The Effect of Disability Insurance Payments on Beneficiaries' Earnings[1]

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Abstract

A crucial issue in studying social insurance programs is whether they affect work decisions through income or substitution effects. The answer is largely unknown for U.S. Social Security Disability Insurance (DI), which is one of the largest social insurance programs in the U.S. The formula linking DI payments to past earnings has discontinuous changes in the marginal replacement rate that allow us to use a regression kink design to estimate the effect of payment size on earnings. Using Social Security Administration data on all new DI beneficiaries from 2001 to 2007, we document a robust income effect of DI payments on earnings. Our preferred estimate is that an increase in DI payments of one dollar causes an average decrease in beneficiaries' earnings of twenty cents. This suggests that the income effect represents an important factor in driving DI-induced reductions in earnings.

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I. Introduction

A core issue in public and labor economics is how public programs affect work decisions. One of the key questions is whether such programs affect work outcomes through income or substitution effects. Separating these is necessary to predict the effects of policy reforms and their welfare consequences. A burgeoning literature is examining whether social insurance programs affect work outcomes through income or substitution effects. For example, Chetty (2008) argues that unemployment insurance (UI) may be less distortionary than previous literature had suggested because the majority of its labor supply effects are due to income rather than substitution effects.

Social Security Disability Insurance (DI) protects workers against the risk of disability through cash payments and Medicare eligibility. Approximately seven percent of the federal outlays are spent on DI and associated Medicare expenses—numerically several times larger than UI. In 2014, U.S. expenditures on UI were $36 billion, whereas expenditures on DI were $213 billion: $145 billion in DI expenses and $68 billion in DI beneficiaries' Medicare outlays. Around five percent of 25-64 year-olds receive DI. Since 1979, the fraction of the population on DI has increased by more than two percentage points, and real expenditures on DI and associated Medicare expenditures have more than tripled (U.S. Treasury 2015, Social Security Administration (SSA) 2015).

It is argued that the growth of DI has played a sizable role in the long-run U.S. trend toward decreasing labor force participation (Parsons 1980, Autor and Duggan 2003). Prior research has established that DI receipt substantially reduces beneficiaries' average employment rates and earnings (*e.g.* Chen and van der Klaauw 2008, Maestas, Mullen and Strand 2013, French and Song 2014, Autor, Maestas, Mullen and Strand 2015).[2] Studies have also shown that DI beneficiaries' employment and earnings respond to DI work rules and the structure of DI payments (*e.g.* Campolieti and Riddell 2012, Borghans, Gielen and Luttmer 2014, Kostøl and Mogstad 2014). Other literature has shown that DI applications and labor force participation are affected by labor market opportunities and DI eligibility rules (*e.g.* Gruber and Kubik 1997, Gruber 2000, Black, Daniel, and Sanders 2002, Autor and Duggan 2003, Karlström, Palme, and

[2] This literature was influenced by the important study of Bound (1989), who found that at most half of DI beneficiaries would work if they were not receiving benefits, as well as Parsons (1980).

Svensson 2008, von Wachter, Manchester and Song 2011, Staubli 2011).[3] Across this literature, decreases in work have often been interpreted as reflecting distortionary moral hazard; for example, Gruber (2013) includes a section on "The Moral Hazard Effects of DI" (*p.* 406).

However, the effects of DI on work estimated in such studies may represent a combination of income and substitution effects. DI creates income effects through the cash and in-kind benefits provided by the program. On average, DI beneficiaries annually receive cash payments of approximately $13,750 and Medicare benefits valued at approximately $7,200 (SSA 2013, Centers for Medicare and Medicaid Services 2014).[4] If leisure is a normal good, these transfers should induce beneficiaries to work less. Substitution effects could arise because earning above the Substantial Gainful Activity limit (SGA), which was $1,040 per month in 2013, can lead to losing DI benefits. This creates an incentive to earn below this level. Separate substitution effects could occur because DI benefits are an increasing function of beneficiaries' lifetime earnings (holding taxes constant).

Autor and Duggan (2007) argue that if DI affects work due to income rather than substitution effects, then in a standard public finance analysis this would have no distortionary impact because income effects simply represent a transfer of resources.[5] Moreover, as with any public program, distinguishing income from substitution effects is crucial for predicting the effects of DI policy reforms on work activity (Hoynes and Moffitt 1999). DI reform proposals have often been focused on improving incentives to work, including U.S. House of Representatives Committee of Ways and Means Chairman Paul Ryan's recent proposal to improve work (*i.e.* substitution) incentives within the Ticket to Work program.[6] However, such a proposal would not increase earnings or participation to the extent that income effects operate. On the other hand, the President's Fiscal Year 2014 Budget proposal to use the chain-weighted Consumer Price Index to calculate the DI Cost-of-Living Adjustment (COLA) would slow the growth rate of DI benefit levels and therefore affect work decisions through an income effect (Office of Management and Budget 2013). To predict the work impacts of such a policy, it is necessary to estimate the income effect of DI.

Despite the importance of estimating the income effect of DI, it has been considered

[3] Many individuals also increase their employment after being terminated from DI (Moore 2015). For a review of earlier work on the impact of DI on work, see Bound and Burkhauser (1999).
[4] We find the value of Medicare benefits by dividing total expenditures, minus the premiums paid by DI beneficiaries, by the number of DI beneficiaries. Here and elsewhere, amounts are expressed in real 2013 dollars.
[5] This assumes no pre-existing distortion.
[6] See http://www.washingtonexaminer.com/gop-plans-overhaul-for-social-security-disability/article/2560440

difficult to do so. For example, Autor and Duggan (2007) write: “The DI program has provided benefits exclusively on a work-contingent basis, so income and substitution effects cannot readily be separated” (p. 120).

The main goal of this paper is to estimate the effect of the magnitude of DI cash payments on beneficiares’ earnings. Our main outcome is pre-tax earnings while on DI, which is relevant to evaluating the net effects of DI expenditures on the government budget, as well as to welfare evaluation (Chetty 2009). We use SSA data on all new DI beneficiaries between 2001 and 2007, and we use a Regression Kink Design (RKD) to exploit discontinuities in the formula relating DI cash benefits to prior earnings (Nielsen, Sorensen and Taber 2010, Card, Lee, Pei and Weber forthcoming). Monthly DI payments are based on a beneficiary’s Primary Insurance Amount (PIA), which is a function of his or her Average Indexed Monthly Earnings (AIME), the average of earnings in DI-covered employment over his or her highest-earning years. This formula is progressive. Figure 1 shows that the marginal replacement rate decreases at two “bend points.” Below a threshold level of AIME (called the “lower bend point”), the marginal replacement rate is 90 percent; between this threshold and the next (called the “upper bend point”), the rate is 32 percent; and above the upper bend point, it is 15 percent. This RKD identification strategy is novel in the DI context.

Although interactions with SSI and other program rules confound the analysis at the lower bend point, the discontinuous change in the marginal replacement rate at the upper bend point allows us to identify the effect of DI cash benefits on beneficiaries’ earnings. With a large sample of 610,271 beneficiaries in the region of the upper bend point, we document a graphically clear, substantial, and statistically robust effect of DI payments on average earnings. A clear increase in the slope of mean earnings at the upper bend point arises for the first time in the year after individuals go on DI and persists in subsequent years. In a baseline specification, the estimates imply that if DI payments are increased by one dollar, beneficiaries decrease their earnings by 20 cents ($p<0.01$). Since mean earnings are low, this corresponds to a large elasticity of earnings with respect to DI payments of -1.92. Our estimates directly answer the policy-relevant question of how changes in benefit payment amounts affect earnings, which is relevant when predicting the earnings effects of a proposal like the chain-weighted COLA. We interpret these results as essentially reflecting only an income effect, because beneficiaries’ earnings are almost always small relative to the SGA limit. We find no evidence that individuals sort around the bend point prior to going on DI. Remarkably, our estimates are similar when we control for linear, quadratic, or cubic functions of the assignment variable; to our knowledge, ours is the first RKD study in which this has been shown. We also conduct several placebo analyses and other robustness checks to verify that we have found a true effect on earnings, as opposed to an underlying nonlinearity in earnings as a function of AIME.

Our estimates rely on clear and robust patterns in the data to estimate the income effect in our context. We isolate the earnings impacts of cash payments in particular, whereas most of the studies of the labor supply impacts of DI cited previously examine the combined effects of cash and medical benefits associated with DI eligibility. A small set of papers examines income effects in other disability contexts. Autor and Duggan (2007) and Autor, Duggan, Greenberg and Lyle (2015) examine an income effect of changing access to Veterans' Administration (VA) compensation for Vietnam War veterans on labor force participation, employment and earnings.[7] Marie and Vall Castello (2012) study the income effect of DI benefits in Spain. Finally, Deshpande (2014) studies the effect of children's SSI payments on parents' earnings. All of these studies find evidence consistent with substantial income effects in these other contexts.[8] Our paper is the first to estimate an income effect specifically in the context of DI in the U.S., which is the largest U.S. federal expenditure on the disabled and one of the largest social insurance programs in the U.S. and around the world.[9]

The remainder of the paper proceeds as follows. Section II describes the policy environment. Section III explains our identification strategy. Section IV describes the data. Section V shows our analysis of income effects. Section VI explores evidence on the extent to which income or substitution effects underlie earnings effects of DI. Section VII concludes. The online appendix contains additional results.

II. Policy environment

DI insures workers for disabilities that are judged to prevent them from earning above SGA. Once on DI, individuals can only work above SGA and retain DI eligibility when they are participating in a Trial Work Period (TWP). A month becomes part of a TWP when monthly earnings are above a level modestly lower than the SGA threshold; in 2013, it was $750. Beneficiaries can complete up to nine months of Trial Work within a rolling 60-month period without putting their DI eligibility at risk. Therefore, the SGA limit is binding only for

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[7] Both studies estimate the reduced-form effects of receiving VA Disability Compensation. Autor *et al.* (2015) conclude that "the effects that we estimate are unlikely to be driven solely by income effects" (p. 3).
[8] In the context of U.S. Civil War veterans, Costa (1995) finds large income effects of pensions on labor supply.
[9] Low and Pistaferri (2012) estimate many parameters simultaneously, including parameters of the work decision.

beneficiaries who have completed a TWP ("TWP completers"). For TWP completers, earning above SGA leads to a review of whether the beneficiary is eligible to continue on DI. A review may be triggered if beneficiaries report monthly earnings above SGA to SSA, or if their annual earnings level reported on tax forms exceeds the annualized SGA limit, $12,480 per year (*i.e.* the monthly limit of $1,040 multiplied by 12) (Schimmel and Stapleton 2011). A substantial percentage of those reviewed are removed from DI; for example, in 2012, 43 percent of these beneficiaries were terminated from the program (SSA 2014b). TWP completers accounted for only 0.9 percent of DI beneficiaries in 2012 (SSA 2013). Among all DI beneficiaries, few have high earnings or exit DI. For example, 0.4 percent of all DI beneficiaries had their eligibility terminated because of substantial work in 2012 (SSA 2013). As DI beneficiaries typically have little to no earnings, they could almost always greatly increase their earnings without triggering a TWP or putting their DI eligibility at risk.

It is complex to calculate DI benefits. For DI beneficiaries who became eligible in 2013, the PIA is calculated as: 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 Figure 1 and SSA 2013). Moreover, calculating AIME requires inflating earnings in each of one's highest-earning years by the National Average Wage Index in each year.[10] Typically, many years go into the AIME calculation: in 2012, 65.5 percent of DI entrants were aged 50 years or older and thus have a relevant earnings history that lasts 28 or more years (SSA 2013). After a beneficiary goes on DI, DI benefits are determined by adjusting PIA through a COLA.

SSI and DI family payment rules can confound the relationship between AIME and benefits received near the lower bend point. SSI provides cash payments and Medicaid to disabled individuals who meet an asset test. In 2013, the federal SSI payment was $710 per month (for those who do not work). Some individuals are dually eligible for both SSI and DI. Dual-eligibles whose PIA is below the federal SSI payment are paid only the SSI payment, and dual-eligibles whose PIA is above their SSI payment only receive DI benefits (after a waiting period). The SSI payment of $710 is nearly identical to $712, the PIA at the lower bend point. Thus, from below to above this bend point the slope of dual-eligibles' net disability benefits (summed over DI and SSI) as a function of AIME increases from zero to 32 percent. However, potentially confounding policy changes occur for dual-eligibles near this bend point, including

[10] The number of years dropped from the full earnings history is determined by the applicant's age and years as a primary caregiver for their children.

moving from Medicaid benefits and a 50 percent cash benefit reduction rate in current earnings (for those on SSI) to Medicare and no benefit reduction below SGA (for those on DI). Moreover, around this bend point beneficiaries can choose outcomes like asset levels to gain eligibility for the program that is more favorable to them, implying that dual eligibility could be endogenous. Finally, for those who are not dual-eligible, the marginal replacement rate decreases at this bend point from 90 percent to 32 percent. Appendix Figure A1 shows that near this bend point, around seventy percent of DI recipients are dual-eligibles. Thus, over both dual-eligibles and non-dual-eligibles, there is little average change in the slope of cash benefits summed across SSI and DI.

DI family payment rules also complicate measurement of the incentives near the lower bend point. The maximum benefits that can be paid to the disabled worker and their spouse and children (the "family maximum") is 85 percent of the worker's AIME, but by statute the family maximum cannot be less than the PIA. The family maximum is equal to PIA below the lower bend point, as PIA is 90 percent of AIME in this range. Once AIME reaches a slightly higher level—$75 above the lower bend point—PIA exceeds 85 percent of AIME, so the family benefit is capped at this level. This means that when considering total family DI payments, the marginal replacement rate is 90 percent of AIME below the lower bend point, 32 percent for the next $75 of AIME, and 85 percent for the next $1,000 of AIME—suggesting that the reaction to the changes in slope may be difficult to detect for this group. Moreover, near the lower bend point, we cannot confidently identify whether a beneficiary has dependents, because the family maximum creates an incentive to report dependents that varies around the lower bend point: additional dependents lead to additional benefits above, but not below, the bend point.

Finally, only a small bandwidth can be used under the lower bend point because AIME is close to zero. All of these factors suggest that *a priori* we do not expect to find meaningful results at this bend point. By contrast, the SSI payment amount is far under PIA for beneficiaries near the upper bend point, implying negligible scope for interaction between DI and SSI. Moreover, only around 10 percent of DI claimants near the upper bend point are dual-eligible (Appendix Figure A1). Finally, near the upper bend point, there is no discontinuous variation in the rules for family DI benefits. Thus, we focus our analysis on the upper bend point.

III. Identification strategy

Card *et al.* (forthcoming) show that, under certain conditions, a change in treatment intensity can identify local treatment effects by comparing the relative magnitudes of a kink in the assignment variable and the induced kink in the outcome variable.[11] This is known as an RKD. Estimates can be interpreted as a treatment-on-the-treated parameter.

In our context, the treatment intensity is the size of DI benefits (*i.e.* PIA), and the assignment variable is AIME when the individual first applies for DI. Our main outcome is mean pre-tax earnings while on DI; this follows Saez (2010) and much subsequent public finance literature using administrative datasets. As a function of AIME, the slope of DI payments changes at the bend point, so we can estimate the causal effect of DI benefits on earnings by comparing the change at the bend point in the slope of earnings to the change in the slope of PIA. If higher benefits cause beneficiaries to earn less on average, then the slope of earnings should increase at the bend point, corresponding to the decrease at the bend point in the slope of PIA.

Mathematically, we want to estimate the marginal effect of DI benefits (B) on earnings (Y) or another measure of work activity. Benefits depend on AIME (A). Using the RKD, we can estimate the effects around a given bend point A_0 as:

$$\mathrm{E}\left[\frac{\partial Y}{\partial B} | A = A_0\right] = \frac{\lim_{\mathrm{A}\to\mathrm{A}_0^+} \frac{\partial \mathrm{E}[\mathrm{Y}|\mathrm{A}=\mathrm{A}_0]}{\partial \mathrm{A}} - \lim_{\mathrm{A}\to\mathrm{A}_0^-} \frac{\partial \mathrm{E}[\mathrm{Y}|\mathrm{A}=\mathrm{A}_0]}{\partial \mathrm{A}}}{\lim_{\mathrm{A}\to\mathrm{A}_0^+} \frac{\partial \mathrm{B(A)}}{\partial \mathrm{A}} - \lim_{\mathrm{A}\to\mathrm{A}_0^-} \frac{\partial \mathrm{B(A)}}{\partial \mathrm{A}}} \tag{1}$$

That is, our estimate of the marginal effect of DI benefits on earnings is the change at the bend point in the slope of earnings divided by the change in the slope of benefits.

Identification of the effect of DI benefits on earnings relies on two key assumptions (Card *et al.*, forthcoming). First, in the neighborhood of the bend point, there is no discontinuity in the slope of the direct effect of AIME on earnings.[12] Second, conditional on unobservables, the density of the assignment variable is smooth (*i.e.* continuously differentiable) in this neighborhood. These assumptions may not hold if we observe sorting in relation to the bend points, as indicated by a change at the bend point in the slope or level of the density of the assignment variable, or by such a change in the distribution of predetermined covariates.

Our assignment variable is AIME from the year of applying for DI ("initial AIME"). Because this is measured before individuals go on DI, it cannot be affected by earnings while on DI. In our context, it would be surprising to observe notable sorting around the bend points prior

[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] For example, beneficiaries' earnings could also be affected by other public programs, or by their marginal product of labor (or hourly wages). We follow Saez (2010) and subsequent literature studying the effects of public programs on earnings in assuming that these factors would jointly have a smooth effect earnings.

to going on DI. Calculating PIA on the basis of an individual's earnings history is complex. This implies that it is difficult for individuals to estimate precisely where their earnings history will put them in relation to the bend points, especially as they are often unaware of relevant Social Security rules (Liebman and Luttmer, 2015).[13] Moreover, individuals 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 decreasing earnings trajectories in the years before applying for DI (von Wachter, Song and Manchester, 2011). A year just prior to applying for DI would typically be among the lowest-earning years and would therefore be excluded from the AIME calculation.

The determination of PIA on the basis of AIME is deterministic; by law, the marginal replacement rate changes around the bend points as described above. Accordingly, our main specification uses a "sharp" RKD where we only need to estimate the numerator of (1), which is the change in the slope of the conditional expectation of earnings at the bend point. If the relationship between an outcome Y and AIME is linear, then we can estimate:

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

where i indexes observations, $D_i = 1[A \geq A_0]$ is a dummy for being above the bend point, and the change in the slope of the outcome at the bend point is β_2. We limit the analysis to observations for which $|A\text{-}A_0| \leq h$, where h is the bandwidth size. As in Card *et al.* (forthcoming), we test for a change in slope by examining whether β_2 is significantly different from zero. We follow Card *et al.* (forthcoming) in using White robust standard errors.

Earnings while on DI are commonly zero, and their distribution is highly skewed; we take the mean of the independent and dependent variables within each bin and run (2) using the aggregated data, weighting each bin by its number of observations. Thus, in (2), i indexes bins. By averaging data within each bin, we estimate standard errors that we view as conservative, following one of Lee and Lemieux's (2010) suggestions in the Regression Discontinuity context. Our main bin size is \$50, the largest size at which all dependent variables pass the two tests of

[13] During our time period, most workers received a Social Security Statement that included an estimate of their PIA if they applied for DI. This estimate could only provide a general idea of their likely benefits, however, as it does not use information on the most recent 2 to 3 years of earnings and used strong assumptions to deal with this and other information gaps (*e.g.*, the Statement assumes the date of eligibility for DI is the current year, whereas in fact it can be up to17 months before or 12 months after filing). The resulting measurement error implies 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, 2013).

excess smoothing for Regression Discontinuity Designs recommended by Lee and Lemieux (2010).[14] Because our outcome is the average earnings over a given period, there is one observation per bin and we do not need to address correlation of errors over time.[15] We also show the results when estimating our regressions at the individual level, or using other bin sizes.

Initial AIME is fixed. However, in certain cases AIME can change while a beneficiary is on DI.[16] 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:

$$Benefits_i = \alpha_0 + \alpha_1(A_i - A_0) + \alpha_2(A_i - A_0)D_i + \varepsilon_i \qquad (3)$$

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.

Aspects of the econometric theory and empirical implementation of RKD have begun to be explored only recently. One is the choice of bandwidth. At the upper bend point, we selected $1,500 as our primary bandwidth, using the graphical patterns as a guide. We show the results across a wide range of bandwidths, including the "data-driven" bandwidths selected by the procedures of Calonico, Cattaneo and Titiunik (2014a, 2014b).

A second issue is how to control for the assignment variable. We call model (2) the "linear" specification because the control for the assignment variable, (A-A_0), is linear. Card *et al.* (forthcoming) use linear and quadratic specifications. Calonico, Cattaneo and Titiunik (2014a) 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 Jaeger (2014) argue that cubic splines

[14] We follow Landais (2014) in applying this to an RKD context.

[15] Results are similar when we use observations for each separate year the outcome is observed, pool the years, include time dummies, and cluster by bin.

[16] 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.

perform better than other estimators. Our approach is to estimate versions of equation (2) with linear, quadratic or cubic controls for the assignment variables.

A final set of issues is whether to allow for a discontinuity at the bend point in the level of the outcome variable, or whether to control for covariates (Ando 2013). We try each option. Thus, for each sample and outcome we will generally produce estimates using nine regressions: the linear, quadratic and cubic regressions; a version of each allowing for a discontinuity in the level of the outcome at the bend point; and a version of each including predetermined covariates.

Interpretation of the RKD estimates

As a benchmark, in Appendix 1 we present a standard lifecycle labor supply model. In the lifecycle model, lifetime wealth affects earnings. Changes in DI payments around the bend points lead to changes in beneficiaries' lifetime wealth and therefore should influence earnings. In this setting, it would be appropriate to calculate the effect of lifetime discounted DI transfer income on earnings. In this setting, under the assumptions of Stone-Geary utility, no uncertainty, and an in Imbens, Rubin, and Sacerdote (2001), we can express earnings in each year as a function of the annual DI transfer payment. We alternatively consider a static framework in Appendix 1, which applies if individuals are myopic or liquidity constrained. In this framework, earnings in a given year instead depend among other things on transfer income in that year, which would motivate calculating the effect on yearly earnings of a marginal change in *contemporaneous* yearly DI payments.[17] Since we do not observe lifetime DI benefits, as a baseline we express the effects as if they arise in the static model or in the Imbens, Rubin, and Sacerdote (2001) framework.

Substitution incentives created by the SGA limit interact negligibly with the income changes we are using, due to several factors. Changes in DI payments due to the change in the replacement rate are small in the local region of the bend point. For example, for a beneficiary whose AIME is $750 above the upper bend point (the midpoint of our baseline bandwidth above the bend point), the change in the marginal replacement rate at the bend point from 0.32 to 0.15 reduces monthly DI income by only $127.5 (relative to having a marginal replacement rate of 0.32 above the bend point).[18] Nearly all DI recipients have low or no earnings, and only a very small fraction are earning near the SGA limit, implying extremely limited scope for this change –

[17] PIA and AIME are monthly measures, and earnings are measured annually. Since the assignment variable is in monthly terms we express earnings in monthly terms by dividing annual earnings by 12. Our regression estimates refer to the additional average earnings over a given time period caused by $1 less in DI over the same time period.
[18] Here -$127.5=$750*-0.17; the change in marginal replacement rate is -17 percentage points (=32 minus 15).

equal to less than one-eighth of SGA – to push desired earnings above SGA. Moreover, beneficiaries can earn over SGA during a TWP without putting their DI eligibility at risk, and the SGA limit binds for only a small fraction – in our sample, only 1.8 percent – of beneficiaries who have completed a TWP. Even among those who have completed a TWP, for whom SGA is binding, we find that many beneficiaries locate *above* SGA (Appendix Figure A2).[19] Thus, to a first approximation we interpret our estimates as representing only an income effect.

Importantly, if hypothetically the SGA limit constrains beneficiaries from increasing their earnings as much as they would in the absence of the limit, then our estimates should reflect a *lower bound* on the income effect.[20] Equally important, regardless of their interpretation, our estimates directly answer the policy-relevant question of how changes in benefit payment amounts affect earnings (without changing substitution incentives at the same time). Thus, the estimates are relevant to estimating the actual effects of proposed policy changes to DI benefit levels (holding substitution incentives as they are in existing policy).

Beneficiaries often are not aware of Social Security rules, and our RKD strategy does not necessarily assume that beneficiaries are aware of the kink in benefits at the bend points. We could observe a response because beneficiaries are reacting, for example, to the amount of DI payments they are receiving, or to their total income, which could be much more salient.

Our estimates represent the effects of changing DI benefit payments while holding other factors constant. 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.

IV. Data

We use SSA data from the 2010 Disability Analysis File (DAF), a compilation of multiple SSA data sources, including the Master Beneficiary Record, Supplemental Security

[19] Appendix Figure A2 also shows little evidence for "bunching" in earnings just below SGA, consistent with the conclusions of Schimmel, Stapleton, and Song (2011). The interpretation of these findings is complicated by the fact that, as in previous literature on earnings around the SGA limit (*e.g.* Gubits *et al.* 2014, Wittenburg *et al.* 2015), we only observe annual earnings, whereas the SGA limit applies monthly. Despite this limitation, note that we still can correctly infer that TWP completers with annual earnings above the annualized SGA limit must be in violation of a monthly SGA limit: if one exceeds the annualized SGA limit, then one must be exceeding the monthly SGA limit *in at least one month of the year*. Moreover, for the substantial fraction of the population that earns the same amount in every month of the year—46.91 percent in the Survey of Income and Program Participation in 2001 to 2007, which provides an illustrative benchmark—bunching below the monthly SGA limit should entail bunching below the annualized SGA limit.

[20] In principle, a cut in benefits in the presence of the SGA limit could lead an individual to move from earning below SGA to earning well above SGA and exiting DI, where another budget set tangency could lie. In this case, our income effect estimates could be larger than those in the absence of SGA. However, as we show, only a negligibly small fraction of beneficiaries earn well above SGA and exit DI.

Record, 831 File, Numident File, and Disability Control File. The DAF contains information on all disability beneficiaries who received at least one month of benefits between 1997 and 2010, and follows outcomes through 2011. It has information on each beneficiary's PIA and AIME; demographics like age, race, and gender; path to DI allowance (*e.g.* whether a claimant was determined eligible by the initial disability examiner or through a hearings-level appeal decided by an Administrative Law Judge (ALJ)); the magnitude of disability payments; and DI program outcomes (*e.g.* whether suspended or terminated for working) (Hildebrand *et al.*, 2012). The data do not contain information on assets, total unearned income from other sources, or hours worked.

Annual taxable W-2 wage earnings through 2011 are obtained by linking to the Detailed Earnings Record (DER). W-2s are mandatory tax returns filed by employers for each employee for whom the firm withholds taxes and/or to whom remuneration exceeds a modest threshold. Our measure of earnings excludes self-employment earnings, as this can often be subject to manipulation (*e.g.* Chetty, Friedman, and Saez 2013); Current Population Survey statistics indicate that only 1.92 percent of the disabled are self-employed.

We use a sample that entered DI between 2001 and 2007 and were aged 21 to 61 years at the time of applying. We choose these years because the rules related to SGA and DI work activity were consistent throughout (after changes in 2000). The restriction to those under 61 avoids interactions with Old Age and Survivors Insurance (OASI) Social Security rules. To focus on beneficiaries whose DI payments are affected by the bend points, we also limit the baseline sample to DI claimants who did not receive SSI at any point in the sample period and who are primary beneficiaries. Following Maestas, Mullen and Strand (2013), the data allow us to examine the four years after DI allowance for each entering DI cohort, meaning the four calendar years beginning with the first full calendar year in which recipients received DI payments (*e.g.* from 2008 to 2011 for the 2007 cohort). Thus, we examine earnings close to when beneficiaries first receive DI and after they have had time to adjust to DI payments and rules.

We clean the data by removing records with missing or imputed observations of basic demographic information (*e.g.* date of birth), 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 outside the range over which the date of disability onset should lie (*i.e.* more than 17 months before or 12 months after the date of filing). This reduces the sample by another 5.5 percent. In addition, we remove beneficiaries who have a PIA based on eligibility for DI under both their record and that of another worker (since total DI benefits may not be a function of one's own AIME in this group), or who had not received DI payments within four years of filing, reducing the sample by another 1.5 percent. We also remove those who died in the years after entering DI, which removes another 14 percent. We eliminate cases in which the data contain unreliable measures of AIME by discarding those with more than four AIME changes, which removes 0.9 percent. 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).

PIA is measured in pre-tax terms. By examining the effect of pre-tax benefits, we answer the policy-relevant question of how a given cut in benefits paid by SSA would affect earnings. Marital status and total family taxable income are not available in our data, preventing us from measuring 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—suggesting that our point estimate of the effect of pre-tax benefits should reflect a lower bound on the effect of after-tax benefits. Appendix Figure A3 shows that measured (pre-tax) PIA in the data changes slope at the bend points in the way the policy dictates.

Table 1 shows summary statistics. We use 610,271 observations around the upper bend point, *i.e.* those for whom initial AIME is within $1,500 of the bend point. Average monthly PIA is $1,773, implying annualized benefits of $21,276. Over the four years before applying for DI, average annual earnings decline from $48,895 to $36,680. Post-award earnings are dramatically lower than pre-application earnings on average: earnings in the four years after first receiving DI are around $2,500 per year. Average annual DI payments are nearly ten times larger than annual earnings. In each of these years, one-fifth to one-quarter of the sample has positive earnings. Average age when applying is 49.8, and 69 percent of the sample is male. Only 0.7 percent of the sample is suspended due to earning above SGA, and only 0.1 percent is terminated from DI.

Since our identification strategy examines earnings patterns around the upper bend point, which is the 82nd percentile of AIME, the estimates will be local to this region. However, the full sample (including those not near the bend points) is similar along most dimensions to those near the upper bend point, except that the upper bend point sample has a higher mean PIA, higher mean pre-DI earnings, and is somewhat more often male. Additionally, the sample around the upper bend point spans from the 59th percentile of AIME to the 95th percentile and represents a substantial fraction of beneficiaries (Appendix Figure A4).

V. Graphical and Regression Analysis of Income Effects

V.a. Preliminary analysis

We begin with validity checks on the empirical method. Figure 2 shows that the number of observations and its slope appear continuous around the upper bend point. Appendix Figure A5 shows that the distribution of six predetermined covariates available in the administrative data—fraction male, average age when applying for DI, fraction black, fraction allowed via hearing, fraction whose disability is a mental disorder, and fraction whose disability is a musculoskeletal condition—appears smooth through the bend point. Table 2 confirms that the number of observations, these predetermined covariates, and the fraction of the sample on SSI (prior to their exclusion) are all smooth through bend point. Similarly to Card *et al.* (forthcoming) and Turner (2014), for each of these dependent variables separately, we examine the coefficient β_2 when we run regressions with polynomials in AIME of each order between three and 12. For each dependent variable, we report β_2 for the polynomial order that minimizes the finite-sample corrected Akaike Information Criterion (AICc). Using a baseline specification without additional controls and with no discontinuity in the dependent variable at the bend point, none of the specifications shows that β_2 is statistically different from zero at the five percent level. Moreover, these regressions are rarely statistically significant for any polynomial order.

We can also examine whether "bunching" occurs in the density of initial AIME around the convex kink in the budget set created by the reduction in the marginal replacement rate around a bend point, because earning an extra dollar that increases AIME leads to a greater increase in DI benefits below the bend point than above it.[21] As described in Appendix 2, standard theory shows that if beneficiaries respond to the incentives this creates before going on DI, then initial AIME should "bunch" around the bend point because the smaller marginal replacement rate above the bend point makes it less worthwhile to earn more than below the bend point (*e.g.* Hausman 1981).

Following Saez (2010), we estimate the extent of such bunching by fitting a smooth polynomial to the earnings density away from the kink and estimating the "excess mass" that occurs above this smooth polynomial in the region of the kink. Specifically, for each earnings bin z_i of width k we calculate p_i, the proportion of the sample with earnings in the range

[21] Working more will not lead to higher DI income if earnings are not in the highest-earning years used to calculate AIME. However, as long as the prevalence of such cases evolves smoothly through the bend point (consistent with our data), the substitution effect should still lead to a greater incentive to earn below each bend point than above it.

[z_i-$k/2$,z_i+$k/2$). The earnings bins are normalized by the distance the bend point, so that for z_i=0, p_i is the fraction of people with earnings in the range [0,k). We run the following regression:

$$p_i = \sum_{d=0}^{D} \beta_d (z_i)^d + \sum_{j=-k}^{k} \gamma 1\{z_i = j\} + \varepsilon_i \qquad (4)$$

This expresses the earnings distribution as a degree D polynomial, plus indicators for each bin within $k\delta$ of the kink, where δ is the bin width. Using the bandwidth of \$1,500, in model (4) we estimate the coefficient γ on a dummy for having final AIME within \$100 of the kink, while controlling for a baseline seventh-degree polynomial through the density of AIME (following Chetty, Friedman, Olsen, and Pistaferri 2011). γ reflects the excess density near the kink.

Table 3 shows that the resulting estimates of γ are precise, insignificant and very small. For example, in the baseline the mean density in the two bins surrounding the excluded region is 895 times larger than γ.[22] These conclusions hold through variations on the baseline estimates: controlling for covariates; using an alternative bandwidth; controlling for an eighth-degree polynomial; and defining the kink as a larger region around the bend point. Consistent with the exposition of the models in Appendix 1, this finding could reflect that future DI claimants do not anticipate or understand the DI income they will receive or that they do not react to the substitution incentives even when correctly anticipating them.[23]

V.b. Main Results

Figure 3 shows average earnings in the four years after DI allowance around the upper bend point. As we would expect if DI payments reduce earnings, the slope clearly increases at the upper bend point and the empirical observations lie close to the fitted lines.

Table 4 shows the estimated earnings effects when we implement the nine regression specifications described earlier. We report the implied effect on earnings of increasing DI benefits by one dollar, under the sharp RKD assumption that the marginal replacement rate changes from 0.32 to 0.17 at the upper bend point. (Appendix Table A2 shows the actual regression estimates we use to generate the implied effects in Table 4.) In our baseline specification, increasing DI benefits by one dollar leads to a substantial decrease in earnings of 20.28 cents at the upper bend point (p<0.01). As the marginal replacement rate *falls* at the bend point, this is consistent with the graphical evidence showing an *increase* at the bend point in the

[22] In Appendix Table A1, we also test for a discontinuity in the level of the number of observations and find no significant discontinuity across any of the specifications at the upper bend point.

[23] In the context of bunching in initial AIME, it is not straightforward to translate γ into a substitution elasticity as in Saez (2010), because it is unknown when individuals anticipate going on DI.

slope of average earnings as a function of AIME. Mean earnings are low, so the implied elasticity of earnings with respect to DI benefits, -1.92, is large.

The estimates are similar when we allow for a discontinuity at the bend point (Column 2) and when controlling for predetermined covariates (Column 3). The estimates are modestly larger under the quadratic and cubic specifications in Columns 4 to 6 and 7 to 9, respectively. Across all nine specifications, the point estimates are relatively stable and range from -19.25 to -27.38 cents (p<0.01 in all nine cases). It is striking that the estimates are so robust when we control for linear, quadratic, or cubic functions of the assignment variable. In other RKD contexts surveyed in Ganong and Jaeger (2014), nearly all studies control for only linear and/or quadratic functions of the assignment variable (although it is possible the results in some of these studies would be robust to controlling for a cubic function). The linear specification without additional controls minimizes the AICc, so we focus on this specification as a baseline. Table 4 also shows that the estimates are remarkably stable across individual years, with baseline estimates that range between -18.29 cents in the third year and -23.02 cents in the first year. Within each year, the estimates are generally stable across all nine specifications.

The paper's main finding—which holds no matter how the income effect is scaled—is that there is a clear, robust, and substantial income effect. We could alternatively express our estimates as the effect of lifetime benefits on monthly earnings. Although we do not observe lifetime benefits, we can make assumptions to get a sense of the order of magnitude. A claimant typically collects DI benefits until becoming eligible for OASI benefits, which are essentially equal to DI benefits and are generally collected until death. Mean life expectancy when initially receiving DI is 20.31 years.[24] We discount benefits at a real rate of three percent as an illustration. Over the 20.31 years, the discounted sum of a dollar in benefits each year is \$15.04. Thus, our baseline point estimate suggests that an increase in lifetime OASDI benefits of \$1 is associated with a decrease in annual earnings around 1.35 cents (= -20.28/15.04).[25]

In Figure 4, we show the graph at the upper bend point without fitted lines, both in a "placebo" period prior to applying for DI and in the period after receiving DI. We consider this figure our clearest visual evidence that earnings while on DI are causally affected by DI payments. In each of the four years prior to applying for DI (panels A, B, C, and D), average

[24] To calculate mean life expectancy we compute the weighted average of life expectancy for each gender from Zayatz (2011), using as weights the fraction of each gender in the region of the bend point (shown in Table 1).
[25] Of course, if the earnings impact were sustained over all 20.31 years, this would imply that a \$1 increase in lifetime discounted OASDI benefits is associated with a decrease in lifetime discounted earnings of 20.28 cents.

earnings appears to be close to a linear function of AIME, with essentially identical slope on both sides of the bend point. Appendix Table A5 confirms that when the outcome is earnings in the four years prior to applying for DI, the estimates are unstable, generally insignificant and imply only a tiny percentage change in slope. The AICc-minimizing specifications all show negative and insignificant estimates. Strikingly, in each of the four years subsequent to receiving DI, there is a sharp increase in slope precisely at the bend point (panels E, F, G, and H), lending credibility to our results because this kink in earnings arises precisely after individuals go on DI.[26]

Thus, beneficiaries' earnings respond to the transfers after they go on DI, but not before. In the lifecycle model in Appendix 1, if these transfers are anticipated in advance, there should be no such change. The evidence is therefore consistent with a number of possibilities. In a lifecycle model, the income effects we document could be associated with changes in transfer income that beneficiaries do not anticipate prior to going on DI (perhaps because they do not know about the magnitude of the income they will receive), which is also consistent with the lack of bunching in initial AIME at the bend point. In principle, it is also possible that the effects of DI income on earnings operate through liquidity effects (as in Chetty 2008). In our context, DI beneficiaries normally should not expect an increase in future income and therefore typically should not want to borrow in a standard lifecycle model, limiting the scope for liquidity effects.[27] It is also possible that beneficiaries behave myopically, effectively treating each period's earnings decision as static—consistent with how we express the income effects.

Figure 5 shows the extensive margin, *i.e.* the fraction of the four years with positive annual earnings. There is an apparent increase in slope around the bend point. The regression analysis in Table 5 shows substantial effects in the linear specifications: a $1,000 increase in annual DI benefits is estimated to decrease the probability of reporting positive annual earnings by 1.29 percentage point ($p<0.01$) in the specification without controls. As only a modest fraction of the sample has positive earnings in any given year, it makes sense that part of the observed earnings response would be operating through the extensive margin. Though these estimates remain positive under the quadratic and cubic specifications, they are smaller and lose

[26] We show these graphs without drawn lines and with larger bins to show the variation in each year as clearly as possible. Appendix Figure A6 shows these results under the same formatting as our other graphs.
[27] As we do not have data on assets or consumption, it is not possible to estimate such effects more directly. Even if we did have data on assets, note that conditional on locating near the bend point, differences in assets should largely be driven by savings preferences, which could be correlated with other determinants of the size of income effects.

statistical significance. We obtain comparable results under specifications with the log odds of participation as the dependent variable. We conclude that there is some visual and statistical evidence of a participation effect at the upper bend point.[28]

We show the main components of the analysis for the lower bend point in Appendix Tables A1, A3, and A4, and Appendix Figures A1 and A3 through A9. The main results at the lower bend point show no significant effects on earnings in any of the nine specifications. However, given the *a priori* reasons above that we would not expect to find a meaningful change in slope at the lower bend point even if there is an income effect on earnings there, this evidence does not lead us to conclude that there is no income effect the lower bend point. The results for the lower bend point are shown for the group of non-dual-eligibles alone, although the results are similar when including (or focusing only on) dual-eligibles.

V.c. Robustness checks and other outcomes

Several exercises further establish the robustness of the earnings estimates. Figure 6 shows how the baseline specification estimate varies over bandwidths between \$500 and \$2,000. The point estimate remains between -17 and -27 cents throughout. The estimates are significant at 5 percent nearly throughout the range of bandwidths. They turn insignificant only when the bandwidth is smaller than \$550, which is not surprising as the smaller sample size leads to less statistical power. The bandwidth selection procedure of Calonico, Cattaneo, and Titiunik (2014a, 2014b) shows an optimal bandwidth of approximately \$650.[29] The point estimate at this bandwidth is -19.80 cents (p<0.01), nearly identical to the baseline.

Ganong and Jaeger (2014) suggest assessing whether the coefficient estimate is larger than those at "placebo" kinks placed away from the true kink. Figure 7 shows the point estimate and 95 percent confidence interval when we run regression (2) for "placebo" kinks placed in \$50 increments from \$1,450 below to \$1,450 above the true location of the upper bend point. Reassuringly, the absolute value of the coefficient is maximized at the location of the actual bend point. It is not surprising that we still estimate a negative, though notably smaller, effect when the placebo bend point is placed somewhat away from the actual bend point, as the change in slope at the actual bend point should drive a negative, though smaller, estimate of the change in

[28] If DI benefits affect employment, then it is hard to interpret estimates of how DI payments affect earnings that are conditional on employment, as the sample is selected on an outcome (*i.e.* a beneficiary having positive earnings). The point estimates suggest insignificant negative impacts of DI benefits on earnings conditional on employment.

[29] We implement a local linear RKD specification with bias correction using the local quadratic estimator and uniform weighting. We set the Imbens-Kalyanaraman regularization value to zero, which is consistent with finding the optimal bandwidth in the RKD context (Card *et al.* forthcoming, Calonico, Cattaneo and Titiunik, 2014b).

slope at nearby placebo bend points. The formal "permutation test" following Ganong and Jaeger (2014) 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 (p<0.05).[30] When earnings in the four years *before* applying for DI is the dependent variable, the permutation test reassuringly shows insignificant effects of DI payments in all nine specifications (p>0.40 throughout).

We next assess the estimates under two sample changes. First, Table 6 shows that when including SSI recipients in the sample, the results are nearly identical to the baseline. Second, disabled workers' dependents can also receive benefits. Near the upper bend point, the total DI benefits payable to a worker and his or her dependents is capped at 150 percent of PIA. For those whose dependents are receiving benefits (32.95 percent of the sample), at the family level the marginal replacement rate therefore changes at the bend point from up to 48 (=32*1.5) to up to 22.5 (=15*1.5) percent. As a baseline, we measure the marginal replacement rate *only* for the primary beneficiary*, i.e.* we express effects as if the marginal replacement rate changes from 32 to 15 percent. This effectively corresponds to an extreme case in which primary beneficiaries' earnings are not influenced by their dependents' DI benefits. An 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). In this case, the change in marginal replacement rates for those with dependents is up to 50 percent larger. (Our data extract does not have information on total DI benefits paid to all dependents, so we cannot calculate the family's exact marginal replacement rate.) Thus, our estimates of the effect of a dollar of benefits on earnings would be up to 16.48 percent (=32.95 percent x 50 percent) smaller if dependent benefits were taken into account. Although we quote the crowdout estimate based on the primary beneficiary's benefit alone as a benchmark, this effectively serves as shorthand for recognizing that in a "unitary" setting the crowdout estimates could be up to 16.48 percent smaller. To show a sample where this is not an issue, in Table 6 we try dropping beneficiaries whose dependents are receiving payments. Again, the results are nearly identical to the baseline.

[30] When using placebo kinks farther from the bend point, or estimating the bandwidth based on the Calonico *et al.* procedure separately at each placebo kink, we also estimate p<0.05. Additionally, following Landais (2014), in Appendix Figure A10 we show the R-squared of the baseline model when the kink is placed at "placebo" locations. The R-squared is maximized close to the actual bend point, again suggesting that we are estimating a true effect on earnings. See also Manoli and Turner (2014).

Table 6 also shows that the fuzzy RKD gives nearly identical results to the basic sharp RKD results. This is because the first stage estimate is extremely close to the -0.17 change in the marginal replacement rate at the bend point assumed in the sharp RKD; for example, in the baseline specification, the fuzzy RKD first stage coefficient is -0.167 (standard error 0.0039).

Appendix Table A6 displays regressions using \$25-wide or \$100-wide bins, and alternatively using the individual-level data (rather than collapsing to the bin level). It also shows the results when we include beneficiaries whose AIME changes more than four times. All of these exercises produce results that are similar to our baseline.

Appendix Figure A11 shows other work-related outcomes in the four years after going on DI: the fraction suspended for work, the fraction terminated for work, and the average DI payments foregone due to beneficiaries working. Each of these outcomes occurs for only a small fraction of the sample. None of the figures displays a clear change in slope at the bend point. Appendix Table A7 confirms that there are no robustly significant effects on these outcomes.

In Section V, we found no evidence that prior to going on DI individuals respond to the substitution incentive created by the convex kink associated with the change in the marginal replacement rate at the bend point. If this substitution incentive affects earnings after going on DI, then again we would expect to find "bunching" in the earnings distribution at the bend point once beneficiaries have received DI for some time. However, we find no such pattern in Appendix Figure A12. The regressions in Appendix Table A8 show a negligible coefficient γ on the dummy for having final AIME near the bend point from specification (4) using the same specification and throughout the same robustness checks as Table 3. It may be unsurprising that we find no evidence of a substitution effect in the context of the convex kink at the bend point, given: (a) the complexity of understanding the linkage between current earnings and future DI benefits; and (b) the fact that earnings after going on DI are sufficiently high to change AIME in only around five percent of cases.

V.d. Effect heterogeneity

Table 7 shows the earnings effects at the upper bend point across subgroups. We report estimates from the baseline linear specification, which minimizes the AICc for all subgroups except beneficiaries whose primary disability is cancer. The effect is substantially larger for women than for men, although the elasticity is modestly higher for males (as women have higher average earnings in our sample). The effect is also substantially larger for those under 45 than for those over 45, although the elasticity is modestly higher in the older group. The effect is modestly larger for those allowed DI eligibility by their initial DI examiner than for those allowed by a ALJ via a hearing after an initial denial, but the elasticity is modestly higher for those allowed by an ALJ. The estimates are similar for black and non-black beneficiaries. The effects are largest for those diagnosed with circulatory conditions, followed by mental disorders, neurological conditions, injuries, "other" disabilities, respiratory conditions, and musculoskeletal conditions, and the elasticities generally follow similar patterns. The estimate for those with cancer is only barely above zero and insignificant (and remains so in its AICc-minimizing specification). At the extensive margin, the point estimates also generally follow similar patterns across groups (throughout the nine specifications).

As our estimates are local to the upper bend point, it is not possible to determine directly whether the results generalize to the full population of DI recipients. However, the results are comparable to the baseline when we re-weight the population so that its demographic characteristics—other than AIME, which we cannot re-weight because we only observe a specific income range around the upper bend point—match those of the full sample. This is not surprising, as we estimate significant and substantial effects in each group separately. For example, the main demographic characteristic that differs in the upper bend point sample is fraction male; when we re-weight so that the percent male matches the percentage in the full sample of DI beneficiaries (*i.e.* 52 percent in the full sample, rather than 69 percent around the upper bend point), the crowdout point estimate, -28.91 cents ($p<0.01$), is modestly larger.

VII. Conclusion

A key open policy question is the size of DI's income effects on earnings. Our main finding is that a $1 increase in yearly DI benefits causes a decrease in yearly earnings around 20 cents around the upper bend point. This could reflect a lower bound in three senses, reinforcing our primary conclusion that income effects are substantial: in some specifications the point estimates are larger (up to -28 cents); the SGA limit could constrain larger responses; and the per-dollar crowdout caused by after-tax benefits should be modestly larger than the effects of pre-tax benefits that we measure.

As a benchmark to assess the size of our estimates, it is informative to compare our estimates to Maestas, Mullen, and Strand (2013, henceforth "MMS") and French and Song (2014, henceforth "FS"). MMS use random assignment to DI examiners and FS use "essentially" random assignment to DI ALJs to examine the overall effects of DI receipt on earnings and employment. We compare our results primarily to MMS and FS because, like us, they examine the U.S. DI program in a similar time period (in the 2000s and the 1990s and 2000s, respectively), and because they use random or essentially random assignment. The MMS estimates apply to DI beneficiaries allowed by DI examiners, while the FS estimates apply to DI beneficiaries allowed by ALJs; we use both types of DI beneficiaries and similar data in our study, albeit from slightly different periods of time. MMS and FS find that DI receipt causes average annual earnings losses (including both intensive and extensive margin effects) of $3,781 and $4,059, respectively, corresponding to earnings crowdout of 18 and 19 cents per dollar of DI benefits, respectively. These crowdout estimates are close to—and insignificantly different from—our baseline estimate of 20 cents, suggesting that the income effect we estimate encompasses essentially all of the earnings crowdout they find.[31] Moreover, when investigating populations more comparable to MMS and FS separately, we continue to find similar results: in Table 7 we find income effects of 23 cents per dollar of DI benefits among those made eligible by the DI examiner (most comparable to the MMS population), and 15 cents among those made eligible by an ALJ (most comparable to FS). When comparing to other literature cited in the Introduction, which typically investigates contexts less similar to ours, again our estimated income effect encompasses a large portion of the overall effects they estimate.[32]

Our results show substantial income effects at the upper bend point. Across groups based on prior income, MMS find the smallest effects in the top quintile, and they find effects in the fourth quintile that are close to the population average. These quintiles are the most comparable to our sample around the upper bend point, which ranges from the 59th to 95th percentile of earnings. If anything, this suggests—though does not imply—that crowdout could be similar or

[31] MMS and FS find that DI receipt reduces the probability of employment by 28 and 26 percentage points, respectively, or 1.22 and 1.11 percentage points per $1,000 of DI benefits, respectively. Again, these are close to our extensive margin income effect estimates of 1.29 percentage points per $1,000 of DI benefits in the linear specification. At the same time, these are around three times larger than the point estimates from our other extensive margin specifications, so it is possible that income effects encompass a smaller (but likely still substantial) portion of the extensive margin effect. Thus, our main conclusion here is that our estimates of *earnings* crowdout encompass a large fraction of the *earnings* crowdout in these studies.

[32] Under alternative assumptions, the income effect estimates from our study continue to be in the same range as the overall crowdout in MMS and FS. First, our largest crowdout estimate, 27 cents per dollar of DI benefits, is also in the same range as—and insignificantly different from—theirs. Second, FS estimate that DI receipt reduces earnings by $4,915 after five years (rather than their baseline three-year horizon), corresponding to a crowdout estimate of 23 cents per dollar of DI benefits. Third, above we calculated the crowdout from these studies by including the average value of Medicare benefits, $7,200 per year, in the value of DI. If instead we exclude the value of Medicare benefits—relevant in the extreme case that receipt of these benefits does not influence DI beneficiaries' earnings—MMS's and FS's baseline results imply earnings crowdout of 27 and 30 cents per dollar of DI benefits, respectively.

larger in other parts of the distribution of prior earnings.[33] Regardless, in previous literature the impact of DI on earnings has often been interpreted as reflecting moral hazard and at the least our results clearly demonstrate that this earnings crowdout does not only reflect moral hazard.

Autor and Duggan (2007) point out that nearly all attempts by SSA to increase the labor supply of DI beneficiaries, such as the Ticket to Work program, have primarily changed substitution incentives. One explanation for the apparent lack of success of these programs, despite the substantial work effects of DI documented in previous studies, is that DI's income effects are very important, whereas its substitution effects could be small enough that Ticket to Work had little impact. Thus, our results showing strong income effects could suggest an explanation for existing patterns in the data—such as the lack of a meaningful increase in workforce integration of DI recipients following the passage of Ticket to Work in 1999—and could help to predict the effects of proposed DI reforms.

For example, if earnings crowdout is 20 cents on the dollar more broadly, this would have implications for the earnings and fiscal consequences of a change in DI benefits, such as the chain-weighting proposal in the President's Fiscal Year 2014 Budget. Chain-weighting would cut DI cash benefits by around three percent for someone who had been on the program for 10 years; for an average beneficiary near the upper bend point in our sample, this would mean an annual benefit cut of $638. Our estimates suggest this would cause an increase in mean annual earnings around $128. Assume, for illustration, that the marginal tax rate on earnings is 0.25 (including both federal payroll and income taxes), and assume the typical case that DI benefits are not taxed. In this case, a $1 cut in DI benefits would increase total federal government revenue by five cents; the Old Age, Survivors, and Disability Insurance Trust Fund alone would gain $2.48 cents in revenue; and the DI Trust Fund alone would gain $0.36 cents.[34] If chain-weighting decreases annual benefits by $638, this would lead to an annual increase in federal government revenues around $32, an increase in OASDI revenues around $16, and an increase in DI revenues of over $2. Such fiscal consequences are relevant as policy-makers consider steps that would affect the finances of the DI Trust Fund, which is projected to be exhausted in 2016 (SSA 2014a).

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[33] MMS's heterogeneity analysis examines extensive margin effects, not earnings, but their large extensive margin effects suggest that a large part of their earnings effects could operate through the extensive margin. Across other subgroups like type of disability, our estimates tend to be larger in subgroups where MMS found larger effects. French and Song (2014) examine groups based on income closer to DI receipt but do not break down by AIME.

[34] $1.05 is calculated as: $1 in benefits plus $0.05 in reduced taxes (=$0.20 multiplied by a 25 percent marginal tax rate). The other revenue impacts are calculated analogously.

Labor economists typically agree that the uncompensated elasticity of labor supply with respect to a large, permanent change in wages is small, but the relative roles of income and substitution effects are less clear (Kimball and Shapiro 2008). In comparison with other evidence on income effects, our estimates are modestly larger than crowdout estimates based on lotteries, which are in the range of 5 to 10 cents on the dollar (*e.g.* Imbens, Rubin, and Sacerdote 2001; Cesarini, Lindqvist, Notowidigdo, and Östling 2014), but smaller than some estimates in the context of retirement pensions (*e.g.* Costa 1995). In non-DI disability contexts, Autor, Duggan, Greenberg, and Lyle (2015) estimate that VA Disability Compensation eligibility reduced labor force participation by 18 percentage points. Marie and Vall Castello (2012) find an elasticity of labor force participation with respect to DI generosity of 0.22 in Spain, while our earnings crowdout estimates are smaller than the SSI children's program estimates of Deshpande (2014). All of these studies examine different contexts than ours, and there is no reason that Social Security disability should have the same income effects on earnings as other disability programs. Thus, we view our findings as compatible with theirs. Indeed, our goal is to estimate income effects *specifically* in the largest disability program, DI, and one of the largest existing social insurance programs.

Although our results are relevant to understanding the effects of the DI program, performing a full welfare analysis of DI would require estimates of many parameters and is beyond the scope of this paper (Diamond and Sheshinski 1995, Meyer and Mok 2013). Nonetheless, the estimates in our paper could provide some of the building blocks for such an analysis. As noted, standard public finance analysis suggests that only substitution, not income, effects lead to distortions (in the absence of a pre-existing distortion). Of course, DI is financed through taxation, which can cause deadweight loss through this separate channel. Future work could more formally consider the implications of our estimates for a welfare analysis.

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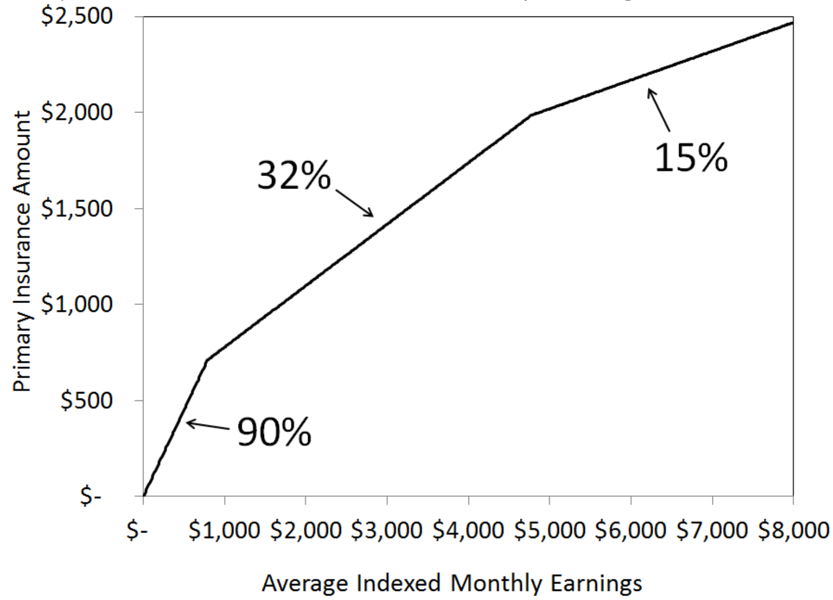
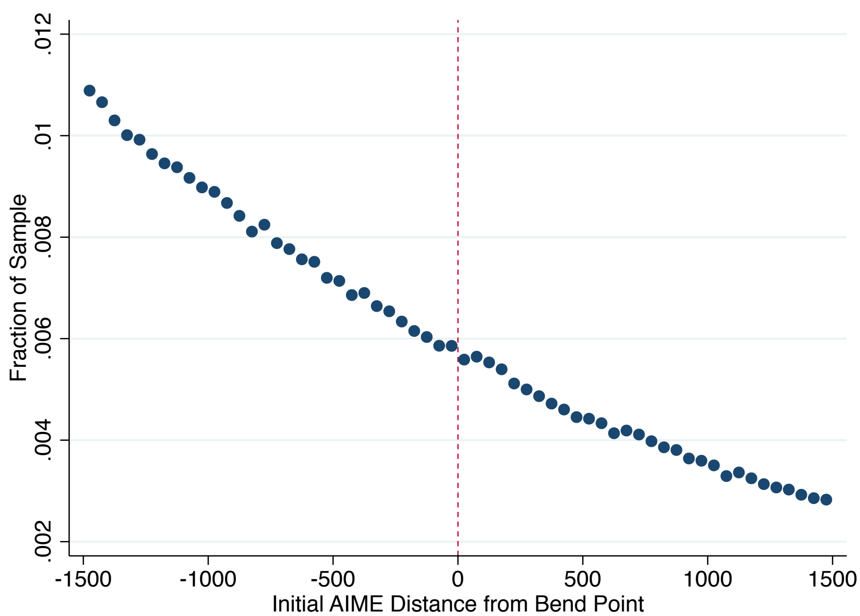
Figure 1. *Primary Insurance Amount as a Function of Average Indexed Monthly Earnings*



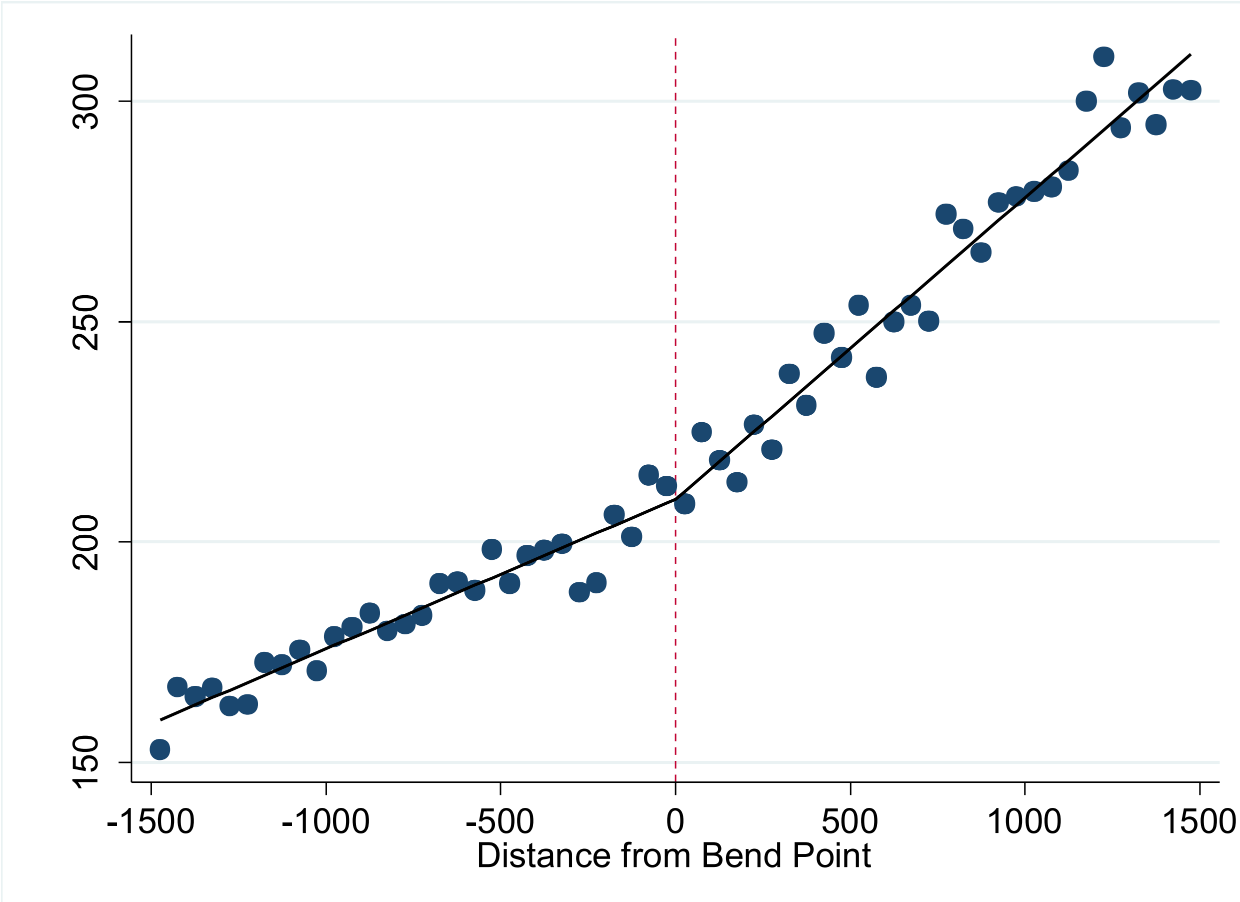
Notes: The figure shows Primary Insurance Amount (PIA) as a function of Average Indexed Monthly Earnings (AIME) in 2013. The source is SSA (2013).

Figure 2. *Initial Density around the Upper Bend Point*



Notes: The figure shows the density of initial AIME in $50 bins as a function of distance of initial AIME to the upper bend point. The number of observations appears smooth through this bend point, with no sharp change in slope or level. The upper bend point is where the marginal replacement rate in converting AIME to PIA changes from 32 percent to 15 percent. The sample includes DI beneficiaries within $1,500 of the upper bend point (see the text for other sample restrictions). The fraction of the sample in each bin is calculated by dividing the number of beneficiaries in each bin by the total number of beneficiaries whose AIME is within $1,500 of the upper bend point. The data are from SSA administrative records.

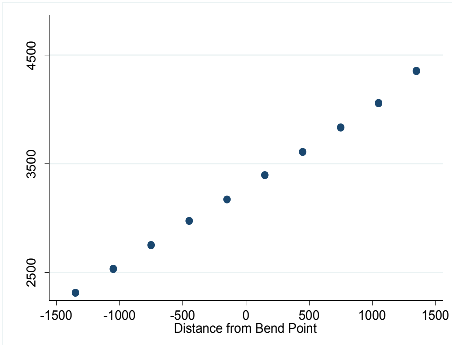
Figure 3. *Average Monthly Earnings after DI Allowance*



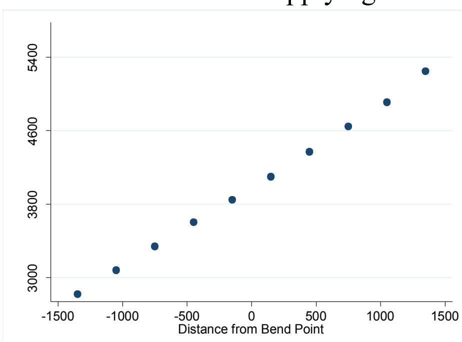
Notes: The figure shows mean monthly earnings in the four years after going on DI, in $50 bins, as a function of the distance of AIME from the bend point, where AIME is measured when applying for DI. The figure shows that mean earnings slope upward more steeply above the upper bend point than below it, with fitted lines that lie close to the data. See other notes to Figure 2.

Figure 4. *Average Monthly Earnings before and after DI Allowance*

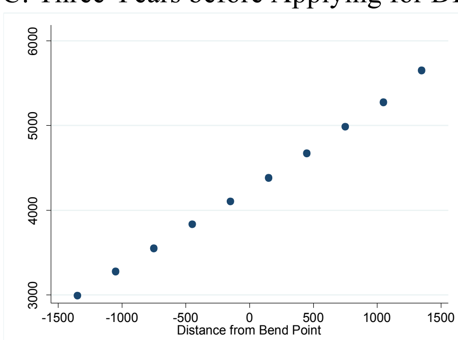
A: One Year before Applying for DI



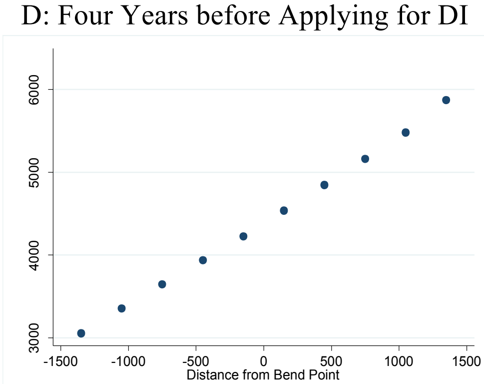
B: Two Years before Applying for DI



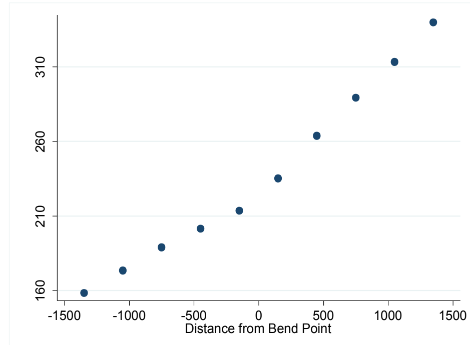
C: Three Years before Applying for DI



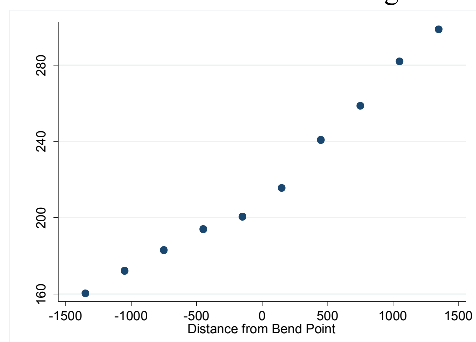
D: Four Years before Applying for DI



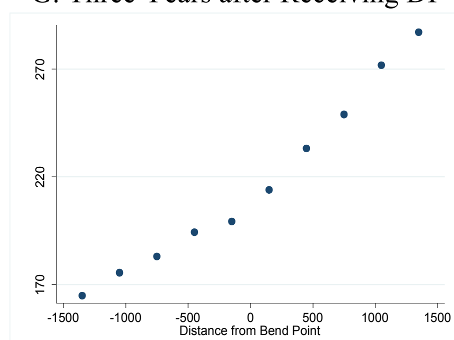
E: One Year after Receiving DI



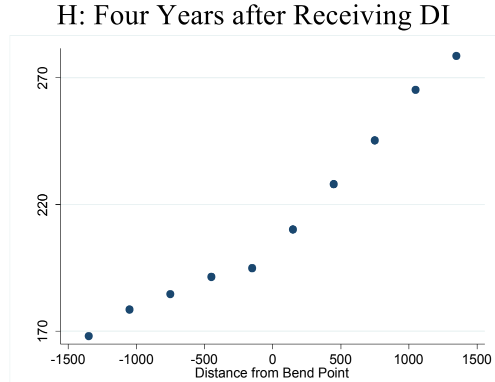
F: Two Years after Receiving DI



G: Three Years after Receiving DI

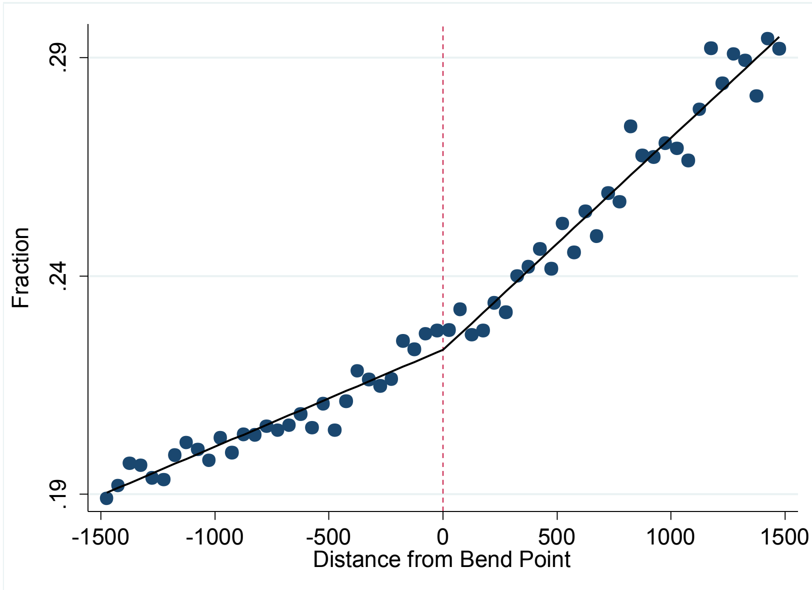


H: Four Years after Receiving DI



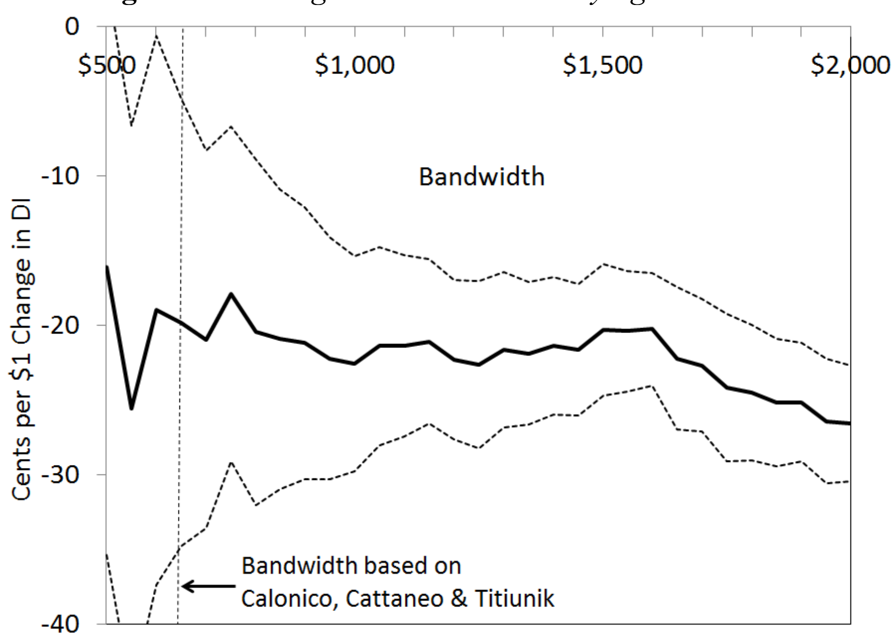
Notes: We use a bin size of $300 to display the variation in each year as clearly as possible (given the loss of power when showing only one year at a time). In years before going on DI, AIME refers to the AIME an individual would have in the prior year, based on their earnings history. In years after going on DI, AIME refers to "initial AIME," *i.e.* from the year before going on DI. Appendix Figure A6 shows the results with the same formatting as other graphs, *i.e.* $50 bins.

Figure 5. *Fraction with Any Annual Earnings after DI Allowance*



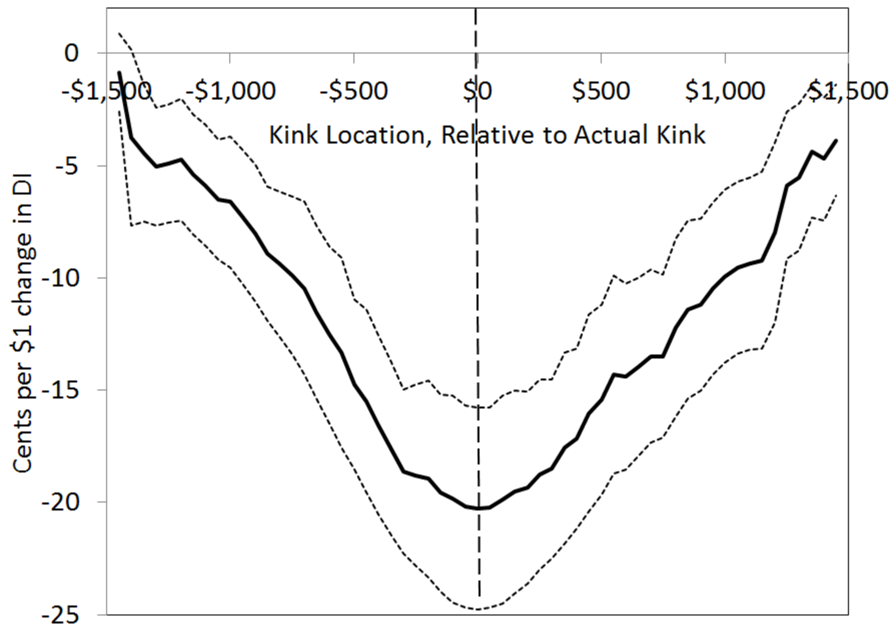
Notes: The figure shows the mean fraction of years when a beneficiary has positive annual earnings, over the four years after going on DI (*i.e.* the mean yearly employment rate over these four years), in $50 bins, as a function of distance from the bend point. The figure shows that the probability of positive earnings appears to slope upward more steeply above the upper bend point than below it. See other notes to Figure 2.

Figure 6. *Earnings Estimates with Varying Bandwidths*



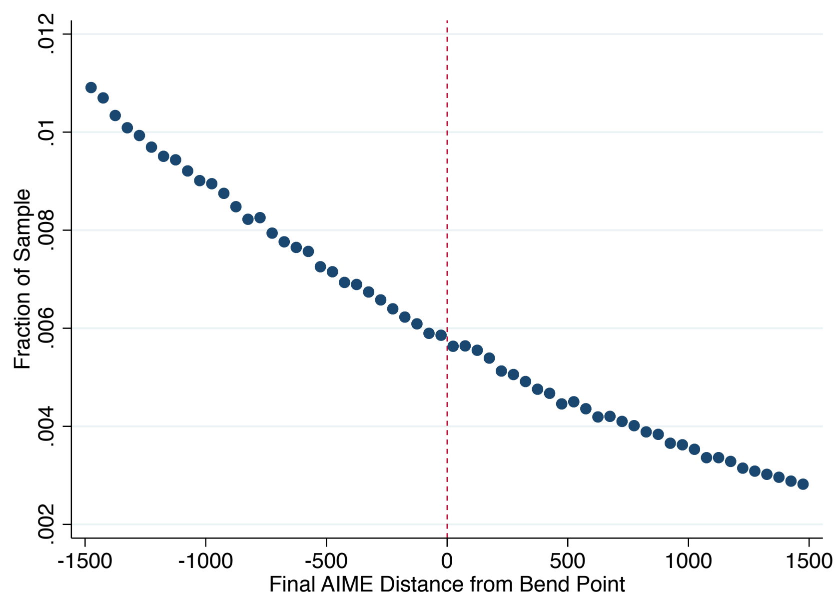
Notes: The figure shows the point estimates and 95 percent confidence intervals (on the y-axis) for the effect of a $1 change in yearly DI payments on mean yearly earnings in the first four years after going on DI that is implied by regression kink design model (2), using bandwidths between $500 and $2,000 (on the x-axis). The figure shows that the absolute value of the point estimate is robustly around 20 cents (or higher), regardless of the bandwidth chosen. Above a $500 bandwidth, the estimate is significantly different from zero at the five percent level. The vertical line at $650 shows the bandwidth recommended by the bandwidth selection procedures in Calonico, Cattaneo and Titiunik (2014a, 2014b). We use the baseline linear specification without controls.

Figure 7. *Earnings Estimates for Placebo Kink Locations*



Notes: The figure show the point estimates and 95 percent confidence intervals for the effect of a $1 decrease in yearly DI payments on mean yearly earnings in the first four years on DI that is implied by replacing the true kink in model (2) with "placebo" kink locations at other locations of AIME relative to the true location (normalized to zero). We use the baseline linear specification without controls. The figure shows that 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.

Figure 8. *Final Density around the Upper Bend Point*



Notes: The figure shows that the density of *final AIME* is smooth around the upper bend point. This demonstrates that substitution effects are not evident; if substitution effects were operating, we should see bunching in final AIME at the bend point. The fraction of the sample in each bin is calculated by dividing the number of beneficiaries in each bin by the total number of beneficiaries whose final AIME is within $1,500 of the upper bend point. Final AIME represents AIME after having been on DI for four years. Note that Figure 8 is subtly different than Figure 2.

Table 1. *Summary Statistics*

	Upper bend point sample		Full sample	
	Mean	Std. dev.	Mean	Std. dev.
Demographic & Employment Information				
Age when applying for DI (years)	49.8	7.0	47.9	8.4
Fraction male	0.69	0.46	0.52	0.50
Fraction black	0.13	0.34	0.14	0.34
Average annual earnings ($):				
4 years before applying for DI	$48,895	$24,941	$36,042	$28,628
3 years before applying for DI	$47,468	$25,938	$35,150	$28,746
2 years before applying for DI	$44,472	$26,921	$33,018	$28,586
1 year before applying for DI	$36,680	$27,211	$27,092	$27,013
Fraction with any annual earnings:				
4 years before applying for DI	0.94	0.23	0.92	0.28
3 years before applying for DI	0.93	0.26	0.90	0.30
2 years before applying for DI	0.91	0.29	0.87	0.33
1 year before applying for DI	0.86	0.35	0.81	0.39
DI Information				
Primary Insurance Amount ($ monthly)	$1,773	$214	$1,369	$482
Annualized DI Payments ($)	$21,276	$2,568	$16,428	$5,784
Fraction allowed DI via hearings	0.29	0.45	0.32	0.47
Fraction by primary disability type:				
Musculoskeletal conditions	0.35	0.48	0.35	0.48
Mental disorders	0.20	0.40	0.23	0.42
Circulatory conditions	0.12	0.33	0.10	0.30
Nervous system	0.08	0.28	0.08	0.27
Injuries	0.05	0.21	0.04	0.21
Respiratory conditions	0.03	0.18	0.03	0.18
Neoplasms	0.04	0.20	0.04	0.19
Other disabilities	0.13	0.33	0.12	0.33
Work-related Outcomes during First Four Years after DI Allowance				
Average annual earnings ($):				
1st year after entry	$2,593	$7,788	$2,519	$21,723
2nd year after entry	$2,448	$7,975	$2,427	$12,612
3rd year after entry	$2,432	$8,102	$2,443	$12,371
4th year after entry	$2,416	$8,197	$2,447	$11,731
Fraction with any annual earnings:				
1st year after entry	0.26	0.44	0.22	0.41
2nd year after entry	0.22	0.41	0.20	0.40
3rd year after entry	0.21	0.41	0.19	0.39
4th year after entry	0.20	0.40	0.19	0.39
Annual fraction suspended due to work	0.007	0.041	0.006	0.041
Annual fraction terminated due to work	0.001	0.018	0.001	0.017
Annual foregone DI pay due to work ($)	$24.71	172.00	$19.30	142.00
Observations	610,271		1,746,020	

Notes: The "upper bend point sample" includes DI beneficiaries within $1500 of the upper bend point. These samples are the same as those considered in our regressions. The source is SSA administrative records on new DI beneficiaries from 2001 to 2007. See the text for sample restrictions.

Table 2. *Smoothness of the Densities and Predetermined Covariates*

Dependent variable	Polynomial minimizing AICc (1)	Estimated kink (2)	Fraction of statistically significant [p=0.05] kinks for polynomials of order 3-12 (3)
Number of observations	9	-0.76	0%
		(1.41)	
Fraction male (x 1,000)	12	-0.100	0%
		(0.097)	
Average age when filing for DI (x 1,000)	10	1.27	40%
		(1.11)	
Fraction black (x 1,000)	12	-0.064	10%
		(0.048)	
Fraction of hearings allowances (x 1,000)	12	-0.024	0%
		(0.087)	
Fraction with mental disorders (x 1,000)	12	-0.075	10%
		(0.056)	
Fraction with musculo. conditions (x 1,000)	12	0.081	0%
		(0.086)	
Fraction SSI Recipients (removed from main sample) (x 1,000)	12	0.711	30%
		(0.377)	

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows that the density of the assignment variable (*i.e.* AIME just prior to applying for DI) and the distributions of predetermined covariates are smooth around the upper bend point. For each dependent variable, we test for a change in slope at the bend point using polynomials of order three to 12. For each of the dependent variables, the table shows: the order of the polynomial that minimizes the corrected Akaike Information Criterion (AICc) (Column 1); the estimated change in slope at the bend point and standard error (Column 2) under the AICc-minimizing polynomial order; and the percent of the regressions with polynomial orders between three and 12 that show a change in slope that is statistically significant at the five percent level (Column 3). Before running the regression, we take bin means of variables in 60 equally-sized bins of $50 width around the bend point, so each regression has 60 observations. See other notes to Table 1.

Table 3. *Estimates of Initial Excess Mass in AIME*

	Baseline (1)	Covariates (2)	Alternative bandwidth (3)	8^{th}-degree polynomial (4)	Excluded region $200 (5)
γ (x 10,000)	0.064	-0.017	-0.55	-0.22	0.28
	[-1.05, 1.18]	[-1.18, 1.15]	[-1.81, 0.71]	[-1.32, 0.88]	[-0.59, 1.15]

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows the point estimates and 95 percent confidence interval on γ, the coefficient on the dummy for being near the kink in initial AIME from regression (4) (reflecting the excess mass per bin near the kink). For readability, the reported value of γ is the true value multiplied by 10,000. The mean density of the two bins immediately outside those nearest to the kink is 0.0057 (or 57.27 when multiplied by 10,000). This is relevant for interpreting estimates of the coefficient γ, as it is always a tiny percentage of 57.27 (on the order of 0.1 percent). The column headings show the different specifications. "Baseline" refers to estimating a seventh-degree polynomial through the earnings distribution within a bandwidth of $1,500 of the bend point and estimating the kink from a region within $100 of the bend point. We use bin means of variables in 60 equally-sized bins of $50 width around the upper bend point, so that each regression has 60 observations. "Covariates" (Column 2) refers to a specification controlling for the mean value of covariates within each bin (mean age, percent male, percent black, and percent allowed at the hearings stage). "Alternative bandwidth" (Column 3) refers to using a bandwidth of $650—the bandwidth selected by the procedure of Calonico, Cattaneo, and Titiunik (2014a, 2014b)—rather than $1,500. "Eighth-degree polynomial" (Column 4) estimates an eighth-order polynomial through the density rather than a seventh-order. "Excluded region $200" (Column 5) refers to estimating the kink from a region of $200 around the bend point, rather than $100.

Table 4. *Income Effect of DI Benefits on Earnings*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Over First Four Years after DI Allowance									
Cents per $1	**-20.28*****	-20.20***	-19.25***	-24.40***	-24.62***	-26.87***	-25.27***	-25.19***	-27.38***
of less DI	**(2.24)**	(2.26)	(3.83)	(8.25)	(8.50)	(8.55)	(8.73)	(8.78)	(9.18)
AICc	**377.56**	379.73	386.29	379.46	381.67	387.75	381.62	383.99	390.32
First Year									
Cents per $1	**-23.02*****	-22.79***	-21.75***	-27.84***	-28.33***	-28.53***	-29.38***	-29.03***	-29.52***
of less DI	**(2.32)**	(2.48)	(3.94)	(6.82)	(6.91)	(6.91)	(7.88)	(8.16)	(7.77)
AICc	**382.28**	384.18	387.63	384.09	385.94	389.35	383.67	385.86	388.98
Second Year									
Cents per $1	**-19.63*****	-19.69***	-19.92***	-27.34***	-27.35***	-29.48***	-28.63***	-29.09***	-30.56***
of less DI	**(2.32)**	(2.34)	(3.65)	(8.11)	(8.28)	(8.17)	(8.49)	(8.46)	(8.81)
AICc	**384.24**	386.44	392.88	385.45	387.75	393.88	387.46	389.65	396.16
Third Year									
Cents per $1	**-18.29*****	-18.35***	-16.60***	-17.84	-17.71	-19.78	-17.67	-17.84	-19.60
of less DI	**(2.62)**	(2.54)	(4.33)	(10.47)	(10.79)	(11.41)	(10.97)	(10.69)	(12.04)
AICc	**394.92**	397.11	403.62	397.13	399.41	406.04	399.43	401.79	408.70
Fourth Year									
Cents per $1	**-20.20*****	-19.97***	-18.72***	-24.58**	-25.08**	-29.68***	-25.39**	-24.78**	-29.82**
of less DI	**(2.58)**	(2.57)	(5.02)	(10.49)	(10.73)	(10.70)	(10.82)	(11.10)	(11.18)
AICc	**401.76**	403.74	409.48	403.73	405.71	410.48	405.95	408.09	413.15
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: ** denotes p<0.05, *** denotes p<0.01. The table contains coefficients and standard errors showing the estimated effect of a one-dollar increase in yearly DI payments on yearly earnings around the upper bend point. The estimates are based on regression model (2) in the text, which is a regression kink design based on estimating the change in slope of mean earnings around the upper bend point. “Covariates” refers to a specification controlling for covariates within each bin (mean age, percent male, percent black and percent allowed at the hearings stage). For the results in the first row, the full set of regression coefficients are reported in Appendix Table A4; to arrive at the Table 4 estimates, we transform the Appendix Table A4 estimates by dividing by the change in slope of PIA as a function of AIME at the bend point, -0.17. The “AICc” is the corrected Akaike Information Criterion and the bolded estimates minimize the AICc within each row. Before running the regression, we take bin means of variables in 60 equally-sized bins of $50 width around the bend point, so each regression has 60 observations. See other notes to Table 1.

Table 5. *Income Effect of DI Benefits on Participation*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
p.p. change per	-1.29***	-1.30***	-0.93***	-0.32	-0.30	-0.43	**-0.37**	-0.42	-0.47
$1,000 less DI	(0.12)	(0.12)	(0.20)	(0.39)	(0.38)	(0.38)	**(0.43)**	(0.44)	(0.42)
AICc	-496.41	-494.21	-493.92	-499.89	-497.85	-492.99	**-500.05**	-498.77	-493.17
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: ** denotes p<0.05, *** denotes p<0.01. The dependent variable is the fraction of the first four calendar years after going on DI in which a beneficiary had positive annual earnings. Coefficients are converted into percentage point changes and scaled by a $1,000 change in DI payments. Thus, the coefficients reflect the effect of a $1,000 increase in DI benefit payments on the probability that an individual has positive earnings in any given year over the first four year after going on DI. For more information, see notes to Table 1 and 4.

Table 6. *Income Effects on Earnings: Robustness Checks*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample Change 1 – Including SSI Recipients									
Cents per $1	**-20.44*****	-20.21***	-17.39***	-19.72***	-20.13***	-21.15***	-21.04***	-20.76***	-21.95***
of less DI	**(1.94)**	(1.94)	(3.59)	(6.98)	(7.29)	(7.00)	(7.67)	(7.67)	(7.68)
AICc	**358.23**	360.08	365.06	360.43	362.38	367.25	362.39	364.69	369.57
Sample Change 2 – Removing Beneficiaries with Payments to Dependents									
Cents per $1	**-21.25*****	-21.18***	-21.65***	-24.21**	-24.37**	-21.72**	-25.38**	-25.51**	-23.20**
of less DI	**(2.50)**	(2.50)	(3.86)	(9.51)	(9.89)	(10.40)	(10.68)	(10.73)	(10.96)
AICc	**232.51**	235.56	241.29	235.46	238.51	244.44	238.37	241.46	246.65
Estimate using Fuzzy Regression Kink Design									
Cents per $1	**-20.62*****	-20.57***	-20.14***	-23.81***	-23.93***	-25.19***	-24.37***	-24.35***	-25.56***
of less DI	**(1.81)**	(1.83)	(3.21)	(6.15)	(6.30)	(6.01)	(6.38)	(6.43)	(6.39)
AICc	**677.24**	681.06	690.77	680.64	684.36	692.19	682.32	686.25	694.00
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows that the basic results are robust to adding SSI recipients to the main sample; to removing beneficiaries with payments to dependents; to adding beneficiaries with many changes in their AIME; and to running a fuzzy RKD. In the case of a fuzzy RKD, we report the *negative* of the estimated coefficient. In other words, in the fuzzy RKD we report $-\beta_2/\alpha_2$, where these refer to the key coefficients of interest in the reduced form and first stage models (2) and (3), respectively. See notes to Tables 1 and 4.

Table 7. *Heterogeneity in the Income Effects*

Category	Subgroup	Cents per $1 less of DI (1)	Elasticity at bend point (2)
All		**-20.28***** **(2.24)**	-1.92
Sex	Males	**-22.86***** **(2.41)**	-2.71
	Females	**-35.47***** **(4.53)**	-2.25
Age at filing for DI	Age < 45 years	**-32.21***** **(6.45)**	-1.75
	Age ≥ 45 years	**-18.75***** **(2.26)**	-2.15
Race	Nonblack	**-20.25***** **(2.39)**	-1.90
	Black	**-17.59***** **(6.64)**	-1.75
Type of allowance	Initial DDS allowance	**-22.79***** **(2.76)**	-1.92
	Hearings allowance	**-15.40***** **(3.40)**	-2.13
When entered DI	Started in 2001-2002	**-20.94***** **(4.77)**	-1.86
	Started in 2003-2004	**-12.53***** **(4.38)**	-1.13
	Started in 2005-2007	**-24.64***** **(3.35)**	-2.47
Primary disability	Mental disorders	**-26.61***** **(5.68)**	-2.62
	Musculoskeletal cond.	**-14.02***** **(3.47)**	-1.83
	Circulatory conditions	**-28.44***** **(5.09)**	-3.56
	Neurological conditions	**-25.33***** **(7.20)**	-2.06
	Injuries	**-23.12** **(14.20)**	-1.53
	Respiratory conditions	**-16.48** **(9.14)**	-2.20
	Cancers	-0.39 (25.33)	-0.013
	All other disabilities	**-20.08***** **(7.66)**	-1.46

Notes: ** denotes p<0.05, *** denotes p<0.01. See notes to Tables 1 and 4. The results shown are from the baseline linear specification without controls and are bolded when this specification minimizes the corrected AICc. All displayed results minimize the AICc except the results for those whose primary disability is cancer; in this case, the baseline linear specification minimizes the Bayesian Information Criteria but not the AICc. The elasticity is calculated as the scaled coefficient multiplied by the PIA value at the kink and divided by the average earnings at the kink, as measured by the constant in the regression. The "scaled coefficient" refers to the change in earnings (in cents) in response to $1 more in the Primary Insurance Amount (PIA), *i.e.* the quantity that is reported in the other main tables. "DDS" refers to Disability Determination Services, *i.e.* beneficiaries allowed by the initial DI examiner.

Appendices (for online publication)

Appendix 1: Illustrative framework for analyzing responses

To ground the empirical analysis, in this Appendix we briefly review a simple lifecycle framework for understanding the effects of the policy change, as well as a static model. Our framework is adapted from Blundell and MaCurdy (1999) to our particular context in which we observe pre-tax-and-transfer earnings as the key outcome of interest. Thus, rather than modeling the tradeoff between consumption and hours worked as in Blundell and MaCurdy, we model the tradeoff between consumption and pre-tax earnings. In other words, following Saez (2010) and other papers that have access to administrative data on earnings but not on hours worked, we model individuals as trading off consumption, in which utility is increasing, against pre-tax earnings, in which utility is decreasing because it requires effort to produce earnings.

Assume, then, that each individual has a quasi-concave utility function that is separable across time from periods t to T: $U_t = U(U^t(C_t, E_t, X_t), U^{t+1}(C_{t+1}, E_{t+1}, X_{t+1}), ..U^T(C_T, E_T, X_T))$, where C_t and E_t are respectively consumption and pre-tax-and-transfer earnings in period t. X_t refers to additional variables that could affect utility. Utility is maximized subject to the intertemporal budget constraint:

$$A_{t+1} = (1 + r_{t+1})(A_t + B_t + Y_t + E_t(1 - \tau_t) - C_t)$$

where A_t represents assets in period t, B_t is DI benefit income, Y_t is other non-asset unearned income (where Y_t has been appropriately adjusted so that virtual income is correctly specified), τ_t represents the net effective marginal tax rate *including* the effects of DI benefits as well as taxes, and r_{t+1} is the interest rate.

With uncertainty, dynamic programming techniques yield the following problem, subject to the asset accumulation rule above:

$$V(A_t, t) = \max\{U(C_t, L_t, X_t) + \kappa E_t[V(A_{t+1}, t+1)]\}$$

Here κ represents the discount factor. Standard dynamic programming techniques yield the following first-order conditions:

$$U_C(C_t, E_t) = \lambda_t$$
$$U_E(C_t, E_t) \geq \lambda_t(1 - \tau)_t$$
$$\lambda_t = \kappa E_t[\lambda_{t+1}(1 + r_{t+1})]$$

where λ_t represents the marginal utility of lifetime wealth in period t.

Earnings supply can then be written as a function of the marginal utility of wealth and and the net implicit tax rate:

$$E_t = L(\lambda_t, 1 - \tau_t, X_t)$$

λ_t reflects the effects of all future income streams and therefore captures the effects of lifetime income, whereas $1 - \tau_t$ reflects price effects. To arrive at a specification where mean earnings can serve as the dependent variable (as in our empirical work), we can linearize the expression for E_t above:

$$E_t = \alpha\lambda_t + \beta(1 - \tau_t) + \gamma X_t$$

Here α reflects an income effect, and β reflects a substitution effect.

In our empirical context, it is possible to distinguish two sets of years:

1. Years *before* the DI income is anticipated to arrive. In these years, there should be no discontinuous change in slope of earnings at the bend point, because the discontinuous change at the bend point in the slope of the marginal utility of lifetime wealth has not yet been anticipated. There should also be no bunching at the convex kink created by the discontinuous change in the marginal replacement rate at the bend point (see Appendix 2), because the substitution effects created by DI are not anticipated.

2. Years *after* the DI income has been anticipated to arrive. In these years, we should see a change in slope of earnings arise at the bend point, due to the income effect of lifetime wealth. If substitution effects are greater than zero, we should also see bunching arise in the earnings distribution at the bend point.

As described in Blundell and MaCurdy (1999), if agents behave completely myopically or if capital markets are constrained so that it is not possible to transfer capital across periods (*e.g.* individuals wish to borrow but are liquidity constrained), then a static specification is appropriate. In Blundell and MaCurdy's static, linearized specification, earnings in a given time period *t* can then be written as a function of the net returns to work $1-\tau_t$ in that period, unearned income B_t+Y_t, and other factors X_t:

$$E_t = \alpha(B_t + Y_t) + \beta(1-\tau_t) + X_t$$

In this case, we would expect no change in slope at the bend point prior to going on DI, but if there are income effects then we would expect a change in slope after going on DI. Since earnings supply in each period is determined by the net returns to work in that period, we also would not expect bunching at the convex kink prior to going on DI, but if substitution effects are greater than zero then we would expect bunching to arise after going on DI.

As in Imbens, Rubin, and Sacerdote (2001), in the lifecycle model with no myopia, if we assume that utility is Stone-Geary and that there is no uncertainty after going on DI, then earnings in each year should also be a linear function of the DI annuity transfer payments in each year, as in the static specification above.

As described in the main text, our estimation strategy is valid if other unobserved determinants of work (*e.g.* Y_t) do not lead to a change in slope in the outcome at the bend point. The models above also do not consider the option value of work that has been considered in the DI context (*e.g.* Coile 2015), though as a benchmark the models above illustrate certain key forces determining earnings.

Appendix 2: Model of earnings response and procedure for estimating excess normalized bunching at kink

2.a. Saez (2010) Model

In Saez (2010), individuals maximize utility u(*c,z;n*) over consumption, *c*, and costly earnings, *z*.[1] Heterogeneity is parameterized by an "ability" parameter *n*, which is distributed according to the smooth CDF *F*(·). Individuals maximize utility subject to the following budget constraint: $c = (1-\tau)z + R$, where *R* is virtual income and τ is the marginal tax rate. Thus, this is

[1] This section often corresponds closely to the description of the Saez methodology in Gelber, Jones, and Sacks (2014).

a static model, as in the static model described in Appendix 1. We refer to the "tax rate" created by the conversion of AIME to PIA. We stress that DI is *not* administered through the tax system and does not create an actual tax. Rather, the economic theory used to describe the incentives this creates is parallel to that governing the effects of taxes. We adopt the tax rate terminology to be consistent with previous literature estimating the effects of taxes on non-linear budget sets. The decrease in the "marginal net-of-tax rate" at the convex kink in the theory corresponds in our empirical context to the decrease in the marginal replacement rate at the bend point in the AIME-to-PIA conversion formula.

Following Saez (2010), we use a quasi-linear and isoelastic utility function:

$$u(c,z;n) = c - \frac{n}{1+1/\varepsilon}\left(\frac{z}{n}\right)^{1+1/\varepsilon}$$

Consider first a linear tax schedule with a constant marginal tax rate τ_0. Observe that with a smooth distribution of skills n, we have a smooth distribution of earnings that is monotonic in skill, provided we make the typical Spence-Mirlees assumption. We refer to individuals' earnings on a linear tax schedule as their "initial" earnings. The probability distribution function of initial earnings is given by $h_0(.)$.

Now consider the introduction of a piecewise linear tax schedule with a convex kink: the marginal tax rate below earnings level z^* is τ_0, and the marginal tax rate above z^* is $\tau_1 > \tau_0$. Given the tax schedule, individuals bunch at the kink point z^*; as explained in Saez (2010), the realized density in earnings has an excess mass at z^*. Those initially locating between z^* and some higher earnings level Δz^* will bunch at the kink z^* once the piecewise linear tax schedule has been introduced.

The "excess mass" B of bunchers will be:

$$B = \int_{z^*}^{z^*+\Delta z^*} h(\xi)d\xi$$

where ξ is the dummy of integration. Define "normalized bunching" b as the amount of bunching at the kink normalized by the density at the kink $h(z)$ under a linear tax schedule:

$$b \equiv \frac{B}{h(z)}$$

2.b. Procedure for estimating excess mass

We seek to estimate the "excess mass" at the kink, *i.e.* the fraction of the sample that locates at the kink under the kinked tax schedule but not under the linear tax schedule. Following a standard procedure in the literature (*e.g.* Saez 2010), we estimate the counterfactual density (*i.e.* the density in the presence of a linear budget set) by fitting a smooth polynomial to the earnings density away from the kink, and then estimating the "excess" mass in the region of the kink that occurs above this smooth polynomial.

Specifically, for each earnings bin z_i, we calculate p_i, the proportion of the sample with earnings in the range $[z_i - k/2, z_i + k/2)$. The earnings bins are normalized by distance-to-kink, so that for $z_i=0$, p_i is the fraction of all individuals with earnings in the range $[0,k)$. To estimate bunching, we assume that p_i can be written as:

$$p_i = \sum_{d=0}^{D} \beta_d (z_i)^d + \sum_{j=-k}^{k} \gamma 1\{z_i = j\} + \varepsilon_i \qquad (6)$$

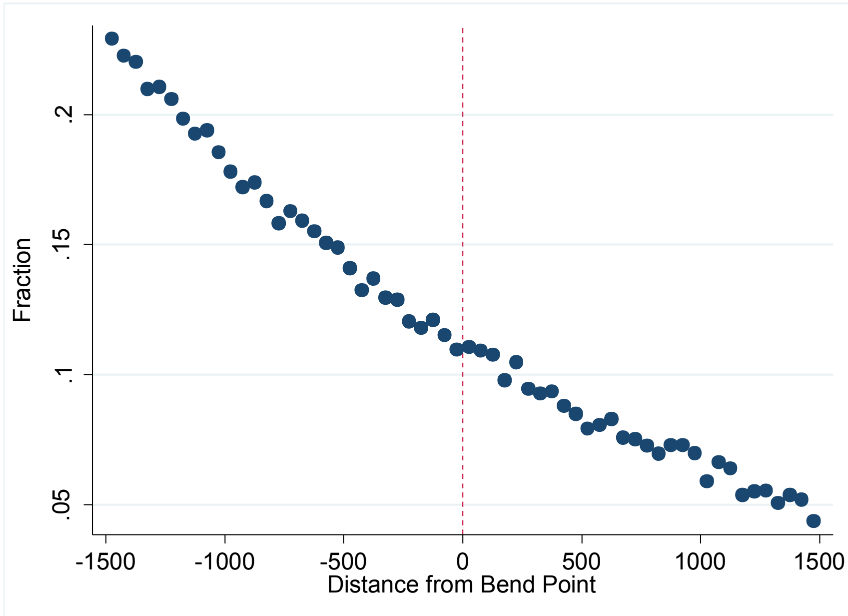
and run this regression (where 1 denotes the indicator function and *j* denotes the bin). This equation expresses the earnings distribution as a degree *D* polynomial, plus a set of indicators for each bin within $k\delta$ of the kink, where δ is the bin width. In our empirical application, we choose D=7, δ=50 and k=1 as our baseline (so that two bins are excluded from the polynomial estimation). As we show, our results are robust to alternative choices of *D*, δ, and *k*.

Our measure of excess mass is $\hat{M} = 2k\hat{\gamma}$, the estimated excess probability of locating at the kink (relative to the polynomial term). This measure depends on the counterfactual density near the kink, so to obtain a measure of excess mass that is comparable at the kink, we scale by the predicted density that we would obtain if there were a linear budget set. This is just the constant term in the polynomial, since z_i is the distance to zero. Thus, our estimate of normalized excess mass is $\hat{B} = \frac{\hat{M}}{\hat{\beta}_0}$. We calculate standard errors using the delta method. We calculate the density in each bin by dividing the number of beneficiaries in the bin by the total number of beneficiaries within the bandwidth; note that this normalization should *not* affect the excess normalized mass or the estimated density, because dividing by the total number of beneficiaries within the bandwidth affects the numerator (*i.e.* $\hat{M}$) and denominator (*i.e.* $\hat{\beta}_0$) of the expression for $\hat{B}$ $(= \frac{\hat{M}}{\hat{\beta}_0})$ in equal proportions and therefore should have no impact on $\hat{B}$.

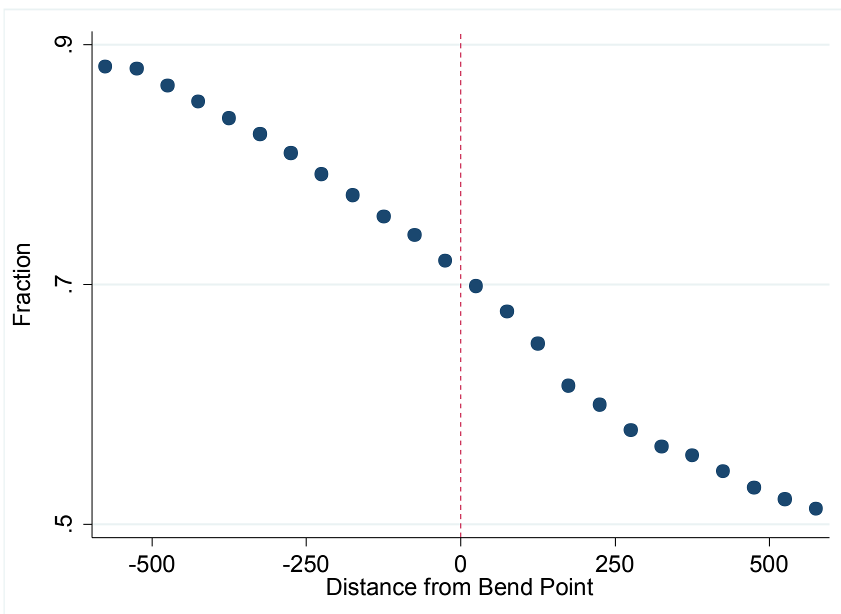
Appendix Figures

Figure A1. *Fraction of Sample Comprised of SSI Recipients*

A: Upper Bend Point



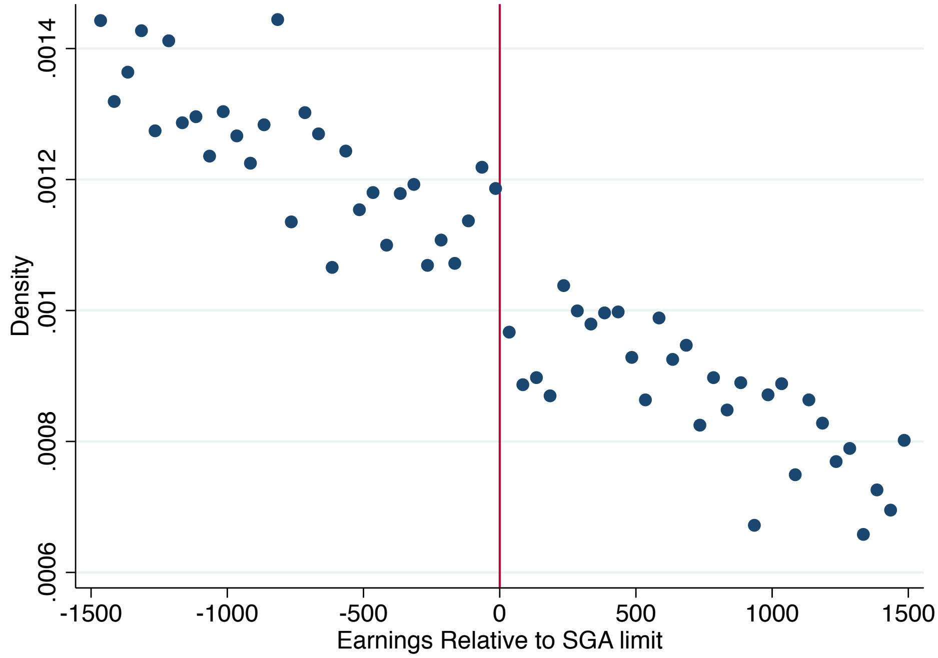
B: Lower Bend Point



Notes: The figure shows that the fraction of the sample that is comprised of SSI recipients as a function of the distance from the bend point. The figure shows that this fraction is smooth around the bend points. See other notes to Figure 2.

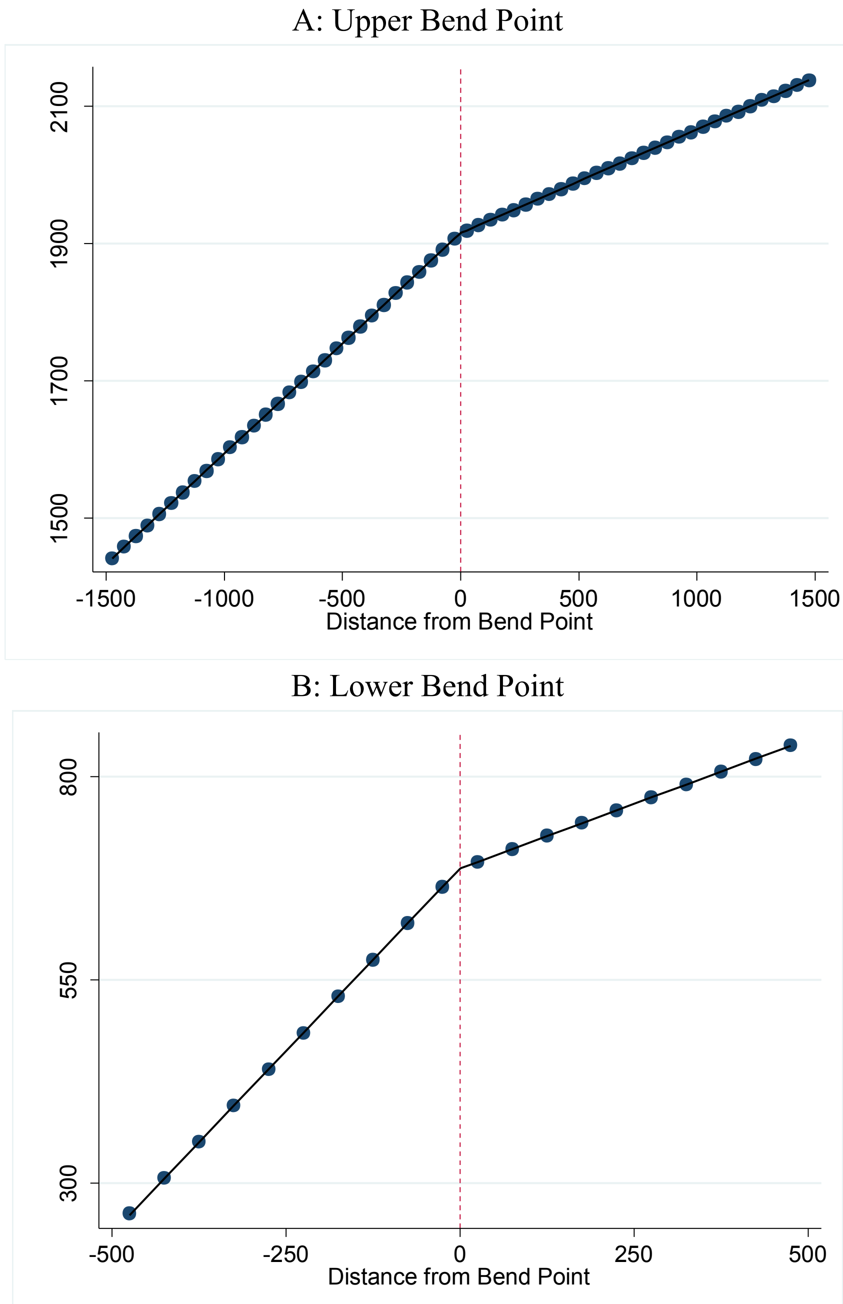
Figure A2. *Distribution of Earnings Relative to Annualized SGA Limit*

A: Distribution of annual earnings in $1,500 range around annualized SGA ($50 bins)



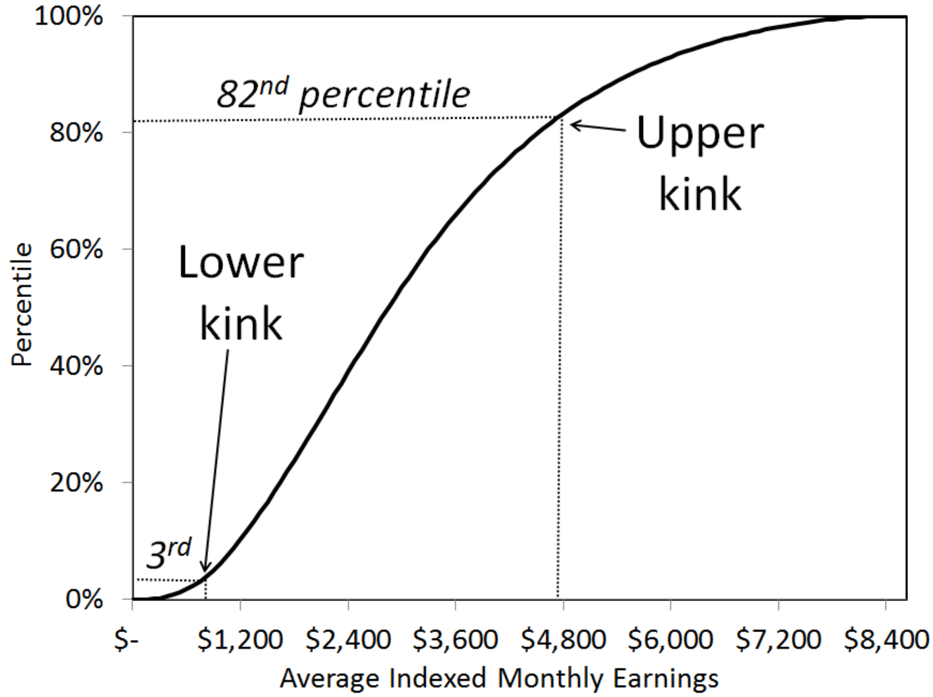
B: Distribution of annual earnings in $1,500 range around annualized SGA ($10 bins)

Figure A3. *Observed Primary Insurance Amount as a Function of Average Indexed Monthly Earnings*



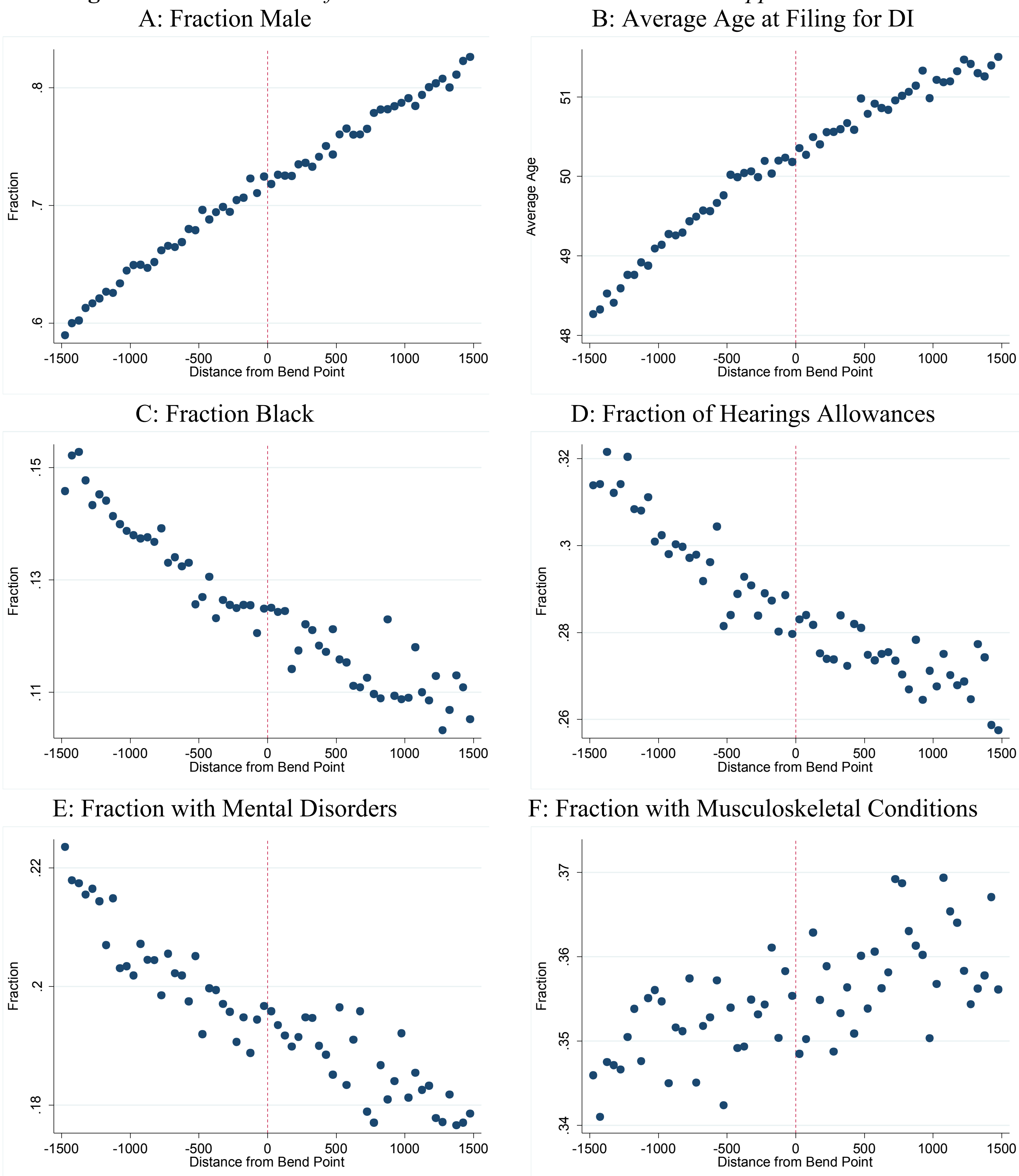
Notes: The figure shows that actual PIA (as measured in our data) is determined by AIME in the way the policy dictates, *i.e.*, with a 90 percent marginal replacement rate below the lower bend point, a 32 percent marginal replacement rate between the lower and the upper bend point and a 15 percent marginal replacement rate above the upper bend point. In both the upper and lower bend point samples, more than 90 percent of beneficiaries in the sample have an actual PIA that is within $2 of the PIA estimated from the statutory formula. The average difference between actual and estimated PIA is $1.97 and $1.24 in the upper and lower bend point samples, respectively.

Figure A4. *Cumulative Distribution Function of the Average Indexed Monthly Earnings of new Disability Insurance Beneficiaries, 2001 to 2007*



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.

Figure A5. *Distribution of Predetermined Covariates around the Upper Bend Point*



Notes: These figures show the distributions of predetermined covariates in $50 bins as a function of distance from the bend point. They show that these distributions are smooth in the region of the bend point. See notes to Figure 2.

Figure A5 (continued). *Distribution of Predetermined Covariates around the Lower Bend Point*

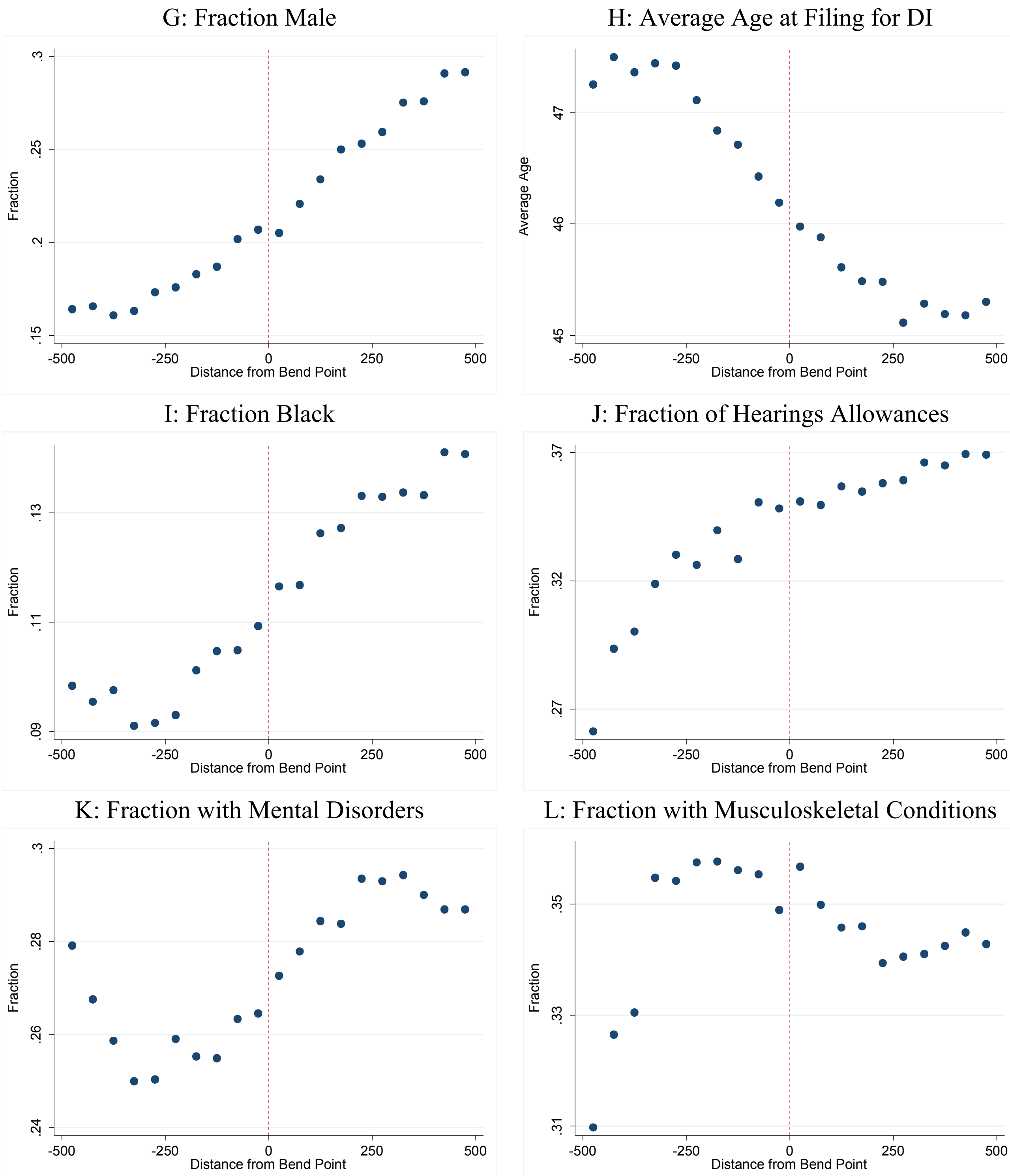


Figure A6. *(a) Average Monthly Earnings before and after DI Allowance*

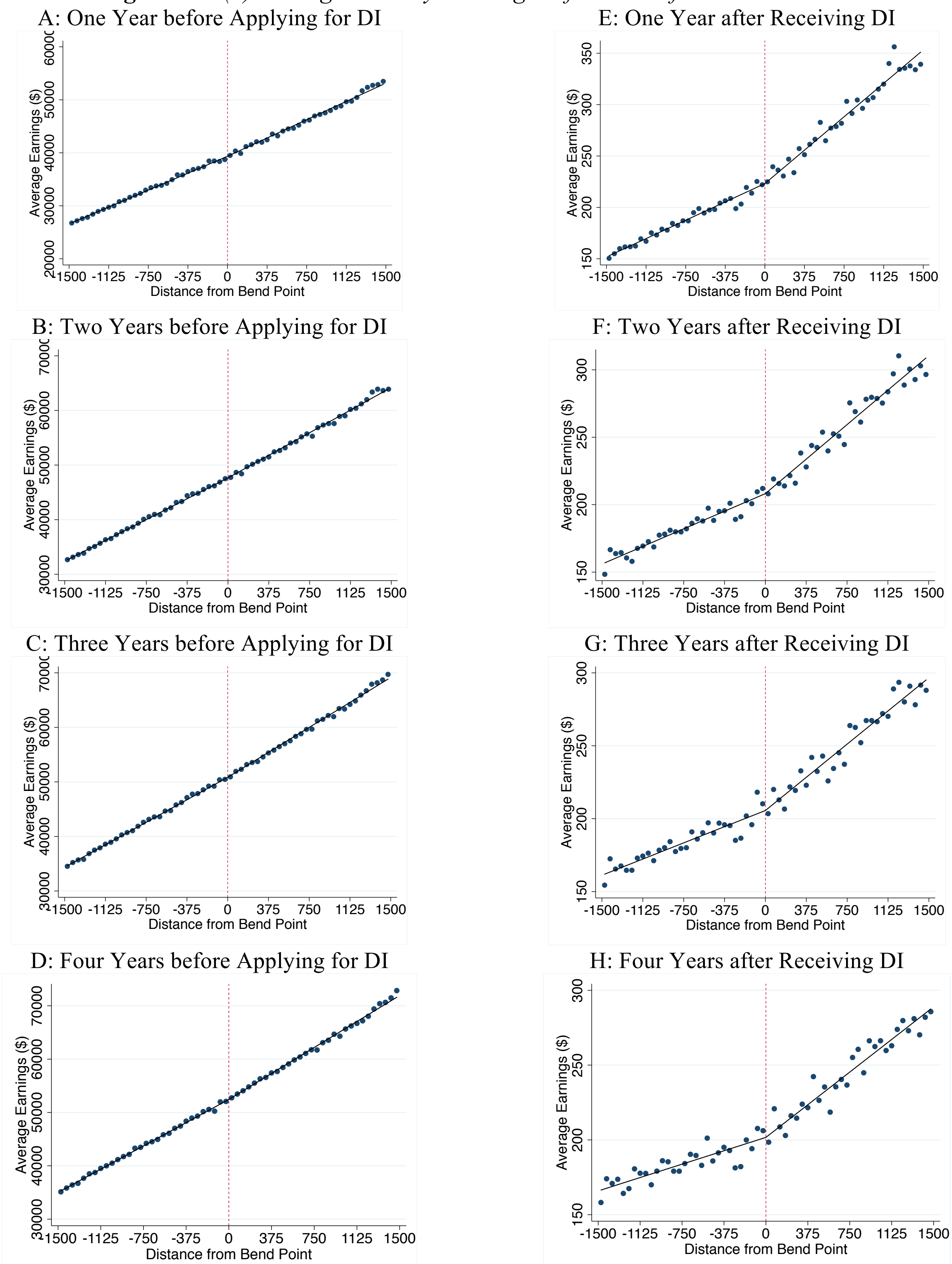
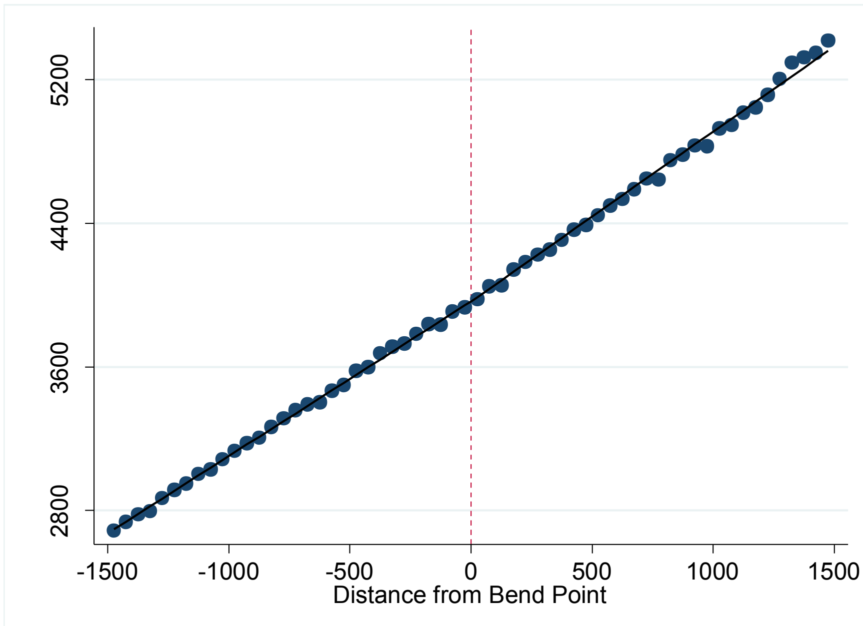
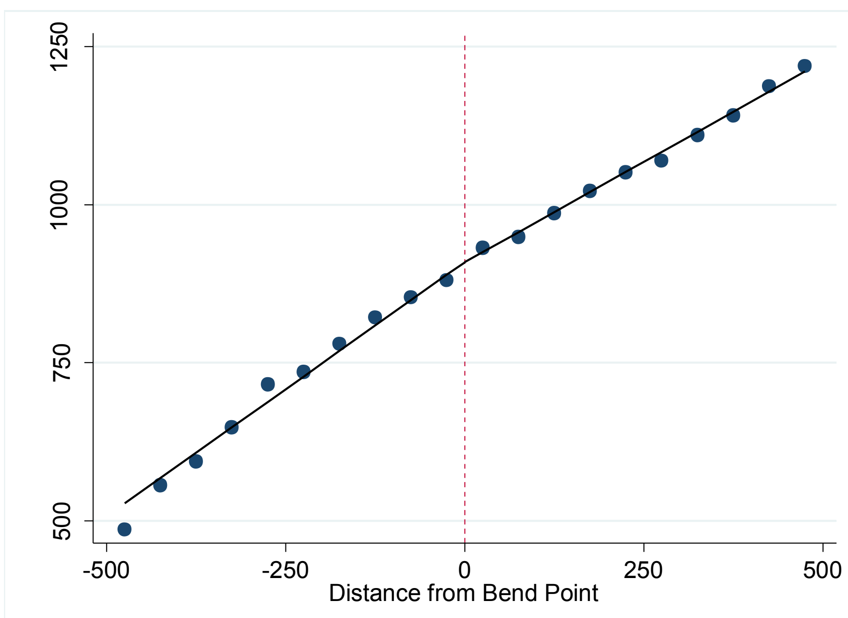


Figure A6. *(b) Average Monthly Earnings in the Four Years before Applying for DI, Aggregating over all Four Years*

A: Upper Bend Point

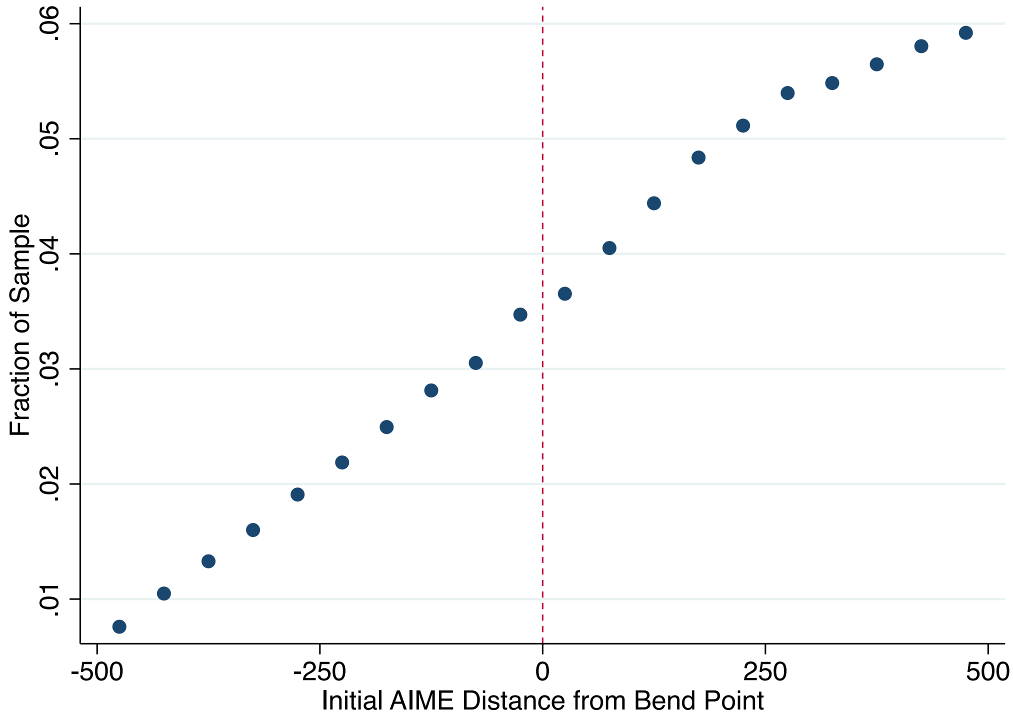


B: Lower Bend Point



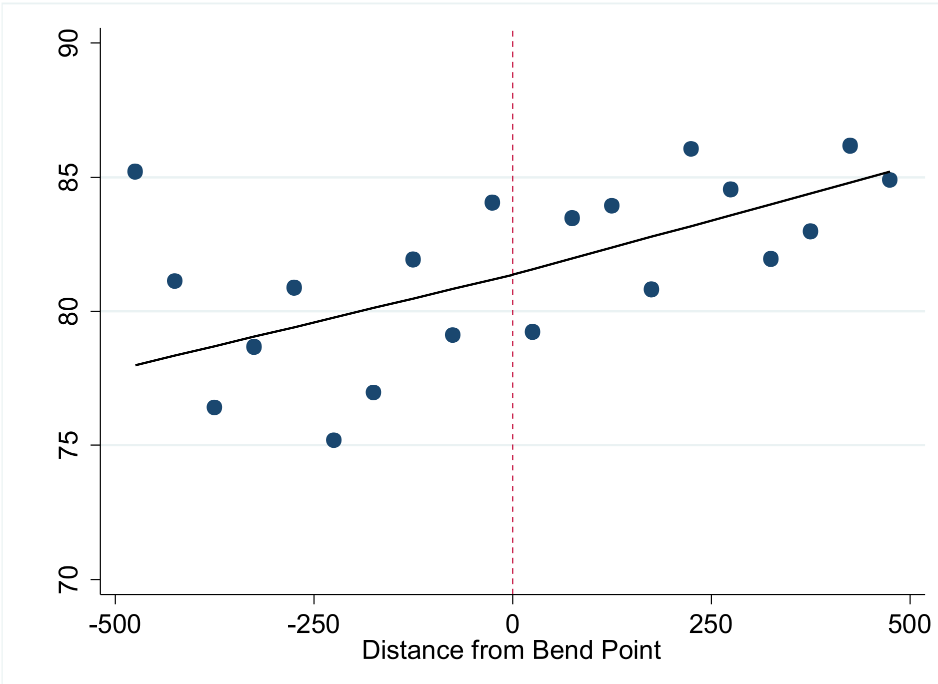
Notes: Panel (a) of the figure is identical to Figure 4 but uses the same formatting as elsewhere in the paper, *i.e.* with $50 bins. Panel (b) shows mean monthly earnings over all four calendar years prior to applying to DI, in $50 bins, as a function of distance from a bend point. At the lower bend point in Panel (b) there appears to be a slight decrease in slope at the bend point, though this is not statistically robust as we show in the appendix table estimates of the placebo effects at the lower bend point. See notes to Figure 2.

Figure A7. *Initial Density around the Lower Bend Point*



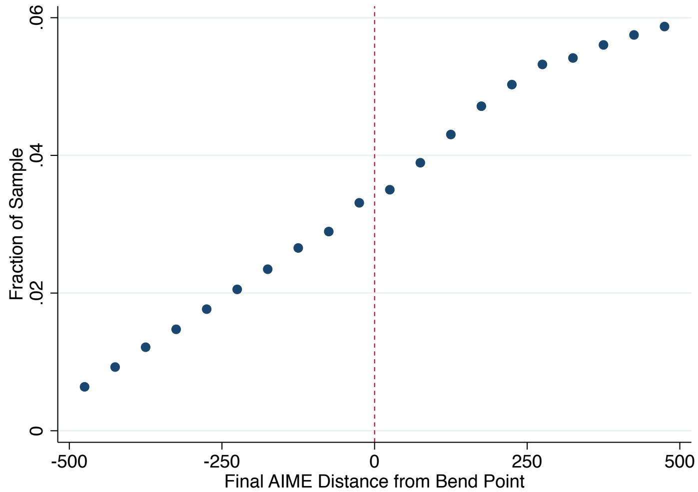
Notes: The figure shows the number of observations in $50 bins as a function of distance from the bend point. The figure shows that the number of observations appears smooth through this bend point. The sample includes DI beneficiaries within $500 of the lower bend point. The AIME of $791 constrains the bandwidth to a value less than that (given that we seek to use a bandwidth that is symmetric on both sides of the bend point). In practice, there are almost no observations below an AIME of $200, as beneficiaries with such low earnings are unlikely to have sufficient quarters of coverage to qualify for DI. Therefore, we use a baseline bandwidth of $500 at the lower bend point. See other notes to Figure 2.

Figure A8. *Average Monthly Earnings in the Four Years after DI Allowance around the Lower Bend Point*



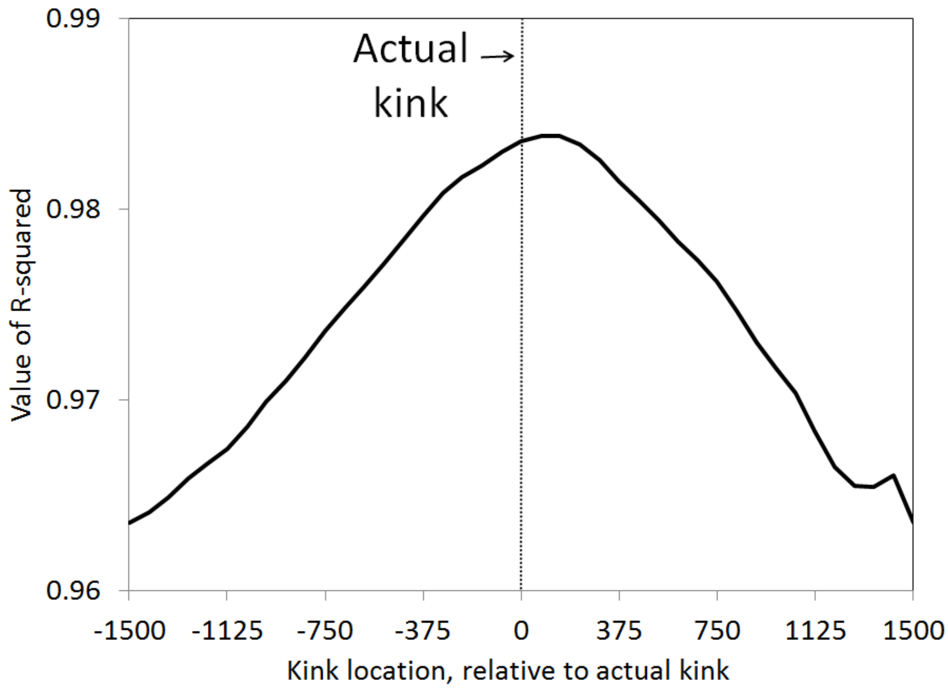
Notes: The figure shows mean monthly earnings in the four years after going on DI, in $50 bins, as a function of distance from the bend point. The figure shows that there is little change in the slope of mean earnings above the upper bend point compared to below it. See other notes to Figure 2.

Figure A9. *Fraction of Sample in Each Bin around the Lower Bend Point as a Function of Final AIME*



Notes: The figure shows that the density is smooth around the lower bend point when plotted in bins defined relative to the *final* AIME observed in our four-year period. This demonstrates that substitution effects are not evident. If substitution effects were operating, we should see bunching in final AIME at the bend point. The fraction of the sample in each bin is calculated by dividing the number of beneficiaries in each bin by the total number of beneficiaries whose AIME is within $500 of the upper bend point. See other notes to Figure 8.

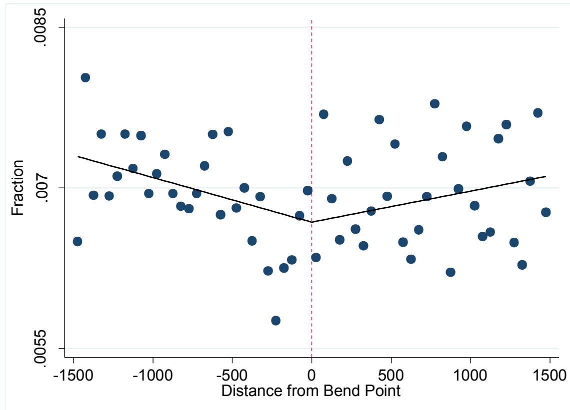
Figure A10. *R-squared as a function of "placebo" kink location around the upper bend point*



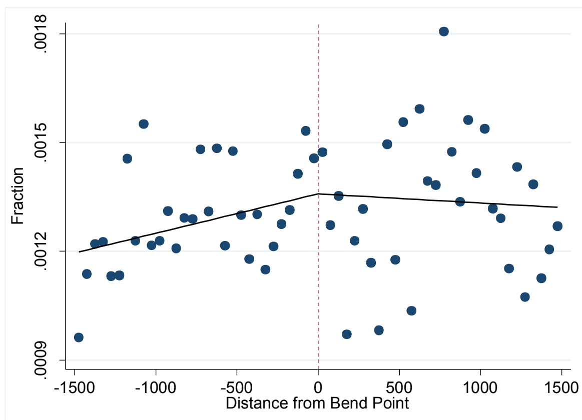
Notes: following Landais (2014), we show the R-squared of the baseline model when the kink is placed at "placebo" locations around the upper bend point. The R-squared is maximized close to the actual bend point, suggesting that we are estimating a true kink in the outcome.

Figure A11. *DI Work-related Outcomes around Upper Bend Point in the Four Years after DI Allowance*

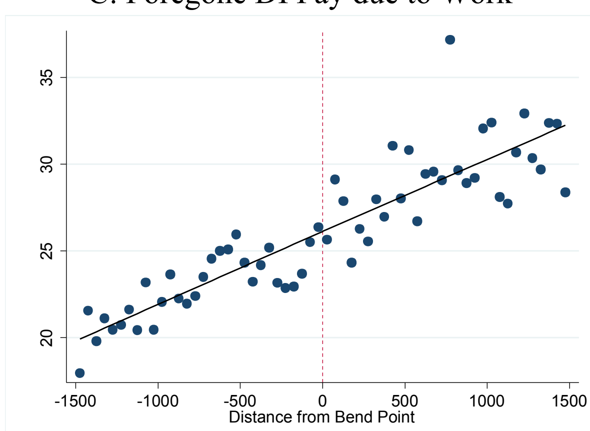
A: Fraction Suspended for Work



B: Fraction Terminated for Work

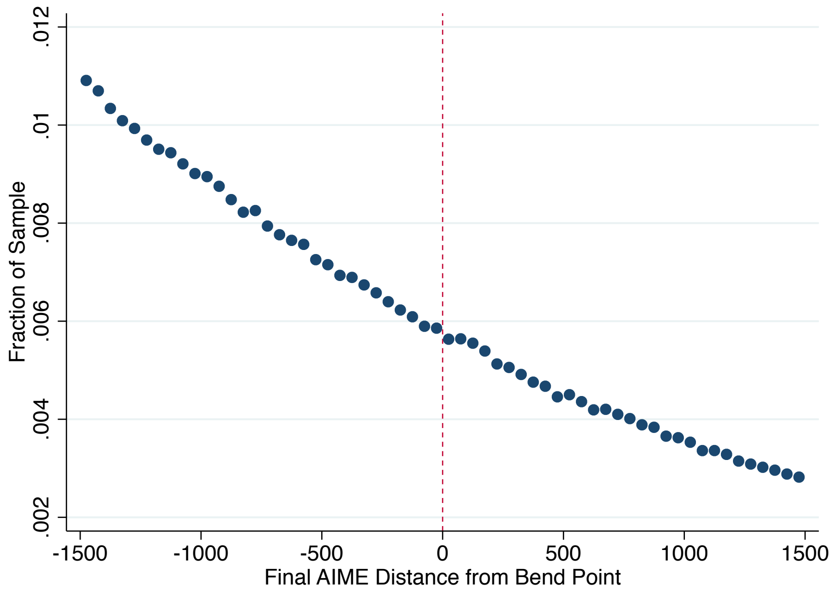


C: Foregone DI Pay due to Work



Notes: These program outcomes are averaged over the first four years on DI , so that the figure shows annualized means. See the notes to Figure 2 and the text for more details.

Figure A12. *Final Density around the Upper Bend Point*



Notes: The figure shows that the density of *final AIME* is smooth around the upper bend point. This demonstrates that substitution effects are not evident; if substitution effects were operating, we should see bunching in final AIME at the bend point. The fraction of the sample in each bin is calculated by dividing the number of beneficiaries in each bin by the total number of beneficiaries whose final AIME is within $1,500 of the upper bend point. Final AIME represents AIME after having been on DI for four years. Note that Figure A12 is subtly different than Figure 2.

Table A1. *Testing for a Discontinuity in the Densities*

	Specification		
	Linear	Quadratic	Cubic
	(1)	(2)	(3)
		A: Upper bend point	
Estimated discontinuity	39.52	**39.52**	23.48
	(322.72)	**(66.91)**	(93.67)
AICc	935.13	**751.42**	753.63
		B: Lower bend point	
Estimated discontinuity	864.01	864.01	**-259.16**
	(473.87)	(467.64)	**(411.00)**
AICc	310.74	304.48	**286.73**

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows that there is no robust discontinuity in the *level* of the density of the number of observations, considered as a function of AIME distance to the bend point, under linear, quadratic, or cubic specifications, at either bend point. See notes to Tables 1, 2, and 3.

Table A2. *Income Effect of DI Benefits on Earnings around the Upper Bend Point Reporting the Full Set of Covariates*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
β_2 (x100)	3.448***	3.434***	3.272***	4.148***	4.185***	4.567***	4.295***	4.282***	4.654***
	(0.381)	(0.384)	(0.652)	(1.403)	(1.445)	(1.453)	(1.485)	(1.492)	(1.561)
AIME (x100)	3.397***	3.372***	3.341**	3.043***	2.979***	3.425***	3.043***	3.020***	3.257**
	(0.187)	(0.240)	(1.317)	(0.688)	(0.761)	(1.284)	(0.696)	(0.802)	(1.516)
AIME^2 (x10,000)				-0.023	-0.025	-0.057	-0.029	-0.029	-0.058
				(0.044)	(0.046)	(0.055)	(0.047)	(0.048)	(0.057)
AIME^3 (x1,000,000)							-0.001	-0.000	-0.001
							(0.002)	(0.002)	(0.002)
Discontinuity		0.625			0.887			0.309	
		(3.323)			(3.447)			(4.616)	
Age at filing			-2.972			-8.251			-7.734
			(6.826)			(8.587)			(8.614)
Fraction male			88.397			72.108			90.347
			(155.00)			(144.85)			(172.88)
Fraction black			99.679			161.24			159.06
			(243.37)			(266.77)			(271.31)
Fraction allowed at hearings level			32.271			17.346			17.937
			(189.04)			(190.79)			(193.74)
Constant	209.67***	209.41***	274.56***	208.78***	208.34***	547.08	208.68***	208.54***	507.98
	(1.643)	(2.220)	(368.41)	(2.535)	(3.231)	(452.64)	(2.582)	(3.481)	(470.38)
R-squared	(2.242)	(2.261)	(3.835)	(8.254)	(8.500)	(8.547)	(8.734)	(8.777)	(9.183)
AICc	377.56	379.73	386.29	379.46	381.67	387.75	381.62	383.99	390.33
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes
Cents per $1 less DI	20.284	20.199	19.248	24.400	24.618	26.867	25.268	25.185	27.376

Notes: ** denotes p<0.05, *** denotes p<0.01. The table contains the full results of model (2) run within $1,500 of the upper bend point for all nine specifications. β_2 refers to the change in slope at the bend point, from regression (2) in the main text. The estimates in the last row are equal to β_2 scaled by the 17 percentage point change in the slope of PIA as a function of AIME at the upper bend point, when it moves from 32 to 15 percent. The "AICc" is the corrected Akaike Information Criterion. For more information, see notes to Tables 1 and 4.

Table A3. *Smoothness of the Densities and Predetermined Covariates around the Lower Bend Point*

Dependent variable	Polynomial minimizing AICc (1)	Estimated kink (2)	Fraction of statistically significant [p=0.05] kinks for polynomials of order 3-12 (3)
Number of observations	12	-14.2 (34.4)	10%
Fraction male (x 1,000)	12	-0.264 (0.803)	0%
Average age when filing for DI (x 1,000)	12	3.84 (15.6)	10%
Fraction black (x 1,000)	12	-0.431 (0.336)	0%
Fraction of hearings allowances (x 1,000)	12	-0.127 (0.945)	0%
Fraction with mental disorders (x 1,000)	12	0.421 (0.483)	0%
Fraction with musculo. conditions (x 1,000)	12	-0.272 (0.641)	0%
Fraction SSI Recipients (removed from main sample) (x 1,000)	12	-0.034 (0.059)	0%

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows that pre-determined variables (*i.e.* demographics and number of observations) are smooth around the bend points. For each of the dependent variables, the table shows: the order of the polynomial that minimizes the corrected Akaike Information Criterion (AICc) (Column 1); the estimated change in slope at the bend point and standard error (Column 2) under the specification with the AIC-minimizing polynomial; and the fraction of the regressions with polynomial orders between 3 and 12 that show a change in slope that is statistically significant at the five percent level (Column (3). See other notes to Table 2.

Table A4. *Effect of DI Benefit on Earnings in the Four Years after Entering DI, Lower Bend Point*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Average Earnings Over the First Four Years									
Cents per $1 of less DI	**-0.17**	-0.24	4.45	3.17	3.40	6.09	6.97	7.88	7.41
	(1.35)	(1.55)	(4.24)	(5.79)	(6.44)	(6.30)	(6.19)	(4.91)	(7.67)
AICc	**93.78**	96.54	100.27	96.02	99.04	104.89	95.45	97.79	110.29
First Year									
Cents per $1 of less DI	-2.21	-2.61	3.66	5.95	6.88	9.48	**11.26**	11.33	11.78
	(1.43)	(1.66)	(3.18)	(5.24)	(6.60)	(4.25)	**(5.53)**	(5.24)	(5.25)
AICc	97.44	99.17	97.62	97.28	98.02	98.59	**92.93**	96.54	102.82
Second Year									
Cents per $1 of less DI	-0.89	-1.19	**6.78**	0.07	0.61	5.37	5.44	6.30	5.99
	(1.59)	(1.76)	**(4.08)**	(6.71)	(7.90)	(6.33)	(7.34)	(6.14)	(7.97)
AICc	103.24	105.62	**102.26**	106.01	108.68	106.97	104.54	107.44	112.66
Third Year									
Cents per $1 of less DI	**0.24**	0.28	2.08	-0.45	-0.53	0.29	2.38	3.53	2.20
	(1.53)	(1.46)	(6.09)	(6.72)	(6.96)	(7.75)	(6.87)	(5.54)	(9.63)
AICc	**101.02**	103.81	112.63	103.80	106.96	117.34	105.64	108.03	122.70
Fourth Year									
Cents per $1 of less DI	**2.18**	2.56	5.27	7.13	6.63	9.20	8.79	10.34	9.68
	(1.59)	(1.86)	(5.54)	(6.97)	(6.01)	(8.96)	(7.42)	(5.54)	(9.76)
AICc	**93.64**	95.30	106.21	95.17	97.59	110.00	97.64	97.72	115.72
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: See notes to Table 1. The table reports coefficients and standard errors showing the estimated effect of a one-dollar increase in yearly DI payments on yearly earnings. "AICc" is the corrected Akaike Information Criterion. The estimates are based on regression model (2) in the text, which is a regression kink design based on relating earnings to the distance between a beneficiary's AIME and the bend point of the formula transforming AIME into PIA. The data are from SSA administrative records. The bolded estimates minimize the AICc within each row.

Table A5. *Placebo Tests using Earnings in the Four Years before Applying for DI at the Upper Bend Point*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Average Earnings in Four Years Before									
Cents per $1 of less DI	-50.85***	-54.65***	-66.10***	-27.50	-20.09	5.40	**10.88**	11.40	28.73
	(8.46)	(8.38)	(11.42)	(25.02)	(27.78)	(28.46)	**(24.24)**	(24.92)	(25.31)
AICc	534.07	528.75	539.03	535.53	529.16	533.49	**512.27**	514.63	514.49
First Year Before									
Cents per $1 of less DI	-33.90***	-37.69***	-51.74***	-22.19	-14.97	-0.93	**17.56**	18.94	21.11
	(9.93)	(10.19)	(18.22)	(34.14)	(39.55)	(34.57)	**(34.96)**	(34.20)	(35.69)
AICc	551.42	548.11	553.50	553.49	549.82	552.99	**536.43**	538.67	542.60
Second Year									
Cents per $1 of less DI	-43.30***	-47.81***	-57.11***	-29.44	-20.85	-0.49	**4.54**	1.22	20.22
	(9.42)	(9.43)	(11.96)	(28.42)	(28.65)	(34.03)	**(32.39)**	(33.96)	(37.12)
AICc	553.54	548.08	559.75	555.56	549.56	558.85	**544.88**	546.55	551.31
Third Year									
Cents per $1 of less DI	-59.45***	-64.14***	-78.43***	-26.73	-17.51	20.07	**15.08**	13.52	44.15
	(9.69)	(9.37)	(13.25)	(30.08)	(31.98)	(33.17)	**(29.08)**	(29.41)	(29.34)
AICc	552.77	546.49	555.06	553.89	546.22	545.47	**534.53**	536.72	529.69
Fourth Year									
Cents per $1 of less DI	-66.77***	-68.97***	-77.11***	-31.63	-27.02	2.96	6.35	11.92	**29.45**
	(10.61)	(10.49)	(14.42)	(35.11)	(37.94)	(40.32)	(34.89)	(33.72)	**(31.79)**
AICc	553.84	554.33	559.39	554.82	554.81	554.77	540.16	540.33	**538.22**
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows that there is no robust significant change in the slope of earnings in "placebo" years *before* individuals go on DI in the four years prior to going on DI (either combined or separately), paralleling the visual patterns shown in Figure 4 and Appendix Figure A6. In particular, there is no effect that is robust and significant across all nine specifications, in contrast to the main results shown in Table 4. See notes to Tables 1 and 4.

Table A6. *Estimates of the Earnings Effects in the Four Years after DI Allowance using Individual-level Data, Different Bin Sizes, or an Expanded Sample*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Main – Using $50 bins									
Cents per $1 of less DI	**-20.28*****	-20.20***	-19.25***	-24.40***	-24.62***	-26.87***	-25.27***	-25.19***	-27.38***
	(2.24)	(2.26)	(3.83)	(8.25)	(8.50)	(8.55)	(8.73)	(8.78)	(9.18)
AICc	**377.56**	379.73	386.29	379.46	381.67	387.75	381.62	383.99	390.32
Using individual-level data									
Cents per $1 of less DI	-20.20***	-20.09***	**-15.00*****	-23.64***	-23.88***	-28.55***	-24.47***	-24.32**	-27.88***
	(2.36)	(2.46)	**(2.32)**	(8.51)	(8.58)	(8.35)	(9.40)	(9.57)	(9.21)
AICc	7815095.6	7815098.5	**7792125.3**	7815098.4	7815101.3	7792125.4	7815101.3	7815104.3	7792128.3
Using $25 bins									
Cents per $1 of less DI	**-20.21*****	-20.10***	-19.03***	-23.85***	-24.11***	-27.92***	-24.80***	-24.68***	-27.66***
	(2.34)	(2.42)	(3.52)	(8.48)	(8.51)	(9.25)	(9.07)	(9.37)	(9.70)
AICc	**853.15**	855.19	855.26	855.05	857.08	856.15	857.04	859.21	858.44
Using $100 bins									
Cents per $1 of less DI	**-20.41*****	-20.35***	-20.45***	-23.89***	-24.06**	-25.63**	-24.78**	-24.80**	-26.10**
	(2.37)	(2.27)	(4.44)	(8.32)	(9.02)	(10.40)	(9.24)	(8.96)	(11.14)
AICc	**167.05**	169.49	177.68	169.27	171.88	180.60	171.77	174.67	180.45
Including Beneficiaries with More than Four AIME Changes									
Cents per $1 of less DI	**-21.25*****	-21.18***	-21.65***	-24.21***	-24.37***	-21.72***	-25.38***	-25.51***	-23.20***
	(2.50)	(2.50)	(3.86)	(9.51)	(9.89)	(10.40)	(10.68)	(10.73)	(10.96)
AICc	**337.08**	339.25	345.64	338.76	340.95	346.56	340.29	342.57	348.77
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows that the basic results are robust to choosing other bin sizes or to running the regressions at the individual level (rather than the bin level). In the individual-level regressions, we cluster by $50 bin; the standard errors are also similar when we cluster at other bin sizes. The standard errors are smaller when we do not cluster, reinforcing our conclusion that the estimates are precise. "AICc" is the corrected Akaike Information Criterion. See notes to Tables 1 and 4.

Appendix Table A7. *Effect on DI Program Outcomes in the Four Years after Allowance*

	Linear models			Quadratic models			Cubic models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	A: Annual Probability of Suspended DI Payments due to Working								
Upper bend point									
p.p. change per	-0.035**	-0.031	-0.028	-0.081	-0.088	-0.123**	-0.085	-0.073	**-0.128****
$1,000 less DI	(0.016)	(0.016)	(0.024)	(0.062)	(0.063)	(0.058)	(0.060)	(0.062)	**(0.058)**
AICc	-753.99	-753.81	-752.19	-754.88	-755.23	-756.62	-754.97	-756.20	**-756.86**
Lower bend point									
p.p. change per	**-0.011**	-0.012	-0.024	-0.082	-0.083	-0.061	-0.087	-0.087	-0.061
$1,000 less DI	**(0.022)**	(0.022)	(0.049)	(0.073)	(0.079)	(0.081)	(0.089)	(0.087)	(0.110)
AICc	**-252.64**	-249.86	-243.47	-251.09	-247.93	-239.13	-251.12	-247.95	-239.13
	B: Annual Probability of Termination from DI due to Working								
Upper bend point									
p.p. change per	0.007	0.005	0.002	-0.033	-0.031	-0.038	**-0.031**	-0.034	-0.038
$1,000 less DI	(0.004)	(0.005)	(0.007)	(0.019)	(0.019)	(0.020)	**(0.020)**	(0.020)	(0.021)
AICc	-877.76	-877.27	-873.70	-883.09	-881.93	-879.59	**-883.19**	-882.16	-879.59
Lower bend point									
p.p. change per	-0.002	0.001	0.037**	-0.006	-0.011	0.022	-0.013	-0.005	**0.017**
$1,000 less DI	(0.009)	(0.007)	(0.014)	(0.035)	(0.031)	(0.017)	(0.042)	(0.032)	**(0.022)**
AICc	-290.40	-290.17	-290.89	-290.42	-290.43	-291.79	-288.05	-287.52	**-292.05**
	C: Foregone DI Payments due to Working								
Upper bend point									
Cents per $1 of less	0.04	0.18	-0.004	-4.60	-4.93**	-5.81**	**-5.46****	-5.16	-6.48**
DI	(0.60)	(0.60)	(1.113)	(2.42)	(2.39)	(2.27)	**(2.62)**	(2.68)	(2.45)
AICc	233.15	233.90	239.21	230.72	230.40	233.87	**229.02**	230.28	231.65
Lower bend point									
Cents per $1 of less	**-0.65**	-0.61	1.25	0.90	0.88	2.04	1.41	1.63	2.14
DI	**(0.38)**	(0.40)	(1.14)	(1.70)	(1.81)	(1.88)	(2.05)	(1.69)	(2.33)
AICc	**39.24**	41.84	46.21	40.10	43.24	50.21	42.27	44.79	55.93
Discontinuity	--	Yes	--	--	Yes	--	--	Yes	--
Covariates	--	--	Yes	--	--	Yes	--	--	Yes

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows estimates of model (2) when the dependent variable is a dummy for whether the beneficiary had suspended DI payments due to working (panel A); a dummy for termination of DI due to working (panel B); and the value of foregone DI payments due to sufficiently high non-DI earnings (panel C). We run linear probability models. "AICc" is the corrected Akaike Information Criterion. See notes to Tables 1 and 4.

Appendix Table A8. *Estimates of Final Excess Mass in AIME*

	Baseline (1)	Covariates (2)	Alternative bandwidth (3)	8^{th}-degree polynomial (4)	Excluded region \$200 (5)
γ (x 10,000)	-0.091	-0.26	-0.54	-0.35	0.036
	[-1.01, 0.83]	[-1.18, 0.66]	[-1.65, 0.56]	[-1.24, 0.55]	[-0.68, 0.75]

Notes: ** denotes p<0.05, *** denotes p<0.01. The table shows the point estimates and 95 percent confidence interval for γ, the coefficient on the dummy for being near the kink, defined in terms of final AIME, from regression (4) (reflecting the excess mass per bin near the kink), which is multiplied by 10,000 for the reader's ease. The mean density per bin (multiplied by 10,000) in the two bins immediately outside those nearest to the kink is 57.68. The table shows that the estimated values of γ are negligible relative to the underlying density (on the order of 0.1 percent as large), indicating that there is no evidence of excess bunching at this kink and therefore no evidence of a substitution effect. "Baseline" refers to estimating a seventh-degree polynomial through the earnings distribution using a bandwidth of $1,500 and estimating the kink from a region within $100 of the bend point. We take bin means of variables in 60 equally-sized bins of $50 width around the upper bend point, so that each regression has 60 observations. "Covariates" (Column 2) refers to a specification controlling for covariates within each bin (mean age, percent male, percent black, and percent allowed at the hearings stage). "Alternative bandwidth" (Column 3) refers to using a bandwidth of $650—the bandwidth selected by the procedure of Calonico, Cattaneo, and Titiunik (2014a, 2014b)—rather than $1,500. "Eighth-degree polynomial" (Column 4) estimates an eighth-order polynomial through the density rather than a seventh-order. "Excluded region $200" (Column 5) refers to estimating the kink from a region of $200 around the bend point, rather than $100. Appendix Table A8 shows the implied elasticities, estimated using the model of Saez (2010), which require more assumptions and should be viewed as illustrative of the relevant range of the elasticity. In these estimates, the estimated elasticities cluster near zero, with confidence intervals that rule out elasticities larger than a moderate level (0.36 or smaller).