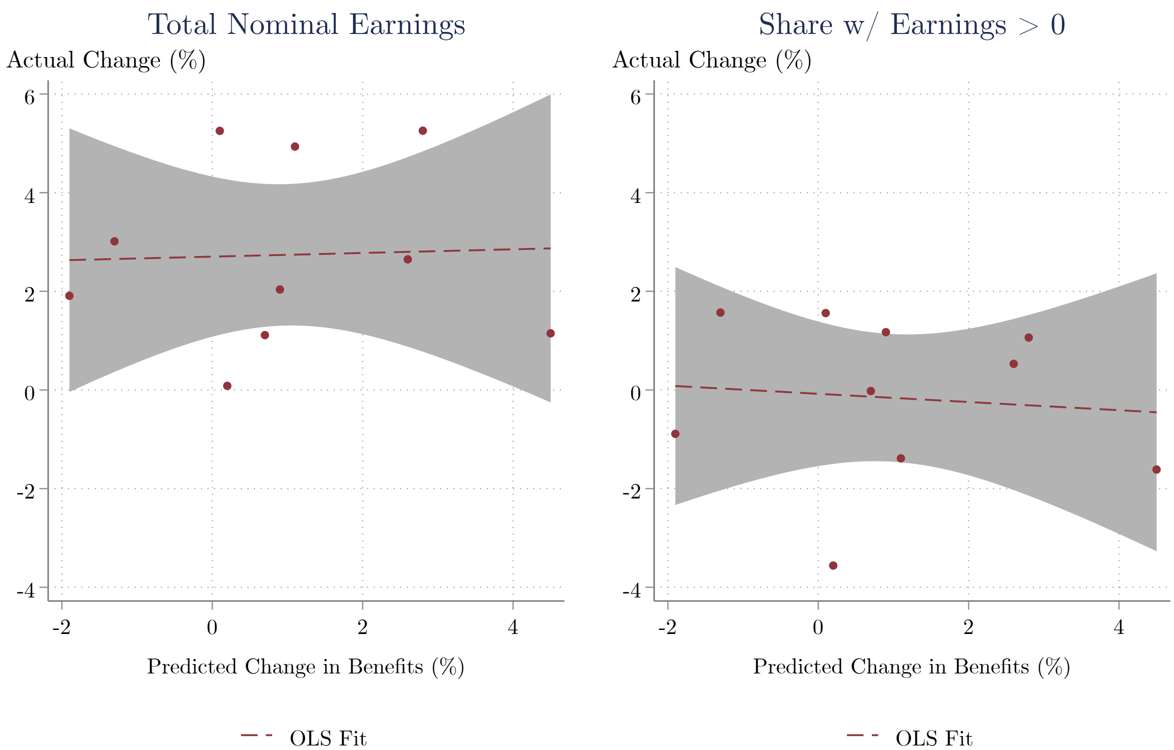
Figure 1: Differences in Benefits by Cohort



Notes: Each observation is a percentage difference between birth cohorts aggregated at the annual level. The x-axis denotes the predicted difference implied by equation (1) assuming two identical earners born on either side of cohort boundary. The left panel shows the full retirement amount before any early claiming penalties, and the right panel shows the actual benefit amount credited. In both cases, the difference in benefits is similar to the difference predicted by the benefit formula. The sample includes males receiving retirement benefits as a primary earner, and born between 1927 and 1937. Source: SSA Benefits Public-Use File, 2004

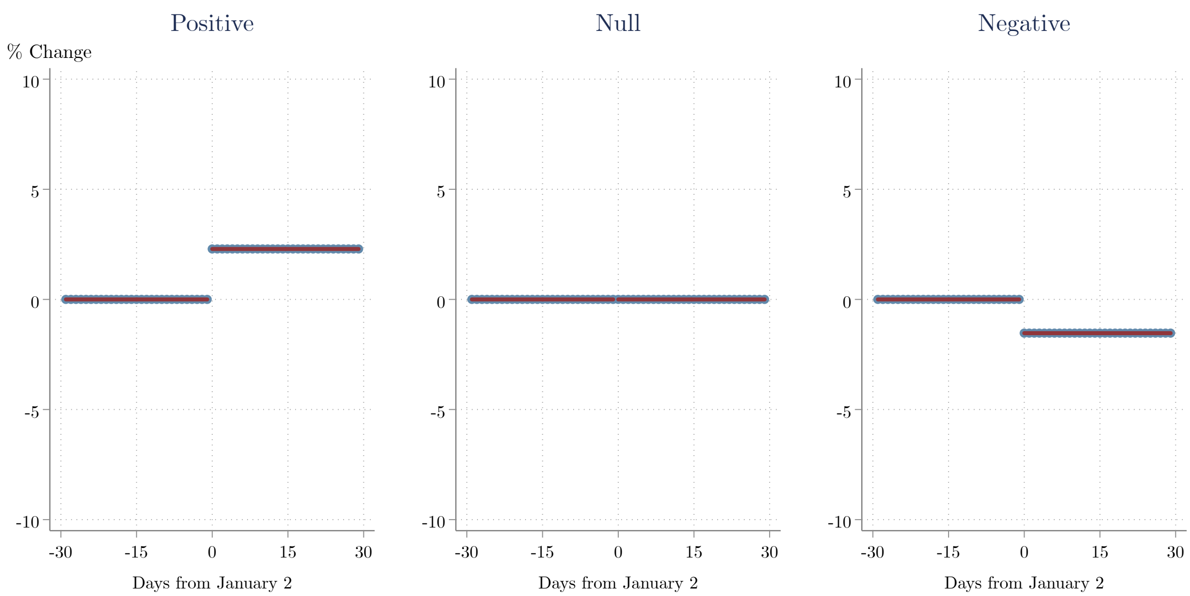
Figure 2: Labor Force Outcomes by Cohort



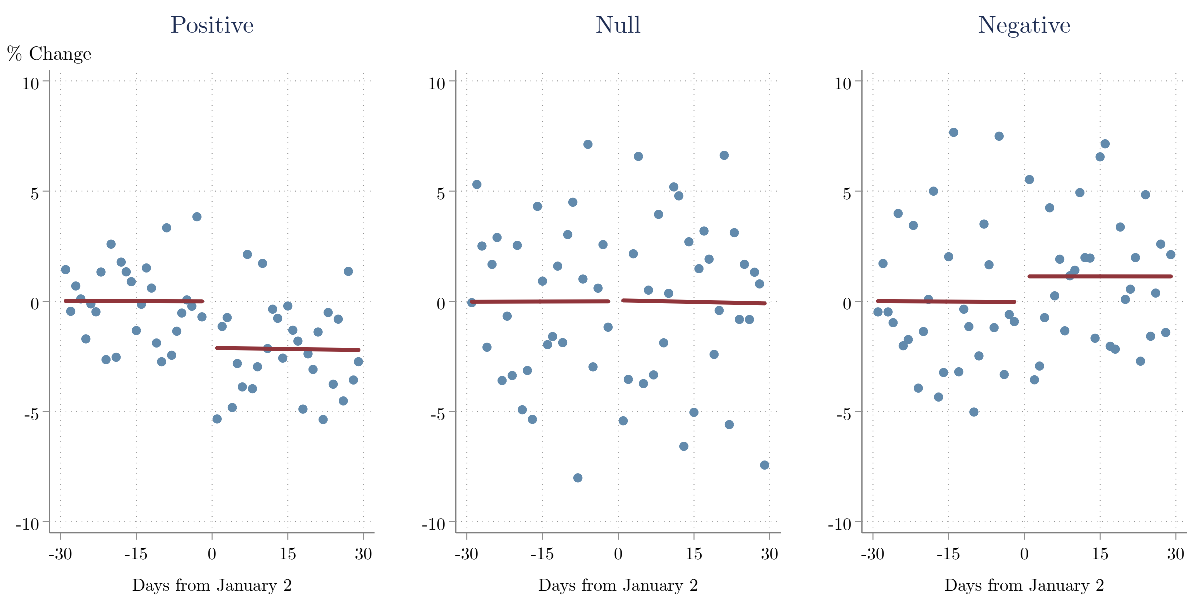
Notes: Each observation is a percentage difference between birth cohorts aggregated at the annual level. Earnings for all cohorts are observed from ages 62 to 66. The left panels shows there is no relationship between the changes predicted by the benefit formula and total nominal earnings. The right panel tests for changes along the extensive margin by examining the share with any labor earnings. Again, there is no clear relationship. This suggests the benefit discontinuities do not influence labor market outcomes.

Figure 3: Comparing Benefits and Medicare Spending by Treatment Type

(a) Social Security Benefits

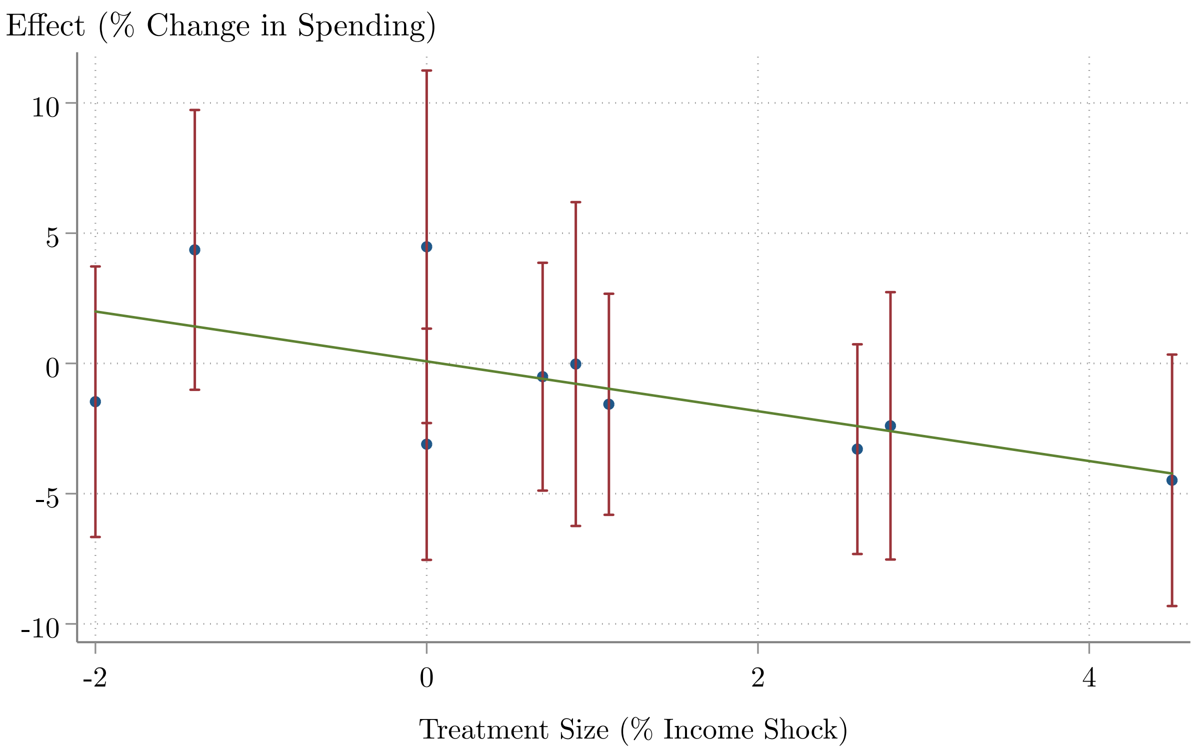


(b) Total Medicare Spending (2006-2011)



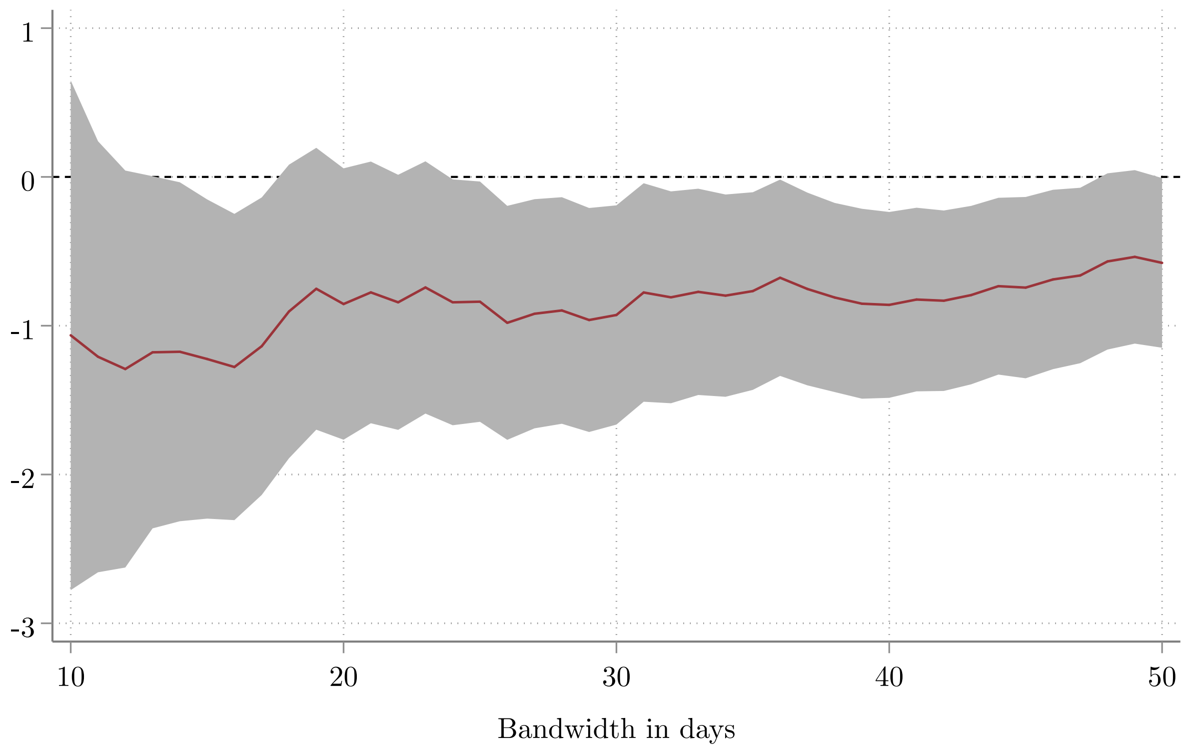
Notes: Columns from left to right show the six cohorts with positive shocks, the two with no shock, and the two with negative shocks. The top row (a) uses equation (1) to compute the change in relative Social Security benefits for individuals with identical nominal earnings. The bottom row (b) shows residualized total log Medicare Part payments where I use equation (2) to remove cohort-specific means and trends on either side of the cutoff. Each observation is an average within a date of birth cell. For positive shock cohorts, average benefits increase by 2.1% and spending declines 2.2%. For negative shock cohorts, average benefits decline by 1.7% and spending increases 1.1%. See text for details on sample restrictions.

Figure 4: Parametric Spending Effects for Each Cohort



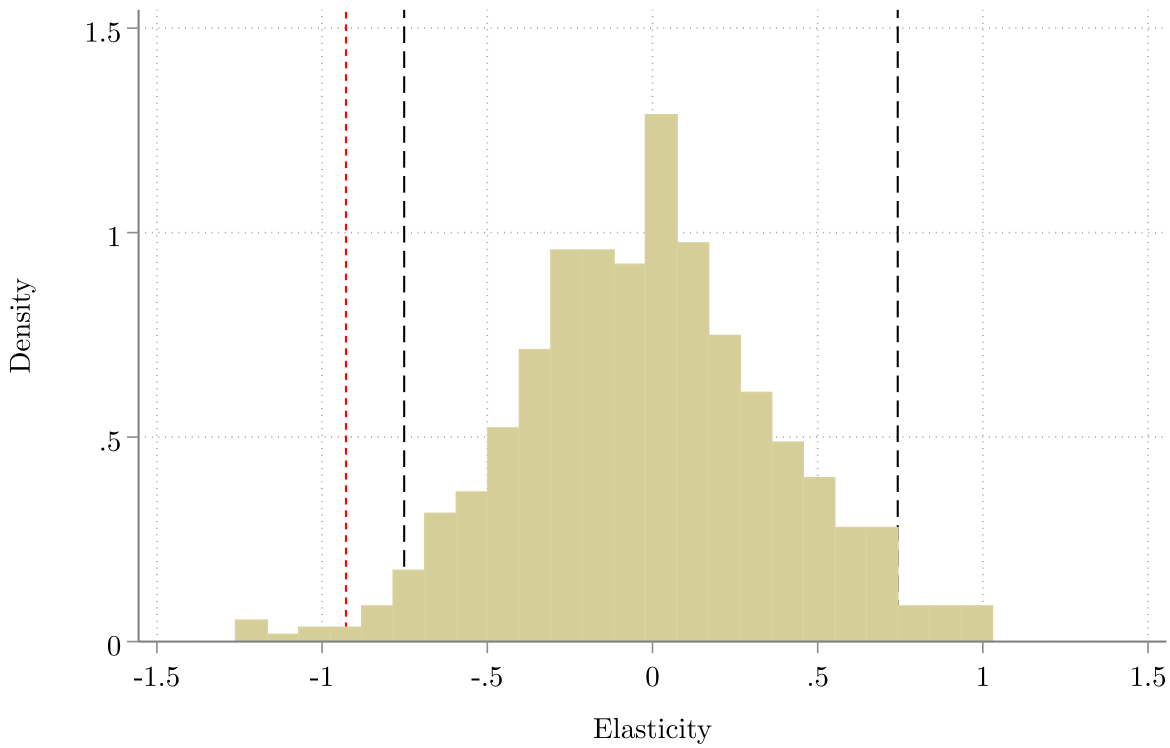
Notes: Each observation is the β_1 coefficient from equation (2) for a given cohort. Confidence interval are computed using robust standard errors. The x-axis denotes the predicted income difference implied by equation (2).

Figure 5: Bandwidth Sensitivity Test



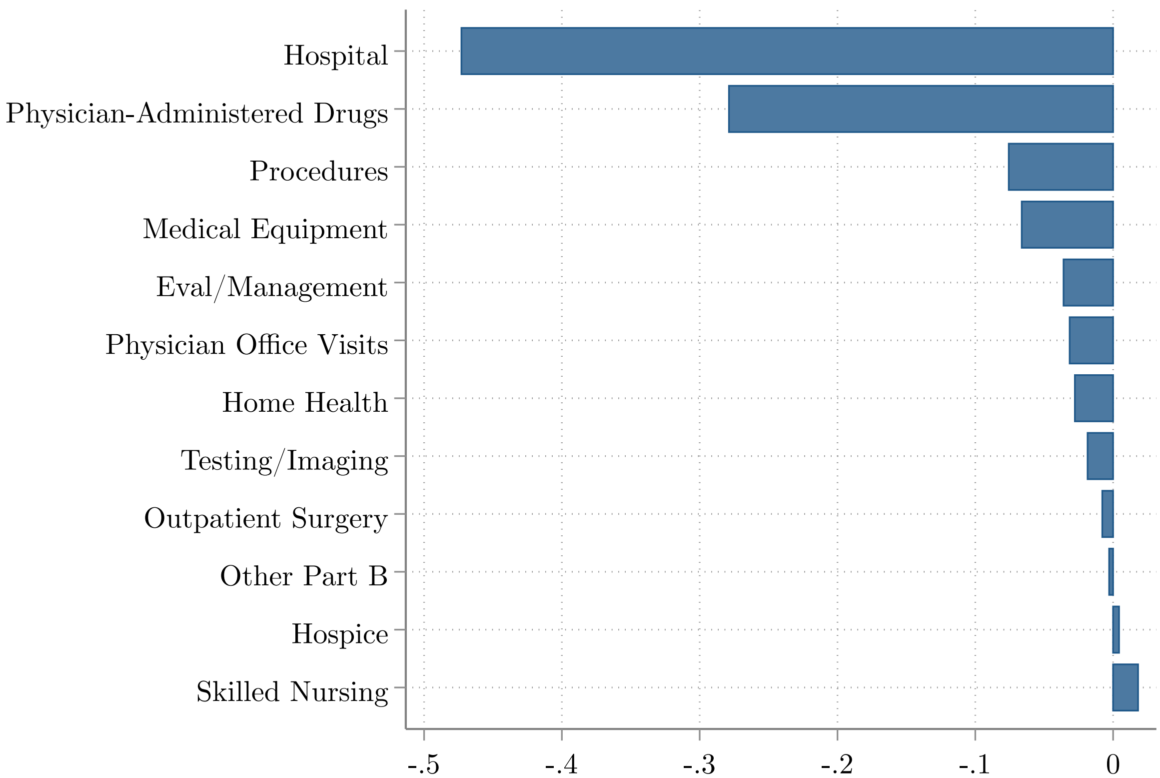
Notes: The solid line depicts the point estimate for the β_1 coefficient from equation (3). The shaded gray area depicts the associated 95% confidence interval. The point estimate consistently hovers around -1 suggesting the results are not sensitive to bandwidth choice.

Figure 6: Distribution of Coefficients for Placebo Discontinuity Dates



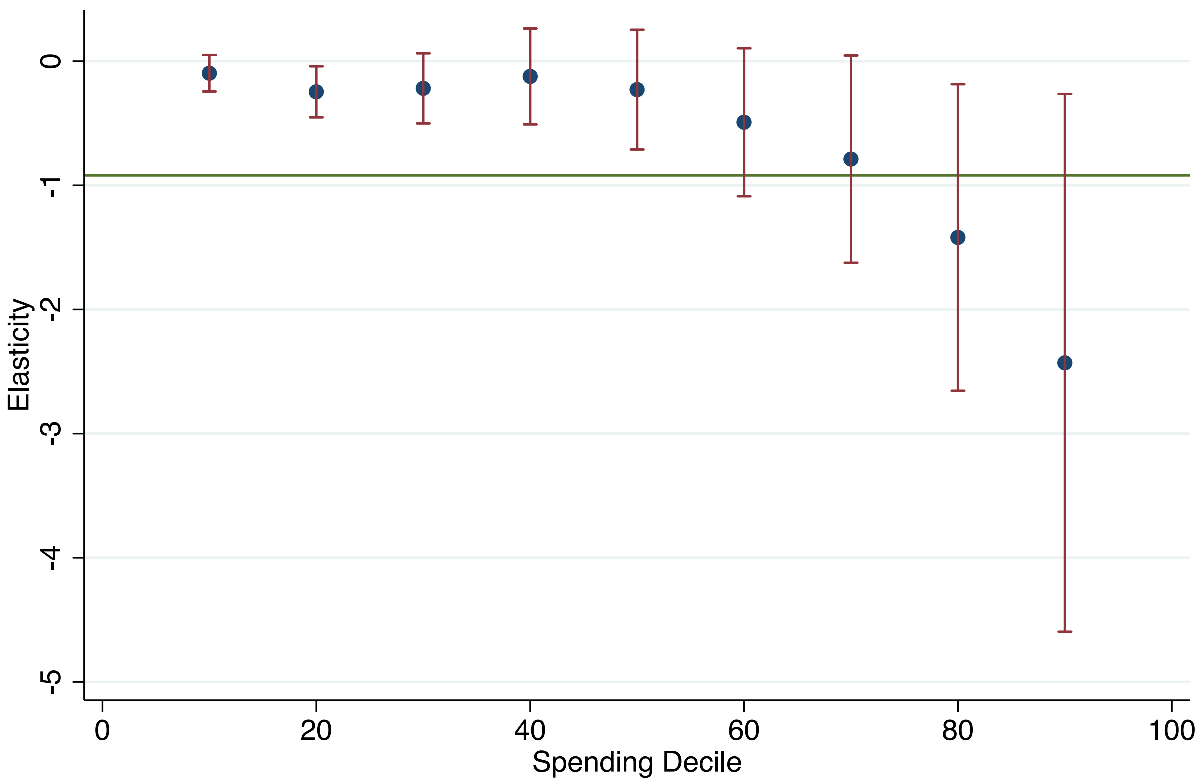
Notes: Coefficients are estimated using 606 placebo dates from months of November to February on either side of the cutoff. Dashed black lines denote the 95% confidence interval. The solid red line denotes the preferred elasticity estimated using equation (3).

Figure 7: Decomposing the Aggregate Effect by Spending Type



Notes: Spending type in levels are estimated using equation (3). Contributions to percent change in total spending are normalized to sum to negative one. Hospital category includes Inpatient Part A and Outpatient Part B. Physician-Administered Drugs refers to Part B drugs. Examples of procedures include, endoscopy, hip replacement, pacemaker insertion, or angioplasty. Other Part B includes payments for anesthesia, dialysis, ambulance, chiropractor, and other unclassified services.

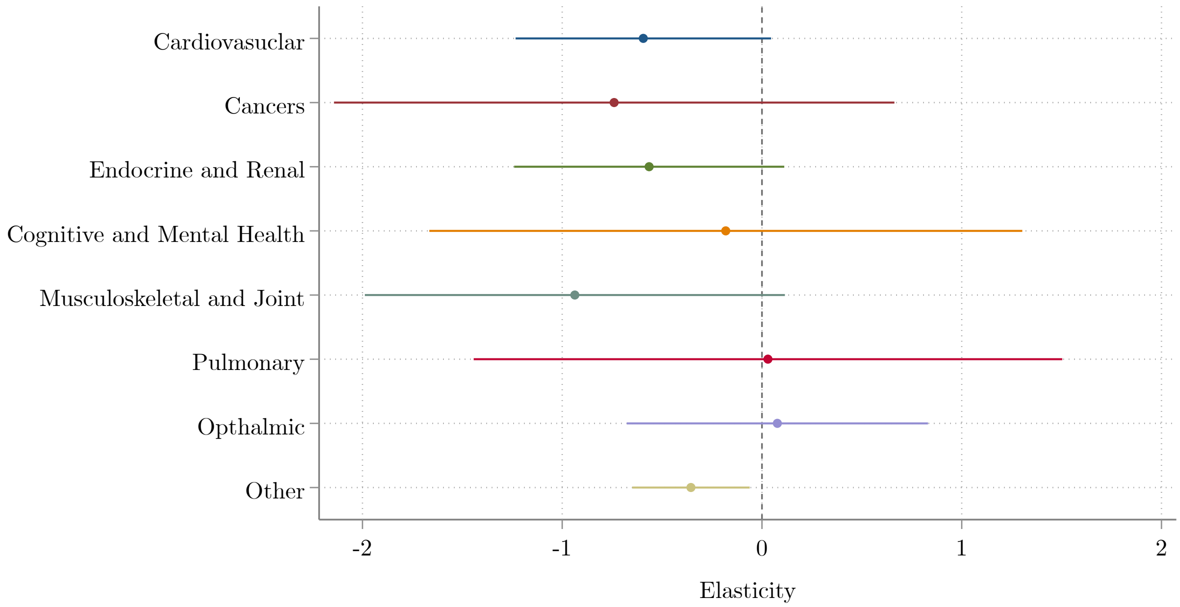
Figure 8: Elasticity by Decile of Total Spending



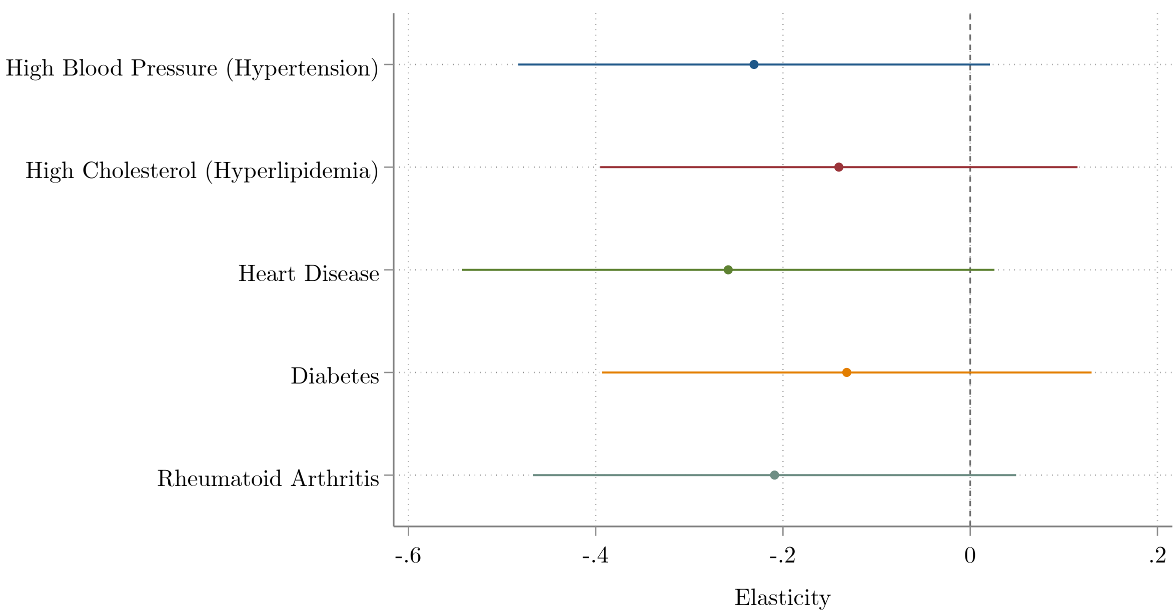
Notes: Coefficients are from a level regression of equation (3) where the dependent variable is the corresponding decile of total Medicare spending. The solid green line depicts the mean elasticity for the full sample.

Figure 9: Elasticities of Chronic Conditions

(a) Elasticities for Chronic Condition Types

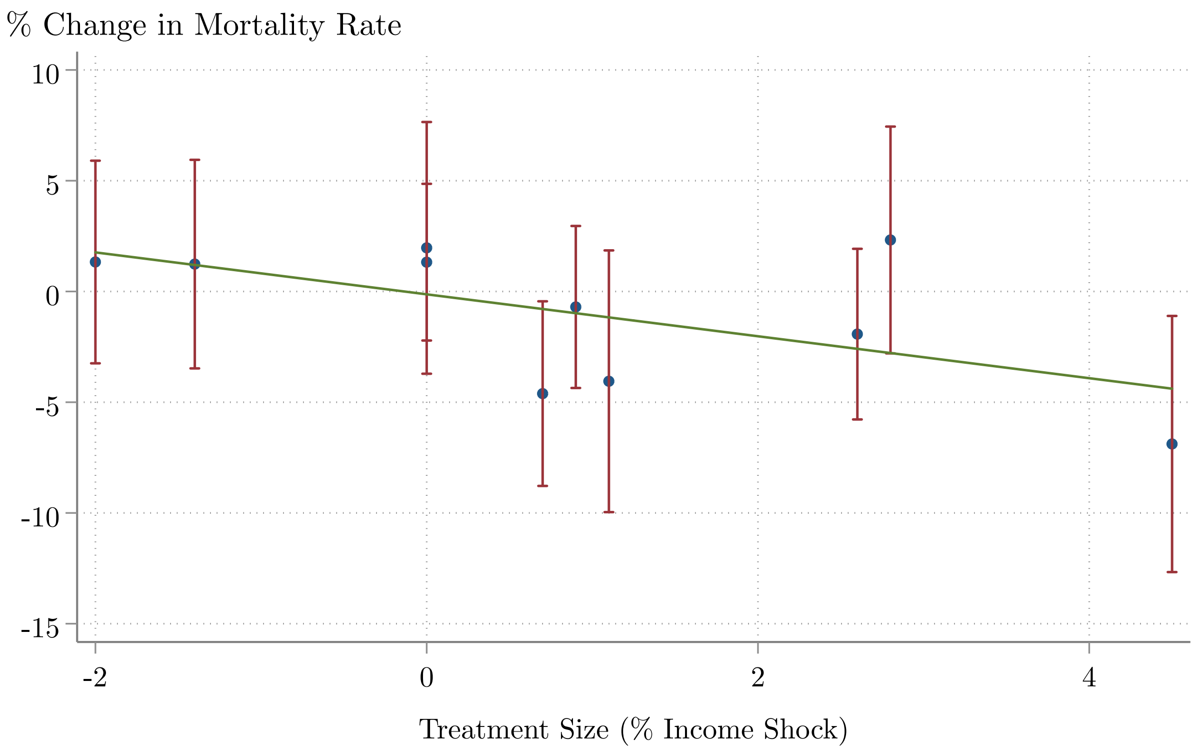


(b) Elasticities for Top 5 Chronic Conditions



Notes: Coefficients and their confidence intervals are computed using equation (3) and a 30-day bandwidth. Each point estimate denotes the percentage change in the fraction of the population with a chronic condition for a 1% increase in Social Security income. Negative coefficients imply income reduce disease incidence. The top panel (a) considers 8 categories which group all the underlying 22 chronic conditions, and the bottom panel (b) consider the top 5 most common conditions.

Figure 10: Parametric Mortality Effects for Each Cohort



Notes: Each observation is the β_1 coefficient from equation (2) for a given cohort where the dependent variable is the log fraction of the baseline sample that has died by the end of 2017. Confidence interval are computed using robust standard errors. The x-axis denotes the predicted income difference implied by equation (2).

Table 1: Treatment Sizes by Cohort

Cohort Discontinuity	Treatment Size (%)	Monthly Income Difference ($)	Age	Years Exposed	Lifetime Income Difference ($)
1927/1928	0.2	3	81	21	720
1928/1929	-1.3	-16	80	20	-3,840
1929/1930	0.9	11	79	19	2,640
1930/1931	0.7	9	78	18	2,160
1931/1932	2.6	31	77	17	7,440
1932/1933	-1.9	-24	76	16	-5,760
1933/1934	0.1	1	75	15	240
1934/1935	1.1	14	74	14	3,360
1935/1936	2.8	35	73	13	8,400
1936/1937	4.5	59	72	12	14,160

Notes: Monthly income difference compares benefits for two workers born a month apart. We assume they have identical earning histories of the average wage index for their whole career. See Appendix for details. Dollar values are inflation-adjusted using SSA COLAs to 2009 levels. Age, years exposed, and lifetime income difference is computed as of 2009.

Table 2: Estimation Sample Summary Statistics

	Mean	Standard Deviation
Restricted Use Medicare Data		
White	0.87	
Dual Eligible ($\leq$ 135% FPL)	0.08	
Part D Enrolled	0.43	
Age	75.5	2.9
Monthly Beneficiary Cost-Sharing Liability	105	112
Monthly Payments Made by Medicare	543	717
Total Monthly Medicare Payments	648	818
Number of Chronic Conditions	4.1	
Died before 2018	0.35	
Public-Use SSA Benefits File		
Monthly Benefit Amount	1,246	408
Age at Claiming	63.6	1.63

Notes: Medicare data is computed from 100% Master Beneficiary Summary File 2006 to 2011. The estimation sample consists of men who are born from 1927 to 1937, never-disabled, receiving Social Security benefits on their own wage history, continuously enrolled in Parts A and B, never enrolled in Medicare Advantage, and alive at end of 2011. The final Medicare sample includes 3,287,465 unique beneficiaries. Social Security data is from the 1% Benefits Public-Use File in 2004. All dollar amounts are nominal spending between 2006 and 2011.

Table 3: Log Total Medicare Spending

	(1)	(2)	(3)	(4)	(5)	(6)
Elasticity	-0.928**	-0.903**	-0.928**	-0.928**	-0.979**	-0.911**
	(0.376)	(0.377)	(0.458)	(0.390)	(0.402)	(0.375)
Observations	570	570	570	570	342	456
R^2	0.841	0.842	0.841	0.841	0.849	0.847
Controls		Y				
Treatment Sign	All	All	All	All	Positive	Non-zero
Cluster	DMY	DMY	DM	Jacknife	DMY	DMY

Notes: OLS regressions from equation (3) using a 30 day bandwidth. Each observation is a date-of-birth cell. The dependent variable is log total spending for Medicare Part A and B from 2006 to 2011. Controls include percents white, black, hispanic, and asian. All treatments include all 10 cohorts including placebos. Positive treatments include the six positive income shocks. Non-zero treatments exclude the 2 placebo cohorts. DM denotes day-month level clusters and DMY denotes day-month-year level cluster. Total individuals when using all cohorts is 455,986. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Log Spending By Payer and Category

	Medicare Payments	Cost-Sharing Payments	Total Part A	Total Part B
Elasticity	-0.943**	-0.847***	-0.699	-1.075***
	(0.397)	(0.303)	(0.587)	(0.327)
Observations	570	570	570	570
R^2	0.839	0.831	0.822	0.761

Notes: OLS regressions from equation (3) using a 30 day bandwidth. Medicare payments are Medicare reimbursements directly to providers. Cost-sharing payments are from supplement insurers or beneficiaries to providers.

Table 5: Log Spending / Admission Counts

	Acute Stays	Readmissions	Preventable Admissions	High Quality Hospitals	Low Quality Hospitals
Elasticity	-1.157***	-1.151	-1.774	-1.299	-1.483
	(0.430)	(1.153)	(1.364)	(1.311)	(1.251)
Observations	570	570	570	570	570
R^2	0.841	0.566	0.603	0.603	0.603

Notes: OLS regressions from equation (3) using a 30 day bandwidth.

Table 6: Log Count of Chronic Conditions

	(1)	(2)	(3)	(4)	(5)
Elasticity	-0.396**	-0.377**	-0.396**	-0.296	-0.376**
	(0.185)	(0.173)	(0.179)	(0.185)	(0.174)
Observations	570	570	570	342	456
R^2	0.927	0.928	0.927	0.928	0.928
Controls		Y			
Treatment Sign	All	All	All	Positive	Non-zero
Cluster	DM	DMY	Jacknife	DMY	DMY

Notes: OLS regressions from equation (3) using a 30 day bandwidth. Each observation is a date-of-birth cell. The dependent variable is log count of chronic conditions from the 2011 chronic condition summary file. See main text and notes on Table X for additional details.

Table 7: Log Percentage Dead by End of 2017

	30 Day Bandwidth			CCT Bandwidth Selection		
	(1)	(2)	(3)	(4)	(5)	(6)
Elasticity	-0.982**	-0.988**	-0.951**	-1.071***	-1.091***	-0.979***
	(0.420)	(0.423)	(0.457)	(0.309)	(0.309)	(0.337)
Intercept			-0.136			-0.408
			(0.868)			(0.667)
Observations	570	570	570	936	936	936
R^2	0.968	0.968	0.968	0.966	0.966	0.966
Race Controls		Y			Y	

Notes: Columns (1, 2, 4, 5) report regressions from equation (3) and columns (3, 6) report regressions equation (4). Columns (1-3) use a uniform 30 day bandwidth for all cohorts and columns (4-6) use separate CCT bandwidths for each cohort. The dependent variable is the log fraction of the baseline sample alive at the end of 2011 that has died by the end of 2017.

Table 8: Fiscal Costs

Source of Change	Cost as % of Original Social Security Benefits
Per-capita Social Security Benefits	1.00
Per-capita Medicare Benefits	-0.37
Total Social Security Beneficiaries	0.19
Total Medicare Beneficiaries	0.09
Net Change	0.91

Appendices

These appendices provide additional results and details. Appendix A presents robustness checks for the empirical strategy and provides econometric details. Appendix B shows descriptive results from other data sources on the correlation between income and mortality and income and health expenditures. Appendix C shows how to derive the income discontinuities from the Social Security benefit formula. Appendix D describes a model of health capital to provide intuition for the results.

A Robustness and Additional Results

A.1 Stacked Regression Discontinuity in Difference

To test the sensitivity of the results, I also consider a specification that builds off the stacked RD design with an added feature from difference-in-discontinuities designs. Intuitively, a difference-in-discontinuities is estimating the change between two distinct regression discontinuity estimates over time. A standard difference-in-difference compares outcomes between a treatment and a control group before versus after a policy change. A difference-in-discontinuities makes the same comparison except treatment and control groups are defined within a narrow bandwidth on either side of a cutoff (Duggan, Gupta and Jackson, 2019; Persson, 2020).

In my setting, the discontinuities are differenced with respect to the discontinuities for the placebo years (1928 and 1934). In the context of a stacked RD, we can achieve this by interacting the January 2 dummy D_i with a vector of 0.01.

$$\log(Y_i) = \beta_0(D_i * 0.01) + \beta_1(D_i * S_c) + \sum_{c \in C} \beta_{2,c} DOB_i + \sum_{c \in C} \beta_{3,c}(DOB_i * D_i) + \beta_c + e_i \qquad (4)$$

Now, β_1 measures the spending elasticity relative to the placebo cohorts. This is equivalent to plotting the coefficients against the income shock and allowing the intercept to vary. An intercept (β_0) statistically different from zero would provide evidence that the classic RD identification assumptions do not hold. Appendix Table 6 shows for all major categories of spending the intercept is near zero, and elasticities are similar to the stacked RD design.

A.2 Stacked Regression Discontinuity by Sample Year

In the primary specification, the unit of observation is a date-of-birth cell and the dependent variable is the log sum of Medicare nominal spending over 6 years. Because Medicare prices are readjusted every year, there is some risk that the outcome is distorted by inflation in medical costs over time. To account for this, I consider an alternate specification where the unit of observation is a (date of birth) by (sample year) cell. This allows for the inclusion of sample year fixed effects which absorbs changes in prices over time.

$$\log(Y_{it}) = \beta_1(D_{it} * S_c) + \sum_{c \in C} \beta_{2,c} DOB_{it} + \sum_{c \in C} \beta_{3,c}(DOB_{it} * D_{it}) + \beta_c + \gamma_t + e_{it} \tag{5}$$

Column (1) in Appendix Table 8 presents estimates from this regression. Column (2) allows for cohort by sample fixed effects and column (3) interacts the slopes with the sample year to allow for different trends within cohort over time. In all cases, the elasticity is unchanged. This suggests that price changes within the sample period are not a concern.

A.3 Bandwidth Selection

In my preferred specification, I use a 30-day bandwidth for all cohorts. Using a narrow, consistent sample has the advantage of avoiding seasonality concerns, and allowing for clean decomposition of total spending by category. Nevertheless, it may be too narrow for certain cohorts. In particular, cohorts with fewer observations or higher variance of spending may require longer bandwidths. To account for this, I use the procedure developed by Calonico, Cattaneo and Titiunik (2014) to select bandwidths separately for each cohort, and then estimate equation (3) in the main text using these cohort-specific bandwidths.[38] Appendix Table 9 presents the results. In general, bandwidths are larger which generates smaller standard errors and smaller point estimates, but the qualitative results are unchanged. We cannot reject these are different from the estimates in the main text.

[38] More recent work by Cattaneo et al. (2016) describes how to interpret regression discontinuity designs with multiple cumulative or non-cumulative cutoffs, but they do not consider a pooling approach when treatments have different signs.

B Income Correlations

B.1 Evidence on Expenditures from Survey Data

In order to give the causal estimates context, it is useful to explore the correlations of income on health expenditures and health outcomes. I rely on survey data because income is not directly observed in the administrative data. Although questions about health expenditures are included in many surveys, accurate measurement is challenging. Definitions of “health expenditures” are not always consistent, there are often multiple payers, and prices are rarely transparent. The Medical Expenditure Panel Survey (MEPS) attempts to minimize these sources of error by linking interviews from households to billing data from their medical providers. Merging the two data sources also allows MEPS to decompose expenditures made by payer.

I consider a sample of males receiving Social Security benefits between ages 65 and 84. Appendix Table 11 presents OLS results with log annual payments for health care as the dependent variable. This excludes premium payments for insurance plans. I calculate spending elasticities with respect to total income for different payer sources. All models include survey year fixed effects, and a quadratic age term. Demographic controls include dummies for race, ethnicity, education, martial status, and census region. Columns (1-2) show the result across all payers, (3-4) show out-of-pocket payments, and (5-6) show payments made by Medicare on beneficiaries’ behalf.

Two patterns emerge. First, demographic controls reduce the coefficient. This suggests that simple bivariate correlations between income and utilization have an upward bias. Second, price exposure matters. Elasticities are positive for out-of-pocket payments not covered by insurance, while elasticities are near zero for total payments across all payers. This makes sense under a model of full insurance. If consumers do not face prices at point of service, then income changes should not directly affect their willingness to use medical services. Expenditures covered by Medicare are almost fully insured because consumers have most of their cost-sharing covered by supplement plans. Given that a large share of Medicare payments are for hospital visits and other less “discretionary” services, the negative coefficient suggests improvements in health. Under this view, income buys more unobserved health investment which leads to better health. Better health reduces the need for Medicare services which are more curative than preventive. Other empirical research in a setting of near full insurance finds similar results. In the RAND Health Insurance Experiment, Phelps (1992) calculated income elasticities of 0.2 or less.

B.2 Evidence on Mortality from Administrative Data

Chetty et al. (2016) provides the best data on the correlation between total income and mortality. They use administrative tax records linked to Social Security mortality data for all individuals with a valid Social Security Number from 1999 to 2014. Their main results are for the full population, but in Online Table 15 they present summary data disaggregated by age. For the elderly, real income is measured at age 61 and mortality is measured as an average annual rate. Although Social Security benefits are computed over lifetime earnings, earnings at 61 provides a reasonable proxy.

Appendix Figure 10 plots the relationship between log income and the log average mortality rate for adults aged over 65. The cross sectional elasticity of mortality with respect to total income (measured as the slope of the OLS fit line) is equal to -0.41. Because the Social Security formula has higher replacement rates for low-income workers, the elasticity with respect to Social Security income would be more negative (larger in absolute value).

C More Detail on Social Security Rules and Data

The benefit discontinuities in this paper arise from the use of wage and price deflators in the Social Security benefit formula. Policymakers have struggled to implement these adjustments in a consistent way throughout the history of the program. The original 1935 Social Security Act had no wage or benefit indexation, and all cost-of-living adjustments required Congress to pass new legislation. Under this regime, the real value of benefits would erode over several years then abruptly increase when Congress intervened.[39] Policymakers attempted to automate this process in the 1972 Social Security Act, but made a technical error in the indexation formula. This led benefits to increase at nearly double the rate of inflation. In 1977, Congress created the modern benefit formula to correct this error, but did so in a way that abruptly cut benefits for individuals born on or after January 2, 1917 – a benefit discontinuity known as "the Notch."

Under the modern formula, workers and their spouse (either current or divorced) are eligible for Social Security benefits if the worker has at least 10 years of creditable labor market earnings. A creditable year is defined as earning above a certain inflation-indexed threshold ($4,480 for 2010).

C.1 Formula in Detail

Suppose individual i is born in calendar year b with taxable nominal earnings n_{it} in year t. SSA computes the average wage index (AWI_t) using IRS administrative data. This is used as an earnings deflator where indexed earnings y_{it} are defined as

$$y_{it} = \begin{cases} n_{it} \cdot \frac{AWI_{b+60}}{AWI_t} & t \leq b + 60 \\ n_{it} & t > b + 60 \end{cases}$$

so that earnings after the year an individual turns 60 are not adjusted. Indexed earnings are ordered such that $y_{i(1)} < y_{i(2)} \cdots < y_{i(n)}$ where $y_{i(1)}$ denotes the minimum indexed earnings in a person's wage history and $y_{i(n)}$ denotes the maximum indexed earnings. Average Indexed Monthly Earnings ($AIME_i$) is calculated as the average of the highest 35 years of indexed annual earnings divided by 12 months

$$AIME_i = \frac{1}{12} \left(\frac{1}{35} \sum_{j=0}^{34} y_{i(n-j)} \right)$$

[39] The lump-sum death benefit is the only feature of the original Social Security program which still exists today but has not been indexed to inflation. It was designed to help families pay for burial expenses after a worker's death. Congress set the maximum benefit at $255 in 1954 and has not updated it since.

Next, the Principal Insurance Amount (PIA_i) is computed as a concave function of $AIME_i$. The progressive formula has a marginal replacement rate that starts at 90%, declines to 32%, and reaches a minimum of 15% for the highest earners

$$PIA_i = \begin{cases} 0.9 \cdot AIME_i & AIME_i < k_{1,b} \\ 0.9 \cdot k_{1,b} + 0.32 \cdot (AIME_i - k_{1,b}) & k_{1,b} < AIME_i < k_{2,b} \\ 0.9 \cdot k_{1,b} + 0.32 \cdot (k_{2,b} - k_{1,b}) + 0.15 \cdot (AIME_i - k_{2,b}) & AIME_i > k_{2,b} \end{cases}$$

The kinks in the benefit schedule vary according to any individual's birth year where

$$k_{1,b} = 180 \cdot \frac{AWI_{b+60}}{AWI_{1977}} \qquad \text{and} \qquad k_{2,b} = 1085 \cdot \frac{AWI_{b+60}}{AWI_{1977}}$$

There is a cost-of-living adjustment ($COLA_t$) which increases benefits by a fixed percentage every December

$$COLA_t = \max\left(\frac{CPI_t}{\max\limits_{\tau<t} CPI_\tau}, 1\right)$$

where CPI_t denotes the mean of CPI-W in the third quarter of the year. This formula is designed to protect beneficiaries against deflation. When the level of the CPI index declines (as occurred in 2009 and 2010) nominal benefits levels stay constant. This protection, however, is "paid for" since later CPI increases are lower. Once the CPI starts to increase again, the base year for computing the percentage is the last highest year, not the previous year. Finally, the benefit is adjusted by the delayed retirement credit (DRC_b) which reduces the benefit for those who retire before the full retirement age (FRA_b), or increases it for those who claim after the FRA. These are indexed by b since they vary by year of birth as shown in Appendix Table 12. The amount credited in year t can be summarized as

$$Benefit_{it} = PIA_i \cdot DRC_b \cdot \prod_{j=b+62}^{t} COLA_j$$

Consider two workers with identical earnings histories who claim benefits at their full retirement age of 65 ($DRC_b = 1$). One worker is born in December of year $b-1$ and the other is born one month later in January of year b. To simplify the calculation, we assume that the highest indexed earnings occur before age 60 and that the same years are included in the maximum 35 year average.

Under these assumptions we can express

$$\frac{AIME_{Jan}}{AIME_{Dec}} = \frac{AWI_{b+60}}{AWI_{b+59}}$$

Because the kinks in the PIA formula also indexed by AWI we can similarly write

$$\frac{PIA_{Jan}}{PIA_{Dec}} = \frac{AWI_{b+60}}{AWI_{b+59}}$$

Regardless of when an individual claims, a cost-of-living adjustment (COLA) is applied in the calendar year after the year of first eligibility when they turn 62. Thus, benefits credited in the month after both individuals have claimed are

$$\frac{Benefit_{Jan}}{Benefit_{Dec}} = \frac{PIA_{Jan} \cdot COLA_{b+62} \cdot COLA_{b+63} \cdot COLA_{b+64}}{PIA_{Dec} \cdot COLA_{b+61} \cdot COLA_{b+62} \cdot COLA_{b+63} \cdot COLA_{b+64}}$$

Assuming that inflation is positive this simplifies to

$$\frac{Benefit_{Jan}}{Benefit_{Dec}} = \frac{AWI_{b+60} \cdot CPI_{b+60}}{AWI_{b+59} \cdot CPI_{b+61}}$$

Then, taking log differences we can express the percentage change in benefits as

$$\%\Delta Benefits \approx \%\Delta AWI_{b+60} - \%\Delta CPI_{b+61}$$

In practice, the impact of the change in base years will be somewhat difference since earnings after 60 are not indexed, and indexation may change which the years which are included in the maximum 35 years.

Appendix Figure 1 depicts how changes in parameters of the benefit formula contribute to the net change in benefits. Each calculation assumes nominal wage are identical on either side of birth date cutoff. Wage growth is positive in every year except for 2009 (birth cohort 1949) when it declined for the first time. Growth in benefits from the CPI is constrained by law to be positive. For beneficiaries born from 1938 to 1943 and 1954 to 1960, the net effect is 1.1 percentage points lower due to the rising retirement age.

C.2 Changes in the Delayed Retirement Credit

Appendix Table 12 shows that half of the cohorts in the sample are affected by a 0.5% change in the delayed retirement credit (DRC). Although these changes violate the assumption that other policies change smoothly around the January 2 cutoff, they are small enough to be ignored. According to SSA Statistical Supplement 2007, Table 6.B5, only 3.6% of men are affected which means the change in real benefits across the whole cohort is less than 0.05%. I exclude disability conversions from the denominator. Figure 1 provides further evidence the DRC changes can be ignored. It shows there is not evidence of changes in claiming behavior.

C.3 Disability Benefits

Although beneficiaries who previously received Social Security disability payments use the same benefit formula, the details of its application differ. Both the number of years in the average and the base year are selected to maximize their PIA computation. The base year is either two years before the onset of their disability, or the normal base year at 60. A further complication is there is substantial bunching in the onset date of disability (29 months before the start of Medicare coverage). Appendix Figure 9 uses the 2017 enrollment file to plot the number of Medicare beneficiaries who have ever been disabled by the date of disability onset. For clarity, the label next to each data point denotes the month of the onset. This bunching is due to an SSA policy that sometimes allows disability examiners to a select on an onset date that results in a more favorable benefit.[40] For this reason, the benefit discontinuities cannot be applied for disability beneficiaries.

[40] POMS DI 25501.300

D A Model of Income and Health Spending

The standard framework for studying the demand for health and medical care is the Grossman (1972) model of health capital. In this model, individuals face a tradeoff between health and consumption of other commodities. Health is a durable stock variable that increases due to health investment or declines due to depreciation. The model makes a clear prediction that increased income will result in higher health investment and improved health outcomes.

A major challenge to testing this prediction in data is defining health investment. In the model, health investment is a single input of health goods and services sold in the market. This input includes everything from emergency department visits and vaccines to good nutrition and a low-stress lifestyle. As Kaestner (2013) notes, aggregating these inputs into a single index of health investment overlooks potential substitution possibilities between inputs. For example, preventive investments in healthy diet or exercise can substitute for medical investments in managing chronic disease.

A useful distinction to make is between ex-ante and ex-post health spending. Ex-ante spending can be broadly defined to include investments in health such as exercise, good nutrition, a low-pollution environment, or preventive care. Ex-post health spending can be narrowly defined to include acute medical care that mitigates the health loss due to a current illness. Because health insurance is more likely to cover ex-post spending than ex-ante spending, an income shock is will affect these two categories differently.

To account for these features, I follow Grossman (2000) and Kaestner (2013) to develop a health capital model that distinguishes between ex-ante and ex-post health investment. Suppose that agents live for two-periods and have preferences over health and other consumption. In the first period, they inherit a health stock, h_1, and tradeoff between consumption, c_1, and how much to invest in future health, i_1. In the second period utility is discounted by β. Consumers are either sick or healthy and the probability of becoming sick, $\rho(i_1)$, declines with the level of investment such that $\rho'(i_1) < 0$. Consumers maximize expected utility

$$U(c_1, h_1) + \beta\rho(i_1)U(c_{2s}, h_{2s}) + \beta(1 - \rho(i_1))U(c_{2h}, h_{2h}) \tag{6}$$

If the consumer remains healthy, the stock of health evolves according to the standard law of motion.

$$h_{2h} = h_1(1 - \delta) + f(i_1) \tag{7}$$

If the consumer gets sick, they decide how much to spend on medical care, m_2. The additional loss to their health stock is denoted by $\lambda(m_2) > 0$ with $\lambda'(m_2) < 0$.

$$h_{2s} = h_1(1 - \delta - \lambda(m_2)) + f(i_1) \tag{8}$$

Income, y, is fixed and medical care is never consumed if a person is healthy. Prices for investment inputs and medical care are, p_i and, p_m, respectively. If there is no saving or borrowing, the budget constraints can be summarized as

$$y = c_1 + p_i i_1 \tag{9}$$

$$y = c_{2h} \tag{10}$$

$$y = c_{2s} + p_m m_2 \tag{11}$$

To summarize, agents have preferences over health and other consumption $U(c, h)$ with two periods and two states of health.

- At $t = 1$ everyone is healthy and inherits health stock h_1
  - They decide consumption and investment (c_1, i_1)
- At $t = 2$ health shock is realized
  - If they remain healthy, there is no need for medical care
    * $h_{2h} = h_1(1 - \delta) + f(i_1)$
    * $c_{2h} = y$
  - If they become sick, they consume medical care to mitigate the loss in health capital
    * Loss to health is $\lambda(m_2) > 0$ with $\lambda'(m_2) < 0$
    * $h_{2h} = h_1(1 - \delta - \lambda(m_2)) + f(i_1)$
    * $c_{2h} = y - p_m m_2$

On average, health status is $\rho(i_1)h_{2s} + (1 - \rho(i_1))h_{2h}$ and medical spending is $\rho(i_1)m_2$. Because consumption decisions in the second period are made after the shock is realized, we solve the problem by backward induction. We first maximize $U(c_{2s}, h_{2s})$ subject to the constraints in equations (3) and (6) to solve for the optimal consumption bundles as a function of first period investment

$c^*_{2s}(i_1)$ and $h^*_{2s}(i_1)$. We then plug these expressions into equation (1) and maximize subject to the constraints in equations (2), (4), and (5).

The first order conditions are complicated and do not provide useful intuition, so instead I rely on simulations to study how income shocks affect medical spending and health. In particular, I follow Koka et al. (2014) and make the following assumptions regarding functional forms:

$$U(c, h) = c^{\alpha} h^{1-\alpha}$$

$$f(i_1) = i_1^{\gamma}$$

$$\lambda(m_2) = 1 - k_m m_2$$

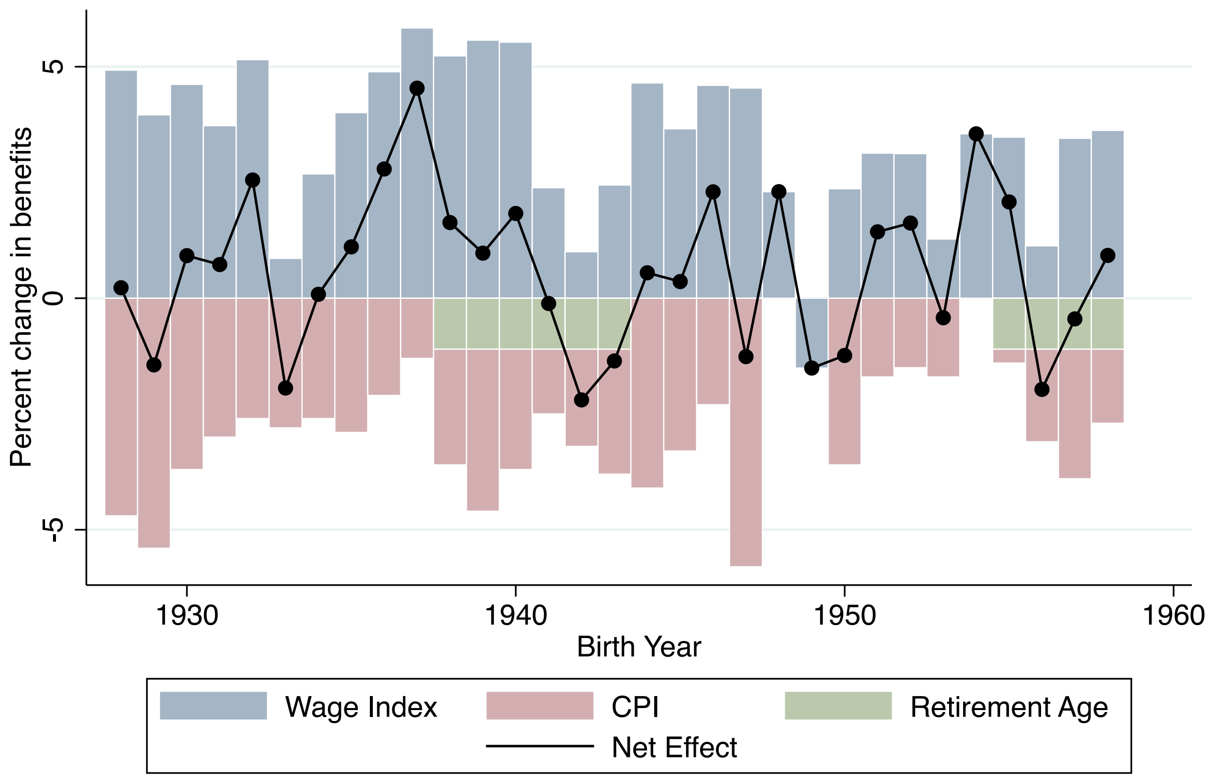
$$\rho(i) = exp(k_i i)$$

and the following assumptions for parameters:

α	γ	δ	p_i	p_m	h_0	k_m
0.6	0.8	0.1	1	1	1	1.5

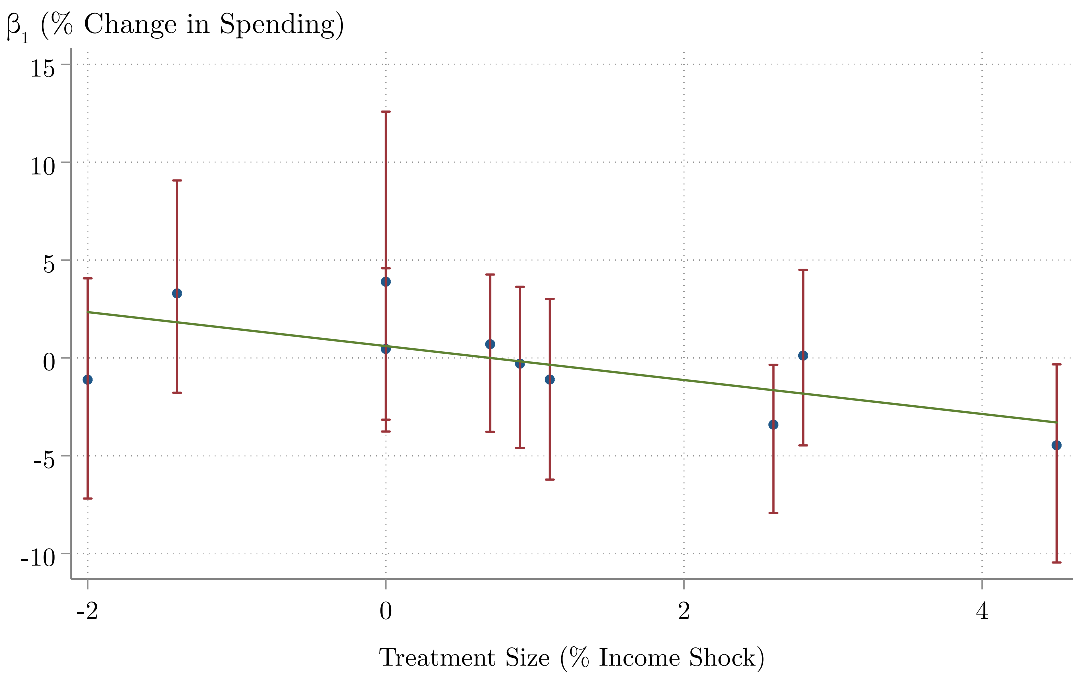
With most of the parameters fixed, I can solve the model numerically and test the relationship between expected medical spending, health investment, and income. In particular, I am interested in k_i, the parameter which affects how much ex-ante investment reduces the probability of illness. I consider two cases: high efficiency of health investment $k_i = -1$, and low efficiency of health investment $k_i = -4$. Appendix Figures 11 and 12 depict these relationships. As in the standard Grossman model, income and health investment are positively related because health is a normal good. In contrast, the relationship between income and medical spending is ambiguous. On one hand, income will increase ex-ante investment which will reduce the probability of illness. On the other hand, if the consumer does get sick, more income will reduce price sensitivity to acute care spending and so medical spending will increase. My empirical results find medical spending declines and health improves matching the simulation with a high efficiency of health investment.

Appendix Figure 1: Parameter Changes for all Benefit Discontinuities



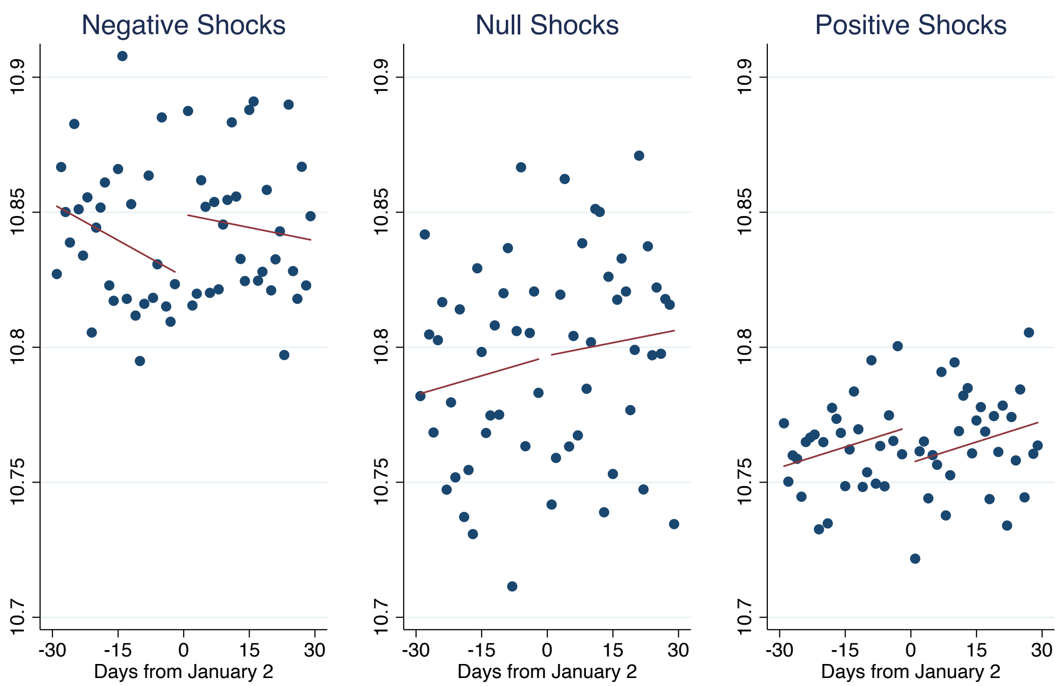
Notes: Benefit discontinuities occur because the Average Wage Index (AWI) and the Consumer Price Index (CPI) grow at different rates. The net effect in the figure is the change in benefits for a person born in January relative to a person born in December with the same nominal wage history. By law, CPI changes can only be positive. For beneficiaries born from 1938 to 1943, the net effect is lower due to the rising retirement age.

Appendix Figure 2: Non-Parametric Spending Effects for Each Cohort



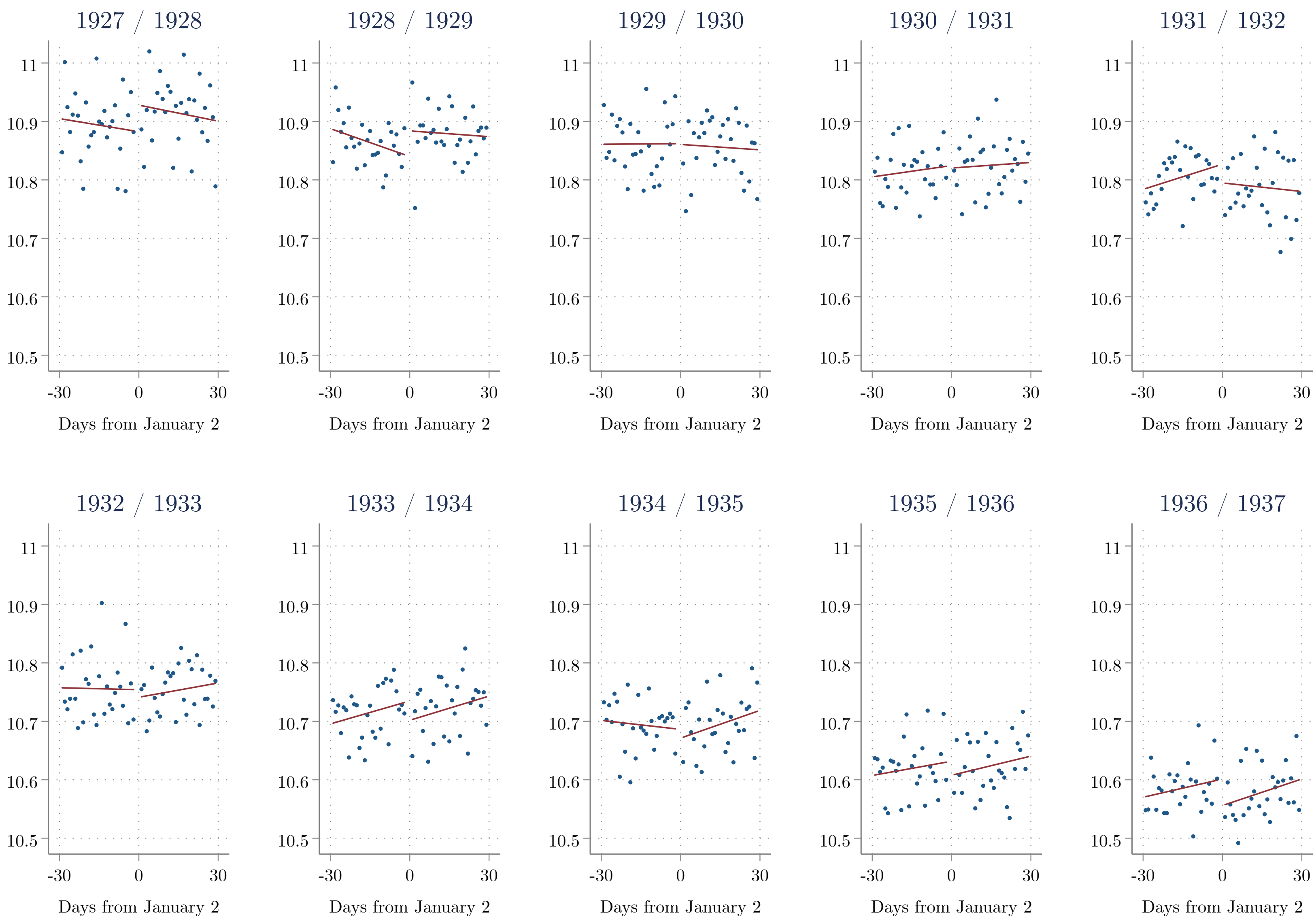
Notes: Each observation is the β_1 coefficient from equation (1) with its associated confidence interval. The x-axis denotes the predicted difference implied by equation (1).

Appendix Figure 3: Log Total Medicare Spending by Treatment Type

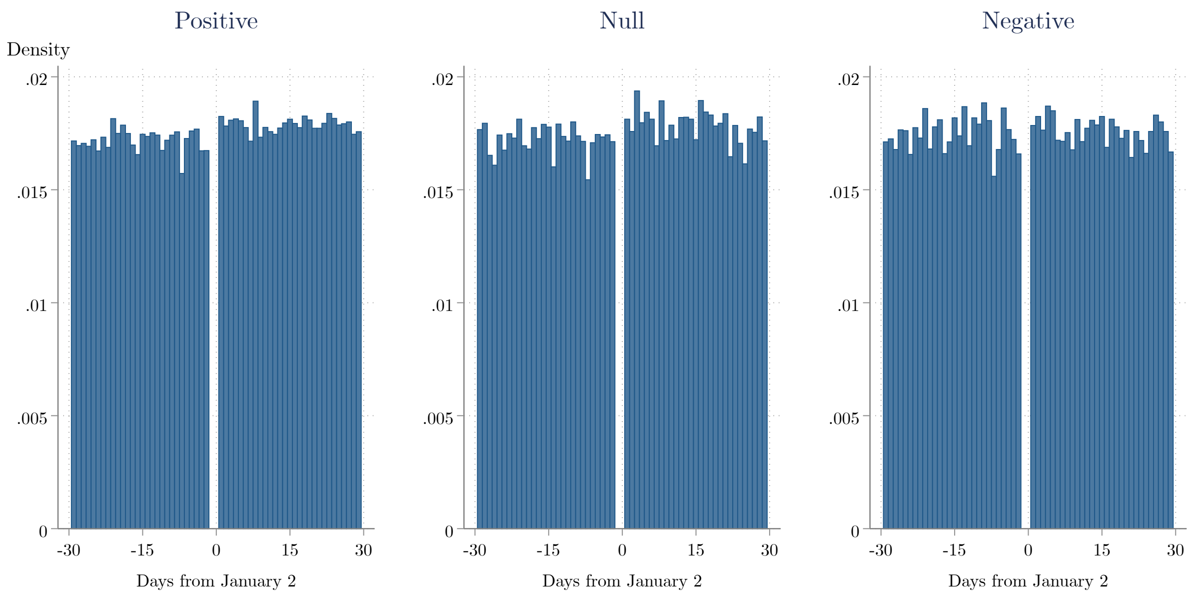


Notes: The outcome is total log Medicare Part payments. See notes on Figure 3.

Appendix Figure 4: Log Total Medicare Spending by Each Cohort

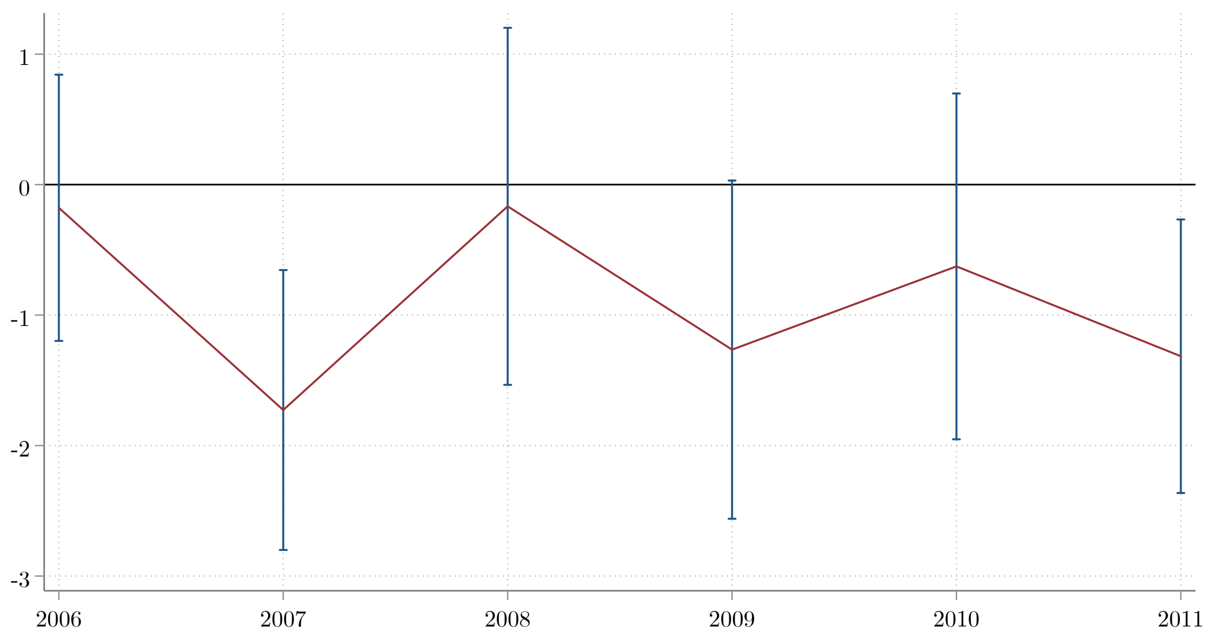


Appendix Figure 5: Histogram by Treatment Type



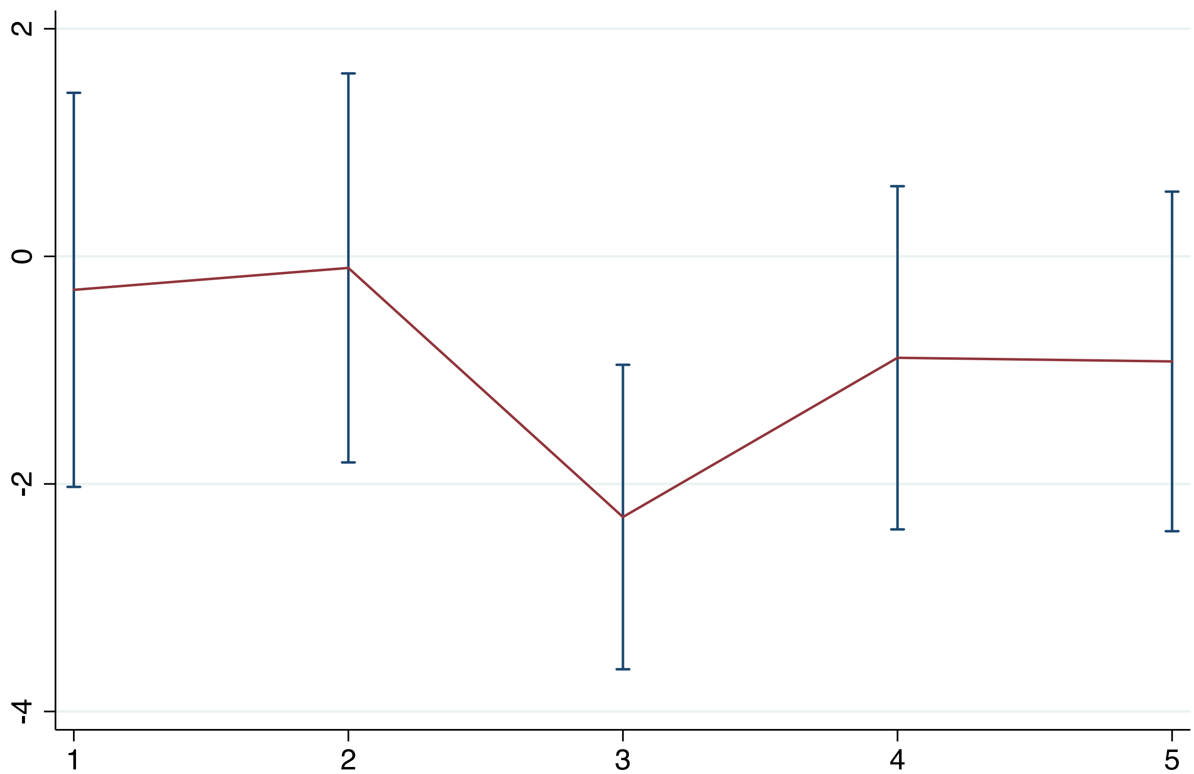
Notes: Density of observations around 30 days from the cutoff in the estimation sample, with January 1 and January 2 birth dates dropped. For all cohorts, there is a decline in reported births on December 26, the day following Christmas.

Appendix Figure 6: Spending Elasticities over Time



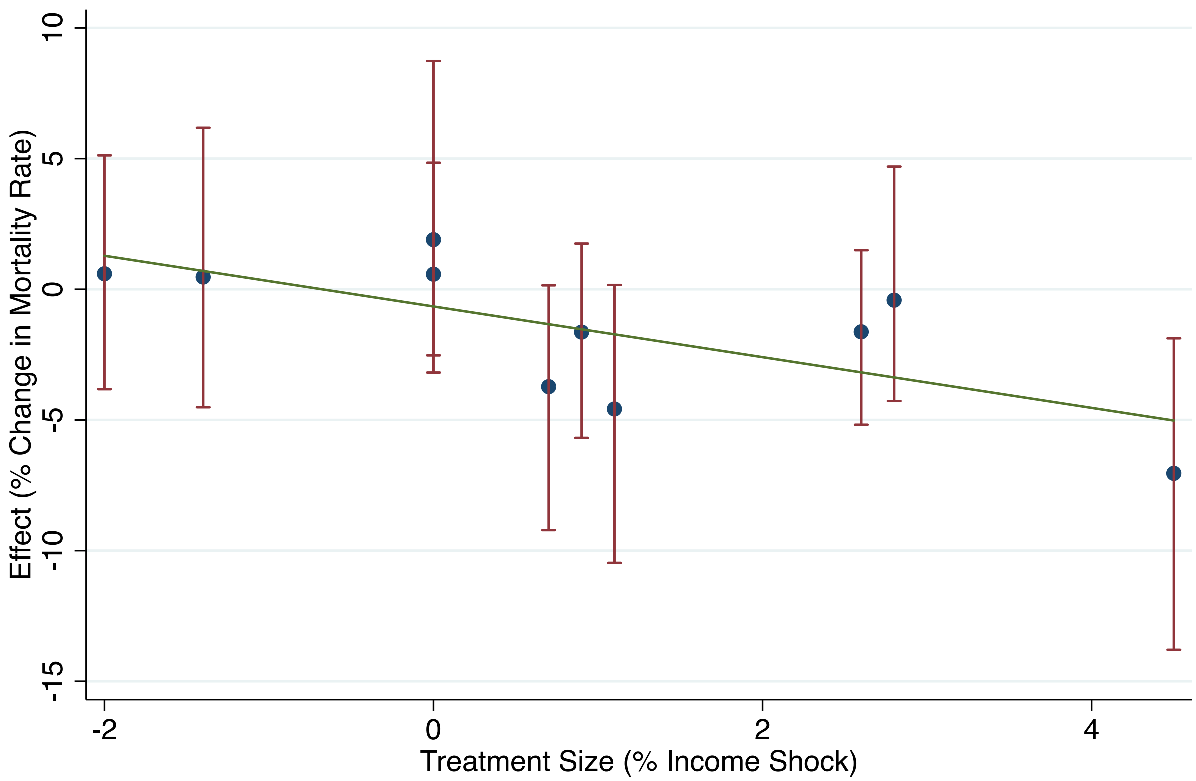
Notes: Elasticities of spending are computed by estimating equation (3). The dependent variable is log total spending for Medicare Part A and B for a given year. Although there is a slight downward slope, there is no clear pattern.

Appendix Figure 7: Spending Elasticities by Zipcode Quintile



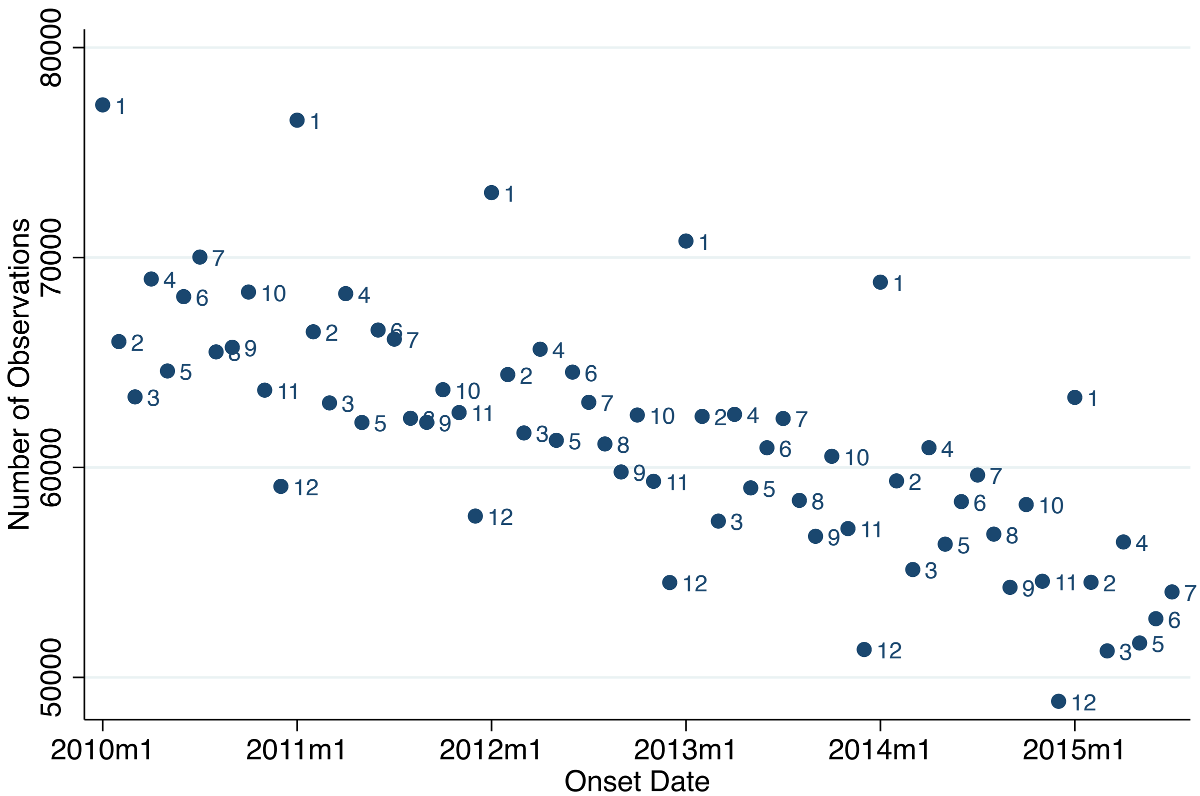
Notes: Elasticities of spending are computed by estimating equation (3) where the sample is divided into 5 zipcode quintiles. The dependent variable is log total spending within a zipcode quintile.

Appendix Figure 8: Non-Parametric Mortality Effects for Each Cohort



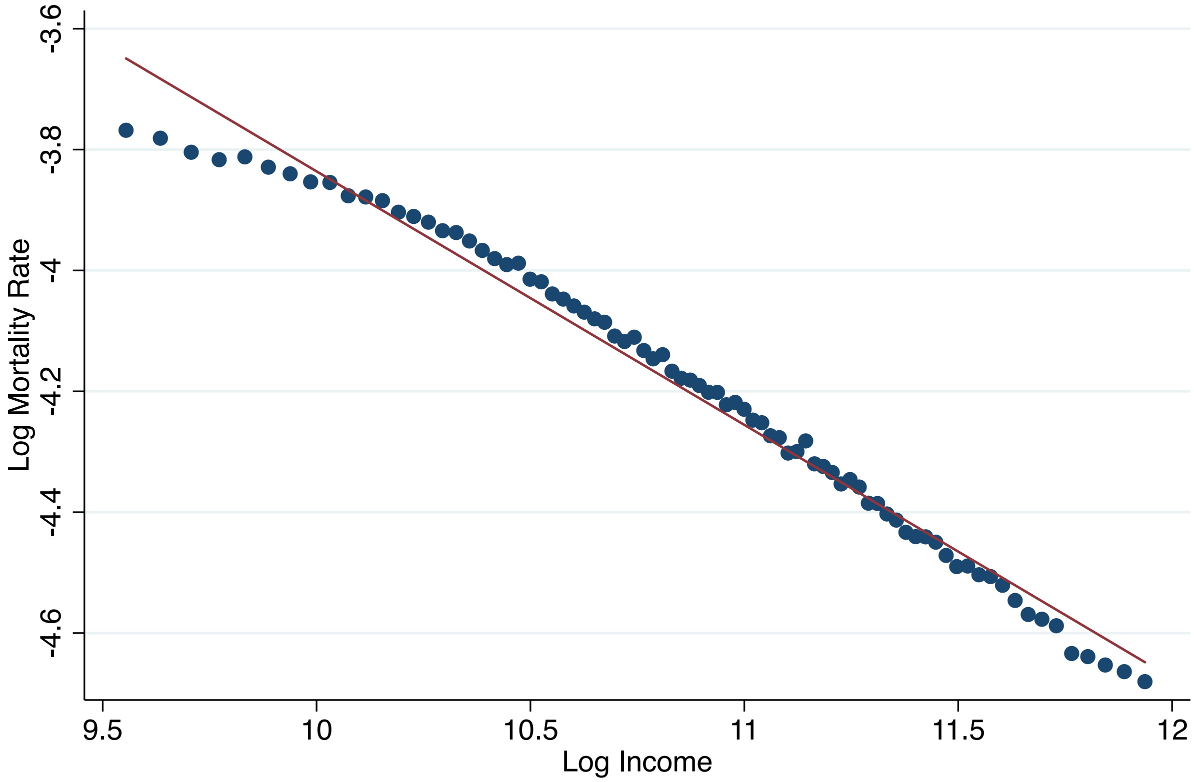
Notes: Each observation is the β_1 coefficient from equation (2) for a given cohort where the dependent variable is the log fraction of the baseline sample that has died by the end of 2017. Bandwidth selection and confidence intervals are computed using CCT procedure.

Appendix Figure 9: Evidence of Bunching Among Disability Beneficiaries



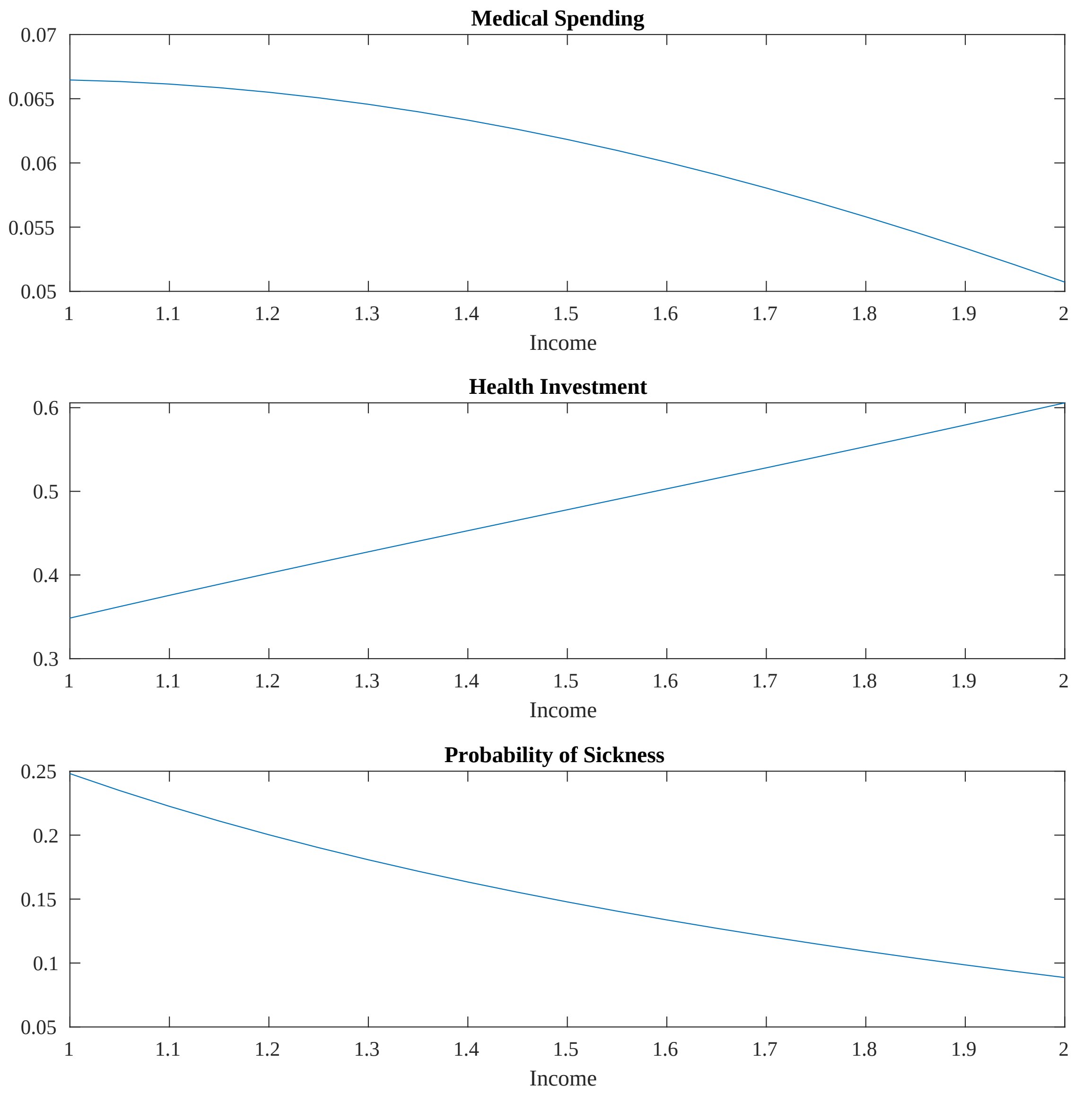
Notes: The figure depicts counts of observations by disability onset date using the 2017 enrollment file. Labels denote month of onset. The bunching is due to an SSA rule which allows flexibility in the date of disability onset if it is advantageous to the beneficiary. Because of this, the benefit formula discontinuities cannot be used for disability beneficiaries.

Appendix Figure 10: Elasticities of Mortality with Respect to Total Income

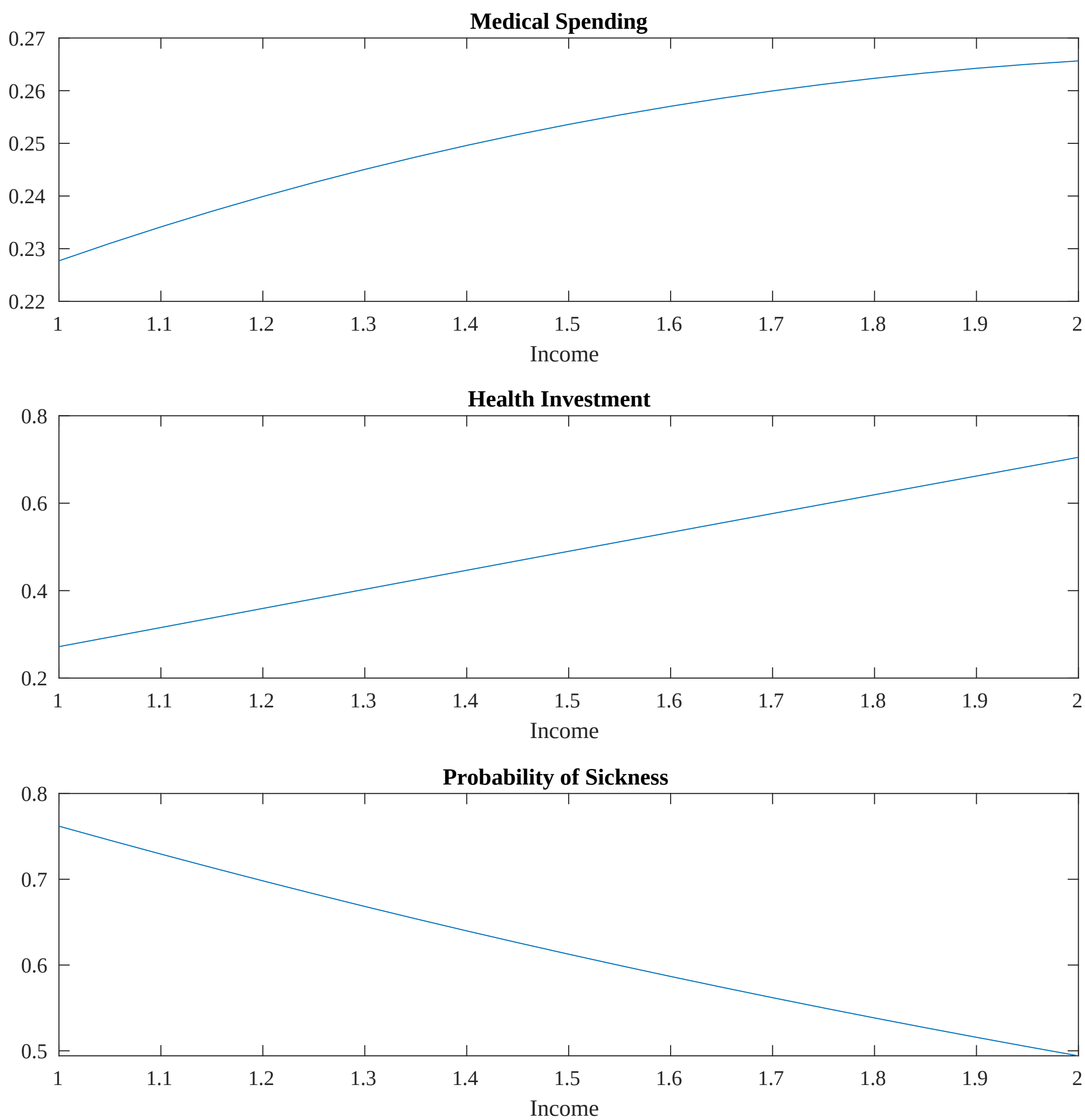


Notes: Summary data provided by Chetty et al. (2016) for adults over 65. Income measured at age 61 and mortality measured from 1999 to 2014. The slope of the OLS fit line is equal to -0.41.

Appendix Figure 11: High Efficiency: $k_i = -1$



Appendix Figure 12: Low Efficiency: $k_i = -4$



Appendix Table 1: Log Share of Population

	Estimination Sample	Part A/B	FFS	Black	Hispanic
Elasticity	-0.30	-0.00	-0.25	-0.02	0.92
	(0.19)	(0.06)	(0.16)	(0.99)	(0.99)
Observations	570	570	570	570	570
R^2	0.688	0.927	0.214	0.416	0.226
Outcome Population Share	60.91	93.09	66.61	6.40	7.14

Notes: This table estimates the elasticity of enrollment outcomes with respect to benefit shocks using equation (3). The elasticity can be interpreted as the effect of 1% change in benefits on the % share of the population with an income. The sample is similar to the estimation sample, except without restrictions on enrollment in Part A, Part B, or Medicare Advantage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 2: Log Share of Population

	Dual Eligible	Part D Enrolled	Any Subsidy
Elasticity	0.336	-0.318	0.428
	(1.077)	(0.322)	(1.073)
Observations	570	570	570
R^2	0.127	0.360	0.125
Outcome Population Share	7.7	42.3	7.8

Notes: This table estimates the elasticity of enrollment outcomes with respect to benefit shocks using equation (3). The sample is the baseline estimation sample.

Appendix Table 3: Log Observation Count Within Date-of-Birth Cell

	Enrollment Sample		Estimination Sample	
	(1)	(2)	(3)	(4)
Elasticity	1.39***	0.40	1.09***	0.21
	(4.28)	(1.05)	(2.97)	(0.49)
Constant		4.66***		4.15***
		(5.33)		(4.19)
Observations	570	570	570	570
R^2	0.954	0.957	0.920	0.923

Notes: This table tests for possible manipulation of the running variable (date-of-birth). Columns (1) and (3) use equation (3) from the main text, and columns (2) and (4) use a regression discontinuity-in-difference specification from the Appendix. The enrollment sample is similar to the estimation sample, except without restrictions on enrollment in Part A, Part B, or Medicare Advantage.

Appendix Table 4: Specification Tests

	(1) Baseline	(2) Baseline w/ Controls	(3) Global Quadratic	(4) Cohort Quadratic
Elasticity	-0.928**	-0.903**	-1.112***	-0.971*
	(0.376)	(0.377)	(0.402)	(0.581)
Observations	570	570	570	570
R^2	0.841	0.842	0.842	0.848
AIC	-1860.1	-1856.3	-1858.2	-1844.5
BIC	-1725.3	-1704.2	-1714.8	-1622.9

Notes: This table compares the fit of various specifications. The global quadratic specification is equation (3) with distance from cutoff squared and its interaction terms included. Cohort-specific quadratic allow these two terms to be estimated separately for each cohort.

Appendix Table 5: Individual and Aggregate Regressions on Levels

	(1)	(2)	(3)	(4)	(5)
Scaled Dummy	-399.0**	-399.0**	-399.0**	-399.0**	-399.0**
	(161.1)	(192.0)	(155.7)	(197.2)	(160.0)
Observations	455968	455968	455968	570	570
Cluster	Robust	Cutoff distance	Date of birth	Cutoff distance	Robust

Notes: OLS regressions from equation (3) with spending level as the dependent variable. The estimate is the effect a 1% change in benefits on total Medicare outlays over 6 years. Columns (1-3) are regressions from individual level micro data. Columns (4-5) are collapsed data at the date-of-birth level.

Appendix Table 6: Log Expenditures by Payer using Difference in Discontinuity Design

	Total	Medicare	Beneficary	Part A	Part B
Intercept	-0.197	-0.212	-0.136	-0.512	0.010
	(0.941)	(0.981)	(0.793)	(1.393)	(0.869)
Elasticity	-0.884**	-0.896*	-0.816**	-0.584	-1.077***
	(0.436)	(0.457)	(0.359)	(0.661)	(0.383)
Observations	570	570	570	570	570
R^2	0.841	0.839	0.831	0.822	0.761

Notes: The table estimates the elasticity of spending using Appendix equation (4). The intercept term denotes the percentage change in spending for placebo cohorts.

Appendix Table 7: Log Medicare Expenditures with Heterogeneity by Age

	(1)	(2)	(3)	(4)	(5)	(6)
Elasticity	-0.880**	-0.884**	-0.968**	-0.874**	-1.022	-0.846**
	(0.355)	(0.367)	(0.386)	(0.358)	(0.708)	(0.400)
Age Interaction		-0.003				
		(0.048)				
Cohort Interaction			0.067			
			(0.145)			
Sample Interaction				0.012		
				(0.051)		
Observations	3420	3420	3420	3420	1881	1539
R^2	0.905	0.905	0.905	0.905	0.846	0.846
Sample	Baseline	Baseline	Baseline	Baseline	Above 75	75 and below

Notes: The table estimates the elasticity of spending using variations on the specification described in Appendix equation (5). The unit of observation is a (date of birth) by (sample year) cell. Column (2-4) include an income elasticity by time interaction. Columns (5) and (6) consider different samples after and below 75.

Appendix Table 8: Log Medicare Expenditures with Flexible Slopes

	(1)	(2)	(3)
income_shock	-0.880**	-0.880**	-0.880**
	(0.355)	(0.357)	(0.362)
Observations	3420	3420	3420
R^2	0.905	0.908	0.909
Fixed Effect Level	Cohort and Sample	Cohort by Sample	Cohort and Sample
Slope Level	Cohort	Cohort	Cohort by Sample

Notes: The table estimates the elasticity of spending using variations on the specification described in Appendix equation (5). The unit of observation is a (date of birth) by (sample year) cell. Column (1) constrains the slope for each cohort to be equal across sample years. Column (2) keeps the slopes constraint but allows for individual cohort-sample fixed effects. Column (3) allows different slopes and fixed effects for each of the 60 cohort-sample year combinations. Errors are clustered at the date of birth level.

Appendix Table 9: Log Expenditures by Payer using Separate Cohort Bandwidths

	Total	Medicare	Beneficary	Part A	Part B
Elasticity	-0.579**	-0.643**	-0.501**	-0.674	-0.604**
	(0.280)	(0.290)	(0.253)	(0.417)	(0.271)
Observations	1132	1150	1004	1214	996
R^2	0.831	0.831	0.806	0.827	0.732
Average CCT Bandwidth	57.47	58.48	51.54	61.69	50.96

Notes: Equation (3) where bandwidths are selected using the CCT procedure.

Appendix Table 10: Log Total Expenditures with Dates of Birth Dropped

	(1)	(2)	(3)	(4)
Elasticity	-0.928**	-0.841**	-0.783**	-0.982**
	(0.376)	(0.366)	(0.338)	(0.390)
Observations	570	580	590	560
R^2	0.841	0.842	0.843	0.840
Dropped Days	Jan 1, Jan 2	Jan 1	None	Dec 26, Jan 1, Jan 2

Notes: The table shows the results are not sensitive to dropping or including particular birth dates.

Appendix Table 11: Elasticities of Health Expenditures by Payer

	All Payers		Out-of-Pocket		Medicare	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Income	0.03**	-0.04***	0.36***	0.20***	-0.13***	-0.17***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	18144	18144	18144	18144	18144	18144
R^2	0.020	0.039	0.059	0.115	0.062	0.072
Demograpics Controls		X		X		X

Notes: The table shows results from OLS regressions of male Social Security beneficiaries aged 65-84 in the Medical Expenditure Panel Survey (2000-2017). All models include survey year fixed effects, and a quadratic age term. Demographic controls include dummies for race, ethnicity, education, martial status, and census region. Robust standard errors in parentheses.

Appendix Table 12: Delayed Retirement Credits and Full Retirement Age

Birth Year	FRA	Benefit (% of PIA) claiming at age			
		62	65	66	70
1924	65	80	100	103	115
1925-26	65	80	100	103.5	117.5
1927-28	65	80	100	104	120
1929-30	65	80	100	104.5	122.5
1931-32	65	80	100	105	125
1933-34	65	80	100	105.5	127.5
1935-36	65	80	100	106	130
1937	65	80	100	106.5	132.5
1938	65.17	79.17	98.89	105.42	131.42
1939	65.33	78.33	97.78	104.67	132.67
1940	65.50	77.5	96.67	103.5	131.5
1941	65.67	76.67	95.56	102.5	132.5
1942	65.83	75.83	94.44	101.25	131.25
1943-54	66	75	93.33	100	132

Notes: The table shows how benefits vary by birth cohort and claiming age.