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Job loss and disability insurance

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HIGHLIGHTS

- We show that job loss is a major cause of disability program entry in Norway.
- The impact of job loss on disability is much larger than previously acknowledged.
- The harder it is to find a new job, the more likely that job loss causes disability.
- The "disability problem" is to a large extent an unemployment problem in disguise.

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ABSTRACT

Based on administrative register data matched with firms' financial statements and closure data collected from bankruptcy proceedings, we show that a large fraction of Norwegian disability insurance claims can be directly attributed to job displacement and other adverse shocks to employment opportunities. For men, we estimate that job loss more than doubles the risk of permanent disability retirement and accounts for one quarter of new disability insurance claims. Firm profitability and tightness of the local labor market also significantly affect employees' likelihood of disability program entry, and the adverse effects of displacement grow stronger when local labor market conditions deteriorate.

1. Introduction

In welfare states, the lines between unemployment and disability insurance are blurred. In this paper, we provide new insights on the causal relationships between individual employment opportunities and disability program enrollment. The study is motivated by the observations that the recent rise in disability benefit recipiency has not been paralleled by any deterioration of health conditions, and that countries with comprehensive disability insurance programs also tend to have very low unemployment rates (OECD, 2010; Røed, 2012). Building on job search theory and existing empirical evidence (Autor and Duggan, 2003; Black et al., 2002), we frame our empirical analyses on the notion that there is a gray area between unemployment and disability insurance, and that shocks to individual employment opportunities may trigger disability insurance claims even when health status remains unchanged.

Because the risks of disability and unemployment will be highly correlated at the individual level, the causal effect of employment opportunities on disability program enrollment will be difficult to identify on the basis of observational data alone. Our empirical strategy is to exploit exogenous sources of variation in individual employment opportunities, generated by variation in employers' economic performance – including profitability, downsizing, and firm closure – and idiosyncratic fluctuations in local industry-specific labor market tightness, to identify causal impacts. The empirical basis is Norwegian administrative employer–employee registers, augmented with firms' audited accounts and information collected from bankruptcy courts. The bankruptcy data make it possible to distinguish genuine mass layoffs from organizational restructuring, demergers, and takeovers.

The adverse consequences of job displacement is the focus of a broad international literature (see, e.g., Hamermesh, 1987; Ruhm, 1991; Neal, 1995; Kletzer, 1998; Kuhn, 2002; Hallock, 2009), including two recent

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studies relying on Norwegian employer–employee data (Rege et al., 2009; Huttunen et al., 2011).[1] The present paper extends this literature in several directions. It is, to our knowledge, the first study to exploit data on mass layoffs resulting from recorded bankruptcies in order to identify the impacts of exogenous displacement on the subsequent disability program and non-participation propensities of affected workers. Based on estimates of the overall number of involuntary job loss in the economy – including those from stable and growing firms – it is also the first study to assess the total impact of job loss on the frequency of disability insurance claims. We further add to the literature by examining more specifically the influences of firms' economic performance and of alternative (local) employment opportunities on employees' likelihood of entering disability insurance programs. And, finally, we examine the *interaction* between these various measures of employment opportunity to test whether the probability that job loss leads to a disability insurance claim declines with local labor market tightness.

In contrast to the existing literature, the paper also explicitly addresses the problem that the root cause of disability program enrollment may be hidden in events that took place many years prior to actual entry into permanent disability insurance. We show that social security careers ending in permanent disability retirement are often extremely long and intricate. Identification of the triggering causes therefore requires long and detailed labor market histories for the population at risk. In order to assess the impact of, e.g., job loss on the subsequent probability of becoming a disability pensioner, we either have to take into account that the outcome may materialize long after its cause, or we have to examine outcomes that materialize closer in time to their cause, but are highly correlated with the subsequent risk of receiving a permanent disability benefit. In this paper we pursue both these strategies; the former by examining entry into permanent disability insurance up to six years after displacement, and the latter by examining entry into temporary disability programs and withdrawal from the labor market.

Our results show that disability insurance and non-participation risks are indeed significantly affected by exogenous change in employment opportunities. Some of the estimated effects are large from an economic viewpoint, particularly for men. Our most reliable indicator for individual displacement is full-time employment in a firm which will go bankrupt within four years. Holding such a job raises, on average, the risk of entering permanent disability retirement during the upcoming six-year period by 2.0 percentage points for male employees and 1.2 percentage points for female employees, when compared to holding a job in a stable firm. Taking into account that the risk of job loss is present even in stable firms, we estimate that displacement raises the risk of permanent disability retirement by as much as 2.6 percentage points (121%) for men and 1.6 percentage points (48%) for women, ceteris paribus. Extrapolating these effects to all job losses in Norway, we infer that job loss accounts for around 28% of all new disability benefit claims among males and for 13% among females in our data. Not surprisingly, we also find strong impacts on the propensity for non-participation. For men, the probability of being outside the labor force after four years increases by 9.0 percentage points (123%) as a result of exogenous job loss. For women, the probability rises by 12.1 percentage points (98%). Disability insurance and non-participation propensities are also affected by more moderate downsizing processes and even by reductions in firm profitability without any observed downsizing. In addition, employment opportunities outside the current workplace play a significant role. A one standard deviation deterioration in local education/industry-specific labor market tightness (conditional on aggregate labor market tightness) raises the probability of permanent disability retirement by around 0.4 percentage points (14%) for men and 0.5 percentage points (also 14%) for women. In support of the hypothesis that disability and unemployment statuses are substitutable, we also identify significant interaction effects between job loss and local labor market conditions. The more difficult it is to find a new job, the higher is the probability that displacement leads to disability retirement.

The causal relationship between employment opportunity and disability insurance propensity will of course also reflect that job loss and unemployment entail adverse health consequences; see Kasl and Jones (2002) for a survey. In particular, our results show that, for *male* employees, job loss raises the mortality rate over a six-year period by 34 percent. For men, our data therefore support recent evidence from Sweden and the United States showing adverse effects of displacement on mortality risk (Eliason and Storrie, 2009b; Sullivan and von Wachter, 2009). However, we fail to find evidence that displacement has adverse health effects for female workers.

The estimates of causal effects of displacement on the propensities for disability insurance and non-participation presented in this paper are an order of magnitude larger than comparable estimates reported in prior studies, such as Rege et al. (2009) and Huttunen et al. (2011). We find that this disparity largely stems from differences in the operational definition of "displacement." While the findings of the prior studies are based on mass layoffs identified from employment registers alone (with, as noted by the authors, the risk of misclassification in cases of reorganizations, demergers, and takeovers), the mass layoffs exploited in this paper are identified on the basis of auxiliary information taken from bankruptcy proceedings. We demonstrate that this approach reduces attenuation bias otherwise associated with the purely register-based method. The revised effect estimates show that job loss is a major factor behind disability program participation in Norway.

2. Institutional background

Workers in Norway are insured against loss of work capacity from health impairment. Social insurance is compulsory and comprises sickness absence benefits, rehabilitation benefits, and disability pension. During sickness absences, the benefit replacement rate is 100%. Sickness absence benefits cannot be paid out for more than 12 months, however. Beyond 12 months, workers are eligible for rehabilitation or disability benefits provided that their work capacity is reduced by at least 50% due to sickness or injury. The replacement ratio associated with rehabilitation benefits or disability pension is typically around 66%. Rehabilitation benefits are temporary (normally 1–3 years), and are paid out during medical and/or vocational rehabilitation attempts. Disability pension is in practice a permanent benefit (lasting until the normal retirement age of 67), as the outflow from disability pension to self-supporting employment is negligible. Except for very short sickness absence spells (three days or less), all social insurance payments require that a physician certifies the health impairment. In more serious cases, the application may also be assessed by independent physicians appointed by the social security administration. It must be certified that health impairment is *the main cause* for the loss of work capacity. If this requirement is met, the law text explicitly states that the social security administration may consider the employment opportunities of the applicant when ruling whether or not the loss of work capacity is sufficiently large to qualify for benefits.

The economic incentives embedded in the social insurance replacement ratios were stable during the time period covered by this paper (1993–2006), although the period covers some attempts at tightening gate-keeping, particularly for disability pensions. For example, the requirement that the certified health impairment must be *the main cause* of the claimant's inability to work was introduced in 1995. Prior to 1995, it was sufficient that health impairment was *among the causes*. In 2000, the rehabilitation requirement was tightened such that disability benefit applicants were required to go through a vocational rehabilitation attempt, unless deemed *obviously futile*.[2] In 2004, the rules regulating the

[1] For previous Norwegian evidence that unemployment is among the key drivers of labor market detachment processes leading to permanent disability retirement, see also Bratberg (1999), Dahl et al. (2000), and Bratsberg et al. (2010).

[2] Apparently, vocational rehabilitation is deemed "obviously futile" quite often. According to our data, as many as 62% of the 2005 disability entrants had never been referred to vocational rehabilitation.

maximum duration of rehabilitation benefit payments were also tightened, leaving less room for extensions beyond one year. The same year saw the introduction of a *time-limited* disability benefit (with a maximum duration of four years). This new benefit effectively substituted for permanent disability pension for younger claimants. However, experiences so far indicate that return to employment from the time-limited disability benefit is modest, and that the arrangement essentially only has postponed entry into the permanent disability program.[3]

The employer is responsible for covering sickness insurance payments during the first 16 days of the sickness absence spell. For longer spells and for permanent disability insurance claims, the costs are covered in full by the public purse. There is no experience rating; hence there are limited pecuniary costs for firms associated with their employees utilizing long-term sickness or disability programs. In fact, when a firm has redundant labor, but finds it difficult to lay off workers due to employment protection regulations, an employee's transition to long-term sickness absence or disability insurance may be profitable for the firm.

Identifying and quantifying the roles of job loss and disemployment in explaining disability insurance claims is especially pertinent to recent developments in Norway. Over the past decades, Norway experienced a staggering rise in temporary and permanent disability program participation. Based on the data used in the present paper, we find that, over the 1993–2006 period, dependency on broadly defined health benefits increased by 34%, from 15.2 to 20.4% of the working-age population, with the ratio of those claiming permanent disability insurance to the number of unemployed rising from 1.2 to 4.0. The growth in disability rolls occurred without any corresponding deterioration in health conditions. To the contrary, subjective health indicators improved, with the proportion of the adult population reporting good or very good health rising from 79% in 1995 to 81% in 2005, and the share reporting bad or very bad health declining from 8 to 6%.[4]

3. Theoretical considerations

Although disability insurance eligibility requires at least 50% reduced work capacity due to sickness or injury, it is plausible that individual preferences and labor market opportunities affect application and approval decisions. Job search theory provides a useful framework for thinking about the process of entry into the disability insurance program in this context; see, e.g., Diamond and Sheshinski (1995), Autor and Duggan (2003), and Rege et al. (2009). Individuals are assumed to have preferences over the alternative labor market states of employment, job search, and inactivity (with or without disability benefits); and job displacement can be viewed as a negative shock to the value of continued labor market participation. It follows directly that there potentially is a group of individuals who prefer employment over inactivity, but nonetheless prefer disability benefit application over search for new employment. Autor and Duggan (2003) label this group "conditional disability insurance applicants," as they will apply for disability benefits only in the event of job loss. The intuition behind the conditional application strategy is that job loss shifts the discounted value of labor market participation below that of inactivity. This may happen both because obtaining a new job will incur search costs and because a new job is hard to find and likely to pay less than the prior job. Barth (1997) shows that there is a significant tenure component in Norwegian wage setting partly generated by a delayed compensation strategy (Lazear, 1981). And, as stressed by Bound and Burkhauser (1999), displacement nullifies the value of job-specific human capital and thus reduces the value of continued labor market participation. Recent empirical evidence from Norway also confirms that displacement leads to significant earnings losses (Huttunen et al., 2011).

Given the relatively strong protection against selective dismissals in Norway, it is probable that many existing employment relationships will continue despite loss of productivity caused by reduced health. In the event of job loss triggered by downsizing or closure, however, the same health problem is likely to reduce the arrival rate of new job offers and shift the distribution of wage offers downwards, and hence make job search less attractive. At the same time, the likelihood of being considered eligible for disability benefits may increase following displacement, since work capacity is assessed relative to realistic employment opportunities. This obviously entails elements of discretionary judgment by the social security administration. Røed and Westlie (2012) present empirical evidence showing that the probability of making a direct transition from unemployed job search to temporary or permanent disability enrollment rises significantly with past unemployment experience, indicating that a long and unsuccessful job search is interpreted as evidence of reduced work capacity.

Employment protection legislation does of course not provide full insurance against selective dismissals. Individual workers may legally be laid off in continuing firms if there is a factual foundation for downsizing or reorganization based on the firm's economic performance. Management may further encourage employees to quit the job, perhaps with some severance payment as a carrot, in order to achieve a desired reorganization without triggering labor conflicts. If the probability of disability program entry rises upon job loss, we would expect the future risk of disability retirement to relate negatively to firm profitability, as high profits reduce the likelihood of dismissals and employer-initiated quits.

Extending the job search model with the option of applying for disability benefits further yields the prediction that the probability of being a conditional disability insurance applicant declines with labor market tightness, as the value of unemployment rises, while the value of inactivity declines, with improved employment opportunities. In particular, an important implication of such a model is that the impact of job loss on the rate of disability program entry is larger the more difficult it is to find a new job. We therefore expect to find a negative interaction effect between job loss and labor market tightness in empirical models designed to explain disability program entry.

4. Data and identification challenges

The data we use in this paper consist of three parts. The first part covers a detailed account of individual labor market and social security event histories from 1992 to 2007, linked with comprehensive information about each individual. The second part includes a description of firms in terms of their employee composition and economic performance. Indicators for economic performance are constructed from annual audited accounting data, which all limited liability firms in Norway are required to make public. The third part contains information about the nature of firm closures. These data are collected from the Norwegian bankruptcy court system. A generic problem facing research based on administrative employer–employee data is to distinguish genuine mass layoffs from "spurious" layoffs, whereby a firm appears to downsize or close down while in reality it splits into smaller entities, merges with another company, or reorganizes in other ways, perhaps without laying off workers at all. A strategy pursued in the existing literature (Fevang and Røed, 2006; Henningsen and Hægeland, 2008; Rege et al., 2009) is to interpret a mass layoff as spurious when a relatively large fraction of the workers make a transition to the same new firm. But this strategy obviously fails to identify a spurious layoff that splits the workforce, e.g., when a large firm is reorganized into several smaller entities. Defining thresholds for the fraction of workers moving together may also be awkward and result in measurement error for small firms. In the present paper, we exploit additional information that we collect from bankruptcy court proceedings and that allows us to distinguish explicitly between closures due to bankruptcy, closures due to voluntary liquidation, and takeovers (with or without a bankruptcy).

[3] Our data show that, by the end of 2004, 8412 persons received a time-limited disability pension. Three years later only 2% had returned to work. As many as 65% remained on time-limited disability and 29% had entered permanent disability.

[4] These numbers are collected from Statistics Norway's level of living sample surveys, and can be downloaded from www.norgeshelsa.no.

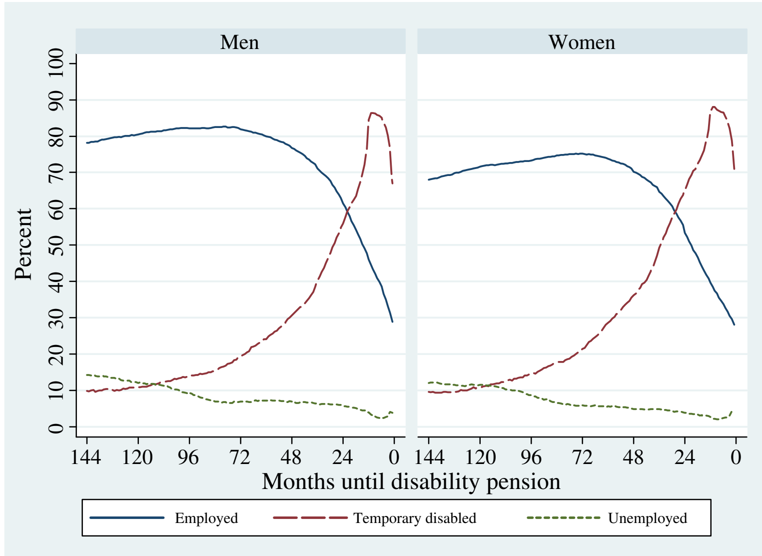

Fig. 1. Past labor market states of 2005 permanent disability program entrants. Note: States are not mutually exclusive, as disability and unemployment may be partial and combined with some employment. Populations consist of 13,194 men and 15,993 women of age 30 or above who entered the permanent disability program in 2005.

A particular problem arising in attempts to identify the causal effect of employment opportunities on subsequent disability insurance claims is the long and variable time lags between the presumed *cause* and its observed *effect*. When granting a new disability pension, the social security administration also sets a "disablement date." This date is meant to reflect the occurrence of the health impairment behind the loss of at least 50% of work capacity. Because benefits are based on earnings up to the time of disablement, the date becomes important for the level of benefits; hence its determination is likely to involve some considerate judgment by the case worker. On the basis of disablement dates recorded in our data, we find that the disablement on average occurs three years before entry into the permanent disability program. The variation across individuals is large, however, and for almost 20% of claimants the duration from disablement until disability retirement is more than five years. The typical duration from disablement to disability pension uptake also varies over time, primarily reflecting the various attempts (referred to above) at curbing the inflow to the permanent disability rolls. To illustrate, in our data the average "waiting time" fell from 38 months for 1997 entrants to 32 months for 2000 entrants, after which it rose to 36 months for 2003 entrants to the permanent disability program (we do not have comparable disablement date statistics for later entrants).

Many disability program entrants have long histories of labor market difficulties, often with combinations of unemployment and health problems. In these cases, it is difficult to identify a particular triggering event. Fig. 1 displays the employment and social security histories – month by month – during the 12-year period prior to permanent disability enrollment for men and women age 30 or older who entered the program in 2005. Almost one quarter of this group received social security transfers such as unemployment benefits as long as 12 years prior to obtaining the permanent disability status. Visible signs of health problems in the group as a whole, in the form of declining employment rates and corresponding increases in the proportion claiming temporary health benefits (rehabilitation or long-term sickness benefits), appeared around six years before disability program entry. Three years before entry into permanent disability, around 40% of the men and 50% of the women claimed temporary disability benefits. These patterns show that the road to permanent disability retirement can be long and winding – often involving unemployment spells as well as periods on temporary health benefits – and that very few cases are straightforward in that there is a once-and-for-all health shock leading quickly and directly to disability retirement.[5]

5. The effect of employment opportunities on disability program entry

5.1. Methodology

To allow for long time lags between employment opportunity shocks (the presumed cause) and entry into the permanent disability program (the possible effect), we have structured our dataset into three four-year time periods, starting at the end of the *base years* of 1993, 1997, and 2001, respectively. We condition the analysis on workers holding a full-time job on January 1st following the base year. In addition, we exclude workers with recent social insurance spells and drop from the samples those who received social security benefits for more than six months during the prior two years. We then examine the probability of permanent disability retirement as well as of transitions to states that involve a high risk of subsequent entry into the permanent disability program, as functions of, inter alia, exogenous change in employment opportunities. We limit the analysis to employees in private sector single-plant firms with more than 10 employees and for which we have access to audited accounting data (which includes all limited liability firms).[6] We also limit attention to individuals who were between 20 and 63 years of age in the base year and who resided in Norway throughout the analysis period. All analyses are conducted separately for men and women.

[5] The apparent decline in temporary disability just before entry into permanent disability displayed in Fig. 1 mirrors the occurrence of a "benefit vacuum" period after temporary disability insurance options are exhausted, but before the application for permanent disability benefits has been approved.

[6] A key to interpretation of our results is that workplace events can be considered *exogenous* with respect to the behavior of the individual employee. Since this assumption may be questionable for small workplaces, below we also present results based on samples of workers in large firms (more than 50 employees) to examine the robustness of our findings. The reason why we restrict attention to single-plant firms is that accounting and closure/takeover data are available at the company level. Hence, the accounting and closure data can be directly matched to workplace data for single-plant firms only. Finally, by focusing on single-plant firms we avoid complications caused by within-firm job transfers following plant closures (Huttunen et al., 2011).

Table 1
Analysis populations and the distribution of outcomes.

	Men			Women		
Base year:	1993	1997	2001	1993	1997	2001
Observations	130,786	189,703	203,781	44,549	59,272	70,373
Disability insurance (temporary or permanent) during next 4 years (%)	9.2	12.6	13.9	14.4	19.2	21.3
Out of labor force 4 years later (%)	7.8	10.6	11.1	14.2	16.8	17.1
Permanent disability program within 6 years (%)	3.0	3.1	2.8	3.9	4.1	3.4

We focus on three alternative outcome measures for the individual:

1. Whether claiming *disability insurance* – temporary or permanent – during the four-year period following the base year.[7]
2. Whether *outside the labor force* four years after the base year.[8]
3. Whether entered the *permanent disability* program within six years of the base year.[9]

Table 1 lists the sizes of the analysis populations and the distribution of outcomes. Comparing the three periods, we note that the incidence of permanent disability program participation rose somewhat between the first and the second period, after which it declined to a level below that of the first period. The incidence of temporary (and permanent) disability program participation rose sharply throughout the three periods; for women it increased quite dramatically, from 14.4% in the 1994–97 period to 21.3% in the 2002–5 period. We interpret the shift from permanent to temporary disability program participation in the third period as reflecting attempts by the social security administration of curbing inflows into permanent disability retirement through more ambitious rehabilitation attempts; see Section 2.

Empirical analysis of the causal impact of employment opportunities on the likelihood of claiming disability benefits requires observed variation in employment opportunities that is *exogenous* to each individual's disability program propensity. Our data give three potential sources of such variation. Two of these operate at the workplace level and consist of mass layoffs and variation in firm profitability, respectively. The third operates primarily at the region-by-occupation level and consists of fluctuations in demand for the type of labor that the worker has to offer outside the present employer. While a mass layoff will have a very direct effect on the displaced workers' employment opportunities, a prediction from the theoretical framework of Section 3 is that poor (or deteriorating) firm performance may involve small-scale layoffs that place pressure on employees to quit "voluntarily" and/or to claim disability benefits of some kind. Fluctuations in local labor demand impinge on the employment opportunities for anyone searching for a new job.

In this setting, true exogeneity of workplace-specific employment opportunities might be questioned as the quality of a firm's workforce also will affect its economic performance and, hence, the likelihood of laying off workers. Moreover, firm-specific employment opportunities may correlate with other disability risk factors related to, e.g., occupation and work practices. We address these possible problems by applying extensive controls for potentially confounding factors, by examining differences in employee composition between different types of firms, and through extensive robustness checks of our findings with respect to the composition of the analysis population. These checks include analyses where we focus on large firms only, as reverse causality is more likely to be a concern for small firms.

For mass layoffs, we have chosen a forward-looking setup and assess the impacts of closure and downsizing events over a four-year period after the base year. This is motivated by the idea that "early leavers" may have started the search for a new job in response to information about an impending mass layoff, leaving remaining workers at the time of mass displacement a selected subset of the original workforce; see Kuhn (2002) for a discussion. The downsizing indicators are computed in a similar fashion as in Rege et al. (2009, p. 764), i.e., as the percent change in the number of full-time equivalent workers between the start of each period and the date exactly four years later.[10] When a workplace is downsized by 100%, we have – in contrast to prior studies – collected direct information on the reason behind the closure, i.e., whether it resulted from a bankruptcy, a voluntary liquidation, or a takeover. Firms' profitability is measured by the annual rate of return on invested capital. We include both initial profitability (in the base year) and the change in profitability over the next four years as explanatory variables in our models.

In order to extract and isolate exogenous variation in local labor market tightness, we start out by constructing two individual and time-specific tightness indices; one reflecting the probability of *becoming* unemployed, the other reflecting the probability of *finding a new job* given unemployment. Gaure and Røed (2007) show that the transition rates between unemployment and employment capture the cyclical fluctuations in labor demand better than the corresponding rates of unemployment. Both indices are computed on the basis of auxiliary (logit) regression models. To be precise, let $u_{it}=1$ if person i becomes unemployed in period t and let $e_{it}=1$ if the unemployed person finds new work within one year. We then set up the following models:

$$\begin{aligned}&\Pr(u_{it}=1)=l(\phi_t+x_{it}\varphi_t),\\&\Pr(e_{it}=1|u_{it}=1)=l(\psi_t+x_{it}\pi_t),\\&t=1994-1996,1998-2000,2002-2004,\end{aligned}\tag{1}$$

where x_{it} includes a large set of individual characteristics (to be explained below) including type of work (educational attainment and industry) and region (travel-to-work area) of residence, and $l(.)$ denotes the logit function, $l(a)=\exp(a)[1+\exp(a)]^{-1}$. Based on these regressions we compute for all individuals and each of the three periods the predicted linear unemployment and reemployment propensity indices, $\widehat{\phi}_t+x_{it}\widehat{\varphi}_t$ and $\widehat{\psi}_t+x_{it}\widehat{\pi}_t$.[11] The two indices are by construction functions of individual covariates and will, at face value, *not* be independent of the error term in statistical models of individual disability program or labor market withdrawal propensities. As we explain below,

7 Temporary disability is measured as having spells of medical or vocational rehabilitation or at least six months of long-term sickness leaves during the four-year interval.

8 Being outside the labor force after four years is defined on the basis of social security and annual earnings data as either 1) having annual earnings or self-employment income below 144,000 NOK (2009 currency; approx 18,000 €) during the last calendar year, 2) receiving permanent disability or rehabilitation benefits in the month of December that year, or 3) receiving long-term sickness benefits in December and for at least six months out of four-year period ending that month. This definition ensures that individuals who either have earnings that are incompatible with self-sufficiency or are observed to rely on long-term social security transfers are classified as being outside the labor force.

9 Our measure of permanent disability also includes the formally time-limited disability benefit introduced in 2004.

10 Note that we do not exploit information on individual layoffs in order to avoid complications from selection bias in cases where some workers are retained by the firm (Henningsen and Hægeland, 2008).

11 The two indices are designed to measure labor market tightness in the first three years of each four-year period. We do not include the fourth year for the reason that labor market tightness is likely to affect the three outcome measures with some time lag.

Table 2
Employment opportunities — descriptive statistics.

	Men				Women			
Base year:	All	1993	1997	2001	All	1993	1997	2001
Observations	524,270	130,786	189,703	203,781	174,194	44,549	59,272	70,373
Age	39.4	39.3	39.1	39.9	38.7	37.9	38.6	39.3
Education								
Compulsory	24.3	26.8	24.8	22.2	24.4	27.9	24.9	21.7
Secondary	56.1	55.2	56.6	56.3	54.1	56.7	55.4	51.4
College/University	19.1	17.6	18.2	20.9	21.0	14.9	19.3	26.3
Earnings in base year (1000 NOK, 2009-value)	402	373	389	434	297	262	287	327
Percent subject to								
Closure w/ bankruptcy	2.6	1.4	2.6	3.2	1.8	1.0	1.8	2.3
10–20% downsizing	9.3	6.9	10.4	9.8	9.9	8.6	10.2	10.5
20–35% downsizing	8.9	5.3	10.5	9.6	10.1	7.4	11.5	10.6
35–99% downsizing	14.0	9.8	15.5	15.4	15.6	12.7	18.3	15.0
Liquidation	5.0	4.7	5.7	4.5	5.6	5.1	6.6	5.2
Takeover	10.0	9.3	12.7	7.9	10.9	10.6	13.5	9.0
Return on capital	0.072	0.079	0.091	0.055	0.072	0.087	0.086	0.050
Change return on capital	−0.008	−0.001	−0.047	0.023	−0.007	−0.012	−0.042	0.026
Risk of unemployment	14.9	14.9	13.2	16.4	15.9	16.7	14.1	17.0
Prob. of reemployment	68.8	73.8	70.8	64.5	58.1	57.0	61.8	56.2

Note: Individual characteristics (age, education, earnings) are measured in base year, while firm downsizing and closure indicators refer to four-year period following the base year.

we deal with this endogeneity problem by controlling for x_{it} in all analyses where the indices appear as explanatory variables, in essence isolating the variation in labor market opportunities that arise from time-varying *effects* of individual characteristics $(\widehat{\varphi}_t, \widehat{\pi}_t)$, in particular those driven by differences in cyclical conditions related to education, industry, and region.

Table 2 provides a descriptive overview of our analysis populations and the variables designed to represent change in individual employment opportunities. Males are strongly overrepresented in the dataset, reflecting our focus on full-time employees in the private sector. Workplace turbulence (in the form of downsizing, closure, or takeover) generally increased from the first to the second period, and declined slightly in the third period. An important exception to this pattern is the bankruptcy rate, which rose significantly over the full data period. Another important pattern to emerge from Table 2 is that takeovers make up a majority of the firm closures in the data. Around 18% of male and female full-time employees in our dataset work in a firm that "disappears" over the next four years,[12] but almost 60% of these jobs are subject to a firm takeover or acquisition and are therefore less likely to entail displacement than jobs in firms that go bankrupt. Note that while we, in cases of firm closure, can use the bankruptcy data to distinguish genuine mass layoffs from, e.g., takeovers and demergers, we are not able to make this distinction for more moderate downsizings. Hence, our downsizing indicators are likely to be "inflated" by organizational changes that do not really involve collective layoffs.

For our three ultimate outcome measures, we estimate the following models:

$$\begin{aligned}\Pr\left(y_{ijt}=1\right) &= l\left(\alpha_{jt}+z_{it}\delta_j+x_{it}\beta_j+\gamma_j\left(\widehat{\phi}_t+x_{it}\widehat{\varphi}_t\right)+\lambda_j\left(\widehat{\psi}_t+x_{it}\widehat{\pi}_t\right)\right)\\ &= l\left(\alpha^*_{jt}+z_{it}\delta_j+x_{it}\beta_j+\gamma_j x_{it}\widehat{\varphi}_t+\lambda_j x_{it}\widehat{\pi}_t\right),\\ \alpha^*_{jt} &= \alpha_{jt}+\gamma_j\widehat{\phi}_t+\lambda_j\widehat{\psi}_t,\end{aligned} \tag{2}$$

where $y_{ijt}(j = 1,2,3)$ denote the three dichotomous outcome indicators described in Table 1, observed for individual i in time period t. The vector z_{it} contains all workplace-specific covariates such as initial firm size, downsizing, closure, turnover, and profitability.[13] As explained above, the vector of individual characteristics (x_{it}) contains information about the (initial) type of work and region of residence. Since we do not have direct information about occupations, the type of work is proxied by a combination of educational attainment and industry (resulting in 21 different job type categories). In addition, we include information about age (i.e., 44 age dummies), nationality (eight classes), actual work experience (six classes), base year log earnings and the change in log earnings from the year prior to the base year, initial family situation (i.e., marital status, number of children and labor market status of the spouse; 10 categories), travel-to-work area (90 categories), and, for older workers, entitlement to early retirement. A complete listing of the explanatory variables (x_{it}, z_{it}) is provided in Appendix A.

A key point to note is that the coefficient vector $\{\beta_j,\gamma_j,\lambda_j\}$ in Eq. (2) can be separately identified only because there is time variation in the parameter estimates $\widehat{\varphi}_t$ and $\widehat{\pi}_t$. Without the t-subscript on these parameters, the regressors $x_{it}, x_{it}\widehat{\varphi}_t$, and $x_{it}\widehat{\pi}_t$ would be perfectly collinear. We have deliberately constructed the model this way in order to ensure that it is *only* the idiosyncratic changes in labor market tightness over time that identify the effects of employment opportunities on the risk of disability program entry and non-participation. In practice, the key source of identification is that different industries and economic regions were subject to different cyclical fluctuations during the three observation periods. For example, while employment opportunities in the manufacturing industries and in agriculture declined over time, particularly for workers with low educational attainment, the employment opportunities in retail, restaurants, and tourism improved.

Since an important aim of this paper is to assess the extent to which individual displacement affects the risk of subsequent disability insurance uptake, we place considerable emphasis on the effects of working in a firm that is going to close down due to bankruptcy over the upcoming four-year period. As Table 2 showed, in any of the three four-year intervals only between 1.0 and 3.2% of workers in our data actually experienced a bankruptcy. This does not imply, however, that displacements are rare. According to Salvanes (1997), as many as 10% of Norwegian jobs are eliminated in a typical year. We therefore expect displacement to be relatively common even

[12] In addition, there are some jobs in our dataset that seemingly disappear because of mismatches between firm identifiers in the two main data sources. Specifically, 1.16% of males and 1.25% of females work in firms that disappear from the employer–employee data during the upcoming four years but do not close down according to the accounting data; and 0.98 and 1.36% work in firms that vanish from the accounting data but not from the employer–employee data. We include these jobs in our analyses, but mark the observations as firm-identifier mismatches.

[13] For firms that close down during the period, we set the change in profitability equal to the sample mean in order to keep the observation in the analysis. Since we have separate dummy variables for firms that close down, this does not affect the estimated effects of the change in profitability, but it does imply that closure effects are measured relative to firms with mean change in profitability.

Table 3
Incidence of registered unemployment during four-year period and mean disability and participation outcomes by downsizing and closure status. Average over three sample periods.

	Men				Women			
	Registered unemployed, 4 yrs (%)	Temp or permanent disability, 4 yrs (%)	Out of labor force after 4 yrs (%)	Permanent disability, 6 yrs (%)	Registered unemployed, 4 yrs (%)	Temp or permanent disability, 4 yrs (%)	Out of labor force after 4 yrs (%)	Permanent disability, 6 yrs (%)
Closure w/ bankruptcy	56.5	18.8	18.8	4.9	62.2	24.7	27.9	4.3
No downsizing (<10%)	12.4	11.3	8.5	2.6	13.1	17.6	14.0	3.4
10–20% downsizing	17.9	13.1	11.3	3.4	19.1	20.1	16.9	4.0
20–35% downsizing	21.9	13.7	11.6	3.3	23.7	20.5	18.1	4.3
35–99% downsizing	26.5	14.0	13.0	3.7	29.5	19.9	19.8	4.3
Liquidation	19.6	10.5	10.4	2.6	25.3	18.5	17.6	3.5
Takeover	20.0	11.6	10.9	2.7	21.6	19.8	16.5	4.4

Table 4
Descriptive statistics by firm closure and downsizing status.

	Closure w/ bankruptcy	Liquidation or takeover	Downsizing	No downsizing(<10%)
Outcome (%)				
Temporary or permanent disability (4 yrs)	19.9	14.1	15.3	12.8
Out of labor force (4 yrs)	20.5	12.7	13.7	9.9
Permanent disability (6 yrs)	4.8	3.3	3.7	2.8
Sickness absence in base year (%)	11.9	10.7	11.5	10.4
Sickness absence yr before base yr (%)	9.6	9.1	9.7	9.0
Female (%)	18.9	26.8	26.5	23.9
Age	38.1	38.8	39.6	39.3
Education				
Compulsory	28.4	24.2	25.1	23.8
Secondary	56.7	54.5	55.1	56.1
College/University	14.1	20.9	19.3	19.6
Earnings in base yr (1000 NOK, 2009)	346	379	374	378
Plant size	61.6	109.4	146.5	110.9
Number of workers (all three periods)	16,462	107,409	195,047	379,546

Note: Sickness absence is recorded in a certain year if the person had at least one absence spell exceeding 16 days.

in stable or growing firms. Table 3 shows how the downsizing and closure indicators correlate with subsequent incidences of registered unemployment (within the corresponding four-year downsizing/closure period) in our data. With unemployment incidence rates of 57% for men and 62% for women, entry into registered unemployment is indeed *much* higher among workers exposed to a bankruptcy-driven closure than among other workers.[14] It is nonetheless clear from the table that unemployment is relatively frequent regardless of the type of downsizing event. The table also reveals that the prevalence of our disability and non-participation outcome measures are higher for workers that faced workplace restructuring than for workers in stable or growing firms, and that, at least for men, the bankruptcy category stands out with high future incidence rates of disability program entry and labor force withdrawal.

To obtain a rough estimate of the overall level of displacements in our own data, we use the unemployment frequencies reported in Table 3 as a starting point. If we assume that all employees in the "closure with bankruptcy" category are actually displaced, we can infer that 56.5% of displaced male workers and 62.2% of displaced female workers register as unemployed during the four-year period in question. If we assume that these same propensities to register for unemployment also apply to workers who lose their job in other (non-bankruptcy) firms, we can use the numbers listed in Table 3 to back out the total number of job losses in our data. Doing this exercise separately for men and women, we estimate that around 31% of both male and female employees in our dataset lose their job over a four-year period.[15] Even in the no-downsizing bracket (<10%), we find that the four-year job-loss rate is 22% for men and 21% for women. To the extent that we interpret the effects of working in a bankruptcy-exposed firm – as opposed to working in a firm with no downsizing – as representing the causal effect of displacement, our estimates will thus clearly be subject to *contamination bias* (Heckman and Robb, 1985). We return to the issue of contamination bias in Section 5.2 below.

As stressed by Rege et al. (2009), the estimated impact of firm closure may be affected by *selection bias* if workers in closing firms differ systematically from workers in continuing firms. Table 4 provides descriptive statistics for the workforces of firms in the various downsizing categories. These statistics show that there are in fact large differences in worker composition across categories. In particular, bankruptcy

[14] It is of interest to note that liquidations seem to involve unemployment entries at the same level as relatively small downsizings. This suggests that liquidations lead to fewer displacements than bankruptcies, although both events involve firm closure. Probable reasons for this pattern is that the classification "liquidated firms" contains some false closures and that an organized liquidation gives more room for maintaining viable economic activities within new firm structures compared to an outright bankruptcy.

[15] The assumption that the propensity for unemployment registration is the same for all types of job loss is of course questionable. On the one hand, one could argue that the marginal employee in a stable firm has weaker labor market prospects than the average employee displaced from a bankrupt firm. Moreover, selective layoffs may carry a stigma and serve as an adverse signal about an employee's productivity; see Gibbons and Katz (1991). These factors imply higher unemployment registration propensities for job losses in stable firms, and thus fewer actual job losses behind a given number of registered unemployed. On the other hand, job losses in continuing firms are typically announced well in advance of the event, leaving displaced workers with more time to search for new jobs and hence avoid being registered as unemployed. And congestion effects in local labor markets may imply that mass layoffs have larger adverse consequences than other layoffs. Such factors suggest higher registration frequencies for job losses in closing firms. It is also worth noting that our 31% estimate is only slightly below what would be expected on the basis of the 10% annual job elimination rate reported by Salvanes (1997), which – provided that the risk is independently distributed across individuals over time – yields a 35% cumulative displacement rate over a four-year period ($1 - 0.9_4$).

Table 5
Estimated percentage point impacts of employment opportunities on disability program entry and non-participation.
Average marginal effects (robust standard errors in parentheses).

	Men			Women		
	Temp. or permanent disability 4 yrs	Out of labor force 4 yrs	Permanent disability 6 yrs	Temp. or permanent disability 4 yrs	Out of labor force 4 yrs	Permanent disability 6 yrs
Closure with bankruptcy	4.72	6.99	2.02	4.30	9.57	1.23
	(0.53)	(0.46)	(0.23)	(0.79)	(0.86)	(0.40)
No downsizing (<10%)	Ref.	Ref.	Ref.	Ref.	Ref.	Ref.
10–20% downsizing	0.46	1.48	0.37	1.14	1.52	0.18
	(0.22)	(0.35)	(0.09)	(0.38)	(0.36)	(0.16)
20–35% downsizing	1.05	1.65	0.45	1.09	2.07	0.41
	(0.20)	(0.21)	(0.10)	(0.37)	(0.36)	(0.18)
35–99% downsizing	1.68	2.89	0.86	0.75	3.96	0.72
	(0.21)	(0.26)	(0.10)	(0.32)	(0.35)	(0.16)
Liquidation	0.78	3.04	0.68	1.63	4.52	0.77
	(0.29)	(0.32)	(0.15)	(0.49)	(0.53)	(0.24)
Takeover	0.01	0.39	0.07	1.30	0.90	0.21
	(0.20)	(0.25)	(0.08)	(0.34)	(0.33)	(0.15)
Initial rate of return on capital[a]	−0.12	−0.58	−0.09	−0.60	−0.46	−0.11
	(0.08)	(0.19)	(0.03)	(0.13)	(0.14)	(0.06)
Change in return on capital[a]	−0.21	−0.41	−0.06	−0.30	−0.39	−0.09
	(0.08)	(0.15)	(0.03)	(0.13)	(0.14)	(0.06)
Risk of unemployment[a]	1.67	−0.07	0.47	1.57	−0.30	0.23
	(0.29)	(0.28)	(0.13)	(0.46)	(0.42)	(0.20)
Probability of reemployment[a]	−0.72	−1.52	0.10	−0.62	−2.23	−0.29
	(0.23)	(0.20)	(0.09)	(0.46)	(0.40)	(0.17)
Percent with outcome = 1	12.22	10.10	2.96	18.81	16.23	3.77

Number of observations: 524,270 (men) and 174,194 (women). Standard errors are clustered within 34,620 (men) and 29,700 (women) firm-by-period cells. The following controls are included in the regressions (number of categories for categorical variables in parentheses): Education/industry (21), age (44), nationality (8), actual work experience (6), initial level and change in log earnings, family situation (10), region of residence (90), size of municipality (5), firm size (4), employee turnover in base year (5), time period (3), firm-identifier mismatch (3), and, for old workers, entitlement to early retirement programs (2).

[a] The variables are standardized, such that they are centered on zero and have a unit standard deviation. Marginal effects are calculated as the effect of a one standard deviation change in the explanatory variable.

firms have fewer female employees, lower fractions of highly educated workers, and lower average earnings than stable firms. Bankruptcy firms also tend to be smaller than other firms. Given the sample sizes reported at the bottom of the table, these differences cannot be attributed to randomness alone; hence they must be accounted for in the empirical analysis. For the analysis, it would be of concern if workers' reliance on health-related benefits in bankruptcy firms deviated from that in other firms even prior to the start of the analysis period. As our analysis samples are conditioned on not having received any *long-term* health benefits prior to the outcome period, such sorting problems should primarily show up in observed short-term benefits, i.e., sick pay. The numbers in Table 4 indicate that the rate of sickness absence during the base year indeed is somewhat higher in bankruptcy firms than in other firms. The year before the base year, however, there are only minor differences between the different firm types. A possible interpretation of these patterns is that the higher absence rate in soon-to-go-bankrupt firms reflects that the downsizing process has already started in some of these firms.

To formally test for whether employees in closing firms, conditional on our explanatory variables, have higher initial absence rates than employees in stable or growing firms, we estimate separate models with indicators for sickness absence in the base year and in the year before the base year, respectively, as the dependent variable. The models are formulated exactly as the models we use for other outcome variables and include the same control variables (see Eq. (2)). Results (not reported in tables) show that the estimated average marginal effect of working in a closing (bankruptcy) firm on absenteeism in the base year is equal to 0.86 percentage points for men (t-value = 2.69) and −0.19 percentage points for women (t-value = −0.80). For the year before the base year, however, we fail to uncover significant differences across firm types; 0.29 percentage points (t-value = 1.28) for men and 0.26 percentage points (t-value = 0.33) for women. We interpret these findings as supporting evidence for the hypothesis that the higher absence rate in the base year in soon-to-go-bankrupt firms captures an early causal effect of the turbulence and stress associated with the forthcoming closure; see Røed and Fevang (2007). The failure to identify significant differences in the year prior to the base year indicates that compositional differences by closure status is not driven by sorting of employees across firms. We nevertheless return to the issue of sorting in terms of past sickness absence in the robustness exercises below.

5.2. Results from the baseline model

Table 5 presents our key results regarding the impacts of employment opportunity on subsequent disability program entry and non-participation for men and women, respectively. For ease of interpretation, we report average marginal effects (multiplied by 100); i.e., the mean percentage point impact of the explanatory variable on each of the three outcome probabilities. Average marginal effects are computed on the basis of relevant comparisons only; for dummy variable sets with more than two categories, each category's average marginal effect is calculated for observations belonging to the category in question and the reference category only (see Bartus, 2005). A complete listing of estimated coefficients is available from the authors.[16]

As Table 5 shows, employment opportunities have large and statistically significant effects on disability program entry and non-employment propensity. For both men and women, the probability of claiming permanent disability benefits after six years, and the

[16] In order to account for any covariance between employees working at the same establishment (and to correct for Moulton (1986) bias), we cluster standard errors within firm-by-period cells. Were we instead to cluster at the establishment level (to also account for any serial correlation across periods), standard errors would be slightly larger than those reported in the tables. To illustrate, the standard error of the coefficient of the bankruptcy variable in the male permanent disability logit equation becomes 0.06118 (21,332 cluster units) as opposed to 0.06082 (34,620 clusters). Note also that the three periods will contain multiple observations of some of the workers in our sample (the baseline samples consist of 524,270 observations of 347,748 males and 174,194 observations of 128,391 females). Using clustering to account for serially correlated errors among individuals with multiple observations raises standard errors by an even smaller amount than clustering within firms.

likelihood of being out of the labor force after four years, rise monotonically with the level of workplace downsizing, ceteris paribus. All three outcome propensities decline with the employer's economic performance and, at least for men, with improvements in local labor market tightness as captured by the risk of unemployment and reemployment variables.

As explained above, our most reliable indicator of *exogenous displacement* is the "closure with bankruptcy during the next four years" variable. As shown in Table 5, such an event raises a male worker's probability of claiming *permanent* disability benefits after six years by 2.0 percentage points when compared to working in a stable or growing firm with average profitability. Given the large and variable time lags in entry into permanent disability status described in Section 2, and because virtually all permanent disability benefit claims are preceded by extended periods on temporary disability benefits and/or by self-supported periods outside the labor force, it is of interest to examine the impacts on these outcomes as well. According to the estimates in Table 5, a bankruptcy raises a male full-time worker's probability of claiming either temporary *or* permanent disability benefits by 4.7 percentage points and the probability of labor force withdrawal (measured four years after the base year) by 7.0 percentage points. These large additional flows into temporary disability and non-participation show that the 2.0 percentage point rise in the permanent disability program participation rate identified after six years does not capture the full effect of displacement.

The effects of job loss on disability insurance claims and non-participation are large for women as well, though generally smaller than those for men when measured relative to the average outcome within gender. For a female full-time worker, bankruptcy raises the risk of permanent disability program entry by around 1.2 percentage points. The risk of temporary or permanent disability rises by around 4.3 percentage points. One reason why the effects tend to be smaller for women than for men, may relate to gender differences in mental distress associated with unemployment – and perhaps not being able to fulfill the traditional breadwinner role – a point to which we return in Section 5.5 below. It is worth noting that the overall impact of bankruptcy on the probability of non-participation is larger for women than for men; the likelihood of non-participation following bankruptcy goes up by 9.6 percentage points for women (compared to 7.0 for men). But, because our analysis covers private sector employees only – leading to a huge overrepresentation of men – some caution is warranted when interpreting gender differences in effect estimates.

The estimates listed in Table 5 show the effect of working in a bankruptcy firm as opposed to a stable or growing firm, and not the effect of displacement per se. We can nevertheless use the estimated effects to evaluate the underlying causal impacts of displacement. As we argued in Section 5.1, displacement is relatively common even in stable and moderately downsizing firms. This implies that the estimated effects of closure with bankruptcy reported in Table 5 in fact understate the causal effects of displacement. Adjusting the point estimates for contamination bias caused by inclusion of treated (i.e., displaced) employees in the non-treatment (no downsizing) group, we find that displacement on average raises the permanent disability program propensity for men by 2.6 percentage points (121%) and by 1.6 percentage points (48%) for women.[17] Likewise, the risk of temporary or permanent disability following job loss rises by 6.0 percentage points (60%) for men and by 5.5 percentage points (32%) for women. Finally, accounting for contamination bias, displacement raises the non-participation propensity by 9.0 percentage points (123%) for men and by 12.1 percentage points (98%) for women. Based on the (admittedly questionable) assumptions that these effects are representative for all displaced workers in our dataset and that our estimate of the overall number of job losses is correct (see Section 5.1), we estimate that displacements account for fully 28% of all new permanent disability benefit claims among males and for 13% among females (see footnote 17 for the exact calculations). Similarly, we find that for men (women), displacements account for 28 (23) percent of transitions to non-employment and for 16 (9) percent of transitions to temporary or permanent disability programs.

The economic performance of surviving firms – as measured by the annual return on their capital base – also has statistically significant effects on transitions into disability programs and non-participation (conditional on the observed level of downsizing). Although the effects on disability benefit claims are moderate in size, they are far from negligible. For example, a one-standard-deviation deterioration in initial profitability and its four-year change will raise the female entry rate into temporary or permanent disability by 0.9 percentage point (0.6+0.3). Our interpretation of this finding is that poor economic performance of the employer does entail small-scale displacement and places pressures on employees with poor health.

Local industry-specific labor market conditions significantly affect transitions into disability programs and non-participation. For example, a one standard deviation increase in the unemployment incidence index raises the likelihood of entering a temporary or permanent disability program by 1.7 percentage points for both men and women (around 14% for men and 9% for women). A negative shock to the local labor market resulting in higher unemployment risk and reduced likelihood of reemployment (both of a magnitude of one standard deviation) is predicted to raise the inflow rate to permanent disability by 0.4 percentage point (i.e., by 14%) for men and 0.5 percentage point (also 14 percent) for women.

Our estimated displacement effects are considerably larger than those reported in two recent studies also based on Norwegian register data. Rege et al. (2009) find that workers originally employed in plants that downsized by more than 60% between 1995 and 2000, were 24% more likely to utilize disability pensions in 2001 than comparable workers in non-downsizing plants. And Huttunen et al. (2011), who define displaced individuals as workers who separate from plants that reduce employment by 30% or more, report that the probability of being outside the labor force is 3.4 percentage points higher seven years after displacement than for otherwise similar, but non-displaced, workers. When we replicate the definition of downsizing used by Rege et al, we also replicate their main result.[18] The implication is that the conventional definition of downsizing and closure based on employer–employee data imparts attenuation bias in estimates. Although both studies take steps to eliminate false downsizings and/or focus on high-seniority workers, register-based downsizing indicators will invariably capture some false downsizings and closures related to outsourcing, demergers, and other forms of organizational change. Moreover, some separations are voluntary, even when they occur in downsizing firms. In fact, the authors point out themselves that their strategies for identifying displacement will involve some misclassifications. Our results, showing much larger effects of displacement on disability benefit uptake and labor market withdrawal, suggest that this indeed is the case.

[17] We adjust for contamination bias by dividing the estimated average marginal effect of "closure with bankruptcy" by the estimated fraction of non-displaced workers in non-downsizing firms. To illustrate, for men the adjusted effect is calculated as $2.02/(1-0.22)=2.59$, where 0.22 is the estimated fraction of displacement over the four-year interval among males in non-downsizing firms; see Section 5.1. We compute the counterfactual disability entry rate – the rate that would have prevailed in the absence of any displacements – as the actual entry rate minus the product of the estimated average effect of displacement and the computed overall rate of displacements. In the example given for men, this yields a counterfactual non-displacement disability rate of 2.14. As the observed rate in the data is 2.96 (see the bottom row of Table 5), we estimate the fraction of overall disability entries that can be attributed to displacements to be $(2.96-2.14)/2.96=0.28$.

[18] Rege et al. (2009) report an estimated odds-ratio associated with 60–100% downsizing of 1.30. Our own corresponding estimate is 1.31.

Table 6
Heterogeneous effects of bankruptcy.
Average marginal effects (robust standard errors in parentheses).

	Men			Women		
	Temp. or permanent disability 4 yrs	Out of labor force 4 yrs	Permanent disability 6 yrs	Temp. or permanent disability 4 yrs	Out of labor force 4 yrs	Permanent disability 6 yrs
Bankruptcy	4.33	9.69	0.49	7.35	12.44	−0.13
	(1.98)	(2.25)	(0.36)	(4.97)	(5.71)	(0.85)
Reemployment index[a]	−0.90	−0.97	0.00	−0.56	−2.07	−0.11
	(0.21)	(0.17)	(0.03)	(0.45)	(0.39)	(0.07)
Bankruptcy ⁎ reemployment[a]	−1.19	−0.75	−0.13	−3.13	−3.77	−0.23
	(0.52)	(0.38)	(0.06)	(1.43)	(1.35)	(0.18)
Bankruptcy ⁎ (age > 50)	0.62	1.85	0.05	−2.27	0.61	0.01
	(0.80)	(0.74)	(1.17)	(2.14)	(2.24)	(0.37)
Early retirement elig.	−3.12	5.92	−0.42	−3.23	9.26	−0.57
	(0.25)	(0.48)	(0.02)	(0.71)	(1.01)	(0.06)
Bankruptcy ⁎ early retire't elig	−2.62	−1.02	−0.42	−5.59	1.36	−0.56
	(1.25)	(0.96)	(0.10)	(4.56)	(6.39)	(0.41)
Log earnings base yr	−3.68	−4.53	−0.62	−1.39	−7.20	−0.52
	(0.20)	(0.16)	(0.04)	(0.39)	(0.34)	(0.07)
Bankruptcy ⁎ log earn base yr	4.09	2.44	0.50	5.55	5.29	0.71
	(0.67)	(0.47)	(0.14)	(1.74)	(1.39)	(0.38)

Control variables include the downsizing, closure, and firm characteristics listed in Table 5 as well as all controls listed in note to Table 5. In addition, the regressions control for interactions between bankruptcy and education/industry, nationality, work region, municipality, firm size and turnover, and time period. The baseline bankruptcy effect is evaluated for a native-born, low-educated manufacturing worker in Oslo and employed in a small firm with low turnover during the first observation period of the study. See also notes to Table 5.
[a] Marginal effects are calculated as the effect of a one standard deviation change in the explanatory variable.

5.3. Heterogeneous effects

According to the theory outlined in Section 3, substitutability between unemployment and disability insurance schemes implies that there is an interaction effect between displacement and local industry-specific labor market tightness. In particular, a prediction from the framework is that the risk of disability benefit uptake following displacement will be higher when it is difficult to find a new job. To examine this possibility – and also investigate the existence of other potential heterogeneous effects – we have estimated models that allow for interactions between the bankruptcy variable and labor market and individual characteristics. Table 6 presents some key results (the full set of results is available from the authors). As predicted, disability benefit uptake depends on local labor market conditions, and poor employment prospects aggravate the adverse effect of displacement. In fact, the coefficient of the interaction term between closure with bankruptcy and the reemployment index is negative for all outcome measures and for both genders. To illustrate, a one standard deviation increase in the reemployment index reduces the probability that a bankruptcy-affected male worker receives temporary or permanent disability benefits by a statistically significant 1.2 percentage point and that of a female worker by as much as 3.1 percentage points. The evidence is thus consistent with the conclusion of Couch and Placzek (2010) that the adverse consequences of job loss are greater during economic downturns.

The table further shows that transition rates to disability programs and out of the labor force following displacement are slightly higher for older workers. This conclusion is turned upside down, however, for workers eligible for early retirement. For the latter group of workers, there does not seem to be any effect of displacement on disability program entry at all, indicating a strong element of yet another social program substitutability, this time between early (state subsidized) retirement and disability pensions. This interpretation is reinforced by the coefficient estimates showing that, among displaced workers, those eligible for early retirement are less likely to enter disability programs, but much more likely to leave the labor force than workers not eligible for early retirement. Another point to note from Table 6 is that there is a tendency for "the social gradient" in disability program entry to be weaker for the flows generated by mass layoffs. This is illustrated by the impacts of prior earnings. In general, there is a strong negative relation between prior earnings and the likelihood of disability benefit uptake, particularly for men. The relation likely reflects heterogeneity in health – in that poor health causes both low earnings and disability – and that the opportunity costs of disability program enrollment are larger for workers with high earnings. Interestingly, this relationship vanishes in bankruptcy firms. Upon job loss, the local labor market opportunities apparently become more important relative to individual background characteristics, again supporting the notion of unemployment-disability substitution.[19]

5.4. Robustness analyses

Even though the results presented in Table 5 account for a rich set of control variables, we cannot a priori rule out that employees in downsizing and closing firms differ systematically from employees in stable or growing firms. For example, the layoff process in closing firms may have started during or before the base year, leaving a selected group of employees in terms of unobserved disability risk. Moreover, there is the concern of reverse causality: If many workers in a small firm become disabled, this may have detrimental effect on the firm's economic performance, and can – at least for small firms – even cause bankruptcy.

Tables 7 and 8 report the estimated average marginal effects of our key explanatory variables from a number of robustness exercises for men and women, respectively. To ease comparisons, in column I we first list the estimates from the baseline model. In column II, we examine whether the estimated effects of bankruptcy are impacted by inclusion of the firm profitability and local labor market tightness measures in the empirical model. The results show that this is not the case — if anything, dropping these measures raises the estimated impact of bankruptcy. Column III lists the estimated effects based on employees in the restricted sample of firms that did not downsize at all during the two years prior to the outcome period. If our results were driven by early sorting caused by an ongoing downsizing process, we would expect estimates to be sensitive to this sample condition. As it turns out, they are

[19] The attempts at tightening gate-keeping referred to in Section 2 might be expected to have affected caseworkers' scopes for considering applicants' employment prospects and thus reduced the effect of job loss over time; see Gruber and Kubik (1997), Campolieti (2004), and de Jong et al. (2011). Although not statistically significant, results indicate somewhat lower bankruptcy effects for men towards the end of our sample period. For women, we do not uncover any systematic differences in estimated bankruptcy effects across the three periods.

Table 7
Robustness analysis for men. Estimated percentage point impacts of employment opportunities on temporary or permanent disability program entry and non-participation. Average marginal effects (AME).

	I Baseline model	II Omit profits and labor demand indices	III Firm size stable last two years	IV More than 50 employees	V No welfare benefits prior two years	VI With controls for past absence	VII Include multi-plant firms	VIII Include region-specific time dummies	IX Employed in the same firm prior five years
Observations	524 270	524 270	489 368	232 684	388 592	524 270	1 137 749	524 270	208 311
A) Temporary or permanent disability program after 4 years									
Closure w/ bankruptcy	4.72	4.90	4.55	4.30	4.45	4.44	4.35	4.68	6.68
Return on capital	−0.12		−0.14	0.00	−0.07	−0.11	−0.19	−0.12	−0.21
Change in ret. capital	−0.21		−0.24	−0.06	−0.15	−0.22	−0.19	−0.18	−0.13
Risk of unempl. index	1.67		1.65	2.09	1.42	1.62	1.15	1.97	1.69
Prob. of reempl. index	−0.72		−0.77	−0.28	−0.59	−0.55	−0.74	−0.81	−0.71
Percent w/ outcome = 1	12.22	12.22	12.18	12.49	8.44	12.22	11.46	12.22	12.87
B) Out of labor force after 4 years									
Closure w/ bankruptcy	6.99	7.82	6.95	7.26	6.28	6.80	6.75	7.01	9.17
Return on capital	−0.58		−0.42	−1.08	−0.59	−0.58	−0.34	−0.52	−0.97
Change in ret. capital	−0.41		−0.33	−0.69	−0.40	−0.42	−0.22	−0.35	−0.59
Risk of unempl. index	−0.07		0.08	−0.60	−0.16	−0.10	−0.43	−0.18	−1.41
Prob. of reempl. index	−1.52		−1.58	−1.61	−1.36	−1.42	−1.66	−1.60	−1.56
Percent w/ outcome = 1	10.10	10.10	9.98	10.71	7.63	10.10	9.82	10.10	10.15
C) Permanent disability program after 6 years									
Closure w/ bankruptcy	2.02	2.15	1.83	2.50	1.76	1.95	1.86	2.01	3.55
Return on capital	−0.09		−0.08	−0.11	−0.06	−0.09	−0.08	−0.08	−0.09
Change in ret. capital	−0.06		−0.07	−0.03	−0.05	−0.07	−0.10	−0.05	−0.06
Risk of unempl. index	0.47		0.44	0.65	0.31	0.45	0.30	0.53	0.75
Prob. of reempl. index	0.10		0.10	0.22	−0.02	0.13	−0.03	0.12	0.26
Percent w/ outcome = 1	2.96	2.96	2.90	3.22	2.03	2.96	2.94	2.96	4.23

not. Column IV presents estimates for employees in large firms only (more than 50 employees). If our results reflected reverse causality, the estimated impacts should drop significantly when we restrict the sample to employees in large firms. They do not.

Columns V and VI report estimates based on the sample limited to workers without welfare benefits at all during the past two years, and estimates based on the full sample, but with additional controls included for past absences (in the form of dummy variables indicating incidences of long-term absence in the base year and in the year before the base year), respectively. If our results were driven by systematic sorting of employees with poor health into bankruptcy firms, the estimated impacts of bankruptcy should drop in these exercises. Once again, they do not.

Column VII presents estimates based on the extended sample of workers employed in multi-plant as well as single-plant firms. If workers in single-plant firms differ systematically from those in multi-plant firms, our results might not generalize to workers at large. Effect estimates based on the extended sample change only marginally relative to the baseline, though, and the slight decline in the estimated effect of bankruptcy is consistent with our presumption that bankruptcies in large (multi-plant) companies often entail the continuation of some of the plants' economic activities, and hence that bankruptcy is a less precise indicator of job loss in multi-plant than in single-plant firms.

Column VIII lists estimates from a model where we have allowed the time dummy variables to vary by region (with the country divided into five regions). If there were regional trends in disability uptake not caused by business cycle developments, our baseline model could confound such trends with business cycle effects. As it turns out, when we allow for region-specific trends, the within-region estimates of labor market tightness effects are, if anything, larger than the estimates of the baseline model. Again, deterioration of local re-employment opportunities raise the probability of disability program entry.

Finally, column IX presents estimates based on reduced samples conditioned on stable employment in the same firm for at least five years. In the literature, restricting the sample to high-seniority workers is a common practice, typically for reasons of eliminating voluntary quits and firings for cause from the group of displaced workers.[20] It is also probable that job loss is a more severe shock for high-seniority workers with more job-specific human capital and a stronger expectation of remaining in their current job than for recent hires. As the column shows, the estimated impacts of bankruptcy rise significantly when we impose the seniority restriction. While the pattern to some extent is explained by much lower contamination of displacements in the reference group of stable firms (not shown), the substantial difference from the baseline nonetheless indicates that the adverse effects of job loss increase with seniority. An implication for the empirical job-loss literature is that studies that focus on high-seniority workers may exaggerate the average impact of worker displacement.

The main message coming out of the robustness exercises is that the estimated marginal effects from our baseline model are highly robust with respect to data delimitation and model specification. If anything, the estimated bankruptcy effects from our baseline model turn out to be on the conservative side; most of the robustness exercises yield stronger effects. For the other parameters of interest (i.e., the coefficients of the profitability and labor market tightness variables), there are only minor variations across the different model specifications and samples.

5.5. *Effects on health*

Our finding that employment opportunities have a strong impact on subsequent disability benefit claims does not necessarily imply that the disability status results *directly* from unemployment only. Previous evidence from Norway suggests that job loss adversely affects employees' physical and mental health conditions (Rege et al., 2009), and

[20] A number of studies adopt the U.S. Bureau of Labor Statistics definition of displacement and limit samples to workers with at least three years of seniority (Fallick, 1996). See also the discussions of high vs. low tenure workers and the implications for measurement of displacement effects in Jacobson et al. (1993) and von Wachter et al. (2009).

Table 8
Robustness analysis for women. Estimated percentage point impacts of employment opportunities on temporary or permanent disability program entry and non-participation. Average marginal effects (AME).

	I Baseline model	II Omit profits and labor demand indices	III Firm size stable last two years	IV More than 50 employees	V No welfare benefits prior two years	VI With controls for past absence	VII Include multi-plant firms	VIII Include region-specific time dummies	IX Employed in the same firm prior five years
Observations	174 194	174 194	135 216	73 063	119 858	174 194	401 060	174 194	61 320
A) Temporary or permanent disability program after 4 years									
Closure w/ bankruptcy	4.30	4.98	4.10	2.47	4.75	4.21	3.59	4.34	6.09
Return on capital	−0.60		−0.60	−0.78	−0.43	−0.50	−0.36	−0.54	−0.91
Change in ret. capital	−0.30		−0.27	−0.50	−0.38	−0.30	−0.13	−0.25	−0.43
Risk of unempl. index	1.57		1.73	1.68	1.46	1.38	1.32	1.68	1.25
Prob. of reempl. index	−0.62		−0.57	−0.56	−0.77	−0.64	−0.77	−0.96	−0.75
Percent w/ outcome = 1	18.81	18.81	18.74	19.81	13.40	18.81	18.60	18.81	18.97
B) Out of labor force after 4 years									
Closure w/ bankruptcy	9.57	10.22	9.27	8.99	8.69	9.52	9.28	9.62	13.89
Return on capital	−0.46		−0.40	−0.88	−0.29	−0.40	−0.20	−0.41	−0.81
Change in ret. capital	−0.39		−0.34	−0.56	−0.48	−0.39	−0.05	−0.33	−0.68
Risk of unempl. index	−0.30		−0.16	−0.89	−0.36	−0.41	−0.06	−0.75	−1.40
Prob. of reempl. index	−2.23		−2.40	−2.76	−1.91	−2.24	−2.02	−2.59	−1.58
Percent w/ outcome = 1	16.23	16.23	16.11	16.06	12.57	16.23	15.61	16.23	15.04
C) Permanent disability program after 6 years									
Closure w/ bankruptcy	1.23	1.36	1.35	1.86	1.62	1.20	1.32	1.27	3.24
Return on capital	−0.11		−0.07	−0.16	−0.06	−0.09	−0.02	−0.10	−0.32
Change in ret. capital	−0.09		−0.09	−0.07	−0.04	−0.09	−0.01	−0.08	−0.01
Risk of unempl. index	0.23		0.23	0.02	0.15	0.24	0.07	0.33	0.44
Prob. of reempl. index	−0.29		−0.29	−0.38	−0.13	−0.29	−0.18	−0.29	−0.22
Percent w/ outcome = 1	3.77	3.77	3.71	3.94	2.59	3.77	3.74	3.77	6.17

evidence from Sweden indicates that it significantly increases the risk of hospitalization due to alcohol-related conditions (Eliason and Storrie, 2009a). There is also empirical evidence showing that the mental distress associated with unemployment typically is more severe for men than for women; see Waters and Moore (2002), McKee-Ryan et al. (2005), or Kuhn et al. (2009). More generally, recent empirical studies find that work tends to be a healthy activity, particularly for workers with illnesses that are responsible for the majority of disability insurance claims in advanced economies, such as musculoskeletal pain and mental disorder; see, e.g., Waddel (2004), Waddell and Burton (2006), and OECD (2008). Markussen et al. (2013) show that continued work during episodes of long-term illness in most cases improves future labor market prospects.

Table 9
Estimated percentage point impacts of employment opportunities on mortality six years after base year. Average marginal effects (robust standard errors in parentheses).

	Men	Women
Closure with bankruptcy	0.26	0.06
	(0.10)	(0.15)
No downsizing (<10%)	Ref.	Ref.
10–20% downsizing	0.11	0.13
	(0.05)	(0.07)
20–35% downsizing	0.09	0.09
	(0.06)	(0.07)
35–99% downsizing	0.09	0.06
	(0.05)	(0.06)
Liquidation	0.08	−0.10
	(0.08)	(0.08)
Takeover	0.10	0.01
	(0.05)	(0.06)
Initial rate of return on capital[a]	0.00	0.00
	(0.02)	(0.02)
Change in return on capital[a]	−0.05	0.01
	(0.02)	(0.02)
Risk of unemployment[a]	0.13	−0.06
	(0.07)	(0.08)
Probability of reemployment[a]	0.02	−0.04
	(0.05)	(0.07)
Percent with outcome = 1	1.09	0.58
Observations	527,684	174,781

[a] Marginal effects are calculated as the effect of a one standard deviation change in the explanatory variable. See also notes to Table 5.

To check for possible health effects of job loss, we extend our samples and include workers who otherwise satisfy initial sample criteria (e.g., age 20–63 in the base year), but who died during the six-year outcome period. We next estimate the impacts of employment opportunities on mortality in exactly the same manner as we have estimated the impacts on other outcome measures. The results reported in Table 9 show that displacement appears to raise mortality for men but not for women. Adjusting the estimated bankruptcy effect for contamination bias caused by inclusion of displaced employees in the control group (non-downsizing firms), we find that displacement raises the six-year mortality rate for men by 0.33 percentage points (34 percent). This implies that around 10% of the deaths among male workers in our data can be attributed to job displacement. A general deterioration of local industry-specific risk of unemployment also tends to raise mortality among men. For women, coefficient estimates of the downsizing variables (without closure) are similar in size to those for men. The latter is consistent with large literature indicating that the uncertainty associated with organizational change adversely affects the health of retained employees; see, e.g., Ferrie (2001) and Røed and Fevang (2007), the latter for recent Norwegian evidence.[21]

6. Concluding remarks

We have shown in this paper that negative shifts in employment opportunities explain significant shares of non-participation and disability insurance dependency in Norway. The causal relationship

[21] The causal link between displacement and mortality risk has also been studied in other countries. For example, Eliason and Storrie (2009b) and Sullivan and von Wachter (2009) report mortality effects among displaced male workers in Sweden and Pennsylvania that are larger than those of the present study. Martikainen et al. (2007) uncover an association between unemployment and mortality risk in Finland, but argue that there is no excess mortality among displaced workers.

between employment opportunities and disability program entry is particularly strong for male workers. According to our baseline estimates, job loss more than doubles the risk of subsequent program entry for men, while raising enrollment by approximately 50% for women. These effects are considerably larger than those of prior studies. We find that the conventional measures of downsizing and firm closures used in employer–employee data impart attenuation bias in estimates, which explains the discrepancy across studies.

For men, we have uncovered evidence that a portion of the job loss effect can be explained by adverse health consequences. For women, no such health effects have been identified. These findings are in accordance with previous evidence indicating that the adverse health impacts of job loss are indeed more severe for men than for women. For both genders, we have found that the impacts of job loss on subsequent disability program entry are larger the worse are local labor market conditions. Moreover, the development of local labor market conditions as well as of the current employer's profitability have distinct impacts on the employees' risk of disability program entry. A probable explanation is that management may coerce workers to apply for disability insurance benefits as a way of cutting costs without having to resort to layoffs, and that their incentives for pursuing such strategies rise in times of low profitability and adverse local economic conditions.

Taken together, the evidence presented in this paper points to a considerable element of substitutability between unemployment and disability insurance. Our findings suggest that the process of reallocating redundant workers from old to new employers is far from seamless, and that many displaced workers permanently change status from supporting the welfare state to becoming supported by it. Significant human capital resources are squandered in this process. The finding that loss of employment is among the major causes of disability program entry – whether it stems from genuine health effects or from adverse shocks to the expected value of labor market participation for given health levels – suggests that appropriate solutions to the "disability problem" should address strategies for improving the employment opportunities of potential claimants rather than focus exclusively on income insurance. If job loss and unemployment are among the root causes of the rising disability problem, it is probable that provision of employment opportunities is among its remedies.

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Appendix A

List of explanatory variables used in the baseline model:

Age in base year: 44 dummy variables; one for each age 20–63.
Marital status in base year: 4 dummy variable; single, married, divorced, widow(er).
Children: 3 dummy variables; No children, 1–2 children, 3 + children.
Spouse/family situation: 3 dummy variables; spouse home, spouse home ⁎ 1–2 children, spouse home ⁎ 3 + children.
Education/industry: 21 dummy variables: low/primary, low/manufacturing, low/retail, low/hotel/restaurant, low/transport, low/finance, low/education, low/health, low/other, medium/primary, medium/manufacturing, medium/retail, medium/hotel/restaurant, medium/transport, medium/finance, medium education, medium/health, medium/other, bachelor degree, graduate school, education missing.
Work experience: 6 dummy variables; 1–5 years, 6–10 years, 11–15 years, 16–20 years, 21–25 years, >25 years.
Earnings: Two scalar variables; log earnings in base year, difference in log earnings from the year before the base year to the base year.
Early retirement eligibility: 2 dummy variables; eligible or not eligible for early retirement benefits during the four-year period in question (eligibility depends on age and on the firm's affiliation to the early retirement program).
Immigrant status: 8 dummy variables; OECD, East Europe, Middle East/North Africa, Other Africa, South East Asia, South America, not immigrant.
Place of residence: 90 dummy variables; corresponding to travel-to-work-areas defined by Statistics Norway.
Size of municipality: 5 dummy variables; <2000, 2–5000, 5–10,000, 10–50,000, >50,000.
Firm size in base year: 4 dummy variables; 11–25, 26–50, 51–200, >200.
Firm turnover in base year: 5 dummy variables; No turnover, 0.1–10%, 10–15%, 15–20%, >20%.
Downsizing: 4 dummy variables; No downsizing <10%, 10–20%, 20–35%, 35–99.9%,
Closure: 5 dummy variables; No closure, closure with bankruptcy, liquidation, takeover.
Firm profitability: 2 scalar variables; Return on capital in base year, change in return on capital from base year (t) to year t+3.
Labor market tightness: 2 scalar variables from auxiliary regression; risk of unemployment and probability of reemployment.
Time: 3 dummy variables, one for each of the three periods in the dataset.

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