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The Effect of Disability Insurance Payments on Beneficiaries' Earnings†

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A crucial issue is whether social insurance affects work decisions through income or substitution effects. We examine this in the context of US Social Security Disability Insurance (DI), exploiting discontinuous changes in the benefit formula with a regression kink design to estimate the income effect of payments on earnings and employment. Using administrative data on all new DI beneficiaries from 2001 to 2007, our preferred estimate is that an increase in DI payments of $1 causes an average decrease in beneficiaries' earnings of $0.20 and that annual employment rates decrease by 1.3 percentage points per $1,000 of DI payments. These findings suggest that the income effect accounts for a majority of DI-induced reductions in earnings. (*JEL* E24, H55, J14, J22, J31)

A core issue in public and labor economics is how public programs affect work decisions. In order to predict the effects of policy reforms on work and estimate their welfare consequences, researchers need not only "reduced form" estimates of program effects but also decompositions into income and substitution effects. Standard public finance analysis indicates that only substitution effects, not income effects, lead to distortions (in the absence of a preexisting distortion). If income effects are important, this leaves less room for distortionary moral hazard.

The primary goal of this paper is to estimate the income effects of Social Security Disability Insurance (DI) benefit payments on beneficiaries' earnings and employment. DI protects workers against the risk of disability through cash payments and Medicare eligibility. Approximately 7 percent of federal outlays are spent on DI

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and associated Medicare expenses, and around 5 percent of 25–64-year-olds receive DI. Since 1979, the fraction of the population on DI has increased by more than 2 percentage points, and real expenditures on DI and associated Medicare expenditures have more than tripled (Bureau of the Fiscal Service 2015, Goss 2015). DI, therefore, represents an important setting for understanding the balance between income and substitution effects. 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; and Autor et al. 2015).[1] 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; and 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 Svensson 2008; Staubli 2011; and von Wachter, Song, and Manchester 2011).[2] Some have argued that the growth of DI has played a sizable role in the long-run US trend toward decreasing labor force participation (Parsons 1980, Autor and Duggan 2003). Across this literature, decreases in work have often been interpreted as reflecting distortionary moral hazard caused by substitution effects; for example, Gruber (2013, 406) includes a section on "The Moral Hazard Effects of DI."

However, the effects of DI on work 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 (Office of the Actuary 2013; Office of Retirement and Disability Policy and Office of Research, Evaluation, and Statistics 2014).[3] If leisure is a normal good, these transfers should induce beneficiaries to work less.

Autor and Duggan (2007) point out that we need to understand DI's income effect in order to understand the program's welfare implications. 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 US 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.[4] However, such a proposal would not increase earnings or employment to the extent that income effects operate. By contrast, 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

[1] 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).

[2] 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).

[3] We calculate the value of Medicare benefits as the total expenditures by DI beneficiaries minus their premiums, divided by the number of DI beneficiaries. Here and elsewhere, amounts are expressed in real 2013 dollars.

[4] See http://www.washingtonexaminer.com/gop-plans-overhaul-for-social-security-disability/article/2560440.

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.

Our main outcome is pretax 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 Social Security Administration (SSA) data on all new DI beneficiaries between 2001 and 2007 and a regression kink design (RKD) to exploit discontinuities in the formula relating DI cash benefits to prior earnings (Nielsen, Sørensen, and Taber 2010; Card et al. 2015). 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.

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, although interactions with Supplemental Security Income (SSI) and other program rules confound the analysis at the lower bend point. 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 $1, beneficiaries decrease their earnings by $0.20. 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 reflecting only an income effect, defined as the marginal effect of unearned income on earnings. The existence of the substantial gainful activity (SGA) limit, which in 2016 requires beneficiaries to keep their monthly earnings under $1,130 to retain eligibility over an extended period, could limit individuals' responses to DI transfer income if they avoid going above the limit. Thus, our estimates, if anything, should reflect a lower bound of the response we might expect in the absence of the constraint imposed by SGA. Since this lower bound is large, our conclusion is that the income effect is large. 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. 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.

Despite the importance of estimating the income effect of DI, it has been considered difficult to do so. Autor and Duggan (2007, 120) write: "The DI program has provided benefits exclusively on a work-contingent basis, so income and substitution

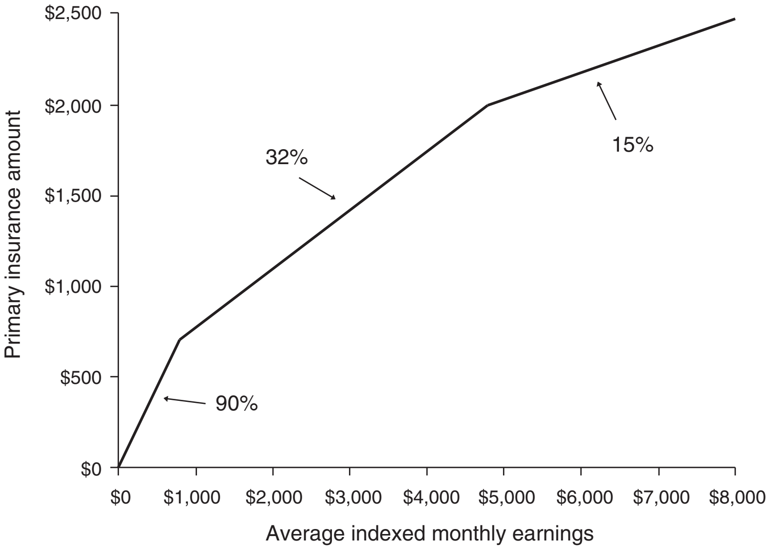

Figure 1. Primary Insurance Amount as a Function of Average Indexed Monthly Earnings

Notes: The figure shows the primary insurance amount (PIA) as a function of average indexed monthly earnings (AIME) in 2013. The percentages are marginal replacement rates.

Source: SSA (2013)

effects cannot readily be separated." Our paper helps to fill this gap, complementing a small set of papers that examine income effects in other disability contexts. Autor and Duggan (2007) and Autor et al. (2016) examine an income effect of changing access to Veterans' Administration (VA) compensation for Vietnam War veterans on labor force participation, employment, and earnings.[5] Marie and Vall Castello (2012) and Bruich (2014) study the income effect of DI benefits in Spain and Denmark, respectively. Finally, Deshpande (2016) 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.[6] Our paper is the first to estimate an income effect specifically in the context of DI in the United States, which is the largest US federal expenditure on the disabled and one of the largest social insurance programs in the United States and around the world.[7]

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

[5] Both studies estimate the reduced-form effects of receiving VA Disability Compensation. Autor et al. (2016, 3) conclude that "the effects that we estimate are unlikely to be driven solely by income effects."

[6] In the context of US Civil War veterans, Costa (1995) finds large income effects of pensions on labor supply.

[7] Low and Pistaferri (2015) estimate many parameters simultaneously, including parameters of the work decision.

I. Policy Environment

DI insures workers for disabilities that are judged to prevent them from earning above SGA. The rules relating to DI eligibility and SGA are relevant to our analysis because the SGA limit could constrain beneficiaries' responses to DI cash benefits, as beneficiaries seek to retain DI eligibility by remaining below the limit.

However, only a small fraction of our sample is directly subject to the SGA limit. 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 beneficiaries who have completed a TWP ("TWP completers"). TWP completers accounted for only 0.9 percent of DI beneficiaries in 2012 (Office of Retirement and Disability Policy and Office of Research, Evaluation, and Statistics 2013). Among all DI beneficiaries, few exit DI. For example, 0.4 percent of all DI beneficiaries had their eligibility terminated because of substantial work in 2012 (Office of Retirement and Disability Policy and Office of Research, Evaluation, and Statistics 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 Office of Retirement and Disability Policy and Office of Research, Evaluation, and Statistics 2013). Moreover, calculating AIME requires inflating earnings in one's highest earning years by the National Average Wage Index in each year.[8] 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 (Office of Retirement and Disability Policy and Office of Research, Evaluation, and Statistics 2013). After a beneficiary goes on DI, DI benefits are determined by adjusting PIA through a COLA. This complexity may limit individuals' ability to accurately estimate their AIME and sort around the bend points on this basis.

The usefulness of the lower bend point as a source of variation is limited by three factors. First, SSI eligibility 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. Some individuals are dually eligible for both SSI and DI. Dual-eligibles whose PIA is below the SSI monthly federal benefit rate receive SSI payments that raise their combined monthly benefits (summed over DI and SSI) to the federal benefit rate, while dual-eligibles whose PIA is above that amount only receive SSI benefits during the DI waiting period. The maximum 2013 SSI monthly federal benefit rate, \$710, is nearly identical to \$712, the PIA at the

[8] 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.

lower bend point.[9] This means that for dual-eligibles the slope of net disability benefits (summed over DI and SSI) as a function of AIME increases from 0 to 32 percent (instead of 90 to 32 percent). This is shown in online Appendix Figure A1.

It is difficult to use this variation in a clean way because it is confounded with other policy variation. Those with PIA below the SSI monthly federal benefit rate are eligible for Medicaid through SSI, whereas those above are eligible for Medicare through DI. Those below are subject to an SSI 50 percent cash benefit reduction rate in current earnings, whereas those above are only subject to the DI SGA rules. We therefore do not include dual-eligibles in our sample. However, dual-eligibles represent 70 percent of all DI beneficiaries in the region of the lower bend point, meaning our estimates at this bend point apply to a highly selected sample.

Second, DI family payment rules complicate measurement of the incentives near the lower bend point, as they also imply nearly smooth payments through this bend point for those with dependents. The maximum benefits that can be paid to the disabled worker plus 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, as shown in online Appendix Figure A1. This suggests that the reaction to the changes in slope may be difficult to detect for this group. Although in principle this raises the possibility that we could simply limit the sample to those without dependents, in fact, we cannot confidently identify whether a beneficiary has dependents near the lower bend point because the family maximum differentially affects the incentive to report dependents below versus above the lower bend point: additional dependents lead to additional benefits above, but not below, the lower bend point. Online Appendix Figure A2 shows that the number of beneficiaries with reported dependents indeed increases sharply above the lower bend point (even though the number of beneficiaries does not rise sharply, as shown in online Appendix Figure A3).

Third, only a small bandwidth can be used under the lower bend point because the AIME value at the lower bend point is close to zero.

All of these factors suggest that a priori we expect that the variation around the lower bend point will not be as useful as that around the upper bend point. Online Appendix Figure A4 shows that only 10 percent of DI claimants near the upper bend point are dual-eligible; moreover, these dual-eligibles only receive SSI during the DI waiting period. The maximum SSI payment amount is far under PIA for beneficiaries near the upper bend point, implying negligible scope for interaction between DI and SSI. 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.

[9] SSI payments can be higher due to state supplements or if an eligible spouse is present; also, the payment can be less than the federal benefit rate due to earned or unearned income or in-kind support.

II. Identification Strategy

Card et al. (2015) 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 treatment variable and the induced kink in the outcome variable.[10] This is known as an RKD. Estimates can be interpreted as a local treatment-on-the-treated (TT) 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 pretax 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

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

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.

A "sharp" RKD only requires estimates of the numerator of (1), because the denominator is known. 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 is a sharp RKD, in which we estimate 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

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

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 $|A - A_0| \leq h$, where h is the bandwidth size. As in Card et al. (2015), we test for a change in slope by examining whether β_2 is significantly different from zero. We follow Card et al. (2015) in using White robust standard errors.

[10] 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.

In (2), i indexes bins of data. 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. 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 excess smoothing for regression discontinuity designs recommended by Lee and Lemieux (2010).[11] 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.[12] We also show the results when estimating our regressions at the individual level, or using other bin sizes.

Identification of the effect of DI benefits on earnings relies on two key assumptions (Card et al. 2015). First, in the neighborhood of the bend point, there is no discontinuity in the slope of the direct effect of AIME on earnings.[13] 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 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 to going on DI. Because calculating PIA on the basis of an individual's earnings history is complex, 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).[14] 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.

[11] We follow Landais (2015) in applying this to an RKD context.

[12] 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.

[13] For example, in principle, 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 on earnings.

[14] 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 two to three 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 to 17 months before or 12 months after filing). The resulting measurement error implies that around the bend points actual PIA should be a smooth function of PIA as estimated on the statement; and, 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).

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,b).

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. (2015) use linear and quadratic specifications. Calonico, Cattaneo, and Titiunik (2014b) propose an RKD estimator where a quadratic term in the assignment variable can be used to correct the bias in the linear estimator. Ganong and Jäger (2014) argue that cubic splines perform better than other estimators. Our approach is to estimate versions of equation (2) with linear, quadratic, or cubic controls for the assignment variables.

A final issue is whether to control for covariates (Ando forthcoming). We try both options. Thus, for each sample and outcome we will generally produce estimates using six regressions: the linear, quadratic, and cubic regressions; and a version of each including predetermined covariates.[15]

Interpretation of the RKD Estimates.—As a benchmark, in online Appendix 1 we present a standard life cycle labor supply model. In the life cycle 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. Under the assumptions of Stone-Geary utility and no uncertainty as in Imbens, Rubin, and Sacerdote (2001), we can express earnings in each year as a function of the annual DI transfer payment, as we describe in the online Appendix. We alternatively consider a static framework in online Appendix 1, which applies if individuals are myopic or liquidity constrained. In this framework, earnings in a given year instead depend on, among other things, transfer income in that year, which would motivate calculating the effect on yearly earnings of a marginal change in *contemporaneous* yearly DI payments.[16] 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.

Since the determination of PIA on the basis of AIME is deterministic, the "treated" group whose TT effects we identify consists of those with AIME at the upper bend point. When we examine subgroups, like those with specific impairments, we identify TT effects for those in these subgroups at the upper bend point.

[15] In the online Appendix, we also show a version of some specifications controlling for a discontinuity in the level of the dependent variable at the bend point to address any remaining concern that sorting around the bend point could cause a discontinuity in the level of the outcome variable.

[16] 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.

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 than the schedule of marginal replacement rates.

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.

III. 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 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, marital status, spousal earnings, or hours worked.[17]

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 primary measure of earnings excludes self-employment earnings, although we include self-employment earnings in subsequent analyses. 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

[17] In the Current Population Survey over the years of our SSA data (2001–2010), of those reporting that "Disability causes difficulty working," 42.76 percent were married.

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. In our main estimates we also remove those who died in the years after entering DI, which removes another 14 percent. Finally, in our main estimates, 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; and Moore 2015).

PIA is measured in pretax terms. By examining the effect of pretax 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 pretax 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 pretax benefits should reflect a lower bound on the effect of after-tax benefits. Figure 2 shows a "first stage" graph, illustrating that average PIA in the data changes slope at the upper bend point 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.[18]

[18] The finding that one-fifth to one-quarter of the sample has positive earnings and the finding that only a small fraction is suspended for work are consistent because the substantial majority of those with positive earnings earns below the SGA limit, and because only 1.8 percent of our sample completes a TWP. 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

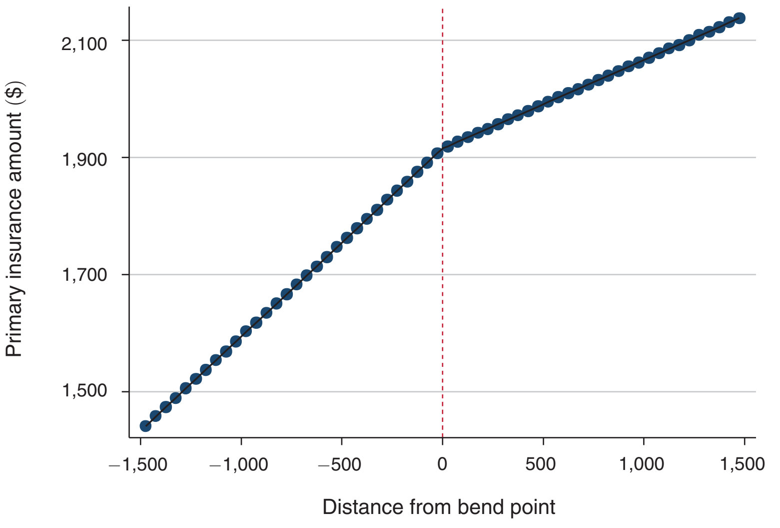

Figure 2. Observed Monthly DI Payments as a Function of Average Indexed Monthly Earnings around the Upper Bend Point

Notes: The figure shows actual DI payments (as measured in our data) as a function of AIME. The figure shows that the effective marginal replacement rate is very close to 32 percent below the upper bend point and very close to 15 percent above it.

Since our identification strategy examines earnings patterns around the upper bend point, which is the eighty-second percentile of AIME, the estimates will be local to this region. However, Table 1 shows that 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 has a higher fraction of males. Additionally, the sample around the upper bend point spans from the fifty-ninth percentile of AIME to the ninety-fifth percentile, and thus represents a substantial fraction of beneficiaries (online Appendix Figure A5).

IV. Graphical and Regression Analysis of Income Effects

A. *Preliminary Analysis*

We begin with validity checks on the empirical method. Figure 3 shows that the number of observations and its slope appear continuous around the upper bend point. The figure also 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

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 2014).

Table 1—Summary Statistics

	Upper bend point sample		Full sample	
	Mean	Standard deviation	Mean	Standard deviation
Panel A. Demographic and 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	$27,013
Fraction with any annual earnings:				
4 years before applying for DI	0.94	0.23	0.92	0.28
3 years before applying for DI	0.93	0.26	0.90	0.30
2 years before applying for DI	0.91	0.29	0.87	0.33
1 year before applying for DI	0.86	0.35	0.81	0.39
Panel B. DI information				
Primary insurance amount ($ monthly)	$1,773	$214	$1,369	$482
Annualized DI payments ($)	$21,276	$2,568	$16,428	$5,784
Fraction allowed DI via hearings	0.29	0.45	0.32	0.47
Fraction by primary disability type:				
Musculoskeletal conditions	0.35	0.48	0.35	0.48
Mental disorders	0.20	0.40	0.23	0.42
Circulatory conditions	0.12	0.33	0.10	0.30
Nervous system	0.08	0.28	0.08	0.27
Injuries	0.05	0.21	0.04	0.21
Respiratory conditions	0.03	0.18	0.03	0.18
Neoplasms	0.04	0.20	0.04	0.19
Other disabilities	0.13	0.33	0.12	0.33
Panel C. Work-related outcomes during first four years after DI allowance				
Average annual earnings ($):				
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
Beneficiaries	610,271		1,746,020	

Notes: The "upper bend point sample" includes DI beneficiaries within $1,500 of the upper bend point. These samples are the same as those considered in our regressions. See the text for sample restrictions. Each of the 610,271 beneficiaries in the sample is observed in years both before and after going on DI.

Source: Social Security Administration Disability Analysis File and Detailed Earnings Record from 2001 to 2007

smooth through the bend point. Similarly to Card et al. (2015) and Turner (2017), 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 3 and 12. For each dependent variable, we report β_2 for the polynomial order that minimizes

I. Initial density of AIME

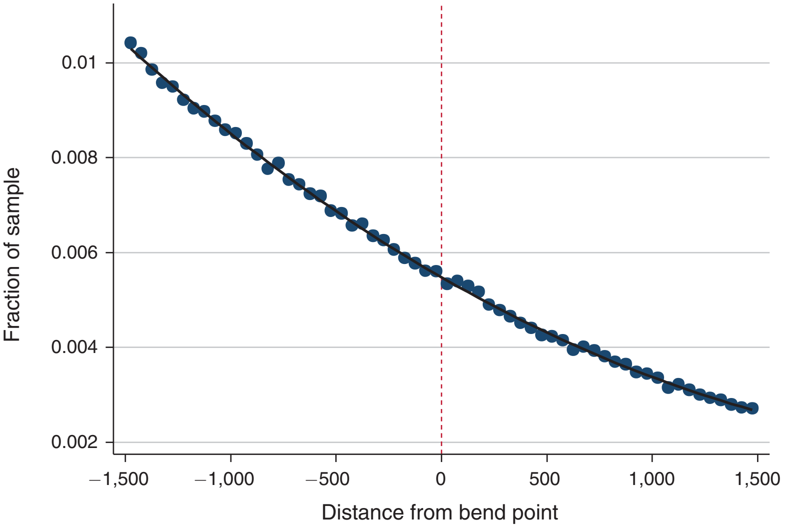

Figure 3. Smoothness of Density and Predetermined Covariates around the Upper Bend Point (*continued*)

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 in the sample. The best-fit line is a ninth-order polynomial that parallels the regression presented in Table 2 that minimizes the corrected Akaike Information Criterion (AICc).

Source: The data are from SSA administrative records.

the finite-sample corrected Akaike Information Criterion (AICc). Using a baseline specification without additional controls, none of the specifications show that β_2 is statistically different from zero at the 5 percent level. Moreover, these regressions are rarely statistically significant for any polynomial order. The test that the coefficients are jointly significant across outcomes in the AICc-minimizing specifications shows $p = 0.20$ at the upper bend point and $p = 0.35$ at the lower.

We show in the online Appendix that there is no evidence for "bunching" 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 (since earning an extra dollar that increases AIME leads to a greater increase in DI benefits below the bend point than above it).[19] Consistent with the exposition of the models in online 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.

[19] 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.

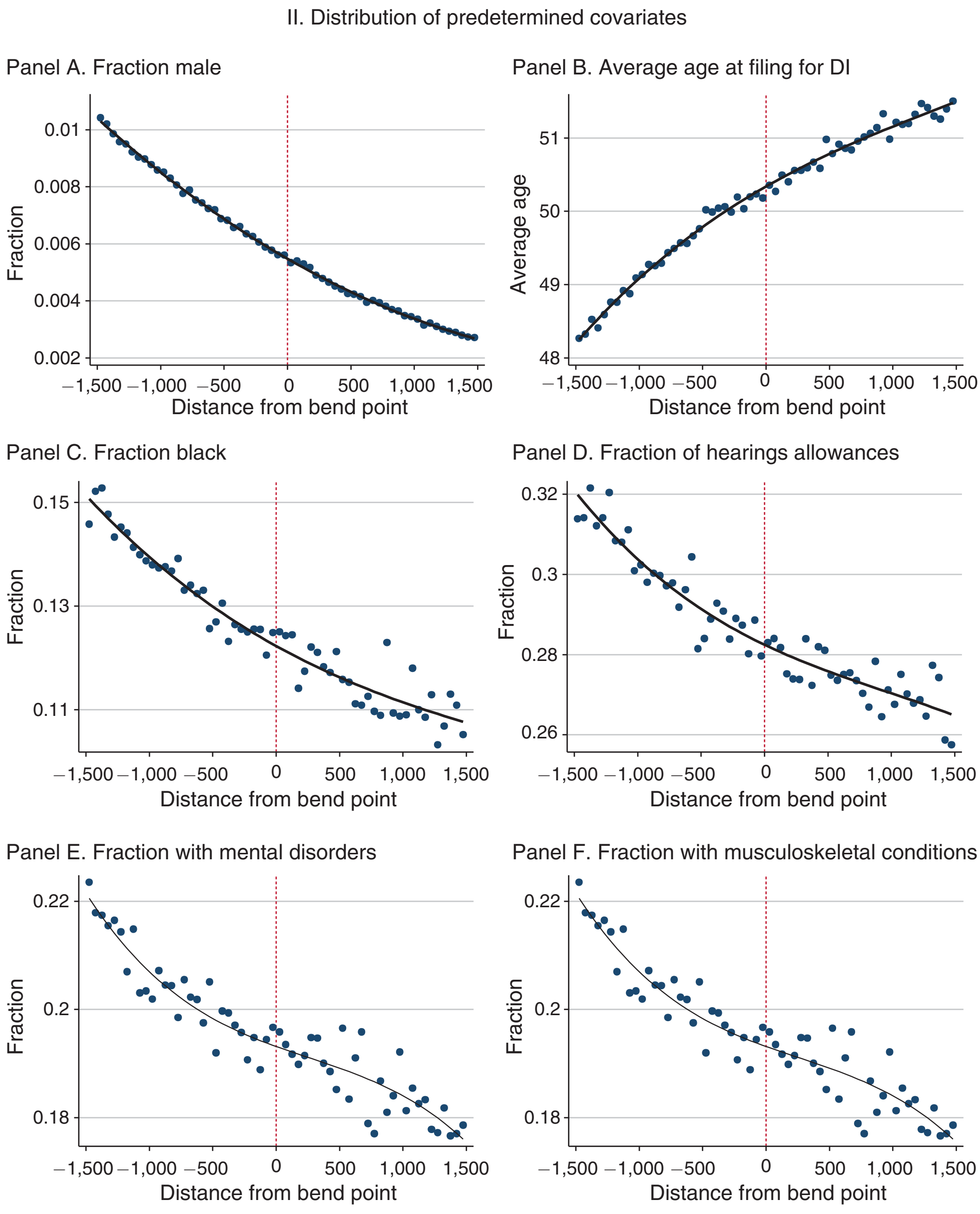

FIGURE 3. SMOOTHNESS OF DENSITY AND PREDETERMINED COVARIATES AROUND THE UPPER BEND POINT (*continued*)

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. The best-fit lines are cubic polynomials.

B. *Main Results*

Figure 4 shows average earnings in the four years after DI allowance around the upper bend point. Consistent with DI payments reducing earnings, the slope clearly increases at the upper bend point and the empirical observations lie close to the

TABLE 2—SMOOTHNESS OF THE DENSITIES AND PREDETERMINED COVARIATES

Dependent variable	Polynomial minimizing AICc (1)	Estimated kink (2)	Fraction of statistically significant kinks, polynomials of order 3–12 (3)
Number of observations	9	−0.76 (1.41)	0%
Fraction male (× 1,000)	12	−0.100 (0.097)	0%
Average age when filing for DI (× 1,000)	10	1.27 (1.11)	40%
Fraction black (× 1,000)	12	−0.064 (0.048)	10%
Fraction of hearings allowances (× 1,000)	12	−0.024 (0.087)	0%
Fraction with mental disorders (× 1,000)	12	−0.075 (0.056)	10%
Fraction with musculoskeletal conditions (× 1,000)	12	0.081 (0.086)	0%
Fraction SSI recipients (removed from main sample) (× 1,000)	12	−0.034 (0.059)	0%

Notes: The table shows that the density of the assignment variable (i.e., initial AIME) and distributions of predetermined covariates are smooth around the upper bend point. We test for a change in slope at the bend point using polynomials of order 3 to 12. For each dependent variable, the table shows: the polynomial order that minimizes the corrected Akaike Information Criterion (AICc) (column 1), the estimated change in slope at the bend point and standard error under the AICc-minimizing polynomial order (column 2), and the percent of estimates of the change in slope that are statistically significant at the 5 percent level (column 3). Before running the regression, we take bin means of variables in bins of $50 width around the bend point, so each regression has 60 observations. See other notes to Table 1.

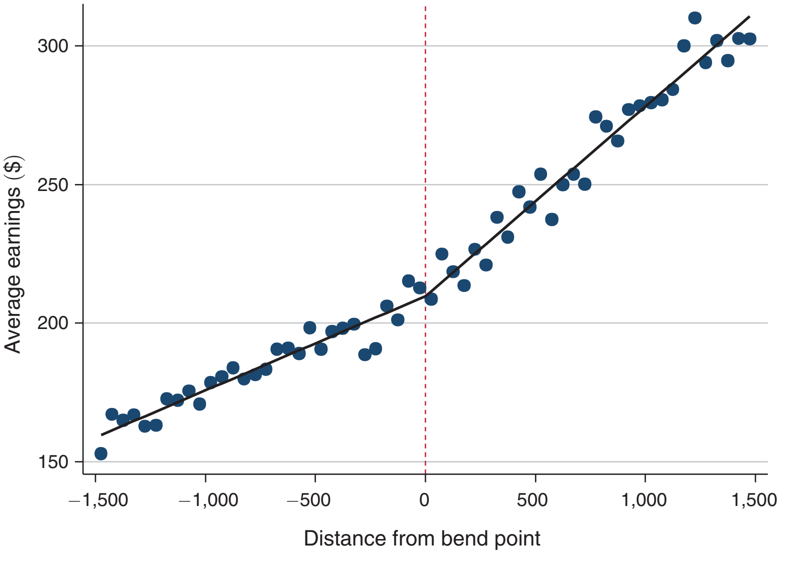

FIGURE 4. AVERAGE MONTHLY EARNINGS AFTER DI ALLOWANCE

Notes: The figure shows mean monthly earnings in the first four years after going on DI, in $50 bins, as a function of 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.

Table 3—Effect of DI Benefits on Earnings and the Annual Probability of Positive Earnings

	Linear models		Quadratic models		Cubic models	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable is mean annual earnings						
Cents per $1 more DI	**−20.28**	−19.25	−24.40	−26.87	−25.27	−27.38
	(2.24)	(3.83)	(8.25)	(8.55)	(8.73)	(9.18)
AICc	**377.56**	386.29	379.46	387.75	381.62	390.32
Panel B. Dependent variable is fraction with any annual earnings						
p.p. change per $1,000 more DI	−1.29	−0.93	−0.32	−0.43	**−0.37**	−0.47
	(0.12)	(0.20)	(0.39)	(0.38)	**(0.43)**	(0.42)
AICc	−496.41	−493.92	−499.89	−492.99	**−500.05**	−493.17
Covariates	No	Yes	No	Yes	No	Yes

Notes: The table contains coefficients and standard errors showing, at the upper bend point, the estimated effect of a $1 increase in yearly DI payments on mean yearly earnings (panel A) and on the fraction with positive annual earnings (panel B). In panel B, coefficients are scaled so that the coefficients reflect the effect of a $1,000 increase in DI benefit payments on the probability of having positive annual earnings. The estimates are based on the regression kink design model (2) in the text around the upper bend point. The full set of regression coefficients for panel A are reported in online Appendix Table A1; to arrive at the Table 3 estimates, we transform the Appendix Table A1 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 in each row. See other notes to Table 1.

fitted lines. Table 3 shows the estimated earnings effects when we implement the six regression specifications described earlier. We report the implied effect on earnings of increasing DI benefits by $1, under the sharp RKD assumption that the marginal replacement rate changes from 0.32 to 0.15 at the upper bend point. (Online Appendix Table A1 shows the actual regression estimates we use to generate the implied effects in Table 3.) In our baseline specification, increasing DI benefits by $1 leads to a substantial decrease in earnings of 20.28 cents at the upper bend point ($p < 0.01$).[20]

In Table 3, the estimates are similar when we control for predetermined covariates (column 2), and they are modestly larger under the quadratic and cubic specifications in columns 3 to 6. 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 studies surveyed in Ganong and Jäger (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). We use the linear specification without additional controls as our baseline because it minimizes the AICc (as well as the Bayesian Information Criterion). This choice is consistent with the argument of Gelman and Imbens (2014) that using higher order polynomials in regression discontinuity settings can lead to sensitivity

[20] 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, which would be appropriate in the dynamic life cycle model (without myopia or liquidity constraints) in online Appendix 1. 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. As a rough calculation, discounting benefits at a real rate of 3 percent over the 20.31 years of mean life expectancy for initial DI recipients (Zayatz 2011), 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.

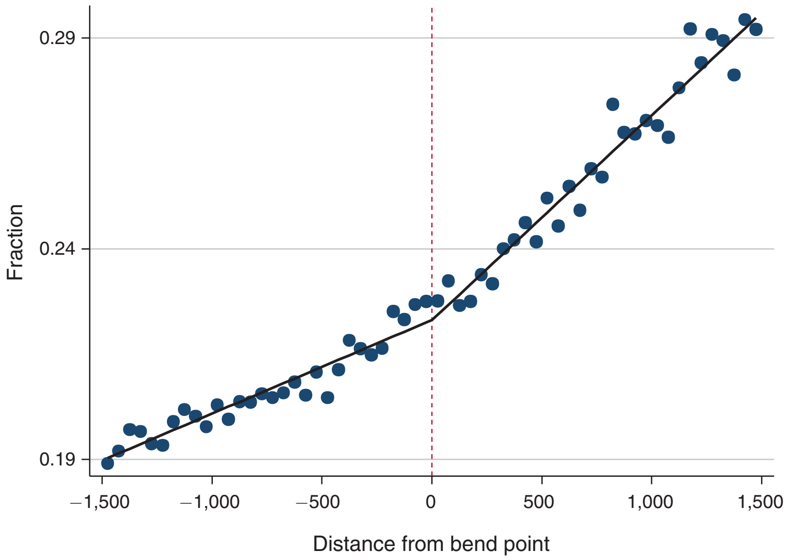

FIGURE 5. AVERAGE ANNUAL FRACTION EMPLOYED AFTER DI ALLOWANCE

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.

to specification and misleading confidence intervals (which if anything applies still more in RKD settings; see Ganong and Jäger forthcoming, though note there is ongoing econometric discussion of this issue in Card et al. 2014).

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 3 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 points 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 statistical significance.[21] In the online Appendix we also show similar patterns when the dependent variable is the probability of any employment over the full four years, rather than the percent of years with positive earnings (online Appendix Figure A6 and online Appendix Table A2). We conclude that there is some visual and statistical evidence of an employment effect at the upper bend point.[22]

[21] We obtain comparable results under specifications with the log odds of the employment rate as the dependent variable.

[22] 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.

In Figure 6 we show the average earnings around 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 earnings appear to be close to a linear function of AIME, with essentially identical slope on both sides of the bend point. Online Appendix Table A3 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 insignificant estimates. Strikingly, in each of the four years subsequent to receiving DI, there is a sharp increase in the 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.[23]

We consider whether the income effects differ by the year since DI allowance. Almost all labor supply studies assume time separability of utility, so that the labor supply decision in a given year is determined by the marginal utility of lifetime wealth and that year's marginal returns to extra work (Blundell and Macurdy 1999). In such a framework, the decisions in each year would in this sense be made separately. If there is a form of non-separability, such as the accumulation of human capital through past work experience, then the value of work in a given period will also include the effects of work on future marginal utility. Nonetheless, one might think that in this sample of DI-eligible individuals with at least ten years of work experience, such issues would be less important. If the period utility function changes across years, due to, for example, changing marginal costs of work stemming from changes in health, then we also might expect different income effects across years (Moore 2015).

Table 4 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 specifications. Figure 7 shows that the estimates move sharply from positive and insignificant in each year before going on DI, to negative and significant in each year after going on DI.

We show the main components of the analysis for the lower bend point in online Appendix Tables A4 through A6, and online Appendix Figures A1 through A4 and A7 through A9. The main results at the lower bend point show no significant effects on earnings in any of the specifications, and these effects are statistically significantly different from those at the upper bend point ($p < 0.01$). However, the a priori reasons above that we would not expect to find a meaningful change in slope at the lower bend point means it is difficult to rule out an income effect at 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.

[23] We show these graphs without drawn lines and with larger bins to show the variation in each year as clearly as possible. Online Appendix Figure A7 shows these results under the same formatting as our other graphs.

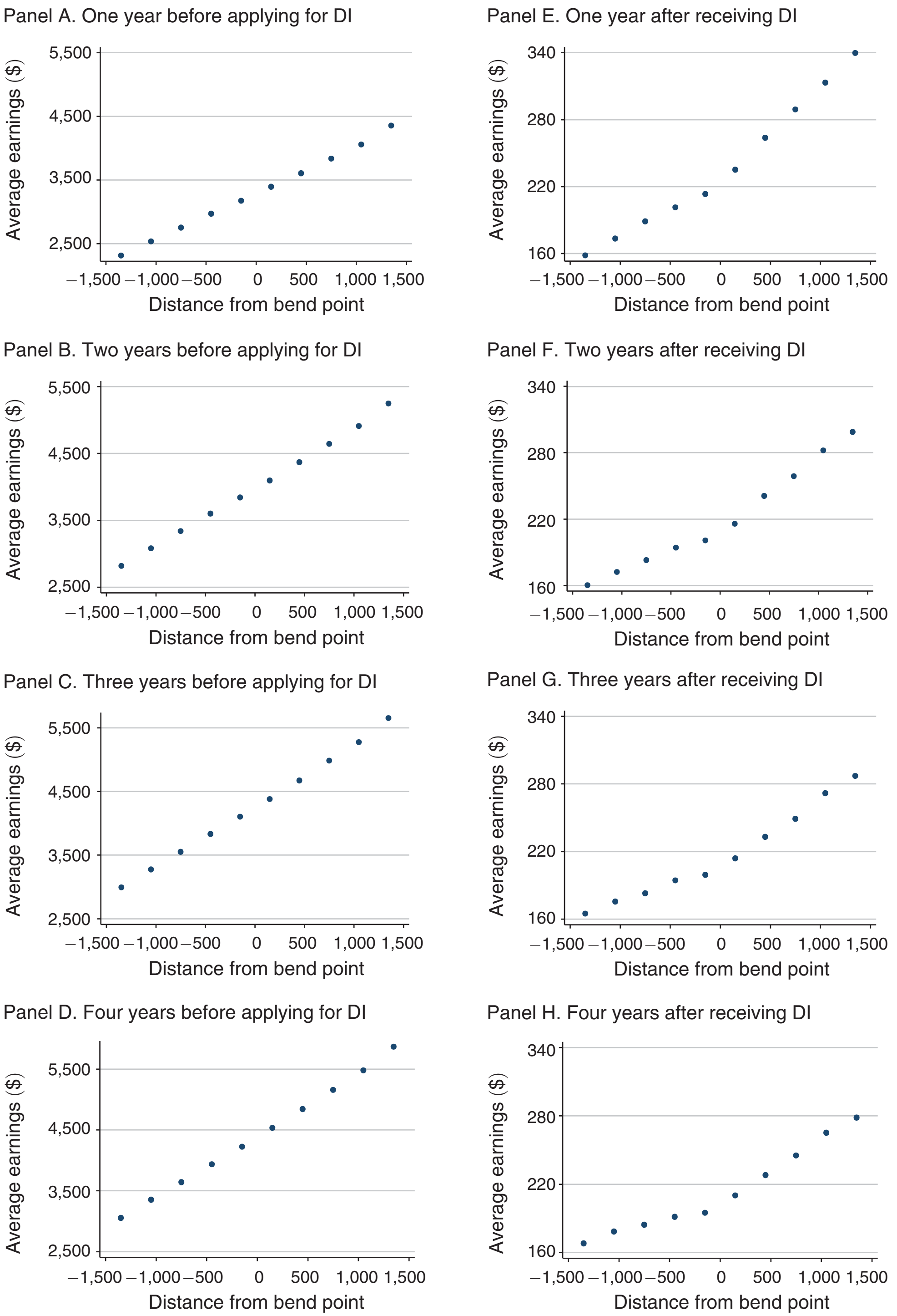

Figure 6. Average Monthly Earnings before and after DI Allowance

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). The panels show the average earnings for the sample in the years before and after receiving DI. Online Appendix Figure A7 shows the results with the same formatting as other graphs, i.e., $50 bins.

TABLE 4—INCOME EFFECT OF DI BENEFITS ON EARNINGS BY YEAR

	1st year on DI (1)	2nd year on DI (2)	3rd year on DI (3)	4th year on DI (4)
Cents per $1 more DI	−23.02 (2.32)	−19.63 (2.32)	−18.29 (2.62)	−20.20 (2.58)

Notes: The table contains coefficients and standard errors for the effect of a $1 increase in DI payments at the upper bend point on earnings in each year after starting DI. All of the results are based on the baseline linear model without controls, which minimizes the AICc in each case. See other notes to Table 3.

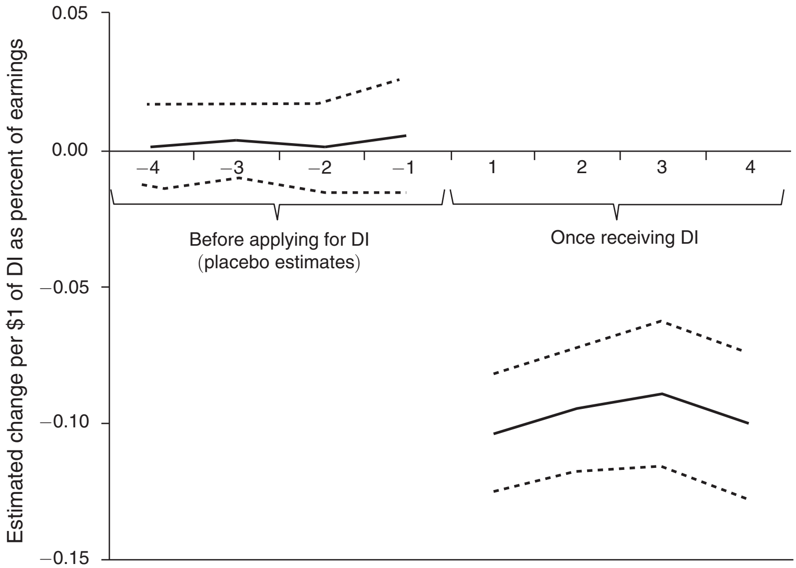

FIGURE 7. EARNINGS EFFECTS OF DI BENEFITS AS A PERCENT OF MEAN EARNINGS AT THE UPPER BEND POINT, BY SINGLE YEARS BEFORE AND AFTER RECEIVING DI

Notes: The figure displays the estimated effects of a $1 increase in DI payments on earnings as a percent of mean earnings at the upper bend point. The coefficients come from the AIC-minimizing regression, separately estimated for individual years from four years before applying for DI to four years after receiving DI. The bold lines show the coefficients, while the dashed lines show the 95 percent confidence intervals in each year. The earnings estimates are scaled by average earnings at the bend point because the mean of the dependent variable is much larger in the period before going on DI than once receiving DI. The figure shows that the estimates move sharply from statistically insignificant in each year before going on DI—when no effects are expected—to negative, relatively large, and statistically significant in each year after receiving DI.

Interpreting Results in Relation to Labor Supply Models.—We interpret our estimates as representing only an income effect. Substitution incentives created by the SGA limit could in principle interact with the income changes we are using, if the income changes would have pushed desired earnings (i.e., hypothetical earnings in the absence of the existence of the SGA limit) above SGA. However, an income effect, i.e., the marginal effect of DI income on earnings in our context, is equally well defined in our setting where there can be a notch in the budget set created by SGA, and in a more traditional setting of a linear budget set. Moreover, any such interactions are likely to be negligible, 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), monthly DI income is reduced by only \$127.50 due to the marginal replacement rate above the upper bend point being 0.32 rather than 0.15.[24] Nearly all DI recipients have low or no earnings, and only a small fraction are earning near the SGA limit, implying extremely limited scope for this change—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; in our sample, only 1.8 percent has completed a TWP. Even among those who have completed a TWP, for whom SGA is binding, we find that many beneficiaries locate *above* SGA (online Appendix Figure A10).[25]

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, where here we use "income effect" to refer to a traditional effect of additional unearned income on a linear budget set.[26] Equally importantly, 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), though the estimates are specific to this (policy-relevant) context.[27]

We find that beneficiaries' earnings respond to the transfers after they go on DI, but not before. In the life cycle model in online 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 life cycle 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 DI receipt is uncertain or they do not know about the magnitude of the income they will receive conditional on DI receipt), 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

[24] Here $-\$127.50 = \$750 \times -0.17$; the change in marginal replacement rate is -17 percentage points (= 32 minus 15).

[25] Online Appendix Figure A10 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.

[26] 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.

[27] Theory does not indicate whether the effects should be larger or smaller in the upper bend point sample than in the population as a whole. However, it is worth noting that if this group with relatively high prior earnings has relatively high earnings potential relative to most others on DI, then this group is *more* likely to be constrained by the SGA limit, leading to a more conservative lower bound.

beneficiaries normally should not expect an increase in future income and therefore typically should not want to borrow in a standard life cycle model, limiting the scope for liquidity effects.[28] 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.

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 48 ($= 32 \times 1.5$) to 22.5 ($= 15 \times 1.5$) 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 50 percent larger. Thus, our estimates of the effect of a dollar of benefits on earnings would be 16.48 percent ($=$ 32.95 percent $\times$ 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. If households are not unitary, as in for example a "collective" model of household bargaining (Chiappori 1992), then payments made to a beneficiary's dependents could have a smaller effect on the beneficiary than payments made to the beneficiary.

C. *Robustness Checks and Other Outcomes*

Several exercises further establish the robustness of the earnings estimates. Online Appendix Figure A11 shows that the estimates are significant and relatively stable over bandwidths between \$500 and \$2,000, including at the bandwidth of \$650 selected by the procedure of Calonico, Cattaneo, and Titiunik (2014a,b).[29] Online Appendix Figure A12 shows that the absolute value of the coefficient is maximized at the location of the actual bend point 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. A formal "permutation test" in the spirit of Ganong and Jäger (2014) shows that the estimate with the kink placed at the actual bend point is statistically significantly larger in magnitude than the distribution of

[28] 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.

[29] This is based on their 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. 2015; Calonico, Cattaneo, and Titiunik 2014a).

placebo estimates.[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 specifications.

We consider self-employment earnings in online Appendix Tables A7 to A10. We find similar earnings and employment effects when we use total earnings (i.e., W-2 wage plus self-employment earnings) instead of wage earnings alone. When we only use self-employment earnings, we find that DI payments reduce self-employment earnings slightly but significantly, but there is no statistically significant effect on the probability of having self-employment earnings (as we might expect since only 1.92 percent of the disabled are self-employed).

We conduct further robustness exercises in terms of our sample and regression specification. Online Appendix Table A11 shows that the results are similar when changing the sample in various ways: first, including SSI recipients in the sample; second, removing beneficiaries with dependents, which is relevant in light of the family labor supply issues discussed above; and third, including decedents' earnings as zeroes after they have died.[31] This last result is not surprising given that Gelber, Moore, and Strand (2017) find no robust evidence of an effect on mortality at the upper bend point. We also show in online Appendix Table A11 that the results are similar under the fuzzy RKD described in online Appendix 3. This is because the first stage estimate is very 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). Online Appendix Table A12 shows that the results are similar when using \$25-wide or \$100-wide bins, when using the individual-year level data (rather than collapsing to the bin level), or when we include beneficiaries whose AIME changes more than four times.[32]

Beneficiaries could also change their program participation in response to the change in marginal replacement rates at the upper bend point. However, online Appendix Figure A14 and online Appendix Table 13 show no significant change in slope at the bend point of 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.

Finally, online Appendix Figure A15 and online Appendix Table A14 show no evidence of bunching in the density around the upper bend point four years after going on DI, much like online Appendix Figure A7 shows no evidence of bunching in initial AIME.

[30] When using placebo kinks farther from the bend point, or estimating the bandwidth based on the Calonico, Cattaneo, and Titiunik (2014b) procedure separately at each placebo kink, we also estimate $p < 0.05$. Additionally, following Landais (2015), in online Appendix Figure A13, we show the R^2 of the baseline model when the kink is placed at "placebo" locations. The R^2 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).

[31] After scaling the baseline crowdout estimate from online Appendix Table A11 to account for the probability of mortality over the four years, the crowdout point estimate is 20.6 cents, nearly identical to our baseline estimate of 20.3 cents in Table 3.

[32] Note that using individual-year level data also helps illuminate how the effects vary within individuals over time. Earnings are serially correlated: over the four years after DI allowance, the correlation of earnings from year t to year $t + 1$ is 0.82, and the correlation of a dummy for positive earnings from year t to year $t + 1$ is 0.78.

D. *Effect Heterogeneity*

Table 5 shows the earnings effects at the upper bend point across subgroups using our baseline linear specification. The effect is larger for women than for men,[33] for those under 45 than for those over 45, and for those allowed DI eligibility by their initial DI examiner than for those allowed by an ALJ via a hearing after an initial denial.[34] The estimates are similar for black and nonblack beneficiaries. Across beneficiaries' primary disabilities, the effects are largest for those diagnosed with circulatory conditions, followed by mental disorders, neurological conditions, injuries, "other" disabilities, respiratory conditions, and musculoskeletal conditions. The estimate for those with cancer is only barely above zero and insignificant. At the extensive margin, the point estimates also generally follow similar patterns across groups (throughout the nine specifications). Over the four years following DI allowance, among those who had zero earnings in year t, we find much smaller earnings crowdout in year $t + 1$—only 2.4 cents (standard error 0.75 cents) from a \$1 increase in DI payments—than in the full sample.

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 the 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, is modestly larger.

V. Comparison to Other Literature

As a benchmark to assess the relative importance 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 at Disability Determination Services (DDS) and FS use random assignment to DI ALJs to examine the overall effects of DI cash and medical benefit receipt on earnings and employment. Thus, the effects they estimate encompass both income and substitution effects of DI, whereas our estimates only relate to income effects. We compare our results to MMS and FS

[33] Note that the separate point estimates for men and women are larger than the estimate for the full population, although the weighted average of the sex-specific estimates lies within the 95 percent confidence interval of the full-population estimate. In general, in a least squares model, the weighted average of the estimates for subgroups need not be the same as the estimate for the full population.

[34] For those with above-median earnings in the four years prior to going on DI, we find crowdout in the baseline model of 51.09 cents from a \$1 increase in DI payments, compared to 15.84 cents for those with below-median earnings. However, mean reversion could affect the results by prior earnings, if this leads to differential nonlinear patterns around the upper bend point in each prior earnings group that is correlated with the RKD variation.

Table 5—Heterogeneity in the Income Effects

Category	Subgroup	Cents per $1 more DI (1)	Mean earnings at bend point (2)	p-value on equality of coefficients within group (3)
All		−20.28	2,516	–
		(2.24)		
Sex	Males	−22.86	2,012	<0.01
		(2.41)		
	Females	−35.47	3,755	
		(4.53)		
Age at filing for DI	Age $<$ 45 years	−32.21	4,392	<0.01
		(6.45)		
	Age $\geq$ 45 years	−18.75	2,079	
		(2.26)		
Race	Nonblack	−20.25	2,401	0.55
		(2.39)		
	Black	−17.59	2,533	
		(6.64)		
Type of allowance	Initial DDS allowance	−22.79	2,826	0.08
		(2.76)		
	Hearings allowance	−15.40	1,723	
		(3.40)		
When entered DI	Started in 2001–2002	−20.94	2,674	0.08
		(4.77)		
	Started in 2003–2004	−12.53	2,637	
		(4.38)		
	Started in 2005–2007	−24.64	2,372	
		(3.35)		
Primary disability	Mental disorders	−26.61	2,415	0.06
		(5.68)		
	Musculoskeletal cond.	−14.02	1,826	
		(3.47)		
	Circulatory conditions	−28.44	1,905	
		(5.09)		
	Neurological conditions	−25.33	2,933	
		(7.20)		
	Injuries	−23.12	3,593	
		(14.20)		
	Respiratory conditions	−16.48	1,784	
		(9.14)		
	Cancers	−0.39	7,211	
		(25.33)		
	All other disabilities	−20.08	3,273	
		(7.66)		

Notes: See notes to Tables 1 and 3. Mean earnings in column 2 are measured by the constant in the regression. Column 3 shows the p-value from a test of the hypothesis that the coefficients are equal within a category.

in order to understand whether income effects are an important component of the overall impact of DI benefit receipt on earnings and employment. Such a comparison is valid because they examine the US DI program in a similar time period—in the 2000s and in the 1990s and 2000s, respectively. We use data on both DI beneficiaries allowed by a DI examiner, and DI beneficiaries allowed by an ALJ, and we use similar administrative data to what MMS and FS use, albeit from a slightly different period 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 $0.18 and $0.19 per dollar of DI benefits, respectively. These crowdout estimates are close to—and insignificantly different from—our baseline estimate of $0.20, suggesting that the income effect we estimate encompasses essentially all of the earnings crowdout they find. MMS and FS find in baseline specifications that DI receipt reduces the probability of employment by 28 and 26 percentage points after two and three years, respectively. Using the fact that on average DI beneficiaries annually receive combined cash and medical benefits worth an average of $20,950 ($13,750 in cash benefits plus $7,200 in medical benefits), this implies that a $1,000 increase in the value of DI benefits reduces the probability of employment by 1.22 or 1.11 percentage points in MMS and FS, respectively. These are close to our extensive margin income effect estimates in the linear specification of -1.29 percentage points per $1,000 of additional DI benefits in the linear specification. However, they are around three times larger than the point estimates from our quadratic and cubic specifications, making it possible that income effects encompass a smaller (but likely still substantial) portion of the extensive margin effect. That said, our extensive margin crowdout estimate in our AIC-minimizing cubic specification, 0.37 percentage points per $1,000 of DI benefits, is closer to those MMS find three to four years after the disability decision, an average of 0.83 percentage points per $1,000 of DI benefits. Thus, our main conclusion here is that our estimates of *earnings* crowdout encompass a large fraction of the *earnings* crowdout in these studies.

Moreover, when investigating populations more comparable to MMS and FS separately, we continue to find similar results. In Table 5, we find income effects of $0.23 per dollar of DI benefits among those allowed by a DDS (most comparable to the MMS population), and $0.15 among those allowed by an ALJ (most comparable to FS).

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, $0.27 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 $0.23 per dollar of DI benefits. Third, if we exclude the average value of Medicare benefits ($7,200 per year) in the value of DI—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 $0.27 and $0.30 per dollar of DI benefits, respectively.

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 fifty-ninth to ninety-fifth percentile of earnings. If anything, this suggests—though does not imply—that crowdout could be similar or larger in other parts of the distribution of prior earnings.[35]

Although the comparison to MMS and FS therefore suggests that income effects are important across a range of assumptions, this comparison is not dispositive. MMS and FS exploit variation that affects whether or not an individual receives DI cash and medical benefits, whereas our variation marginally affects DI cash payments. Comparing the marginal effect of additional benefits to the average effect of a discrete change in benefits will be most valid if the marginal and average effects are similar. Moreover, our sample around the upper bend point is not exactly the same as the population in MMS or FS. Note, however, that we estimate large income effects in every demographic group except the small group diagnosed with cancer, implying that re-weighting our sample to mimic the MMS or FS demographic composition could not change the finding that our estimated income effect accounts for a large fraction of theirs. Furthermore, the main characteristic that diverges in our sample from the MMS and FS samples is the fraction male (which is, e.g., 50 percent of the sample in FS), and when we re-weight to the fraction in their samples, we find moderately larger point estimates than our baseline. When comparing to other literature cited in the introduction to this paper, which typically investigates contexts less similar to ours, again our estimated income effect encompasses an important portion of the overall earnings and employment effects.

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 $0.05 to $0.10 on the dollar (e.g., Imbens, Rubin, and Sacerdote 2001; Cesarini et al. 2015), but smaller than some estimates in the context of retirement pensions (e.g., Costa 1995). In non-DI disability contexts, Autor et al. (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 (2016). 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.

[35] 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, such as those based on type of disability, our estimates tend to be larger in subgroups where MMS found larger effects. French and Song (2014) examine groups based on prior income closer to DI receipt but do not break down their estimates by AIME.

VI. 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 of approximately \$0.20 at 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 absolute magnitude of the point estimate is larger (as much as 27 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 pretax benefits that we measure. In previous literature, the impact of DI on earnings has often been interpreted as reflecting moral hazard, but our results clearly demonstrate that this earnings crowdout does not only reflect moral hazard. In fact, our crowdout estimates are similar to those in previous literature encompassing both income and substitution effects, suggesting that much of the impact of DI on earnings could relate to income effects, rather than moral hazard.

Chetty (2006) points out that in a standard labor supply model, as risk aversion grows, income effects on labor supply become larger relative to substitution effects (given an estimate of the complementarity of leisure and consumption). Thus, if income effects are important relative to substitution effects in our context, this could be rationalized through relatively high risk aversion. In turn, this could suggest that DI benefits are relatively valuable to this population by insuring against the risk of disability. 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 important, whereas small substitution effects limit the impact of Ticket to Work. 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 \$0.20 on the dollar more broadly, this would have implications for both 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 3 percent for someone who had been on the program for ten 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 of 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 \$0.05; the Old Age, Survivors, and Disability Insurance Trust Fund alone would gain 2.48 cents in revenue, while the DI Trust Fund would gain 0.36 cents.[36] If chain-weighting decreases annual benefits by \$638, this would

[36] \$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.

lead to an annual increase in federal government revenues of around $32, an increase in OASDI revenues of around $16, and an increase in DI revenues of over $2. Such fiscal consequences are relevant, especially as there are around nine million DI primary beneficiaries and policymakers are considering steps to improve the financial outlook of the DI Trust Fund, which is projected to be exhausted in 2023 (Office of the Chief Actuary 2016).

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. If income effects are important, then the deadweight loss from DI may be substantially less than previously thought.

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