

Does Student Loan Forgiveness Drive Disability Application?

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Student loan debt in the US exceeds \$1.3 trillion, and unlike credit card and medical debt, typically cannot be discharged through bankruptcy. Moreover, this debt has been increasing: the share of borrowers leaving school with more than \$50,000 of federal student debt increased from 2 percent in 1992 to 17 percent in 2014. However, federal student loan debt discharge is available for disabled individuals through the Department of Education's Total and Permanent Disability Discharge (TPDD) mechanism through certification of a total and permanent disability. In July 2013, the TPDD expanded to include receipt of Social Security Disability Insurance (SSDI) or Supplemental Security Income (SSI) as an eligible category for discharge, provided medical improvement was not expected. Using data from the Survey of Income and Program Participation (SIPP) matched to SSI and SSDI applications, we find that SSDI and SSI application rates increased among respondents with student loans relative to rates among those without student loans. Our estimates suggest the policy change raised the probability of applying for SSDI or SSI in a given quarter among student loan-holders by 50% (baseline rate per quarter is approximately 0.3%), generally increasing SSI and SSDI awards. However, these induced award recipients were unlikely to receive the disability designation necessary to obtain student loan discharge. Given that the geographic distributions of student loan indebtedness and historical SSDI/SSI program participation differ, there are strong implications for both the size and location of SSDI and SSI beneficiaries. Furthermore, these findings highlight the importance of learning from policy changes in programs that interact with SSDI and SSI to better understand the drivers of disability program participation.

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Introduction

Rising costs of postsecondary education coupled with increasing labor market returns to postsecondary education have jointly created a rapid increase in the number of students financing some or all of a postsecondary education with student loans over the past two decades (Lochner and Monge-Naranjo, 2016). Simultaneously, finding and keeping a job (with or without a college education) has become a much less certain prospect (Lochner and Shin, 2014; Hoynes et al., 2012). Thus, the traditional way of getting rid of one's student loans—paying them off—has become more challenging, and some loan-averse students have sought out alternative means of financing their education (Marcus 2016). Student loan debt is increasing particularly quickly among older Americans; between 2005 and 2015, the amount of federal student loan debt held by borrowers age 50 to 64 increased from \$43 billion to \$183 billion (GAO 2016).

Student loan debt has been linked to delays in marriage (Bozick and Estacion 2014), declining homeownership (Houle and Berger 2015), reduced entrepreneurship (Ambrose et al. 2015), and a decrease in work-life balance satisfaction and reduced childbearing (Velez et al. 2018). Student loans are generally not dischargeable in bankruptcy.

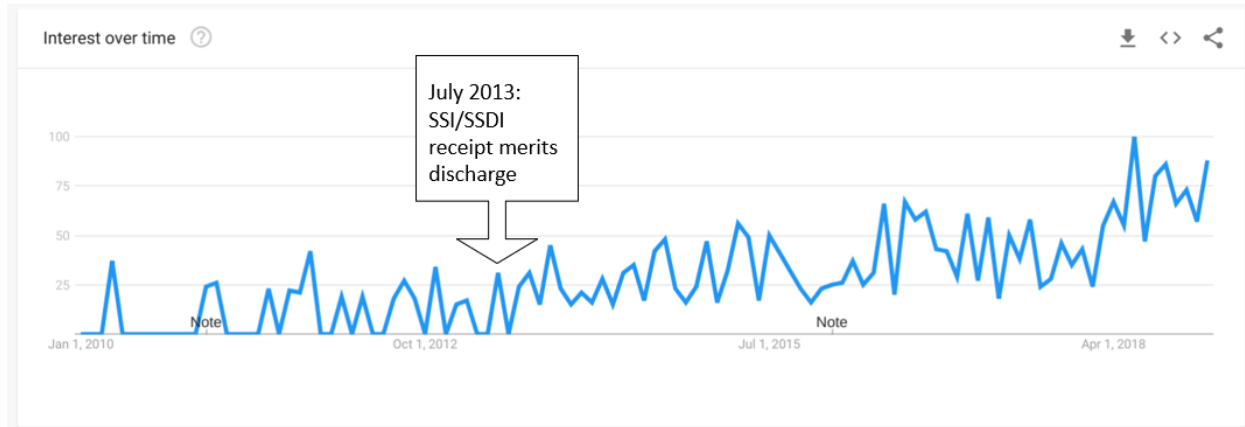
Certain fields like teaching, medicine, and public service offer loan forgiveness programs. However, for those in other career fields or otherwise ineligible for these programs, an earnings shock can strain one's ability to repay student loans. The onset of a work-limiting disability is an example of such an earnings shock, and the Department of Education recognizes the difficulty of repaying student loans on a fixed income by offering student loan discharge for disabled individuals.

Disability discharge of student loans

Prior to 2013, the Department of Education's Total and Permanent Disability Discharge (TPDD) allowed for two classes of student loan borrowers to have their federal student loans discharged: veterans with a 100% service-connected disability rating or an individual unemployability rating, or an individual who has a licensed physician certify an inability to engage in substantial gainful activity due to a physical or mental impairment lasting at least 60 months or that will result in death. However, in July of 2013, the TPDD mechanism expanded to include individuals who were receiving SSDI or SSI benefits, although this expansion was limited to SSDI or SSI beneficiaries with a "medical improvement not expected" determination, a subset of all beneficiaries, as discussed below in the SSDI and SSI section.

In theory, the prior allowance for a physician's certification is the same criterion as that used by SSA in determining a disability of at least 60 months (an inability to perform substantial gainful activity) and does not require the individual satisfy the SSI asset test or be covered by SSDI, but the ability to have the SSA determination process directly satisfy the documentation requirements of TPDD substantially reduces the TPDD-related application costs, as well as increases the benefit of SSDI or SSI participation. Furthermore, this expansion to the TPDD criteria was widely publicized, increasing the saliency of TPDD as a pathway to student loan discharge, as evidenced in the frequency of searches for "tpd discharge" reported by Google Trends.

Figure 1: Google Searches for “tpd discharge” from 2010-2018



Note: Y-axis shows an index of frequency of Google searches over this time period, with the lowest value in this time period indexed to 0, and the highest value indexed to 100. The frequency of searches roughly doubled after the policy change (comparing 2012 and 2013), but Google does not make the base number of searches available. The data have not been smoothed or filtered.

Searches for “tpd discharge” were at their lowest prior to the July 2013 expansion; after this expansion there was a sustained increase in the frequency of searches relative to before. This increase does not appear to have subsided. Although not conclusive evidence, the timing of this first uptick in awareness and information seeking as to TPDD appears to have aligned with the expansion of TPDD to include SSDI or SSI beneficiaries.

Social Security Disability Insurance and Supplemental Security Income

SSDI and SSI are the largest federal disability programs not limited to veterans or federal employees and are both administered by the Social Security Administration (SSA), with the same definition of disability: a health condition preventing earning at the Substantial Gainful Activity (SGA) level that is expected to last at least 12 months or result in death, where SGA is defined as earning at least \$1,260 per month and is indexed to the national wage level. In addition to this earnings limitation, both programs have non-medical requirements. Applicants for SSDI must have a recent work history covered by Social Security: at the time of their disability applicants must have earned 40 quarters of coverage¹ overall, 20 of which had to have been earned in the past 10 years, although younger workers have lower requirements. Applicants without this work history are denied on technical grounds. Applicants for SSI cannot have deemed resources over \$2,000 per person, or \$3,000 per couple, with certain exceptions, such as the value of a home or car. Individuals applying to either program, or both concurrently, go through the disability determination process administered by their states' Disability Determination Services (DDS).

This determination process involves a substantial amount of time and documentation, requiring applicants to satisfy the non-medical requirements, as well providing medical and vocational information to allow for determination by an examiner. Although SSDI-only applicants can begin this process online, applicants for SSI or concurrent SSDI-SSI applicants must file their claims either over the phone or in-person at a Social Security field office.

¹ Workers earn a quarter of coverage for every \$1,410 of Social-Security-covered earnings, up to four per year. This amount is indexed to wage growth.

Deshpande and Li (2019) find that the proximity and congestion of a local field office have a substantial impact on the likelihood of applicant.

The up-to-five-step determination process is described below, with the first step conducted by the field office and steps two through four conducted by the DDS examiner:

- 1) Applicants with earnings at or above the SGA level are denied.
- 2) Applicants with impairments deemed not severe or of short duration are denied.
- 3) Applicants with impairments that meet or equal a condition in the Listing of Impairments are allowed on medical grounds.
- 4) Applicants who do not satisfy stage (3) and have a capacity for past work are denied.
- 5) Applicants who do not satisfy stage (3), but do not have capacity for past work or for any work in the national economy are allowed; otherwise they are denied.

The median processing time for an initial application was 3.4 months. However, only a third of initial applications are allowed, requiring the remaining applicants to either accept this denial or appeal this denial, either to another examiner through reconsideration when required (with a median processing time of 8 months), or directly to an Administrative Law Judge (ALJ). Approximately half of those initially denied (that is, a third of the overall initial applicant pool) eventually appeal to an ALJ, where the median processing time is 28.8 months, with, ultimately, only just over 40 percent of applicants being accepted (Autor et al. 2015). This application process can thus take a substantial period of time for a typical applicant, during which the applicant cannot engage in SGA and is more likely than not to be denied benefits.

However, less frequently discussed in the literature of SSDI or SSI application is the *category* of determination upon award. Applicants whose applications are successful can fall into one of three categories: “medical improvement expected” with a Continuing Disability Review (CDR) scheduled 6 to 18 months later to determine if the individual has medically improved; “medical improvement possible” with a CDR scheduled for 3 years later; or “medical improvement not expected” with a CDR scheduled for 5 to 7 years later. It is only this latter category of determination that leads to eligibility for TPDD, and in SSA’s Disability Analysis File, only 25.1% of SSDI/SSI beneficiaries have a “medical improvement not expected” determination.²

Although SSDI and SSI have the same disability determination process, they differ in the non-disability components of eligibility: SSDI is part of the Old Age, Survivors, and Disability Insurance system, often generally referred to as “Social Security,” and is thus social insurance. Applicants must have a sufficient work history to be eligible for, and thus covered by, SSDI, and their potential monthly benefit is based on their history of Social Security covered earnings. SSI, on the other hand, is a means-tested program, with a maximum federal benefit that is reduced by receipt of other income.

A frequent topic of research in disability research is explaining the substantial temporal and geographic variation in participation in these programs. Decades of per-capita SSDI program growth, referred to as a “fiscal crisis unfolding” (Autor and Duggan 2006), continuing through the Great Recession (Liebman 2015), have given way to declining applications and awards in recent years (SSA, 2018). These changes have not been confined to mere changes in the overall size of the program: the composition of disabling conditions has shifted toward mental and

² See https://www.ssa.gov/disabilityresearch/documents/daf_puf/DAFPublicUseFileDocumentation.pdf
Page 97.

musculoskeletal conditions, as well as toward female beneficiaries amidst increased female labor force participation through the end of the 20th century (Duggan and Imberman 2009).

Another facet of heterogeneity is the wide variation in disability program participation across counties within the US, from fewer than 1 percent to over 20 percent of county population. The literature exploring the explanations for this geographic variation point to differences in underlying disabling health conditions, strength of the local economy, and participation rates among those with disabling condition as the drivers for these differences, not differences in program administration (Gettens et al., 2018).

Indeed, one of the earliest well-identified studies of drivers of local SSDI participation showed that closures of local coal mines led to corresponding upticks in local disability program participation rates (Black et al., 2002). Studies such as this one began an ongoing literature in defining “conditional applicants”: individuals who may satisfy the medical definition of disability but will apply only in the presence of a non-health factor. This literature seeks to identify the non-health shocks that can induce application in this population, thereby revealing the conditions and types of individuals under which such individuals’ disability program application is contingent. Benefit replacement rates for low-skill workers (Autor and Duggan 2003), availability of health insurance (Maestas et al. 2014), and declining value of Social Security retirement benefits (Duggan et al. 2007) have all been linked to greater likelihood of application to SSDI or SSI. However, recent evidence on the impact of Medicaid expansion of disability program participation has shown zero or only a small impact (Anand et al. 2019, Schmidt et al. 2019), indicating that the type of health coverage and the affected population are vital for understanding the population of conditional applicants.

A more recent line of research concerned with conditional applicants has focused not on these factors external to SSDI or SSI, but on transaction or information costs associated with the programs themselves as a driver in application prevalence. These studies have shown that closures of local Social Security offices (Deshpande and Li 2019), expansion of online application options (Foote et al. 2019), and merely informing individuals that they are covered by SSDI (Armour 2018) all can account for differences in SSDI application rates on the order of double-digit percentage differences.

This transaction cost-related research on SSDI and SSI join a larger recent literature showing the importance of information costs and barriers to public program participation, from the EITC (Bhargava and Manoli 2015) to SNAP (Daponte et al. 1999) to postsecondary enrollment (Hoxby and Turner 2015; Barr and Turner 2018; Dynarski et al. 2018). In contrast to the theoretical basis for imposing ordeals to increase targeting efficiency – wherein those less likely to participate in the presence of transaction costs represent those least in need of the program at hand – these studies often show that the transaction-cost-marginal applicant is as likely to be accepted onto SSDI or SSI, if not more likely, than the average applicant. Indeed, increasing evidence points to frictions in optimization *increasing* among the neediest potential beneficiaries (Finkelstein and Notowidigdo 2019), suggesting that “ordeals” mechanisms may not be an appropriate second-best tool in these contexts.

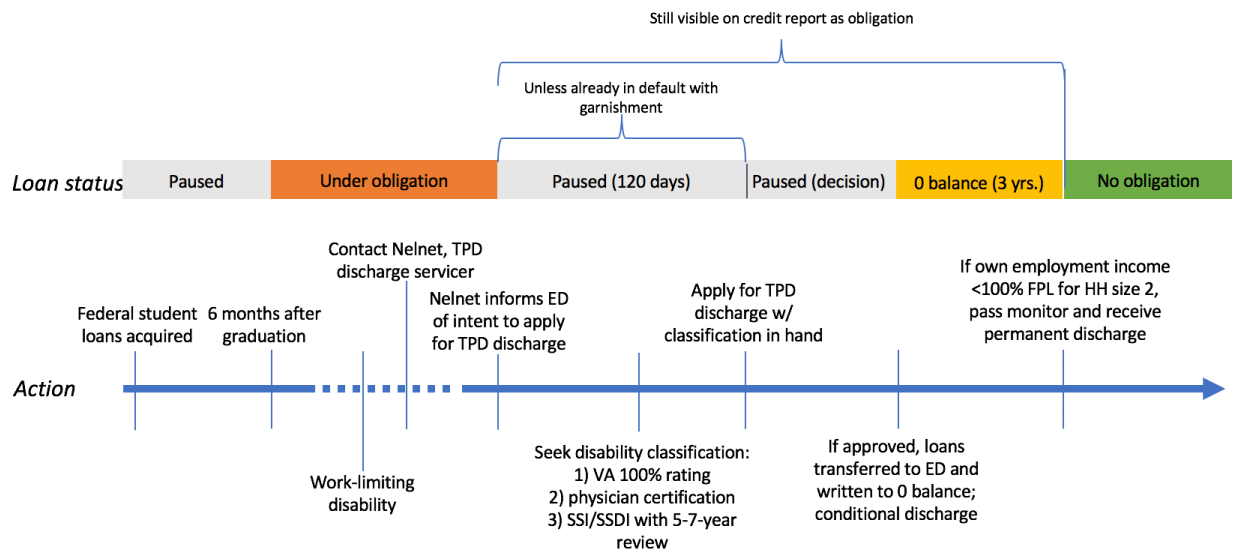
This study contributes to the aforementioned research base in three ways: first, it provides evidence of a new margin for disability application based not on health or income, but debt. Although it is not the first to do so, other work in this area is still in-progress, with preliminary indications that bankruptcy, foreclosure, eviction, and home sale peak around the time of disability application (Deshpande et al. 2019). Second, the role of student debt in driving new SSDI and SSI applications has strong implications for the geographic distribution of disability

program participation, given that the geographic distribution in student loan indebtedness, and hence the marginal applicants identified in this study, differs considerably from the pre-existing geographic distribution of SSDI and SSI program participation. And third, the nature of the TPDD policy change and the resulting magnitude of behavioral response in increased SSDI/SSI application speaks to a prior lack of awareness or existing transaction costs in the TPDD program, especially given the costly and restrictive SSDI/SSI route to TPDD.

Obtaining a student loan discharge through the TPDD program

As shown in Figure 2, the TPD discharge is not an instant process. Any amount of time after experiencing a work-limiting disability, a student loan borrower can contact Nelnet, the Department of Education’s (ED) servicer for the disability discharge program. If the borrower expresses interest in applying for TPD discharge, Nelnet notifies the ED of his or her intent to apply, and student loan payments are paused for 120 days unless the borrower is already in default. The borrower then can choose a method of demonstrating disability. After the 2013 policy change, one of these methods is receiving SSI or SSDI with a 5-7 year review period. A borrower could newly apply for SSI or SSDI, or already be receiving SSI or SSDI. With that certification in hand, the borrower applies for TPD discharge through Nelnet. At this point, payments on his or her federal student loans are paused indefinitely while a decision is made (again, assuming the loans are not already in default). If the application is approved, the student loans are transferred to the ED and the balance is zeroed out for a three-year monitoring period. Note that the student loans are still visible on the credit report while paused and throughout the monitoring period as an obligation.

Figure 2: TPD Discharge Application Process and Timeline



During the monitoring period, the borrower is subject to income verification. If his or her own employment income (non-wage and salary income is excluded, as is other household income) remains below 100 percent of the federal poverty line for a household of two adults for the entirety of the three-year period, the monitoring period ends and he or she receives a permanent discharge, with no remaining obligation on federal loans. The discharge of student loans was considered taxable income until January 2018.

Data

The chief requirement of any analysis of the impact of the SSDI/SSI TPD discharge expansion on SSDI/SSI application is a data source containing both measures of SSDI/SSI application and variation in “treatment,” be it pre-policy-change vs. post-policy change, existing federal student loan indebtedness, or, preferably, both. The first choice for such a data source would be individually matched administrative records containing both Social Security records with timing of SSDI/SSI application and Department of Education or credit bureau data on federal student loan indebtedness; however, such a match is beyond the scope of this analysis.

Instead, our primary analysis draws on survey data – the 2014 Panel of the Survey of Income and Program Participation (SIPP) and its accompanying self-reported student loan indebtedness variables, as well as its rich array of sociodemographic variables – matched to Social Security Form 831 files, which contain a record for every application to SSDI, SSI, or both for each individual in the 2014 SIPP that gives permission for such a match for research purposes.

SSI/SSDI application data

Our measures of SSDI and SSI application and award, as well as characteristics of the applicants and awardees, come from the same source: SSA Form 831 files. Each state’s Disability Determination Service (DDS) renders the initial medical determination for individuals applying for disability benefits under Title II (SSDI) and Title XVI (SSI); the Form 831 file contains data from these DDS decisions.

A Form 831 record is established once the DDS makes its decision. Subsequent decisions or corrections to earlier decisions result in additional records with the same identifiers but with new values and dates. The key identifying fields, Social Security Number (SSN) and Beneficiary Identification Code (BIC), identify the individual and Filing Date (FLD) records the filing date of an application. Yearly Form 831 files are available back to 1988 and have uniform record layouts beginning with 1989. A person may have several 831 records in the same and/or different yearly files, and multiple Form 831 records are present if a person filed concurrent Title II and Title XVI claims, if an initial claim was denied and was then appealed, or if more than one application was filed.

Of particular relevance for this analysis are which program the applicant applied to, recorded in the RID variable, and whether the applicant applied concurrently to both, recorded in the CCF variable. The result of their determination – whether they were determined to be disabled – is recorded in the RDT variable, whether the disability is deemed permanent (and hence eligible for TPD discharge) is recorded in the DPM variable, and the body system corresponding to the primary diagnosis code is recorded in the BS variable, from which we construct three types of awardees: musculoskeletal, mental conditions, and other.

We discuss below how we construct application and award outcomes from these Form 831 files differently for our two analyses.

SIPP student loan data

The Census Bureau has administered a version of the SIPP since 1984, eliciting information on income from multiple sources, public program participation, work characteristics, and family and household structure, among other individual- and household-level characteristics. The SIPP is a longitudinal survey: once a new SIPP panel begins, households are re-interviewed every wave, running from 2.5 to 4 years. The SIPP sample universe is the civilian, noninstitutionalized

population of the United States, with the 2014 SIPP sample is a multistage stratified sample of 53,070 housing units based on addresses from, chiefly, the 2010 Decennial Census.

The SIPP underwent a major re-engineering starting with the 2014 panel: in contrast to prior panels, in which respondents were interviewed every four months with references to work, income, and program participation in each month since the last interview, the 2014 SIPP conducts annual interviews, with references to the twelve preceding months. Therefore the first wave of the 2014 SIPP refers to the 2013 calendar year. To address data quality issues related to recall bias over a longer reference period, the 2014 SIPP includes an Event History Calendar (EHC) to enhance respondent recall. According to the 2014 SIPP Panel Users' Guide: "The EHC helps respondents recall information in a more natural 'autobiographical' manner by using life events as triggers to recall other economic events. For example, a residence change may often occur contemporaneously with an employment change. The entire process of compiling the calendar focuses, by its nature, on consistency and sequential order of events, and attempts to correct for otherwise missing data. The EHC was developed through a series of annual field tests conducted between 2010 and 2013" (Census Bureau 2016).

During SIPP interviews, respondents can consent to administrative data linkage using personal information; consenting respondents who match to a Social Security record are given a Protected Identity Key (PIK) that links individuals' SIPP data with administrative records. Of the 72,065 respondents in universe for SSA disability programs in wave 1 of the 2014 SIPP, 64,721 (90 percent) of the cases had had a successful match between SIPP data and SSA administrative records (Giefer et al. 2015), with the remaining 10 percent of the cases unmatched due to either a lack of respondent consent or an inadequacy of SIPP personal information in facilitating a match.

Unlike the 2008 SIPP panel, the 2014 SIPP includes questions specifically about debt from student loans and educational expenses. For each reference month, respondents are asked about a range of debt, but our primary analysis focuses on individuals who answer in the affirmative to "Owed any money for student loans or educational-related expenses during the reference period." The earliest reference period available is January 2013, therefore preceding the 2013 TPDD change.

Finally, we use only an indicator for any student loan debt to further isolate ourselves from any such bias: TPDD results in discharge of federal student loan debt. Provided individuals have other forms of student loan debt not discharged via TPDD, or at least not as quickly, such individuals would still be included in our treatment group.³

Descriptive statistics

Using the 90 percent of SIPP respondents with matched SSA records, we construct an analytic file at the quarter by person level, following a discrete hazard approach of individuals "at risk" of first SSDI or SSI application (Allison 1984, Armour 2018). That is, starting in quarter one of 2010, we include an observation for each individual for each quarter in which he or she has not applied for SSDI or SSI, or applied for SSDI or SSI for the first time in that quarter, with no additional observations after their first application. We follow individuals through the end of 2016, with SSDI or SSI application observed from the Form 831 records. We then match individual-level characteristics from the first reference month from wave 1 of the 2014 SIPP (again, referencing calendar year 2013). Although there are potentially multiple Form 831 per SIPP respondent, our measures of application correspond to the earliest such application. We

³ Furthermore, research by Akers and Chingos (2014) suggests that more than a third of students with federal student loan debt are unaware they have federal debt, suggesting that the distinction may be unclear to many, again leading to conservative bias.

also construct our award based on the filing date corresponding to their awarded application record.

We conduct analyses over two analytic samples: one of all matched SIPP respondents, and a second based on those covered by SSDI in the previous year, calculated based on the Summary Earnings File from the prior year meeting the recent work and total quarter requirements. Summary statistics for these two samples, as well as the subgroups with and without student debt, are reported in Table 1. Student debt holders tend to be younger and more educated, are more likely to have credit card debt, and have an average total student debt level of over \$23,000.

Empirical strategy

We follow a discrete-hazard approach, not a duration model, to estimating the effect of the TPDD expansion on SSDI/SSI application (Allison 1984). A traditional duration model would require a well-defined and observable “beginning” of the duration, or in this context, the onset of a potentially disabling health condition. In the context of disability policy studies, not only is disability onset difficult to observe in most surveys, the relevant onset – when a condition first manifested, when it first began to interfere with participation opportunities, when it first began to interfere with work, when other health conditions or life circumstances exacerbated any interference, or employment separation – depends vitally on the context at hand. Instead, the discrete-hazard approach allows us to define at a point in time the population “at risk” of SSDI or SSI, and, accounting for a range of individual and economic characteristics, estimate the effect of a policy change in the presence of well-defined treatment and control groups. Using our Form 831 files, we construct a longitudinal file wherein an individual contributes an observation in a quarter if he or she is “at risk” in that quarter: if they are age 18 to 65 and have not previously applied SSDI or SSI, and, in the case of our SSDI application model, had sufficient Quarters of Coverage to be covered by SSDI by the end of the year prior to the current quarter. Our sample window stretches from Quarter 1 of 2010 to Quarter 4 of 2016. Given the timing of the treatment during Quarter 3 of 2013, we exclude this quarter from the file. In the 699,478 quarter-year observations “at-risk” of SSDI application in this window, we observe 2,194 SSDI or SSDI/SSI-concurrent first-time applications. In the 1,071,957 quarter-year observations “at-risk” of either SSDI or SSI application, we observe 3,243 first-time applications to one of these disability programs.

We merge on to this longitudinal file our Wave 1 2014 SIPP file which contains rich sociodemographic variables as well as student loan indebtedness, used to define our treatment and control groups. For the reasons indicated above – TPDD does not necessarily apply to non-federal student debt and the timing of the student debt question – we define our treatment group as those reporting *any* student loan debt in their own name for January of 2013, and who are treated beginning after Quarter 3 of 2013. Our control group are those reporting no student loan debt for January of 2013.⁴

Our individual-level analysis then takes the form of a classic difference-in-differences specification:

$$Application_{iq} = \alpha + \delta(q > 2013Q3) + \rho Studebt_i + \beta(q > 2013Q3)Studebt_i + \Gamma X_{iq} + \gamma_q + \varepsilon_{iq} \quad (1)$$

⁴ We conduct similar analyses with student loan debt in *any* month of 2014 as our treatment, and in *no* month of 2014 as our control, with no significant differences in results.

Where β represents our treatment effect – the estimated coefficient on application for those with student loan debt in quarters *after* the TPDD policy change. We control for a range of individual-level characteristics, represented in the X matrix: age, age-squared, sex, race/ethnicity (non-Hispanic white, non-Hispanic black, non-Hispanic other race, and Hispanic), marital status, and educational attainment.⁵ The collection of coefficients γ correspond to quarter fixed effects. The specification above represents a linear specification, although our primary specification is a logit. We cluster our standard errors at the individual-level but have also estimated linear models with two-way clusters at the individual and quarter level, with no change in significance of our findings; we also present results based on unweighted and weighted logit specifications, although our preferred causal estimate is based on the unweighted logit with individual-level clustered standard errors (Solon et al. 2015). For robustness, we estimate our analysis both with OLS and WLS specifications, with the latter weighting based on the 2014 SIPP Wave 1 person-level weights, with qualitatively and quantitatively similar results. We conduct analyses with two samples: all “at-risk” individuals, where the outcome measured is SSDI, SSI, or SSDI/SSI-concurrent applications; and a subset of individuals covered by SSDI in the prior year, where the outcome measured is SSDI or SSDI/SSI-concurrent applications, since this outcome is only possible for those covered by SSDI.⁶ Since we report odds ratios, in the presence of a detectable policy effect, we expect restricting the sample to those covered by SSDI to increase the proportional increase in the likelihood of an application observed, since we do not observe technical denials in our data.

We do not focus on SSI-only applicants, since the wealth measures in the SIPP are not detailed enough for a determination of whether an individual would satisfy the resources limit in SSI.

The underlying assumption behind this estimation approach is that, conditional on observable covariates, relative rates of disability program application among those with and without student debt in early 2013 would be unchanged over our sample window, if not for the July 2013 change in TPDD eligibility mechanism. This persistent difference in disability program participation rates between those with and without student debt is accounted for by our estimate of ρ ; furthermore, we account for population-wide changes in rates of disability program application with δ , both as a general “post-July-2013” control, and in additional specifications, as quarter-specific fixed effects.

The primary threat to this assumption is the presence of differences in disability program participation by student loan indebtedness, *over the time period in question, independent of the TPDD discharge change*. In our analyses below, we attempt to address this issue by conducting placebo tests, such as estimating TPDD “effects” for those with other forms of debt who should be unaffected by the TPDD change, as well as by estimating pre-TPDD time-trend differences by student-loan indebtedness to determine whether relative disability program participation rates appear to be changing over time.

Additionally, we examine the extent to which different types of individuals are induced to apply to SSDI or SSI by the TPDD change. For example, the welfare implications of new applicants differ if these applicants were previously high earners who opted to leave employment

⁵ Note that due to our discrete-hazard empirical approach, whereby we define our sample as those “at-risk” of *first-time* SSDI or SSI application, our outcome variable is binary and an absorbing state, and hence we are unable to include individual-level fixed effects.

⁶ Technical denials, including SSDI denials because the applicant is not covered by SSDI, are not observed in our matched Form 831 files.

to enter long-term disability programs or if they were previously not working and are transitioning from non-employment to SSDI or SSI. We also examine which types of disabling health conditions appear most responsive, the differential effects of which have implications for the composition of the SSDI and SSI rolls. Finally, we estimate the likelihood of not just an SSDI or SSI award, but an SSDI or SSI award for which “medical improvement is not expected,” the requirement for TPDD eligibility.

Results

Table 2 shows the treatment odds ratio⁷ estimates, indications of level of significance, and 95 percent confidence intervals from different versions of Equation 1 over the full sample, with SSDI, SSI, or SSDI/SSI-concurrent application all resulting in a left-hand side value of one. Moving from column 1 to 4 represents a movement to a more saturated model: Column 1 contains just a constant, dummy for any student debt, dummy for post-2013 Q3, and the interaction term estimating our treatment effect. Our logit odds ratio estimate is highly statistically significant, corresponding to a treatment effect of a 48% increase in the odds of disability program application in each quarter. Adding in demographic controls, quarter fixed effects instead of just a post-2013 Q3 dummy, and clustering standard errors at the individual level results in a similarly highly statistically significant treatment effect of a 46% increase in the odds of disability program application in each quarter. The final column limits the sample to those with some college or greater educational attainment; this treatment effect is correspondingly higher, consistent with student debt load disproportionately held by those with educational attainment in these categories.

Table 3 next limits the analysis to the sample covered by SSDI with the outcome being application to SSDI. The magnitude of the estimates declines slightly, with a new treatment effect for the most saturated model – column 4 – of a 36% increase in application odds; again, this estimate is higher for the subsample with some college or higher educational attainment. For robustness, Appendix Tables 1 and 2 provide parallel estimates as those in Table 1, although employing OLS or WLS specifications. The findings, in both statistical significance and point estimate, quite similar to the logit estimates from Table 2. However, those covered by SSDI, by definition, have a more recent attachment to the labor force or higher earnings than those not covered by SSDI. Although these estimates are not statistically significantly different than those from Table 2, this sample difference is one explanation for why the point estimates are lower.

Table 4 next provides estimates of the time path of application between those with student debt and those without to determine whether there were prior trends, as well as the pathway of the treatment effect after the policy change. Due to the large number of quarters and limited number of applications, we aggregate our analytic sample to the year level, excluding the partially treated year 2013. We include an indicator variable for student debt, and an interaction with student debt for each year, omitting 2012 as the latest pre-treatment interaction. We estimate both logit and OLS models across three samples: the entire at-risk SSDI/SSI population, those covered by SSDI in the prior year, and those covered by SSDI in the prior year *who also*

⁷ Technically, exponentiated log odds from interaction terms in logit models, such as the difference-in-differences treatment term reported in this analysis, are not odds ratios, but ratios of odds ratios. For simplicity, we refer to them as odds ratios in this paper, but we also note that in the current context, the rate of “success,” or application to SSDI or SSI, stands at only 0.3 percent, and thus the denominator of any odds (1-Pr(application)) is very close to one. Thus, even in the presence of a tripling of the application rate, this ratio of odds ratios and odds ratios themselves approximate a risk ratio to the hundredth decimal place.

had zero earnings in the prior year, as measured in the administrative SSA Summary Earnings Record.

All these specifications show that there is no statistically significant pre-trend, whereas those with student debt are substantially more likely to apply for SSDI or SSI starting in 2014. Depending on the sample, this effect persists, or even increases, through 2015, but, with the exception of a single OLS model, is no longer statistically significant by 2016.

We note that the confidence interval for the estimates for 2016 contain not just a null effect of one, but also the estimated odds ratios for 2014 and 2015. But these estimates are consistent with the theory that this policy change resulted in an influx of new applicants due to “pent-up” demand for student loan debt relief, with a corresponding decline in applications after this stock of applicants apply, akin to the difference between immediate and long-run differences in divorce rates after divorce policy changes (Wolfers 2006). However, given that we define our treatment group based on 2013 student loan indebtedness, these noisier estimates may also be indicative of attenuation bias, which we cannot directly address in this analysis.

Up to this point, we have focused on either any disability program application, or SSDI (and SSDI/SSI-concurrent) application among those covered by SSDI. Table 5 expands this analysis to examine impacts on application to these programs separately, as well as the impact on award rates, and composition of awards.

Columns 1 and 2 show that the 46% odds ratio estimate from column 4 of Table 2, and the 36% odds ratio estimate from column 4 of Table 3 are being driven by SSDI-only applications. That is, there is no evidence of an increase in SSDI/SSI-concurrent application, whereas the estimated increase for SSDI-only is a 60% odds ratio.

Although we find strong application impacts, the implications for SSDI and for individuals differ dramatically if these applications are overwhelmingly accepted or overwhelmingly denied. If the latter, these individuals were induced to apply and went through the lengthy application process with no success, and in the process increasing the workload of the DDSs. If the former, these are indeed individuals who qualify for SSDI. We find that the former story tends to dominate: the increase in SSDI awards (106% odds ratio increase) exceeds that of the application effect (60%), increasing the targeting efficiency of the program (Deshpande and Li 2019). That is, it appears that these conditional applicants – those with student debt seeking a TPDD through the new SSDI/SSI discharge route – are disproportionately likely to be awarded disability benefits. In other words, the increase in the potential value of disability program participation – discharge of federal student loans – increased the targeting efficiency of disability programs.

But were these induced applicants likely to qualify for TPDD? Column 7 and 8 look at whether these awards resulted in a permanent disability classification (satisfying the “medical improvement not expected” categorization), which is the requirement for TPDD, or a non-permanent classification, which would carry with it SSDI or SSI benefits, but not convey automatic TPDD eligibility to the awardee through the SSA pathway. Although there is a positive point estimate on the impact on permanent disability awards, this estimate is not statistically significant; the *non-permanent* disability classification appears to be driving the estimated treatment effect of an increase in awards. That is, the policy inducement to apply for SSDI/SSI – discharge of federal student loans for those accepted onto these programs *and determined to be permanently disabled* – induced more application to SSDI, led to improved SSDI targeting, but did not result in statistically significantly increased permanent disability determinations. However, these applicants and awarded SSDI/SSI beneficiaries, now aware of

TPDD, may seek out the physician route to discharge their federal student loans. Unfortunately, the data used in these analyses cannot identify the extent to which these SSDI/SSI applicants go on to have their debt discharged through this alternative route.

Table 6 explores further heterogeneity in awards and applications; namely, whether the treatment effect varies by body system of the primary diagnosis code of awardees, and whether the application effect varies by recent work. We separate out body system of awards into three categories: musculoskeletal, mental disorder, and other body system. Although we estimate positive coefficients for all three conditions, the estimate for mental disorders is not statistically significant; the musculoskeletal estimate is positive, with an estimated 56% increase in the odds of application; however, the effect is even higher for other body systems, estimated at a 72% increase in the odds of application. It is difficult to disentangle differences in application increases from screening, since musculoskeletal and mental conditions have been described as less easily verifiable (Duggan and Imberman 2009), and hence these increases in awards may be the result of a screening out of these less verifiable conditions. However, these estimates imply the presence of conditional applicants among “more verifiable” conditions.

Column 4 presents results from a specification including both the treatment variable and an interaction term with whether the individual had any positive earnings in the previous year (as well as an unshown variable with whether the un-interacted variable of any earnings in the previous year). The treatment estimate rises considerably, although the estimate on the interaction term among those with earnings in the prior year nearly completely offsets this effect. That is, the overall treatment effect appears to be driven by those who are already not earning at all.⁸ Consistent with the finding that these conditional applicants were more likely to be awarded benefits than the average applicant, Table 6 indicates that these conditional applicants do not appear to be coming from those currently working or those with “less verifiable” conditions.

Finally, in our individual-level analysis, Table 7 shows heterogeneous treatment estimates by age group, across the overall sample and the SSDI-covered sample, and across logit and OLS models. Our general finding is that the results are consistently statistically significant for the 50-59 population; this finding is consistent with the “aching to retire” narrative of Duggan et al. 2007, in which small to moderate changes in Social Security retirement benefits can induce application among older Americans. The estimates for the 60-65 population are generally larger, although with this smaller population, many of these higher point estimates are not statistically significant. However, multiple specifications also find an effect among the 30-39 population, suggesting that student debt discharge, and seeking relief thereof, is a potentially strong motivation for those earlier in their working-age life, resulting in application to these long-term disability programs, from which return-to-work is rare.

Robustness and falsification tests

The Table 4 estimates showed that our estimated effect did not pre-date the policy change, indicating that our analysis passes this “lead” placebo test. Table 8 provides another approach to a placebo test: if we conduct our analysis not with student debt interacted with a post-July 2013 policy change, but with another type of debt holding, do we still see an effect? Since there were not corresponding SSDI/SSI policy changes for these other kinds of debt in this time period, we should see no significant effect. One issue that arises though is that there is correlation between

⁸ See Appendix Table PC for estimates by application/award type for the subsample with no earnings in the prior year.

holding different types of debt. In this table, we show results from both standalone estimates, and while including these other debt types.

We find that whenever student debt is included, we estimate the same, statistically indistinguishable coefficient. When we include credit card debt post-2013 as our treatment on its own, we find a statistically significant estimate, but this estimate becomes insignificant when we include our student debt treatment variable. Including “All Other” debt is never statistically significant, indicating that our estimated effect is due to the TPDD policy change, and not other debt-related trends in SSDI/SSI application.

In an ideal world, we would be able to demonstrate a corresponding increase in TPD discharges coming from the SSDI/SSI qualification route. However, the ED can only identify TPD discharge participants for 2014-present; furthermore, our analyses indicate that most of these induced SSDI/SSI applicants were *not* designated as “medical improvement not expected,” the designation required for TPDD discharge, and thus would have to seek the physician TPDD route. What we can learn from the limited data available, shown in Table 9, is that when the ED started sending letters to *existing* SSDI/SSI recipients who held federal student loans and a “medical improvement not expected” designation notifying them about the program and their eligibility in 2016, there was a large increase in TPD discharge approvals. Thus, the SSDI/SSI-receiving population does seem to be sensitive along information and cost-reduction margins.

Conceptual framework

These empirical results present a potential puzzle. To understand the factors driving them, let us consider the marginal applicant—the disabled individual who would not apply for SSDI/SSI without this TPDD pathway, but who would once he or she could potentially receive both benefits. Although there are potentially many current SSDI/SSI beneficiaries who were induced to apply for TPDD after the expansion, especially after the later information outreach program conducted by ED, our empirical approach focuses on induced to newly apply for SSDI/SSI. While there is limited data available on TPDD utilization, specifically on the fraction of applicants that are successful for each TPDD type, the median amount of discharged debt among successful TPD applicants in FY2015 was \$17,500 and the average was about \$25,000 (GAO 2016). However, the automatic notification of SSI/SSDI recipients with federal student loan debt took place in the spring of that fiscal year, so this number may differ greatly in our study period. Finally, while we abstract from timing concerns for the purposes of this model, note that using the physician or SSA pathway to apply for TPDD requires first that the prospective applicant obtain a certification of disability, either from a physician, or from the “medical improvement not expected” SSDI/SSI designation, which requires a physician visit for the application process. The prospective applicant can then complete the TPDD application paperwork.

The goal of this model is to attempt to rationalize the observed behavior: a lack of both TPDD and SSDI/SSI prior to the 2013 TPDD expansion, followed by increased disability program application by those with student debt after this expansion.

We begin with a single-period utility maximization framework, where utility is a function of resources, plus any program awards, minus the costs of application. In the pre-policy change period, disabled individuals can choose to apply for SSI or SSDI, TPDD (with physician certification of disability), both, or neither. Let W refer to an individual’s resources, DI be the value of SSDI or SSI benefits if awarded, and $TPDD$ the value of discharged debt if TPDD is successful. Denote the non-physician costs associated with SSDI/SSI application as c_{DI} , denote

the non-physician costs associated with TPDD application as c_{TPDD} , and the cost of visiting a physician to certify disability, for either SSDI/SSI or TPDD, as c_{Phys} .

However, SSDI/SSI award or TPDD discharge are not certain, and if pursued separately, these probabilities are given by:

\widehat{p}_{DI} : Expected probability of acceptance onto DI
 \widehat{p}_{Phys} : Expected probability of TPDD via physician certification

Since the marginal applicant under study – one who applies for SSDI/SSI only after the TPDD expansion – did not apply for SSDI/SSI before the expansion, we can infer that:

$$\widehat{p}_{DI}U(W + DI - c_{DI} - c_{Phys}) + (1 - \widehat{p}_{DI})U(W - c_{DI} - c_{Phys}) < U(W) \quad (2)$$

Or the expected utility from applying for SSDI/SSI, incurring any associated costs with application, is below utility from not applying. We can also infer that the expected utility from TPDD application via physician certification was below their utility from not applying, since it was the TPDD expansion that induced SSDI/SSI application:

$$\widehat{p}_{Phys}U(W + TPDD - c_{TPDD} - c_{Phys}) + (1 - \widehat{p}_{Phys})U(W - c_{TPDD} - c_{Phys}) < U(W) \quad (3)$$

Finally, before the TPDD expansion, our marginal applicants chose not to apply for both SSDI/SSI and TPDD physician certification, to which we assign three probabilities based on potential outcomes:

$\widehat{p}_{DI, Phys}$: Probability of acceptance onto DI and TPDD via physician certification
 $\widehat{p}_{DI, !Phys}$: Probability of acceptance onto DI, without TPDD via physician certification
 $\widehat{p}_{!DI, Phys}$: Probability of DI denial and TPDD via physician certification

We assign separate probabilities to allow for potential correlation between SSDI/SSI and TPDD outcomes, instead of assuming independence. We assume that an individual can use a single physician visit to satisfy both SSDI/SSI and TPDD application, but since the marginal applicant did not apply for both, we can infer that:

$$\begin{aligned} & \widehat{p}_{DI, Phys}U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ + & \widehat{p}_{DI, !Phys}U(W + DI - c_{TPDD} - c_{Phys} - c_{DI}) + \widehat{p}_{!DI, Phys}U(W + TPDD - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + (1 - \widehat{p}_{DI, Phys} - \widehat{p}_{DI, !Phys} - \widehat{p}_{!DI, Phys})U(W - c_{TPDD} - c_{Phys} - c_{DI}) < U(W) \end{aligned} \quad (4)$$

However, after the TPDD expansion, we observe an increase in SSDI/SSI applications among those with student debt, implying that the marginal applicant had expected utility from pursuing the SSDI/SSI route to TPDD higher than the outside of option of no application. We assume the following probabilities associated with application:

$\widehat{p}_{DI, MINE}$: Probability of acceptance onto DI, with Medical Improvement Not Expected

$p_{DI, \widehat{!MINE}}$: Probability of acceptance onto DI, without Medical Improvement Not Expected

We have just two independent probabilities, instead of three in the pre-expansion period, since an individual cannot satisfy the medical improvement not expected requirement if they are not awarded benefits. The choice to apply after the TPDD expansion therefore implies:

$$\begin{aligned} & p_{DI, \widehat{MINE}} U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + p_{DI, \widehat{!MINE}} U(W + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + (1 - p_{DI, \widehat{MINE}} - p_{DI, \widehat{!MINE}}) U(W - c_{TPDD} - c_{Phys} - c_{DI}) \geq U(W) \end{aligned} \quad (5)$$

Note here that because marginal applicants revealed themselves to not pursue TPDD via the physician certification route in the pre-period, under constant or decreasing absolute risk aversion, they will not apply for physician certification in the event of a lack of success pursuing the SSDI/SSI route.

We can combine this inequality with the previous one to arrive at:

$$\begin{aligned} & p_{DI, \widehat{MINE}} U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + p_{DI, \widehat{!MINE}} U(W + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + (1 - p_{DI, \widehat{MINE}} - p_{DI, \widehat{!MINE}}) U(W - c_{TPDD} - c_{Phys} - c_{DI}) \\ & > p_{DI, \widehat{Phys}} U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + p_{DI, \widehat{!Phys}} U(W + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + p_{!DI, \widehat{Phys}} U(W + TPDD - c_{TPDD} - c_{Phys} - c_{DI}) + \\ & (1 - p_{DI, \widehat{Phys}} - p_{DI, \widehat{!Phys}} - p_{!DI, \widehat{Phys}}) U(W - c_{TPDD} - c_{Phys} - c_{DI}) \end{aligned} \quad (6)$$

Before rearranging and simplifying, note that there were no SSDI/SSI policy changes in this period, and thus:

$$p_{DI, \widehat{MINE}} + p_{DI, \widehat{!MINE}} = p_{DI, \widehat{Phys}} + p_{DI, \widehat{!Phys}} \quad (7)$$

That is, the likelihood of any type of DI award is the same, regardless of medical improvement determination or the outcome of the physician certification. The above inequality therefore simplifies to:

$$\begin{aligned} & (p_{DI, \widehat{MINE}} - p_{DI, \widehat{Phys}}) U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\ & + (p_{DI, \widehat{!MINE}} - p_{DI, \widehat{!Phys}}) U(W + DI - c_{TPDD} - c_{Phys} - c_{DI}) > \\ & p_{!DI, \widehat{Phys}} \left(U(W + TPDD - c_{TPDD} - c_{Phys} - c_{DI}) - U(W - c_{TPDD} - c_{Phys} - c_{DI}) \right) \end{aligned} \quad (8)$$

The values of each term therefore imply:

$$\begin{aligned}
& (p_{DI, \widehat{MINE}} - p_{DI, \widehat{Phys}})U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) + \\
& (p_{DI, \widehat{!MINE}} - p_{DI, \widehat{!Phys}})U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\
> & (p_{DI, \widehat{MINE}} - p_{DI, \widehat{Phys}})U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) + \\
& (p_{DI, \widehat{!MINE}} - p_{DI, \widehat{!Phys}})U(W + DI - c_{TPDD} - c_{Phys} - c_{DI}) \\
& > p_{DI, \widehat{Phys}}(U(W + TPDD - c_{TPDD} - c_{Phys} - c_{DI}) \\
& \quad - U(W - c_{TPDD} - c_{Phys} - c_{DI})) \geq 0 \tag{9}
\end{aligned}$$

Simplifying, we have:

$$\begin{aligned}
& (p_{DI, \widehat{MINE}} + p_{DI, \widehat{!MINE}} - p_{DI, \widehat{Phys}} - p_{DI, \widehat{!Phys}})U(W + TPDD + DI - c_{TPDD} - c_{Phys} - c_{DI}) > \\
& p_{DI, \widehat{Phys}}(U(W + TPDD - c_{TPDD} - c_{Phys} - c_{DI}) - U(W - c_{TPDD} - c_{Phys} - c_{DI})) \geq 0 \tag{10}
\end{aligned}$$

Recalling equation (7), the first multiplicative component becomes 0, yielding a contradiction:

$$0 > p_{DI, \widehat{Phys}}(U(W + TPDD - c_{TPDD} - c_{Phys} - c_{DI}) - U(W - c_{TPDD} - c_{Phys} - c_{DI})) \geq 0$$

The observed lack of SSDI/SSI or TPDD in the pre-expansion period, and the increased SSDI/SSI application after the TPDD expansion, cannot be explained by this rational model.

What would be able to explain this pattern of behavior? One explanation would be a lack of awareness of the TPDD physician certification route; if there were a substantial information cost, or a zero expected probability of TPDD through this route, which changed after the TPDD expansion and increased awareness of TPDD, then we could no longer assume that individuals had rationally opted not to apply for TPDD in the pre-expansion period, either alone or in combination with SSDI/SSI application. Instead, we would only be able to infer that they had preferred to apply for SSDI/SSI based on the expected benefits from those programs alone. Indeed, one implication of this explanation would be TPDD physician certification applications among post-expansion induced SSDI/SSI applicants who were not assigned “medical improvement not expected” designations. Unfortunately, a lack of access to longitudinal TPDD data precludes conducting such an analysis, and TPDD program participation data currently available only cover the post-expansion period. However, future work examining more recent policy changes such as the automatic notification of student loan-holding SSDI/SSI recipients could determine the plausibility of the information margin story.

Another potential explanation is that the cost of physician certification (c_{Phys}) may not be symmetric between the two processes of SSDI/SSI certification and TPDD certification. Most major metropolitan areas have at least one SSA office nearby, and the offices may serve as an important resource in explaining the steps necessary to obtain physician certification and walking the potential applicant through the paperwork, reducing implicit costs. Future work employing individual-level data with information on distance from SSA offices could shed light on the plausibility of this mechanism: if debt-holding individuals located near an SSA office were more likely to be induced to apply for SSDI/SSI than debt-holding individuals located away from SSA offices, then implicit costs may be a compelling explanation.

Conclusions, policy implications, and future work

Student loan indebtedness is a substantial weight on many individuals' and households' finances, just as coping with chronic and long-term health conditions that interfere with employment can be. The July 2013 expansion of the Total and Permanent Disability Discharge program to include permanently disabled Social Security Disability Insurance and Supplemental Security Income recipients therefore represents an opportunity to alleviate much of this financial strain through an existing disability determination process. Indeed, our analyses show that this expansion led to a substantial increase in applications to SSDI among those with student loan indebtedness, and these applicants were disproportionately likely to be accepted onto the program, implying that these induced applications improved the targeting efficiency of SSDI. Furthermore, this increase in SSDI applications and awards is largely driven by those who had zero earnings in the year prior to application, suggesting only limited disemployment effects from the program expansion. However, the increase in awards was not driven by an increase in *permanent* disability awards: these new SSDI recipients were generally not eligible for TPDD discharge through the SSA pathway, as they were determined to have shorter-termed disabilities.

The result of induced applications with higher-than-average targeting efficiency among those with student loans has implications for both the overall size of disability programs and their geographic distribution. The geographic distribution of student loan borrowers is distinct from the distribution of prior SSI/SSDI applicants and recipients, as shown in Figures 3 and 4. If use of student loans and the disability discharge program persists, the extant distribution of SSA offices may diverge from that of the recipient population, and the opening of new offices may have additional implications for program applications (Deshpande and Li 2019).

Thus, the TPDD discharge may have brought to disabled individuals' attention the existence of SSDI, or otherwise increased its perceived value, although our findings indicate that any increase in value due to federal student loan discharge would not have been realized through the SSDI route alone. Instead, our findings suggest that program interactions with disability programs—specifically, changes in eligibility for debt relief upon eligible for these disability programs—can result in substantial changes in disability program participation. Examination of the relative transaction costs associated with participation in these programs, however, implies a lack of full awareness of existing paths to student loan discharge or a misperception of application costs, since our purely rational model cannot explain the patterns we observe. These findings provide suggestive evidence that further efforts to improve information and reduce application costs (such as the recent efforts to notify existing loan-holding SSI/SSDI recipients and 100%-disability-rated veterans of their eligibility for TPDD) may dramatically increase usage of these programs. Future research can continue to shed light on the role of debt structure in disability program participation, as well as the impacts on the financial, mental, and physical well-being of those affected by this TPDD expansion.

Tables and Figures

Table 1. Summary statistics for individual-level data (SIPP-SSA match)

| | All | Respondents Reporting Any Student Debt in January 2014 | Respondents Reporting No Student Debt in January 2014 | All, SSDI Covered in Prior Year |
|-------------------------------|----------|--|---|---------------------------------|
| Age | 41.06 | 35.13 | 42.22 | 41.94 |
| Female | 0.52 | 0.58 | 0.51 | 0.50 |
| White, Non-Hispanic | 0.63 | 0.66 | 0.62 | 0.67 |
| Black, Non-Hispanic | 0.13 | 0.16 | 0.12 | 0.13 |
| Hispanic | 0.17 | 0.11 | 0.18 | 0.13 |
| Other, Non-Hispanic | 0.08 | 0.07 | 0.08 | 0.06 |
| Less than High School | 0.10 | 0.01 | 0.12 | 0.07 |
| High School | 0.31 | 0.13 | 0.35 | 0.30 |
| Some College | 0.31 | 0.41 | 0.29 | 0.32 |
| College Plus | 0.28 | 0.45 | 0.25 | 0.30 |
| Married | 0.52 | 0.45 | 0.54 | 0.55 |
| Any Credit Card Debt | 0.31 | 0.49 | 0.28 | 0.35 |
| Any Student Debt | 0.16 | 1.00 | 0.00 | 0.18 |
| Total Student Debt | 3,801.23 | 23,183.71 | 0.00 | 4,325.19 |
| Post July 2013 X Student Debt | 0.07 | 0.43 | 0.00 | 0.09 |
| N | 41,272 | 6,767 | 34,505 | 30,895 |

Table 2. Discrete-Hazard Logit: Applied to SSDI or SSI

| | (1) | (2) | (3) | (4) | (5) |
|-------------------------|--------------|--------------|--------------|--------------|---------------|
| Treatment Odds Ratio | 1.48*** | 1.46*** | 1.46*** | 1.46*** | 1.66*** |
| 95% Confidence Interval | (1.15, 1.89) | (1.14, 1.87) | (1.14, 1.87) | (1.14, 1.87) | (1.18, 2.33) |
| Time Dummy | Post | Post | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes |
| Demographics | No | Yes | Yes | Yes | Yes |
| SE Cluster | No | No | No | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | None | None | None | None | Some College+ |
| N | 1,071,957 | 1,071,957 | 1,071,957 | 1,071,957 | 638,714 |
| Pre-2013 Average | 0.0030 | 0.0030 | 0.0030 | 0.0030 | 0.0020 |

Note: Quarter-level, discrete-hazard logit analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Discrete-Hazard Logit: Applied to SSDI

| | (1) | (2) | (3) | (4) | (5) |
|-------------------------|-------------------------------|-------------------------------|-------------------------------|-------------------------------|--|
| Treatment Odds Ratio | 1.40** | 1.36** | 1.36** | 1.36** | 1.51** |
| 95% Confidence Interval | (1.05, 1.86) | (1.02, 1.81) | (1.02, 1.81) | (1.02, 1.82) | (1.07, 2.14) |
| Time Dummy | Post | Post | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes |
| Demographics | No | Yes | Yes | Yes | Yes |
| SE Cluster | No | No | No | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year; Some College+ |
| N | 699,473 | 699,474 | 699,475 | 699,476 | 450,226 |
| Pre-2013 Average | 0.0031 | 0.0031 | 0.0031 | 0.0031 | 0.0021 |

Note: Quarter-level, discrete-hazard logit analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Year-Level Discrete Hazards of SSDI or SSI Application

| | Logit | | | OLS | | |
|--|--------------|-----------------------------------|--|------------|-----------------------------------|--|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 2010 X Student Debt Odds Ratio/Estimate | 1.44 | 1.37 | 0.65 | 0.00173 | 0.00118 | -0.00932 |
| 95% Confidence Interval/SE | (0.92, 2.24) | (0.81, 2.34) | (0.13, 3.10) | (0.00166) | (0.00205) | (0.0120) |
| 2011 X Student Debt Odds Ratio/Estimate | 1.16 | 1.27 | 1.69 | 0.000927 | 0.00148 | 0.00716 |
| 95% Confidence Interval/SE | (0.72, 1.86) | (0.73, 2.22) | (0.46, 6.14) | (0.00154) | (0.00193) | (0.0139) |
| 2014 X Student Debt Odds Ratio/Estimate | 1.65** | 1.74** | 8.66*** | 0.00404** | 0.00409** | 0.0619*** |
| 95% Confidence Interval/SE | (1.04, 2.62) | (1.02, 2.96) | (2.54, 29.49) | (0.00159) | (0.00196) | (0.0172) |
| 2015 X Student Debt Odds Ratio/Estimate | 1.90*** | 1.63* | 2.08 | 0.00540*** | 0.00381** | 0.0206* |
| 95% Confidence Interval/SE | (1.19, 3.03) | (0.94, 2.81) | (0.43, 10.11) | (0.00157) | (0.00189) | (0.0124) |
| 2016 X Student Debt Odds Ratio/Estimate | 1.49 | 1.29 | 1.98 | 0.00398*** | 0.00250 | 0.0200 |
| 95% Confidence Interval/SE | (0.91, 2.44) | (0.72, 2.29) | (0.41, 9.61) | (0.00149) | (0.00179) | (0.0126) |
| Time Dummy | Year | Year | Year | Year | Year | Year |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes | Yes |
| Demographics | Yes | Yes | Yes | Yes | Yes | Yes |
| SE Cluster | Individual | Individual | Individual | Individual | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 | 2013 | 2013 | 2013 | 2013 | 2013 |
| Other Restriction | None | SSDI- Covered in Prior Year | Covered in Prior Year, No Earnings in Prior Year | None | SSDI- Covered in Prior Year | Covered in Prior Year, No Earnings in Prior Year |
| N | 239,214 | 155,918 | 10,201 | 239,214 | 155,918 | 10,201 |

Note: Quarter-level, discrete-hazard logit and OLS analyses. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Discrete-Hazard Logit, by Outcome

| | Applied only SSDI (1) | Applied Concurrently (2) | Accepted only SSDI (3) | Accepted Concurrently (4) | Accepted with Permanent Disability (5) | Accepted without Permanent Disability (6) |
|----------------------------------|-------------------------------|--------------------------------|-------------------------------|---------------------------------|---|--|
| Treatment Odds Ratio Estimate | 1.60** | 1.09 | 2.06** | 2.65** | 1.66 | 3.66*** |
| 95% Confidence Interval | (1.08, 2.37) | (0.70, 1.69) | (1.10, 3.86) | (1.09, 6.49) | (0.88, 3.13) | (1.90, 7.05) |
| Time Dummy | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes | Yes |
| Demographics | Yes | Yes | Yes | Yes | Yes | Yes |
| SE Cluster | Individual | Individual | Individual | Individual | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | None | None |
| N | 699,473 | 699,473 | 699,473 | 699,473 | 1,071,957 | 1,071,957 |

Note: Quarter-level, discrete-hazard logit analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.

Table 6. Discrete-Hazard Logit by Primary Diagnosis and by Prior Work

| | Discrete-Hazard Logit by Primary Diagnosis | | | Logit by Prior Work |
|---------------------------------------|--|-------------------------------|-------------------------------|-----------------------------------|
| | Musculoskeletal (1) | Mental Disorder (2) | Other Body System (3) | SSDI or SSI Application (4) |
| Treatment Odds Ratio Estimate | 1.56** | 1.12 | 1.72*** | 3.56*** |
| 95% Confidence Interval | (1.04, 2.33) | (0.67, 1.86) | (1.15, 2.59) | (1.76, 7.20) |
| Treatment X Any Earnings in Last Year | | | | 0.32*** |
| 95% Confidence Interval | | | | (0.15, 0.69) |
| Time Dummy | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes |
| Demographics | Yes | Yes | Yes | Yes |
| SE Cluster | Individual | Individual | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Other Restriction | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year | SSDI-Covered in Prior Year |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| N | 699,478 | 699,478 | 699,478 | 699,478 |

Note: Quarter-level, discrete-hazard logit analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Discrete-Hazard Models: Applied to SSDI or SSI

| | Logit | | OLS | |
|--------------------------------------|--------------|-------------------------------|--------------|-------------------------------|
| | (1) | (2) | (3) | (4) |
| Age 18-29 X Post Odds Ratio/Estimate | 0.72 | 0.59 | 0.00004 | -0.000309 |
| 95% Confidence Interval/SE | (0.37, 1.39) | (0.26, 1.34) | (0.000251) | (0.000358) |
| Age 30-39 X Post Odds Ratio/Estimate | 1.80** | 1.61 | 0.00102** | 0.000788* |
| 95% Confidence Interval/SE | (1.10, 2.96) | (0.89, 2.90) | (0.000418) | (0.000447) |
| Age 40-49 X Post Odds Ratio/Estimate | 1.42 | 1.60* | 0.000972 | 0.00121* |
| 95% Confidence Interval/SE | (0.86, 2.37) | (0.92, 2.81) | (0.000617) | (0.000691) |
| Age 50-59 X Post Odds Ratio/Estimate | 2.06*** | 2.37** | 0.00195** | 0.00171** |
| 95% Confidence Interval/SE | (1.20, 3.54) | (1.21, 4.61) | (0.000772) | (0.000807) |
| Age 60-65 X Post Odds Ratio/Estimate | 2.35** | 1.84 | 0.00297 | 0.00300 |
| 95% Confidence Interval/SE | (1.06, 5.20) | (0.78, 4.30) | (0.00205) | (0.00288) |
| Time Dummy | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes |
| Demographics | Yes | Yes | Yes | Yes |
| SE Cluster | Individual | Individual | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | None | SSDI-Covered in Prior Year | None | SSDI-Covered in Prior Year |
| N | 1,071,957 | 699,478 | 1,071,957 | 639,416 |

Note: Quarter-level, discrete-hazard logit and OLS analyses. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Discrete-Hazard Logit: Applied to SSDI or SSI

| | (1) | (2) | (3) | (4) | (5) | (6) |
|------------------------------------|--------------|--------------|--------------|--------------|--------------|--------------|
| Student Debt X Post Odds Ratio | 1.48*** | | 1.42*** | | 1.48*** | 1.44*** |
| 95% Confidence Interval | (1.15, 1.89) | | (1.10, 1.82) | | (1.15, 1.89) | (1.12, 1.85) |
| Odds Ratio | 1.48 | | 1.42 | | 1.48 | 1.44 |
| Credit Card Debt X Post Odds Ratio | | 1.21** | 1.17 | | | 1.18* |
| 95% Confidence Interval | | (1.01, 1.45) | (0.97, 1.40) | | | (0.98, 1.42) |
| Odds Ratio | | 1.21 | 1.17 | | | 1.18 |
| Other Debt X Post Odds Ratio | | | | 0.98 | 0.95 | 0.93 |
| 95% Confidence Interval | | | | (0.82, 1.18) | (0.79, 1.14) | (0.77, 1.12) |
| Odds Ratio | | | | 0.98 | 0.95 | 0.93 |
| Time Dummy | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes | Yes |
| Demographics | Yes | Yes | Yes | Yes | Yes | Yes |
| Weights | No | No | No | No | No | No |
| SE Cluster | Individual | Individual | Individual | Individual | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | None | None | None | None | None | None |
| N | 1,071,957 | 1,071,957 | 1,071,957 | 1,071,957 | 1,071,957 | 1,071,957 |

Note: Quarter-level, discrete-hazard logit analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Total Number of Approved Discharges (Borrowers)

| Year | Physician | SSA | SSA | | VA | | Total |
|------|-----------|--------|--------|--------|--------|---------|-------|
| | | | Match | VA | Match | VA | |
| 2014 | 66,278 | 22,813 | 0 | 5,966 | 0 | 95,057 | |
| 2015 | 69,842 | 24,644 | 0 | 9,274 | 0 | 103,760 | |
| 2016 | 61,283 | 17,270 | 83,143 | 9,233 | 0 | 170,929 | |
| 2017 | 59,717 | 7,346 | 47,319 | 11,624 | 0 | 126,006 | |
| 2018 | 53,166 | 6,367 | 37,879 | 10,644 | 16,293 | 124,349 | |

Note: Table gives the number of approved discharges of federal student loan debt through the Total and Permanent Disability Discharge program. These data are only available starting in 2014, after the SSA pathway was introduced. Additionally, in 2016, SSA and the Department of Education aligned their databases and notified eligible disabled borrowers about the program. In 2018, Veterans Affairs (VA) conducted the same alignment and notification. In 2019, the ED began automatically discharging the debt of eligible disabled veteran borrowers.

Source: Federal Student Aid, Department of Education

Figure 3. Percent of county with any student loan debt.

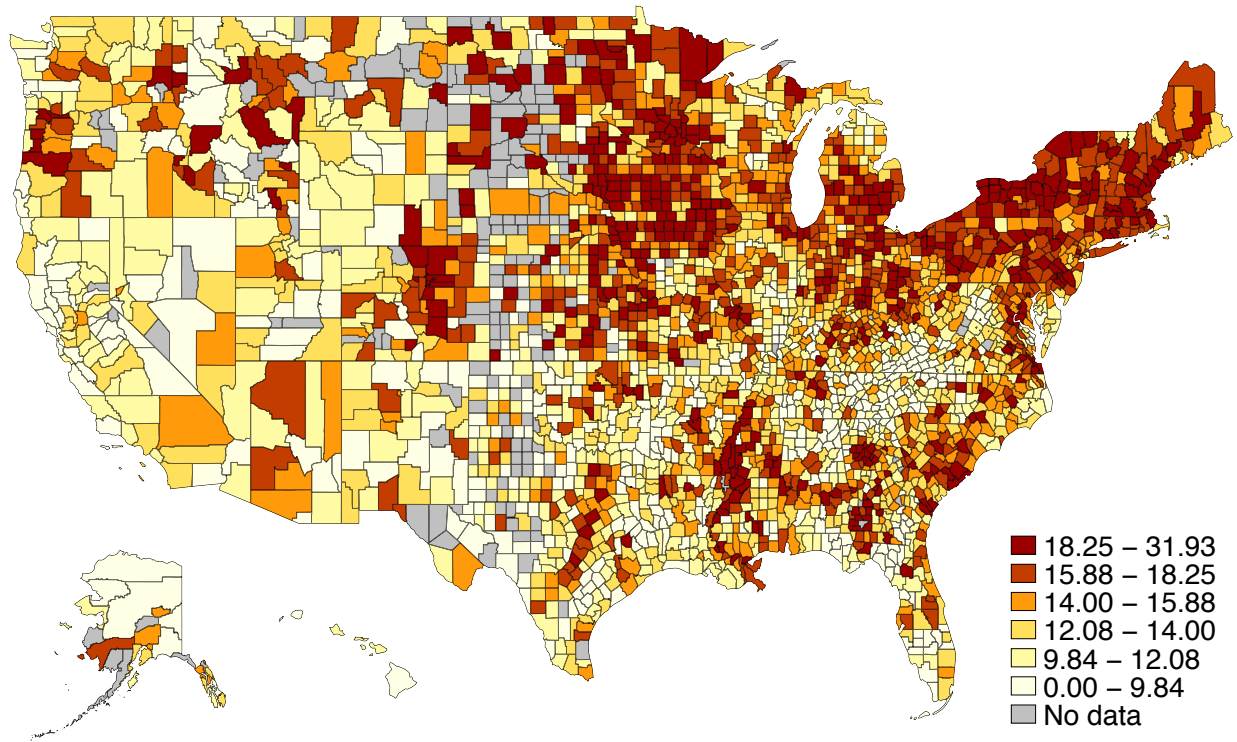
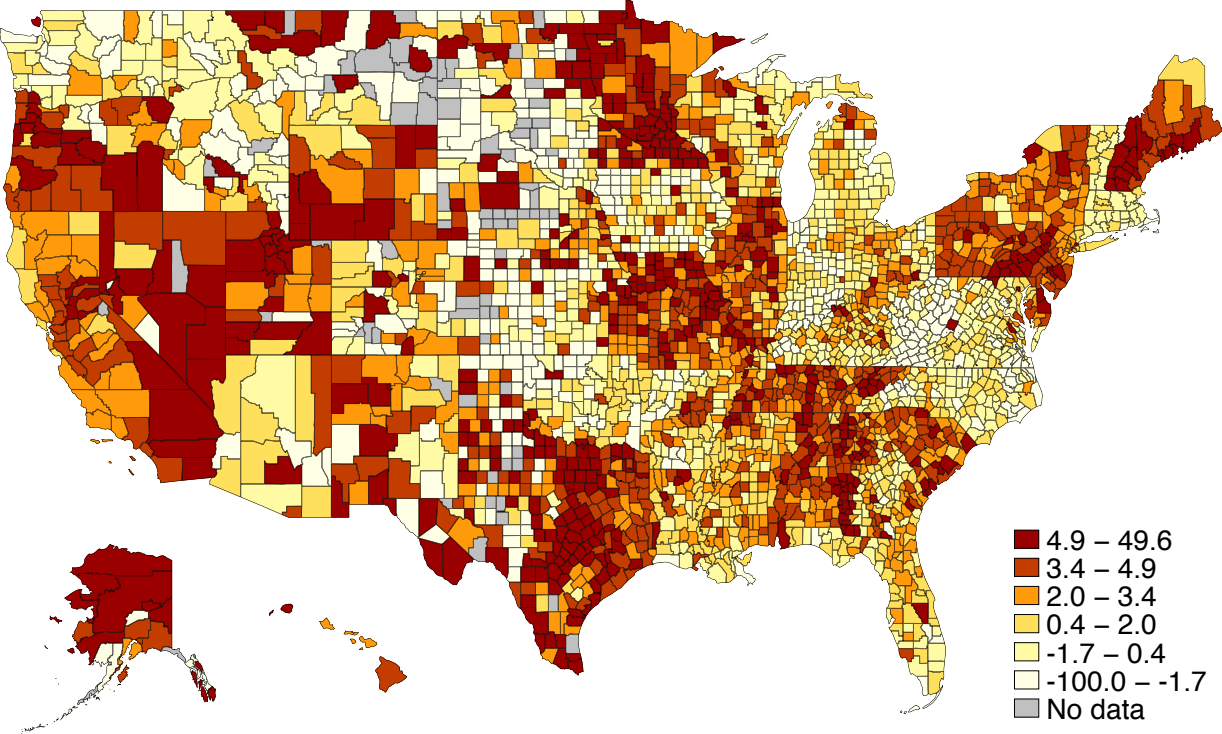


Figure 4. County SSDI application rate, 2012.



Appendix Table 1. Discrete-Hazard OLS: Applied to SSDI or SSI

| | (1) | (2) | (3) | (4) | (5) |
|------------------------|------------|-------------|--------------|--------------|---------------|
| Treatment Estimate | 0.00102*** | 0.000984*** | 0.000983*** | 0.000983*** | 0.000754*** |
| SE | (0.000270) | (0.000269) | (0.000269) | (0.000221) | (0.000228) |
| Time Dummy | Post | Post | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes |
| Demographics | No | Yes | Yes | Yes | Yes |
| SE Cluster | No | No | No | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | None | None | None | None | Some College+ |
| N | 1,071,957 | 1,071,957 | 1,071,957 | 1,071,957 | 638,714 |
| Pre-2013 Average | 0.0030 | 0.0030 | 0.0030 | 0.0030 | 0.0020 |

Note: Quarter-level, discrete-hazard OLS analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2. Discrete-Hazard Weighted Least Squares: Applied to SSDI or SSI

| | (1) | (2) | (3) | (4) | (5) |
|------------------------|-------------|-------------|--------------|--------------|------------------|
| Treatment Estimate | 0.000855*** | 0.000829*** | 0.000829*** | 0.000829*** | 0.000684*** |
| SE | (0.000241) | (0.000241) | (0.000241) | (0.000220) | (0.000220) |
| Time Dummy | Post | Post | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes |
| Demographics | No | Yes | Yes | Yes | Yes |
| SE Cluster | No | No | No | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | None | None | None | None | Some College+ |
| N | 1,071,957 | 1,071,957 | 1,071,957 | 1,071,957 | 638,714 |
| Pre-2013 Average | 0.0030 | 0.0030 | 0.0030 | 0.0030 | 0.0020 |

Note: Quarter-level, discrete-hazard WLS analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared. *** p<0.01, ** p<0.05, * p<0.

Appendix Table 3. Discrete-Hazard Logit, by Outcome

| | Applied only SSDI (1) | Applied Concurrently (2) | Accepted only SSDI (3) | Accepted Concurrently (4) | Accepted with Permanent Disability (5) | Accepted without Permanent Disability (6) |
|----------------------------------|--|--|--|--|---|--|
| Treatment Odds Ratio Estimate | 3.01** | 3.95*** | 3.56 | 24.00*** | 2.80* | 3.83** |
| 95% Confidence Interval | (1.05, 8.62) | (1.47, 10.61) | (0.61, 20.86) | (2.50, 229.95) | (0.86, 9.11) | (1.19, 12.31) |
| Time Dummy | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter | YearXQuarter |
| Any Student Debt Dummy | Yes | Yes | Yes | Yes | Yes | Yes |
| Demographics | Yes | Yes | Yes | Yes | Yes | Yes |
| SE Cluster | Individual | Individual | Individual | Individual | Individual | Individual |
| Years | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 | 2010-2016 |
| Excluded Period | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 | 2013 Q3 |
| Other Restriction | SSDI-Covered in Prior Year, No Earnings in Prior Year | SSDI-Covered in Prior Year, No Earnings in Prior Year | SSDI-Covered in Prior Year, No Earnings in Prior Year | SSDI-Covered in Prior Year, No Earnings in Prior Year | No Earnings in Prior Year | No Earnings in Prior Year |
| N | 45,596 | 45,596 | 45,596 | 45,596 | 181,698 | 181,698 |

Notes: Quarter-level, discrete-hazard logit analysis. Includes years 2010-2016. Demographics include race, ethnicity, sex, educational attainment, age, and age squared.

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