

Disability Insurance Income Saves Lives*

March 2018

Alexander Gelber
UC Berkeley Goldman School of Public Policy and NBER

Timothy Moore
University of Melbourne

Alexander Strand
Social Security Administration

Abstract

We show that higher payments from U.S. Social Security Disability Insurance (DI) reduce mortality. Using administrative data on all new DI beneficiaries from 1997 to 2009, we exploit discontinuities in the benefit formula through a regression kink design. We estimate that \$1,000 in annual DI payments decreases the annual mortality rate of lower-income beneficiaries by approximately 0.1 to 0.25 percentage points, implying that the elasticity of annual mortality with respect to annual DI income is around -0.6. The mortality effects imply large benefits in the welfare analysis of disability insurance.

* Gelber: agelber@berkeley.edu; Moore: timothyjmoore@gmail.com; Strand: alexander.strand@ssa.gov. This research was supported by Social Security Administration Grant NB14-06 to the SSA Disability Research Consortium at the National Bureau of Economic Research (NBER). We thank Paul O'Leary for helping us to understand the Disability Analysis File data, and we thank John Bound, David Cutler, Manasi Deshpande, Mark Duggan, Bill Evans, Itzik Fadlon, Nathan Hendren, Erzo Luttmer, Zhuan Pei, Stefanie Stantcheva, Lesley Turner and David Weaver for helpful suggestions. We thank seminar participants at the Australian Labor Econometrics Workshop, Korea University, NBER Public Economics Meeting, NBER Summer Institute, National University of Singapore, North Carolina State University, Purdue University, University of Illinois at Urbana-Champaign, University of Indiana at Bloomington, University of Michigan, and UC Berkeley for comments. We thank the UC Berkeley Burch Center and Berkeley Institute for the Future of Young Americans for research support. The use of the Synthetic SIPP for this research was made possible through the use of Cornell University's Synthetic Data Server, which has received funding through NSF Grants SES-1042181 and BCS-0941226, and through a grant from the Alfred P. Sloan Foundation. We are grateful to Gita DeVaney, Sree Kancherla, and Matt Unrath for outstanding research assistance. The findings and conclusions expressed are solely those of the author(s) and do not represent the views of SSA, any agency of the Federal Government, or the NBER. All errors are our own.

I. Introduction

Disability insurance is a key part of the safety net provided by social insurance programs, as it protects workers and their families from the major economic risks associated with a permanent disability that prevents or limits work. U.S. Social Security Disability Insurance (DI) currently insures over 150 million American adults against these risks, and in 2016 paid approximately \$147 billion to 11 million disabled workers and their families (Social Security Administration (SSA) 2016). Beneficiaries are heavily dependent on these DI payments: 80 percent are in households that receive more than half of their income from DI, and 31 percent are in households that had no income other than from DI (Bailey and Hemmeter 2014). DI beneficiaries are also in poor health: approximately 14 percent of those who entered DI between 2006 and 2010 died within four years, a mortality rate that is roughly ten times the rate for working-age adults in the general population (Arias 2014, Zayatz 2015).

Given these characteristics, a fundamental policy question is whether DI income improves the health of those who receive it. There is a surprising lack of evidence on this question. To the best of our knowledge, none of the existing literature has studied the causal effects of DI payments on health outcomes in the U.S. or considered the valuation of such potential benefits. Indeed, Chetty and Finkelstein (2013) note the limited attention given to the potential benefits of DI: “One particularly important program that has received relatively little attention in terms of measuring benefits...is disability insurance” (*p.* 189).

The primary goal of this paper is to examine how disability insurance income may affect beneficiaries’ mortality. We estimate the causal effect of income on mortality by using a Regression Kink Design (RKD) applied at three “bend points” in the formula that determines DI benefit amounts. The monthly DI payment – known as the Primary Insurance Amount (PIA) – is a progressive function of a beneficiary’s Average Indexed Monthly Earnings (AIME), which is the average of past earnings in Social Security-covered employment over the individual’s highest-earning years. As shown in Figure 1, the marginal rate at which PIA depends on AIME discontinuously changes from 90 percent to 32 percent at the “lower bend point” and from 32 percent to 15 percent at the “upper bend point.” In addition to these two bend points, family payment rules create a third bend point at which the marginal replacement rate for a family’s combined benefits to the primary beneficiary and dependents changes from 85 percent to 48 percent of AIME. We refer to this as the “family maximum bend point.” The lower, family

maximum and upper bend points occur, respectively, at the fourth, 30th and 84th percentiles of AIME. It is important to note that Medicare eligibility and other program rules do not vary around the bend points, which implies we will estimate the impact of DI income rather than DI eligibility *per se*.²

Our primary outcome is the average annual mortality rate during the first four years on DI. We use SSA microdata on all new DI beneficiaries from 1997 to 2009, covering 3,648,988 beneficiaries in the full sample. Intuitively, the RKD allows us to assess whether there are sharp changes in the slope of the relationship between mortality and AIME (our assignment variable) that correspond to the sharp changes in DI payments as a function of AIME at these bend points.³

We find that DI payments reduce mortality, particularly among lower-income beneficiaries. At the lower bend point, where mean annual DI income is \$8,543, we estimate that an increase of \$1,000 in annual DI payments decreases beneficiaries' annual mortality rate by 0.26 percentage points.⁴ At the family maximum bend point, where mean annual DI income for the primary beneficiary is \$12,648 and a further \$6,324 is paid for the beneficiary's dependent(s), we estimate that an increase of \$1,000 in annual DI payments decreases beneficiaries' annual mortality rate by 0.09 percentage points. These estimates correspond to elasticities of mortality with respect to DI income of -0.56 and -0.57, respectively. Around the upper bend point, where the primary beneficiary receives mean annual DI income of \$20,777 per year, we find no robust evidence of an effect, though our confidence intervals cannot rule out substantial effects. We perform several robustness and placebo tests and verify that our estimates at the lower and family maximum bend points represent true causal policy effects, as opposed to underlying non-linearities in the relationship between mortality and AIME. The literature has found a concave raw relationship between lifespan and income in the general population (Preston 1975, Chetty *et al.* 2017). Our estimates mirror this concavity – with larger absolute effects at lower lifetime income levels – demonstrating that the causal relationship (beyond the raw correlation) is concave in our setting.

² Weathers and Stegman (2012) study the health effects of accelerating Medicare eligibility for new DI beneficiaries.

³ We have previously used this identification strategy and context to examine the effect of DI income on beneficiary earnings (Gelber, Moore, and Strand 2017). For more background on the RKD, see Card *et al.* (2015).

⁴ All dollar amounts are expressed in 2013 dollars.

Our baseline point estimates imply that it costs around \$59,000 to save an additional life year at the lower bend point, and about \$237,000 at the family maximum bend point. \$50,000 is considered the “lower boundary” on the value of a statistical life year (VSLY) measure recommended by the latest major expert panel (Neumann, Cohen, and Weinstein 2014, Neumann *et al.* 2017). Therefore, our results suggest that the gains in life expectancy we document represent an important benefit of DI not recognized in previous estimates of optimal disability insurance benefit levels. For example, our estimates imply that among the approximately 32,000 individuals entering DI each year within our bandwidth of the lower bend point, increasing annual DI income by \$1,000 per beneficiary would save more than 8,000 life years.

In considering the potential benefits of DI, existing studies of the welfare effects of DI largely focus on its value for smoothing consumption or reducing income volatility, without considering direct measures of health outcomes (*e.g.*, Bound *et al.* 2004, Chandra and Samwick 2005, Ball and Low 2009, Meyer and Mok 2013, Low and Pistaferri 2015, Autor *et al.* 2017).⁵ To date, empirical evidence on the health effects of DI payments has been limited to a study using Dutch disability reforms that found opposite-signed effects of DI income on mortality for men and women (García-Gómez and Gielen 2017). The scant evidence on the causal health effects of DI contrasts with the large and growing literature quantifying the costs associated with the reduction in work due to disability insurance.⁶ One reason for the relative lack of evidence may be the difficulty in identifying causal effects of income on health for a program that specifically targets people whose health is poor, leading to potential reverse causality concerns (Smith 1999). Other evidence is limited to the larger literature on how income affects health in non-DI contexts.⁷ That larger literature provides little guidance for evaluating DI, however, as

⁵ Deshpande (2016) also examines how Supplemental Security Income for low-income youth affects income volatility. See Diamond and Sheshinski (1995) for a theoretical exploration of optimal DI. As we discuss later, in some models it would be sufficient to observe only non-health consumption to calculate the welfare effects of DI, while in others it would be necessary to observe mortality (as well as other aspects of health consumption).

⁶ For example, see Bound (1989), Gruber and Kubik (1997), Gruber (2000), Black, Daniel, and Sanders (2002), Autor and Duggan (2003), Chen and van der Klaauw (2008), von Wachter, Song, and Manchester (2011), Weathers and Hemmeter (2011), Campolieti and Riddell (2012), Maestas, Mullen, and Strand (2013), Borghans, Gielen, and Luttmer (2014), French and Song (2014), Gubits *et al.* (2014), Kostøl and Mogstad (2014), Autor *et al.* (2015), Moore (2015), Coile (2016), and Gelber, Moore, and Strand (2017). For a review of earlier work, see Bound and Burkhauser (1999).

⁷ A large literature spanning many disciplines has established that there is a strong positive correlation between income and good health, including reduced mortality and morbidity (*e.g.* Kitigawa and Hauser 1973). However, in some cases it has been difficult to establish whether these observed correlations are due to a causal relationship of

beneficiaries' high mortality rates and low average income makes them very different from other populations. Direct estimates of the impact of DI income on health are also relevant to current policy, as changes to DI benefit levels were considered in the President's Fiscal Year 2014 Budget (Office of Management and Budget 2013) and discussions preceding the Bipartisan Budget Act of 2015.⁸

By identifying a group of Americans for whom income strongly affects life expectancy, our findings also inform the literature on the economic determinants of health in general. Our estimates are generally larger than other estimates of the effect of income on mortality in developed countries (Cutler, Deaton, and Lleras-Muney 2006). However, our estimates of the elasticity of mortality to income of around -0.56 are within the range found in previous studies of high-mortality, low-income groups in other contexts, including old-age pensioners in Russia (-0.94 in Jensen and Richter 2004), U.S. Union Army veterans receiving pensions in the early 1900s (-0.57 in Salm 2011), and elderly recipients of conditional cash transfers in Mexico (-0.18 in Barham and Rowberry 2013). Our results show that the lifespan of individuals in the U.S. with disabilities and low lifetime income can benefit from additional income in ways that are similar to individuals in less developed economies or from earlier time periods. Our results also highlight the more general importance of incorporating health-related benefits in evaluating social insurance programs, even in the modern developed country context.

The remainder of the paper is structured as follows. Section II describes the policy environment. Section III explains our identification strategy. Section IV describes the data. Section V shows our graphical analysis and RKD estimates of the effects. Section VI discusses implications for the welfare analysis of DI. Section VII concludes. The online appendix contains additional material.

II. Policy environment

income being protective of health (Smith 1999, Deaton 2003). For examples of studies that examine the health effects of income from social insurance or transfer programs other than DI, see Duflo (2003), Case (2004), Jensen and Richter (2004), Snyder and Evans (2006), Salm (2011), Barham and Rowberry (2013), Evans and Garthwaite (2014), Aizer *et al.* (2016), and Hoynes, Schanzenbach, and Almond (2016). For examples of studies that use other types of income, see Preston (1975), Preston and Taubman (1994), Ruhm (2000), Deaton and Paxson (2001), Case, Lubotsky, and Paxson (2002), Akee *et al.* (2013), and Cesarini *et al.* (2016). A related question is how employment or job displacement affects health (Sullivan and von Wachter 2009).

⁸ See https://www.washingtonpost.com/news/wonk/wp/2015/10/27/big-changes-to-disability-benefits-could-be-possible-with-this-budget-deal/?utm_term=.25979bae29ef

DI insures workers for disabilities that limit their ability to work.⁹ DI payments begin in the month an individual's DI eligibility is determined. The rules determining DI income form the basis for our identification strategy. A DI primary beneficiary's PIA, which is the monthly payment the beneficiary will receive, is calculated using their AIME. AIME depends on annual earnings from the age of 21 to a disabled worker's date of eligibility for DI. Earnings are converted to current values using the National Average Wage Index (NAWI), and then the lowest one-fifth of earnings years – up to five years – are dropped.¹⁰ Earnings in the remaining years are averaged and converted to monthly values to establish the AIME.

AIME is converted to PIA using a formula designed to provide higher replacement rates for individuals with relatively poor earnings histories. For DI beneficiaries who became eligible in 2013, PIA was equal to 90 percent of the first \$791 of AIME, plus 32 percent of the next \$3,977 of AIME, plus 15 percent of AIME over \$4,768; see the solid line in Figure 1. The formula creates kinks in the graph at \$792, where the marginal replacement rate declines from 90 percent to 32 percent, and at \$4,769, where the marginal replacement rate declines from 32 percent to 15 percent.¹¹ We follow SSA terminology by referring to these as “bend points”: the initial change in the marginal replacement rate is the “lower bend point,” and the second change is the “upper bend point.”¹² The bend points were set through the 1977 Social Security Act Amendments and are adjusted annually based on the NAWI. Policy-makers crafting this law set the bend points in order to achieve Social Security benefit levels that were both progressive and

⁹ Beneficiaries qualify for DI because they are judged to have disabilities that prevent them from earning above the “Substantial Gainful Activity” limit. The Social Security Act, Section 223(d), defines disability as the “inability to engage in any substantial gainful activity by reason of any medically determinable physical or mental impairment which can be expected to result in death or which has lasted or can be expected to last for a continuous period of not less than 12 months.” Some of the background discussion in this paper, particularly in the Policy Environment, Empirical Strategy, and Data sections, overlaps and draws on Gelber, Moore, and Strand (2017), due to the parallel setting. However, the results in this paper are entirely new as they address mortality, a different outcome.

¹⁰ Earnings are converted to the year of eligibility using the NAWI with a two-year lag (*e.g.*, 2007 earnings are scaled by NAWI values for 2005). At least two years must be used in the AIME computation. Disabled workers who have fewer than three years discarded from the AIME calculation (under the rule dropping one-fifth of low earnings years), may have up to three additional years removed based on child care if they had no earnings and a child aged under three years. See SSA (2015) for more information.

¹¹ For clarity, note that “kink” is used both to describe the change in the PIA-AIME schedule at the bend points, and the change in slope in the outcome variable around the bend points.

¹² In practice, the PIA is capped both by the maximum tax paid annually on covered earnings, and also maximum family benefit rules that we discuss below. See SSA (2013) for more information.

financially sustainable, but not with the direct aim of achieving particular effects on outcomes such as mortality (Kelley and Humphreys 1994).

Another kink in the relationship between AIME and DI payments is created by the “family maximum” rules, which determine the benefits that can be paid to the disabled worker and their spouse and children (Romig and Shoffner 2015). Dependent payments are made for family members who are expected to have relied on the primary beneficiary financially; these are typically children under 18 or a spouse caring for children under 16.¹³ Dependents receive 50 percent of a primary beneficiary’s PIA, subject to the cap created by the family maximum rules.¹⁴ This cap specifies that the total DI benefits a family receives from a worker’s earnings record cannot be greater than the minimum of 85 percent of AIME or 150 percent of PIA. (It also cannot be less than PIA.) For DI beneficiaries with dependents, what we will call the “family maximum bend point” occurs at the AIME level at which the binding rule changes from the 85-percent-of-AIME rule below the bend point, to the 150-percent-of-PIA rule above the bend point.¹⁵ As shown in Figure 1, in 2013 this bend point occurs where AIME is \$1,860. The primary beneficiary’s marginal replacement rate is 32 percent at that point. Thus, when considering the determination of total family benefits, at the bend point the marginal replacement rate for each dollar of AIME changes from 85 percent (under the 85-percent-of-AIME rule) to 48 percent (*i.e.*, 150 percent of the 32 percent replacement rate). The family maximum rules imply that the total payments to dependents are the same regardless of whether there are one or multiple dependents.

We therefore have three bend points at which the marginal relationship between DI benefits and AIME changes: the lower and upper bend points that affect the DI payments to the primary beneficiary, and the family maximum bend point that affects the total family DI payments to primary beneficiaries plus their dependents.

¹³ A (former) spouse can receive benefits at any age if he or she is caring for children under age 16. The majority of dependents are minors: among DI awards made in 2013, 76 percent of dependents were children under 18. A further 11 percent were spouses caring for children aged under 16, eight percent were students aged 18 or 19, and five percent were disabled adult children (SSA 2015).

¹⁴ If an auxiliary DI beneficiary designates the “representative payee” as the primary beneficiary, the auxiliary’s DI payments are paid as one payment to the primary beneficiary.

¹⁵ The term “family maximum bend point” could also refer to the rules for the maximum family payments for Old Age and Survivors Insurance (OASI), which are different. We use the term as it applies to DI.

Two policy issues affect how we interpret the policy variation. The first relates to how family maximum rules affect DI payments near the lower bend point. Dependent benefits are not paid at low levels of AIME, because the initial 90 percent marginal replacement rate exceeds the 85-percent-of-AIME cap on total family payments. As a result, the marginal replacement rate at which AIME is converted to total family payments in this range is 90 percent. As illustrated in Figure 1, once AIME reaches a sufficient level that PIA is equal to 85 percent of AIME, which occurs at an AIME that is \$75 higher than the AIME at the lower bend point, the family-level marginal replacement rate becomes 85 percent (due to the 85-percent-of-AIME cap). Thus, for beneficiaries with dependents, there is little change in this marginal replacement rate around the lower bend point. This attenuates the marginal change in DI income at the lower bend point for all beneficiaries (*i.e.*, combining both those with dependents and without), relative to the change we would observe only for beneficiaries without dependents. However, due to data limitations we cannot confidently identify whether a beneficiary near the lower bend point has dependents.¹⁶

The second issue is how Supplemental Security Income (SSI) interacts with DI for “dual-eligible” beneficiaries, *i.e.* beneficiaries who are eligible for both programs. SSI provides cash and Medicaid to disabled individuals who, apart from a home and a car, have only a few thousand dollars in assets. The monthly SSI federal benefit rate for individuals in 2013 was \$710.¹⁷ Most individuals who are eligible for both SSI and DI receive only DI, with two exceptions. First, some newly awarded DI beneficiaries are eligible for SSI but have DI benefits that are greater than their SSI benefits; these beneficiaries still can claim SSI benefits during the DI waiting period, which is at most five months (there is no waiting period for SSI). After this waiting period, these individuals receive only DI. Second, some DI beneficiaries have a PIA that is less than the SSI federal benefit rate. These individuals receive both DI and SSI benefits on an ongoing basis (as well as SSI during the DI waiting period). These beneficiaries’ total benefits,

¹⁶ We cannot confidently identify whether a beneficiary has dependents near the lower bend point because the family maximum differentially affects the incentive to report dependents below *vs.* above the lower bend point: additional dependents lead to additional benefits above, but not below, the lower bend point. Appendix Figure A1 shows that the number of beneficiaries with reported dependents indeed increases sharply above the lower bend point (even though the number of beneficiaries does not rise sharply, as shown in Figure 2). Appendix Table A1 confirms that the percent of the sample with dependents rises discontinuously at the lower bend point.

¹⁷ Most states supplement this with additional cash payments. SSI recipients also generally receive Medicaid coverage and SNAP (food stamp) eligibility; in California, SSI recipients receive a supplement in lieu of SNAP. They are subject to more restrictive work rules that include benefit reductions for earnings above \$65 per month.

summing DI and SSI, are equal to the SSI federal benefit rate (with the SSI program paying the difference between their DI benefit and the SSI federal benefit rate).

For dual-eligible beneficiaries whose AIME puts them under the lower bend point, SSI eligibility therefore implies that total disability payments (summing over DI and SSI) do not change as a function of AIME. The SSI monthly payment amount of \$710 is nearly identical to \$712, the PIA they would receive if they had an AIME that put them at the lower bend point. In effect, this implies that for dual-eligibles the slope of total disability benefits as a function of AIME increases from zero to 32 percent near the lower bend point (as opposed to the change from 90 to 32 percent among non-dual-eligibles). However, it is difficult to exploit this change, as other policy variation also affects dual-eligibles around the bend point. Those with a PIA below the SSI monthly federal benefit rate and who meet SSI's other qualifications including its asset test are eligible for Medicaid through SSI, whereas those above are only eligible for Medicare through DI after a waiting period. Those below are subject to SSI's 50 percent benefit reduction rate for current earnings greater than \$65 a month, whereas those above are not. SSI eligibility also weakens the role of the family maximum rules, as DI benefits replace SSI payments one-for-one, including the case of DI dependent benefits and SSI benefits for children.

To address these issues with the dual-eligible sample, we remove DI beneficiaries from our sample who also receive SSI during the waiting period and/or concurrent with DI receipt.¹⁸ By removing dual-eligibles, we remove those who are eligible for Medicaid through SSI. We verify that the probability of being dually eligible for DI and SSI is smooth around the bend points, consistent with the supposition that SSI receipt is not a margin on which there is sorting in relation to the bend points. Other criteria for Medicaid eligibility include current income, assets, and family structure. In our sample without dual-eligibles, Medicaid eligibility therefore should not systematically vary around the bend points in a discontinuous way, as AIME is determined by individual lifetime (not current) earnings.¹⁹

¹⁸ It is not feasible to use our identification strategy on the combined population of dual-eligibles and non-dual-eligibles around the lower bend point, because the change at the lower bend point in the mean marginal replacement rate averaged over dual-eligibles and non-dual-eligibles is close to zero.

¹⁹ Table A13 finds no significant effect of DI income on earnings at the lower or family maximum bend points. However, higher DI payments cause lower earnings at the upper bend point. If this affects Medicaid eligibility and thus health, in principle this could affect the interpretation—though not the validity—of our mortality estimates at this bend point. However, the relatively high assets of beneficiaries at this bend point suggest any impact on Medicaid eligibility should be slight. In practice we find little effect of income on mortality at this bend point.

III. Empirical strategy and interpretation of estimates

A. Identification strategy

We exploit this policy variation using an RKD, which uses a change in the slope of treatment intensity to identify local treatment effects by comparing the relative magnitudes of the kink in the treatment variable and the induced kink in the outcome of interest. Estimates can be interpreted as a “treatment-on-the-treated” parameter (Card *et al.* 2015).²⁰ In our context, the treatment intensity is the size of DI benefits (*i.e.*, the PIA or the family maximum), the assignment variable is the AIME observed when the individual first applies for DI, and our primary outcome variable is the mortality rate of beneficiaries after entering DI.

Mathematically, we want to estimate the marginal effect of DI benefits (B) on the probability of mortality (Y). Benefits depend on AIME (A). Using the RKD, we can estimate the marginal effect around a given bend point A_0 as:

$$E \left[\frac{\partial Y}{\partial B} \middle| A = A_0 \right] = \frac{\lim_{A \rightarrow A_0^+} \frac{\partial E[Y|A = A_0]}{\partial A} - \lim_{A \rightarrow A_0^-} \frac{\partial E[Y|A = A_0]}{\partial A}}{\lim_{A \rightarrow A_0^+} \frac{\partial E[B|A = A_0]}{\partial A} - \lim_{A \rightarrow A_0^-} \frac{\partial E[B|A = A_0]}{\partial A}} \quad (1)$$

That is, the marginal effect we estimate is the change at the bend point in the slope of mortality as a function of AIME, divided by the change in the slope of DI benefits. Mortality is often analyzed with a hazard model (*e.g.* Cox 1972); however, the econometrics of RKD have not been established for hazard models (*cf.* Calonico, Cattaneo and Titiunik 2014, Card *et al.* 2015).

Identification of the effect of DI benefits on mortality through this RKD relies on two key assumptions (Card *et al.* 2015). First, in the neighborhood of a bend point, the direct marginal effect of the assignment variable on the outcome of interest must be smooth (*i.e.*, continuously differentiable). Second, conditional on unobservables, the density of the assignment variable is continuously differentiable in this neighborhood. These assumptions may not hold if we observe sorting in relation to the bend points, as reflected by a change at a bend point in the slope or level of the density of the assignment variable, or in the distribution of predetermined covariates.

Such sorting appears implausible in our context and would be surprising to find in the data. Our assignment variable is AIME measured when individuals apply for DI (“initial AIME”). Because this is measured before individuals go on DI, it cannot be affected by earnings

²⁰ Recent work applying the RKD includes Landais (2015) and Manoli and Turner (forthcoming).

while on DI. Furthermore, calculating PIA on the basis of an individual’s earnings history is complex, so it is difficult for individuals to estimate precisely where their earnings history will put them in relation to the bend points. Typically, the AIME calculation takes account of many years of earnings history: in 2012, 66 percent of DI entrants were aged 50 years or older and thus had a relevant earnings history lasting 28 or more years (SSA 2013). This, together with the use of lagged values from the NAWI to adjust both earnings and bend points and the dropping of the lowest-earnings years, makes it hard for DI applicants to generate a particular AIME. Moreover, individuals are often unaware of relevant Social Security rules (Liebman and Luttmer 2015). Even if individuals were aware of these rules, they would typically have to change their earnings over long periods of time to change their AIME substantially. This is especially difficult for disabled workers, who typically experience declining earnings in the years before applying for DI (von Wachter, Song, and Manchester 2011). The year prior to applying for DI would typically be among the lowest-earning years and would therefore be excluded from the AIME calculation.

B. RKD implementation

The identification results for RKD are relatively new (Nielsen, Sørensen, and Taber 2010, Calonico, Cattaneo, and Titiunik 2014, Card *et al.* 2015), and many issues related to its empirical implementation are unsettled. We follow available guidance for the RKD and – where none is available for the RKD – guidance for the regression discontinuity design (RDD), while also assessing the robustness of the results to alternative choices.

We use a “sharp” RKD as our main specification. When B is a deterministic function of A , the denominator of (1) is known and only the numerator needs to be estimated. The determination of PIA and family maximum on the basis of AIME is determined by law. Moreover, we show that the observed DI benefits are nearly identical to the Social Security formulas.²¹ Accordingly, in our main specification we assume that the PIA depends deterministically on the AIME as shown in Figure 1, and we estimate only the numerator of (1), the change in the slope of the conditional expectation of the mortality rate at the bend point. If the relationship between the mortality rate Y and AIME is linear, then we can estimate:

$$Y_i = \beta_0 + \beta_1(A_i - A_0) + \beta_2(A_i - A_0)D_i + \varepsilon_i \quad (2)$$

²¹ We show that the average difference between actual and estimated PIA is \$1.80 around the lower bend point, \$2.18 around the family maximum bend point, and \$2.62 around the upper bend point. We also verify that a “fuzzy” RKD that uses actual DI payments to estimate the denominator in (1) produces similar results.

where i indexes observations and $D_i = 1[A \geq A_0]$ is a dummy for being above the bend point. We limit the analysis to observations for which $|A - A_0| \leq h$, where h is the bandwidth size. The slope of the mortality rate as a function of AIME below the bend point is captured by β_1 , and we test for a change in slope in that relationship at the bend point by examining whether β_2 is significantly different from zero. Our primary coefficient of interest is therefore β_2 . ε_i is an error term. Following Card *et al.* (2015, 2017) we use White robust standard errors. For small changes in income, we postulate that the relationship between DI income and mortality can be characterized as linear and use a linear probability model as our baseline, but we verify in the Appendix that our results are materially unchanged under a grouped logit specification.

Our main outcome of interest is the mean annual mortality rate averaged over the first four years that individuals receive DI. We calculate mortality rates using data aggregated to bins that span \$50 of AIME, which is the largest size at which all of our dependent variables pass the two tests of excess smoothing for RDD recommended by Lee and Lemieux (2010). In other words, in each of these bins, we compute the probability of dying within four years of initially receiving DI, and then we divide by four to compute the annual mortality rate. Thus, i indexes bins in equation (2). By averaging data within each bin, we estimate standard errors that we view as conservative, following another of Lee and Lemieux’s (2010) suggestions in the RDD context.²² We also show results when estimating our regressions at the individual level and using other bin sizes.

A key issue is the choice of bandwidth. Several bandwidth selection algorithms have been proposed for RKD, including a MSE-optimal “data-driven” procedure (Calonico, Cattaneo, and Titiunik 2014) and a “rule-of-thumb” procedure (Card *et al.* 2015). Card *et al.* (2017) caution researchers against assuming there is a default procedure, and show that different approaches may perform better or worse depending on the empirical application. We adopt the following approach. We implement the bandwidth selection procedures recommended by Calonico, Cattaneo, and Titiunik (2014), and choose the symmetric bandwidths that minimize MSE (rounding the bandwidth in \$50 increments to match our use of bin-level data). For the average four-year mortality rate – our main outcome of interest – our bandwidths are \$400 at the

²² This choice also implies that we use a continuous dependent variable instead of a binary one. Our approach therefore avoids issues related to estimation and inference when a binary outcome is relatively uncommon, which is relevant here as the probability of death for any one individual can be relatively low.

lower bend point, \$700 at the family maximum bend point and \$650 at the upper bend point. We use these respective bandwidths throughout the analysis wherever possible, as doing so allows a direct comparison of different results for the same bend point. We also assess the robustness of the results to the choice of bandwidth.

Another issue is how to control for the underlying relationship between the assignment variable and our outcomes. A variety of approaches have been adopted.²³ Our initial specification (2) controls for the linear term ($A-A_0$). A linear specification should be appropriate if there is a constant marginal relationship between income and mortality, as might be expected when using a narrow range for income. However, we also wish to address the possibility that the baseline relationship between the mortality rate and AIME is better captured through the addition of higher-order terms to (2), such as quadratic or cubic terms. Our approach therefore is to estimate versions of equation (2) with (a) linear, (b) linear and quadratic, or (c) linear, quadratic, and cubic terms in AIME, demonstrating robustness to all three choices.

Two final issues are whether to allow for a discontinuity in the level of the mortality rate at the bend points and whether to control for covariates. When treatment effects are heterogeneous, the imposition of continuity is necessary for the change in slope at the bend point to be considered a causal parameter (Card *et al.* 2015). However, Ando (2017) suggests that imposing continuity may increase the likelihood of spurious results, while also arguing the addition of covariates minimizes the likelihood of spurious results. As robustness checks, we implement specifications allowing for a discontinuity or controlling for covariates.

C. Interpretation of the estimates

We interpret our RKD results as reflecting the effects of larger DI transfer payments on mortality. *A priori*, larger DI transfer payments could lead either to decreases or increases (or no change) in mortality. For example, increased DI transfer payments could lead individuals to purchase more of goods that allow them to avoid mortality (*e.g.*, a better diet or treatment for disability-related conditions). On the other hand, increased DI transfer payments could lead individuals to work less (Gelber, Moore, and Strand 2017), and working less could lead to

²³ Card *et al.* (2015) use linear and quadratic specifications. Calonico, Cattaneo and Titiunik (2014) propose an RKD estimator where a quadratic term in the assignment variable can be used to correct the bias in the linear estimator. Ganong and Jäger (2014) argue that cubic splines perform better than other estimators.

increased mortality (see, *e.g.*, Snyder and Evans 2006, Fitzpatrick and Moore 2018).²⁴ Our estimates should be interpreted in light of the fact that current and future monthly DI and subsequently OASI payments are stable. Payments generally only change with annual cost of living adjustments, and the PIA calculated for DI is also used when an individual reaches the Full Retirement Age and is transferred to OASI (Gelber, Moore and Strand 2017). The main exception is when individuals receive “back pay” in the first month of DI receipt to cover retroactively the period between becoming disabled and DI receipt.²⁵

The group whose “treatment on the treated” effects we identify consists of those with AIME near the bend points. Our estimates represent the effects of changing DI benefit payments while holding other factors constant, thus holding constant variation stemming from the DI application process, the role of Medicare, or DI earnings rules. Like other papers based on local variation, including others in the DI literature, our identification strategy does not attempt to estimate general equilibrium impacts of DI.

It is also important to clarify the role of dependent payments in interpreting the estimates. In our sharp RKD specification at the lower and upper bend points, the first stage only measures payments to the primary beneficiary. Measuring the first stage in this way effectively corresponds to an extreme case in which primary beneficiaries’ mortality is not influenced by their dependents’ DI benefits. However, it is possible that the primary beneficiary’s mortality is also influenced by the benefits paid to his or her dependents. For example, one alternative assumption is a “unitary” model of the family, in which the family acts as if it maximizes a single utility function and therefore pools the unearned income of all family members (Becker 1976). This would have implications for interpreting the estimates at each of the bend points.

As discussed in Section II, when considering overall DI payments to the family in the full sample near the lower bend point, the “first stage” relationship between DI benefits and AIME is attenuated, relative to the benefit schedule shown in the solid line in Figure 1 in which the marginal replacement rate changes from 90 percent below the bend point to 32 percent above it.

²⁴ We interpret the estimates as income effects, as the changes in DI transfers around the bend points do not create substitution effects (Gelber, Moore, and Strand 2017).

²⁵ The timing of income can affect mortality, although it is a short-term effect that should have little impact on mortality measured over four years. Although Evans and Moore (2011) find that there is a short-term increase in mortality following the receipt of Social Security income, this effect is primarily present in the first week after receipt and largely offset by declines in mortality three to four weeks afterwards.

In particular, Figure 1 shows that there is effectively little change in the family benefit marginal replacement rate in the region surrounding the bend point for those with dependents. This implies that, relative to assuming the marginal replacement rate changes from 90 percent to 32 percent for the full sample, in the unitary model the relevant change in the marginal replacement rate measured in the denominator of (1) would be smaller. Since we make the “sharp RKD assumption” that the change in the marginal replacement rate is from 90 percent to 32 percent in the full sample, our estimate of the absolute treatment effect should be interpreted as a *lower bound* on the true absolute effect if dependent benefits affect the primary beneficiary’s mortality. This is the first of several reasons described throughout the paper that we estimate a lower bound on the true absolute effect.

Meanwhile, the family maximum bend point only applies to those with dependents. Thus, at the family maximum bend point our estimates should be interpreted as the impact of variation in dependent benefits on the primary beneficiary’s mortality. We find significant effects at this bend point, demonstrating that dependent benefits do indeed affect the primary beneficiary’s mortality in this sample.²⁶ If households are not unitary, for example as in a “collective” model of household bargaining (Chiappori 1992), then payments made to a beneficiary’s dependents could have a smaller effect on the beneficiary than his or her own payments.

IV. Data

To apply the RKD, we use administrative data from the 2010 version of the Disability Analysis File (DAF) (previously called the Ticket Research File). The DAF is a compilation of multiple administrative data sources from the Social Security Administration, including the Master Beneficiary Record, Supplemental Security Record, 831 File, Numident File, and Disability Control File. The DAF contains information on all disability beneficiaries who received benefits in at least one month between 1997 and 2010. It includes information on AIME and PIA. The data sources that are used to construct the DAF also provide information on each beneficiary’s demographic characteristics, including age, race, and gender; DI program activity, including path to allowance (*e.g.*, whether a claimant was determined to be eligible by the initial

²⁶ In the unitary model, the change in marginal replacement rates for those with dependents is 50 percent larger at the upper bend point. Although we quote the mortality estimate based on the primary beneficiary’s benefit alone as a benchmark, in a unitary setting these estimates would be smaller by one third for beneficiaries with dependents (who represent approximately one third of the sample).

disability examiner or through a hearings-level appeal) and the magnitude of disability payments; and exact date of death (day, month, and year) (Hildebrand *et al.* 2012). We obtained updated information on date of death through 2013 in order to extend the period over which we could track beneficiaries' mortality. Annual taxable W-2 wage earnings through 2011 are obtained by linking to the Detailed Earnings Record (DER). We do not have data on assets, total unearned income from other sources, marital status, spousal outcomes, hours worked, or cause of death.²⁷

The mortality information in the DAF comes both from the Master Beneficiary Record and the Numident File. SSA receives this information from beneficiaries' family members, as well as from funeral homes, financial institutions, government agencies and postal authorities. SSA also contracts with state vital statistics bureaus to provide dates of death to manage program payments. SSA policy is to verify death reports for DI beneficiaries from sources it considers less accurate. SSA data miss a small number of deaths (Government Accountability Office 2013, SSA Office of Inspector General 2012, 2017). However, there is no evidence that the fraction of deaths that are missing varies by DI income, and no SSA policies related to death reporting depend on the size of income payments.²⁸ Therefore, the degree of measurement error should not change around the bend points and should not confound our variation. However, if SSA data miss a small number of deaths, we may under-estimate the causal effect of DI benefits on mortality, suggesting that our estimates represent lower bounds on the true effects.

The effect of DI receipt on mortality is an important, but different, question than the one we study. We isolate the effect of DI payments on mortality, which is relevant to evaluating the benefits of the cash transfer component of DI.²⁹

We choose a sample of individuals who entered DI between 1997 and 2009 and who were aged 21 to 61 years at the time of filing. This allows us to observe whether these individuals died within a follow-up period of four years after beginning to receive DI payments, meaning the four years beginning with the first month in which recipients received DI payments.

²⁷ In the Current Population Survey over the years 2001-2010, of those reporting that "Disability causes difficulty working," 42.76 percent were married.

²⁸ The role of DI eligibility in mortality reporting could matter if the variation in DI income affects DI exit rates at the bend points. However, in Gelber, Moore, and Strand (2017) we show that the bend points do not affect the likelihood of exiting the DI program in order to return to work.

²⁹ SSA does not verify deaths for non-DI beneficiaries, implying that mortality is measured with greater error among non-beneficiaries. In estimating the effect of DI receipt on mortality, this should create non-classical measurement error and therefore lead to biased and inconsistent estimates.

Four years is also the period following DI receipt that is used in Maestas, Mullen, and Strand (2013) and Gelber, Moore, and Strand (2017). The upper age restriction to those under 61 avoids interactions with rules associated with OASI. To focus on beneficiaries whose DI payments are affected by the bend points, we also limit the sample to DI primary beneficiaries who did not receive SSI at any point in the sample period, thus eliminating dual-eligibles who collect SSI during the DI waiting period and/or on an ongoing basis.

We clean the data by removing records with missing or imputed observations of basic demographic information (*e.g.*, date of birth or sex), which reduces the sample by 2.0 percent. We also remove records in which there is no initial AIME or PIA value, or in which the stated date of disability onset used for the PIA calculation is more than 12 months before the date of filing or 17 months after the date of filing (the range over which documented date of disability onset should lie). This reduces the sample by another 5.5 percent. In addition, we remove individuals who have a PIA based on eligibility for DI under both their record and that of another worker or who had not received DI payments within four years of filing, reducing the sample by another 1.5 percent. We additionally clean the data to remove cases in which the data contain unreliable measures of AIME by removing those with more than four AIME changes, which removes 3.4 percent. The SSA data systems typically have a small number of cases with unusual or implausible records; these sample restrictions are similar to those generally made when using these data (*e.g.*, von Wachter, Song, and Manchester 2011, Maestas, Mullen, and Strand 2013, Moore 2015, Gelber, Moore, and Strand 2017).

At the family maximum bend point, we limit the sample to beneficiaries for whom a dependent benefit is also paid within two months of their own initial payment. This is necessary because the eligibility of dependents can change over time due to changes in marital status, the employment of spouses, and the age and education activities of children (SSA 2017), and we want to minimize error by choosing a sample that is subject to the payment formula during the period of observation. Removing beneficiaries whose auxiliary payments began outside this window removed 27.3 percent of this sample. The samples for lower and upper bend points include both beneficiaries with and without reported dependents.³⁰

³⁰ Although in principle we could restrict the lower bend point sample to beneficiaries without dependents in order to address the measurement issues related to the family maximum rules described above, it would not be prudent to

PIA is measured in pre-tax terms. By examining the effect of pre-tax benefits, we answer the policy-relevant question of how a change in benefits paid by SSA would affect mortality. Since marital status and total family taxable income are not available in our data, we cannot measure the relevant tax rate. After-tax benefits are slightly smaller than pre-tax benefits—and the marginal replacement rate associated with after-tax benefits should change at the bend point by slightly less—again suggesting that our estimates should reflect lower bounds on the absolute effects of after-tax benefits.

Table 1 shows summary statistics. In the full sample, we have data on 3,648,988 observations. Average PIA is \$1,360. PIA is a monthly measure of DI payments, so that \$1,360 in monthly payments translates into an annualized benefit of \$16,315. Annual mortality rates in the four years after first receiving DI range between 2.6 percent (fourth year after program entry) and 7.0 percent (first year after program entry). Average age when applying is 48.6, and 53.1 percent of the sample is male. For approximately half of the sample, the primary disability is either a musculoskeletal condition (29.7 percent) or mental disorder (20.1 percent), with neoplasms (cancer) (11.6 percent) and circulatory conditions (largely heart disease) (10.3 percent) also common.

The table also shows the summary statistics for samples around each of the bend points. Those around higher bend points have higher mean PIA. The lowest mortality rates are observed for the family maximum bend point sample; beneficiaries must have a dependent to be included resulting in this relatively young sample. Appendix Figure A2 shows that the lower, family maximum, and upper bend points correspond to the 4th, 30th, and 84th percentiles of the AIME distribution, respectively.³¹

For additional context on the circumstances of DI recipients around the bend points, in Appendix Table A2 we use data from the Survey of Income and Program Participation (SIPP), linked by the Census Bureau to administrative data on program participation and income from the Social Security Administration and the Internal Revenue Service (SIPP/SSA/IRS files). We use the 1996, 2001, 2004 and 2008 SIPP panels, each representative of the U.S. civilian,

do so because we do not reliably measure the number of dependents around the lower bend point (see the footnote above regarding Appendix Figure A1).

³¹ An AIME at the 30th percentile of the distribution for the full population (combining both those with and without dependents) puts beneficiaries with dependents at the family maximum bend point.

noninstitutionalized population, and the restricted-use “gold standard” SIPP/SSA/IRS files (discarding data that are missing or imputed). DI recipients as a whole show important signs of economic disadvantage; for example, across the three bend points, DI income is the only source of income for between 40 and 50 percent of recipients. Focusing on medians to reduce the influence of outliers, measures of households’ resources are generally much larger for respondents assigned to the upper bend point region than the other two bend points. This is true for both overall current income and various measures of assets, including human capital, home ownership, and overall wealth. Most measures indicate those at the lower bend point have somewhat lower economic resources than those at the family maximum bend point; however, those at the family maximum bend point are younger, helping to explain why other measures such as net wealth are slightly lower here than at the lower bend point.

For further context, we use data from the Consumer Expenditure Survey (CES). Appendix Table A3 shows the summary statistics for DI recipient and non-recipient households.³² The summary statistics show that the DI beneficiary samples have substantially lower total expenditures than does the non-DI sample, particularly for DI beneficiaries with below-median current income. However, relative to the non-DI sample, health expenditures are substantially higher among DI beneficiaries (consistent with Ganong and Noel 2017).

V. Graphical and Regression Analysis

In using these data, we begin our analysis with validity checks on our empirical method. We then estimate our main results, demonstrate their robustness, and estimate heterogeneous effects by time period and demographic group.

V.a. Preliminary analysis

As an initial validity check, Figure 2 shows that the density of the number of observations in each bin, and its slope, appear continuous around the bend points. Appendix Figure A1 shows that at AIME levels around the family maximum bend point, the percent of the full sample with reported dependents also appears smooth. Appendix Figure A3 shows the distributions of the means of six predetermined covariates available in the administrative data: percent male, percent

³² DI beneficiaries are not directly identified in the CES. However, Moore and Ziebarth (2014) show that Social Security payments are nearly always to DI beneficiaries when everyone in the household is aged under 60. Note that the CES does not necessarily capture all forms of household expenditure. For discussion of the measurement issues associated with the CES, see Meyer and Sullivan (2011) and Bee, Meyer and Sullivan (2015).

black, percent allowed via hearing, percent whose disability is a mental disorder, percent whose disability is a musculoskeletal condition, and average age when applying for DI. All of these appear smooth through all three of the bend points. Appendix Figure A4 shows a “first stage” graph: as expected, measured PIA in the dataset shows changes in slope at the bend points in AIME in precisely the ways the policy dictates.

Our regressions in Table 2 confirm that the number of observations, these predetermined covariates, and the percent of the potential sample that receives SSI in our sample period (shown in Appendix Figure A5), are all smooth in the region of the bend points. We adopt an approach similar to Card *et al.* (2015) by examining whether the first derivative changes at the bend point, as measured by coefficient β_2 , when we run separate regressions with polynomials in AIME of order between three and five. For each dependent variable, we select the polynomial order that minimizes the finite-sample (corrected) Akaike Information Criterion (AICc) and report the change in slope at the bend point for that specification. We use our baseline specification with binned covariates and outcomes, without additional controls, and with no discontinuity in the dependent variable at the bend point. Out of 24 regressions (eight outcomes for each of three bend points), only one estimate of the change in the slope at the bend point is significantly different from zero at the ten percent level. As expected, the coefficients in these regressions are jointly insignificant around each bend point separately, and among all three bend points pooled. In the cubic specification specifically—which will show significant results in our main specifications with mortality as the outcome—only two estimates out of 24 are significant at the 10% level, a rate that is consistent with random chance (none of the estimates is statistically significant at the level of 5% or less).

Our appendix tables show further validity checks. Appendix Table A4 verifies that there is no evidence of a change in the level of the density of the running variable around each of the three bend points. Appendix Table A1 also verifies that there is no change in the level or slope of the percent of the sample with dependents in the full sample with AIME around the family maximum bend point. We further show in Appendix Table A5 that there is no evidence for “bunching” in the density of initial AIME.³³ Other literature has found an effect of DI payment

³³ We calculate “bunching” in the way described in detail in Gelber, Moore, and Strand (2017), similar to the method in Saez (2010). Working more will not lead to higher DI income if earnings are not in the highest-earning

size on DI applications and receipt (Black, Daniel, and Sanders 2002, Autor and Duggan 2003, von Wachter, Song, and Manchester 2011). Our finding, by contrast, is that we find no effect of payment variation on DI receipt locally around the bend points; this is consistent with the expectation that such small variation appears difficult to understand and not particularly salient, in contrast to the variation used in this previous literature.³⁴

All of these results suggest that individuals do not appear to locate their AIME strategically and RKD methods are appropriate for estimating causal treatment effects. As we discussed above, it is not surprising to find that there is no sorting around the bend points given that it is difficult to understand, calculate, and manipulate AIME. In Gelber, Moore, and Strand (2017), we also find that beneficiaries' earnings respond to the transfers after they go on DI, but not before. As we explain in Gelber, Moore, and Strand (2017), this evidence is consistent with the possibility that DI onset occurs relatively unexpectedly, supporting the notion that the effects we document are associated with changes in transfer income that were not anticipated by our sample prior to going on DI. This is also consistent with the evidence in the current paper showing the smoothness of the covariates – including the overall density and the densities of different disabilities – and the lack of bunching. This collection of evidence suggests that individuals did not behave as if they anticipate the DI transfers, and did not change health investments in advance of DI receipt based on variation in their anticipated future DI income around the bend points (see Grossman 1972 or Philipson and Becker 1998 on such effects).

V.b. Main Results

Having demonstrated that our empirical strategy passes these tests, in Figure 3 we show the mean yearly mortality rate for the four years after DI allowance around each of the bend points. There is a visible discontinuous increase in the slope of the mortality rate as a function of AIME, above the lower bend point relative to below it (*i.e.*, the negative slope becomes flatter). The estimated change in slope shown in the fitted lines fits the empirical observations well.

years used to calculate AIME. The incentives for working while on DI are discussed in detail in Gelber, Moore and Strand (2017).

³⁴ The Social Security Statement workers receive could only provide an approximate measure of their likely benefits (Gelber, Moore, and Strand 2017), implying that around the bend points: (1) actual PIA should be a smooth function of PIA as estimated on the Statement; and (2) it should be difficult to choose earnings to sort around the bend point on the basis of the information provided by the Statement. This does not rule out that the Statement has some general effects on application behavior (Armour forthcoming).

Around the family maximum bend point, the slope also appears to increase notably at the bend point. These results suggest that a decrease in DI benefits causes an increase in mortality at these bend points. The figure shows that there is little visible change in slope at the upper bend point.

Table 3 shows the estimated mortality effects at each of the three bend points when implementing regression (2). For ease of interpretation, we report the implied percentage point effect on the mean annual mortality rate of increasing annual DI benefits by \$1,000, which corresponds to variation in AIME that lies well within our bandwidths.³⁵ (Appendix Table A6 shows the full set of coefficients that we rely on to generate the implied effects presented in Table 3.³⁶) As the MSE-minimizing bandwidths for the quadratic and cubic specifications may be quite different from our “baseline” bandwidths, we initially report results using the Calonico *et al.* (2014) bandwidths for those specifications.

At the lower bend point, we find that an increase in DI benefits leads to a substantial reduction in mortality. In the linear model, the estimates show an elasticity of mortality to DI payments of -0.56, and an elasticity of -0.76 with respect to DI payments plus earnings (imputed at the bend point using the regressions underlying Table A6). In this specification, a \$1,000 increase in DI payments causes the yearly mortality rate to decrease by 0.26 percentage points. The estimated effects are larger in the quadratic and cubic models; all three estimates are statistically significant at the one percent level. Given that we use different MSE-minimizing bandwidths for each model presented in Table 3, it is difficult to compare model fit across specifications, but when we hold the bandwidth constant across specifications we find that the linear specification minimizes the AICc. Thus, we focus most on the results of our baseline linear specification.

Recall that we interpret the (absolute value of) our estimate at the lower bend point as a lower bound. Since we estimate a large effect, the fact that this is a lower bound only strengthens our conclusion that the effect is large. To obtain a partial sense of the extent to which this reflects a lower bound, note that Appendix Figure A1 shows that up to around 20 percent of the sample

³⁵ This corresponds to an increase in the PIA of \$83, which occurs at an AIME value of \$144 above the lower bend point, \$225 above the family maximum bend point, and \$490 above the upper bend point.

³⁶ To calculate the estimated effects in Table 3 on the basis of the Appendix Table A6 regressions, under our sharp RKD framework we divide the estimated β_2 in Appendix Table A6 by the change in slope in the first stage shown in Figure 1. For example, for the lower bend point, we divide the coefficient by $-0.58 = 0.90 - 0.32$.

reports a dependent once AIME is above the lower bend point in the region in which beneficiaries have an incentive to report dependents. Assuming that the change in the marginal replacement rate is zero for those with dependents and that this group shows an equal effect to the group without dependents, this would imply that our reported point estimates understate the true effect at this bend point by up to 20 percent (not accounting for other factors that could cause us to under-estimate the effect).

We also find that an increase in DI benefits leads to a substantial reduction in mortality at the family maximum bend point. In this specification, a \$1,000 annual increase in household DI payments causes a 0.091 percentage point decrease in the annual mortality rate of the primary beneficiary. The elasticity of mortality with respect to DI payments is -0.57, and the elasticity of mortality with respect to DI payments plus earnings is -0.67. The point estimates are larger in the quadratic and cubic specifications; the standard errors also increase but the estimates retain significance at the ten percent and five percent significance levels, respectively.

At the upper bend point, all three specifications show insignificant impacts of DI payments on the mortality rate. The point estimates are negative, but they are small and imprecisely estimated.

Taken together, these results suggest the largest and most robust impacts of DI payments are on mortality among lowest-income beneficiaries, *i.e.*, at the lower bend point followed by the family maximum bend point. It is perhaps not surprising that the largest effects of DI payments occur among the lowest-income groups; this is consistent with—and, using causal identification techniques, provides support for—literature from Preston (1975) to Chetty *et al.* (2017) that finds the correlation between income and mortality is strongest at low income levels.

V.c. Robustness and further validity checks

We perform several exercises to further establish the robustness of the mortality estimates. In Figure 4 and Table 4, we show results for four placebo samples. Panel A of Figure 4 shows the mean annual mortality rate in Years 1 to 4 of DI beneficiaries *without* dependents whose AIME puts them in the region of the family maximum bend point. These beneficiaries are unaffected by the family maximum rules, and therefore we should not see a change in the mortality-AIME relationship at the family maximum bend point. The figure suggests that there is no such change, and this is confirmed by the regression results in Column 1 of Table 4.

Figure 4 Panels B through D show the mortality rates for *non*-DI beneficiaries as a function of AIME at each of the three bend points. We create a sample of non-beneficiaries from the 2011 Continuous Work History Sample (CWHS), a one percent sample of active Social Security Numbers. The analysis sample consists of living DI-insured workers age 18-57 in 1997 who have never applied for DI. AIME is calculated as if they had become eligible for DI during 1997. We match these data to the Numident and measure mortality from 1998 to 2010.³⁷ In Figure 4 Panels B through D, there is no clear increase in slope at any of the bend points, and this is confirmed by the regression estimates in Columns 2 to 4 of Table 4 showing insignificant changes in slope.

Several appendix figures show additional robustness exercises, again focusing on the linear specification. Appendix Figure A6 show that the point estimates at the lower and family maximum bend points are fairly stable as the bandwidth is varied. As would be expected, the estimates become statistically significant once a sufficiently large bandwidth is used to achieve sufficient statistical power.³⁸ Appendix Figure A7 shows the elasticity estimate and 95 percent confidence interval when we run the RKD regression (2) for “placebo” bend points that are located throughout the range of AIME values covered by the bandwidths (in the spirit of Ganong and Jäger (forthcoming)). The figure shows that in both the lower bend point and the family maximum samples, the largest (in absolute value) and most statistically significant coefficients occur precisely at the actual location of the bend points. The formal “permutation test” following Ganong and Jäger (forthcoming) shows that the estimate with the kink placed at the actual bend point is statistically significantly larger in magnitude than the distribution of placebo estimates.³⁹

Additional appendix tables show further robustness checks. Within the specification for each polynomial, the estimates are similar when we allow a discontinuity at the bend point (Appendix Table A7), or when using a fuzzy RKD in which the “first stage” relates average observed PIA over the initial four years to initial AIME, rather than a sharp RKD (Appendix Table A8).⁴⁰ We also show that the estimates are similar when controlling for predetermined covariates at the individual level or removing the impact of covariate means at the bin level

³⁷ As we might expect, this non-beneficiary group has much lower mortality rates than the DI samples.

³⁸ Given the potential for bandwidth to be mis-specified, in Appendix Figure A6 we also vary the bandwidth for the upper bend point sample. The estimates are small and generally not statistically significant at the five percent level.

³⁹ When using placebo kinks farther from the bend point, we also estimate $p < 0.05$.

⁴⁰ We describe the fuzzy RKD in greater detail in Appendix 1.

(Appendix Table A9). This is mirrored in Appendix Figure A8, where we show that there are sharp changes in the slope of the residualized mortality outcome at both the lower bend point (in the full sample) and the family maximum bend point (in the sample with dependents) after removing the impact of covariates, and over a much wider range of AIME.⁴¹ To explicitly address the binary nature of mortality as an outcome, Appendix Table A10 shows that the results are similar to the baseline when we run a grouped logit model in which the dependent variable is $\ln[\text{mortality rate}/(1 - \text{mortality rate})]$ and the coefficients are transformed into marginal effects by multiplying them by $[(\text{mortality rate})/(1 - \text{mortality rate})]$. In Appendix Table A10 we also show nearly identical results to the baseline when we use individual-level data in running our main specification, rather than bin means (Appendix Table A10 Column 3); when we use finer \$10 or \$25 bins of AIME rather than \$50 in our baseline (Appendix Table A10 Columns 4 and 5, and Appendix Figures A9 and A10); and when we remove the sample restriction of excluding beneficiaries with more than four AIME changes (Appendix Table A10 Column 6). Appendix Table A11 shows that our placebo tests still show insignificant results when we use the bandwidths selected by the Calonico, Cattaneo, and Titiunik (2014) procedure separately for these CWS samples, rather than using our “baseline” bandwidths.

V.d. Effect heterogeneity

Our estimates may vary by time period or sub-group of the population. We first consider how the mortality effects vary by the period receiving DI. Estimates of the cumulative effect by year since going on DI using the linear model are shown in Figure 5, from Year 1 to Year 8.⁴² For this figure we restrict the sample to beneficiaries entering DI between 1997 and 2005, so that everyone is followed for eight years. (In Appendix Table A12, we show that the results for the first four years are similar using the main sample.)

⁴¹ Appendix Figure A8 Panel A also shows that, in the full sample, the mortality residuals broadly decline with AIME. This contrasts with Figure 3, which shows that mortality falls with AIME in the region of the lower bend point but rises with AIME at the upper bend point. The reason for the discrepancy relates to the effects of age and other characteristics that are correlated with AIME. In terms of age, older DI beneficiaries are over-represented at high AIME levels – since they have typically had more high earnings years and also benefit from the large adjustments that come from the NAWI when applied over long time periods – and also have higher mortality rates. Appendix Figure A8 Panel B shows that, among DI beneficiaries with dependents, the residualized mortality rate slopes down in AIME below the family maximum bend point, and only begins to slope upward above the bend point, where the slope is affected by the lower marginal replacement rate. Selection into DI also can affect the absolute level of the mortality-income gradient.

⁴² “Year 1” is defined as the first full year beginning with the month an individual starts to receive DI; “Year 2” as the second full year; and so on.

At the lower bend point, the point estimates of the *percentage point* effect are significant in each year and grow as further effects accumulate (Panel A).⁴³ Panel B shows that the *percentage* effect on mortality at the lower bend point is relatively constant throughout the period, suggesting that the impact relative to the baseline mortality rate remains fairly constant as the underlying annual mortality rate declines. At the family maximum bend point (Panels C and D), the effects become significant at the five percent level beginning in Year 4. After Year 4, the estimates are relatively constant in both percentage point and percent terms (growing slightly in percentage point terms and falling slightly in percent terms).

Table 5 shows the mortality effects at the lower and family maximum bend points in different subgroups of the population, reporting estimates using the linear model that again minimizes the AICc when holding the bandwidth constant across specifications for comparability. Broadly speaking, the absolute values of the point estimates of the effects are usually larger in groups with higher baseline mortality rates. At the lower bend point, the point estimate of the absolute effect on mortality is significantly larger for black relative to non-black beneficiaries, significantly larger for those initially allowed DI through an initial Disability Determination Service (DDS) assessment relative to those allowed via a hearing (after an initial denial), significantly larger for women relative to for men, and significantly smaller for mental or musculoskeletal disorders than for individuals with all other disabilities, particularly cardiovascular conditions. The estimates are insignificantly different for those who entered DI in earlier years (1997-2005) *vs.* later years (2006-2009), and insignificantly different for beneficiaries who are older at filing (aged 45 and older) *vs.* younger (aged below 45). The differences in mortality outcomes across these groups are mostly similar at the family maximum bend point, with significantly larger point estimates for those allowed via DDS than a hearing, and significantly smaller for mental and musculoskeletal disorders than all other disabilities.

As our estimates are local to the bend points, it is not possible to determine directly whether the results generalize to the full population of DI recipients. However, Table 1 shows that those allowed via DDS, and those with cancers or circulatory disorders, are under-represented at the lower and family maximum bend points relative to the full DI population. As a

⁴³ These are the cumulative effects from receiving \$1,000 of additional DI payments each year.

result, when we re-weight the population so that its demographic characteristics match those of the full sample, we estimate effects that are modestly larger than the baseline.

V.e. Potential Mechanisms

The changes in mortality due to DI income that we document could reflect the effects of changes in leisure, other consumption, or investment in health. Like many other estimates of causal effects, our RKD design is well suited to determine the causal impact of DI payments on mortality, but less well equipped to determine the mechanisms that mediate these impacts.

In our context, we can identify the income effect of DI benefits on earnings using our RKD variation in the payment schedule. As in Gelber, Moore, and Strand (2017), Appendix Table A13 verifies that there is a strong income effect of DI benefits on earnings at the upper bend point in our sample, but no large or statistically significant effects at the lower or family maximum bend points (both when we do and do not include the deceased in our sample). Thus, the significant effects on mortality we estimate at the lower and family maximum bend points do not appear to be associated with earnings effects in response to changes in DI income.⁴⁴

We can also investigate whether DI is associated with changes in expenditures. Survey data with relevant information, such as the SIPP and the CES, lack adequate sample sizes to apply an RKD to examine this directly. Power calculations show that our RKD will be (extremely) under-powered to detect the effect of DI payments on expenditures or consumption in these datasets. Thus, we pursue an alternative strategy to develop more suggestive evidence on the association of additional income with expenditures in the DI population. Appendix Table A3 shows that DI recipients near the lower and family maximum bend points have few economic resources, suggesting that the marginal propensity to consume and the consequent impact of DI payments on mortality could be particularly large in these groups.

Using data on DI beneficiaries – and separately non-beneficiaries – in the CES from 1986 to 2012, in Appendix Table A14 we regress measures of expenditures on household income, controlling for age, age squared, sex, as well as dummies for self-reported disability or illness, educational attainment, race, and state. There are several important caveats to interpreting these

⁴⁴ Although we lack definitive evidence, in principle it is possible that effects on earnings could help explain the lack of an effect on mortality at the upper bend point, for example if earnings losses due to DI payments at this bend point are associated with lower hours worked and this leads to an increase in mortality as in the Snyder and Evans (2006) study of OASI.

results as the effect of income on expenditures: unobserved heterogeneity could bias the coefficients; our measure of income reflects current income, not lifetime income as in a lifecycle model without liquidity constraints; and income (and other variables) are measured with error. Despite these important issues, the results may be suggestive of hypotheses for the mechanisms.

When overall expenditure is the dependent variable, the coefficient on current income is large among DI households – and 74 percent higher than for non-DI households. In absolute terms we find larger coefficients on income among DI households relative to non-DI households particularly when the dependent variable is expenditure on food, housing, utilities, health care, and transportation. Moreover, for these expenditures we generally find especially large coefficients on income among below-median-income DI households relative to above-median.

Among poorer DI households, then, additional income – including income from DI – could help individuals to afford life-saving consumption or investments, both through additional health expenditures and through other expenditures. Additional health expenditures could be particularly valuable during the initial 24-month waiting period for Medicare eligibility under SSDI; indeed Figure 5 shows that the largest percentage point effects occur initially. Among poorer DI households, higher income could also be associated with lower stress that leads to better health and longevity (*e.g.* Evans and Garthwaite 2015), greater expenditure on utilities could be associated with better heating that helps avoid deaths from diseases like pneumonia (particularly in immunosuppressed populations), or additional expenditures on food could support more nutritious food that lowers the incidence of cardiovascular conditions (Cutler, Glaeser, and Shapiro 2003). Such mechanisms are plausibly consistent with our heterogeneity results in Table 5, showing the largest effects among individuals with cardiovascular conditions and cancers; these are “health-care amenable” conditions in which mortality can be substantially affected by care (Nolte and McKee 2003), and medical and public health literature show that diet and temperature also have important effects on mortality from such conditions (*e.g.* Gasparini *et al.* 2015; Schwingshackl 2017). Meanwhile, mental and musculoskeletal disorders are low-mortality conditions for which it may be difficult to identify mortality effects even when there are broader positive health impacts (Nolte and McKee 2003). Future research can build on this analysis to continue to explore and suggest potential mechanisms.

VI. Implications

These estimates have important implications for calculating the cost of saving a life-year through DI payments. We use our estimates to calculate how survival curves change when annual DI payments are increased by one dollar beginning with new DI entrants. To evaluate the cumulative impact of DI income, we calculate the area between these survival curves for Years 1 through 16. We scale the change in life-years by the discounted value of DI payments using an illustrative three percent real rate and taking into account the fact that those who have died no longer receive payments.⁴⁵ These results imply that saving a statistical life year requires around \$58,574 in additional expenditure at the lower bend point, and \$236,626 at the family maximum bend point (both $p < 0.05$).⁴⁶ In measuring the value of an additional life year, it is important to consider the quality of life – which may be lower for those with medical conditions – as reflected in a Quality Adjusted Life Year (QALY). Neumann, Cohen, and Weinstein (2014) and Neumann *et al.* (2017) have suggested using \$100,000 or \$150,000 per life year as a benchmark QALY for the general population. They also suggest \$50,000 as a “lower boundary” (*p.* 797). Finkelstein, Hendren, and Luttmer (2015) use \$25,000 for a Medicaid recipient’s valuation of their own life-year. Across all of these ways of valuing life-years, our estimates show that at the lower bend point the cost of saving an additional life year is in a reasonably comparable range to the QALY, while at the family maximum bend point the benefits of additional DI payments are also an important factor relative to the additional outlay.⁴⁷

Less-healthy individuals may have lower QALY valuations. However, Cutler and Richardson (1997) find that, even after adjusting for life quality, QALYs for those with serious health problems would still be at least twice the Neumann *et al.* (2014, 2017) “lower boundary.”⁴⁸ Likewise, lower-income individuals may have lower QALY valuations (Viscusi

⁴⁵ The results (available upon request) are similar under alternative discount rates from one to five percent.

⁴⁶ This \$236,626 at the family maximum bend point is calculated using the combined payments to the primary beneficiary and the dependent; assuming it is only the payments to the primary beneficiaries that reduce their mortality, then saving a statistical life year requires around \$157,751 in additional expenditure at the family maximum bend point. The estimates are insignificant and uninformative at the upper bend point.

⁴⁷ For context, Almond *et al.* (2010) estimate that saving a statistical life through greater Medicaid spending costs around \$550,000, or less than the \$2.7 million value of a statistical life calculated in Cutler and Meara (2000), while Sommers (2017) finds that saving a statistical life through state Medicaid expansions costs between \$327,000 and \$867,000. However, this is a substantially different context than ours, involving publicly provided medical care, as opposed to additional income in our setting.

⁴⁸ Cutler and Richardson (1997) estimate that the average QALY weights for the conditions in which we estimate the largest effects – cardiovascular conditions and cancers – are 0.77 and 0.70, respectively, which are not far from

and Aldy 2003), though VSLYs from individual willingness-to-pay in the low-income Medicaid population are still estimated as in a fairly comparable range (Finkelstein, Hendren, and Luttmer 2015). These considerations further strengthen our argument that the benefits we uncover here are large and relevant for policy decisions.

In a full benefit-cost analysis, it would be necessary to value the opportunity cost of additional DI expenditures, including the effect of higher taxes on the lifespan of those taxed to fund DI income. Such an analysis would also require estimates of additional unknown parameters, such as the effect of DI income on beneficiaries' Medicare expenditures. It is therefore beyond the scope of this paper to provide a full analysis of the costs and benefits of DI payments or their optimal level (*e.g.*, Bound *et al.* 2004, Meyer and Mok 2013, Low and Pistaferri 2015). What we conclude from our illustrative exercise is that the cost of saving a life-year among the lowest-income groups is in a comparable range to QALYs. In Gelber, Moore, and Strand (2018), we are performing illustrative calculations of the implications of our mortality estimates for the optimal DI replacement rate.⁴⁹

We can also use our estimates to calculate the life-years saved by a reform to DI. We focus on a \$1,000 annual increase in DI payments, which is in a comparable range to the increases eventually reached in certain recent policy proposals (*e.g.* Congressional Budget Office 2017). Our estimates over a 16-year period imply that a \$1,000 increase in annual DI payments leads to an increase in lifespan of 0.26 years at the lower bend point and 0.068 years at the family maximum bend point. Using data within our bandwidths of the lower and family maximum bend points, comprising 31,701 individuals on average per newly-entering DI cohort at the lower bend point and 22,133 at the family maximum bend point, this implies an average of 8,216 life-years saved per DI cohort at the lower bend point and 1,497 life-years saved per DI cohort at the family maximum bend point, or 9,713 across both bend points. Although the full lifespan of all individuals is not available since many are still alive as of this writing, given

the average QALY weight for all other conditions, 0.85. Thus, the QALYs in the conditions for which we estimate large effects should be in the same range as the QALYs in the disabled population as a whole.

⁴⁹ The optimal social income insurance replacement rate is judged by trading off the protections they provide through reducing consumption risk against the moral hazard costs (Baily 1978, Chetty 2006, Chetty and Finkelstein 2013). In evaluating this tradeoff empirically, consumption has been measured in a way that excludes health (Gruber 1997, Chetty 2008, Meyer and Mok 2013). If individuals are optimizing, the optimal replacement rate should be the same whether using health or non-health consumption to calibrate the Baily-Chetty formula. However, if individuals are not optimizing then this need not be the case, as we explore in detail in Gelber, Moore, and Strand (2018).

average lifespans we can say that the equivalent number of lifespans saved per DI cohort would be 1-2 orders of magnitude smaller, on the order of 100 to 200. Even limiting attention to those around these bend points shows that the aggregate health benefits of higher DI payments is large.

VII. Conclusion

A key policy question regarding DI is the extent to which DI income affects mortality. Our evidence demonstrates that DI income reduces mortality, particularly among lower-income beneficiaries. In particular, at the lower and family maximum bend points, the point estimates of the elasticities of mortality with respect to DI payments are -0.56 and -0.57, respectively, corresponding to annual mortality reductions of 0.26 and 0.09 percentage points per \$1,000 of annual DI benefits. Meanwhile, we find no significant effect at the upper bend point. We interpret the estimate at the lower bend point as a lower bound due to the measurement issues described above, and we interpret the estimates at all of the bend points as lower bounds on the effect of after-tax benefits. If anything, these factors strengthen our conclusion that the effects are large for lower-income beneficiaries. The results show that this high-mortality, low-income population in the U.S. responds more than more advantaged groups in developed economies today, but similarly to people living in less developed economies around the world today or in earlier time periods in the U.S.

These results imply that the DI payment outlay associated with saving a life-year through additional DI payments is in a comparable range to the QALY at the lower bend point, and that the benefits of additional DI payments are also an important factor relative to the additional outlays at the family maximum bend point. The lifespan gains are therefore a substantial factor in assessing the welfare effects of DI payments. Our estimates imply that within our bandwidths of the lower and family maximum bend points, a \$1,000 annual increase in DI payments would save 9,713 life-years per DI cohort in the years we investigate. This is presumably at least as large as the number of life-years saved in the full DI sample in these years, implying large aggregate health effects of DI payments.

As noted, our estimates are local to the region of the bend points. However, our estimates do demonstrate that large mortality benefits of DI payment size are observed among at least some beneficiaries, whereas previous analyses of the benefits of DI have implicitly ignored such mortality benefits. Moreover, the sample near the bend points we study spans a large fraction of all DI recipients and is therefore of great interest in studying the program. At the same time, our

mortality effect estimates do not necessarily generalize to non-DI populations – indeed this is a prime motivation for studying the DI program separately. DI recipients have particularly high mortality rates, and their mortality probability might be particularly affected by transfer income.

As a function of lifetime income, the concavity of our estimates is akin to the concave correlations in the general population (Preston 1975, Chetty *et al.* 2017). Further investigation of whether the causal relationship is concave in the general population – not only the DI population we have studied – would be valuable.

DI is one important context, but there is a more general lesson from our results: social insurance programs in the modern, developed country context can have large, previously unrecognized welfare benefits due to mortality reductions. This could apply not only in DI but perhaps also in other programs with predominantly high-mortality and/or low-income beneficiaries, such as workers' compensation, sickness insurance, or SSI. Future papers could fruitfully investigate these issues.

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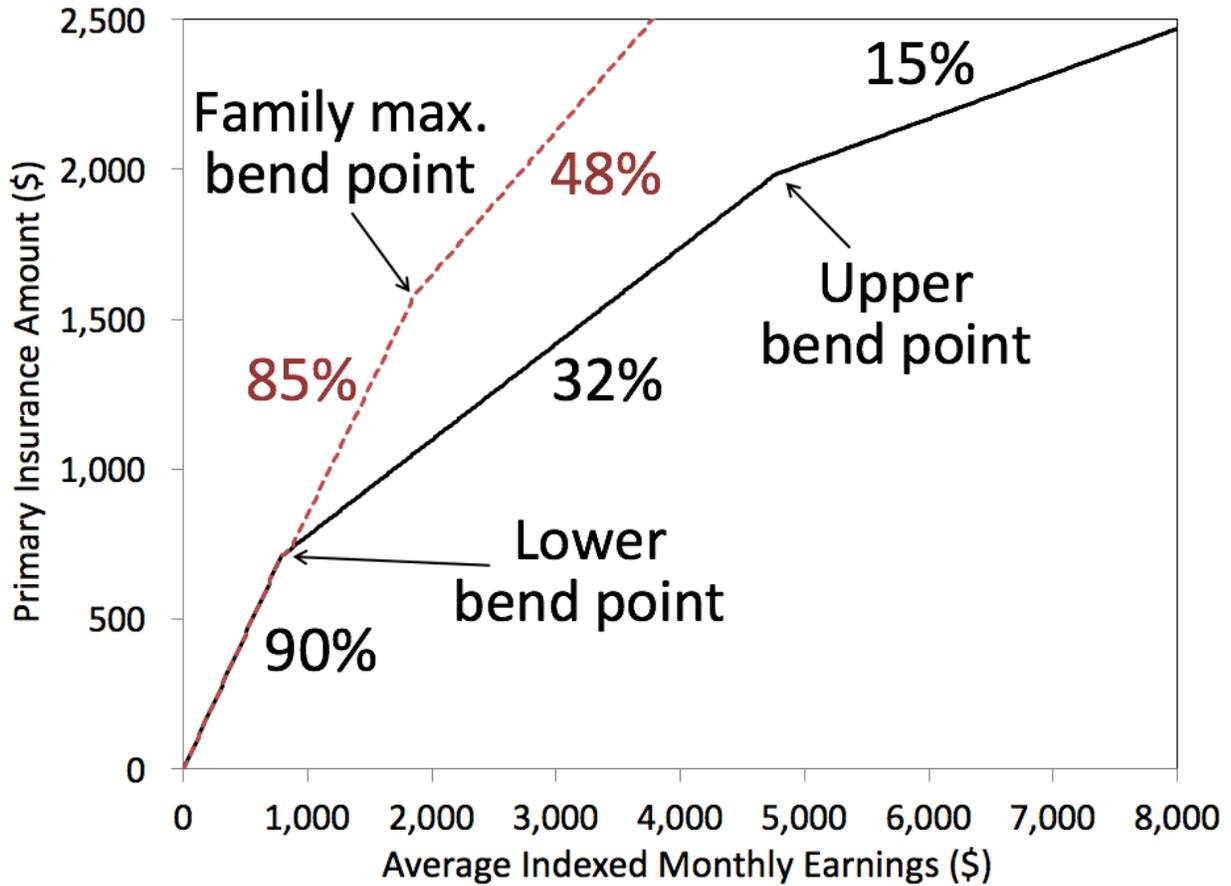
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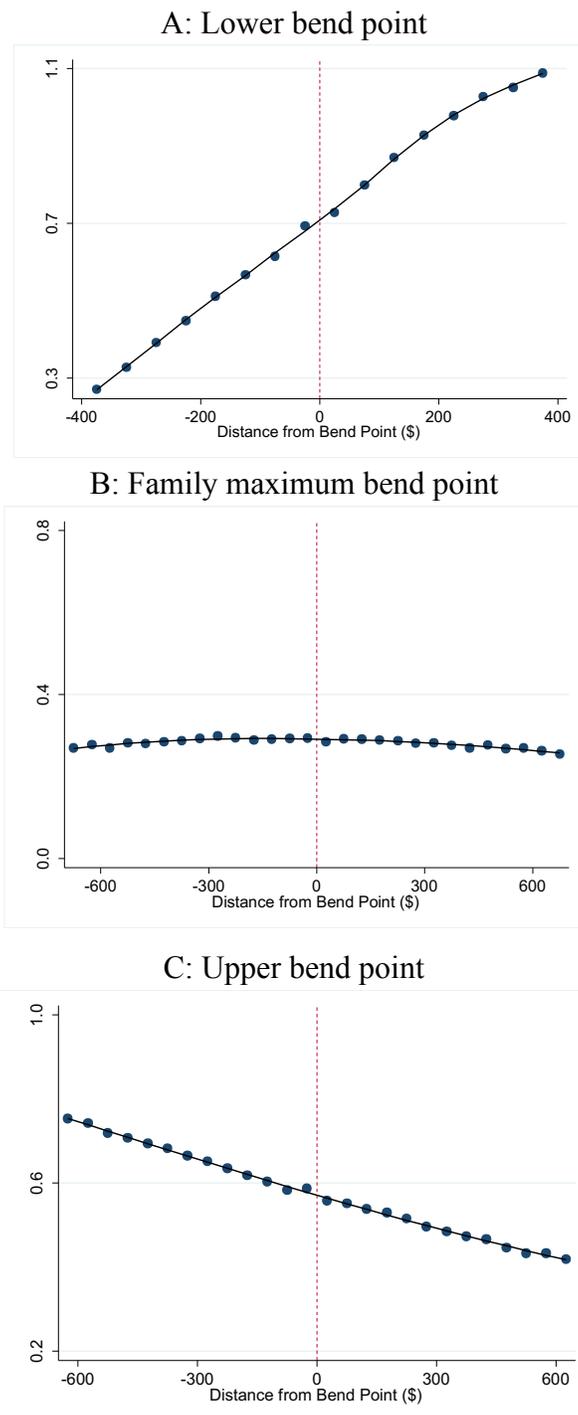
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Figure 1. Relationship of Primary Insurance Amount to Average Indexed Monthly Earnings



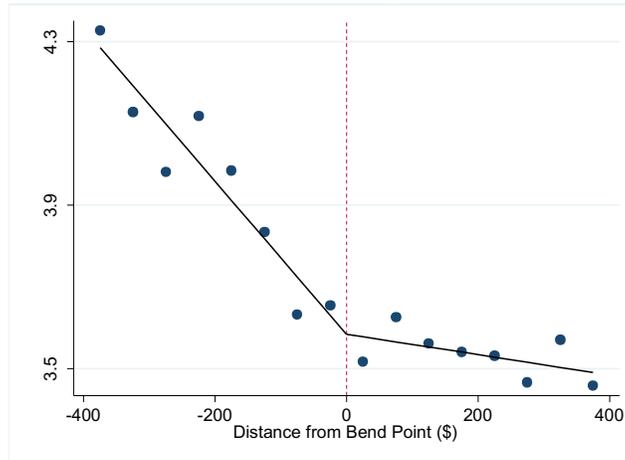
Notes: The solid black line displays the relationship between Average Indexed Monthly Earnings (AIME) and the Primary Insurance Amount (PIA) for beneficiaries. The red dashed line shows the maximum family benefits that can be paid to beneficiaries and their dependents. The family maximum bend point occurs when the binding rule changes from family payments not being larger than 85 percent of AIME to the one that it may not be larger than 150 percent of PIA. This means that the marginal rate changes from 85 percent to 48 percent of AIME (which is equal to 150 percent of the 32 percent replacement rate). The 150 percent rule applies to AIME values higher than this bend point, so at the upper bend point the marginal rate for the family maximum changes from 48 percent (150 percent of 32 percent) to 22.5 percent (150 percent of 15 percent). An AIME at the 30th percentile of the distribution for the full population (combining both those with and without dependents) puts beneficiaries with dependents at the family maximum bend point.

Figure 2. Smoothness of Density around the Bend Points

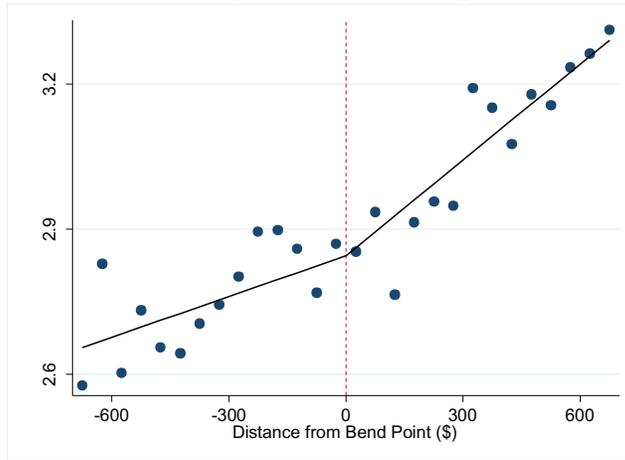


Notes: The figure shows the density of initial AIME in \$50 bins as a function of distance of initial AIME to each bend point. The figure shows the percent of the sample in each bin. The density appears smooth through the bend points, with no sharp change in slope or level. The percent of the sample in each bin is calculated by dividing the number of beneficiaries in each bin by the total number of beneficiaries in the sample. In each panel, we show a range for the density that spans 0.8 percentage points. The best-fit line is a cubic polynomial that allows for a discontinuity in the first derivative above the bend point, which is similar to the approach used for Table 2. The data are from SSA administrative records.

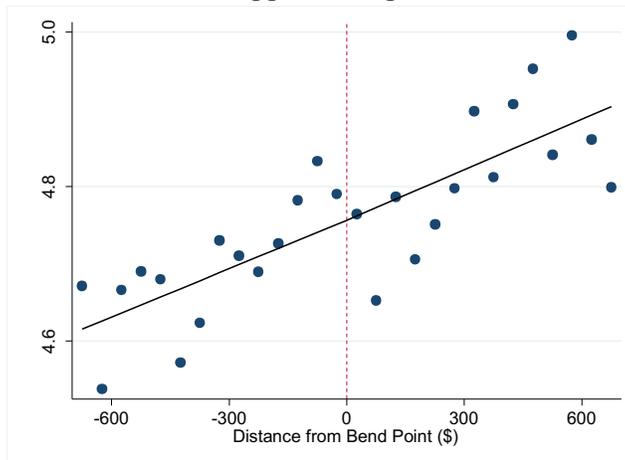
Figure 3. Annual Percent Mortality Rates around the Bend Points
A: Lower bend point



B: Family maximum bend point



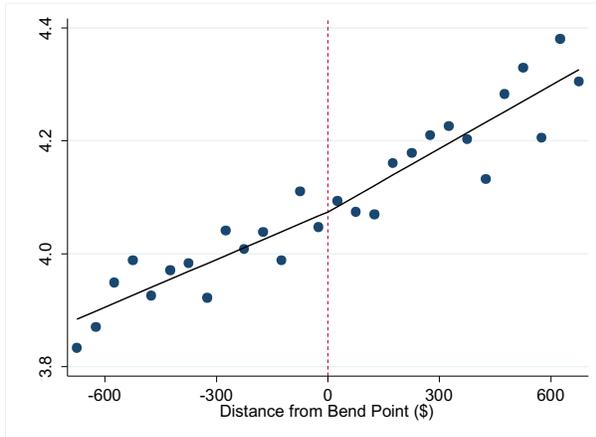
C: Upper bend point



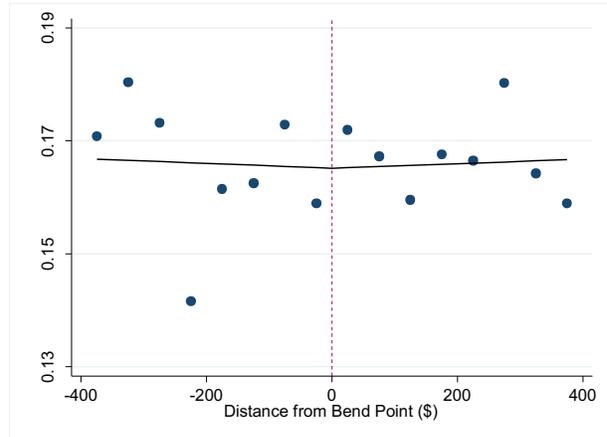
Notes: The figure shows the mean annual mortality rate in percent in the first four years after going on DI, in \$50 bins, as a function of distance of AIME from the bend point. The figure shows that, at the lower and family maximum bend points, the mortality rate slopes upward more steeply above the bend point than below it, with fitted lines that lie close to the data.

Figure 4. Mortality among Placebo Groups

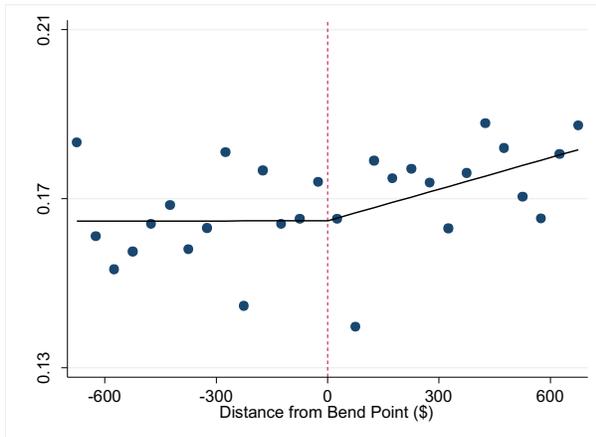
**A: Family maximum bend point –
Beneficiaries without dependents**



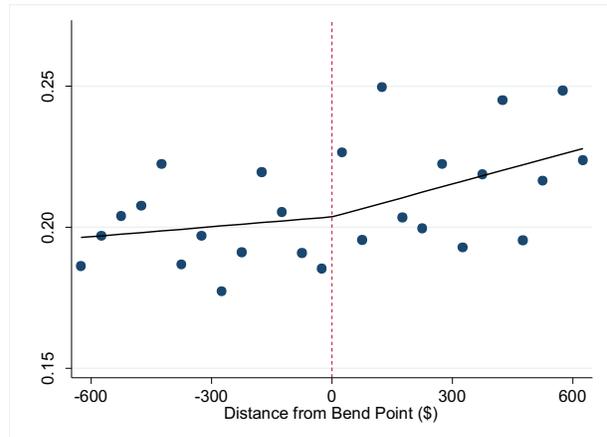
**B: Lower bend point –
Non-beneficiaries**



**C: Family maximum bend point –
Non-beneficiaries**

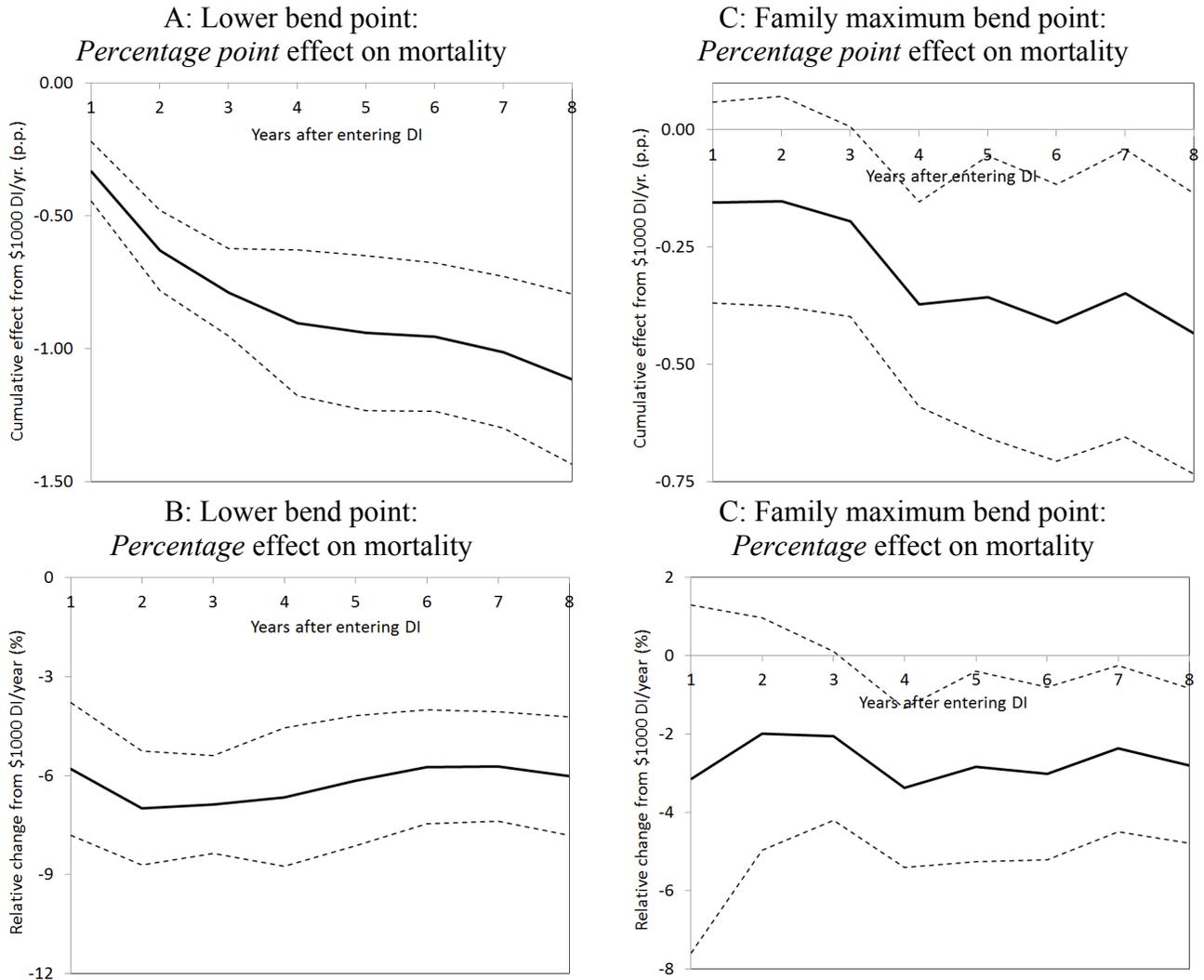


**D: Upper bend point –
Non-beneficiaries**



Notes: The figure shows that we find no noticeable changes in slope in various placebo samples. Panel A shows the sample of DI beneficiaries without dependents. Panels B through D show results from the Continuous Work History Sample One Percent File around each of the three bend points.

Figure 5. Cumulative Effect of DI Benefits on Mortality, Beneficiaries entering DI 1997-2005



Note: The panels show the cumulative mortality effect for each year after receiving DI from Year 1 to Year 8. In this figure we examine beneficiaries entering DI between 1997 and 2005 because this allows us to examine a consistent sample across all years from 1 to 8.

Table 1. Summary Statistics

	Lower bend point		Family max. bend point		Upper Bend point		Full sample	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
<u>Demographic Information</u>								
Age when applying for DI (years)	46.9	9.72	40.8	8.04	50.7	7.16	48.6	8.61
Fraction male	0.231	0.421	0.502	0.500	0.720	0.449	0.531	0.499
Fraction black	0.120	0.325	0.165	0.371	0.122	0.327	0.135	0.341
<u>Program Information</u>								
Primary Insurance Amount (PIA)	\$675	\$126	\$1,091	\$141	\$1,845	\$135	\$1,360	\$480
- Annualized PIA	\$8,105	\$1,514	\$13,098	\$1,691	\$22,134	\$1,614	\$16,315	\$5,764
Fraction allowed DI via a hearing (after an initial denial)	0.317	0.465	0.324	0.468	0.247	0.432	0.283	0.450
Fraction by disability type:								
Musculoskeletal cond.	0.308	0.462	0.262	0.439	0.293	0.455	0.297	0.457
Mental disorders	0.237	0.425	0.281	0.450	0.166	0.372	0.201	0.401
Other disabilities	0.455	0.498	0.457	0.498	0.541	0.498	0.502	0.500
- Cancers	0.103	0.303	0.098	0.297	0.130	0.337	0.116	0.320
- Circulatory conditions	0.077	0.267	0.072	0.259	0.125	0.331	0.103	0.304
<u>Cumulative Mortality Rates</u>								
1 st year after entry	0.062	0.241	0.052	0.222	0.081	0.272	0.070	0.256
2 nd year after entry	0.097	0.300	0.080	0.271	0.125	0.331	0.110	0.313
3 rd year after entry	0.124	0.329	0.100	0.300	0.160	0.366	0.140	0.347
4 th year after entry	0.146	0.353	0.116	0.321	0.190	0.392	0.166	0.372
Observations	412,124		287,723		546,776		3,648,988	

Notes: "SD" denotes the standard deviation. The lower bend point sample includes DI beneficiaries within \$400 of the lower bend point; the family maximum bend point sample includes DI beneficiaries with dependents within \$700 of the kink induced by the family maximum schedule; and the upper bend point sample includes DI beneficiaries within \$650 of the upper bend point. These samples are the same as those considered in our regressions.

Table 2. Smoothness of the Densities and Predetermined Covariates

	Density (x100,000) (1)	Predetermined covariates (x 1,000)					Musculo. conditions (7)	Density of SSI (x100,000) [excluded] (8)
		Age at DI filing (2)	Male (3)	Black (4)	Hearings allowed (5)	Mental disorders (6)		
<i>A: Lower bend point</i>								
Estimated kink (Std. error)	0.378 (0.467)	1.871 (2.949)	0.154 (0.114)	0.051 (0.058)	-0.174* (0.092)	-0.029 (0.081)	-0.044 (0.092)	-1.052 (1.437)
Poly. degree	5	5	5	5	5	5	5	4
<i>B: Family maximum bend point</i>								
Estimated kink (Std. error)	0.063 (0.063)	0.256 (0.947)	0.029 (0.078)	-0.015 (0.049)	-0.007 (0.084)	-0.015 (0.065)	0.037 (0.077)	0.030 (0.074)
Poly. degree	5	5	5	5	5	5	5	5
<i>C: Upper bend point</i>								
Estimated kink (Std. error)	0.058 (0.123)	-1.496 (0.968)	-0.023 (0.072)	-0.020 (0.017)	0.002 (0.067)	-0.014 (0.034)	0.072 (0.045)	0.009 (0.044)
Poly. degree	5	4	5	5	5	5	5	5

Notes: The table shows that the density of the assignment variable (*i.e.*, initial AIME) and distributions of predetermined covariates are smooth around the bend points. We test for a change in slope at the bend point by fitting a polynomial through the data window and allowing for a discontinuity in the first derivative above each bend point. We use cubic, quartic, and quintic polynomials, and report the coefficient and standard error from the polynomial order that minimizes the corrected Akaike Information Criterion (AICc). Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

Table 3. Effect of DI Benefits on Mortality Rates

	RKD estimates			Annual DI pay at bend point (4)	Annual mortality at bend point (p.p.) (5)	Elasticities [linear model]	
	Linear model (1)	Quadratic model (2)	Cubic model (3)			In terms of DI payments (6)	In terms of DI pay + earnings (7)
<i>A: Lower bend point</i>							
p.p. change per \$1,000 of DI	-0.261*** (0.045)	-0.422*** (0.133)	-0.520*** (0.120)	\$8,543	3.58	-0.556*** (0.096)	-0.764*** (0.127)
Bandwidth	\$400	\$450	\$550				
<i>B: Family maximum bend point</i>							
p.p. change per \$1,000 of DI	-0.091*** (0.034)	-0.148* (0.090)	-0.153** (0.078)	\$12,648	2.85	-0.570*** (0.217)	-0.667*** (0.245)
Bandwidth	\$700	\$850	\$950				
<i>C: Upper bend point</i>							
p.p. change per \$1,000 of DI	-0.014 (0.079)	-0.012 (0.228)	-0.075 (0.171)	\$20,777	4.76	-0.063 (0.353)	-0.099 (0.530)
Bandwidth	\$650	\$750	\$1000				

Notes: The table contains coefficients and standard errors showing the estimated effect of increasing annual DI benefit payments by \$1,000 on the annual mortality rate at each bend point. The estimates are based on the regression kink design model (2) in the text. The full set of regression coefficients for the linear model is reported in the Appendix. For the family maximum bend point, the annual DI pay at the bend point refers to the mean annual pay to the primary beneficiary; mean annual pay to dependents is \$6,324. The annual DI payments shown in Table 3 at each bend point differ slightly from the means shown in Table 1 because Table 3 shows the exact benefit amounts, whereas Table 1 shows the means within the bandwidth of each bend point. For calculating the elasticities, we measure earnings in pre-tax terms. See other notes to Table 1. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

Table 4. Placebo Estimates

	Family maximum: DI beneficiaries <i>without</i> dependents (1)	Non-beneficiaries		
		Lower bend point (2)	Family max. bend point (3)	Upper bend point (4)
p.p. change per \$1,000 DI	-0.023 (0.022)	-0.0012 (0.0053)	-0.0056 (0.0051)	-0.0133 (0.0176)
Mortality rate at bend point (p.p)	4.07	0.165	0.165	0.204

Notes: The table shows that we find no significant “effects” in various placebo samples. Column 1 shows the sample of DI beneficiaries without dependents, who show no significant effect of DI benefits on mortality around the family maximum bend point. Columns 2 through 4 show results from samples of non-beneficiaries constructed using the Continuous Work History Sample One Percent File. These placebo samples also show no significant effects. We use the baseline linear specification throughout. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

Table 5. Heterogeneity in the Mortality Effects

Category	Subgroup	Lower bend point			Family maximum bend point		
		p.p. change per \$1,000 of DI (1)	Annual mortality at bend point (p.p.) (2)	p-value on equality of coefficients in group (3)	p.p. change per \$1,000 of DI (4)	Annual mortality at bend point (p.p.) (5)	p-value on equality of coefficients in group (6)
All		-0.261*** (0.045)	3.58		-0.091*** (0.034)	2.85	
Type of DI allowance	Initial DDS allowance	-0.223*** (0.051)	4.69	0.03	-0.140*** (0.050)	3.76	0.02
	Hearings allowance	-0.082** (0.033)	1.25		0.018 (0.029)	0.95	
Race	Nonblack	-0.257*** (0.059)	3.57	0.04	-0.102*** (0.037)	2.80	0.42
	Black	-0.504*** (0.150)	3.72		-0.048 (0.101)	3.06	
Sex	Males	-0.064 (0.104)	3.69	<0.01	-0.149** (0.057)	2.90	0.18
	Females	-0.315*** (0.034)	3.55		-0.044 (0.055)	2.79	
Year began on DI	1997-2005	-0.302*** (0.065)	4.39	0.74	-0.065 (0.045)	3.01	0.32
	2006-2009	-0.257*** (0.050)	2.66		-0.129** (0.053)	2.56	
Category of primary disability	Mental disorders	-0.054 (0.045)	1.00	<0.01	-0.007 (0.033)	0.73	0.03
	Musculo. Conditions	-0.031 (0.029)	1.01		0.032 (0.027)	0.76	
	All other disabilities	-0.372*** (0.085)	6.71		-0.210*** (0.072)	5.35	
	- Cancers	-0.615 (0.404)	17.5		-0.560*** (0.219)	16.2	
	- Cardiovascular conditions	-0.219*** (0.076)	3.98		-0.037 (0.110)	2.95	
Age when filing DI	Age < 45 yrs.	-0.153*** (0.038)	2.03	0.29	-0.053 (0.039)	2.25	0.32
	Age ≥ 45 yrs.	-0.249*** (0.065)	4.39		-0.129 (0.083)	4.12	

Notes: See notes to Tables 1 and 3. The mean mortality rate in Column 2 is measured by the constant in the regression. Column 3 shows the p-value from a test of the hypothesis that the coefficients are equal within each category. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

APPENDIX: FOR ONLINE PUBLICATION ONLY

Appendix 1. Fuzzy RKD specification

Initial AIME is fixed. However, in certain cases AIME can change while a beneficiary is on DI. First, the documented date of disability onset may change through the DI application and award process, thus changing the years on which the AIME calculation is based. This accounts for more than 80 percent of adjustments to AIME. Second, SSA observes earnings with a lag, so additional information on pre-DI earnings may be provided and change the AIME calculation. Third, beneficiaries may have sufficient earnings while on DI to have their AIME updated; our tabulations show that in approximately five percent of cases, AIME is updated for this reason.

The adjustments to AIME are typically minor, so initial AIME measures AIME in subsequent years with only modest error. To account for AIME changes, we also estimate a “fuzzy RKD,” where the “reduced form” model remains (2) but it is scaled by the “first stage” estimates of the change in the slope of mean realized DI benefits while a beneficiary is on DI:

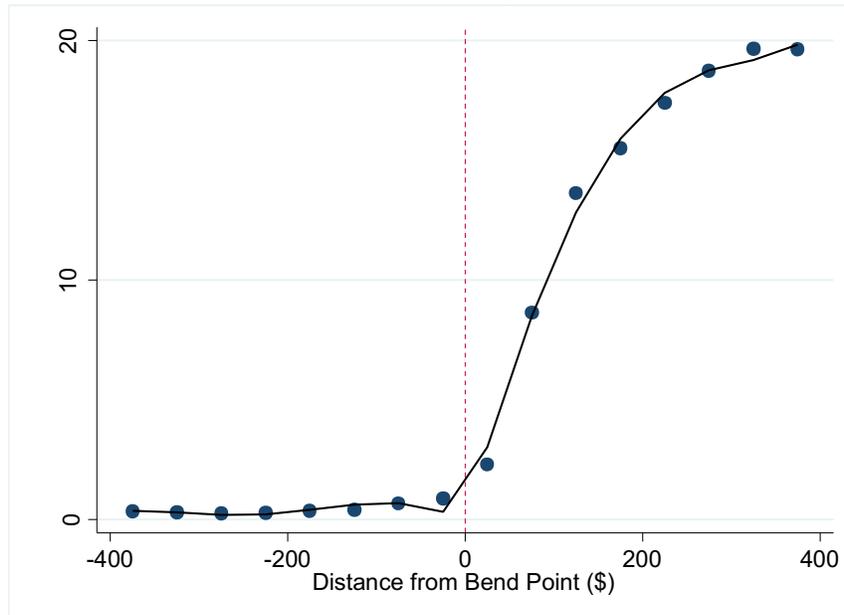
$$Benefits_i = \alpha_0 + \alpha_1(A_i - A_0) + \alpha_2(A_i - A_0)D_i + \varepsilon_i$$

The effect of a dollar of DI benefits on average earnings is then given by β_2/α_2 . However, some of the measured changes in AIME once on DI could be due to measurement error rather than true changes, potentially leading to lack of precision in the first stage.

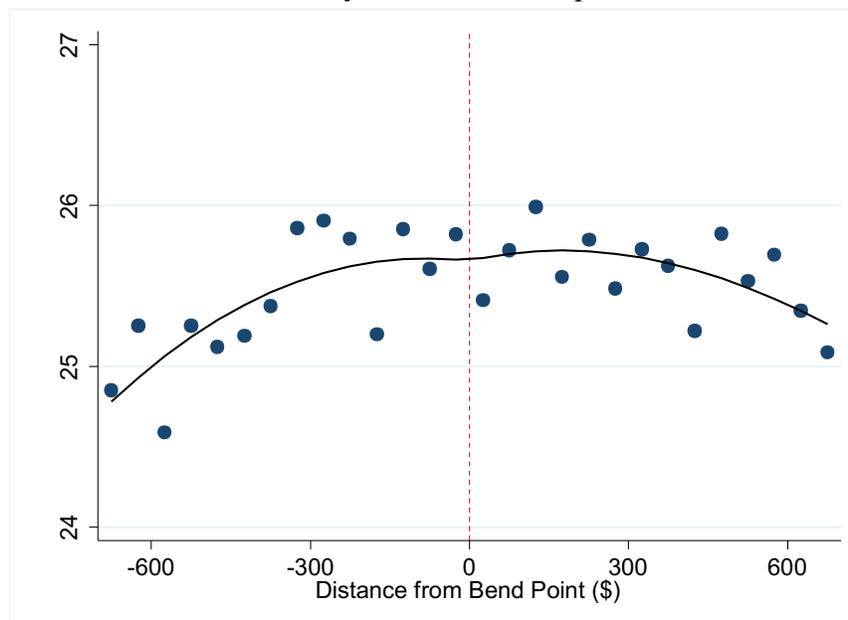
In practice, AIME changes are sufficiently minor that we obtain essentially identical results using the sharp and fuzzy RKD. We use the sharp RKD as our baseline, while also showing the results using the fuzzy RKD.

Appendix Figure A1 Percent of Beneficiaries with Reported Dependents

A: Lower bend point

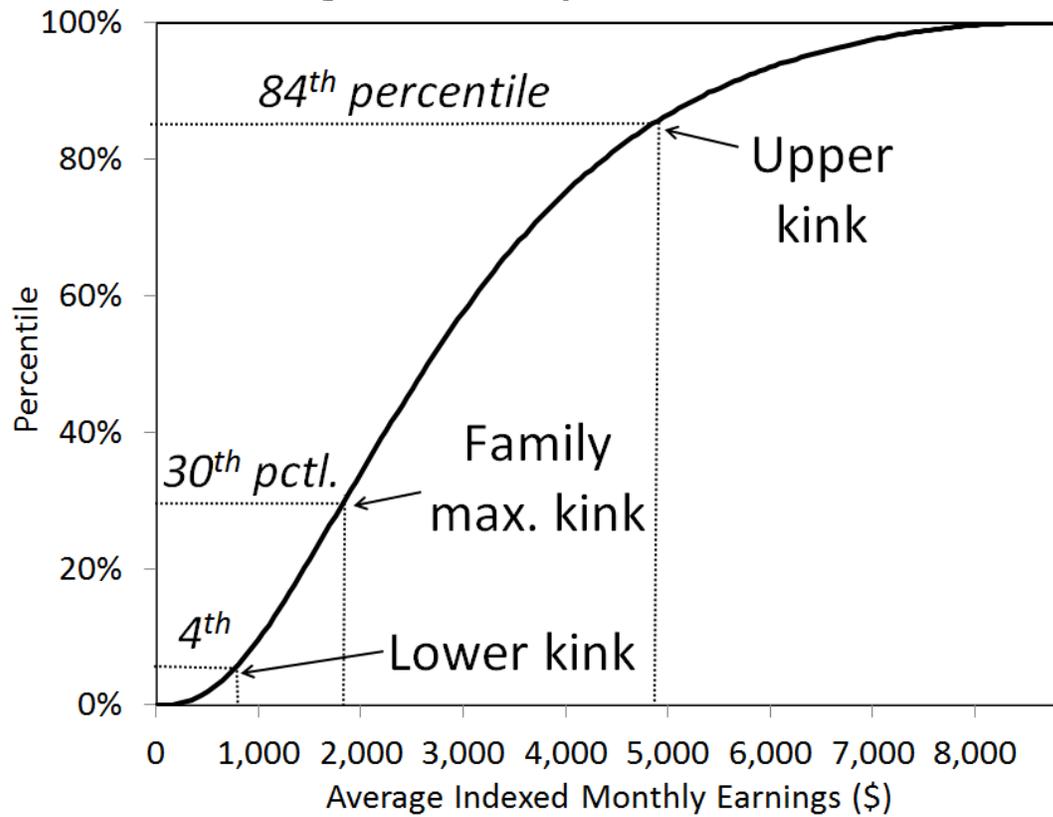


B: Family maximum bend point



Notes: The figure shows the percent of beneficiaries with reported dependents in each \$50 bin around the lower and family maximum bend points. Panel A shows that the percent of beneficiaries with reported dependents rises sharply above the lower bend point, precisely where there are increased incentives to report additional dependents. This is why we cannot select the sample based on number of reported dependents at the lower bend point. By contrast, Panel B shows that the number of reported dependents is smooth around the family maximum bend point, suggesting that the changes in family maximum rules do change the probability of claiming dependents.

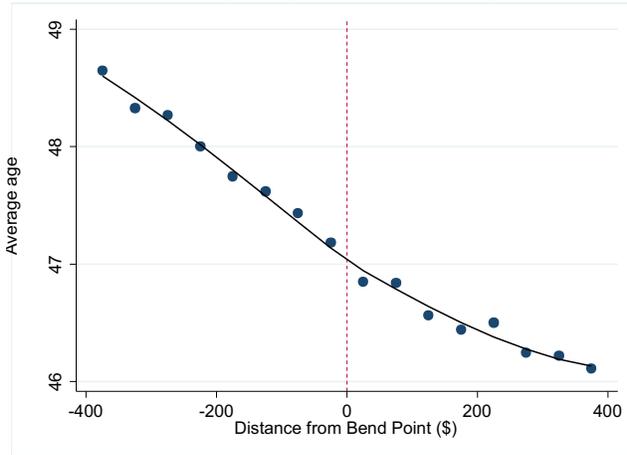
Appendix Figure A2 Cumulative Distribution Function of the Average Indexed Monthly Earnings of new Disability Insurance Beneficiaries



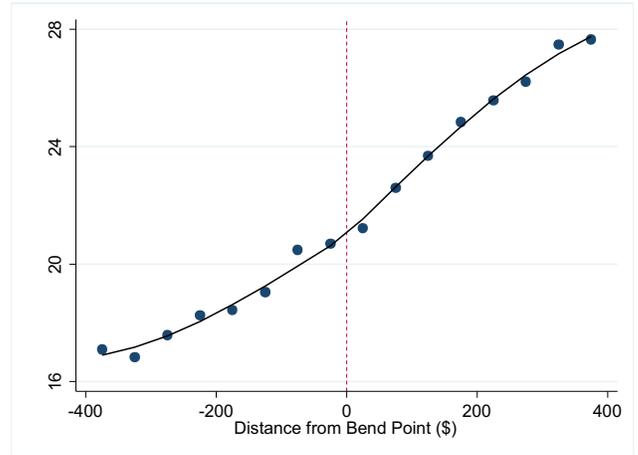
Notes: The source is SSA administrative records on new DI beneficiaries from 2001 to 2007. See the text for sample restrictions and Table 1 for the characteristics of this full sample.

Appendix Figure A3a Distribution of Predetermined Covariates – Lower Bend Point

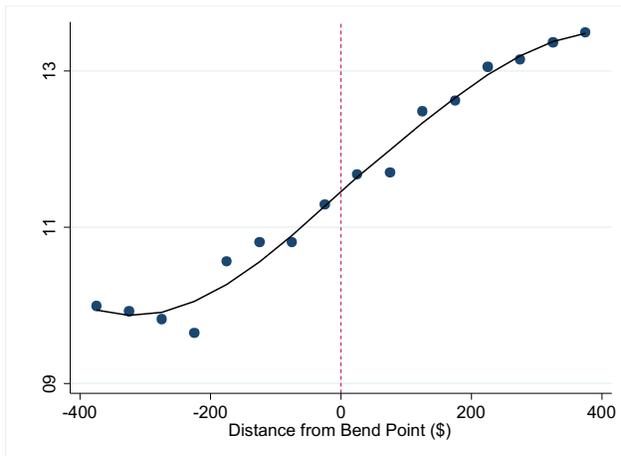
A: Average Age when Starting DI



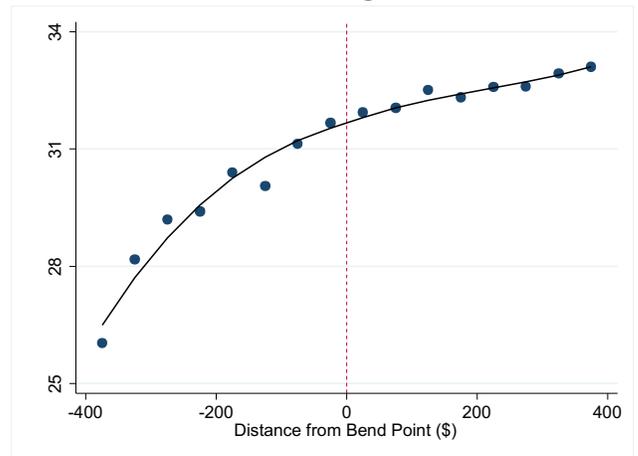
B: Percent of Males



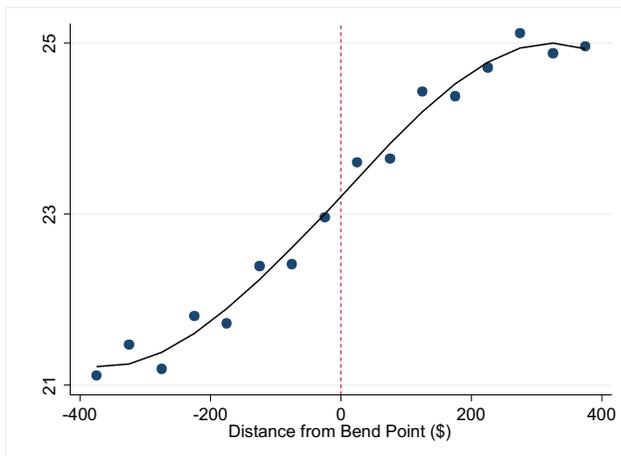
C: Percent Black



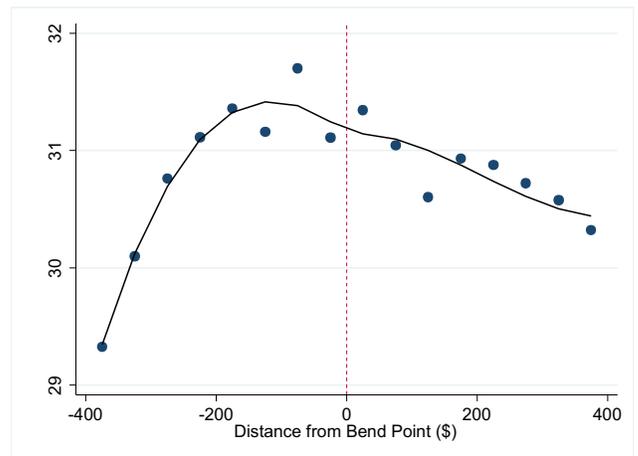
D: Percent of Hearings Allowances



E: Percent with Mental Disorders

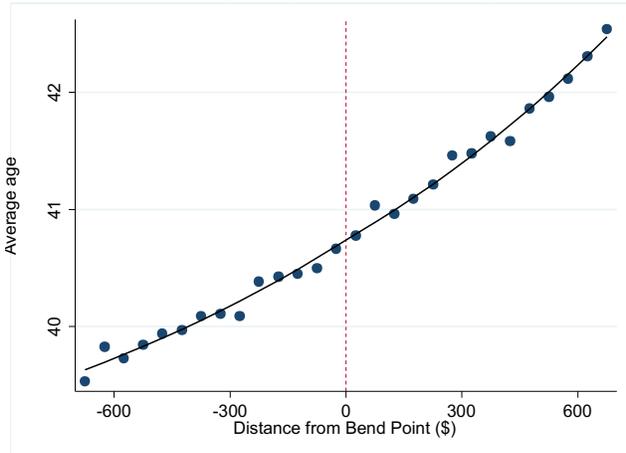


F: Percent with Musculoskeletal Conditions

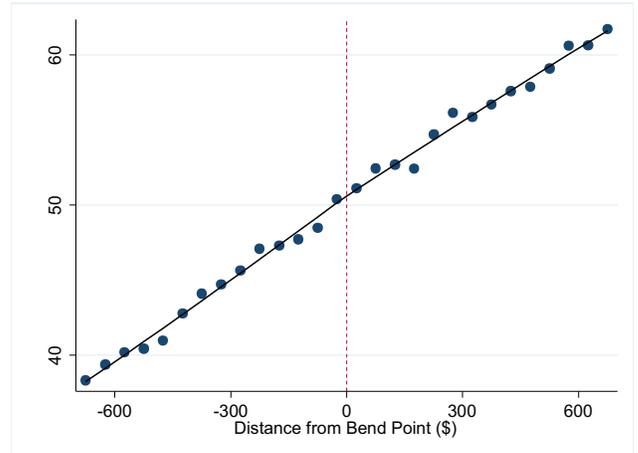


Appendix Figure A3b Distribution of Predetermined Covariates – Family Maximum Bend Point

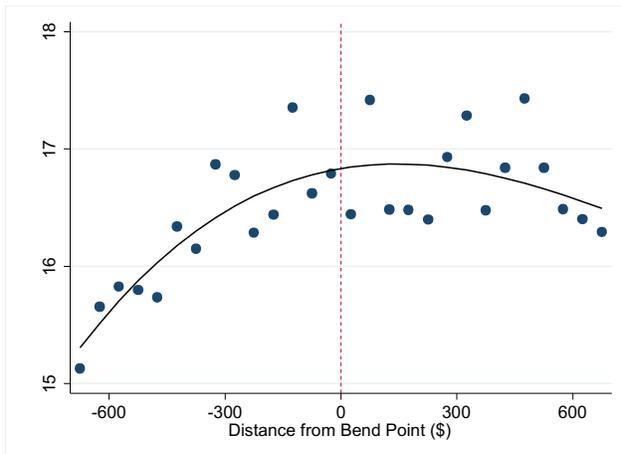
A: Average Age when Starting DI



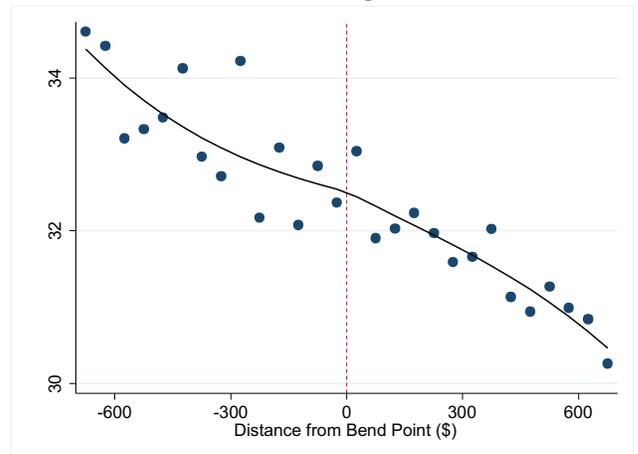
B: Percent of Males



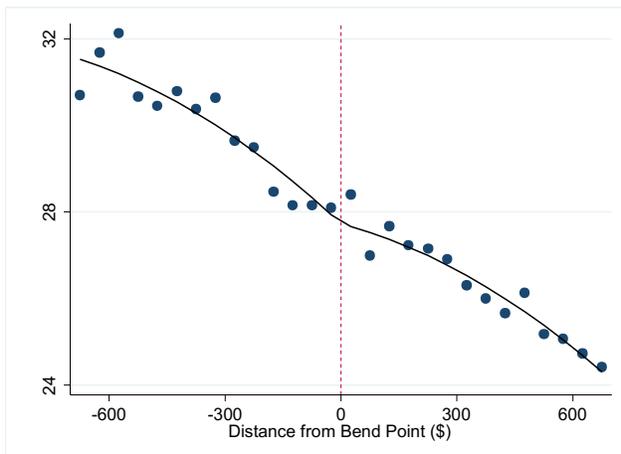
C: Percent Black



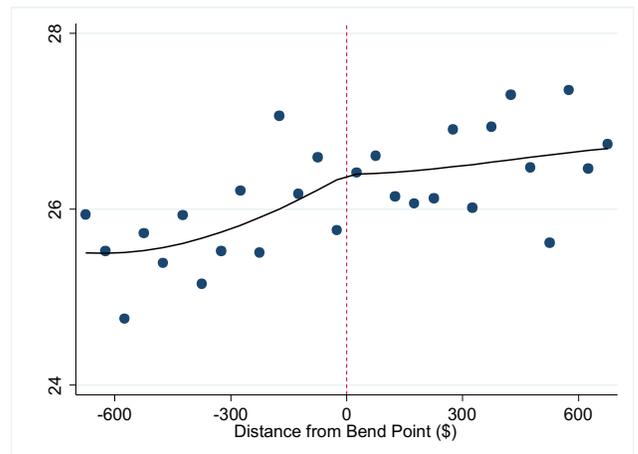
D: Percent of Hearings Allowances



E: Percent with Mental Disorders

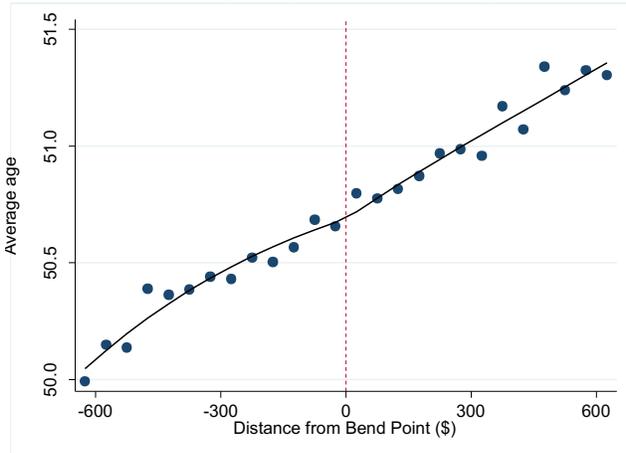


F: Percent with Musculoskeletal Conditions

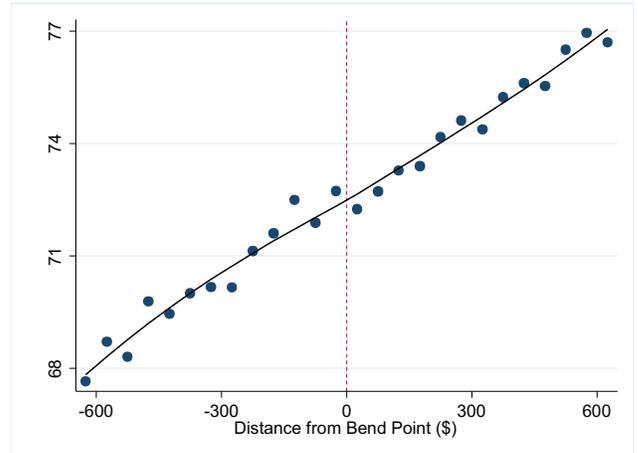


Appendix Figure A3c Distribution of Predetermined Covariates – Upper Bend Point

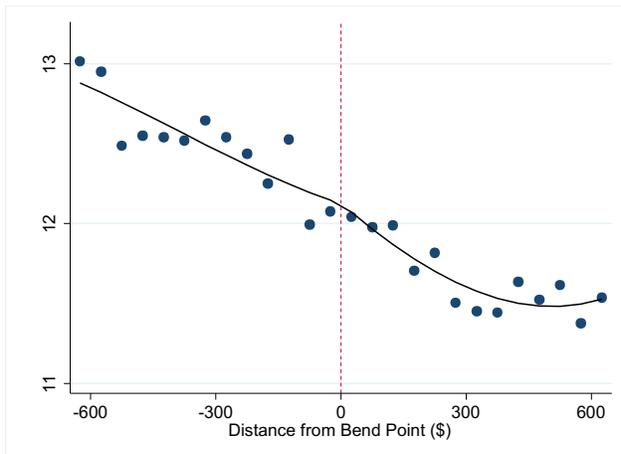
A: Average Age when Starting DI



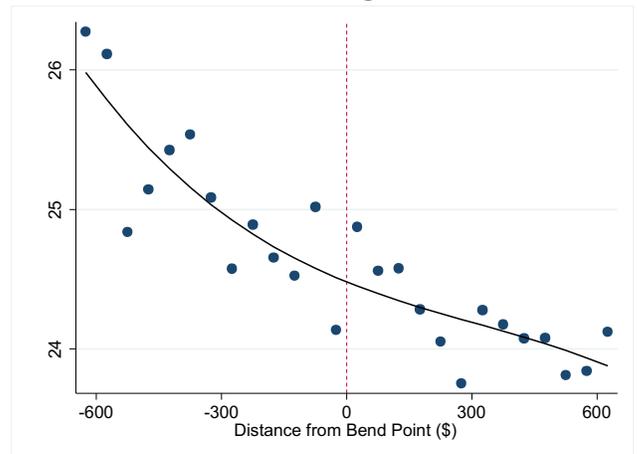
B: Percent of Males



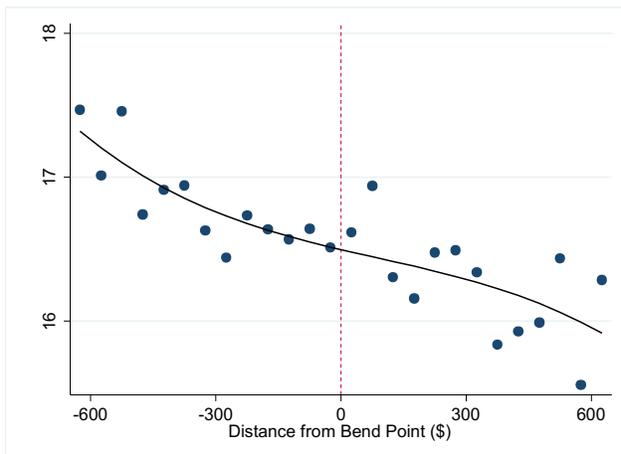
C: Percent Black



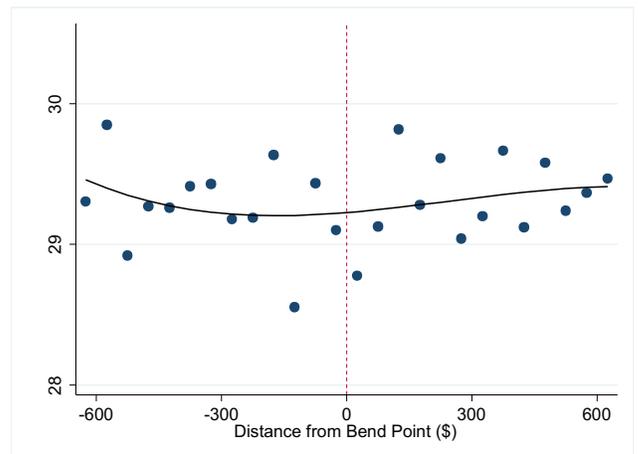
D: Percent of Hearings Allowances



E: Percent with Mental Disorders



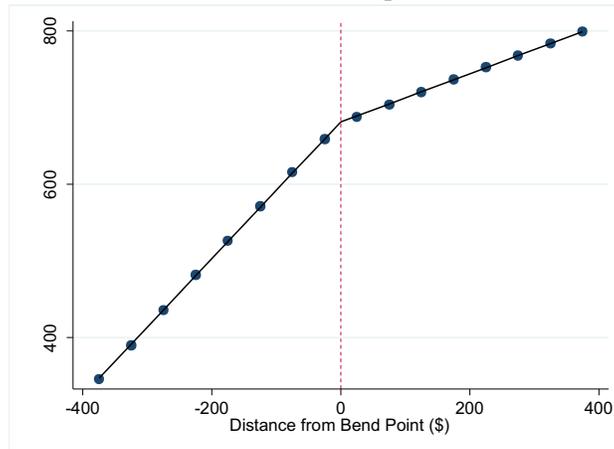
F: Percent with Musculoskeletal Conditions



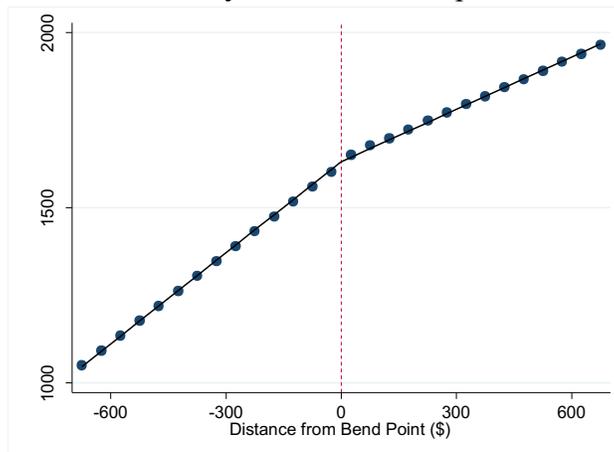
Notes: These figures show the distributions of predetermined covariates in \$50 bins as a function of distance from each bend point. They show that these distributions are smooth in the region of the bend point. The best-fit lines are cubic polynomials.

Appendix Figure A4 Actual Monthly DI Payments as a Function of Average Indexed Monthly Earnings

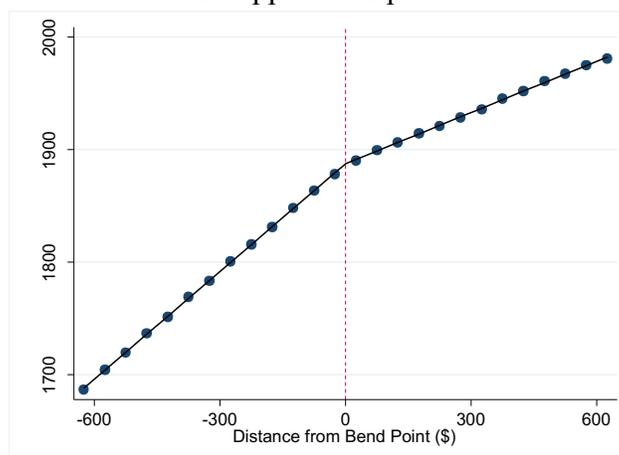
A: Lower bend point



B: Family maximum bend point



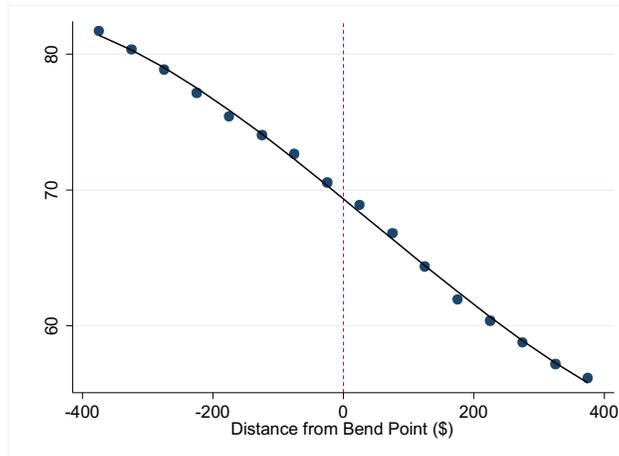
C: Upper bend point



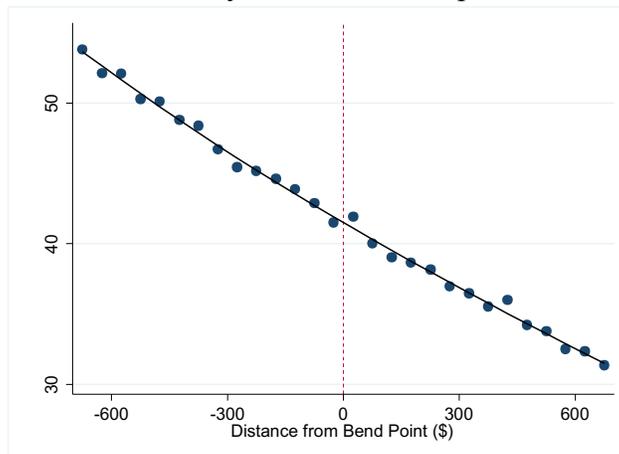
Notes: The figure shows actual DI payments (as measured in our data) as a function of AIME. The figure shows that the effective marginal replacement rate is very close to those indicated by the formulating translating AIME to PIA.

Appendix Figure A5 Smoothness of Dual DI/SSI Recipients [Excluded from Main Sample]:
Percent of those on DI who are Dual-Eligible for DI and SSI

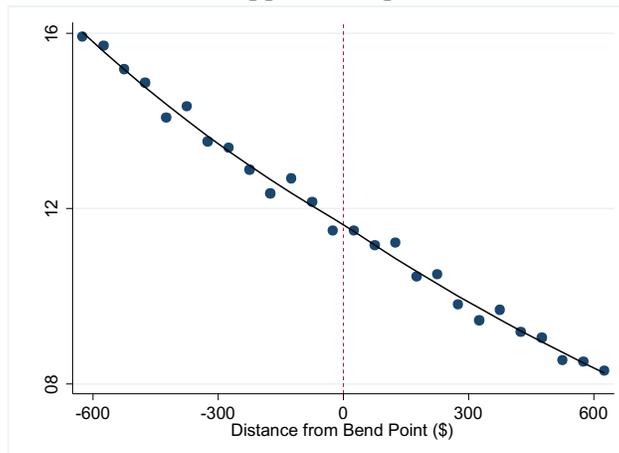
A: Lower bend point



B: Family maximum bend point

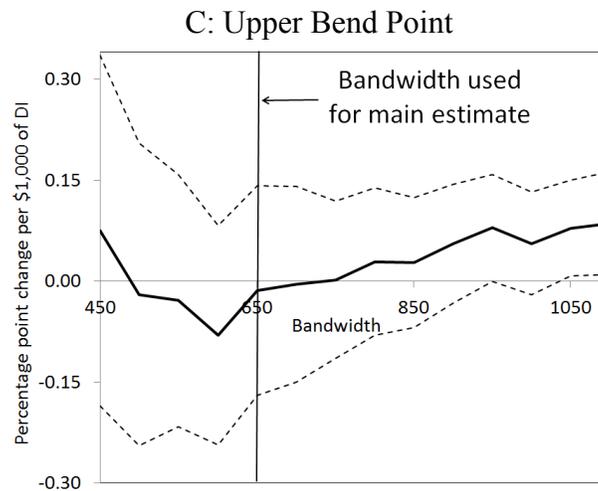
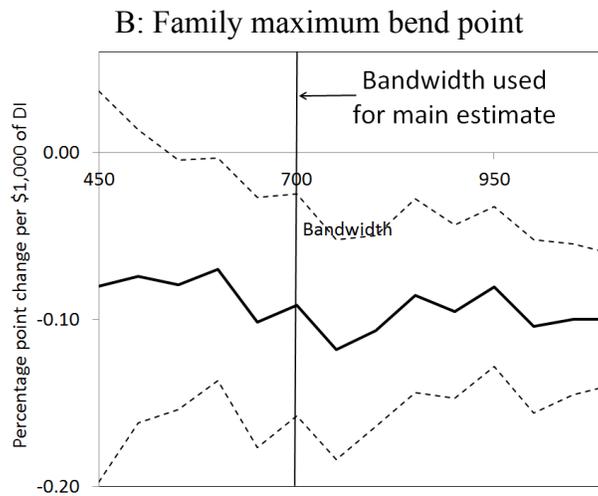
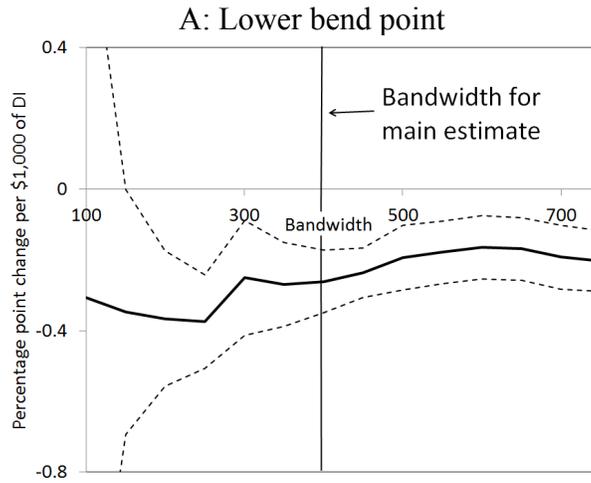


C: Upper bend point



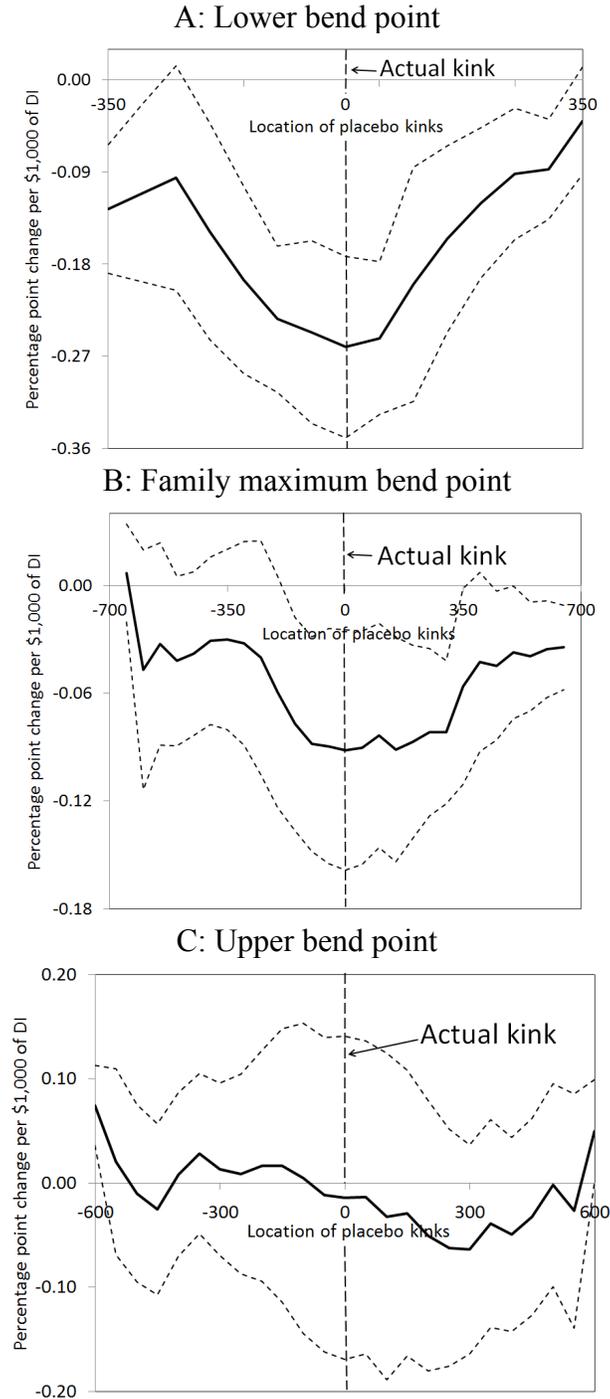
Note: See notes to Appendix Figure A2.

Appendix Figure A6 Mortality Estimates using Varying Bandwidths



Notes: The figure shows that the point estimates at the lower bend point and family maximum bend point are relative stable as a function of the bandwidth chosen.

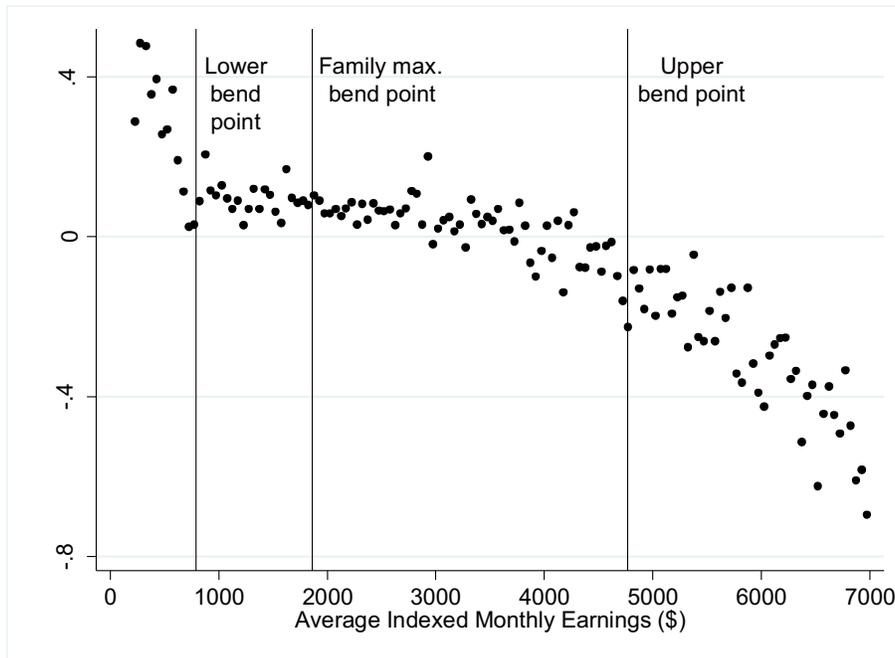
Appendix Figure A7 Mortality Estimates for Placebo Bend Point Locations



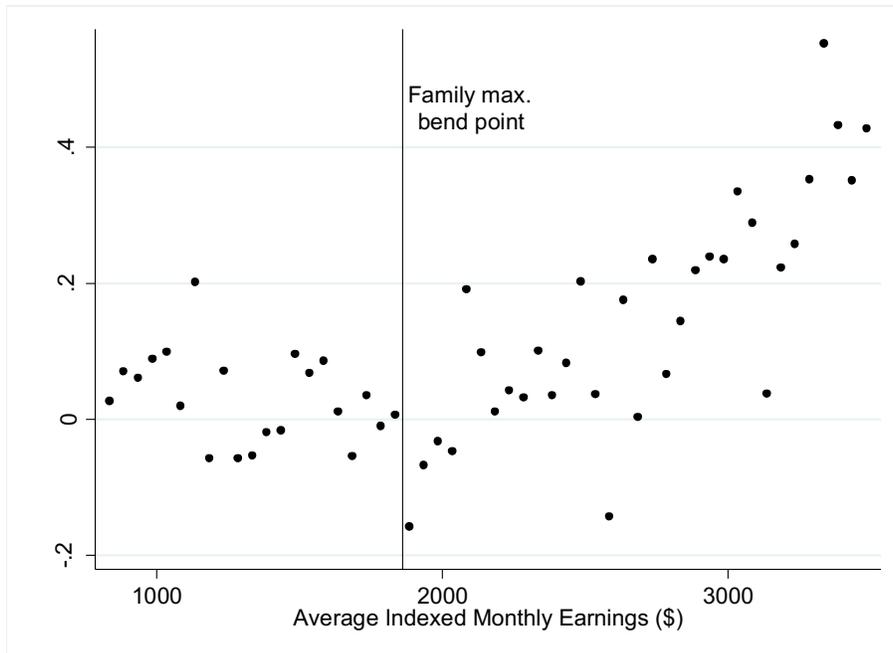
Notes: The figure show the point estimates and 95 percent confidence intervals for the effect of a \$1,000 increase in annual DI payments on mortality in the first four years on DI that is implied by replacing the true bend point in model (2) with “placebo” bend point locations relative to the true bend point (normalized to zero). We use the baseline linear specification. The figure shows that, at the lower and family maximum bend points, the absolute value of the coefficient is maximized at the actual bend point (*i.e.* the coefficient itself is minimized at the actual bend point), supporting the contention that there is in fact a change in slope occurring at the true bend point.

Appendix Figure A8 Distribution of Mortality Rate Residuals Controlling for Age, Year of Entry, State, Male, Black, Allowance Level

A: All Beneficiaries



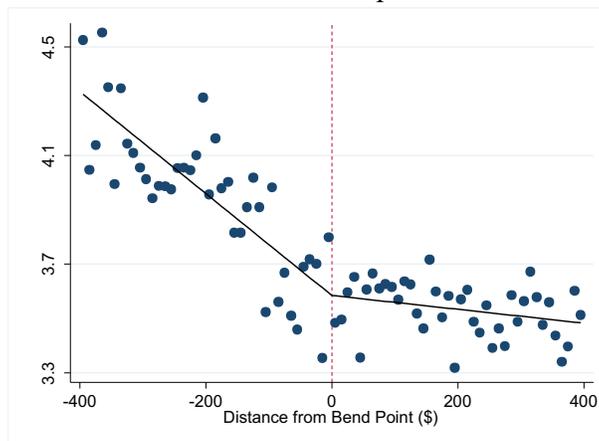
B: Beneficiaries with Dependents around the Family Maximum Bend Point



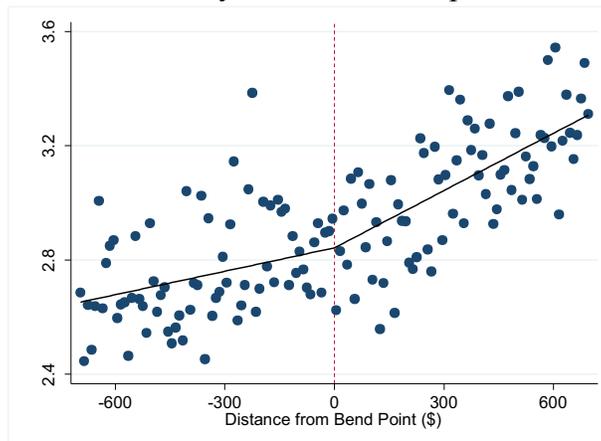
Notes: The vertical lines in the figures show the locations of the bend points. Panel A shows the residuals of the mortality rate after controlling for covariates in individual-level regressions, over the full range of AIME. The figure shows a clear, large discontinuity in the slope of the mortality rate at the lower bend point. Allowance level refers to whether the DI beneficiary was allowed at the DDS or hearings stage of disability determination. Panel B also shows the residuals of the mortality rate after controlling for covariates in individual-level regressions, depicting a clear increase in slope at the bend point when limiting the sample only to beneficiaries with dependents.

Appendix Figure A9 Annual Mortality Rates around the Bend Points – Using \$10 bins

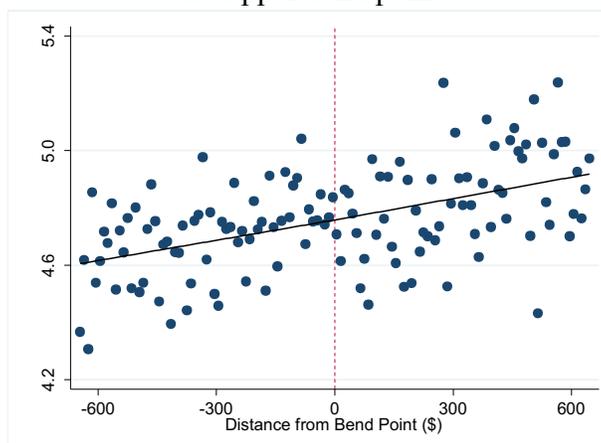
A: Lower bend point



B: Family maximum bend point



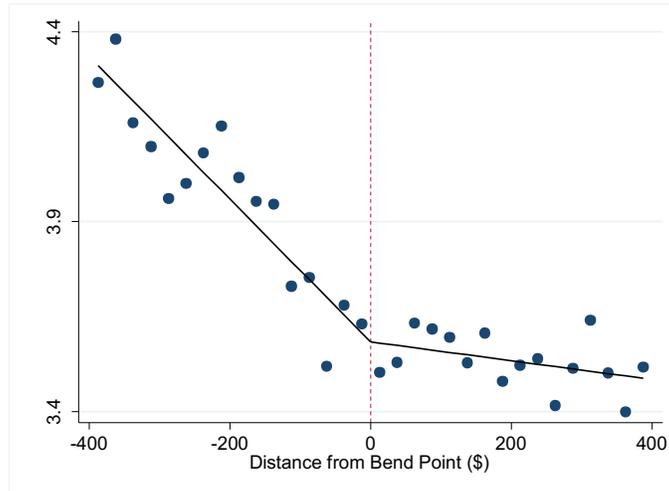
C: Upper bend point



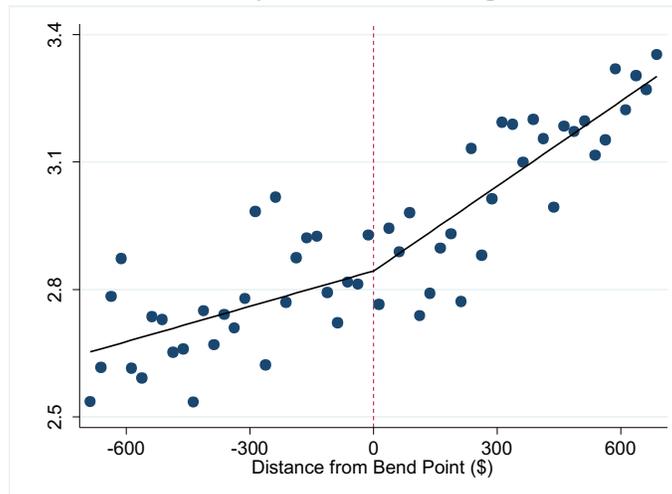
Note: The figure is identical to Figure 3 but uses \$10 bins rather than \$50 bins.

Appendix Figure A10 Annual Mortality Rates around the Bend Points – Using \$25 bins

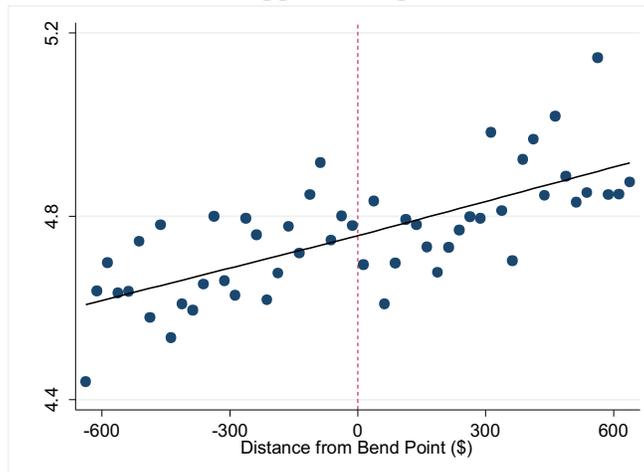
A: Lower bend point



B: Family maximum bend point



C: Upper bend point



Note: The figure is identical to Figure 3 but uses \$25 bins rather than \$50 bins.

Appendix Table A1. Assessing Smoothness of the Fraction with Dependents

	Testing for a discontinuity in percent with dependents		Testing for a kink in percent with dependents (x100) (3)
	Linear model (1)	Quadratic model (2)	
<i>A: Lower bend point</i>			
Estimated discontinuity (Std. error)	0.856*** (0.259)	0.282 (0.521)	--
Estimated kink (Std. error)	--	--	0.151*** (0.027)
Polynomial degree	1	2	5
<i>B: Family maximum bend point</i>			
Estimated discontinuity (Std. error)	-0.017 (0.020)	-0.020 (0.027)	--
Estimated kink (Std. error)	--	--	0.002 (0.004)
Polynomial degree	1	2	5

Notes: The table shows that there is a discontinuity in the level and slope of the percent of the full sample with dependents at the lower bend point, but no such discontinuity at the family maximum bend point. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses. Around the family maximum bend point, the sample includes both those with and without dependents. See notes to Tables 1, 2, and 3.

Appendix Table A2. Economic Characteristics of DI Beneficiaries near the Bend Points

	Lower bend point			Family maximum bend point			Upper bend point			All respondents receiving DI income		
	Median (1)	Mean (2)	SD (3)	Median (4)	Mean (5)	SD (6)	Median (7)	Mean (8)	SD (9)	Median (10)	Mean (11)	SD (12)
<u>Demographic Information</u>												
Fraction married	1	0.654	0.478	1	0.731	0.445	1	0.634	0.484	1	0.618	0.486
Number of children	2	2.010	1.428	3	2.687	1.362	2	1.592	1.307	2	1.728	1.406
Fraction w/o high school	0	0.230	0.423	0	0.151	0.359	0	0.058	0.232	0	0.107	0.309
<u>Wealth (including housing)</u>												
Home ownership rate	1	0.669	0.473	1	0.696	0.462	1	0.847	0.361	1	0.740	0.439
Housing wealth	\$19,708	\$80,866	\$130,927	\$29,021	\$61,157	\$100,676	\$45,233	\$80,591	\$97,855	\$32,467	\$82,017	\$126,173
Non-housing wealth	\$9,701	\$69,896	\$162,893	\$14,368	\$88,865	\$248,411	\$31,894	\$116,614	\$249,652	\$26,630	\$109,681	\$320,580
Total net wealth	\$55,457	\$150,762	\$231,191	\$51,120	\$150,022	\$294,987	\$104,169	\$197,205	\$295,596	\$84,282	\$191,699	\$385,906
<u>Income and income support</u>												
Total income in first month of SIPP	\$904	\$1,482	\$1,766	\$1,460	\$1,808	\$1,598	\$3,350	\$3,723	\$2,644	\$2,456	\$2,592	\$3,038
Fraction receiving food stamps	0	0.073	0.261	0	0.109	0.312	0	0.009	0.081	0	0.038	0.191
Food stamp income in 1 st SIPP month	0	\$21	\$100	0	\$23	\$79	0	\$2.00	\$21	0	\$7.00	\$49
AFDC/TANF recipient rate	0	0.012	0.108	0	0.001	0.017	0	0.002	0.022	0	0.003	0.054
AFDC/TANF income in 1 st SIPP month	0	\$2.35	\$22	0	\$0.70	\$10	0	\$0.68	\$8	0	\$0.96	\$18
Fract. of households w/ only DI income	1	0.502	0.503	0	0.403	0.492	0	0.418	0.495	0	0.429	0.495
Observations	81			101			115			729		

Notes: “SD” denotes the standard deviation. The data are from the Survey on Income and Program Participation (SIPP) (1996, 2001, 2004 and 2008 panels) linked to SSA program management files. We use results using the Census Bureau’s “Completed Gold Standard Files” and do not use respondents with imputed data. All respondents are aged between 21 and 61 in the first month that the SIPP was conducted. The samples around each of the bend points are based on DI payments reported in the administrative data. For each Survey on Income and Program Participation (SIPP) panel (1996, 2001, 2004 and 2008), we take respondents at different sections of the cumulative distribution function of DI payments that match the locations of the bend point samples in the SSA administrative data: the lower bend point sample is between 0.6% and 11.8% of DI payments; the family maximum bend point sample is between 12.9% and 47.6% of DI payments (and conditional on having dependent children under 18 years old); the upper bend point sample is 76.6% and 91.6%. The fraction married and the average number of children is higher for the family maximum sample than the other two groups, which reflects the presence of dependents for the family maximum sample. The number of respondents who meet this criterion relative to DI beneficiaries with dependent payments suggests we are slightly overstating the number of SIPP/SSA/IRS respondents who are actually subject to the family maximum bend point; unfortunately, direct information on dependent DI payments is not available in the SIPP/SSA/IRS files. Dollars are converted to 2013 values. Note that the linked data contain a limited number of variables from both the SIPP and SSA administrative data files; see <https://www.census.gov/programs-surveys/sipp/guidance/sipp-synthetic-beta-data-product.html>.

Appendix Table A3. Summary Statistics for the Consumer Expenditure Survey

Dependent variable	Respondents receiving DI income			Respondents not receiving DI income (4)
	All (1)	Below-median income (2)	Above-median income (3)	
Total expenditures	\$8,686.72 (8,863.33)	\$5,722.35 (5,592.25)	\$11,901.09 (11,137.75)	\$11,546.30 (10,460.05)
Food	\$1,230.86 (794.67)	\$1,006.70 (671.81)	\$1,085.66 (746.09)	\$1,521.17 (1,273.32)
Housing	\$2,525.16 (2,400.98)	\$1,770.90 (1,781.23)	\$3,343.03 (2,884.96)	\$3,469.89 (3,602.42)
Utilities	\$708.38 (483.69)	\$589.24 (435.87)	\$837.57 (539.96)	\$732.28 (516.30)
Home furnishings	\$329.88 (875.94)	\$194.60 (634.57)	\$476.56 (1,122.15)	\$428.86 (1,296.06)
Apparel	\$318.23 (641.47)	\$203.46 (430.36)	\$442.68 (837.28)	\$436.14 (1,115.77)
Transportation	\$2,270.78 (5,787.11)	\$1,420.77 (3,945.94)	\$3,192.48 (75,570.43)	\$2,251.51 (5,389.00)
Health care	\$626.75 (792.68)	\$478.85 (626.22)	\$787.12 (973.62)	\$507.27 (814.21)
Entertainment	\$367.84 (581.33)	\$222.64 (333.73)	\$525.28 (772.42)	\$639.84 (1,575.40)
Personal care	\$61.46 (85.11)	\$38.85 (54.00)	\$85.86 (109.96)	\$79.40 (107.09)
Miscellaneous	\$118.35 (834.03)	\$109.86 (1,033.97)	\$127.55 (540.95)	\$108.37 (1,085.14)
N	673	337	336	19,136

Note: The data are from the Consumer Expenditure Survey from 1986 to 2012, covering all individuals aged 21 to 61. Expenditures refer to real total expenditures last quarter.

Appendix Table A4. Assessing Smoothness of the Densities

	Density of sample (x1,000)		Density of SSI recipients [excluded] (x1,000)	
	Linear model (1)	Quadratic model (2)	Linear model (3)	Quadratic model (4)
<i>A: Lower bend point</i>				
Estimated discontinuity (Std. error)	-0.296 (0.805)	-0.372 (1.518)	-0.097 (1.124)	1.122 (1.600)
<i>B: Family maximum bend point</i>				
Estimated discontinuity (Std. error)	-0.026 (0.029)	-0.017 (0.043)	-0.005 (0.139)	0.108 (0.268)
<i>C: Upper bend point</i>				
Estimated discontinuity (Std. error)	-0.020 (0.089)	-0.126 (0.153)	0.015 (0.071)	-0.006 (0.127)

Notes: The table shows that there is no discontinuity in the *level* of the number of observations per bin, considered as a function of AIME distance to the bend point at any of the bend points. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses. Around the family maximum bend point, the sample is limited to those with dependents. See notes to Tables 1, 2, and 3.

Appendix Table A5. Estimates of Excess Mass in Initial AIME

	Baseline (1)	8 th -degree polynomial (2)	Excluded region \$200 (3)
<i>A. Lower bend point</i>			
γ (x 10,000)	0.543 (1.376)	-0.651 (1.523)	-0.820 (0.612)
<i>B. Family maximum bend point</i>			
γ (x 10,000)	-0.373 (0.385)	-0.213 (0.402)	-0.195 (0.286)
<i>C. Upper bend point</i>			
γ (x 10,000)	0.376 (0.770)	0.563 (0.802)	0.181 (0.448)

Notes: The table shows the point estimates and 95 percent confidence interval on the coefficient on the dummy for initial AIME being near the bend point (reflecting the excess mass per bin near the bend point). For readability, the reported value of γ is the true value multiplied by 10,000. “Baseline” refers to estimating a seventh-degree polynomial through the earnings distribution and estimating the kink from a region within \$100 of the bend point. “Eighth-degree polynomial” (Column 2) estimates an eighth-order polynomial through the density rather than a seventh-order. “Excluded region \$200” (Column 3) refers to estimating the kink from a region of \$200 around the bend point, rather than \$100. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

Appendix Table A6. Effect of DI Benefits on Mortality Rates: Reporting Full Set of Covariates

	Lower bend point			Family max. bend point			Upper bend point			
	Linear model (1)	Quadratic model (2)	Cubic model (3)	Linear model (4)	Quadratic model (5)	Cubic model (6)	Linear model (7)	Quadratic model (8)	Cubic model (9)	
p.p. change per \$1,000 of DI	-0.261*** (0.045)	-0.422*** (0.133)	-0.520*** (0.120)	-0.091*** (0.034)	-0.148* (0.090)	-0.153** (0.078)	-0.014 (0.079)	-0.012 (0.228)	-0.075 (0.171)	
β_2 (x10 ⁴)	0.162*** (0.028)	0.263*** (0.083)	0.327*** (0.076)	0.038*** (0.014)	0.062* (0.037)	0.064** (0.032)	-0.003 (0.014)	-0.002 (0.041)	-0.014 (0.031)	
AIME (x10 ⁴)	-0.187*** (0.018)	-0.238*** (0.043)	-0.290*** (0.044)	0.028*** (0.010)	0.016 (0.021)	0.014 (0.018)	0.023*** (0.008)	0.020 (0.021)	0.017 (0.015)	
AIME ² (x10 ⁷)		-0.131 (0.077)	-0.233*** (0.073)		-0.015 (0.024)	-0.016 (0.017)		0.002 (0.025)	-0.013 (0.015)	
AIME ³ (x10 ¹⁰)			0.230*** (0.073)			0.005 (0.010)			-0.009 (0.008)	
Constant (x 10)	0.358*** (0.003)	0.355*** (0.004)	0.353*** (0.004)	0.285*** (0.003)	0.283*** (0.003)	0.283*** (0.003)	0.476*** (0.003)	0.476*** (0.004)	0.475*** (0.003)	
R-squared	0.935	0.951	0.936	0.874	0.897	0.918	0.697	0.684	0.774	
Bandwidth	\$400	\$450	\$550	\$650	\$700	\$850	\$950	\$650	\$750	\$1000

Notes: The table contains the full results of model (2) for all three specifications at the three bend points. β_2 refers to the change in slope at the bend point, from regression (2) in the main text. The estimates in the first row are equal to β_2 scaled by the decrease in the slope of PIA as a function of AIME at each bend point. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses. For more information, see notes to Tables 1 and 3.

Appendix Table A7. Effect of DI Benefits on Mortality Rates with Discontinuity at Bend Point

	<u>Linear specification</u>		<u>Quadratic specification</u>		<u>Cubic specification</u>	
	Base model (1)	With discontinuity (2)	Base model (3)	With discontinuity (4)	Base Model (5)	With discontinuity (6)
<i>A: Lower bend point</i>						
p.p. change per \$1,000 of DI	-0.261*** (0.045)	-0.261*** (0.047)	-0.422*** (0.133)	-0.423*** (0.139)	-0.520*** (0.120)	-0.537*** (0.146)
Bandwidth	\$400		\$450		\$550	
<i>B: Family maximum bend point</i>						
p.p. change per \$1,000 of DI	-0.091*** (0.034)	-0.092*** (0.034)	-0.148* (0.090)	-0.148 (0.098)	-0.153** (0.078)	-0.154* (0.083)
Bandwidth	\$700		\$850		\$950	
<i>C: Upper bend point</i>						
p.p. change per \$1,000 of DI	-0.014 (0.079)	-0.029 (0.072)	-0.012 (0.228)	0.004 (0.215)	-0.075 (0.171)	-0.106 (0.163)
Bandwidth	\$650		\$750		\$1000	

Notes: The table is identical to the baseline estimates, except that this table adds a dummy reflecting a potential change in the level of the outcome at the bend point. The table shows similar results to the baseline in Table 3. See other notes to Table 3.

Appendix Table A8. Effect of DI Benefits on Mortality Rates – Fuzzy RKD Estimates

	Sharp Linear RKD (1)	Fuzzy Linear RKD (2)
<i>A: Lower bend point</i>		
Change in DI replacement rate at bend point	-0.58	-0.577*** (0.004)
p.p. change per \$1,000 of DI payments	-0.261*** (0.045)	-0.262*** (0.045)
<i>B: Family maximum bend point</i>		
Change in DI replacement rate at bend point	-0.37	-0.368*** (0.037)
p.p. change per \$1,000 of DI payments	-0.091*** (0.034)	0.092*** (0.035)
<i>C: Upper bend point</i>		
Change in DI replacement rate at bend point	-0.17	-0.167*** (0.002)
p.p. change per \$1,000 of DI payments	-0.014 (0.079)	-0.014 (0.081)

Notes: In certain cases, AIME can change while a beneficiary is on DI. To account for AIME changes, we also estimate a “fuzzy RKD,” where the “reduced form” model remains (2) but it is scaled by the “first stage” estimates of the change in the slope of mean realized DI benefits while a beneficiary is on DI. The adjustments to AIME are typically minor, so initial AIME measures AIME in subsequent years with only modest error. Thus, the results are similar to the baseline results reported in Table 3.

Appendix Table A9. Effect of DI Benefits on Residualized Mortality Rates or Using Individual-Level Controls

	Base results: unadjusted mortality rates (1)	Using residuals (2)	Using individual controls (3)
<i>A: Lower bend point</i>			
p.p. change per \$1,000 of DI payments	-0.261*** (0.045)	-0.149*** (0.043)	-0.141*** (0.040)
<i>B: Family maximum bend point</i>			
p.p. change per \$1,000 of DI payments	-0.091*** (0.034)	-0.062** (0.032)	-0.067* (0.035)
<i>C: Upper bend point</i>			
p.p. change per \$1,000 of DI payments	-0.014 (0.079)	-0.014 (0.063)	-0.035 (0.066)

Notes: The table shows the baseline estimates (Column 1), except that this table adds two specifications. First, in Column 2 we use the residuals of mortality after controlling for the bin means of age, as well as dummies for year of entry into DI, state, male, black, and whether the DI beneficiary was allowed at the DDS or hearings stage of disability determination. Second, in Column 3 we run the regressions at the individual level while controlling for age, as well as dummies for year of entry into DI, state, male, black, and whether the DI beneficiary was allowed at the DDS or hearings stage of disability determination. The alternative specifications show similar results to the baseline. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses. See other notes to Table 3.

Appendix Table A10. Robustness of the Mortality Effects

	Main estimate (1)	Grouped logit model (2)	Changing aggregation			Including those with >4 AIME changes (6)
			Individual-level data (3)	Using \$10 bins (4)	Using \$25 bins (5)	
<i>A. Lower bend point</i>						
p.p. change per \$1,000 of DI payments	-0.261*** (0.045)	-0.241*** (0.044)	-0.263*** (0.041)	-0.263*** (0.042)	-0.262*** (0.039)	-0.243*** (0.039)
<i>B. Family maximum bend point</i>						
p.p. change per \$1,000 of DI payments	-0.091*** (0.034)	-0.078** (0.035)	-0.094*** (0.034)	-0.096*** (0.031)	-0.094*** (0.029)	-0.094*** (0.037)
<i>C. Upper bend point</i>						
p.p. change per \$1,000 of DI payments	-0.014 (0.079)	-0.009 (0.079)	-0.006 (0.078)	-0.007 (0.078)	-0.008 (0.079)	0.042 (0.083)

Notes: The table shows that the baseline results on the effect of DI benefits on mortality are robust to other specifications. See the main text for explanations of each specification, as well as other notes to Table 3. We represent the main results in Column 1. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

Appendix Table A11. Placebo Tests of Non-beneficiaries using CCT Bandwidths

	Non-beneficiaries		
	Lower bend point (1)	Family max. bend point (2)	Upper bend point (3)
p.p. change per \$1,000 of DI	-0.0034 (0.0051)	-0.0007 (0.0058)	-0.0025 (0.0041)
Mortality rate at bend point (p.p)	0.0016	0.0017	0.0021
CCT bandwidth	\$500	\$600	\$1,550
Number of non-beneficiaries within range	153,293	277,071	351,266

Notes: The table shows that the placebo tests of non-beneficiaries are robust to using the bandwidths selected by the procedure of Calonico, Cattaneo, and Titiunik (CCT) (2014) in the Continuous Work History Sample One Percent File that we use, rather than using the same bandwidths as our main specification as we do in Table 4.

Appendix Table A12. Cumulative Effect of DI Benefits on Mortality by Years Receiving DI

	After 1 year receiving DI (1)	After 2 years receiving DI (2)	After 3 years receiving DI (3)	After 4 years receiving DI (4)
<i>A: Lower bend point</i>				
Change from \$1,000 of DI each year (p.p.) (a)	-0.393*** (0.063)	-0.725*** (0.095)	-0.920*** (0.108)	-1.044*** (0.181)
Cumulative mortality rate at bend point (p.p) (b)	6.11	9.56	12.12	14.33
Percentage change in mortality [row (a) / (b)]	-6.43*** (1.06)	-7.58*** (1.02)	-7.59*** (0.92)	-7.28*** (1.31)
<i>B: Family maximum bend point</i>				
Change from \$1,000 of DI each year (p.p.) (a)	-0.162 (0.114)	-0.143 (0.120)	-0.182 (0.121)	-0.365*** (0.136)
Cumulative mortality rate at bend point (p.p) (b)	5.08	7.90	9.84	11.38
Percentage change in mortality [row (a) / (b)]	-3.20 (2.29)	-1.81 (1.54)	-1.84 (1.25)	-3.21*** (1.22)

Notes: The table shows the cumulative mortality effect for each year after receiving DI from Year 1 to Year 4, around each of the bend points. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses. See other notes to Table 3.

Appendix Table A13. Effect of DI Benefits on Earnings

	Removing dead from sample		Including dead in sample	
	Mortality bandwidth (1)	GMS earnings bandwidth (2)	Mortality bandwidth (3)	GMS earnings bandwidth (4)
<i>A: Lower bend point</i>				
Cents / \$1 change in DI	-0.41	-7.03*	1.66	-4.83
(Std. error)	(2.02)	(4.27)	(2.86)	(4.08)
Bandwidth	\$300	\$500	\$350	\$500
<i>B: Family maximum bend point</i>				
Cents / \$1 change in DI	3.73	--	-1.20	--
(Std. error)	(3.38)		(2.67)	
Bandwidth	\$850		\$1000	
<i>C: Upper bend point</i>				
Cents / \$1 change in DI	-21.33**	-20.26***	-20.55***	-18.90***
(Std. error)	(10.77)	(2.31)	(6.96)	(1.84)
Bandwidth	\$600	\$1500	\$650	\$1500

Notes: The table shows that we estimate robust significant effects of DI on earnings only at the upper bend point, consistent with our results in Gelber, Moore, and Strand (2017). The estimates differ very slightly from those in Gelber, Moore, and Strand due to slightly different sample selection criteria described in the main text. Robust standard errors [* p<0.10, ** p<0.05, *** p<0.01] are shown in parentheses.

Appendix Table A14. Regressions of Expenditures on Income and Controls

Dependent variable	Respondents receiving DI income			Respondents not receiving DI income
	All	Below-median income	Above-median income	
	(1)	(2)	(3)	(4)
Total expenditures	0.40*** (0.053)	0.44*** (0.11)	0.37*** (0.081)	0.23*** (0.0086)
Food	0.036*** (0.0065)	0.052*** (0.017)	0.038*** (0.010)	0.015*** (0.00079)
Housing	0.11*** (0.021)	0.11*** (0.045)	0.12*** (0.034)	0.073*** (0.0033)
Utilities	0.018*** (0.0031)	0.027** (0.011)	0.015*** (0.0051)	0.0061*** (0.00033)
Home furnishings	0.018** (0.0075)	0.024** (0.012)	0.021 (0.013)	0.014*** (0.0017)
Apparel	0.011*** (0.0031)	0.037* (0.020)	0.0042 (0.0059)	0.014*** (0.0032)
Transportation	0.11*** (0.031)	0.058 (0.035)	0.059 (0.044)	0.031*** (0.0037)
Health care	0.018*** (0.0061)	0.045*** (0.013)	0.020* (0.011)	0.0042*** (0.00055)
Entertainment	0.017*** (0.0052)	0.0039 (0.0086)	0.018*** (0.0067)	0.017*** (0.0017)
Personal care	0.0035*** (0.00058)	0.0038*** (0.0012)	0.0031*** (0.000967)	0.0017*** (0.000085)
Miscellaneous	0.0044* (0.0023)	0.059 (0.044)	0.0045 (0.0030)	0.0016*** (0.00050)
N	544	255	289	17,020

Note: The data are from the Consumer Expenditure Survey from 1986 to 2012, covering all individuals aged 21 to 61. Expenditures refer to real total expenditures last quarter. The table reports coefficients (and standard errors in parentheses) from regressions of expenditures on current income, a dummy for reporting being ill or disabled, age, age squared, sex, dummies for education categories (no high school, high school graduate, college graduate, more than college), race dummies (White, Black, Native American, Asian, multi-racial, not specified), state, and calendar year. The number of observations in the above-median group is modestly larger than in the below-median group because the covariates are missing slightly more often in the below-median group. The number of observations are different here than in Appendix Table A3 because of these missing covariate observations.