Disability and Distress: The Effect of Disability Programs on Financial Outcomes*

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Abstract

We provide the first evidence on the relationship between disability programs and markers of financial distress: bankruptcy, foreclosure, eviction, and home sale. Rates of these adverse financial events peak around the time of disability application and subsequently fall for both allowed and denied applicants. To estimate the causal effect of disability programs on these outcomes, we use variation induced by an age-based eligibility rule and find that disability allowance substantially reduces the likelihood of adverse financial events. Within three years of the decision, the likelihood of bankruptcy falls by 0.77 percentage point (31 percent), and the likelihood of foreclosure and home sale among homeowners falls by 1.8 percentage points (34 percent) and 1.8 percentage points (15 percent), respectively. Conversely, the likelihood of home purchases increases by 0.60 percentage point (14 percent) within three years. We present evidence that these changes reflect true reductions in financial distress. Considering these extreme events increases the optimal disability benefit amount and suggests a shorter optimal waiting time.

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More than 6 percent of working-age adults in the United States receive disability benefits through the Social Security Disability Insurance (SSDI) program or the Supplemental Security Income (SSI) program. The expansion of these programs over the past decades has prompted a public debate about their costs and benefits. On the cost side, disability programs can distort decisions about work and human-capital investment. On the benefits side, disability programs can provide protection against major consumption shocks such as disability.

Research on disability programs has focused mostly on the costs of these programs, especially on their labor-supply effects, which are often interpreted as moral-hazard costs. Several studies have found that allowance onto disability insurance reduces labor force participation by about 30 percentage points.[1] If interpreted as moral hazard, these labor supply effects suggest that disability programs involve some disincentive costs.

Yet there is little evidence on the other side of the analysis, the benefits of disability programs. To our knowledge, there are no quasi-experimental studies that assess the effects of US disability programs on outcomes other than labor supply and mortality.[2] In the absence of such studies, evidence on how disability programs affect quality of life, residential stability, and consumption is mostly anecdotal. In *Evicted*, Matthew Desmond writes of a recipient of the SSI program that "her $754 monthly [SSI] check was more reliable than any job she could get," and explains that landlords seek out SSI recipients because their stable income makes them reliable tenants (Desmond, 2016). These hypotheses have yet to be tested in empirical research.

This paper presents the first evidence on the effect of disability programs on financial outcomes. We link administrative records from the Social Security Administration's (SSA) SSDI and SSI programs to records on bankruptcy, foreclosure, eviction, home purchases, and home sales.[3] These financial outcomes are not direct measures of consumption or well-being.

[1] Bound (1989) uses the labor supply of denied disability-insurance applicants as an upper bound for allowed applicants, concluding that employment among disability-insurance recipients would be, at most, 30 percentage points higher had they been denied. Updating Bound's analysis, von Wachter et al. (2011) find similar effects for older cohorts and larger effects for younger cohorts. Chen and van der Klaauw (2008) find smaller employment effects for more-recent applicants. Maestas et al. (2013) and French and Song (2014) use examiner- and judge-based fixed-effects instruments to estimate labor-supply effects of around 30 percentage points. Moore (2015) estimates similar effects for disability recipients whose eligibility based on drug and alcohol addiction was terminated as part of the 1996 welfare reform law.

[2] Autor et al. (Forthcoming) study the effects of disability benefits receipt on consumption in Norway, and Gelber et al. (2018) study the effect of disability benefits on mortality in the US. Meyer and Mok (2018) study differences in the consumption drop surrounding disability for those who receive disability benefits and those who do not. Low and Pistaferri (2015) model the role of disability benefits and their interaction with other welfare programs in insuring the consumption of disability recipients.

[3] All of the non-SSA data we study exist in the public domain. Gross and Trenkamp (2015) were the first to link bankruptcy data to SSA administrative data.

They are rather "tail events," events that occur infrequently and are associated with large drops in consumption. In the absence of administrative data on consumption, studying these extreme events sheds light on fluctuations in consumption that would otherwise be entirely unobservable.

Using this novel dataset, we document three descriptive facts. First, rates of bankruptcy, foreclosure, and eviction among applicants are higher than in the general population, suggesting that disability applicants are more likely to experience financial distress. Second, for disability applicants, rates of these adverse financial events increase leading up to the application date and peak around the application date. This trend indicates that disability applicants apply for disability programs when they are in relatively high financial distress.

This evidence of selection effects and time effects points to the need for causal identification of the effect of disability programs on financial outcomes. To identify the causal effect, we exploit an administrative rule that governs how the SSA evaluates applicants. During the fifth step of the initial determination process, disability examiners decide whether an applicant can work in some capacity given his or her disability as well as vocational factors such as age, education, and experience. SSA guidelines require examiners to use more-lenient standards for older applicants. Applicants who are older than age 55 at the time of decision are judged using more-lenient standards than applicants between ages 50 and 55, who in turn are judged using more-lenient standards than applicants below age 50. These age-based rules allow us to isolate the causal effect of disability receipt on financial outcomes.[4]

We find that being allowed onto disability programs at the initial level (before appeals) results in large declines in rates of bankruptcy, foreclosure, and home sale. Initial allowance reduces the likelihood of filing for bankruptcy by a statistically significant 0.77 percentage point, or 31 percent, in the next three years. For homeowners, the likelihood of experiencing foreclosure in the three years after initial decision falls by 1.8 percentage points (34 percent) and the likelihood of selling a home falls by 1.8 percentage points (15 percent), both statistically significant. Allowance onto disability programs also increases home purchases by a statistically significant 0.60 percentage point (14 percent). Most of the change in housing transactions is driven by allowed applicants becoming first-time homeowners or being less likely to sell their home overall, not by a change in the likelihood of moving from one home to another. These results suggest that some program recipients use their benefits to purchase homes or to stay in homes that they might otherwise have sold or lost to foreclosure.

The validity of these quasi-experimental estimates could be threatened if either applicants

[4] Chen and van der Klaauw (2008) first studied these age-based eligibility rules in order to estimate the effect of disability insurance on labor supply. Carey et al. (2019) exploit the vocational grid in order to understand variation in disability applications over the business cycle.

or disability examiners manipulate the age at decision by deferring applications. Indeed, we find that the number of applications increases by 3 percent at the age thresholds. However, we find no evidence of *differential* manipulation based on financial distress. We find no discontinuity in the pre-application rates of financial distress. To test for dynamic selection based on expected financial distress, we conduct a simple bounding exercise. We find that even if applicants whose applications are deferred were to have bankruptcy and foreclosure rates of zero, their existence could not fully explain the main reduced-form estimates. We also find no discontinuity in post-application rates of financial distress for applicants whose case is decided prior to the fifth step of the disability initial determination process. All of this suggests that the strategic timing of some applications has little effect on the main results.

Of course, these financial outcomes are not direct measures of recipient welfare. We outline the assumptions required to interpret the declines in adverse financial events as a "wealth effect:" true reductions in financial distress due to the transfer of cash and health insurance, and therefore improvements in recipient welfare. We consider alternative mechanisms, such as changes in access to credit and demand for credit. We conclude based on evidence from several sources that they are unlikely to drive the results and, if anything, would lead us to underestimate the wealth effect.

To consider the welfare implications of these results, we use our estimates to extend the standard calculations of optimal disability benefits in two ways. First, we incorporate tail consumption risk into optimal-benefits calculations. Standard calculations of optimal-benefit levels use the difference between mean consumption in the good state of the world and mean consumption in the bad state of the world as a sufficient statistic for the welfare gains from insurance. However, risk-averse agents also value the likelihood of extreme losses in consumption in addition to their mean consumption. We map the "tail events" that we observe into consumption changes using survey data. Foreclosure, for example, is associated with an annual $6,300 drop in consumption, based on an event-study analysis using the Panel Study of Income Dynamics. We find that the optimal annual benefit increases by several hundred dollars when we use these estimates of tail consumption risk in optimal-benefits calculations. This increase would likely be even larger considering effects along the entire consumption distribution, since the variance of consumption would be larger.

Second, we extend the standard calculations of optimal-benefit levels to consider spillovers to third parties from disability programs. We focus on higher property values for neighboring property owners from the reduction in foreclosures, though there could be other spillovers from disability programs. We find that the optimal annual benefit amount increases by approximately one hundred dollars when considering spillovers to neighboring property owners. Another way to put the property-related spillovers in context is to compare them to the effect of the disability programs on earnings. Disability allowance reduces labor market earnings by \$3,500 over three years; it increases housing values due to an averted foreclosure by roughly \$2,600, which is 75 percent of the decrease in earnings.[5]

Finally, we consider the implications of our results for the optimal *timing* of disability benefits. The descriptive exercises below suggest that disability-program applicants apply for the program when they are financially distressed, both relative to the general population and relative to their own histories. The quasi-experimental exercises below suggest that disability-program benefits substantially reduce financial distress. All else equal, these two sets of findings together make the case for shorter wait times, though they must be weighed against the administrative costs and potential selection effects of shorter wait times.

The paper proceeds as follows. Section 1 describes the datasets and data-merge procedures. Section 2 presents descriptive facts on financial outcomes for the disability-program-applicant population. Section 3 describes the age-based eligibility rule, presents preliminary visual evidence of the causal effect, and presents IV estimates of the effect of disability programs on financial outcomes. Finally, Section 5 discusses the implications for recipient and social welfare and presents optimal benefit calculations, and Section 6 concludes.

1 Institutional Background and Data

1.1 Background on Disability Programs and the Financial Outcomes We Study

SSA administers the Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) programs. SSDI and SSI have the same medical requirements but different non-medical requirements. SSDI requires an earnings history.[6] SSI requires applicants to have low income and low assets. Individuals can apply for and receive benefits from both programs concurrently if they meet both sets of requirements. If applicants are allowed onto both programs, the SSI benefit is reduced by approximately the amount of the SSDI benefit.

The non-medical eligibility of both disability programs also requires applicants not to engage in substantial gainful activity (SGA). The SGA threshold for 2017 was \$1,170 per month, which means that applicants will be denied for benefits if they earn more than \$1,170 a month on average. The average monthly benefit for SSDI in 2017 was \$1,197 and the maximum monthly benefit for SSI for an individual was \$735.[7] Program rules prohibit

[5]We quantify this positive externality and incorporate it into calculation of the marginal value of public funds (MVPF) for disability programs (Hendren, 2016). The positive spillovers due to reductions in bankruptcy and foreclosure increase the MVPF of disability programs from 0.99 to 1.04.

[6]The earnings history is usually the applicant's, though there are circumstances under which a widow or widower applies based on their spouse's earnings history.

[7]Annual Statistical Report on the Social Security Disability Insurance Program, 2017, Table 2; and Fast

disability recipients from performing SGA and receiving disability benefits at the same time.

Bankruptcy is a legal procedure available to debtors overwhelmed by their debts. Bankruptcy filers can either file for Chapter 7 and have their debts discharged entirely, or file for Chapter 13 and commit to a repayment plan. The Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 imposed a means test for Chapter 7 discharge, and the number of bankruptcy filings, particularly Chapter 7, plummeted after 2005 as a result of this reform.[8] Filing for either type of bankruptcy is expensive; bankruptcy attorney fees typically cost at least $1,000, and many households must thus "save up" for bankruptcy (Gross et al., 2016).

In contrast to bankruptcy, the foreclosure process is initiated by a lender in response to a borrower who has become delinquent on a secured loan. The mortgage lender first issues a precaution notification and only then may choose to pursue a forced home sale in order to recover the remaining mortgage debt. Depending on state law, the time required to complete a foreclosure process varies from six months to eighteen months. In some cases, lenders and homeowners can reach an agreement or negotiate a settlement plan so that the debtors can keep the home.

Eviction is a legal process that landlords use to remove tenants for failing to pay rent or breaking other terms of the lease.[9] After an initial grace period, a landlord can choose to file a request with the court and the tenant will be served. If the judge grants the landlords request, an order is placed with the sheriff and the sheriff evicts the tenant. Depending on jurisdiction and case backlogs, the entire eviction process varies from 30 days to more than six months.

1.2 Merging Social Security Disability Records to Financial Records

We link administrative records from the Social Security Administration to records on bankruptcies, foreclosures, evictions, and home transactions. Figure 1 summarizes the data merges. We start with an extract of the SSA 831 Disability File (F831) that includes the universe

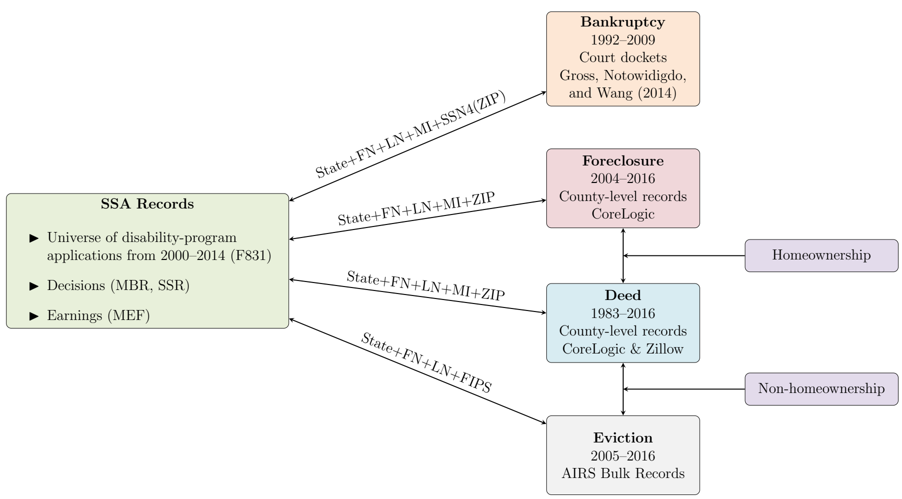
Facts and Figures about Social Security, 2018.

[8]Before 2005, consumers could choose under which chapter they wanted to file. Filers faced a tradeoff: under Chapter 7, their "non-exempt" assets would be divided among their creditors, while under Chapter 13, they would not lose their assets. Social Security benefits are excluded from the means test, and so allowance onto a disability program does not mechanically affect the choice of bankruptcy chapter (Social Security Rulings 79–4).

[9]"Formal" eviction involves both removing the tenant and recovering back rent, while "summary" eviction involves only removing the tenant. Both processes involve legal filings with a court, but many landlords prefer summary eviction as it is relatively easy to file without the assistance of an attorney and the procedure is shorter. There are also "informal eviction" cases where tenants are forced to end their lease due to difficulties, such as large increases in rent, created by landlords. As these cases do not involve any court filing, we are not able to observe these cases in our data.

of disability-program applicants who received a disability decision between 2000 and 2014. The F831 files provide identifiers, including Social Security number (SSN), first name, last name, middle initial, and ZIP Code of residence; application history, including the dates of application and initial decision and the reason for the decision; and demographic information, including body system code, specific diagnosis, and, for those who are allowed, medical diary reason, which determines the frequency of continuing disability reviews. For the purposes of the quasi-experimental analysis, we use the classification of regulation basis codes in the F831 files developed by Wixon and Strand (2013). We then link the F831 extract to extracts of several other SSA datasets. The Master Beneficiary Record (MBR) provides the final disability decision and decision date for SSDI applicants, and the Supplemental Security Record (SSR) provides these variables for SSI applicants. The Master Earnings File (MEF) provides annual earnings for all workers. The Structured Data Repository (SDR) provides applicant ZIP Codes after 2010.

Figure 1: Merging Social Security Disability Records to Financial Records



Notes: This figure describes the identifiers we use to link the administrative records. We start with Social Security Administration records: disability-program applications from 2000–2014 from the 831 Disability File (F831), disability-program decisions from the Master Beneficiary Record (MBR) and Supplemental Security Record (SSR), and earnings from the Master Earnings File (MEF). We then link the SSA data to bankruptcy records compiled by Gross et al. (2016), to foreclosure records from CoreLogic, to deeds records from CoreLogic and Zillow, and to eviction records obtained from AIRS. We use the CoreLogic and Zillow data to establish a sample of homeowners for the foreclosure sample and non-homeowners for the eviction sample. “SSN4” indicates the last four digits of Social Security number. “FN” indicates first name, “LN” indicates last name, and “MI” indicates middle initial.

We link the SSA data to public records on several financial outcomes: bankruptcies, foreclosures, evictions, and home deeds. We summarize the merge procedures here and provide more detail in Appendix A. Bankruptcy records, collected by Gross et al. (2016), consist of a near-census of personal bankruptcies for a majority of bankruptcy districts from the mid-1990s through 2009 (2011 for some districts). The bankruptcy records list the names and last four digits of SSN of the filers, date of filing, chapter, and address. We link SSA records to these bankruptcy records using primarily the last four digits of SSN, first name, last name, middle initial, and state.[10]

We combine records on home transactions from two sources, CoreLogic and Zillow, so as to ensure that the coverage is as comprehensive as possible. The combined data covers home purchases and sales across the United States from 1983 to 2016.[11] These records include seller and buyer names, transaction dates and amounts, property ZIP Code, and characteristics of the house.[12] We remove blank entries, duplicate transaction records, commercial properties, and intra-family transfers.

The housing records do not include unique individual identifiers such as SSN. For that reason, we merge the SSA records to housing transactions based on first name, last name, middle initial, and ZIP Code. These merge variables could be problematic if different residents of the same ZIP Code have the same name. For that reason, we drop individuals with more than six transactions associated with their names and ZIP Codes, which consist of less than 1 percent of the sample population for most states. We use this merge to identify homeownership, home sales, and home purchases.

We use CoreLogic foreclosure records from 2004 to 2016. We remove blank entries, duplicate records, commercial properties, records with missing or invalid names, and records in which cases were settled without the properties being auctioned. In addition, we drop ZIP Codes from our initial record linkage process if defendant names are missing in more than 10 percent of the foreclosure records. Our main foreclosure sample comes from the population of homeowners identified in the CoreLogic-Zillow deeds data. As a validity check, we link the foreclosure records to the home deeds and find that 82 percent of the foreclosure records link to a deeds record. We link the SSA disability records to the foreclosure records using first name, last name, middle initial, and ZIP Code.

[10] To account for potential variations in the first name (such as "Tom" versus "Thomas"), we also use alternative merges based on the last four digits of SSN, last name, ZIP Code, and state.

[11] Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the Zillow data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the authors and do not reflect the position of Zillow Group. Based on conversations with staff at CoreLogic and Zillow Research, the availability and quality of deeds varies across counties and time. To avoid analysis on small cells or areas with poor coverage, we restrict the sample to ZIP Codes with an average of at least fifteen home purchases or fifteen home sales per year between 2000 and 2014.

[12] We observe sales dates in most cases and some other dates such as filing dates and signature dates. These dates are usually the same or within 10 days of each other. In our data harmonization process, we use the earliest dates as transaction dates. We provide more details on the construction of home transaction data in Appendix A.

We use eviction records from American Information Research Services (AIRS), which collects public-record eviction court filings covering nearly 40 percent of the U.S. residential areas for various time periods. In addition, we collect eviction court filings in Harris County, Texas, from the county court's website. Each eviction court filing provides defendant names, filing date, and judgment information. We drop blank records and records with invalid names or ZIP Codes. In addition, we remove eviction filings that have been dismissed or settled. Our main eviction sample comes from the population of non-homeowners identified in the CoreLogic-Zillow deeds data. We map ZIP Code to FIPS county code to address high mobility among the renter population, and we merge eviction records from 2005 to 2016 to the SSA records based on first name, last name, FIPS county code, and middle initial when available.[13]

Appendix A discusses the reliability of each merge. The bankruptcy merge is the most reliable because it involves full name and last four digits of SSN, a near-unique combination. Using the bankruptcy data, we simulate the foreclosure and deeds merges by dropping the last four digits of SSN as an identifier, and we simulate the eviction merge by additionally dropping middle name. Dropping those identifiers results in some attenuation of the estimates, but does not substantially reduce the quality of the merge.

With the exception of the bankruptcy data, each of the merges between the SSA records and financial records requires using ZIP Code or FIPS county code as a key linking variable. The SSA records provide the applicant's ZIP Code of residence at the time of application; if the applicant moved before or after applying, we do not observe the other ZIP Codes in which that applicant lived. Of course, not observing all ZIP Codes of residence will affect the number of financial events that we observe. Appendix B shows that this issue likely causes attenuation of the estimates of the causal effect of disability allowance on home purchases, eviction, and foreclosure. As long as disability allowance does not shift home purchases (or evictions or foreclosures) that would have occurred anyway (i.e., inframarginal home purchases) from the application ZIP Code to other ZIP Codes, then this data issue will solely bias us against finding an effect. However, if disability-program allowance does shift inframarginal home purchases from within- to outside-ZIP-Code (or vice versa), then the sign of the bias cannot always be determined. The same conclusions apply to merges using FIPS county codes.

[13]The availability of middle names is substantially lower in the eviction data (15 percent) than in the deeds (70 percent) or foreclosure records (55 percent).

Table 1: Summary Statistics for the Bankruptcy, Foreclosure, and Eviction Samples

	Bankruptcy sample				Foreclosure sample				Eviction sample			
	Full Sample		Quasi-Exp. Sample		Full Sample		RD Sample		Full Sample		Quasi-Exp. Sample	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Fraction SSI adults	0.54	0.50	0.50	0.50	0.31	0.46	0.34	0.47	0.55	0.50	0.55	0.50
Fraction DI adults	0.60	0.49	0.63	0.48	0.83	0.38	0.81	0.40	0.62	0.49	0.63	0.48
Fraction reaching step 5	0.68	0.47	1.00	0.00	0.70	0.46	1.00	0.00	0.69	0.46	1.00	0.00
Fraction initially allowed	0.35	0.48	0.41	0.49	0.44	0.50	0.46	0.50	0.38	0.48	0.45	0.50
Fraction finally allowed	0.54	0.50	0.65	0.48	0.64	0.48	0.70	0.46	0.51	0.50	0.62	0.49
Mental condition	0.26	0.44	0.19	0.39	0.16	0.36	0.16	0.36	0.27	0.44	0.19	0.39
Musculoskeletal condition	0.30	0.46	0.41	0.49	0.37	0.48	0.47	0.50	0.31	0.46	0.44	0.50
Age	44.4	12.6	52.4	2.7	51.1	10.0	52.8	2.7	45.6	13.06	52.6	2.72
Male	0.52	0.50	0.55	0.50	0.52	0.50	0.55	0.50	0.54	0.50	0.56	0.50
Pre-decision annual earnings	$14,349	$18,334	$16,857	$20,079	$20,831	$21,327	$20,831	$20,807	$12,182	$16,946	$13,432	$17,426
Years of education	11.5	2.53	11.5	2.62	12.3	2.40	12.1	2.31	11.8	2.55	11.7	2.58
Ever experience financial event	0.12	0.32	0.14	0.35	0.13	0.33	0.13	0.34	0.18	0.38	0.18	0.39
Experience event before decision	0.09	0.28	0.11	0.31	0.06	0.24	0.07	0.26	0.09	0.28	0.09	0.29
Experience event after decision	0.04	0.19	0.04	0.19	0.07	0.26	0.07	0.26	0.10	0.30	0.10	0.30
Number of states	47		47		48		48		16		16	
Number of state-ZIP/FIPS	20,973		20,973		14,422		14,422		319		319	
Number of applicants (millions)	18.7		2.2		3.6		0.6		5.8		0.8	

Notes: This table presents summary statistics for the bankruptcy, foreclosure (conditional on homeownership), and eviction (conditional on non-homeownership) samples, and within each of these samples for the "full sample" and for the "quasi-experimental sample" used in Section 3. The "bankruptcy sample" consists of disability-program applicants who have an initial decision date in 2000–2009. The "foreclosure sample" consists of disability-program applicants who appear in the deeds records (homeowners) and who have an initial decision date in 2005–2014. The "eviction sample" consists of disability-program applicants who do not appear in the deeds records (non-homeowners) and who have an initial decision in 2005–2014. Samples involving "foreclosure" and "bankruptcy" outcomes exclude ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period; samples involving "eviction" outcomes exclude FIPS county codes of residence at application that have an average of fewer than fifteen recorded events per year during the corresponding period. "Reaching step 5" denotes reaching step 5 of the disability determination process as depicted in Appendix Figure A3. "Pre-decision annual earnings" are average annual earnings in the three years before the decision date. "Ever experience financial event" and "experience event before/after decision" are indicators for filing for bankruptcy, experiencing foreclosure, or experiencing eviction. "Number of states" includes the District of Columbia for the foreclosure sample.

1.3 Sample Statistics

Table 1 presents summary statistics for the bankruptcy, foreclosure, and eviction samples. The first column for each outcome corresponds to the full sample, and the second column to the sample we use for the quasi-experimental analysis in Section 3. The bankruptcy sample includes disability-program applicants who have an initial decision date between 2000 and 2009 and reside in a ZIP Code with an average of at least five recorded bankruptcies per year over the 1992–2011 period covered by the bankruptcy data.[14] The average applicant in this sample has less than a high school education (11.5 years) and annual earnings of $14,300 prior to the initial decision. Thirty-five percent of the sample is allowed at the initial level and 54 percent is eventually allowed after all appeals. Bankruptcy rates are high: 12 percent ever file for bankruptcy between 1992 and 2011, with 10 percent ever filing for Chapter 7 and 2 percent ever filing for Chapter 13.

The foreclosure sample consists of applicants who have an initial decision date between 2005 and 2014 and reside in a ZIP Code with an average of at least five recorded foreclosures over the 2005–2014 period covered by the foreclosure data. Because we condition the foreclosure sample on homeownership, the applicants in the foreclosure sample are more-educated and higher-income than the applicants in the bankruptcy sample. The average applicant in this sample is a high school graduate (12.3 years of education) and average annual pre-decision earnings are $20,800. SSDI applicants are disproportionately represented relative to the bankruptcy sample, and applicants are less likely to have mental conditions and more likely to have musculoskeletal conditions compared to the bankruptcy sample. Foreclosure rates among these home-owning applicants are high: 13 percent of the sample ever experiences a foreclosure between 2004 and 2016.[15]

The eviction sample consists of applicants who do not appear in homeowner records and who apply from the 16 states for which we have eviction records. Average annual pre-decision earnings are lower than in the bankruptcy sample since the eviction sample is conditioned on non-homeownership. Eighteen percent of applicants ever experience eviction over the 2005–2014 period covered by the eviction data.

Table 2 presents summary statistics for the home-sale and home-purchase samples. The home-sale sample is conditioned on homeownership and therefore looks similar to the foreclosure sample. Nearly one-half of home-owning applicants sell a home over the 1986–2015 period for which we have deeds data. The home-purchase sample consists of applicants who

[14] We eliminate ZIP Codes with few bankruptcies, foreclosures, or real-estate transactions from the main analysis sample in order to avoid small cells, invalid ZIP Codes, and areas with poor data coverage.

[15] In the foreclosure sample that is unconditional on homeownership, roughly 3 percent of applicants ever experience foreclosure. Foreclosure rates unconditional on homeownership are substantially lower, since applicants are less likely to be homeowners.

Table 2: Summary Statistics for the Home-Sale and Home-Purchase Samples

	Home sale sample				Home purchase sample			
	Full Sample		Quasi-Exp. Sample		Full Sample		Quasi-Exp. Sample	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Fraction SSI adults	0.31	0.46	0.31	0.46	0.55	0.50	0.53	0.50
Fraction DI adults	0.81	0.40	0.80	0.40	0.63	0.48	0.65	0.48
Fraction reaching step 5	0.69	0.46	1.00	0.00	0.69	0.46	1.00	0.00
Fraction initially allowed	0.43	0.50	0.46	0.50	0.35	0.48	0.41	0.49
Fraction finally allowed	0.63	0.48	0.71	0.45	0.52	0.50	0.63	0.48
Mental condition	0.16	0.37	0.16	0.37	0.26	0.44	0.18	0.39
Musculoskeletal condition	0.37	0.48	0.47	0.50	0.31	0.46	0.43	0.49
Age	50.4	10.2	52.8	2.7	44.8	12.8	52.5	2.7
Male	0.52	0.50	0.55	0.50	0.53	0.50	0.55	0.50
Pre-decision annual earnings	\$22,047	\$22,227	\$22,605	\$22,145	\$13,175	\$17,244	\$14,959	\$18,309
Years of education	12.1	2.4	12.0	2.4	11.6	2.4	11.6	2.4
Ever experience event	0.44	0.50	0.44	0.50	0.18	0.38	0.22	0.41
Experience event before decision	0.17	0.38	0.18	0.39	0.14	0.35	0.17	0.38
Experience event after decision	0.30	0.46	0.28	0.45	0.07	0.26	0.09	0.28
Number of states	49		49		49		49	
Number of state-ZIPs	22,631		22,631		24,094		24,094	
Number of applicants (millions)	6.6		1.1		29.3		3.8	

Notes: This table presents summary statistics for the home-sale and home-purchase samples, and within each sample for the "full sample" and for the "quasi-experimental sample" used in Section 3. The home-sale sample consists of disability-program applicants who appear in the deeds records (homeowners) and who have an initial decision date in 2000–2014. The home-purchase sample consist of disability-program applicants who have an initial decision date in 2000–2014. Each sample excludes ZIP Codes of residence at application that have an average of fewer than fifteen recorded events per year during 2000–2014. "Reaching step 5" denotes reaching step 5 of the disability determination process as depicted in Appendix Figure A3. "Pre-decision annual earnings" are average annual earnings in the three years before the decision date. "Ever experience event" and "experience event before/after decision" are indicator functions for home purchases or sales. "Number of states" includes the District of Columbia.

have an initial decision date between 2005 and 2014 and reside in a ZIP Code with at least fifteen home purchases over the 2000–2015 period. Since it is not conditioned on homeownership, this sample looks more similar to the bankruptcy sample, with relatively low incomes and low education levels. Of this sample, 18 percent of applicants ever purchase a home between 1983 and 2016.

2 Cross-Sectional and Temporal Selection into Disability Application Distress

We document a series of descriptive facts that suggest both cross-sectional and temporal selection of individuals into disability application on the basis of financial distress.

Fact 1: Disability applicants have higher rates of adverse financial events than the general population, meaning that there is cross-sectional selection into disability application.

We first compare rates of adverse financial events in the disability-applicant population to rates of adverse financial events in the general population. Figure 2 plots annual bankruptcy rates, foreclosure rates, and eviction rates for disability-program applicants and the general population. Unconditional rates are given by the opaque bars, and rates conditional on homeownership (for foreclosure) and non-homeownership (for eviction) are given by the translucent bars.

Bankruptcy rates are slightly higher in the disability-applicant population relative to the general population in the year that applicants apply for disability benefits; they are slightly lower three years before application, which could be mechanical since bankruptcy is a rare event. For foreclosure, disability applicants have lower rates when we do not condition on homeownership, since disability applicants are less likely than the general population to be homeowners. Conditioning on homeownership, however, we find that foreclosure rates peak in the year of disability application at twice the rate of the general population. Eviction rates are much higher in the disability-applicant population than in the general population when we do not condition on non-homeownership, especially in the year of the disability application. Conditioning on non-homeowner (renter) status makes the eviction rate in the disability-applicant population and general population comparable.

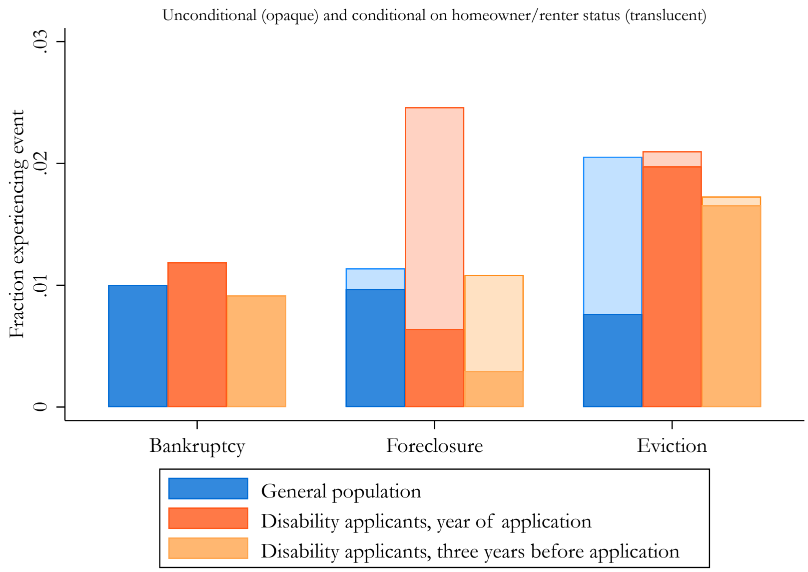
The spike in adverse financial events in the year of application has several possible interpretations: disability applicants may apply for disability benefits in response to a financial shock, or a health shock or other type of shock may lead to both disability application and financial distress. To investigate the timing of financial shocks and disability application more closely, we next estimate event-study regressions around disability application and disability decision.

Fact 2: Rates of adverse financial events exhibit an "Ashenfelter's peak" around the time of disability application, meaning that there is temporal selection into disability application.

In Appendix Section C, we develop the following event-study specification to explore how the risk of bankruptcy, foreclosure, and eviction evolve around the time of disability application and decision:

$$Y_{ct} = \alpha_c + \gamma_t + \sum_{\tau} \beta^d_\tau (\text{Allow}_c \times D^d_{ct}) + \sum_{\tau} \beta'^d_\tau D^d_{ct} + \sum_{\tau} \mu^a_\tau (\text{Allow}_c \times D^a_{ct}) + \sum_{\tau} \mu'^a_\tau D^a_{ct} + \varepsilon_{ct}. \quad (1)$$

Figure 2: Rates of Adverse Financial Events in the General vs. Disability-Applicant Population



Notes: This figure presents bankruptcy, foreclosure, and eviction rates among the general population and the disability-program applicants across different application cohorts. Unconditional rates are in opaque bars, and conditional foreclosure (homeowners) and eviction (non-homeowners) rates are in translucent bars. The unconditional bankruptcy sample consists of disability-program applicants who have an initial decision date in 2000–2009. The unconditional foreclosure and eviction sample consists of disability-program applicants who have an initial decision date in 2005–2014. The conditional foreclosure sample consists of disability-program applicants who appear in the deeds records (homeowners) and who have an initial decision date in 2005–2014. The conditional eviction sample consists of disability-program applicants who are non-homeowners at the time of experiencing evictions and who have an initial decision in 2005–2014. Samples involving bankruptcy and foreclosure outcomes exclude ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period; samples involving eviction outcomes exclude FIPS county codes of residence at application that have an average of fewer than fifteen recorded events per year during 2005–2014. The denominator of the bankruptcy, foreclosure, and eviction rates for the general population is calculated using the 2010 Census population for individuals 18 years or older.

Here, Y_{ct} is a financial outcome for cohort c in month t, where cohort is defined by application month, decision month, and allowance status; D^d_{ct} and D^a_{ct} are event-month indicator functions relative to initial decision date and application date, respectively; and Allow_c is an indicator function for being approved for disability benefits at initial decision. The β'^d_τ coefficients give the financial outcome in initial-decision event time for the denied, controlling for application event time; the sum $\beta^d_\tau + \beta'^d_\tau$ gives this value for the allowed. Similarly, the μ'^a_τ give the financial outcome in application event time for the denied and the sum $\mu^a_\tau + \mu'^a_\tau$ give this value for the allowed, controlling for initial-decision event time.

This specification is a standard event-study specification that we adapt to control for both application event time and decision event time. Since the initial decision usually occurs within a year of application, it is important to separate time trends around the two dates. If, for example, there is selection into the timing of application, we might mis-attribute a pattern that is associated with the application to the decision instead. This strategy exploits variation in examiner decision time to identify the patterns around application and decision separately.

Figure 3 presents the application-event-time coefficients and decision-event-time coefficients from equation (1), with the mean of the outcome at event month 0 added to all event months. For all three adverse events, the application-event-time coefficients (left-hand side of Figure 3) suggest that financial distress peaks around the time of application and then falls, even after controlling for decision event time. In other words, applicants apply for disability benefits after a period of increasing financial distress. It could be that a deterioration in health increases financial distress and drives disability-program application, or that high financial distress drives application. The peak in bankruptcy filings is just after the application date while the peak in foreclosures is a few months later, likely because there are multiple steps between default and foreclosure.[16]

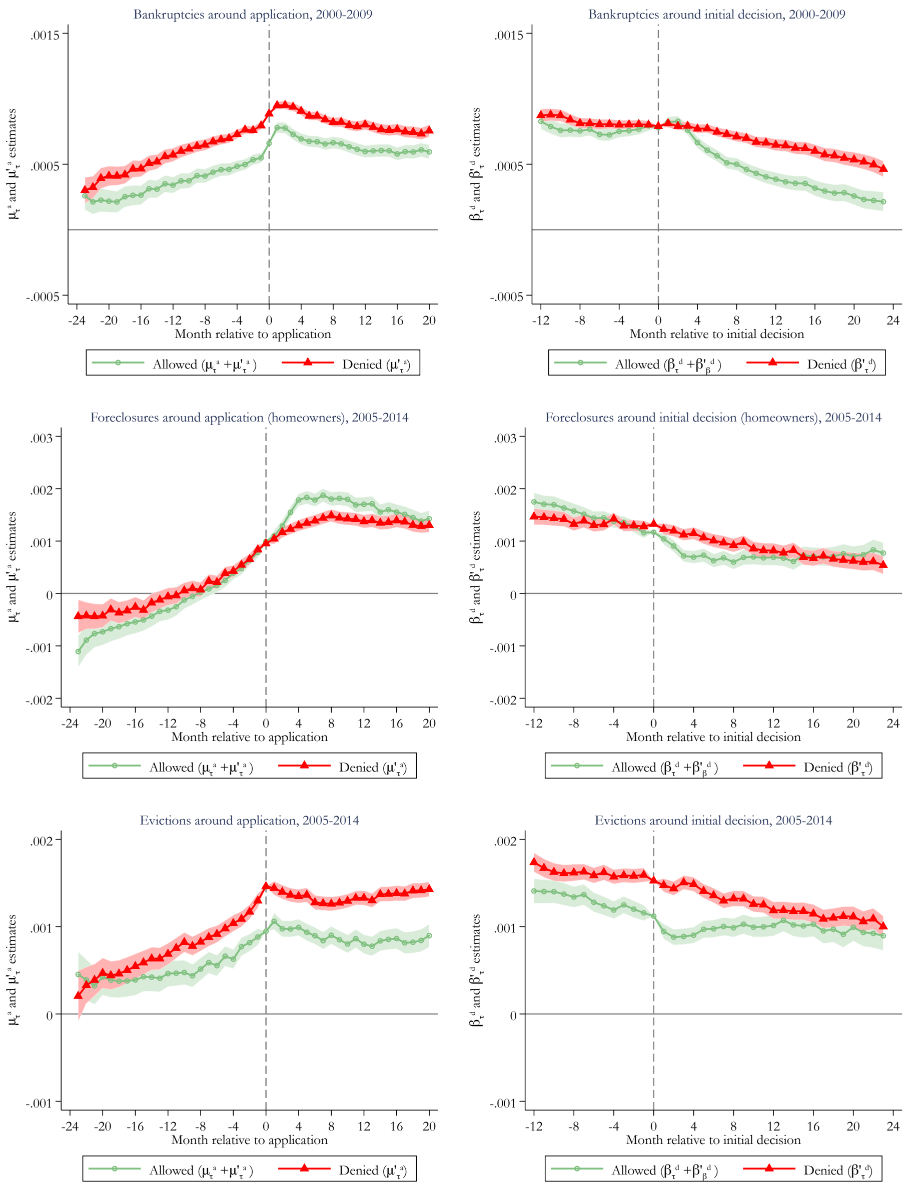
Fact 3: Rates of adverse financial events decline for both allowed and denied applicants after the initial disability decision.

The decision-event-time coefficients (right-hand side of Figure 3) suggest a downward trend in bankruptcies, foreclosures, and evictions for both allowed and denied applicants preceding the decision, controlling for application date. After the initial decision, bankruptcy rates continue falling for the denied, but they decline further for the allowed. This is suggestive evidence that allowance onto disability programs reduces the risk of bankruptcy relative to denials. However, the graph also makes clear that considering only the trend for the allowed would lead to an overestimate of the decline in bankruptcies attributable to disability allowance, since bankruptcy risk also declines for the denied.[17] Denied applicants may find other margins of adjustment that reduce their financial distress following their denial from the program. For foreclosures and evictions, the rates of these events exhibit a sharper drop for the allowed relative to the denied immediately after the initial decision, though the trends eventually converge.

[16] The fall in bankruptcies, foreclosures, and evictions after the application date could reflect households making other adjustments in consumption and saving. Or it could be a mechanical decline if most of the households at risk for these events have already experienced them.

[17] This is true even after controlling for final decision date, in which some of the initially denied are allowed on appeal.

Figure 3: Trends in Adverse Financial Events Around Disability Application and Decision Dates



Notes: These figures plot estimates from the event-study specification in equation (1). The upper-left panel plots application event indicator functions for bankruptcy relative to the month of application, for allowed applicants ($\mu^a_\tau + \mu'^a_\tau$) and denied applicants (μ'^a_τ). Upper-right panel plots initial-decision event indicator functions for bankruptcy relative to the month of decision, for allowed applicants ($\beta^d_\tau + \beta'^d_\tau$) and denied applicants (β'^d_τ). Middle-left and middle-right panels are analogous for foreclosure, and bottom-left and bottom-right graphs for eviction. The bankruptcy sample consists of disability-program applicants who have an initial decision date in 2000–2009. The foreclosure sample consists of disability-program applicants who appear in the deeds records (homeowners) and who have an initial decision date in 2005–2014. The eviction sample consists of disability-program applicants who do not appear in the deeds records (non-homeowners) and who have an initial decision in 2005–2014. Samples involving "foreclosure" and "bankruptcy" outcomes exclude ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period; samples involving "eviction" outcomes exclude FIPS county codes of residence at application that have an average of fewer than fifteen recorded events per year during 2005–2014.

Interpreting the descriptive facts

Where do these descriptive facts lead us? First, they suggest that applicants apply to disability programs when they are facing substantial financial distress, both relative to the general population and relative to their own histories. This peak in financial distress at application means that the cash transfer from disability benefits could potentially produce large reductions in financial distress. Indeed, the trends around disability decision provide suggestive, though not conclusive, evidence that disability programs reduce financial distress. Second, these facts indicate that application timing is non-random and that even denied applicants experience declines in financial distress following their initial decision. Given this evidence of selection and trends in event time, we conclude that estimating the causal effect of disability programs on financial outcomes requires a quasi-experimental strategy. We turn to such a strategy next.

3 Quasi-Experimental Estimates of the Effect of Disability Receipt on Financial Outcomes

To estimate the causal effect of disability programs on financial outcomes, we use age-based variation in eligibility for disability programs.[18] SSA evaluates applicants with a five-step process, described in Appendix Figure A3. During the first two steps, examiners deny applicants if they have engaged in substantial gainful activity since onset of their disability (step 1) or if their impairment is not considered severe (step 2). During step 3, applicants with listed medical impairments are allowed onto the program. During step 4, applicants are denied if the examiner deems that they could still do the work that they had done before the disability onset.

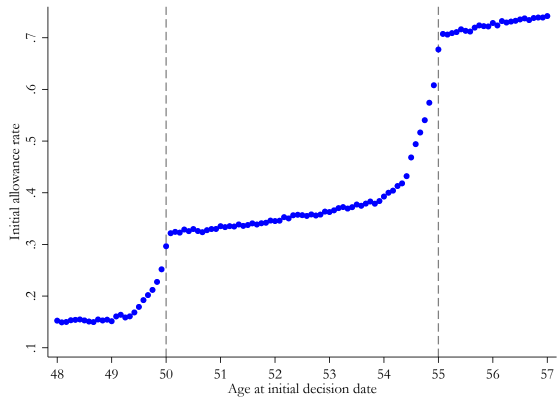
Finally, during step 5, examiners evaluate whether the applicants who cannot do past work can adjust to another type of work. Examiners first determine the individual's maximum work capability (e.g., sedentary, light, heavy, etc.) and then divide applicants in these groups into cells based on age, education, previous work experience, and the nature of their past work.[19] We exploit SSA guidelines instructing disability examiners to use more-lenient standards for applicants who are above ages 50 and 55 relative to those below ages 50 and 55 at step 5.[20]

[18] Chen and van der Klaauw (2008) originally used the age-55 threshold, along with data from the Survey of Income and Program Participation linked to SSA application and award data, to study the effect of receiving disability benefits on labor supply.

[19] The full vocational grid used in this process is available here: http://policy.ssa.gov/poms.nsf/lnx/0425025035 (DI 25025.035).

[20] We use the classification of Wixon and Strand (2013) to map the "regulation basis code" in the F831

Figure 4: Initial Allowance Rate at Step 5 By Age



Notes: This figure plots the initial allowance rate by disability-program applicant age at step 5 of the disability determination process. This figure is based on the home-purchase sample: applicants who reach step 5 of the disability determination process, who have an initial decision date in 2000–2014, and whose ZIP Code of residence at application has an average of at least fifteen recorded home purchases per year during this period.

Figure 4 plots initial allowance rate by age at decision and so describes the age-based variation that we exploit to identify the effects of disability benefits. The initial allowance rate jumps at ages 50 and 55 because examiners are instructed to use the more-lenient standards at these cutoffs.[21] There exists a trend break six months before each cutoff, with increasing initial allowance rates up to the threshold. This trend break is driven by the SSA's "borderline age rule," which allows examiners discretion in applying the more-lenient standards to applicants near the cutoff. The rule tells examiners: "If a claimant is within a few days to a few months of reaching a higher age category and using the chronological age results in a denial, consider using the higher age category if it results in a favorable determination, after you evaluate all factors."[22]

The approximately 130 disability determination services (DDS) offices exercise discretion in implementing the borderline age rule. Some offices ignore the rule entirely and treat applicants in the "borderline" period the same as the other applicants below the cutoff. Other offices fully implement the borderline age rule such that an increasing fraction of applicants in the borderline period are allowed. Still others partially implement the borderline age rule

files into steps. We do not find discontinuities in years of education.

[21]There also exists a threshold at age 45, but in practice the discontinuity in allowance rates at age 45 is close to zero.

[22]SSA guidance (DI 25015.006) does not give a precise cutoff for what constitutes a borderline age, other than that the adjustment not exceed 6 months: "We do not have a more precise programmatic definition for the phrase 'within a few days to a few months.' We define the term 'a few' using its ordinary meaning, a small number. Consider a few days to a few months to mean a period not to exceed six months."

such that there is an increasing fraction of applicants allowed in the borderline period but still a jump in allowance rates at the age cutoff. We take advantage of the variation across offices in the implementation of the borderline age rule in our estimation strategy.

Who are the applicants affected by this quasi-experiment? The least-severe applicants are denied in earlier steps of the process and the most-severe applicants are allowed in earlier steps of the process. As a result, the applicants who are evaluated in step 5 have conditions that do not meet the medical listings but are still potentially severe and often hard-to-verify. Tables 1 and 2 suggest that relative to the full samples, the quasi-experimental samples have higher earnings and are more likely to have a musculoskeletal condition and less likely to have a mental condition.

3.1 Estimation Strategies

The borderline age rule makes this age-based variation similar to but distinct from a standard regression discontinuity design. We deal with this unconventional feature of the quasi-experiment by using three alternative estimation strategies: 1) a standard regression discontinuity design, 2) a "donut" regression discontinuity that excludes applicants with a borderline age, and 3) our preferred strategy—which we call the "office classification" strategy—that exploits heterogeneity across DDS offices in their implementation of the borderline age rule. We find that the three strategies lead to nearly identical point estimates, with the office classification yielding the most precise estimates (see Appendix Figure A9).

For the standard regression discontinuity design, we present estimates from an RD specification that stacks the age-50 and age-55 cutoffs:

$$Y_i = \alpha + \beta \mathbb{1}\{\text{Age}_i > 0\} + \gamma \text{Age}_i + \delta \mathbb{1}\{\text{Age}_i > 0\} \times \text{Age}_i + \varepsilon_i. \tag{2}$$

In this specification, Y_i is an outcome for applicant i, Age_i is the applicant's age at decision, and $\mathbb{1}\{\text{Age}_i > 0\}$ is an indicator for applicant's age at decision being greater than the age cutoff (either 50 or 55 years, normalized to zero).

For the "donut" regression discontinuity, we exclude applicants with a borderline age. Since the applicants in the five months before the cutoff are partially treated, the donut specification drops them from the estimation of equation (2).

For the "office classification" strategy, we exploit the heterogeneity across DDS offices in their implementation of the borderline age rule. We classify DDS offices into three types based on how they implement the borderline age rule. Figure 5 presents examples of each type. "RD offices" are offices that ignore the borderline age rule entirely; we name them as such because a plot of initial allowance rates for those offices looks like a typical RD. "Spline offices" are offices that fully implement the borderline age rule such that there is no discontinuity at the cutoff at all, only trend breaks six months before the cutoff and at the cutoff. "Hybrid offices" are offices that partially implement the borderline age rule: there is a trend break in the initial allowance rate six months before the cutoff, but also a jump at the cutoff.[23] Appendix E discusses different ways of classifying offices and demonstrates that the results are robust to alternative classification methods.

In Appendix D, we start with standard regression specifications for each type of office: RD, Spline, and Hybrid. We then develop the following main specification for the endogenous variable and financial outcome by combining the specifications for each office type:

$$\begin{aligned} Y_i \quad = \quad & \beta_0 + \sum_{\substack{j\in\{\text{TypeRD},\\ \text{TypeHybrid}\}}} \sum_{T\in\{50,55\}} \beta_{\text{RD}_{j,T}} \mathbb{1}\{\text{Age}_i > T\} \times \text{Type } j_i + \sum_{T\in\{50,55\}} \beta_{2,T}\text{Age}_i \\ & + \sum_{\substack{j\in\{\text{TypeSpline},\\ \text{TypeHybrid}\}}} \sum_{T\in\{50,55\}} \beta_{\text{Spline1}_{j,T}} \text{Age}_i \times \mathbb{1}\{\text{Age}_i > T-6\} \times \text{Type } j_i \\ & + \sum_{\substack{j\in\{\text{TypeSpline},\\ \text{TypeHybrid}\}}} \sum_{T\in\{50,55\}} \beta_{\text{Spline2}_{j,T}} \text{Age}_i \times \mathbb{1}\{\text{Age}_i > T\} \times \text{Type } j_i \\ & + \sum_{T\in\{50,55\}} \beta_{5,T}\text{Age}_i \times \mathbb{1}\{\text{Age}_i > T\} \times \text{TypeRD}_i + \varepsilon_i, \end{aligned} \tag{3}$$

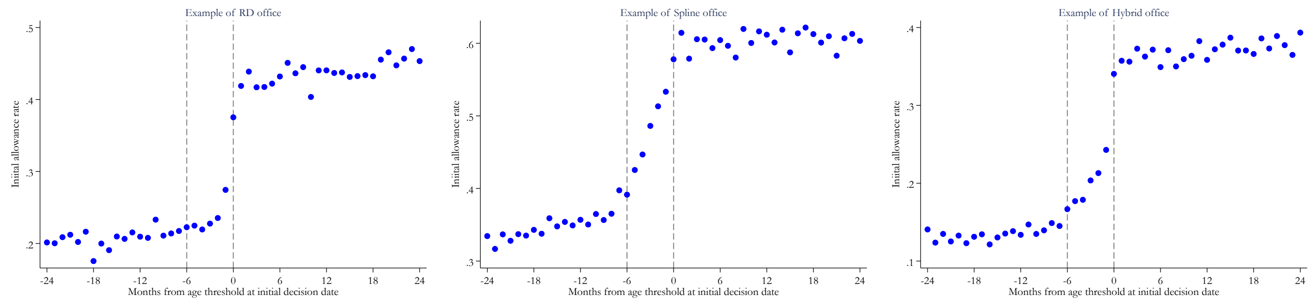
where Y_i is a financial outcome for applicant i, Age_i is the applicant's age at decision relative to age $T \in \{50, 55\}$, $\mathbb{1}\{\text{Age}_i > T\}$ is an indicator for being above than the age cutoff at the decision date, and $\mathbb{1}\{\text{Age}_i > T - 6\}$ is an indicator for being above the threshold six months before the age cutoff. The coefficients $\beta_{\text{RD}_{j,T}}$ give the effect on financial outcomes of being above the age T cutoff for type $j \in \{\text{TypeRD}, \text{TypeHybrid}\}$, like standard RD coefficients of interest. The coefficients $\beta_{\text{Spline2}_{j,T}}$ give the effect on the trend in financial outcomes of being above the age T cutoff for office type $j \in \{\text{TypeSpline}, \text{TypeHybrid}\}$, like standard regression kink coefficients of interest. The coefficients $\beta_{\text{Spline1}_{j,T}}$ measure the kink at the minus-six-month cutoff. We use the variables corresponding to the coefficients $\beta_{\text{RD}_{j,T}}$, $\beta_{\text{Spline1}_{j,T}}$, and $\beta_{\text{Spline2}_{j,T}}$ as instruments in the IV estimation.

3.2 Tests of Validity

In a standard RD design, the identifying assumption requires that assignment to treatment is as good as random around the threshold. This assumption could be violated if some applicants strategically wait until age 50 or 55 to apply, or if there is differential sorting for other reasons. We follow the standard approach and test for a discontinuity in the density of applicants and in applicant covariates across the age thresholds (McCrary, 2008).

[23] "RD offices" make up 20 percent of total offices, and so RD estimates based solely on those offices are imprecise.

Figure 5: Examples of RD, Spline, and Hybrid Offices



Notes: These figures plot initial allowance rates at step 5 of the disability determination process relative to the disability-program applicant's age at the initial decision date for specific DDS offices. The left-hand-side graph is an example of an RD office; the middle graph is an example of a Spline office; and the right-hand-side graph is an example of a Hybrid office. Age is calculated as months from age 50 or age 55, whichever threshold is closer. These figures are based on all disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2014.

Figure 6 plots the number of applicants by age relative to the nearest age threshold by age at initial decision (left-hand panel), which is the running variable. The number of applicants jumps by roughly 3 percent at the age 50 and age 55 thresholds.[24] The right-hand panel plots age at application, since applicants have more control over when they apply than when their case is decided. The discontinuities in the application date densities are less obvious, which suggests that applicants themselves do not defer applications until they reach age 50 or 55. Although the discontinuity in age at initial decision is small, it suggests that DDS examiners might defer the decision date for some cases until after the age threshold.[25]

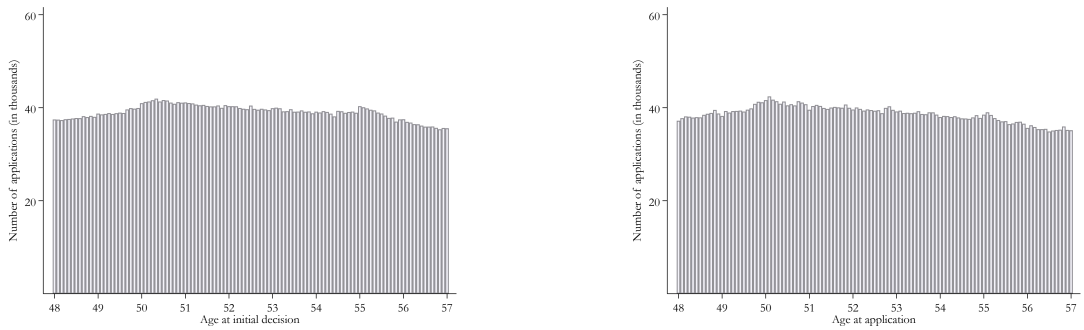
Manipulation by either applicants or examiners is a concern to the extent that different types of applicants end up on either side of the cutoff. If examiners are responsible for deferring some applications, the relevant question for interpreting our results is whether they are selectively deferring applications on the basis of financial distress or something correlated with financial distress. In particular, to explain away the main results, the examiners would have to be selectively deferring the applications of individuals who are *less* likely to experience financial distress in the future. This is unlikely since examiners observe neither earnings nor measures of financial distress, and never communicate directly with the applicant.

Nevertheless, we perform several tests of validity. We test for discontinuities in appli-

[24]Chen and van der Klaauw (2008) study a sample of approximately 1,000 applicants from the 1990s and find that the standard RD assumptions are satisfied—they estimate no discontinuous change in the density of applicants or applicants' covariates in their sample. By contrast, we detect violations of the standard RD assumptions in our sample, which includes the several million applicants that reach step 5 between 2000 and 2014.

[25]We also perform the formal test designed by McCrary (2008) and find a log discontinuity in the density that is 0.472 with standard error 0.00424 for age 50 and 0.483 with standard error 0.00433 for age 55.

Figure 6: Histograms of Age at Initial Decision and Application at Step 5



Notes: These figures present histograms of age at initial decision (left panel) and application (right panel) for disability-program applicants in the home-purchase sample: applicants who reach step 5 of the disability determination process, who have an initial decision date in 2000–2014, and whose ZIP Code of residence at application has an average of at least fifteen recorded home purchases per year during this period.

cant covariates using equation (2) for the combined age-50 and age-55 samples. Table 3 reports discontinuities in applicant characteristics across the age-50 and age-55 cutoffs for the bankruptcy and foreclosure samples.[26] There is no discontinuity in the rate of bankruptcy or foreclosure prior to application. There are statistically significant but economically small discontinuities in other variables. Applicants above the age-50 and age-55 cutoffs have annual earnings $250 higher (1.5 percent) than those below the age-50 and age-55 cutoffs, have 0.03 fewer (0.2 percent) years of education, and are 0.3 percentage points (0.8 percent) more likely to apply with a musculoskeletal condition and 0.4 percentage points (2.4 percent) less likely to apply with a mental condition. These estimates suggest that applicants whose applications are deferred until ages 50 or 55 are not a random sample, but they differ only slightly on observable dimensions compared to those applications that are not delayed.

In principle, differences in unobservables could bias RD estimates of the effect of disability benefits on financial outcomes: discontinuities in financial outcomes at the age thresholds might be driven not by the effects of disability programs, but by selection into which applicants defer their application past the age thresholds. Although the magnitude of the discontinuities is small, we probe the direction and magnitude of the potential bias in several ways. All of the exercises suggest that the bias is likely to be small.

First, we predict adverse financial events prior to application for each applicant based on pre-determined characteristics. We test for a discontinuity in the predicted adverse event at the age thresholds, with the results at the bottom of Table 3. For both the bankruptcy and

[26] Appendix Table A5 and A6 present covariate-balance tests for the eviction, net-home-sale and net-home-purchase samples.

Table 3: Covariate Balance Test – Bankruptcy and Foreclosure Samples

	Bankruptcy sample			Foreclosure sample		
	Pt. Est. (Std. Err.)	Mean	% of mean	Pt. Est. (Std. Err.)	Mean	% of mean
Covariate						
Pre-application adverse financial event	-0.00123	0.110	-1.1%	-0.00216	0.073	-3.0%
	(0.000912)			(0.00137)		
Pre-app earnings	251.7***	$16,557	1.5%	300.0***	$20,481	1.5%
	(56.18)			(108.3)		
Years of education	-0.0261***	11.5	-0.2%	-0.0126	12.1	-0.1%
	(0.00794)			(0.0124)		
Musculoskeletal	0.00322**	0.410	0.8%	0.00650**	0.471	1.4%
	(0.00143)			(0.00265)		
Respiratory	0.00115**	0.039	2.9%	-0.000835	0.036	-2.3%
	(0.000578)			(0.000994)		
Cardiovascular	0.00116	0.109	1.1%	0.000485	0.089	0.5%
	(0.000922)			(0.00152)		
Endocrine	-0.00125**	0.047	-2.6%	-0.00360***	0.036	-10.1%
	(0.000626)			(0.000978)		
Neurological	0.00149**	0.064	2.3%	0.00220	0.076	2.9%
	(0.000718)			(0.00141)		
Mental	-0.00449***	0.187	-2.4%	-0.00361*	0.157	-2.3%
	(0.00114)			(0.00193)		
Special/other	-0.00120*	0.060	-2.0%	0.000256	0.047	0.5%
	(0.000691)			(0.00113)		
p-value on joint F-test		0.000			0.000	
Predicted adverse financial event occurs	0.000245***	0.107	0.2%	-9.97e-05*	0.071	-0.1%
	(8.13e-05)			(5.43e-05)		
R^2 of prediction regression		0.0342			0.0362	
N (in millions)		2.22			0.60	

Notes: This table reports reduced-form estimates for the listed covariates for the bankruptcy and foreclosure samples, where we put the covariate on the left-hand-side of the RD specification in equation (2) and report β with standard errors in parentheses. The table reports the p-value on the F test for the joint significance of all covariates. Pre-application earnings are average annual applicant earnings in the three years prior to the year of application, from the Master Earnings File. Years of education is self-reported years of education from the 831 Disability File. Body system codes (musculoskeletal, respiratory, cardiovascular, endocrine, neurological, mental, special/other) come from the 831 Disability File. "% of mean" denotes point estimate as a percent of control mean, where control means are the average value of the variable for applicants who are under age 50 or 55 by 6 to 10 months. For "predicted adverse financial outcome," we first regress an indicator for having the adverse financial outcome prior to the initial decision date on a set of covariates (pre-application earnings, years of education, male, body system code dummies, and ZIP dummies). We then put "predicted adverse financial outcome" on the left-hand-side of the RD specification in equation (2) and report estimates of β. The bankruptcy sample consists of disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2009. The foreclosure sample consists of disability-program applicants who reach step 5 of the disability determination process, who appear in the deeds records (homeowners), and who have an initial decision date in 2005–2014. Each sample excludes ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

foreclosure samples, we find that the discontinuities in the predicted outcomes are statistically significant but economically small, at just 0.2 and 0.1 percent of the means, respectively, and have the opposite signs.[27] Second, controlling for applicant characteristics does not change the results (Table A13). Third, as described below, we find no evidence of an effect of the thresholds on outcomes *before* the date of application (Appendix Figure A5). Were the effects to be driven by applicants who delay their application until the nearest age threshold, we would expect to observe differences at the threshold in pre-application outcomes, as well.

Another potential confounder is differential mortality. If individuals on one side of the cutoff are more likely to die, then they are mechanically less likely to appear in financial records. This might lead us to conclude, erroneously, that rates of financial distress are lower on the side of the cutoff with higher mortality. The existing research on income receipt and mortality, however, suggests that such a bias would work in the direction opposite of our findings. Gelber et al. (2018) find that higher levels of disability-program benefits reduces mortality. Similarly, Evans and Moore (2011) find that short-run mortality increases in the general population after the receipt of income. If those findings hold for this population, then the findings imply that individuals just above the age threshold would be less likely to die than those just below. This would bias us in the direction of finding a positive effect of disability allowance on adverse financial outcomes. We instead find a negative effect, implying that our estimate is a lower bound for the true effect.

These exercises suggest that *static* selection is small: fixed-in-time differences between those who delay application and those who do not are unlikely to be driving the overall effect. But these exercises do not rule out a more-dynamic type of selection. It could be the case that applicants whose applications are deferred may differ based on characteristics that change around the date of disability application.

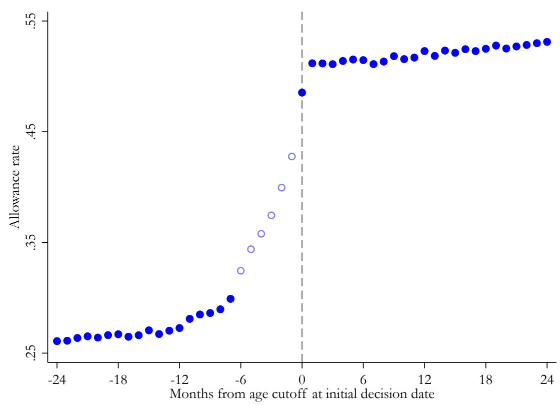
We perform two exercises to test for dynamic selection. First, we determine how different the "bunchers" would have to be from the general applicant pool to explain our findings. The details of these calculations are presented in Appendix F. We find that applicants whose applications are deferred until after the age thresholds must have extremely different (in some cases, impossibly different) rates of financial distress after application in order to explain away our results. For bankruptcy and foreclosure, even if the rates of bankruptcy and foreclosure among bunchers were *zero*, the reduced-form estimates would still suggest that disability programs reduce these adverse financial events. For home sales, we find that bunchers would have to have future-net-home-sale rates that are, at most, one-third of the overall mean. For home purchases, bunchers would have to exhibit net-home-purchase rates

[27] To be sure, these prediction regressions have low values of R^2.

that are at least twice the overall mean.[28]

Finally, Appendix Table A14 presents an additional falsification check for dynamic selection. When individuals apply for disability programs, they may not know at what step of the determination process their case will be decided. Indeed, we observe increases in the density of applicants at the thresholds even when we restrict the sample to applicants whose case was decided in steps one through four. Yet Appendix Table A14 suggests no discontinuity in post-application outcomes for those applicants whose case was decided in steps one through four. This finding is reassuring, because if applicants whose decisions are deferred until they reach certain age thresholds were to have very different outcomes, we would observe a discontinuity even for those applicants who were denied before Step 5. And yet, we observe no such thing.[29] Together, these tests give us confidence that the main results are not explained by either static or dynamic selection.

Figure 7: Initial Allowance Rate at Step 5 Relative to Applicant Age



Notes: Figure plots initial allowance rate at step 5 of the disability determination process relative to the disability-program applicant's age at the initial decision date for applicants in the home-purchase sample: applicants who reach step 5 of the disability determination process, who have an initial decision date in 2000–2014, and whose ZIP Code of residence at application has an average of at least fifteen recorded home purchases per year during this period. Age is calculated as months from age 50 or age 55, whichever threshold is closer.

3.3 Visual Evidence Based on the Standard and Donut RD Strategies

We first present visual evidence based on the standard and donut RD strategies, starting with initial allowance and moving to the financial outcomes of interest. Figure 7 combines

[28]We also implement an empirical strategy designed by Gerard et al. (2018) for regression discontinuity designs with manipulated running variables. As shown in Table A15, the set estimates exclude zero for bankruptcy and foreclosure, but include zero for net home sales and net home purchases.

[29]The 5-year estimate for net home sales is statistically significant at the 5-percent level, but in the opposite direction as the main estimates. The 1-year and 3-year estimates are not statistically significant.

the age-50 and age-55 thresholds and plots the average initial-allowance rate against age in months relative to the nearest age threshold. The share of applicants initially allowed onto disability programs jumps by about 20 percentage points from 6 months before the cutoff to immediately after the cutoff. The six months leading up to the cutoff, shown in hollow markers, reflect the borderline age rule. The jump in final allowance rate, shown in Appendix Figure A4, is smaller, about 7 percentage points.

Figure 8 presents the reduced-form pattern for foreclosure, bankruptcy, net home purchase, and net home sale within the three years after the decision.[30] The lines are fitted using a "donut" strategy, excluding the hollow markers that correspond to the borderline age period. There is a clear decrease in the foreclosure rate across the cutoff, and a clear increase in the home purchase rate. Although the bankruptcy and net-home-sale graphs are noisier, there also appears to be a decrease in these adverse financial events across the cutoff. As described by Lusardi et al. (2011), the sale of a home is one of the main coping mechanisms to which households turn when facing a financial shortfall. All four discontinuities thus suggest a decrease in financial distress: fewer adverse events and an increase in the likelihood of purchasing a new home.[31]

Finally, Appendix Figure A8 presents the reduced-form pattern for earnings in the three years after the initial decision. The graph is a mirror image of the first stage, with earnings declining before the cutoff, dropping at six months before the cutoff, and falling further at the cutoff. This pattern is consistent with the findings of Chen and van der Klaauw (2008), who find that labor supply decreases at the age thresholds. There is no apparent discontinuity in earnings at the cutoff before the initial decision.

3.4 Reduced-Form and IV Estimates Across Estimation Strategies

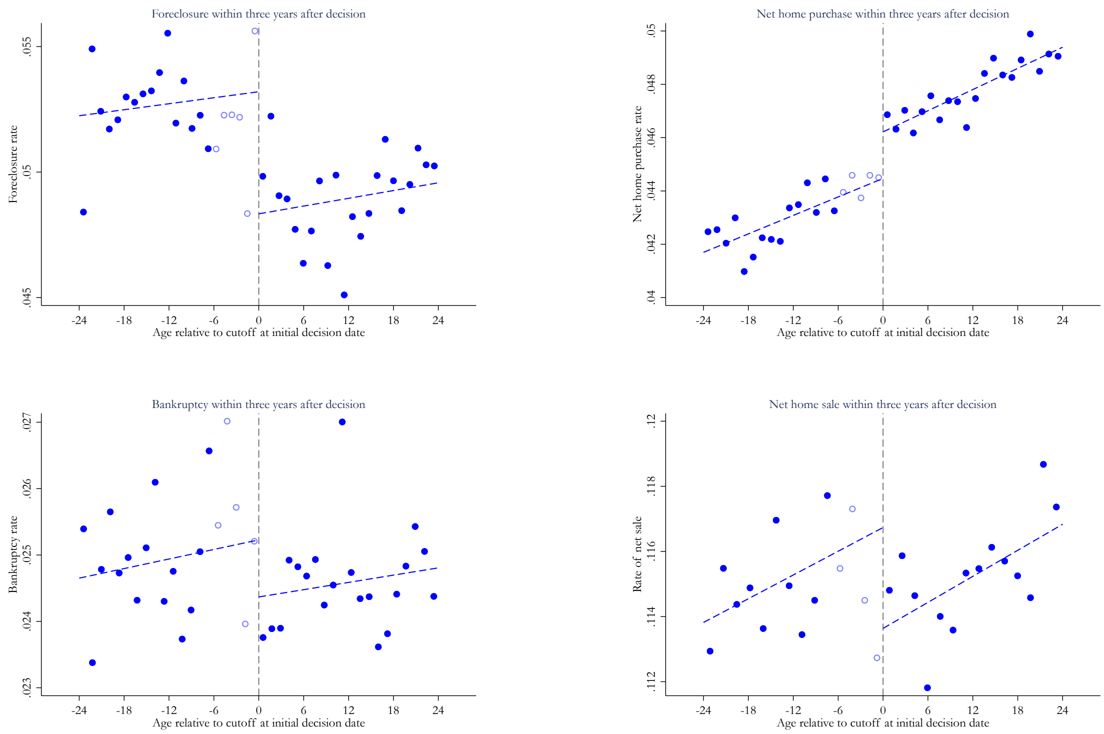
We use the three estimation strategies discussed above—standard RD, donut RD, and the office classification strategy—and find that they produce similar estimates. This section presents results based on the office classification strategy. Appendix Tables A10 and A11 present estimates for the standard RD and donut RD strategies. Appendix Figure A9 shows that the point estimates across the three estimation strategies are almost identical, though

[30]We define a "net" home sale as a home sale that is not accompanied by a home purchase within six months before or after the sale, and analogously for net home purchase. By limiting to "net" home sales, we are less likely to pick up moves, which are difficult to interpret normatively, and more likely to pick up distressed sales. A drawback to this approach is that "net" sales and purchases are more prone to bias than other outcomes as a result of unobserved transactions in ZIP Codes other than the application ZIP Code. Appendix B discusses this bias. Appendix Figure A7 shows the reduced-form pattern for "gross" home sales and home purchases.

[31]Appendix Figure A6 presents the analogous figures for eviction. The estimates for eviction in Appendix Table A12 are too imprecise to be meaningful.

the estimates are less precise for the standard RD and donut RD strategies.

Figure 8: Bankruptcy, Foreclosure, Net Home-Sale, and Net Home-Purchase Rates Relative to Applicant Age



Notes: These figures plot the bankruptcy and foreclosure rates within three years after initial decision (left-hand side panel) and before initial decision (right-hand side panel) relative to the disability-program applicant's age at the initial decision date. Age is calculated as months from age 50 or age 55, whichever threshold is closer. Figures are based on quantile spaced binning, allowing each bin to have the same number of observations. Dashed lines are fitted using a "donut" strategy, excluding the hollow markers that correspond to the borderline age period. The bankruptcy sample consists of disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2009. The foreclosure sample consists of disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2005–2014. The "home-sale sample" and "home-purchase sample" consist of disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2000–2014. Each sample excludes ZIP Codes of residence at application that have an average of fewer than fifteen recorded events per year during the corresponding period. Samples involving "foreclosure" and "bankruptcy" outcomes exclude ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period; samples involving "net home sale" and "net home purchase" outcomes exclude ZIP Codes of residence at application that have an average of fewer than fifteen recorded events per year during the corresponding period.

The first-stage and reduced-form estimates for the office classification strategy, equation (3), are presented in Appendix Tables A7, A8, and A9. These results are consistent with the visual patterns in the corresponding graphs. The probability of initial allowance jumps by 15–22 percentage points for RD offices and roughly 7 percentage points for Hybrid offices at the age-50 cutoff. The jumps are slightly higher at the age-55 cutoff: roughly 16 percentage points for RD offices and 7–10 percentage points for Hybrid offices. For RD and Hybrid offices, the trend break at the minus-six-month cutoff is positive and at the zero cutoff is negative, as we would expect from the first stage figures. The estimates for final allowance have the same sign but are smaller in magnitude. The joint F-test on the $\beta_{\text{RD}_{j,T}}$, $\beta_{\text{Spline1}_{j,T}}$, and $\beta_{\text{Spline2}_{j,T}}$ coefficients of interest yields a p-value of less than 0.0001, indicating strong instruments.

For bankruptcy, foreclosure, and home sale, the reduced-form estimates largely have the expected sign, the opposite of the first-stage sign. For example, at both age cutoffs, bankruptcy rates decrease when the initial allowance rate jumps at the cutoff for RD and Hybrid offices; they decrease when the initial allowance rate trend increases at the minus-six-month cutoff for Spline and Hybrid offices; and they increase when the initial allowance rate trend drops at the zero cutoff for Spline and Hybrid offices. Although some reduced-form estimates are individually statistically significant at conventional levels, most are imprecise on their own.

Table 4 presents the IV estimates for the office classification strategy using the two-stage-least-squares estimator with initial allowance rate as the endogenous variable.[32] We find that initial disability allowance reduces the bankruptcy rate by a statistically significant 0.77 percentage point (31 percent). Among homeowners, the likelihood of experiencing foreclosure falls by 1.8 percentage points (34 percent) and the likelihood of a net home sale falls by 1.8 percentage points (15 percent) within three years. These estimates are statistically significant. Net home purchases increase by 0.60 percentage point (14 percent) within three years.

To verify that selection does not drive these results, Table 4 presents falsification tests for the same outcomes in the years *before* the initial decision. As we would expect from the falsification graphs in Appendix Figure A5, none of these pre-decision estimates are statistically significant. We also show that the estimates do not change when we control for applicant characteristics (see Table A13).

In order to recover an IV estimate, we treat initial allowance as the endogenous variable of interest. There are, however, other ways in which we could specify the first stage. For instance, we could specify the endogenous variable as final allowance onto disability programs or the number of months on the program. We focus instead on initial allowance because it captures both the allowance itself and also the timing of allowance. Figure 3 suggests that the financial distress of applicants peaks very close to the date of their application. For

[32]Appendix Table A11 presents analogous estimates using the standard RD specification, equation (2). The estimates in that table are similar though less precise.

applicants in severe financial distress, we would expect initial allowance to have a larger effect on consumption than allowance on appeal months later.[33] Indeed, Table 4 suggests that most of the effect we observe appears in the first year after the initial decision.

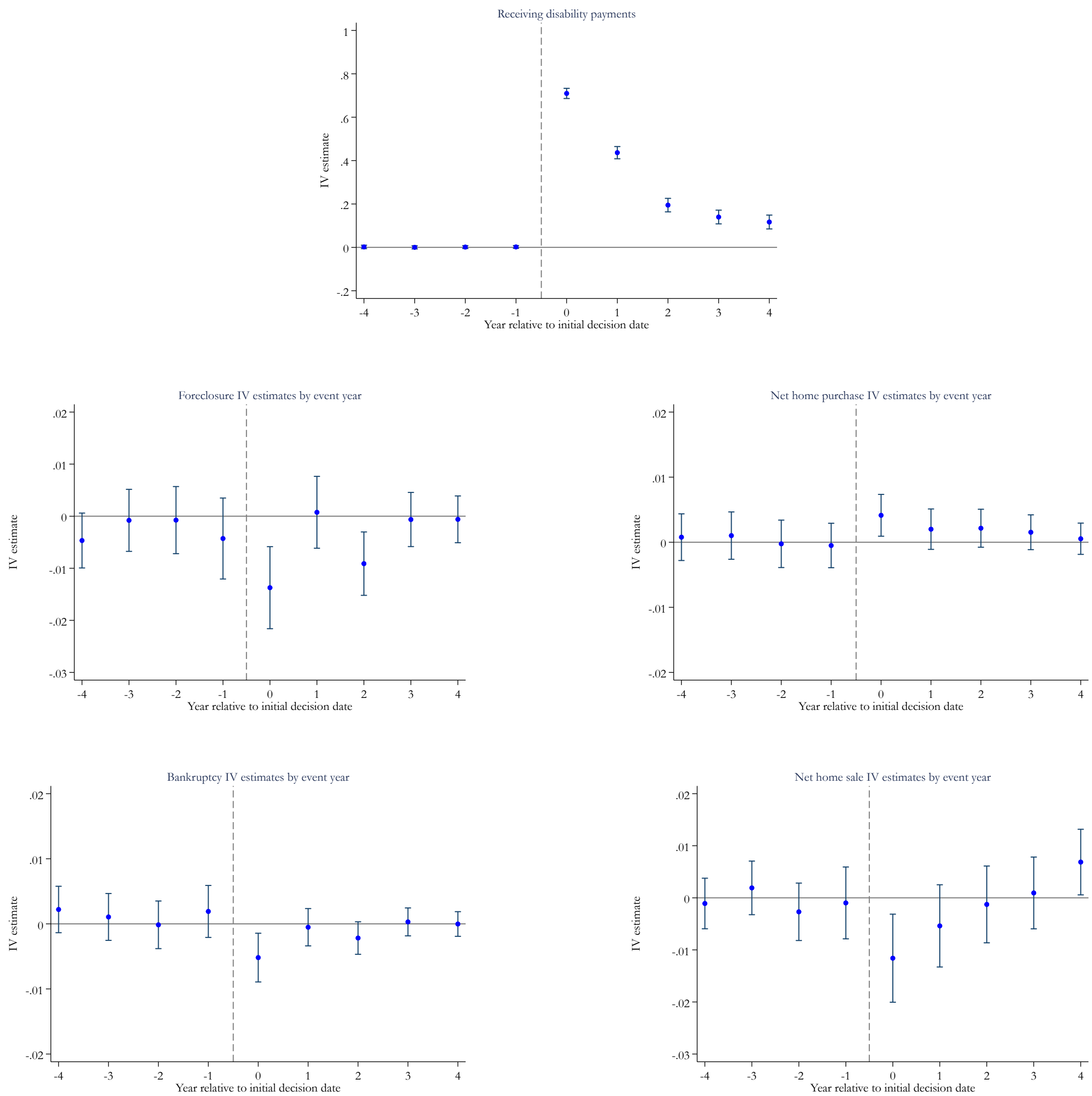
Table 4: Instrumental Variable Estimates of the Effect of Initial Disability Allowance

	After initial allowance			Before initial allowance			
	Within 1 year Pt. Est. (Std. Err.) [Cntrl. Mean]	Within 3 years Pt. Est. (Std. Err.) [Cntrl. Mean]	Within 5 years Pt. Est. (Std. Err.) [Cntrl. Mean]	Within 1 year Pt. Est. (Std. Err.) [Cntrl. Mean]	Within 3 years Pt. Est. (Std. Err.) [Cntrl. Mean]	Within 5 years Pt. Est. (Std. Err.) [Cntrl. Mean]	N (in millions)
Bankruptcy	-0.00496***	-0.00773***	-0.00744**	0.00178	0.00210	0.00301	2.22
	(0.00191)	(0.00268)	(0.00303)	(0.00203)	(0.00325)	(0.00402)	
	[0.0123]	[0.0251]	[0.0323]	[0.0132]	[0.0361]	[0.0573]	
Foreclosure (conditional on homeownership)							
	-0.0134***	-0.0175***	-0.0179***	-0.00396	-0.00282	-0.00585	0.60
	(0.00401)	(0.00566)	(0.00630)	(0.00396)	(0.00549)	(0.00616)	
	[0.0251]	[0.0518]	[0.0647]	[0.0229]	[0.0463]	[0.0599]	
Net home sale (conditional on homeownership)							
	-0.0113***	-0.0175***	-0.0111	-0.00106	-0.00240	-0.00147	1.06
	(0.00430)	(0.00659)	(0.00765)	(0.00352)	(0.00500)	(0.00581)	
	[0.0452]	[0.115]	[0.164]	[0.0292]	[0.0624]	[0.0876]	
Net home purchase							
	0.00392**	0.00605**	0.00726**	-0.000511	0.00216	0.00475	3.82
	(0.00163)	(0.00252)	(0.00290)	(0.00174)	(0.00289)	(0.00349)	
	[0.0176]	[0.0438]	[0.0595]	[0.0202]	[0.0595]	[0.0892]	

Notes: This table reports instrumental-variable estimates of the effect of disability benefits on financial outcomes. The "bankruptcy" regressions are based on the bankruptcy sample: disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2009. The "foreclosure" regressions are based on the foreclosure sample: disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2005–2014. The "net home-sale" regressions are based on the home-sale sample: disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2000–2014. The "net home-purchase" regressions are based on the home-purchase sample: disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2014. A "net" home sale is defined as a home sale that is not accompanied by a home purchase within six months before or after the sale, and analogously for net home purchase. Samples involving "foreclosure" and "bankruptcy" outcomes exclude ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period; samples involving "net home-sale" or "net home-purchase" outcomes exclude ZIP Codes of residence at application that have an average of fewer than fifteen recorded corresponding events per year during 2000–2014. Standard errors in parentheses; control means in square brackets are the average value of the variable for applicants who are under age 50 or 55 by 6 to 10 months. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

[33] Alternatively, we would focus on final allowance as the endogenous variable of interest if only allowance itself, and not the timing of the allowance, matter for outcomes. Scaling the reduced-form estimates by final allowance would result in IV estimates that are often larger than the control mean, though the confidence intervals include reductions smaller than the control mean.

Figure 9: Instrumental Variable Estimates by Event Year



Notes: Figures present instrumental variable estimates of the effect of disability-program allowance on financial outcomes by event year around the initial decision date. The top graph plots estimates of the likelihood of receiving disability benefits in each event year. This sample is the same as the sample for "bankruptcy": disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2009. The "foreclosure" regressions are based on the foreclosure sample: disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2005–2014. The "net home-purchase" regressions are based on the home-purchase sample: disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2014. The "net home-sale" regressions are based on the home-sale sample: disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2000–2014. A "net" home sale is defined as a home sale that is not accompanied by a home purchase within six months before or after the sale, and analogously for net home purchase. samples involving "net home-sale" or "net home-purchase" outcomes exclude ZIP Codes of residence at application that have an average of fewer than fifteen recorded corresponding events per year during 2000–2014.

Next, Figure 9 explores the pattern of the IV estimates over time.[34] The top panel shows the likelihood of receiving disability benefits in each event year. As expected, there is minimal difference between those just below the age thresholds and those just above the age thresholds in the likelihood of receiving disability benefits before the initial decision. In the year of the decision, we observe a 70-percentage-point difference in the likelihood of receiving disability benefits across the age thresholds. The likelihood of receiving benefits then attenuates rapidly, falling to about 40 percentage points the year after the decision and eventually stabilizing around 15 percentage points. This attenuation is driven by two phenomena: denied applicants reapplying for benefits in later years and then being allowed onto the programs (particularly for SSDI), and allowed applicants leaving the program in future years (particularly for SSI, see Appendix Figure A10).

The pattern of attenuation in the top panel of Figure 9 is important in interpreting our main results. The figure reveals that the "treatment" coming from this quasi-experiment is not the effect of receiving disability benefits indefinitely, but rather the effect of receiving disability benefits earlier than the applicant otherwise would have. On average, including the zeros, applicants above the age cutoff receive disability benefits for 0.9 additional months relative to those just below the cutoff. The effects that we estimate may still reflect the *belief* on the part of the applicant that they will receive disability benefits indefinitely, but the effects are expected to dissipate mechanically over time as the first stage attenuates.

The remaining panels of Figure 9 present IV estimates by event year for foreclosure, net home purchase, bankruptcy, and net home sale. Those outcomes are relatively infrequent events, and so it is unsurprising that the individual-year estimates have large confidence intervals. Nevertheless, the time path of the treatment effects generally mirrors the first stage: no treatment effect before the year of the initial decision, an immediate effect in the year of the initial decision, and then dissipating effects after. The estimates are too imprecise to determine whether the effects dissipate because of attenuation of the first stage or because of actual changes in the treatment effect.

Finally, we consider heterogeneity in the IV estimates. Table 5 presents IV estimates by education, gender, and program (SSDI or SSI). The table suggests strong bankruptcy effects for women: a decline of 1.3 percentage points, compared to 0.4 percentage points for men, within three years. By contrast, the effects for foreclosure, home sale, and home purchase are stronger for men than for women, and stronger for DI applicants as compared to SSI applicants. Since DI applicants have higher incomes than SSI applicants and are more likely to be homeowners, they may be more likely to be on the margin of experiencing foreclosure,

[34] These results use the same specification as the main IV estimates, but with a separate regression for each event year.

selling a home, or purchasing a home.

4 Understanding the Channels through which Disability Benefits Affect Financial Outcomes

We find that initial disability allowance leads to large reductions in bankruptcies, foreclosures, and home sales. In order to assess the implications of these results for recipients' welfare, we must consider the mechanisms through which disability benefits affect household financial outcomes. One possible channel is a wealth effect: disability programs relax the recipient's budget constraint by increasing income, reducing income volatility, and providing access to health insurance. If the reduced-form results reflect primarily a wealth channel, then we can interpret the reductions in bankruptcy and foreclosure as reductions in financial distress and therefore as improvements in recipient welfare.[35]

There are, however, alternative mechanisms through which disability benefits might affect financial outcomes, and those mechanisms have more-ambiguous implications for recipients' welfare. For example, if disability benefits change access to credit or demand for credit, then benefits could affect bankruptcy rates and foreclosure rates mechanically by changing either the number of disability recipients who use credit or the amount of credit they use. We discuss these alternative mechanisms and the expected direction and magnitude of their effects. A combination of empirical evidence and institutional details suggests that a wealth effect is the most likely channel through which disability benefits affect financial outcomes. If so, we can interpret the reduced-form results as a reduction in financial distress and an improvement in recipient welfare.

Credit access and credit demand. Disability benefits could affect either the supply of credit or demand for credit. On the supply side, benefits could increase access to credit, prompting lenders to offer more credit cards, bank loans, and mortgages in response to the higher incomes of disability recipients. This increase in access to credit could have two potential effects. First, it could mechanically increase bankruptcy and foreclosure rates since individuals can only default if they have access to credit. Indeed, we find that benefits increase home purchases, which likely means they increase mortgage underwriting. But overall we find that disability benefits lead to a *decline* in bankruptcies and foreclosures, so

[35] In the short term, the wealth channel could actually increase bankruptcy filings by providing households with enough money to pay bankruptcy fees. Bankruptcy attorney fees typically cost at least $1,000, and many households must thus "save up" for bankruptcy (Gross et al., 2016), filing only when they have the funds to do so. If so, this would make our reduced-form estimates an under-estimate of the wealth effect operating through lower financial distress.

Table 5: Instrumental Variable Estimates by Subgroup

	After initial allowance – within 3 years			
	Pt. Est.	(Std. Err.)	Cntrl. Mean	N (in millions)
Bankruptcy				
All	-0.00773***	(0.00268)	0.0251	2.22
Less than high school education	-0.00772*	(0.00396)	0.0187	0.65
High school or more	-0.00633*	(0.00372)	0.0276	1.37
Male	-0.00393	(0.00349)	0.0231	1.17
Female	-0.0126***	(0.00445)	0.0280	0.95
SSDI adults	-0.00801**	(0.00339)	0.0309	1.39
SSI adults	-0.00802***	(0.00311)	0.0180	1.11
Foreclosure (conditional on homeownership)				
All	-0.0175***	(0.00566)	0.0518	0.60
Less than high school education	0.00120	(0.0115)	0.0483	0.13
High school or more	-0.0215***	(0.00659)	0.0527	0.46
Male	-0.0195***	(0.00732)	0.0531	0.32
Female	-0.0134	(0.00917)	0.0500	0.27
SSDI adults	-0.0201***	(0.00604)	0.0537	0.49
SSI adults	-0.0248**	(0.00995)	0.0525	0.20
Net home sale (conditional on homeownership)				
All	-0.0175***	(0.00659)	0.115	1.06
Less than high school education	-0.0176	(0.0121)	0.101	0.22
High school or more	-0.0195**	(0.00796)	0.118	0.77
Male	-0.0245***	(0.00833)	0.111	0.57
Female	-0.0109	(0.0110)	0.121	0.46
SSDI adults	-0.0219***	(0.00709)	0.119	0.84
SSI adults	-0.0138	(0.0111)	0.102	0.33
Net home purchase				
All	0.00605**	(0.00252)	0.0438	3.82
Less than high school education	0.00599*	(0.00359)	0.0279	1.10
High school or more	0.00784**	(0.00339)	0.0499	2.49
Male	0.00786**	(0.00336)	0.0439	2.03
Female	0.00398	(0.00405)	0.0450	1.64
SSDI adults	0.00917***	(0.00338)	0.0573	2.47
SSI adults	0.00310	(0.00230)	0.0198	2.02

Notes: This table reports instrumental-variable estimates of the effect of being 50 years or older and 55 years or older at the initial decision date on reduced-form financial outcomes by subgroups. The "bankruptcy" regressions are based on the bankruptcy sample: disability-program applicants who reach step 5 of the disability determination process, and who have an initial decision date in 2000–2009. The "foreclosure" regressions are based on the foreclosure sample: disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2005–2014. The "net home-sale" regressions are based on the home-sale sample: disability-program applicants who appear in the deeds records (homeowners), who reach step 5 of the disability determination process, and who have an initial decision date in 2000–2014. The "net home-purchase" regressions are based on the home-purchase sample: disability-program applicants who reach step 5 of the disability determination process and who have an initial decision date in 2000–2014. A "net" home sale is defined as a home sale that is not accompanied by a home purchase within six months before or after the sale, and analogously for net home purchase. Samples involving foreclosure or bankruptcy exclude ZIP Codes of residence at application that have an average of fewer than five recorded events per year during the corresponding period; samples involving net home-sale or net home-purchase outcomes exclude ZIP Codes of residence at application that have an average of less than fifteen recorded corresponding events per year during 2000–2014. Control means are the average value of the variable for applicants who are under age 50 or 55 by 6 to 10 months. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

such an "access to credit" effect would lead us to under-estimate the wealth effect.

Second, greater access to credit could lead households to roll over debt onto credit cards or other new products and thus avoid default. If this were the case, we would expect the additional loans to postpone bankruptcy but not to eliminate it entirely. Yet the 5-year estimates in Table 4 do not suggest a reversal in the effects on bankruptcy, foreclosure, or home sale in later years. Although we cannot rule out a later increase in adverse financial events entirely, we think that is unlikely based on the long-term estimates of Table 4.

Disability benefits could also affect demand for credit through an income effect. We find that disability benefits increase recipient income (see Appendix Table A16). If credit is a normal good, then disability benefits will increase demand for credit, which could mechanically increase bankruptcies and foreclosures. However, as with greater credit access, this mechanical increase would lead us to under-estimate the wealth effect. On the other hand, if credit is an inferior good, then disability benefits will decrease demand for credit, which could mechanically reduce bankruptcies and foreclosures. Although this is possible, we think it is unlikely that demand for credit is decreasing in income for households with such low levels of income—recall that average annual pre-decision earnings are less than $20,000. Calculations based on the 2016 Survey of Consumer Finances indicate that, for the lower part of the income distribution, applications for credit are increasing in income.

Incentive Effects. Another way that disability benefits could mechanically affect financial outcomes is by changing the incentive to file for bankruptcy or repay debts. Suppose, for instance, that disability-program rules (e.g., income or asset tests) either impose restrictions on or encourage recipients to file for bankruptcy, default on a mortgage, or buy or sell a home. Or suppose that the bankruptcy process (or foreclosure or home-transaction process) treats disability recipients differently than other individuals. In either case, benefits could then affect the rates of these financial events.

To the best of our knowledge, however, disability program rules do not affect the incentives to file for bankruptcy or default on a mortgage. Disability benefits are not contingent on bankruptcy or foreclosure and adjudicators at the SSA are not supposed to consider financial markers like bankruptcy or foreclosure when deciding whether to continue a recipient's benefits.[36] In terms of home transactions, the SSI asset test exempts one home, so one might hypothesize that some recipients purchase a home (or do not sell their home) in order to shift assets from non-exempt to exempt categories in order to maintain eligibility. In practice,

[36]Initial examiners do not interact with the recipient in person during a continuing disability review. Administrative law judges do interact with disability applicants and recipients in person, so it is possible that they consider financial distress. Official agency guidelines require adjudicators to restrict their attention to only medical and vocational criteria.

however, we find effects on home purchases are concentrated among the DI population and are smaller for the SSI population.[37]

Turning to bankruptcy, Social Security benefits are exempt from the Chapter 7 means test, meaning that allowance onto disability programs does not reduce the ability to file for Chapter 7. For Chapter 13, Social Security benefits may help recipients create a debt repayment plan that a court is more likely to approve, but we would consider this a wealth effect rather than an incentive effect. Federal disability benefits are protected in bankruptcy, which might increase the incentive to take on debt and file for bankruptcy, but this incentive would work in the opposite direction of the reduced-form results.[38]

Considering foreclosure, regulations prohibit lenders from garnishing disability benefits to cover mortgage debt not covered by the foreclosure sale, which could increase the incentive to default on a mortgage. But this too would lead to an *increase* in foreclosure rates after allowance, which would lead us to under-estimate the wealth effect.[39]

Finally, turning to home transactions, some public lending programs treat disability recipients differently than other potential homeowners.[40] This could mean that SSI recipients get better loan terms and therefore are more likely to purchase a house than disability applicants who are denied. However, rates of homeownership among SSI applicants are low and these lending programs are small in scale.

Summary. Although we cannot rule out these alternative mechanisms, we conclude from the evidence and institutional details that, for the most part, they either work in the opposite direction of the results or would likely be small in magnitude. The most likely channel then is the wealth channel: allowance onto disability programs increases applicants' wealth and thus they become solvent. Newly allowed applicants can meet their financial obligations, and this wealth leads to a decrease in bankruptcies, foreclosures, and home sales.

Why does disability allowance have such a large effect on financial distress? One reason is that disability applicants are in severe financial distress at the time of application. Figure 2 suggests that applicants' risk of bankruptcy, foreclosure, and eviction is much higher than the general population, and Figure 3 shows that it is high relative to the applicants' lifetime risk, peaking just after they apply for benefits. For this population, then, it is perhaps

[37]Importantly, SSI determination involves an asset test: applicants with assets beyond a threshold are automatically denied. That aspect of SSI creates an incentive for applicants not to purchase a home. That said, the SSDI determination process includes no such asset test, and we find roughly similar treatment effects across the two programs.

[38]See, e.g., "Bankruptcy Can Help Seniors Protect Assets," *New York Times*, May 13, 2015.

[39]Section 207 of the Social Security Act, 42 U.S.C. §407.

[40]For example, Connecticut's "Home of Your Own Program" offers better terms to recipients with disabilities and accepts SSI allowance as proof of disability. See https://mymortgageinsider.com/qualify-mortgage-disability-income

unsurprising that a monthly disability check and health insurance has a large effect on financial outcomes. Indeed, the monthly disability check represents an increase in income for applicants. Appendix Table A16 presents IV estimates for earnings and income. Disability allowance causes annual earnings to decline by $1,150 and total observed income—annual earnings plus disability-program benefits—to decrease by $140 within three years after the decision.[41]

These results are consistent with previous studies showing that the social safety net can have a large effect on these same outcomes. Hsu et al. (2018) study unemployment insurance and foreclosure and find that increases in benefits drastically reduce foreclosures. Their estimates suggest that a one-standard-deviation increase in unemployment-insurance benefits cuts a layoff-related increase in foreclosures by more than half. Similarly, in studying the Oregon Health Insurance Lottery, Baicker et al. (2013) find that Medicaid "nearly eliminates" catastrophic medical debt, reducing its incidence by 81 percent. Gallagher et al. (2019) find that households eligible for Affordable Care Act marketplace subsidies experienced a 25 percent decline in mortgage delinquency rates.

5 Welfare Implications

In this section, we consider the welfare implications of our estimates of the effect of disability programs on financial distress. We incorporate our estimates into standard optimal-benefits calculations from the public-finance literature. Typically, optimal-benefits calculations only consider mean consumption, and, in particular, the simple difference between mean consumption in the good state of the world and mean consumption in the bad state of the world.[42] Such an approach is appropriate if mean consumption is a sufficient statistic for the welfare gains from insurance. In that case, one could simply turn to publicly available surveys to measure mean consumption and evaluate the welfare effects of the program.

But that approach would be problematic for two reasons. First, risk-averse disability-program applicants care not only about mean consumption but also about the likelihood of

[41]This decline in earnings is smaller than previous estimates of the effect of disability programs on earnings. For instance, it is roughly 30 percent of the effect on earnings estimated by Maestas et al. (2013). The earnings effect we estimate may be smaller for a number of reasons. The sample here, by necessity, includes only older applicants. Moreover, the variation we study comes from the fifth step of the initial-determination process, while the variation studied by Maestas et al. (2013) comes from initial examiner assignment. The complier populations for these instruments could be different. In particular, the complier population for the instrument in this paper is likely to have lower earnings potential than the complier population for the examiner instrument, since the average rejected applicant at step 5 has been judged unable to do their previous job in step 4.

[42]For instance, Gruber (1997), Bronchetti (2012), and Lawson (2017) all approximate average marginal utility with the marginal utility of average consumption.

extreme losses in consumption. Second, standard optimal-benefits calculations focus only on the transfer from individuals in the good state to individuals in the bad state, ignoring potential spillovers to those in the good state. For these reasons, we extend standard welfare calculations in two ways. First, we adapt optimal-benefits calculations to capture the effect of disability programs on tail consumption risk, under assumptions that we outline below.[43] Second, we incorporate into optimal-benefits calculations our estimates combined with existing estimates of the spillover effects of foreclosure to neighboring property owners.

5.1 Adapting Optimal-Benefit Calculations to Consider Tail Consumption Risk

We use our estimates to illustrate that tail consumption risk, as proxied by the financial events we observe, can play an important role in the calculation of optimal benefits. We make several assumptions to illustrate this point. First, we assume that these tail events—foreclosure, bankruptcy, eviction, and home sale—represent risk, which is uncertain from the agent's perspective, rather than heterogeneity, which is known to the agent. Second, we assume that there are no other forms of formal or informal insurance, such as spousal labor supply. Third, we consider only the ex-post value of disability benefits conditional on becoming disabled, not the ex-ante insurance value of the disability system prior to becoming disabled. We also abstract away from the ex-ante moral-hazard incentive problem that considering financial distress in the calculation of optimal benefits might encourage financial distress or applications from financially distressed individuals.

Consider the following adaptation of the Baily-Chetty (Baily, 1978; Chetty, 2006) framework, in which a social planner sets the benefit amount b and tax t to balance risk protection for the agent against the effect of moral hazard on the government budget. In this adaptation, agents face a small risk of a large consumption loss and the risk for disabled agents depends on b:

$$\begin{aligned}
\max_{c_a^l, c_a^h, c_d^l, c_d^h} \quad & (1-p)[(1-q_a)u(c_a^h) + q_a u(c_a^l)] + p[(1-q_d(b))u(c_d^h) + q_d(b)u(c_d^l)] + \Psi(p) \\
\text{s.t.} \quad & c_a^h = A_a + w - t, \quad c_a^l = A_a + w - t - L, \\
& c_d^h = A_d + b, \quad c_d^l = A_d + b - L, \\
& t(1-p) - pb \geq 0.
\end{aligned}$$

Here, p is the likelihood of disability, c_a^l (c_a^h) represents low (high) consumption in the able-bodied state (including assets A_a and wages w), c_d^l (c_d^h) represents low (high) consumption in the disabled state, q_a is the likelihood of a large consumption loss L associated with an

[43] It is important to recognize that the current structure of SSDI and SSI may not be optimal. Alternative policies may better address not just the problem of disability but also applicants' financial distress. Those alternative policies, however, are beyond the scope of this paper.

extreme financial event in the able-bodied state, and $\Psi(p)$ reflects the leisure value of not working. The parameter $q_d(b)$ is the likelihood of loss L in the disabled state and depends on the benefit b. Making q_d endogenous reflects the evidence from our IV estimates that disability programs not only increase consumption through the cash transfer but also make the worst states of the world (those with large consumption losses) less likely to arise.

Rewriting the problem in terms of b yields the following first-order condition:

$$\begin{aligned} & (1-p)[-(1-q_a)u'(c_a^h) - q_a u'(c_a^l)]\frac{dt}{db} \\ + \quad & p[(1-q_d(b))u'(c_d^h) + q_d(b)u'(c_d^l) - q_d'(b)u(c_d^h) + q_d'(b)u(c_d^l)] = 0. \end{aligned}$$

Totally differentiating the balanced budget constraint yields

$$(1-p)\frac{dt}{db} = p\left[1 + \varepsilon_{p,b}\frac{1}{1-p}\right],$$

where $\varepsilon_{p,b}$ is the elasticity of the likelihood of disability with respect to the benefit b. Finally, substituting terms yields the following condition at the optimal b^*:

$$\frac{\varepsilon_{p,b}}{1-p} = \frac{Eu'(c_d^*) - Eu'(c_a^*) - q_d'(b)[u(c_d^{*h}) - u(c_d^{*l})]}{Eu'(c_a^*)}. \tag{4}$$

We parameterize the probability of loss in the disabled state as follows:

$$q_d(b) = a_0 - a_1 b,$$

where a_0 is the baseline probability of consumption loss for the disabled population from our descriptive estimates, and a_1 is the scaled causal effect of benefits on likelihood of an extreme consumption loss from our causal estimates. This parameterization assumes that the effect of disability benefits on the likelihood of the loss is linear—in other words, that the first dollar of benefits has the same effect as the ten-thousandth dollar.

We calculate L from survey data. Note that L need not be the causal effect of bankruptcy or foreclosure on consumption. Instead, we seek to measure the consumption drop *associated* with the financial distress for which these events are proxies. In order to estimate L, we calculate the average household food and housing expenses within three years of an adverse event based on households experiencing foreclosure or bankruptcy in the Panel Study of Income Dynamics (PSID). We find an annual drop of \$6,300 in average household food and housing expenses within three years of a foreclosure.[44]

[44] We use the PSID-provided measures on household expenses since 1999 and calculate annual household expenses using the sum of food and housing expenses. Due to data limitations, we apply the estimated consumption drop associated with foreclosure to all adverse financial events: foreclosure, bankruptcy, and distressed home sales. Questions on bankruptcy were only added to the survey in 1996, so we have insufficient power to estimate the consumption drop associated with bankruptcy alone. Appendix Figure A11 presents the event-study plot.

Baseline: $a_0 \neq 0$**,** $a_1 = 0$**, approximate average marginal utility with marginal utility of average consumption.** We first establish a baseline in which $a_0 \neq 0$ and $a_1 = 0$, meaning that we temporarily ignore the causal effect of b on tail consumption risk q_d. This baseline corresponds to the standard Baily-Chetty condition, which is usually implemented by approximating average marginal utility with the marginal utility of average consumption. In our context, this approximation is:

$$\frac{\varepsilon_{p,b}}{1-p} = \frac{Eu'(c_d^*) - Eu'(c_a^*)}{Eu'(c_a^*)} \approx \frac{u'(\bar{c}_d^*) - u'(\bar{c}_a^*)}{u'(\bar{c}_a^*)}, \tag{5}$$

where

$$\begin{aligned} \bar{c}_d &= q_d c_d^l + (1-q_d)c_d^h = \bar{A}_d + b - q_d L, \\ \bar{c}_a &= q_a c_a^l + (1-q_a)c_a^h = \bar{A}_a + \bar{w} - t(b) - q_a L. \end{aligned}$$

To establish the baseline, we take the current average annual disability benefit of $13,000 to be the optimal benefit amount, b^*, under a utility function with constant relative risk aversion (CRRA) and a coefficient of relative risk aversion, γ, of 2. Using our estimates of $\varepsilon_{p,b}$, q_a, and q_d and an estimate of $\bar{A}_a + \bar{w}$ from survey data, we solve for the value of $\bar{A}_d$ that rationalizes the current benefit level as optimal. We use these parameters in our calculations. Table 6 reports the baseline optimal benefit amount for $\gamma = 2$ and $\gamma = 4$ using the empirical approximation in equation (5). Note that this column simply reflects the *assumption* that $13,000 is optimal under $\gamma = 2$. Using the parameter values that rationalize this assumption, the optimal benefit is slightly larger for $\gamma = 4$.

Scenario 1: $a_0 \neq 0$, $a_1 = 0$**, use exact average marginal utility.** The approximation in equation (5) is less accurate when agents are more prudent (i.e., non-linear marginal utility of consumption) and when they face larger consumption losses or a higher likelihood of consumption loss. The exact implementation of equation (4) when $a_1 = 0$ is

$$\frac{\varepsilon_{p,b}}{1-p} = \frac{Eu'(c_d^*) - Eu'(c_a^*)}{Eu'(c_a^*)} = \frac{[(1-q_d)u'(c_d^h) + q_d u'(c_d^l)] - [(1-q_a)u'(c_a^h) + q_a u'(c_a^l)]}{(1-q_a)u'(c_a^h) + q_a u'(c_d^l)}.$$

Scenario 1 in Table 6 shows optimal benefit calculations using this parameterization. Depending on the value of γ and the baseline risk, the optimal benefit increases by $50 to $170 relative to the baseline.[45] The optimal benefit is higher using the exact formula because the increase in marginal utility from the consumption loss is larger than the decrease from

[45] We consider three scenarios for baseline risk: one based on foreclosure risk only (2 percent for the able-bodied, 5 percent for the disabled); one based on foreclosure plus bankruptcy risk (3 percent for the able-bodied, 8 percent for the disabled); and one based on foreclosure plus bankruptcy plus net-home-sale risk where we assume that 50 percent of net-home sales are distressed (5 percent for the able-bodied, 13 percent for the disabled).

a comparable consumption gain. Note that this increase is likely an underestimate of the true increase in the optimal benefit amount we would obtain were we able to consider the full distribution of consumption. We observe only certain extreme events, but if disability benefits shift mass from bad states to good states more generally, then considering effects on the full distribution of consumption could increase the optimal benefit amount under Scenario 1 substantially.

Scenario 2: $a_0 \neq 0$, $a_1 \neq 0$, use exact average marginal utility. Finally, we consider the implementation of equation (4) when $a_0 \neq 0$ and $a_1 \neq 0$, so that $q_d(b)$ depends on b. Making q_d endogenous has an ambiguous effect on optimal benefits. On the one hand, a higher benefit level has even more value to the agent than before, in that it reduces the likelihood of extreme consumption loss. This effect is reflected in the additional term in equation (4): $q_d'(b)[u(c_d^{*h}) - u(c_d^{*l})]$. On the other hand, by reducing the likelihood of extreme consumption loss, a higher benefit level means more equal consumption between the able and disabled states. This offsetting effect is reflected in the term $Eu'(c_d^*) = q_d(b)u'(c_d^l) + (1 - q_d(b))u'(c_d^h)$, which is smaller, and therefore closer to $Eu'(c_a^*)$, when b is larger. From Scenario 2 in Table 6, making q_d endogenous increases the optimal benefit by about \$100 relative to Scenario 1 when $\gamma = 2$ and decreases it by about \$40 when $\gamma = 4$.

This exercise illustrates that incorporating the risk of an extreme consumption loss, as proxied by financial events like foreclosure, can change optimal benefit calculations substantially. In our calculations, the annual optimal benefit amount increases by up to \$240, or around 2 percent. This number is likely a lower bound for the true increase if we were to consider the entire distribution of consumption, since we will underestimate the difference between average marginal utility the marginal utility of the average.

5.2 Adapting Optimal-Benefit Calculations to Consider Spillovers

In addition to tail risk, we adapt optimal-benefit calculations to consider the spillovers associated with allowance onto disability programs. Previous research on foreclosures, evictions, and bankruptcies suggests that these events impose negative externalities on third parties. For example, Campbell et al. (2011) extrapolate from their difference-in-difference estimates and forecasting models to calculate that each foreclosure during the Great Recession lowers neighborhood property values by \$148,000 to \$477,000.

Consider the Social Planner's problem in the previous subsection. We model the spillovers related to property values by assuming that the program benefit, b, produces some fraction $s \in [0, 1]$ in aggregate spillovers to property values. In other words, benefits not only increase consumption in the disabled state, but also increase consumption in the able-bodied state,

Table 6: Optimal Benefit Calculations

	Baseline risk		Optimal benefit			
γ	q_a	a_0	Baseline	Scenario (1)	Scenario (2)	Scenario (1) w/ spillover
2	0.02	0.05	\$13,000	\$13,040	\$13,120	\$13,180
4	0.02	0.05	\$13,230	\$13,310	\$13,280	\$13,400
2	0.03	0.08	\$13,000	\$13,060	\$13,170	\$13,200
4	0.03	0.08	\$13,230	\$13,340	\$13,290	\$13,430
2	0.05	0.13	\$13,000	\$13,100	\$13,240	\$13,230
4	0.05	0.13	\$13,230	\$13,390	\$13,340	\$13,490

Notes: This table presents the optimal benefit in different scenarios, assuming a constant relative risk aversion (CRRA) utility function and a risk of becoming disabled of $p = 0.06$. The parameter q_a (a_0) denotes the baseline risk of experiencing an adverse event in the able-bodied (disabled) state. In the first two rows of both panels, we consider the probability of experiencing foreclosure only; in the next two rows, we consider the probability of experiencing foreclosure or filing for bankruptcy; in the last two rows, we consider the probability of experiencing foreclosure, filing for bankruptcy, or selling a home in distress (assuming 50 percent of net home sales are distressed). For the optimal benefit calculation, we assume $A_a + w = \$40,000$ based on the Health and Retirement Study (HRS). Under the assumption that the current disability benefit level \$13,000 is optimal based on equation (5) under CRRA with $\gamma = 2$, we obtain $A_d = \$31,880$ under $q_a = 0.02$ and $a_0 = 0.05$, $A_d = \$31,980$ under $q_a = 0.03$ and $a_0 = 0.08$, and $A_d = \$32,200$ under $q_a = 0.05$ and $a_0 = 0.13$. foreclosure. We estimate $L = \$6,300$ from the Panel Study of Income Dynamics (PSID), and we calculate the elasticity of non-employment with respect to the benefit amount $\varepsilon_{p,b} = 0.021$ from our data. For Scenario (3), $q_d(b^*)$ denotes the endogenous probability of experiencing an adverse event in the disabled state under the optimal disability benefit. We use $b = \$13,000$ to scale the casual estimates.

through the reduction in nearby foreclosures. The aggregate spillover amount, $s \times b$, is divided among all able-bodied agents, which in the model is $\frac{1-p}{p}$. The only change in the Social Planner's problem from the previous subsection is the consumption of the able-bodied agent:

$$c_a = A_a + w - t + \frac{sb}{\frac{1-p}{p}}.$$

The Baily-Chetty condition under Scenario 1 with spillovers becomes

$$\frac{\varepsilon_{p,b}}{1-p} = \frac{Eu'(c_d^*) - Eu'(c_a^*) \cdot [1 - sp]}{Eu'(c_a^*)}. \tag{6}$$

All else equal, a larger spillover, s, increases the difference in the marginal utilities across states and therefore increases the optimal benefit, b^*.

To determine a reasonable value for s, we use our estimates of the effect of initial disability allowance on foreclosure combined with estimates from the literature of the decline in neighboring property values from foreclosure. We find that initial disability allowance reduces the likelihood of foreclosure by 1.75 percentage points. Campbell et al. (2011) estimate a decline of at least $148,000 in neighboring property values for each foreclosure. Multiplying these two numbers, we approximate that 6.6 percent of the disability benefit amount accrues to neighboring property owners through the reduction in foreclosures.

We use this value of s to determine how the optimal benefit changes. As shown in Table 6, considering property-value spillovers increases the optimal benefit by approximately $130 for $\gamma = 2$ and by $90 for $\gamma = 4$ relative to Scenario 1. The increase is smaller for a larger degree of risk aversion because the consumption of able-bodied agents is valued less at higher levels of risk aversion. Disability programs may also create other spillovers that we do not consider here.[46]

5.3 Considering Optimal Benefit Timing

In addition to the optimal benefit *level*, this analysis can also inform the optimal *timing* of disability benefits. Figure 3 suggests that applicants, on average, apply for disability benefits when they are in peak financial distress and have a high marginal utility of consumption relative to their lifetime average. In addition, our causal estimates suggest that initial disability allowance, which occurs several months after application and often after the 5-month statutory waiting period, dramatically lowers rates of financial distress. Based on those two findings, it is likely that awarding disability benefits sooner would avert a substantial amount of financial distress among applicants.[47] Of course, awarding benefits sooner also involves higher administrative costs and could change the composition of the applicant pool. Determining the optimal wait time requires weighing these considerations against the benefits suggested by our estimates.

[46]Another way to put the real-estate-related spillovers in context is to compare it to the effect of the disability programs on earnings. We find that disability allowance reduces labor market earnings by $3,450 over three years, and it increases housing values due to averted foreclosures by roughly $2,590, which is 75 percent of the decrease in earnings. We also calculate the marginal value of public funds (MVPF), which is the ratio of the marginal benefits of a policy to its marginal cost (see Jacobs (2018) for a review). In Appendix G, we use our estimates to calculate the MVPF, as derived by Hendren (2016) and Hendren (2017), incorporating spillovers to third parties and fiscal externalities. We calculate an MVPF of 1.04 for disability programs when considering effects on foreclosure and bankruptcy. The ratio is smaller, 0.99, when we ignore these effects because of the large positive spillovers to third parties and to the government from reductions in foreclosures and bankruptcies.

[47]Autor et al. (2015) find that longer waiting times result in worse labor market outcomes for rejected applicants. Prenovitz (2018) uses backlogs as an instrument and find small increases in DI wait time can have negative implications that extend beyond labor force participation for applicants.

6 Conclusions

This paper provides the first evidence of the effect of disability programs on financial outcomes. We merge the universe of Social Security disability applicants to nationwide records on bankruptcies, foreclosures, evictions, home sales, and home purchases to create the first sample of disability applicants linked to financial records. We document that disability applicants have high rates of financial distress, both relative to the general population and relative to their own lifetime profiles. We then use this data linkage in combination with a quasi-experiment created by the disability determination process to identify the impact of disability programs on bankruptcy, foreclosure, home sales, and home purchases. We find that allowance onto disability programs leads to large reductions in bankruptcies, foreclosures, and home sales, and to increases in home purchases. We consider the mechanisms through which disability programs can affect financial outcomes, including wealth, credit access, credit demand, and incentives from program rules. The evidence indicates that most of the reduction in adverse financial events reflects a true reduction in financial distress and an increase in recipient welfare.

Our findings inform both the optimal magnitude and the optimal timing of benefits. Regarding the magnitude of benefits, the estimates suggest that disability programs confer large welfare gains to recipients and to third parties. The reduction in the likelihood of an extreme consumption loss is more valuable than the increase in average consumption alone would suggest. Under certain assumptions regarding the relationship between financial events and consumption, incorporating the reduction in tail consumption risk increases the optimal benefit by at least several hundred dollars. The estimates also suggest sizable spillovers from disability programs to non-recipients, especially neighboring homeowners whose property values increase as a result of the reduction in foreclosures. Determining the optimal generosity of disability programs requires weighing these benefits against the moral-hazard costs of these programs.

Finally, this paper also provides evidence on the optimal timing of benefits, which depends on their marginal consumption-smoothing value relative to the associated moral-hazard costs along the time path of benefits (Kolsrud et al., 2018). The findings suggest that disability programs could avert even more financial distress if awarded earlier. The argument for long wait times is that they reduce administrative costs and could, in theory, improve the targeting of disability programs. However, we find that applicants apply for disability programs when they are in peak financial distress, and that benefits reduce financial distress substantially when they are awarded several months later. These findings suggest that awarding benefits sooner could avert a substantial amount of financial distress. Determining the optimal wait time requires weighing these gains against potential administrative and targeting costs.

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