Incomplete draft; please do not quote

October 12, 2012

Getting Disabled Workers Back to Work – How Important are Economic Incentives?

Elisabeth Fevang[†], Inés Hardoy [§], and Knut Røed[†*]

Abstract

Based on a large social quasi-experiment in Norway, with a complete overhaul of social insurance benefit entitlements for temporary disabled persons, we investigate the impacts of the benefit level on the duration and outcome of temporary disability spells. For the transition rate to regular employment, we find a negative benefit elasticity around 0.3, implying that a 10 % cut in benefits raise the transition rate to employment by 3 %. This is a smaller number that what has previously been identified for unemployment insurance in Norway, but it nevertheless indicates the presence of a considerable work-capacity among the temporary disabled.

Keywords: Temporary disability, rehabilitation, hazard rate models, labor supply
JEL classification: H55, I38, J22

[†] The Ragnar Frisch Centre for Economic Research, Oslo, Norway
[§] Institute for Social Research, Oslo, Norway
[*] This paper is part of the project "Activation and Social Insurance", financed by the Norwegian Research Council (grant #185201). Correspondence to: Knut Røed, the Ragnar Frisch Centre for Economic Research, Gaustadalléen 21, 0349 Oslo, Norway. E-mail: knut.roed@frisch.uio.no.

1 Introduction

Over the last decades, many countries have experienced a significant growth in the caseloads of disability insurance programs; see, e.g., Autor and Duggan (2003), Duggan and Imberman (2006), Bratsberg *et al.* (2010), and Burkhauser and Daly (2011). Although "the disability problem" has clearly been overshadowed by the soaring unemployment rates after the onset of the Great Recession, it represents one of the major challenges for industrialized economies over a longer-term horizon. Recent empirical evidence also indicates that there is an element of substitutability between unemployment and disability insurances, and that the root cause of many disability insurance spells is really lack of acceptable employment opportunities; see, e.g., Black *et al.* (2002), Autor and Duggan (2003), Rege *et al.* (2009), and Bratsberg *et al.* (2010). This suggests that when the Great Recession comes to its close, we may find that it has left behind a challenging, and potentially long-lasting, disability problem; see Røed (2012).

Economists have long been concerned about the apparent lack of appropriate work incentives in disability insurance programs. The increasing awareness of the overlap between unemployment and health problems, and the accumulating evidence that many disability insurance claimants are in possession of a considerable remaining work capacity (French and Song, 2009; Maestas et al., 2011; Von Wachter et al., 2011; Kostøl and Mogstad, 2012), make this issue even more acute. Yet, the empirical evidence on the impacts of economic incentives in disability insurance programs is sparse and fragmented. While there have been numerous investigations into the issue of how unemployment insurance (UI) affect unemployment duration, see, e.g., Fredriksson and Holmlund (2006), and Card *et al.* (2007), or Røed *et al.* (2008) for recent overviews, there have, to our knowledge, been very few investigations into the impacts of economic incentives on the duration and outcomes of disability insurance spells. As pointed out by Autor *et al.* (2012) in relation to the missing evidence on the possible disincentives effects embedded in the U.S. Social Security Disability Insurance Program (SSDI), a major reason for this is the lack of exogenous variation in benefit levels. In addition, the large differences between countries in the way they have designed (and labeled) their (often many) disability insurance programs (OECD, 2010) have made research findings less “transferable” across countries.

Yet, there are some pieces of evidence indicating that labor supply behavior of disability insurance claimants sometimes do respond to economic incentives. In particular, research based on the U.S. workers’ compensation program for work-related injuries – exploiting the variation in coverage plans across workplaces and workers – has found that insurance spells become longer as the level of compensation increases; see, e.g., Butler and Worrall (1985), Meyer et al. (1995), and Krueger and Meyer (2002). There is also a more recent piece of evidence based on an evaluation of the private Long Term Disability program, also indicating that spell duration depends positively on the benefit level, although large statistical uncertainty makes it difficult to draw clear conclusions in this case (Autor et. al., 2012). Kostøl and Mogstad (2012) show that when a Norwegian permanent disability insurance program was reformed such that claimants were allowed to keep more of their benefits if their incomes were topped up with labor earnings, many recipients started to work (more). Finally, there is some evidence based on reforms of the Swedish sick-pay system, showing that the return-to-work hazard for employed absentees declines significantly with the level of compensation (Johansson and Palme, 2002; Henreksson and Persson, 2004).

Based on administrative register data from Norway, the present paper offers new evidence on the impacts of financial incentives on the duration and outcome of disability insurance spells. The program we examine is a *temporary disability insurance* (TDI) program; it covers workers who have exhausted their one-year sick-pay entitlements with their employer (or who were not entitled to sick-pay in the first place), but who have not (yet) been defined as permanently disabled; see the next section for details. This program has become very important in Norway, both because of its rapidly increasing caseloads, and because of its role as the major arena for medical and vocational rehabilitation attempts. Figure 1 illustrates the rising significance of the temporary disability insurance program in Norway, both in absolute terms and – more strikingly – relative to the unemployment insurance program. In 1992, there were roughly two persons claiming unemployment insurance (UI) for each person claiming temporary disability insurance (TDI) in Norway. By 2008, this pattern had been dramatically reversed, with almost four TDI claimants for each UI claimant.

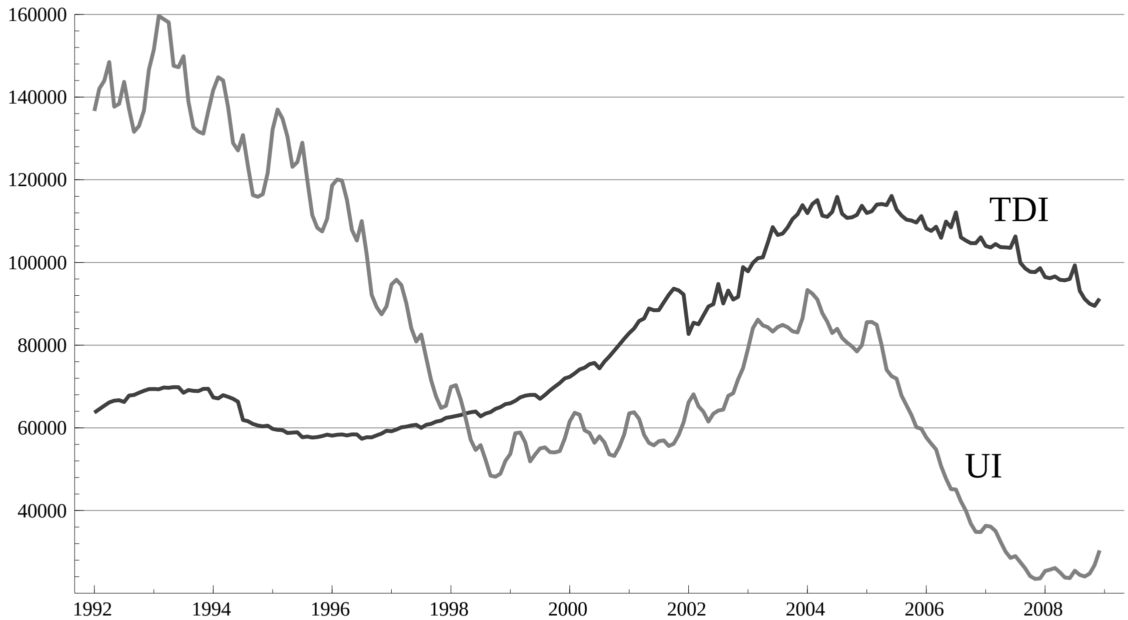

Figure 1. The numbers of temporary disability insurance (TDI) and unemployment insurance (UI) claimants in Norway 1992.1-2008.12.
Source: Own calculations based on administrative registers

To reliably identify the role of economic incentives, we take advantage of a social experiment in the form a full-scale overhaul of the TDI benefit scheme in January 2002. This overhaul introduced a new principle in the calculation of benefits, from being based on the entire income path of the individual, to become based on the income observed in the last year (or the last three years) just prior to disablement. Furthermore, the reform involved higher minimum benefits and reduced maximum child allowances. This implied that the benefit level was raised for individuals with some income paths and personal characteristics, while it was reduced for others. On average, the reform implied a change in individual benefit entitlements (positive or negative) of 22 percent. We use this reform to uncover how the compensation level affects the duration and outcome of TDI spells for persons who are deemed to be 100 % disabled at the time of entry into TDI. This is done within the framework of univariate and multivariate hazard rate models, where the (log) benefit level is the explanatory variable of interest. To ensure that this variable helps us identifying the causal effect of interest, we control for spurious correlations between replacement levels and individual resources/behaviors by conditioning the analysis on the hypothetical (log) replacement levels that the claimants *would have had* under both the old (pre-reform) and the new (post-reform) regimes. Since the reform affected different claimants differently, this does not generate a perfect multicollinearity problem, but ensures that the benefit-variation used to identify the causal response is entirely reform-driven. We demonstrate the credibility of this approach through a number of robustness exercises and “placebo-analyses”.

The main finding of our paper is that economic incentives do matter for the duration and outcomes of temporary disability insurance spells. The elasticity of the exit rate from TDI with respect to the benefit level is approximately minus 0.25, implying that, say, a 10 % increase in the benefit level is expected to increase spell duration by 2.5 %. Despite the huge differences in institutional setting, this is not very far from results obtained for the U.S. workers’ compensation program (Butler and Worrall, 1985; Meyer et al., 1995). Extending our model to a competing risks setting where we account for the alternative destination states, we find that the incentive effect is somewhat stronger for transitions to employment. The elasticity of the employment hazard with respect to the TDI benefit level is approximately minus 0.33. In comparison, the corresponding elasticity for unemployment insurance (UI) claimants in Norway has previously been estimated to average around minus 0.65 (Røed and Zhang, 2005). TDI claimants thus seem to be less responsive to financial incentives than UI claimants. However, taking into account that the TDI claimants analyzed in this paper were considered 100 % disabled at the time of entry into the program, we view the estimated responses as considerable. They clearly illustrate the existence of a “common support” for unemployment and disability insurance programs.

2 Institutions and data

There are basically three (often sequential) social insurance programs providing wage replacement for persons with health problems in Norway. The first is sickness benefits for employees (sick-pay). These benefits typically provide 100 % wage compensation, but with a maximum duration of one year. During this period, the employees are also protected against displacement on grounds related to the sickness. The second is the temporary disability insurance program, which is the program examined in the present paper. It provides benefits to employees who have exhausted their sick-pay – and in most cases no longer have a job – as well as to some individuals who were not eligible for sick-pay because they did not have a job at the time of disablement either. TDI benefits typically amount to around two thirds of previous earnings, subject to a minimum and a maximum threshold. There is no absolute duration limit for these benefits, although guidelines suggest that payments for more than 3-4 years can be granted only in exceptional cases. During the TDI period, a number of services are offered aimed at helping/pushing the claimants back to work, including medical and vocational rehabilitation activities. The third program is that of permanent (or, as explained below, semi-permanent) disability insurance (PDI). These benefits also pay around two thirds of previous earnings, and entail no further rehabilitation attempts. With the exception of a firm pay liability period during the first 16 days of sick-pay spells, all three programs are fully paid for by the state and financed through general (payroll) taxation. In contrast to, e.g., the workers' compensation programs in the U.S., there is no requirement that the sickness/disability is work-related. But all claims need to be certified by a physician (except for sick-pay spells lasting only a few days).

Entitlement to TDI requires that the work capacity is reduced by at least 50 % due to sickness, injury, or invalidity. A TDI spell consists of periods with medical and/or vocational rehabilitation. During medical rehabilitation, the claimant receives medical or psychological treatment and/or is allowed to recover through rest. During vocational rehabilitation, he/she participates in educational activities or in work training. There is no definitive time constrain on vocational rehabilitation, but programs rarely exceed three years.

The analyses in this paper are based on all new entries to TDI from January 1999 through December 2004. In the middle of this period (January 2002) the TDI benefit system was reformed. Before the reform, the benefits were calculated on the basis of a so-called "pension model", implying that a claimant's compensation level was determined by a combination of the number of years with earnings above a certain threshold (up to a maximum of 40 years) and the actual income earned in the 20 best years. Potential future earnings until the ordinary retirement age of 67 years were included in this calculation, assuming a continuation of the income level earned the last 1-3 years before the disablement occurred or (if higher) during the best half of all previous income years. Immigrants with few years of residence in Norway were not fully compensated. For breadwinners, there were substantial means-tested allowances for children and non-working spouses. And since the TDI benefit was considered a pension, it was subject to a lower tax rate than labor earnings.

After the reform, TDI benefits are calculated on the basis of earnings during the past calendar year or the average of the past three years (whichever is highest). The replacement ratio is 66 percent of earnings up to a ceiling of approximately 500,000 NOK, which roughly corresponds to 67 000 EUR (2012).[1] The child allowances has been reduced by up to two thirds (but no longer means-tested), and the allowance for a non-working spouse has been removed completely. Rather than being considered a pension, the new TDI benefit generates pension entitlements. This implies that the new benefit is subject to a higher tax rate than the old one, but at the same time makes a contribution to the individual's old age pension. The reform also implied a rise in the minimum level of TDI benefits, from around NOK 70,000 NOK (EUR 11,000) before the reform to NOK 112,000 (EUR 18,000) after the reform.[2] And contrary to the pre-reform regime, immigrants now only need three years of residence in Norway to receive the same compensation level as natives.

The reform was implemented such that persons who started their TDI spell before the reform were subject to the old calculation rules throughout their spell (even if it stretched into the post-reform period), whereas persons who started afterwards were subject to the new rules. However, for persons who had an ongoing spell at the time of the reform – and who would have received higher benefits based on the new regulations – it was possible to apply for an immediate transfer to the new system.

The reform produced winners and losers; see Hardoy *et al.* (2004). Among the winners were claimants with very low or unstable past earnings and immigrants with few

[1] All amounts in this paper are inflated to 2012 value, and EURO-equivalents are computed on the basis of the exchange rate of August 2012.

[2] The minimum level was further raised to NOK 126,000 (EUR 20,000) in January 2004.

years of residence in Norway, particularly those without children. Among the losers were claimants with a recent decline in earnings and claimants with many children and/or a non-working spouse. Some claimants were more or less "sheltered" from the pecuniary impact of the reform, however. In particular, all public sector workers are covered by an occupational pension arrangement that effectively shields them from changes in the level of social security benefits. The reason for this is that the occupational pension system in the public sector prescribes the same effective replacement rate of 66 percent (of the earnings level just prior to disablement) for all employees. The social security payments are simply topped up to achieve this outcome. Hence, any change in the social security benefit is automatically offset through a counteracting change in the occupational benefit payment. For this reason, we remove the persons with a public sector occupational pension entitlement from our main analysis sample, and use them as a foundation for a "placebo analysis" instead.

Our data consist of merged administrative registers, encrypted to prevent identification of individuals. They cover all TDI spells in Norway on a monthly basis, their starting dates and their stopping dates. By combining information from several administrative registers, we are able to compute the benefit entitlements corresponding to the pre-reform and post-reform regimes (regardless of which regime each person actually belonged to) on the basis of essentially the same information as that available to the Social Security Administration (SSA). We are also able to identify the outcome of each spell in terms of the main economic activity afterwards. Finally, our data include comprehensive information about the claimants, such as gender, age, educational attainment, marital status, number and age of children, the origin country of immigrants (and years since migration), and place of residence.

The starting point of our analysis is the set of all "new" entrants to TDI in Norway during the period from the beginning of 1999 through 2004. A new entrant in a month t is defined as a person who has a recorded starting date in this month and did not receive TDI benefits in any of the last 12 months prior to month t. We make this rather strict definition of "newness" to ensure that we really follow individuals from the beginning of a benefit claim period. A spell is assumed to have ended in a month t if the spell has a recorded stopping date in that month, and the person did not receive any TDI benefits the following three months (*t+1*, *t+2*, or *t+3*). Shorter periods out of TDI are censored (implying that we merge spells that are less than three months apart). We distinguish between three different destination states: i) employment, ii) permanent (or semi-permanent) disability, and iii) "other" (typically non-participation or unemployment).

In the main analysis, we define "employment" as having labor earnings and/or business income amounting to at least 7,000 NOK per month during the 12-month-period directly following the exit. In a robustness exercise, we define it instead as having a recorded employment spell in the employer-employee-register (regardless of earnings) or a business income exceeding 7000 NOK per month.[3] In both cases, we give priority to employment transitions, implying that transitions for which we both observe employment and a social security transfer consistent with one of the other two destination states, are defined as transitions to employment. The notion of "semi-permanent" disability insurance arises from the introduction in 2004 of a time-limited disability pension in Norway, in addition to the TDI analyzed in this paper and the permanent disability insurance (PDI). The idea was to create a program somewhere between these two, without the rehabilitation/activation ambitions embedded in TDI, but also without the permanency associated with PDI. In practice, it turned out to be a sort of waiting-room for PDI, and it was abolished in 2010. An important point to note in the present context is that while the benefits paid in the permanent disability program are calculated in basically the same way as in the pre-reform TDI, the benefits in the short-lived "semi-permanent" disability insurance program were calculated as in the post-reform TDI. As we return to below, this may have some impact on our attempts to identify the effects of the TDI benefit level on the transition to permanent/semi-permanent disability.

We limit the analysis to persons of age 27-55 who were registered as 100 percent disabled at the time of entry. The cut-off at age 27 is imposed because there are special rules applying for people who become disabled before that age due to particularly serious and objectively verifiable disabilities, and, based on our data, we are not able to identify this group. The cut-off at 55 is imposed to stay clear of issues related to early retirement.

---

[3] The reason why we do not rely on the employer-employee-register as our main strategy is that the quality of the stopping dates of these records is poor for the period covered in this analysis; hence, we worry that we then include past employment spells that were not appropriately out-registered.

In total, this gives us 209,857 TDI spells. However, in the causal analysis, we can only use spells for which the compensation level was affected by the reform, i.e., spells not covered by a public occupational pension. This leaves us with 177,401 spells (the remaining spells are used in a placebo analysis). Table 1 offers some descriptive statistics for our main sample. We see no conspicuous changes in entry composition from the pre-reform to the post-reform period, except for a decline in previous earnings which most likely reflects the deterioration in aggregate labor demand; see Figure 1. The fraction of TDI claimants who had returned to regular employment four years after program entry rose from 44 % for the pre-reform cohort to around 50 % for the post-reform cohort. Again, the explanation for this change is likely to be of cyclical nature, reflecting the strong economic recovery from around 2004. The fractions who after four years had moved on to a permanent disability benefit were roughly the same in the two cohorts, around 22-23 %.

Table 1. Descriptive statistics for the main sample

	Pre-reform (1999-2001)	Post-reform (2002-2004)
Number of new entrants to TDI (N)	77,188	100,213
Mean spell duration (# months, right-censored at four years)	23.19	22.85
Outcome of spells (fractions)		
Regular employment	0.46	0.48
Disability	0.13	0.14
Other	0.20	0.18
Censored	0.21	0.20
Mean level of TDI benefits (NOK 2012 value)	191,022	219,269
Key individual characteristics (at entry)		
Demographic characteristics		
Mean age	40.46	40.55
Fraction women	0.50	0.51
Fraction married/cohabiting	0.53	0.51
Fraction with children under 4 years of age	0.11	0.11
Fraction with children under 19 years of age	0.57	0.57
Fraction born in non-OECD country	0.07	0.08
Educational attainment (fractions)		
Compulsory school only	0.42	0.39

Lower secondary school	0.21	0.17
Upper secondary school	0.26	0.30
College/University ecucation	0.10	0.12
Situation before entry to rehabilitation benefits		
Fraction with occupational pension plan	0.11	0.08
Fraction with sick leave in previous year	0.83	0.79
Months on sick leave previous year	8.87	8.42
Months on sick leave last 5 years	14.45	15.19
Fraction who experienced unemployment last 3 years	0.33	0.32
Fraction who received social assistance last 3 years	0.25	0.24
Fraction who was employed the previous year	0.69	0.64
Months employed previous year	6.52	6.27
Average annual wage income last 3 years (2012-value)	347,376	320,275
State 4 years after entry (fractions)		
Regular employment	0.44	0.50
Permanent disability	0.23	0.22
Other*	0.18	0.15
Still in TDI	0.15	0.14

* Include persons for which no activity or transfer is recorded (46 %), persons who claim social assistance (18 5 %), persons who registers as unemployed (11 %), persons participating in vocational programs without receiving TDI benefits (34 %), and persons starting on ordinary education (2 %).

Since TDI benefit entitlements are determined on the basis of the claimants' own past labor market behavior, the cross-sectional variation in benefit levels is of course anything but randomly assigned. However, the 2002-reform induced a *random-assignment-like* source of variation related to the exact *timing* of entry into the program. The key idea of the present paper is to exploit this particular source of variation to identify and estimate the causal effects of interest. In the remainder of this section, we introduce some notation, and take a brief look at some descriptive patterns that emerge when we compare the "winners" and the "losers" of the reform, before we turn to a more formal empirical analysis in the next section.

Note that we have no ambition of identifying the effects of the reform as such. That would be very difficult, since the implementation of the reform coincided with other developments that probably also affected the duration and outcomes of TDI spells. In particular, it coincided with a significant cyclical downturn in Norway that resulted in rising unemployment during 2002 and 2003, followed by a very strong recovery (see Figure 1). Moreover, after the turn of the century, the government's policy priorities have shifted markedly towards more intensive use of vocational rehabilitation programs. The already existing – but not very eagerly enforced – rule that disability benefits could only be granted provided that vocational rehabilitation had been tried first (unless it was "obviously futile") was called to life. In addition, in 2004 new regulations prescribed faster transitions from medical to vocational rehabilitation.

It is the *idiosyncratic* impacts of the reform on individual benefit levels we use to identify the behavioral responses to the level of TDI benefits. Let b_i^o be the (natural log of the) benefit level according to the old (pre-reform) rules and let b_i^n be the benefit level according to the new (post-reform) rules (adjusted for less generous tax treatment).[4] For entrants in 1999-2001, actual benefits b_i^a are equal to b_i^o until January 2002, after which it is equal to b_i^n if $b_i^n > b_i^o$ and b_i^o otherwise. For entrants in 2002-2004 b_i^a is always equal to b_i^n. Note, however, that both benefit levels (b_i^n, b_i^o) can be computed for all entrants. This implies that we can also calculate each individual's hypothetical benefit gain g_i resulting from the reform as $g_i = b_i^n - b_i^o$.

Figure 2 presents the distributions of the hypothetical benefit gains g_i for TDI entrants in our main sample prior to and after the reform, respectively. There are two important points to note from this graph. The first is that the reform had a substantial impact on benefit levels. On average, the absolute value of the gain $(\frac{1}{N}\sum_i |g_i|)$ was as large as 43,700 NOK (measured in 2012-value), which corresponds to 22 percent of the average pre-reform benefit level. There were more winners (75 %) than losers, and the average gain was approximately 26,000 NOK. The second point to note is that the gains-distributions were very similar before and after the reform, and the average gain increased by a mere 1,300 NOK. This suggests that the reform did not noticeably affect entrance into the program. Since our empirical strategy relies on the reform-initiated change in benefits being exogenous as viewed from the agents' point of view, this is reassuring.

[4] Since after-tax benefit levels are affected by potentially endogenous factors (such as the labor supply of other family members), we compute annual benefit levels *before* tax, but adjust the post-reform benefit levels downward by the general difference in pre- and post-reform tax rates (the difference between the tax rate on pensions and wage earnings).

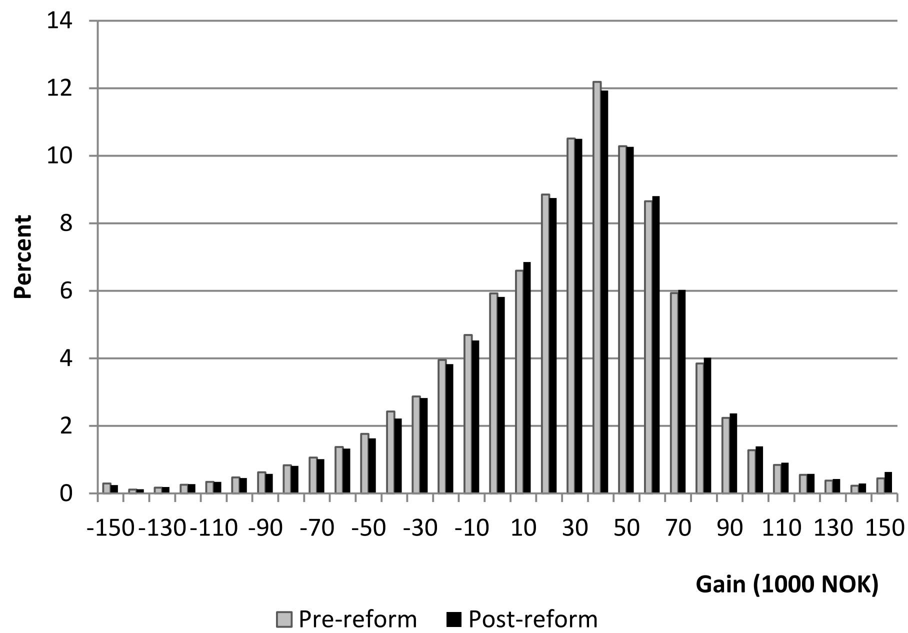

Figure 2. The distribution of gains before and after the reform
Note: Numbers on horizontal axis indicate cell-midpoints, with the range of each cell being 10,000 NOK (except at the two ends, where, e.g., 150 means > 145,000)

An entrant with a positive gain $g_i > 0$ would clearly prefer the post-reform benefit regime, while a person with $g_i < 0$ would prefer the pre-reform regime, ceteris paribus. Let (A_0,A_1) denote the groups with $g_i > 0$ who entered rehabilitation before and after the reform, respectively; and let (B_0,B_1) denote the corresponding groups with $g_i < 0$. Since the A and B groups were affected in opposite directions by the reform, we can use a simple descriptive difference-in-difference (DiD) methodology to obtain a rough indication of whether the reform affected behavior or not. Table 2 presents some descriptive statistics on the outcomes recorded for potential "winners" and "losers", both before and after the reform. The differences-in-differences (and the ratios-of-ratios) in mean outcomes indicate that there is a positive causal relationship between the benefit level and spell duration. While the persons with characteristics implying higher benefits in the post reform period (the A-group) had roughly the same spell durations before and after the reform, the persons with characteristics implying lower benefits (the B-group) had significantly shorter durations (1.3 months). Hence, the DiD effect estimator for the effect of being a winner rather than a loser is a 1.3 month increase in TDI duration. Looking at the ratios-of-ratios (RoR) instead indicates a 6 % increase (which is in line with the DiD estimator). The corresponding estimated effects of higher benefits on the distribution of outcomes are small, but with a slightly larger decline in transitions to employment than in the transitions to the two competing states.

Table 2. The relative performance of hypothetical winners and losers – before and after the reform. Entrants in 1999-2001 and 2002-2004

	Pre-reform (1999-2001)		Post-reform (2002-2004)		Difference in Differences	Ratio of Ratios
	A_0	B_0	A_1	B_1	$(A_1 - B_1) - (A_0 - B_0)$	$(A_1/B_1)/(A_0/B_0)$
Mean duration	22.73	24.63	22.71	23.30	1.30	1.06
Percent of spells ending in						
Regular employment	48.6	38.7	49.8	41.5	-1.6	0.96
Disability pension	1.7	1.9	2.0	2.4	-0.2	0.93
Unemployment	13.0	14.3	13.7	14.9	0.1	1.01
Other	16.7	21.8	15.1	20.4	-0.2	0.97
Censored	20.0	23.3	19.4	20.8	1.9	1.09
Number of spells (N)	58339	18849	76744	23469		

Although the DiD-estimators indicate that there were behavioral responses to the changes in benefit levels, it is difficult to give these effects a clear quantitative interpretation, since they represent averages over a distribution of benefit changes. This is what we turn to in the next section.

3 Empirical analysis

To quantify the effects of marginal changes in the benefit level on the duration and outcome of rehabilitation, we set up multivariate mixed proportional hazard rate model (MMPH). The model is designed to exploit the random-assignment-like variation in benefit levels arising from the reform. An important element of our strategy is to use *both* the two hypothetical (b_i^n, b_i^o) *and* the actual benefit levels (b_i^a) as explanatory variables in the statistical analysis. The idea is that the hypothetical benefit levels then capture all the spurious effects arising from the fact that the benefit schedules depend on past behavior, while the determination of which of the two benefit levels the claimant actually gets is quasi-randomly assigned; i.e., it only depends on the *timing* of entry.

The statistical model we use can be explained as follows: We start out with $i=1,\ldots,N$ new entrants to medical rehabilitation during the period from January 1999 to December 2004. Let $k=1,\ldots,3$, denote the set of potential events; i.e., employment ($k=1$), permanent disability ($k=2$), and other ($k=3$). Spells that are still ongoing at the end of 2008 are right-censored. We also right-censor spells in cases where the claimants die or emigrate to another country.

As we observe labor market status by the end of each month only, we set up the statistical model in terms of grouped hazard rates. To start with, we write the integrated month-specific hazard rates φ_{kit} as functions of the benefit level b_i^a, observed (time-varying) individual characteristics x_{it}, calendar time s_t, spell duration d_t, and unobserved individual characteristics v_{ki}; i.e.,

$$\varphi_{kit} = \int_{t-1}^{t} \theta_{kis} ds = \exp\left(\sigma_{kt} s_{it} + \lambda_{kd} d_{it} + \delta_k b_i^a + \beta_k x_{it} + v_{ki}\right), \quad k = 1,\ldots,3, \tag{1}$$

where θ_{kis} is the underlying continuous-time hazard rate, which is assumed to be constant within each month.[5] The parameters of interest here are the benefit elasticities (δ_k). However, as noted above, the benefit level b_i^a is computed in a way that makes it dependent on past labor market behavior and current family situation in a rather complex way; hence it is unlikely that b_i^a is uncorrelated to the unobserved characteristics v_{ki}. But, provided that that the unobserved characteristics are time-invariant, we can represent the linear dependencies between them and the actual benefit level by functions linking them to the two hypothetical benefit levels instead; i.e.,

$$v_{ki} = \alpha_{ok} b_i^o + \alpha_{nk} b_i^n + \varepsilon_{ik}, \quad k = 1,\ldots,3. \tag{2}$$

We then have – by construction – that ε_{ik} is orthogonal to b_i^a. Hence, by including the two hypothetical benefit levels in Equation (1), we can obtain unbiased estimates of the benefit elasticities. The intuition is that while the benefit level calculated according to, say, the

[5] The vector s_t contains one indicator for each calendar time quarter in the analysis period (39 dummy variables), d_t contains indicator variables denoting spell duration also measured in quarters $(d = 1,2,3,4,5,6,7,8,9,10,11,12,>12)$. The vector of individual characteristics x_{it} contains the following variables: gender and family situation (10 dummy variables), nationality (2 dummy variables), education (5 dummy variables), age (32 dummy variables), county (19 dummy variables).

pre-reform rules can have causal effects in the pre-reform period only, its spurious effects apply to the post-reform period as well. The hazard rates used to estimate the model are consequently specified as:

$$\varphi_{kit} = \exp\left(\sigma_{kt} s_{it} + \lambda_{kd} d_{it} + \delta_k b_i^a + \beta_k x_{it} + \alpha_{ok} b_i^o + \alpha_{nk} b_i^n + \varepsilon_{ik}\right), \quad k = 1, \ldots, 3. \qquad (3)$$

To avoid unjustified restrictions, we estimate the model in a completely nonparametric fashion, implying that unobserved heterogeneity is treated as a joint discrete distribution with an unknown number of support points. Following recommendations provided by Gaure *et al.* (2007), we have used the Akaike Information Criterion (AIC) for model selection. The likelihood function and the algorithm used to maximize it are described in a separate Appendix.

Table 3 presents the key estimation results. In that table, we also present results for a univariate hazard rate model, *i.e.*, a model where all the exit states are aggregated into a single one and where we thus only focus on TDI duration. Our results indicate that higher benefits reduce the exit rates from TDI. Based on the single risk model, the elasticity of the exit hazard with respect to the benefit level is estimated to minus 0.25. The results from the competing risks model indicate that the elasticity is somewhat larger for transitions to employment and permanent/semi-permanent disability insurance (around minus 0.33), whereas it is smaller for other destinations (around minus 0.20). The estimated impact on the transition to permanent disability should be interpreted with a bit care, however, since there was a reform also in the permanent disability insurance system in January 2004, which introduced the post-reform TDI benefit calculation rules even in this program (see Section 2).

It is notable that both the pre- and post-reform hypothetical benefit levels are associated with significantly higher exit rates, particularly to employment. This implies that if we had failed to control for these two variables – and instead used all the observed variation in the actual benefit level to identify its causal effects – the estimated elasticities would have become seriously biased. More specifically, for the single risk model, we would then have estimated a benefit elasticity of minus 0.003 instead of minus 0.253. For the competing risk model, we would have estimated a benefit-elasticity in the employment hazard of plus 0.572 (with standard error 0. 012) instead of minus 0.311; hence we would erroneously have concluded that higher benefits resulted in a significantly higher transition rate to employment. This is not surprising, since higher benefits typically results from a stronger attachment to the labor market, and hence associated with better return-prospects as well. Our vector of 120 control variables is apparently far from sufficient to remove completely the influences of spurious correlation.

Table 3. Estimation results main sample (standard errors in parentheses)

	Single risk model	Competing risks model		
		Employment	Permanent disability	Other
Log actual benefit level (causal effect)	-0.253***	-0.331***	-0.323***	-0.199***
	(0.023)	(0.040)	(0.070)	(0.046)
Log benefit level pre-reform rules	0.073***	0.398***	-0.001	-0.266***
	(0.013)	(0.024)	(0.039)	(0.026)
Log benefit level post-reform rules	0.205***	0.824***	-0.256**	-0.717***
	(0.020)	(0.037)	(0.064)	(0.044)
Number of support points in heterogeneity distribution	3		10	

Note: The number of spell-observations is 177,401 for both models. *(**)(***) Statistically significant at the 10(5)(1) percent level. The estimated models include the following control variable sets: Gender and family situation (10 dummy variables), nationality (2 dummy variables), education (5 dummy variables), age (32 dummy variables), county (19 dummy variables), calendar time quarter (39 dummy variables), and spell duration (13 dummy variables).

Table 4 presents some robustness exercises, some models estimated for subgroups, and some placebo-analyses. Robustness is evaluated along three dimensions; see part I of the table. First, we drop the incorporation of unobserved heterogeneity into the model. This has virtually no impact on any of the estimated effects. Second, we drop the imposition of the transitional rules applying for spells that were ongoing at the time of the reform (January 2002); i.e., instead of assuming that those with higher benefits in the new system switched immediately, we assume that they continued with their “old” benefit level. This reduces the estimated employment elasticity, which is consistent with a presumption that those with higher benefits in the new system actually did switch. Finally, we re-estimate the competing risks model with an alternative definition of employment, based on records in the employer-employee-register rather than on recorded earnings (see above). This change has virtually no impact on the estimated employment elasticity.

Table 4. Estimated effect of actual benefit level – alternative models(standard errors in parentheses)

	Single risk model	Competing risks model		
		Employment	Permanent disability	Other
Main model/sample (from Table 3)	-0.253***	-0.331***	-0.323***	-0.199***
	(0.023)	(0.040)	(0.070)	(0.046)

I. Robustness				
Without unobserved heterogeneity	-0.254*** (0.023)	-0.301*** (0.032)	-0.307*** (0.052)	-0.222*** (0.040)
Without imposing transition rules	-0.236*** (0.018)	-0.282*** (0.033)	-0.307*** (0.060)	-0.280*** (0.038)
With alternative job definition		-0.310*** (0.033)	-0.270*** (0.065)	-0.249*** (0.046)
II. Group specific models				
Men (N=87,633)	-0.274*** (0.032)	-0.264*** (0.045)	-0.321*** (0.075)	-0.307*** (0.054)
Women (N=89,768)	-0.235*** (0.033)	-0.317*** (0.047)	-0.331*** (0.092)	-0.092 (0.065)
Married/Cohabitants (N=98,977)	-0.320*** (0.033)	-0.425*** (0.047)	-0.318*** (0.096)	-0.266*** (0.066)
Divorced (N=39,377)	-0.254*** (0.047)	-0.261*** (0.073)	-0.499*** (0.120)	-0.153* (0.090)
Never married (N=53,534)	-0.157*** (0.048)	-0.206*** (0.083)	-0.116 (0.170)	-0.077 (0.093)
III. "Placebo" models				
Persons with occupational pension public sector (N=32,456)	-0.023 (0.078)	-0.080 (0.111)	0.286 (0.215)	-0.094 (0.208)
Erroneously placed reform in pre-reform period (N=77,108)	0.032 (0.043)	-0.009 (0.062)	-0.113 (0.153)	0.017 (0.087)
Erroneously placed reform in post-reform period (N=100,213)	-0.026 (0.025)	-0.068 (0.042)	-0.213** (0.082)	0.000 (0.052)

*(**)(***) Statistically significant at the 10(5)(1) percent level.

Part II of the Table 4 reports estimates obtained when the main sample is split into different sub-groups. We find that men and women respond similarly to changes in financial incentives, although the employment elasticity is somewhat larger for women than for men. Looking at the responses by family situation, we find that married persons are more strongly affect by economic incentives than those who live alone.

Finally, part III of the table shows the results from three different "placebo" analyses. The first is simply a re-estimation of the main model on the sample of persons who presumably were protected from the impacts of the reform, because they were eligible for a public occupational pension (see Section 2). It should be noted, however, that some members of this group probably were affected by the reform to some extent, due to limited coverage (e.g., because they had worked in the public sector for a very short time); hence we would not expect this to be a completely "pure" placebo sample. Nevertheless, the results indicate that the "effects" in this group were close to zero. The two other placebo-analyses are constructed by imposing *false* reforms in the middle of the pre-reform and the post-reform periods; i.e. we estimate the models *as if* the reform occurred at these times (for the pre-reform period, we then right-censor all spells at the time of the genuine reform). Again, the results tend to indicate that there were no effects, confirming the causal interpretation of the effects identified in our main model. An exception from this pattern is the identification of a negative effect of TDI benefits on the transition to permanent (or semi-permanent) disability in the post-reform period. We believe that this finding is related to the introduction the semi-permanent disability insurance program January 2004, which were based on the same benefit calculation rules as those applying for TDI after the reform (see Section 2 above). The introduction of this program gave persons with large positive gains (g_i) an incentive to postpone any transition to a more lasting disability insurance until the new semi-permanent benefit was introduced, potentially giving rise to the negative effect.

4 Conclusion

Based on Norwegian administrative registers, we have utilized a large "social experiment" – consisting of a complete overhaul of the temporary disability insurance (TDI) system – to estimate the impacts of economic incentives on the duration and outcome of TDI spells. Our conclusion is that a 10 percent cut in the benefit level induces approximately a 3 percent increase in the hazard rate to regular employment, and also a 3 percent increase in the transition rate to permanent disability. We have also shown that the estimated effect is similar for different subgroups, although the labor supply responses seem somewhat larger for women than for men, and also larger for married than for single persons. Our results support the view that there is a labor supply potential among temporary disabled persons, and that the realization of this potential to some extent depend on the design of social insurance.

Appendix

In this section, we derive the likelihood function for the model estimated in this paper. Let

$$w_{kit} = \sigma_{kt} s_{it} + \lambda_{kd} d_{it} + \delta_k b_i^a + \alpha_{ok} b_i^o + \alpha_{nk} b_i^n + \beta_k x_{it}, \quad k = 1, 2, 3, \tag{4}$$

where $(\sigma_{kt}, \lambda_{kd}, \delta_k, \alpha_{ok}, \alpha_{nk}, \beta_k)$ are the parameters to be recovered from the data.

The probability that individual *i* makes a transition to state *k* during period *t* is equal to:

$$p_{ki}(w_{kit}, \varepsilon_i) = \left(1 - \exp\left(-\sum_k \exp\left(w_{kit} + \varepsilon_{ki}\right)\right)\right) \frac{\exp\left(w_{kit} + \varepsilon_{ki}\right)}{\sum_k \exp\left(w_{kit} + \varepsilon_{ki}\right)}, \quad \varepsilon_i = (\varepsilon_{1i}, \varepsilon_{2i}, \varepsilon_{3i}) \,. \tag{5}$$

Let o_{kit} be an outcome indicator variable, which is equal to 1 if the corresponding observation ended in a transition to state *k*, and zero otherwise, and let O_i be the complete set of outcome indicators available for individual *i* (all periods at which individual *i* has been at risk of making a transition of some sort). The contribution to the likelihood function formed by a particular claimant, conditional on the vector of unobserved variables ε_i can then be formulated as:

$$L_i(\varepsilon_i) = \prod_{o_{kit} \in O_i} \left[\begin{array}{l} \prod_k \left[\left[\left(1 - \exp\left(-\sum_k \exp(w_{kit} + \varepsilon_{ki})\right)\right) \dfrac{\exp(w_{kit} + \varepsilon_{ki})}{\sum_k \exp(w_{kit} + \varepsilon_{ki})} \right]^{o_{kit}} \right] \\ \times \left[\exp\left(-\sum_k \exp(w_{kit} + \varepsilon_{ki})\right) \right]^{\left(1 - \sum_k o_{kit}\right)} \end{array} \right]. \tag{6}$$

In order to arrive at the marginal likelihood, we need to integrate unobserved heterogeneity v_i out of Equation (6). We do this nonparametrically, to make sure that the results are really driven by the data and not by unjustified restrictions. In practice, this implies that the vectors of unobserved attributes are discretely distributed (Lindsay, 1983) with the number of mass-points chosen by adding points until it is no longer possible to increase the likelihood function (Heckman and Singer, 1984). Let *Q* be the (a priori unknown) number of support points in this distribution and let $\{\varepsilon_l, q_l\}$, $l = 1, 2, \ldots Q$, be the associated location vectors and probabilities. In terms of observed variables, the likelihood function is then given as

$$L = \prod_{i=1}^{N} \sum_{l=1}^{Q} q_l L_i(\varepsilon_l), \quad \sum_{l=1}^{Q} q_l = 1 \,. \tag{7}$$

The algorithm we use starts out estimating a null-model without unobserved heterogeneity (Q=1), and then expands the model step by step with one additional support point in each round. Each time, we identify a candidate for a new support point by assigning a new point with probability zero and select its location vector such that the derivative in the direction of positive probability is positive. For this we use a simulated annealing approach. We then maximize in three steps; first with respect to the probabilities, then with respect to the entire heterogeneity distribution, and finally with respect to all parameters in the model simultaneously. For the maximizations we use a combination of BFGS, a Newton method with line-search, and a trust-region method. Standard errors are lifted from the diagonal of the inverse of the (negative) Fisher matrix.

References

Abbring, J. H. and Van den Berg, G. J. (2003) The Nonparametric Identification of Treatment Effects in Duration Models. *Econometrica*, Vol. 71, 1491-1517.

Autor, D. and Duggan, M. G. (2003) The Rise in the Disability Rolls and the Decline in Unemployment. *The Quarterly Journal of Economics*, Vol. 118, 157-205.

Autor D., Duggan, M., and Gruber J. (2012) Moral Hazard and Claims Deterrence in Private Disability Insurance. NBER Working Paper No. 18172.

Black, D., Daniel, K., and Sanders, S. (2002) The Impact of Economic Conditions on Participation in Disability Programs: Evidence from the Coal Boom and Bust. *American Economic Review*, Vol. 92, 27–50.

Bratsberg, B., Fevang, E., and Røed, K. (2010) Disability in the Welfare State: An Unemployment Problem in Disguise? IZA Discussion Paper No. 4897.

Burkhauser, R. V. and Daly, M. C. (2011) *The Declining Work and Welfare of People with Disabilities: What Went Wrong and a Strategy for Change*. AEI Press, Washington D.C.

Butler, R. J., and Worral, J. D. (1985), Work Injury Compensation and the Duration of

Non-work Spells, *Economic Journal,* Vol. 95, 714-724.

Card, D., Chetty, R. and Weber, A. (2007) The Spike at Benefit Exhaustion: Leaving the Unemployment System or Starting a New Job? American Economic Review, Vol. 97, No. 2 (PAP), 113-118.

Duggan, M. and Imberman, S. (2006) *Why Are Disability Rolls Skyrocketing?* In D. Cutler and D. Wise (eds.): Health in Older Ages: The Causes and Consequences of Declining Disability among the Elderly. University of Chicago Press.

Fredriksson, P. and Holmlund, B. (2006) Improving Incentives in Unemployment Insurance: A Review of Recent Research. *Journal of Economic Surveys*, Vol. 20, 357–86.

French, E. and Song, J. (2009) The Effect of Disability Insurance Receipt on Labor Supply. Working Paper, Federal reserve Bank of Chicago.

Gaure, S., Røed, K., and Zhang, T. (2007) Time and Causality: A Monte Carlo Assessment of the Timing-of-Events Approach. *Journal of Econometrics*, Vol. 141, 1159–1195.

Hardoy, I., Storvik, Aa., and Torp, H. (2004) Hvem år mer og hvem får mindre? Effekter av nye berekningsregler for stønader til livsopphold under attføring og rehabilitering. Rapport 2004:14, Institutt for samfunnsforskning.

Heckman, J. and Singer, B. (1984) A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data. *Econometrica*, Vol. 52, 271-320.

Henrekson, M. and Persson M. (2004) The Effects on Sick Leave of Changes in the Sickness Insurance System. *Journal of Labor economics*, Vol. 22, 87-114.

Johansson P. and Palme, M. (2002) Assessing the Effects of a Compulsory Sickness Insurance on Worker Absenteeism. *Journal of Human Resources*, Vol. 37, No. 2, 381-409.

Kostøl, A. R., and Mogstad, M. (2012) How Financial Incentives Induce Disability Insurance Recipients to Return to Work. IZA Discussion Paper No. 6702.

Krueger, A. B. and Meyer, B. D. (2002) Labor Supply Effects of Social Insurance. In Auerbach, A. J. and Feldstein, M. (eds.) *Handbook of Public Economics*, Vol. 4. Elsevier Science, North-Holland, 2002; 2327-92.

Lindsay, B. G. (1983) The Geometry of Mixture Likelihoods: A General Theory. *The Annals of Statistics*, Vol. 11, 86-94.

Maestas, N., Mullen, K. and Strand, A. (2011) Does Disability Insurance Receipt Discourage Work? Using Examiner Assignments to Estimate Causal Effects of SSDI Receipt. Rand Working Paper, WR-853-2.

Meyer, B. D., Viscusi, W. K. and Durbin, D. (1995) Workers' Compensation and Injury Duration: Evidence from a Natural Experiment, *American Economic Review*, Vol. 85, 322-340.

OECD (2010) Sickness, Disability and Work: Breaking the Barriers - A Synthesis of Findings across OECD Countries, OECD, Paris.

Rege, M., Telle, K., and Votruba, M. (2009) The Effect of Plant Downsizing on Disability Pension Utilization. *Journal of the European Economic Association*, Vol. 7, No. 5, 754–785.

Røed K. (2012) Active Social Insurance. *IZA Journal of Labor Policy*, forthcoming.

Røed, K., Jensen, P., and Thoursie, A. (2008) Unemployment Duration and Unemployment Insurance - A Comparative Analysis Based on Scandinavian Micro Data. *Oxford Economic Papers*, Vol. 60, No. 2, 254-274.

Røed, K. and Zhang, T. (2003) Does Unemployment Compensation Affect Unemployment Duration? *Economic Journal*, Vol. 113 (2003), 190-206.

Røed, K. and Zhang, T. (2005) Unemployment duration and economic incentives - A quasi random-assignment approach. *European Economic Review* , Vol. 49, 1799-1825.

Von Wachter, T., Song, J., and Manchester, J. (2011) Trends in Employment and Earnings of Allowed and Rejected Applicants to the Social Security Disability Insurance Program. American Economic Review, Vol. 101, No. 7, 3308-3329.