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Temporary Disability and Economic Incentives^{*}

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Abstract

We investigate the impacts of economic incentives on the duration and outcome of temporary disability insurance (TDI) spells. The analysis is based on a large quasi-experiment taking place in Norway, involving a complete overhaul of the TDI benefit system. Our findings show that the labour supply of TDI claimants does respond to both the benefit level and the level of local labour demand. The estimated elasticity of the transition rate to employment with respect to the benefit level is -0.33. We also find that the TDI benefit level significantly affects the transition rate to alternative social insurance programmes.

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

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Over the last decades, the caseloads of disability insurance programmes have risen considerably in many countries (Autor and Duggan, 2003; Burkhauser and Daly, 2011; Bratsberg *et al.*, 2013). Although the increasing participation in disability programmes has been overshadowed by soaring unemployment rates following the onset of the Great Recession, such growing caseloads represent a major long-term challenge for industrialised economies. Empirical evidence has indicated that disability programme entry rates are countercyclical, and that the root causes of disability insurance claims are often related to job loss and/or the absence of acceptable employment opportunities (Black *et al.*, 2002; Autor and Duggan, 2003; Duggan and Imberman, 2009; Rege *et al.*, 2009; Bratsberg *et al.*, 2013). Hence, although recent empirical evidence from the US has failed to identify a direct relationship between disability programme entry and unemployment insurance exhaustion (Mueller *et al.*, 2015), there may be an element of substitutability between unemployment and disability insurance. In any case, the marked countercyclical pattern of disability programme entry suggests that when business cycle conditions have normalised, the recession might have left behind a challenging and potentially long-lasting disability problem (Røed, 2012).

Economists have long been concerned about the apparent lack of appropriate work incentives in disability insurance programmes. The increased awareness of the overlap between unemployment and health problems, paired with accumulating evidence that many disability insurance claimants possess at least some capacity for work (Bound, 1989; Chen and van der Klauuw, 2008; Von Wachter *et al.*, 2011; Maestas *et al.*, 2013; French and Song, 2014; Kostøl and Mogstad, 2014), make it even more important to ensure that disability insurance programmes are designed to encourage the labour supply. Yet, the empirical evidence concerning the impacts of the economic incentives embedded in these programmes is sparse and fragmented. While there have been numerous investigations into the issue of how unemployment insurance (UI) affects

unemployment duration (Fredriksson and Holmlund, 2006; Card *et al.*, 2007; Røed *et al.*, 2008), there have, to our knowledge, been only a few studies on the impacts of economic incentives on the duration and outcomes of disability insurance spells. As Autor *et al.* (2014) noted in relation to the lack of evidence about possible disincentive effects in the US Social Security Disability Insurance Program (SSDI), a major reason for this research gap is the lack of exogenous variation in benefit levels.

However, there is some evidence to suggest that the labour supply of disability insurance claimants does respond to economic incentives. In particular, studies based on US workers' compensation programmes for work-related injuries – exploiting the variation in coverage plans across workplaces and workers – have found positive relationships between the compensation level and the duration of insurance spells (Butler and Worrall, 1985; Meyer *et al.*, 1995; Krueger and Meyer, 2002). More recent evidence from the US, this time based on an evaluation of a private long-term disability programme, also indicates that the duration of insurance claims depends positively on the benefit level, although the large degree of statistical uncertainty makes it difficult to draw clear conclusions in this case (Autor *et al.*, 2014). Kostøl and Mogstad (2014) studied a reform to the Norwegian permanent disability insurance programme that allowed claimants to retain more of their benefits when their incomes were topped up with labour earnings, and found that many recipients started to work (or worked more hours) following the reform. Finally, evidence based on reforms to the Swedish sick pay system shows that the return-to-work transition rate for employed absentees generally depends negatively on the compensation level (Johansson and Palme, 2002; Henrekson and Persson, 2004), although benefit reductions in the form of initial 'waiting days' (with zero replacement) may raise absenteeism by reducing incentives to return to work once the waiting period is completed, since a relapse into sick leave would involve a new waiting period (Pettersson-Lidbom and Thoursie, 2013).

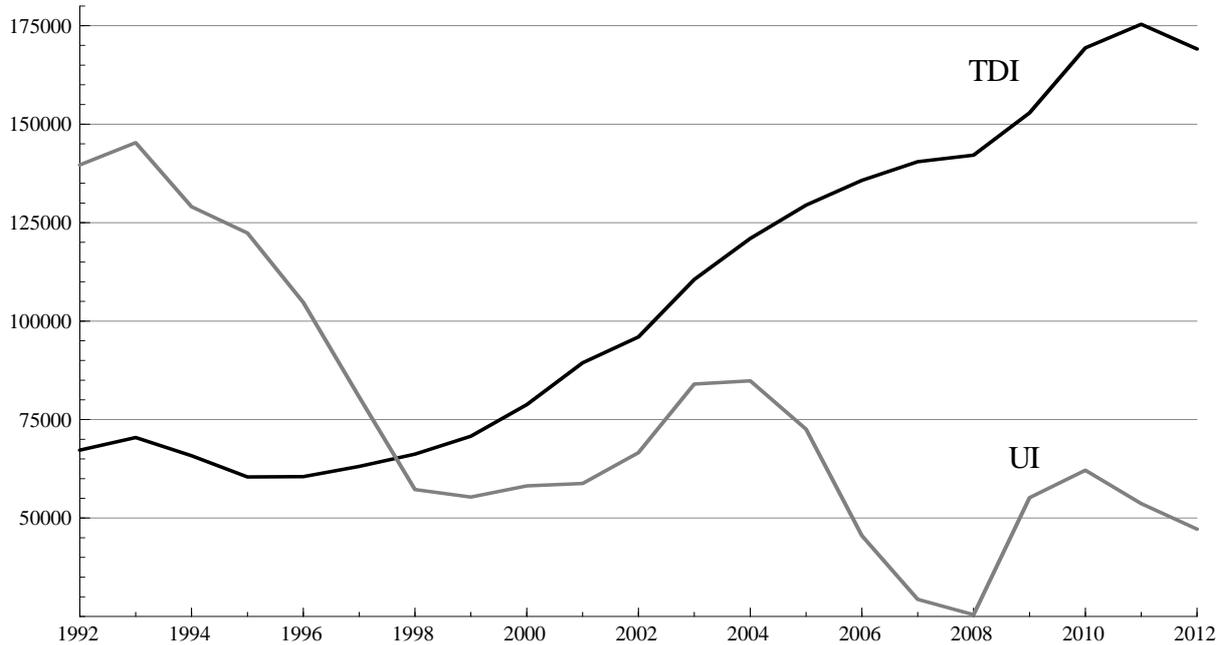


Fig. 1. The numbers of temporary disability insurance (TDI) and unemployment insurance (UI) claimants in Norway 1992-2012 (annual averages of stocks registered at the end of each month).

Source: Authors' own calculations based on administrative registers. The TDI system underwent some changes and relabelling during the period covered by this graph, and so the reported numbers include 'rehabiliterings- og attføringspenger' (1992-2010), 'tidsbegrenset uførestønad' (2004-2010), and 'arbeidsavklaringspenger' (from 2010).

Based on administrative register data from Norway, the present paper offers new evidence on the impacts of financial incentives on the duration and outcomes of disability insurance spells. The programme we examine is a *temporary disability insurance* (TDI) programme that covers workers who have exhausted their one-year sick pay entitlements from 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 programme has become very important in Norway, both because of its rapidly increasing caseload 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 programme, both in absolute terms and, even more strikingly, relative to the unemployment insurance programme. In the early 1990s, there were more than two

people claiming unemployment insurance (UI) for each person claiming temporary disability insurance (TDI). Two decades later, the pattern is dramatically reversed, with there now being around three to four TDI claimants for every one UI claimant.

To reliably identify the role of economic incentives, we take advantage of the full-scale overhaul of the TDI benefit scheme that occurred in January 2002. This overhaul introduced a new principle for the calculation of benefits, which went from being based on the entire labour-based income history of the individual to being based only on the income from labour observed in the last year (or the last three years) prior to disablement. Furthermore, the reform raised the minimum level of benefits and reduced the maximum child allowance payment. Although the reform was originally intended to be cost neutral, the data we use in the present paper show that it resulted in an average increase in the benefit level by around 14%. Perhaps even more importantly, it increased the benefit level for individuals with certain income paths and personal characteristics, whereas it reduced benefits for others. In terms of *absolute* benefit changes, the average change (positive or negative) generated by the reform was as large as 23%. We use this reform to examine how the compensation level affects the duration and outcome of TDI spells. We must emphasise that we are not primarily interested in the impacts of the reform as such, but in the way in which the reform can be exploited to identify behavioural parameters of more general interest – through the way it *idiosyncratically* changed individual TDI entitlements.

Our analysis is performed within the framework of a multivariate (mixed) hazard rate model, where the (log) benefit level is the explanatory variable of interest. To ensure that this variable permits us to identify the causal responses to the level of monetary compensation, we control for spurious correlations between replacement levels and individual resources/behaviours 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. In addition, the

analyses account for calendar time effects in a nonparametric fashion. Since the reform affected different claimants differently, this does not give rise to a perfect multicollinearity problem, but does ensure that the variation in benefits used to identify causal responses 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 affect the duration and outcomes of temporary disability insurance spells. The elasticity of the transition rate to employment with respect to the benefit level is estimated to be -0.33 , whereas the corresponding elasticities of the transitions to permanent disability, regular unemployment, and non-participation are estimated to be -0.25 , -0.39 , and -0.17 , respectively. In total, a 10% increase in the benefit level therefore implies a decrease of around 2-4% in the hazard rates out of TDI. To place the implied labour supply responsiveness in perspective, it may be noted that for unemployment insurance (UI) claimants in Norway, the elasticity of the transition rate to employment with respect to the UI benefit level has previously been estimated to average around -0.65 (Røed and Zhang, 2005). TDI claimants thus seem to be less responsive to financial incentives than UI claimants. The overall impact of the TDI reform – as reflected in the 14% increase in the average benefit level – was to raise the expected TDI duration by around three weeks and to reduce the proportion of insurance spells insurance periods eventually ending in a return to employment (within eight years) by approximately half a percentage point.

The remainder of this paper is structured as follows. Section 1 provides an overview of the institutional setting in which the reform took place. Section 2 describes the data and offers definitions of the outcome variables, while Section 3 presents some summary descriptive statistics. Section 4 develops the empirical strategy. The main results are presented in Section 5, while

the robustness and reliability checks are detailed in Section 6. Finally, Section 7 concludes the paper.

1. Institutional Setting

There are basically three (normally sequential) social insurance programmes that provide wage replacement for people 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, employees are also protected against displacement on grounds related to their sickness. The second form of insurance is the temporary disability insurance (TDI) programme, which is the programme examined in the present paper. It provides benefits to employees who have exhausted their sick pay – and who 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. TDI benefits typically amount to around two thirds of previous earnings, subject to a minimum and a maximum threshold. A TDI spell consists of periods of medical and/or vocational rehabilitation. During medical rehabilitation, the claimant receives medical or psychological treatment and/or is allowed to recover through rest. On the other hand, vocational rehabilitation includes participation in courses/education or work training intended to enhance employability. The medical rehabilitation period is in principle limited to one year, although additional periods are frequently granted. In the period covered by our data, there was no definite limit on the overall length of TDI. The third insurance programme is permanent disability insurance (PDI). These benefits also amount to around two thirds of previous earnings, and entail no further rehabilitation or activation attempts.

With the exception of a firm pay liability period during the first 16 days of sick pay, all three programmes are fully paid for by the state and financed through general (payroll) taxation.

In contrast to, for example, the workers' compensation programmes in the US, there is no requirement that the sickness/disability be work-related, although all tax-financed claims need to be certified by a physician. Indeed, in order to be eligible for TDI, an authorised physician must certify that health impairment is *the main cause* of the loss of at least 50% of the work capacity. However, the distinction between the disability and unemployment insurance programmes is somewhat blurred, since the law explicitly states that actual employment opportunities may be taken into account in the assessment of the health impairment.

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. Prior to 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 in the last one to three years before the disablement occurred or (if higher) during the best half of all previous years after the age of 17. Immigrants with only a few years of residence in Norway were not fully compensated. For breadwinners, there were substantial means-tested allowances for children and non-working spouses. Further, since the TDI benefit was considered a pension, it was subject to a lower tax rate than labour earnings.

Following 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% of earnings up to a ceiling of approximately NOK 500,000, which roughly corresponds to

USD 86,000 (2012).¹ The child allowances have been reduced by up to two thirds (but have also ceased to be means-tested), while the allowance for a non-working spouse has been completely removed. 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 involved a considerable rise in the minimum (annual) level of TDI benefits, from around NOK 82,000 (USD 14,000) before the reform to NOK 131,000 (USD 23,000) after the reform. Lastly, contrary to the pre-reform regime, immigrants now only need three years of residence in Norway in order to receive the same level of compensation as natives (and be entitled to the same minimum levels).

The reform was implemented in such a way that persons who were in an ongoing TDI spell at the time of the reform maintained the benefit level they already had (according to the old rules) for one more year; hence, they were not transferred to the new system until January 2003. This transitional rule was publicised in September 2001, approximately four months before the reform's implementation.²

The reform produced potential winners and losers (Hardoy *et al.*, 2004). Among the winners were claimants with very low or unstable past earnings and immigrants with only a few years of residence in Norway, particularly those without children. Among the losers were claim-

¹ All amounts in this paper are inflated to the 2012 value, and USD equivalents are computed on the basis of the exchange rate as of March 2013.

² When the reform was originally passed by the parliament in 1999, it was decided that persons with ongoing TDI spells at the time of the reform's implementation would be immediately entitled to the higher of the two benefit alternatives. However, this rather generous transitional rule was revoked before it came into force. In the budget proposal for 2002 (presented in September 2001), the government instead stated that it had revised the transitional rules so that all persons with ongoing spells would maintain their old benefit level for one year before being moved over to the new system. It was no longer possible to choose the best alternative. In an earlier version of this paper (Fevang *et al.*, 2013), we built the analysis on the presumption that the originally declared transitional rules had been implemented. As this turned out to be incorrect, most of the results in the paper have been revised. In practice, the results did not change very much and so all conclusions are basically the same as before.

ants with a recent decline in earnings as well as some claimants with many children and/or a non-working spouse. However, some claimants were more or less ‘sheltered’ from the pecuniary impact of the reform. In particular, public sector workers are covered by an occupational pension arrangement designed to shield them from changes in the level of social security benefits as it prescribes the same effective replacement rate of 66% (of the earnings level just prior to disablement) for all employees. TDI claimants with occupational pensions from the public sector were still affected by the reform to a certain extent, for example in the form of higher tax rates and changed child allowances.

The transitional rules implied an incentive for the reform’s losers (those with small or negative gains) to apply for TDI before January 2002 and for (some) winners (those with large positive gains) to postpone their application until after that date. The scope for such strategic planning was limited though, as in practice the timing is most often determined by the time of entry into the preceding one-year sick leave spell or – for unemployed jobseekers – by the occurrence of significant adverse health shocks or by new physician/caseworker assessments regarding existing health problems. Looking at the data, we actually find that the observed gains implied by the reform were slightly larger for those entering TDI in the last four months prior to the reform’s implementation (NOK 26,545) than for those entering in the first four months after (NOK 25,886), i.e. the opposite of what we would worry about if we were concerned about the strategic timing of entries. We nevertheless return to this issue below by assessing the robustness of our estimation results with respect to the inclusion/exclusion of spells potentially affected by timing considerations.

2. Data and Definition of Outcomes

Our data consists of merged administrative registers, encrypted to prevent the identification of individuals. They cover all TDI spells in Norway on a monthly basis. 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. We are also able to identify the outcome of each spell in terms of the main economic activity afterwards. Finally, our data includes 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), place of residence, and labour market history.

The starting point for our analysis is the set of all ‘new’ entrants to TDI in Norway during the period from the beginning of 1999 through to the end of 2004.³ A new entrant in a month t is defined as a person who has a recorded starting date in this month and who did not receive TDI benefits in any of the last 12 months prior to month t . We adopt this rather strict definition of ‘newness’ to ensure that we really do follow individuals from the beginning of a benefit claim period. A TDI 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 in the following four months ($t+1$, $t+2$, $t+3$ or $t+4$). Shorter periods out of TDI are censored (implying that we merge spells that are less than four months apart). There are basically four (mutually exclusive) ways in which a TDI spell can end: i) with a transition to employment, ii) with a transition to the permanent dis-

³ The claimants studied here all received so-called medical rehabilitation benefits and/or vocational rehabilitation benefits (‘rehabiliterings/attføringspenger’). In 2010, the transfer changed name to work assessment benefits (‘arbeidsavklaringspenger’).

ability insurance programme, iii) with a transition to regular registered unemployment (with or without unemployment insurance eligibility), or iv) with uninsured non-participation (or to jobs that are too small to satisfy our definition of ‘employment’; see below). One important goal of the programme is to promote transitions to employment. Claimants may apply for permanent disability benefits, however, if they still consider themselves unfit for regular work, normally only after appropriate rehabilitation attempts have been made (or at least seriously considered by the Social Security Administration). Claimants may also be declared fit for work even when they do not have a job to go to. In such cases, they may choose to register as unemployed or simply withdraw from the labour force without any income support (except, possibly, for means-tested social assistance).

In our main model specification, we define ‘employment’ as having labour earnings and/or business income amounting to at least NOK 7,000 (USD 1,200) per month during the 12-month period directly following the exit from TDI. In annualised terms, this threshold corresponds to the so-called ‘base amount’ in the Norwegian public pension system, which for the period considered in this paper defined the lowest earnings level required to accumulate pension entitlements. From a comparative perspective, it may also be noted that it roughly corresponds to the ‘substantial gainful activity’ threshold used by the Social Security Administration to determine disability benefit eligibility in the US.⁴ We give priority to employment transitions, implying that those transitions for which we observe both employment and a social security transfer are defined as transitions to employment.

Our main sample consists of people aged 27 to 55 who were registered as 100% disabled at the time of entry to TDI and who were not covered by an occupational pension in the public

⁴ In the robustness analysis, we use a wider employment definition as an alternative.

sector. The focus on people who were classified as 100% disabled at the time of entry is motivated both by the fact that this group constitutes the vast majority (86%) of the TDI claimants in our data and to ensure that we have a relatively homogenous group in terms of employment situation. For both fully and partially disabled claimants, the dominant medical diagnoses are musculoskeletal diseases, back pain, and mental disorder. While people with a 100% disability typically do not have an employer, many partially disabled people continue in their previous job, albeit with reduced hours. Indeed, it is the observation that a person actually has a job that normally causes the disability status to be defined as partial rather than full. The mechanisms that determine exits from TDI are thus likely to be different for these two groups. We do also report some results for the partially disabled, but – due to data limitations – these will be based on a slightly simplified version of our main model.

The minimum cut-off age of 27 is imposed because there are special rules for people who become disabled before that age due to particularly serious and objectively verifiable disabilities. Unfortunately, based on our data, we are not able to identify this group. The maximum cut-off age of 55 is imposed so as to avoid issues related to early retirement. Finally, the reason we drop people covered by an occupational pension in the public sector is that such claimants were sheltered from the main impacts of the reform (see Section 1 above). Further, our data does not allow us to correctly compute the moderate impacts that they nevertheless were exposed to. This leaves us with 156,658 TDI spells in our main sample.

3. Descriptive Statistics

Table 1 offers some descriptive statistics regarding the main sample, divided into pre- and post-reform entrants. As we explain in the next section, the identification strategy pursued in this paper does *not* require the characteristics distributions of entrants to be the same before and after

the reform; hence, Table 1 is not intended to serve as input to a balancing test. The composition of claimants is likely to vary from year to year for reasons that are unrelated to the TDI reform, such as cyclical fluctuations and changes in the demographic composition of the population. What is important for our purpose is that any changes in the composition were not initiated by the reform, a point to which we return below.

The primary role of the reform in our analysis is to provide a source of exogenous variation in TDI benefits. Since TDI benefit entitlements in general are determined on the basis of the claimants' own past labour market behaviour, the cross-sectional variation in benefit levels is clearly anything but randomly assigned. The average rise in the benefit level that resulted from the reform cannot be exploited to identify its causal effects either, since confounding changes in the economic environment may have occurred. In particular, the reform coincided with a moderate cyclical downturn in Norway that resulted in rising unemployment during 2002 and 2003, followed by a very strong recovery (see Figure 1). Instead, we exploit the fact that the 2002 reform induced a *random assignment-like* source of variation in benefit levels related to the exact *timing* of entry into the programme. It is the *idiosyncratic* impacts of the reform on individual benefit levels – and these alone – that we use to identify the behavioural responses to the level of TDI benefits.

Table 1.
Descriptive statistics for the main sample

	I	II
	Pre-reform entrants (1999-2001)	Post-reform entrants (2002-2004)
Number of new TDI entrants (N)	69,508	87,150
Individual and background characteristics (at entry)		
Demographic characteristics		
Mean age	40.2	40.3
Women (%)	50.2	50.9
Married/cohabiting (%)	51.8	50.6
Separated/divorced (%)	18.8	18.5
Children below 18 years of age (%)	47.7	47.8
Immigrants (%)	14.6	17.2
Educational attainment (%)		
Compulsory school only	42.6	39.7
Lower secondary school	20.1	16.9
Upper secondary school	25.7	29.2
College/University education	10.4	12.5
Unknown	1.2	1.7
Employed previous year (%)	83.8	83.7
Average annual earnings for last 3 years (NOK, 2012 value)	300,915	307,310
Months with social insurance payments over last 3 years	16.3	16.8
Mean level of annual TDI benefits at entry (NOK, 2012 value)		
Mean level according to pre-reform rules	188,076	217,010
Mean level according to post-reform rules	213,158	217,010
Outcomes		
Mean duration (# of months, right-censored at 48 months)	24.7	24.6
Outcome of spells (%)		
Regular employment	43.0	45.4
Permanent disability	18.0	16.8
Unemployment	1.5	2.0
Non-participation (without income support)	17.1	17.5
Spells still in progress after 48 months	20.4	18.4
Economic status four years after entry to TDI (%)		
Regular employment	39.5	45.7
Permanent disability	24.4	23.0
Unemployment	1.6	1.0
Non-participation (without income support)	10.4	9.7
Still/back in TDI	24.2	20.7

Note: To ensure the comparability of the pre-reform and post-reform outcome distributions, we have right-censored all TDI spells after 48 months for the purpose of calculating the mean durations in this table, since this is the longest time period for which our data allows us to directly track the TDI spells starting at the end of our observation period.

We will now provide a more detailed description of how the reform actually affected individual benefit levels. Let b_i^o be 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).⁵ For new entrants in 1999-2001, actual benefits b_i^a are equal to b_i^o until January 2003, after which they are equal to b_i^n . For new entrants in 2002-2004, b_i^a is always equal to b_i^n . Given the rich data available, we are able to compute both benefit levels (b_i^o, b_i^n) for all entrants, exactly as they were paid out or *would have* been paid out had the claimant entered TDI during the other regime. 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 the hypothetical benefit levels for almost everyone. The average *absolute* size of the change ($\frac{1}{N} \sum_i |g_i|$) was as large as NOK 43,399 (USD 7,500), which corresponds to 23% of the average pre-reform benefit level. There were more hypothetical winners (76%) than losers, and the average gain was NOK 26,182 (USD 4,500). The second point to note is that the gains distributions were quite similar for entrants before and after the reform. Although there was a slight movement toward entrants benefitting from the reform, the average gain among post-reform entrants was only NOK 1,877 (USD 320) higher than for pre-reform entrants, which is less than 1% of the average benefit level. This

⁵ Since post-tax benefit levels are affected by potentially endogenous factors (such as the labour supply of other family members), we compute the 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).

indicates that the reform's effect on the pattern of entries was minor. This is further illustrated in Figure 3, where we have plotted the number of entrants and their average gain levels for each month. The number of entrants trended upwards throughout our observation window. There was a rise in average gains during 1999 and the first half of 2000, but no conspicuous changes around the time of the reform in 2002. 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. We return to the validity of the exogeneity assumption in a separate robustness and reliability analysis below.⁶

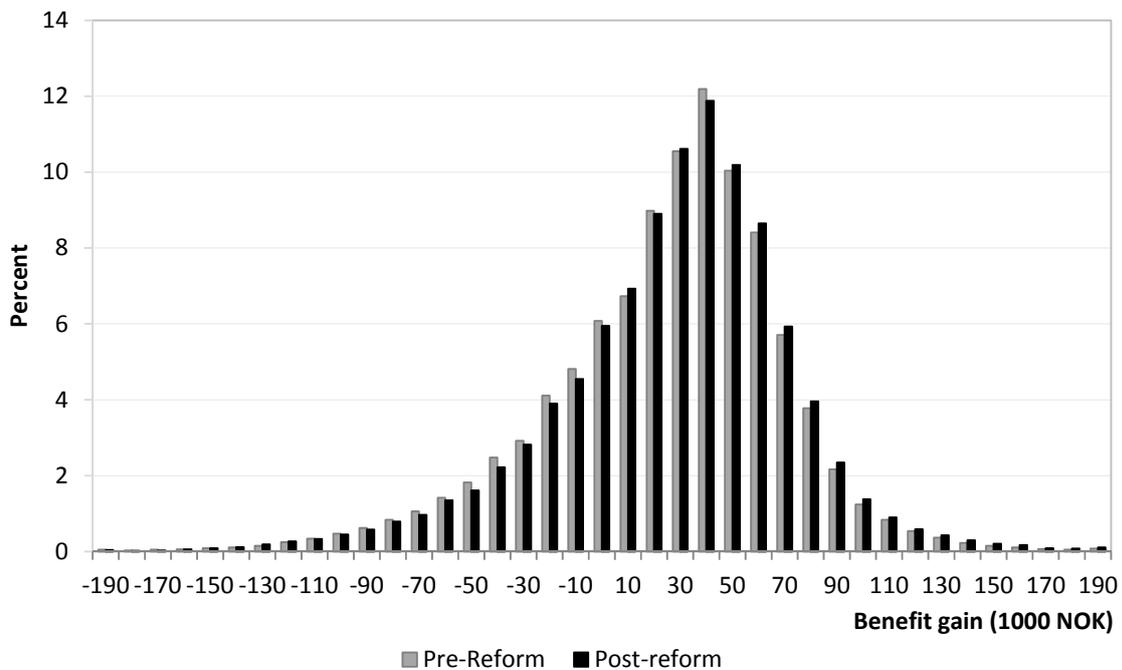


Fig. 2. The distribution of annualised benefit gains (g_i) for TDI entrants before and after the reform (1000 NOK, inflated to 2012 values).

Note: The benefit gain is computed as the benefit entitlement according to the new rules minus the benefit entitlement according to the old rules. Numbers on the horizontal axis indicate cell-midpoints, with the range of each cell being NOK 10,000 (except at the two ends, where, for example, 190 means > 185,000).

⁶ The upper panel of Figure 3 indicates that there was a marked drop in the number of entrants in November 2001, i.e. two months prior to the implementation of the reform. However, since there were no corresponding changes in average gains, this does not seem to have resulted from the “strategic” timing of entry. In the robustness analysis below, we check the sensitivity of our results with respect to the inclusion of entrants just before and just after the reform. We also assess potential selection bias by means of placebo regressions.

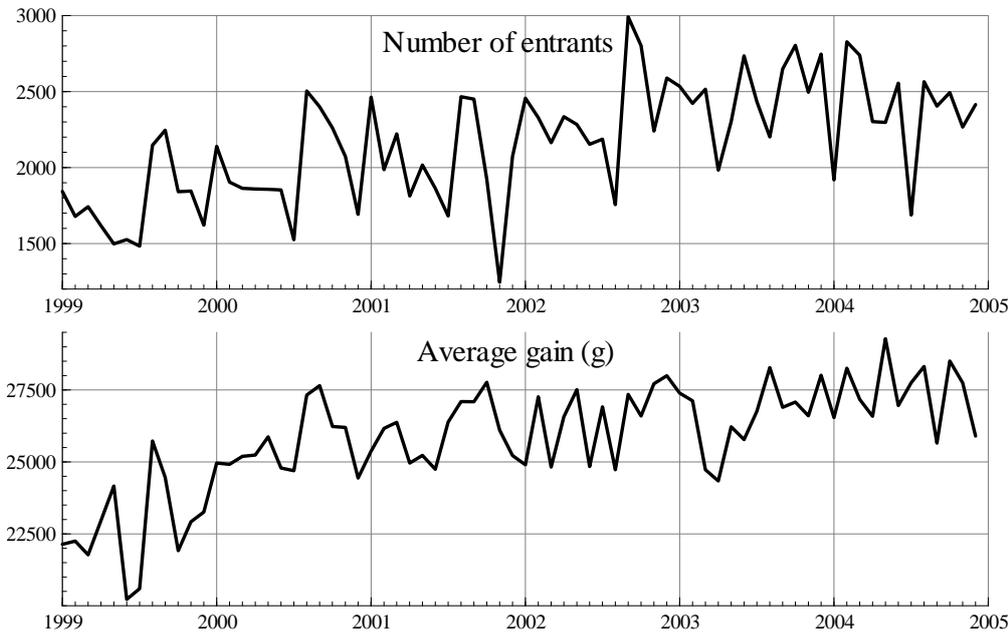


Fig. 3. Number of entrants to TDI each month (upper panel) and their average hypothetical gains (lower panel).

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*. Hence, in order to descriptively single out indications of reform effects, we can look at time trends in the relative outcomes for these two groups. A simple comparison of spell durations for potential winners ($g_i > 0$) and losers ($g_i < 0$) before and after the reform shows that whereas the average duration of spells dropped from 25.8 to 24.5 months for potential losers, it increased from 24.3 to 24.6 months for potential winners (with spell durations censored at 48 months in both cases). A simple difference-in-difference estimate would thus indicate a 1.6 month increase in spell duration resulting from being a potential winner rather than a potential loser. To investigate the time trends in more detail, Figure 4 shows, for entrants from January 1999 through December 2004, how the fraction exiting the programme within one year developed for potential winners relative to potential losers. Although there are significant high-frequency movements in this ratio, there

seems to have been a considerable downward shift around the time of the reform. While the potential winners tended to have around a 10-20% higher probability of ending their spell within one year before the reform, they had similar – or even slightly lower – probabilities afterwards. This is exactly what we would expect to see if the benefit level has a positive causal effect on the duration of the TDI spell.

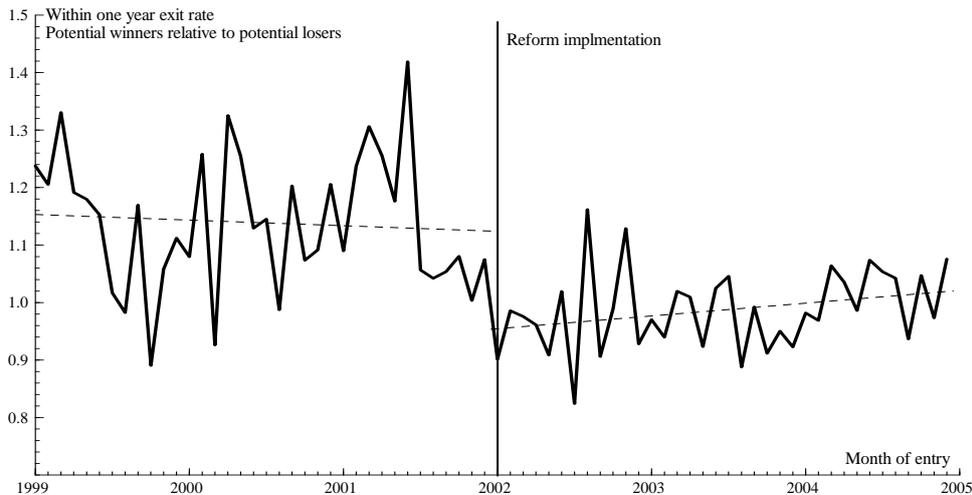


Fig. 4. Ratio of within one year exit rates from TDI for hypothetical winners and losers of the 2002 reform by month of TDI entry (January 1999 to December 2004).

Note: Dotted lines are OLS regression linear trend lines for the before and after reform periods respectively, based on the 72 monthly outcome ratio observations. The two slope coefficients are both insignificantly different from zero (t-values -0.51 and 1.18, respectively). The negative shift in the intercept at the time of the reform is statistically significant (point estimate -0.20 and t-value -4.26).

Although the observed behaviour of the potential winners and losers indicates that there were significant behavioural responses to the changes in benefit levels, it is difficult to assign these responses a clear quantitative interpretation, since they represent averages over a distribution of benefit changes. We address this issue in the next section.

4. Empirical Strategy

We estimate a number of multivariate (competing risks) mixed proportional hazard rate models to quantify the effects of marginal changes in the benefit level on the duration and outcome of TDI spells. The models are designed to exploit the random assignment-like variations in benefit levels

arising from the reform. An important element of our strategy is to use *both* the two hypothetical (b_i^o, b_i^n) levels *and* the actual benefit level (b_i^a) as explanatory variables in the statistical analysis (recall that $b_i^a = b_i^o$ for entrants in the old regime and that $b_i^a = b_i^n$ for entrants in the new regime). The idea is that the hypothetical benefit levels then capture all of the spurious effects arising from the fact that the benefit schedules depend on past behaviour, 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 disablement.

We now explain our empirical strategy in more detail. We start with $i=1, \dots, N$ new entrants to TDI during the period from January 1999 to December 2004. Let $k=1, \dots, 4$, denote the set of potential events, i.e. employment ($k=1$), permanent disability ($k=2$), unemployment ($k=3$), and non-participation without income support ($k=4$). TDI 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.

As we only observe labour market status at the end of each month, we formulate the statistical model in terms of grouped hazard rates. To begin with, we write the integrated month-specific hazard rates φ_{kit} as functions of the benefit level b_i^a , calendar time s_t , local labour market tightness LMT_{it} , TDI spell duration d_{it} , observed (time-varying) individual characteristics x_{it} , and unobserved (time-invariant) individual characteristics v_{ki} :

$$\varphi_{kit} = \int_{t-1}^t \theta_{kis} ds = \exp\left(\delta_k \log(b_i^a) + \sigma_{kt} s_t + \gamma_k LMT_{it} + \lambda_{kd} d_{it} + \beta_k x_{it} + v_{ki}\right), \quad k = 1, \dots, 4, \quad (1)$$

where θ_{kis} is the underlying continuous-time hazard rate, which is assumed to be constant within each month. The vector s_t contains one indicator for each calendar time quarter in the analysis period (39 dummy variables). The local labour market tightness indicator LMT_{it} is computed on

the basis of auxiliary register data covering all (non-disabled) unemployed jobseekers in Norway and measures for each of Norway's 90 travel-to-work areas and for each month the (log of the) fraction of unemployed jobseekers who found work. The vector d_{it} contains indicator variables denoting claimant i 's spell duration measured in quarters. The vector of individual characteristics x_{it} contains the following variables: gender and family situation (9 dummy variables), nationality (2 dummy variables), education (4 dummy variables), age (32 dummy variables), county (18 dummy variables), and labour market tightness at the time of entry.⁷ All of the explanatory variables in (1) are explained in more detail in a separate appendix.

The main parameters of interest are the benefit elasticities (δ_k). As noted above, the benefit level b_i^a is computed in a way that makes it dependent on past labour market behaviour and current family situation in a rather complex manner; hence, it is unlikely that b_i^a is uncorrelated with the unobserved characteristics v_{ki} . However, provided 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:

$$v_{ki} = \alpha_{ok} \log(b_i^o) + \alpha_{nk} \log(b_i^n) + \varepsilon_{ik}, \quad k = 1, \dots, 4. \quad (2)$$

We then have – by construction – that ε_{ik} is orthogonal to $\log(b_i^a)$. 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 as calculated according to, say, the pre-reform rules can have causal effects in the pre-reform period only, its spurious effects are at work in the

⁷ Local labour market tightness at the time of TDI entry is included to account for any cyclical variation in the sorting into the programme.

post-reform period as well. Consequently, the hazard rates used to estimate the model are specified as:

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

$$k = 1, \dots, 4.$$

To further illustrate how this model identifies the benefit elasticities, it may be noted that if we let R_i be a reform dummy (a variable equal to one for those whose benefits are calculated according to the new rules and zero otherwise) we have that $\log(b_i^a) = R \log(b_i^n) + (1-R) \log(b_i^o)$.

Hence,

$$\delta_k \log(b_i^a) + \alpha_{ok} \log(b_i^o) + \alpha_{nk} \log(b_i^n) = (\alpha_{ok} + \delta_k (1-R_i)) \log(b_i^o) + (\alpha_{nk} + \delta_k R_i) \log(b_i^n), \quad (4)$$

which highlights that the causal effect δ_k is identified in Equation (3) by the ‘extra’ correlations between the old/new benefit levels and the hazard rates that arise in periods in which the two respective incentive variables actually apply. While the old and new benefit levels, as well as the reform indicator, are represented in Equation (3) as control variables (the reform indicator through the calendar time dummy variables), it is the *interactions* between the reform indicator and the two incentives variables that identify the particular behavioural effects of interest. Note that the right-hand side of Equation (4) contains the four variables $\{(1-R_i) \log(b_i^o), R_i \log(b_i^n), \log(b_i^o), \log(b_i^n)\}$, and that our model implies the restriction that the two first $\{(1-R_i) \log(b_i^o), R_i \log(b_i^n)\}$ have exactly the same effect (same causal effect of the benefit level before and after the reform). It therefore follows that the model is identified.

In this model, it is assumed that the causal incentive effect is – at each point in time – appropriately represented by the currently applicable benefit level b_i^a . This is uncontroversial insofar as the benefit level is constant throughout the spell. However, as explained above, the transi-

tional rules implied that persons with ongoing spells at the time of the reform would have their benefit levels adjusted one year after the reform. Farsighted individuals may take this into account before it happens; hence, the new benefit level may give rise to a causal effect some time before the adjustment actually takes place. To assess the potential impact of this mechanism, we estimate, as part of a robustness analysis, a model where we right-censor all ongoing spells at the time of the reform (i.e. one year before the new benefit level was implemented for ongoing spells). In addition, we estimate the model (with uncensored data) under the alternative assumption that the *new* benefit level became causally relevant immediately (i.e. we disregard the transitional rules and set up the model *as if* all adjustments took place January 2002).

To avoid unjustified restrictions on the distribution of unobserved heterogeneity, we estimate the model in a completely nonparametric fashion, thus implying that unobserved heterogeneity is treated as a joint discrete distribution with an unknown number of support points. Following recommendations by Gaure *et al.* (2007), we have used the Akaike Information Criterion (AIC) for model selection. The likelihood function as well as the algorithm used to maximise it are described (for a model with exactly the same structure as the one used here) in Røed and Westlie (2012).

5. Main Estimation Results

Table 2 presents the main estimation results from our hazard rate model. The results indicate that higher benefits significantly reduce exit rates from TDI. The estimated benefit elasticities are -0.33 for the transition to employment, -0.25 for the transition to permanent disability, -0.39 for the transition to unemployment, and -0.17 for the transition to unsupported non-participation. The results in Table 2 also show that the transition rate to employment is positively affected by local labour market tightness. The estimated elasticity of 0.19 implies that a 10% increase in the ob-

served local transition rate from regular unemployment to employment implies a 1.9% increase in the hazard rate from TDI to employment, *ceteris paribus*.

Table 2.
Estimation results for fully disabled TDI entrants - main sample (standard errors in parentheses)

	I	II	III	IV
	Employment	Permanent disability	Unemployment	Non-participation
The actual benefit level				
Log actual benefit level (causal effect)	-0.325*** (0.039)	-0.251*** (0.058)	-0.387*** (0.138)	-0.166*** (0.045)
Local labour market tightness				
Log monthly transition rate from regular unemployment to employment in travel-to-work area	0.190*** (0.046)	0.012 (0.073)	-0.075 (0.239)	0.137* (0.075)

Note: The number of TDI spell-observations is 156,658. The preferred model had eight support points in the heterogeneity distribution. *(**)(***) = statistically significant at the 10(5)(1)% level. The estimated models include the following control variable sets: gender and family situation (9 dummy variables), nationality (2 dummy variables), education (4 dummy variables), age (32 dummy variables), county (18 dummy variables), calendar time quarter (39 dummy variables), spell duration (12 dummy variables), hypothetical benefit levels (2 variables), and local labour market tightness at entry (1 variable).

The labour supply responsiveness implied by the benefit elasticity is considerable, yet it is still smaller than what has previously been estimated for unemployment insurance (UI) claimants. Based on a similar econometric approach – and again with access to random assignment-like variation in benefit levels – Røed and Zhang (2005) estimate that the elasticity of the transition rate to employment with respect to the UI benefit level is -0.65, i.e. twice as large as the one estimated for TDI claimants here. Financial incentives are thus more important for UI claimants than for TDI claimants. The sensitivity of the TDI claimants' employment transitions with respect to both the benefit level and the local labour market tightness nevertheless suggest that there is a considerable work capacity among TDI claimants that can be mobilised under the right circumstances. As explained in Section 1, the Social Security Administration is supposed to take a person's actual employment opportunities into account when deciding on whether or not a given health impairment satisfies TDI eligibility rules. Hence, even for the 100% disabled, the health problems

need not be such that any kind of work is impossible. For many claimants, this probably implies that the stronger their work motivation is, and the better the local labour market conditions are, the less likely it is that a given health impairment becomes a decisive barrier to employment.

Given that the TDI benefit level affects all four exit rates to varying degrees, it is not clear from Table 2 how it influences the duration of the TDI spell or the distribution of final destination states. To shed light on the overall effects associated with the full set of competing risks, we provide a simulation exercise, which is based on observed individual characteristics at entry and draws from the estimated distribution of unobserved heterogeneity, where we use the estimated model to simulate event histories. These simulations are produced under alternative assumptions regarding the level of TDI benefits: i) as they are recorded in the data, ii) as they were computed in the old regime; and iii) as they are computed in the new regime. We use these simulations to provide estimates of the overall *reform* effects, i.e. the impacts on average spell duration and destination state distribution of substituting the new benefit system for the old, *ceteris paribus*. Since the new benefit system implied a 14% rise in the average benefit level, we clearly expect that the reform resulted in longer spell durations. It is less clear how it affected the distribution of outcomes. To obtain confidence intervals for the estimates of interest, we use a parametric bootstrap procedure; i.e., we make 120 random draws from the distribution of parameter estimates (based on the assumption of multivariate normality) and use each drawing in a separate simulation exercise.⁸ In the simulations, each entrant is followed month-by-month until some transition takes place on the basis of ‘lotteries’ where the predicted monthly transition probabilities are compared

⁸ Note that we make draws from the vector of 484 parameters attached to observed covariates only, since the parameters describing the unobserved heterogeneity are not normally distributed (Gaure *et al.*, 2007). We thus condition on the individual draws of unobserved heterogeneity. The draws of parameter estimates are made by means of the Cholesky decomposition. That is, let L be a lower triangular matrix, such that the covariance matrix is $V = LL'$. Let z_s be a vector of 484 draws from the univariate standard normal distribution collected for trial s . Let \hat{b} be the vector of point-estimates. The parameters drawn for trial s are then given as $b_s = \hat{b} + Lz_s$.

with draws from uniform distributions. To avoid extrapolations outside our empirical basis, TDI spells still ongoing after eight years are right-censored.

The results of the simulations are presented in Table 3 (Columns II-V), together with a description of the actual recorded outcomes (Column I). A comparison of the recorded outcomes and the simulation results based on correct benefit levels (Column II) shows that the simulation model reproduces the distribution of the actual observed outcomes relatively well. Moving on to simulations based on the old and new benefit levels (Columns III and IV) and the resultant reform impact estimates (Column V), we find that the reform caused the average duration of TDI spells to increase by around 0.7 months (21 days). This number is based on the inclusion of the very long right-censored spells with their censored durations (eight years). Further, since the reform also raised the fraction of very long spells by approximately 0.4 percentage points, we expect the total duration effect to be somewhat larger. Looking at the distribution of destination states, our simulation results show that the reform reduced the fraction of spells ending in employment within eight years by around 0.5 percentage points; whereas the fractions ending in disability, unemployment, or non-participation were largely unchanged. Our results therefore indicate that the primary effect of the reform-initiated benefit increase was to reduce the number of employment transitions in favour of a higher fraction of ongoing spells after eight years. While more generous TDI benefits clearly postpone transitions to alternative social insurance programmes (unemployment and permanent disability insurance), we cannot say for certain that the fraction eventually moving on to these programmes is reduced.

Table 3.
Simulation results

	I	II	III	IV	V
	Observed in data	Simulated, based on actual benefit levels	Simulated, based on benefit levels calculated with old rules	Simulated, based on benefit levels calculated with new rules	Estimated reform effect (14% increase in benefit level on average) [95% confidence interval]
Mean duration (months)	28.46	31.23	30.82	31.52	0.70 [0.51, 0.90]
Outcome of TDI spells (%)					
Regular employment	51.83	50.75	51.05	50.53	-0.52 [-0.86, -0.10]
Permanent disability	20.84	19.58	19.56	19.60	0.04 [-0.23, 0.31]
Unemployment	2.03	2.84	2.88	2.84	-0.04 [-0.24, 0.11]
Non-participation	19.52	21.54	21.48	21.59	0.11 [-0.16, 0.45]
Right-censored	5.77	5.28	5.04	5.44	0.41 [0.18, 0.60]

Note: The results reported in Columns II-V are based on 120 simulation sets, each based on a separate drawing of parameters (parametric bootstrap). The reported mean durations include right-censored observations (with the duration recorded at censoring). In the observed data (Column I), right-censoring occurs at the end of 2008. In the simulations, right-censoring occurs eight years after entry. The reported numbers are the averages over all 120 simulation sets, and 95% confidence intervals are obtained by removing the three most extreme simulation results at each end.

As explained above, in our main sample we have only included people who were classified as 100% disabled at the time of entry into the programme. However, we have also estimated the model for entrants who were classified as partially disabled at this point, and who probably had a job at the time of entry. To be able to estimate the model with this smaller sample, we had to merge the two outcomes of unemployment and non-participation. The results are presented in Table 4. The estimated benefit elasticity for the transition to employment is here slightly larger than that for the main sample, i.e. -0.35, whereas the impact of local labour market conditions is smaller (and also statistically insignificant). The latter is as expected, given that most of the TDI claimants in this dataset already have an employer.

Table 4.
Estimation results for partially disabled TDI entrants (standard errors in parentheses)

	I	II	III
	Employment	Permanent disability	Unemployment or non-participation
The actual benefit level			
Log actual benefit level (causal effect)	-0.349*** (0.075)	-0.127 (0.237)	-0.664*** (0.214)
Local labour market tightness			
Log monthly transition rate from regular unemployment to employment in travel-to-work area	0.137 (0.092)	-0.118 (0.273)	-0.239 (0.332)

Note: The number of spell-observations is 26,179. The preferred model had four support points in the heterogeneity distribution. *(**)(***) = statistically significant at the 10(5)(1)% level. For the list of control variables, see the note to Table 2.

6. Robustness and Reliability

Can we be sure that the benefit elasticities estimated in this paper actually represent causality?

We note two potential sources of bias. The first is *selected inflows*, i.e. changes in the pattern of inflows to TDI caused by the changes in the benefit schedule. This could violate the assumption embedded in Equation (2) that there is a stable correlation between unobserved heterogeneity and the hypothetical benefit levels. The second potential source is *biased time developments*, i.e.

changes in the economic environment (apart from the benefit level) that affected people with different benefit gains systematically differently.

To empirically assess the potential problem of selected inflows, we check for possible ‘irregularities’ around the time of the reform by re-estimating the model with all spells starting from six months before to six months after the reform excluded from the analysis. As can be seen from Table 5, this does not change the results much, and the estimated employment elasticity is almost exactly the same as in the main sample.

Table 5.
Estimated effect of actual benefit level for fully disabled entrants – alternative models (standard errors in parentheses)

	I	II	III	IV
	Employment	Permanent disability	Un-employment	Non-participation
Main model/sample (from Table 2; N=156,658)	-0.325*** (0.039)	-0.251*** (0.058)	-0.387*** (0.138)	-0.166*** (0.045)
Reduced samples				
Excluding TDI spells that started from 6 months before to 6 months after the reform (N=135,591)	-0.322*** (0.042)	-0.272*** (0.062)	-0.279* (0.155)	-0.158*** (0.049)
Excluding TDI spells that started with vocational rehabilitation (N=145,112)	-0.324*** (0.034)	-0.272*** (0.052)	-0.330** (0.138)	-0.162*** (0.041)
Excluding TDI spells that started in 2004 (N=128,186)	-0.332*** (0.040)	-0.245*** (0.060)	-0.437*** (0.147)	-0.161*** (0.046)
‘Placebo’ samples				
Erroneously placed reform in pre-reform period (N=69,508)	-0.053 (0.070)	-0.110 (0.141)	-0.043 (0.349)	0.138 (0.095)
Erroneously placed reform in post-reform period (N=87,150)	0.024 (0.057)	-0.192 (0.126)	0.152 (0.214)	0.021 (0.073)
Alternative modelling assumptions (N=156,658)				
More flexible controls for pre-reform and post-reform benefit levels (quadratic function, including interaction term)	-0.332*** (0.037)	-0.217*** (0.054)	-0.384*** (0.137)	-0.130*** (0.045)
With ongoing TDI spells right-censored at the time of the reform	-0.396*** (0.044)	-0.414*** (0.072)	-0.503*** (0.156)	-0.249*** (0.053)
Without imposing transitional rules for ongoing TDI spells	-0.287*** (0.036)	-0.351*** (0.056)	-0.276** (0.126)	-0.210*** (0.044)
Applying a wider definition of employment (lower earnings threshold)	-0.318*** (0.036)	-0.218*** (0.058)	-0.402** (0.170)	-0.158*** (0.048)

Note: The variables included in these models are the same as those reported for Table 2.

*(**)(***) = statistically significant at the 10(5)(1)% level.

To investigate whether economic trends systematically correlate with the individual factors determining benefit gains/losses, we present in Table 5 the results from two ‘placebo’ analyses. They are constructed by imposing *false* reforms in the middle of the pre-reform and the post-reform three-year periods, i.e. we estimate the models *as if* the reform occurred at these times.⁹ Again, the results tend to indicate that there were no biased trends, thereby supporting the causal interpretation of the effects identified in our main model. A potential concern is that the Social Security Administration somehow treated the potential winners and losers of the reform differently, for example by prioritising the losers in the allocation of vocational rehabilitation programme slots post-reform. To examine this possible problem, we have collected data on the timing of the first transition to vocational rehabilitation, and have estimated a separate single risk hazard rate model with vocational rehabilitation as the only outcome (all other transitions entail right-censoring) and with exactly the same explanatory variables as those used in the main analysis (Equation 3). The estimated impact of the actual benefit level is then -0.032 (with a standard error of 0.033), which is small in magnitude as well as statistically insignificant. In this estimation, we dropped people who had already participated in vocational rehabilitation from the start of the TDI spell (7% of the sample). To ensure that our main results are not driven by a selection problem associated with this particular group, we also re-estimated the main model on this reduced sample (see Table 5). The results are almost identical to those obtained for the complete sample. A final concern is that other coincident policy changes could have altered the composition of TDI entrants in some way that correlates with the key incentive variables. However, we do not see any clear candidates for this, with the possible exception of the introduction of a new

⁹ We right-censor all TDI spells at the end of the three-year periods to maintain the symmetry embedded in the main model, and also to prevent the spells from being affected by the genuine reform.

semi-permanent disability insurance programme in 2004, which in some cases could possibly have substituted for the TDI programme evaluated in this paper. To check for this, we have also estimated the model without the 2004 entrants. Again, the results provided in Table 5 show that the coefficient estimates of interest remain virtually unchanged.

We also assess robustness with respect to some modelling choices (see the lower panel of Table 5). First, to examine whether our results are sensitive to the way we have controlled for the pre-reform and post-reform benefit levels, we allow these controls to enter in a quadratic rather than a linear fashion (i.e. we add $(b_i^o)^2$, $(b_i^n)^2$, and $b_i^o b_i^n$ to Equations (2) and (3)). This has virtually no impact on the estimates of interest. Second, to check whether our results are sensitive with respect to the treatment of TDI spells that were ongoing at the time of the reform (January 2002) – and, hence, were subject to the transitional rules – we right-censor all ongoing spells at the time of the reform’s implementation. This raises the estimated elasticities somewhat. A plausible interpretation of this finding is that if the new benefit level gradually becomes more causally relevant as the end of the transitional period approaches, the right-censoring of these spells essentially implies elimination of a source of measurement error. As an alternative check on the role of these spells, we instead drop the imposition of the transitional rules, i.e. we assume that all ongoing spells were granted the new benefit immediately upon the reform’s implementation. In this case, the estimated effects on employment and unemployment transitions become somewhat smaller, perhaps indicating that this specification induces *more* measurement error. Notably, all the main conclusions remain unchanged regardless of the specification. Finally, we assess the results’ robustness by using an alternative – and wider – definition of employment, which also encompasses earnings levels that are only half of the substantial gainful activity threshold used in

the main analysis. The number of job transitions then rises by 4.4 percentage points. However, the estimated benefit elasticities change only slightly (see the bottom row of Table 5).

7. Conclusion

Based on Norwegian administrative registers, we have utilised a large ‘social experiment’ consisting of the complete overhaul of the temporary disability insurance (TDI) system to estimate the impacts of economic incentives on the duration and outcome of TDI spells. For fully disabled TDI claimants, we find that a 10% cut in the benefit level induces approximately a 3.3% increase in the hazard rate to regular employment, a 2.5% increase in the hazard rate to permanent disability, and a 3.9% increase in the hazard rate to regular unemployment (the latter from a very low level). We also find that the transition rate from temporary disability to employment is sensitive with respect to labour market conditions. An increase in the local labour demand corresponding to a 10% increase in the transition intensity from unemployment to employment yields approximately a 1.9% increase in the transition intensity from TDI to employment for fully disabled recipients. The results for the much smaller group of partially disabled TDI claimants indicate similar responses.

The estimated labour supply responses reported in this paper are roughly half as large as those previously estimated for unemployed jobseekers in Norway. This suggests that temporarily disabled persons do indeed face a more restricted labour market choice set than the registered unemployed. Given that the people included in our analysis are deemed to be seriously disabled, we nevertheless consider the identified responses to be substantial. Taken together, our results support the view that there exists a significant labour supply potential among temporarily disabled persons, and that the realisation of this potential can to some extent be encouraged by means of financial incentives. Although this probably implies that there is an efficiency loss associated

with the relatively generous TDI benefit level in Norway (with replacement rates around 66%) – in terms of too low transition rates to regular employment – it does not necessarily follow that a lower benefit level is the desired policy from a welfare perspective. Generous TDI benefits protect those who are unable to find substantially gainful employment, as well as their dependents, from poverty. In the presence of liquidity constraints, it also provides claimants who ultimately have the capacity to return to the labour market with more time in which to find a suitable and viable job match.

Although the behavioural responses to the TDI benefit level are both substantively and statistically significant, they are much too small to suggest that the 2002 reform, which implied an average increase in benefit levels of 14%, can fully explain the large increase in TDI claimants in Norway. Our point estimates imply that the reform, by lowering the exit rates, may have been responsible for increasing the stock of TDI claimants by approximately 4-5%. By comparison, the number of TDI claimants rose by as much as 35% from 2001 (the last year before the reform) to 2004, and this increase appears to have been part of a rather persistent upwards trend (see Figure 1). Hence, in order to understand the overall rise in TDI programme participation, other explanations are called for.

Appendix: Overview of Explanatory Variables

The hazard rate models estimated in this paper include the following explanatory variables:

Incentives and labour market conditions:

Log (actual benefit level)

Log (benefit level calculated on the basis of old rules)

Log (benefit level calculated on the basis of new rules)

Log (local monthly transition rate from unemployment to employment) at the time of TDI entry

Log (local monthly transition rate from unemployment to employment) in current quarter

Individual characteristics (time-varying variables are updated during TDI spells):

Sex (one dummy variable=1 for females)

Age (one dummy variable for each age 27, 28, ..., 59)

Children (three dummy variables; for having children in the age categories 0-3, 4-6, and 7-12)

Sex and children (three interaction terms; between sex (female) and the three children-related variables)

Married/cohabiting (one dummy for being married/cohabiting)
 Divorced/separated (one dummy for being divorced/separated)
 Immigration status (two dummy variables; for being an immigrant from an OECD country and a non-OECD country, respectively)
 Education (four dummy variables; for having upper secondary, higher secondary, college/university, or unknown education, respectively)

Geography

One dummy variable for each county in Norway (1, 2, ..., 19)

Time

One dummy variable for each quarter (1999.1, 1999.2, ..., 2008.4)

Spell duration

One dummy variable every four months (1, 2, ..., 13)

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