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Unemployed or disabled? Disability screening and labor market outcomes of youths

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Unemployed or Disabled?

Disability Screening and Labor Market Outcomes of Youths*

Ragnhild C. Schreiner†

Abstract

This paper examines the effect of being granted temporary disability insurance (TDI), as opposed to a non-health related benefit, on later labor market outcomes of youths who are seeking temporary income support from the state. In Norway, there has been a development over time towards a more lenient screening to TDI, and this development has been more pronounced in some municipalities than in others. Using local screening leniency as an instrument for TDI receipt, I find that being granted TDI benefits significantly reduces later labor market attachment of youths whose benefit receipt would differ according to their municipality of residence, and the year of entry to the benefit system.

Keywords: Social insurance, disability screening, youth unemployment, program evaluation

JEL Classification: C21, C26, I18, H55

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1 Introduction

In most industrialized countries, different benefit programs are designed to insure against different types of income shocks. Unemployment insurance (UI) is meant to insure against loss of income due to job loss, and disability insurance (DI) is meant to insure against loss of income due to a reduction in work capacity. However, evaluations of work capacity are inevitably subjective, and are therefore likely to be closely related both to the particular job in question and to perceptions of the general labor market conditions. Accordingly, the distinction between different income support programs is not always clear-cut, and this might be reflected in the negative correlation between DI and UI benefits observed in cross-country comparisons of OECD countries (Røed, 2012). Benefit substitution has implications for state budgets, if being granted DI as opposed to UI make people more likely to exit the labor market. If this is the case also for youths, benefit substitution (towards more use of health-related benefits) is particularly alarming, as they have their entire working-lives ahead of them. This raises the question of whether granting a health-related benefit to youths, who are in the gray area between having a health and an unemployment problem, is a good way of bolstering them to succeed in the labor market in the longer term. This paper evaluates the effects of being granted a health-related benefit as opposed to another type of benefit, in the context of the Norwegian welfare system, on later labor market outcomes of youths who are seeking temporary income support from the state.

Several studies have looked at the causal effect of DI receipt on labor supply by assuming that in the case that the allowed DI recipients had been rejected, they would have supplied the same amount of labor as the applicants who were actually rejected (see e.g. Bound (1989); Chen and Van der Klaauw (2008); French and Song (2014); Maestas et al. (2013); von Wachter et al. (2011)). In the settings studied in these papers, all based on data from the US, the alternative to DI is usually no benefit receipt, or an appeal process ending in DI allowance for a significant number of the initially rejected applicants.[1] In many welfare states however, this is not the relevant comparison: the alternative to DI is instead a non-health related type of

[1] Maestas et al. (2013) find that around half of the initially rejected applicants are eventually granted DI, French and Song (2014) find that 40% of initially denied applicants are allowed benefits within three year, and Chen and Van der Klaauw (2008) estimate that 24% of the rejected applicants in their sample eventually become beneficiaries.

income support from the state. This paper differs from the existing literature in this aspect; I analyze the effects on later labor market attachment of being granted a health-related benefit, with the alternative being a non-health related type of income support. Moreover, while the literature is mostly concentrated on older workers, I look at youths, who have higher potential lifetime employment.

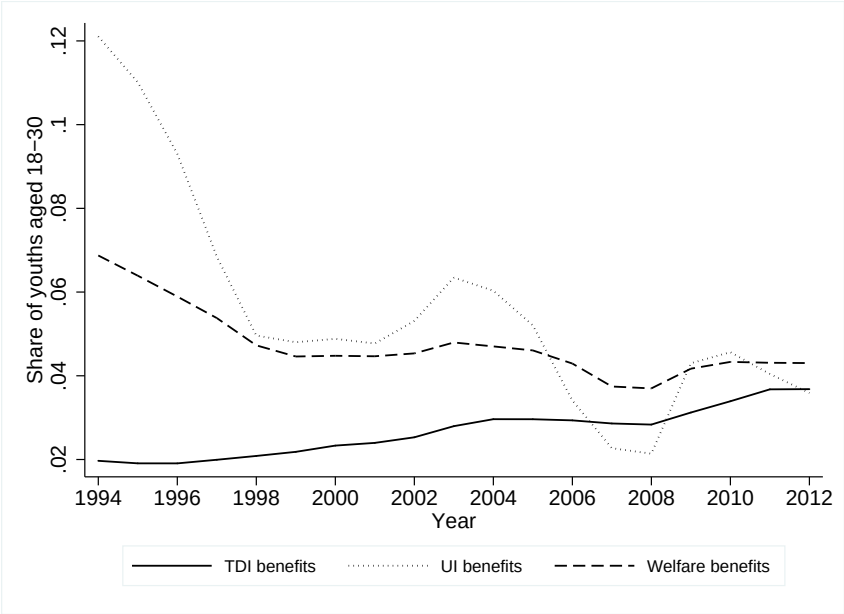
Norway has three main benefit programs for youths seeking income support from the state: temporary DI (TDI), UI and means-tested welfare benefits.[2]. A challenge to identifying the causal effect of health-related benefit receipt is that non-employed youths differ on dimensions such as health, labor market experience and motivation for work, and these differences might have a bearing both on the probability of being selected to the different benefit schemes, and on future labor market outcomes. To get around this problem, I exploit that Norway has experienced a sharp increase in the use of health-related benefits among youths. From 1994 to 2012, the share of youths aged 18-30 on TDI doubled to 4%. Over the same time period, the inflow to the alternative temporary benefit programs decreased (see Figure 1). The allocation to the different benefit programs is performed by Social Security Administration (SSA) case workers at local offices, and it turns out that over time, the screening to the TDI program has become more lenient, allowing more youths a health-related benefit. Moreover, the development towards a more lenient TDI screening has been more pronounced in some municipalities than in others. I argue that this creates some randomness (from the youths' point of view) in the likelihood of being granted TDI as opposed to another type of temporary income support.

Since screening leniency is unobserved, I create a proxy by measuring the share of older entrants to the benefit system who are granted TDI in a given municipality and year, adjusting for their pre-entry labor market status and UI benefits eligibility. I use this measure as an instrumental variable for the probability that a youth is granted TDI, and estimate local average treatment effects for the marginal population of youths whose benefit receipt would potentially differ according to which SSA office they reside to, and the year of entry to the benefit system. Using year and municipality fixed effects, I essentially link changes in labor market outcomes of youths to changes in TDI screening leniency within municipality over time. This approach controls for changes in the demand for TDI benefits that are similar across municipalities, and allows me

[2]Very few individuals are granted DI without first receiving TDI for some years.

to identify the causal effect of health-related benefits receipt. By controlling for pre-entry labor market status and UI benefits eligibility, I deal with the correlation between the proxy for local TDI screening leniency - the share of older entrants to the benefit system who are granted TDI - and the local unemployment rate. The results from my analyzes show considerable negative effects on subsequent wage income, and a corresponding increase in benefit dependency, from being granted a health-related benefit; The population of recipients whose type of benefit would differ according to the SSA office they reside to and the year of entry to the benefit system, are 30 percentage points more likely to be non-employed, not in education nor training after three years compared to others who instead were assigned to welfare or UI. The results are robust to a number of checks for remaining correlation between the proxy variable for TDI screening leniency and other time-varying variables affecting labor market outcomes of youths. First, I include a full set of interacted region (commuting zone) and year fixed effects, and the economical and statistical significance of the results are robust to this inclusion. Next, I run two sets of placebo regressions, one using past income as outcome variable, and another estimating on a matched sample of youths who are similar to the benefit recipients, but who are not in contact with the local SSA office. The results from the placebo regressions indicate that if anything, I under-estimate the effects of interest. Finally, I address concerns of local peer effects in benefit uptake by reducing the sample to youths with no family members on benefits, and by excluding workers who are close in age to the sample of youth when creating the proxy for screening leniency.

Figure 1: Temporary Income Support Among Youths in Norway, 1994-2012



Note: Share of youths aged 18-30 on TDI, UI and welfare benefits over the last two decades in Norway. Benefit receipt is defined as being on benefits at least one month some time during a year. If a person receives more than one type of benefit in the same year, the youth is registered with a benefit in the following hierarchical order: TDI, UI and means-tested welfare. The three temporary income support programs are described in more detail in Section 2.1.

Quantifying the effects of a "medicalization" of unemployed youths is important for policy makers in a number of high-income countries, such as Norway and the UK, who have experienced an increase in the DI receipt among youths (Banks et al., 2015; Kaltenbrunner Bernitz et al., 2013). The findings in this paper may also be relevant for countries looking to implement efficient (or avoid harmful) policies to induce non-employed youths to return to the labor market. Labeling youths with a medical diagnosis may alter their perceptions about their chances of succeeding in the labor market. Some youths may also perceive the health-related benefit allowance as society's acceptance of their non-participation. This is consistent with the "sick-role theory" of Parsons (1951), stating that people who are designated sick are exempted from certain social responsibilities such as financial independence. My findings give support to these theories. However, there are also other mechanisms through which the type of benefit granted can affect future labor market participation. One mechanism could be differences in treatments and/or activation requirements across benefit programs. Recipients of TDI benefits often get medical follow-up, and sometimes participate in vocational rehabilitation programs, while recipients of UI benefits sometimes participate in training programs. For young welfare recipients in Norway,

the activation requirements have varied considerably across municipalities, but are in general limited (Hernæs et al., 2016). Another mechanism could be lock-in effects in the TDI program. The maximum duration of the UI benefits program is two years, while a considerable number of youths participate in the TDI program for more than four years (Sørbø and Ytterborg, 2015). Short-term lock-in effects might have negative long-term consequences through a reduction in accumulated human capital. Previous studies have found evidence of "scarring" effects on later income and employment among youths who enter the labor market in times of poor business cycle conditions (Gregg, 2001). This is often explained by a reduction in early work experience (Greenwald, 1986; Lockwood, 1991; Raaum and Røed, 2006; Rosholm, 1997), or by psychological discouragement effects (Clark et al., 2001). A third potential mechanism is the role and the expectations that follow when a youth is labeled as disabled. Being granted a health-related benefit involves an increased attention on health-shortcomings as opposed to employment opportunities. Moreover, staying sick is a prerequisite for continued benefit receipt, and attempts of work-resumption involves a risk of losing income support, through signaling that the youth is no longer sick enough to be eligible. Unfortunately, data which could be used to disentangle the various mechanisms are not easily available.

2 Institutional Setting and Data

2.1 Institutional Setting

The main temporary income support program in Norway is the UI program. Eligibility depends on the level of income previous to application, with a replacement rate of around sixty percent of net income the previous year (or the average over the previous three years). The maximum duration is two years. The second largest temporary income support program among youths is means-tested welfare, where the size of the payments depends on living expenses (which varies across municipalities). The third alternative for temporary income support is the TDI program. Youths seeking support from the state have a right to an evaluation of their work capacity to be considered for TDI benefits. Eligibility requires a reduction in work capacity of at least 50%, caused by a health-related problem. The Social Security Law (Folketrygdloven) defines work capacity as a person's ability to meet the demands from a job, given their health, education,

competence, experience and family situation. The law also states that social, economic or other life style problems do not constitute medical diagnoses, but can cause medical conditions such as anxiety or depression.[3] Most TDI recipients enter from sickness benefits. These benefits have a 100% wage replacement rate, however, eligibility is conditional on employment, and the maximum duration is one year. Individuals who have not recovered after exhausting the sickness pay, usually continue on to receiving TDI benefits. TDI benefits corresponds to around two thirds of previous income. Non-employed individuals can be eligible for a minimum TDI benefit, as long as they fulfill the requirement of having a reduced work capacity. Since sick leave benefits are more generous than TDI benefits (except for among people with very low income), it is not financially beneficial to apply for TDI benefits without first exhausting the one year maximum duration of sick leave benefits. However, employment is a pre-requisite for sick leave, and youths who do not enter from sick leave are therefore likely to be unemployed, or enter from a low-paid job, and hence have a weaker labor market attachment than those who enter from sick leave. In contrast to the UI program, the TDI program does not (in practice) have a maximum duration, and many youths receive TDI for more than three years (Sørbø and Ytterborg, 2015). The three temporary income support programs are administrated at the local SSA office. Norway has around 450 SSA offices, in general one in each municipality (more in the big cities).[4]

2.2 Data

The starting point for the empirical analysis is annual records of all new spells of TDI, UI and welfare benefits for youths aged 18-30, that were started between 1994 and 2010.[5] To capture only new entrants to the benefit system, sample inclusion is conditional on no benefit receipt (except sick leave) the previous six months. This implies that the youths can have

[3] Folketrygdloven §11.

[4] Until 2007, the three benefit programs for temporary income support were administered at separate offices. The UI and TDI offices were run by the central state administration, and the welfare offices were run by the municipalities. Since the choice of appropriate income support program is not always obvious, clients were often sent back and forth between the different offices. To increase the efficiency of the system, a reform was implemented that merged the administration of all the benefit programs into one office. To investigate whether the reform interferes with the identification strategy, I re-estimate the model presented in Section 4, using data from the pre-reform time period only. I also re-estimate the model when excluding the municipalities with multiple SSA offices. The overall results are robust to these alternative specifications (see Column VIII and IX of Table 3).

[5] This time period is chosen since income records are available from 1993-2014 and benefits records from 1992-2013 (2015 for DI benefits).

up to two spells within a year, as long as they are below the age of thirty at the onset of the spells. Table 1 reports descriptive statistics on all TDI spells (Column I), UI spells (Column II) and welfare spells (Column III).[6] I characterize the entrants to the benefit system by their UI benefit eligibility and by four mutually exclusive pre-entry labor market statuses: "from sick leave", "not on sick leave, but some prior income", "from education" and "from a weak or unknown labor market attachment (NEET)" (see Appendix A.1 for exact definitions). UI benefits eligibility is defined according to an income criterion from which there are some exemptions, for instance, youths who have newly served in the military. This explains why there are some UI benefits recipients who are listed as not eligible in Column II of Table 1.[7] For 42% of the TDI spells, the youth was on sick leave for at least six of the twelve months prior to entry (Column I). As expected, the UI benefits recipients have the highest mean income before entry to the benefit system. The welfare recipients have the lowest average pre-entry income level, which may be be related to a lower average age (Column III). When it comes to labor market outcomes three years after entry, the TDI recipients have the least favorable outcomes, with the lowest average income and the highest average benefit transfers. Throughout the paper, I consider the following labor market outcomes:

(a) Annual wage income measured in 1,000 NOK $\approx$ 123 USD. All monetary amounts in this paper are deflated to 2014-value, and the conversion to US dollars is based on the average exchange rate for 2015 (1 USD = 8,1 NOK).

(b) Benefit transfers measured in 1,000 NOK.

(c) NEET: Not in employment, education or training.

(d) On (non-temporary) DI benefits.

[6] TDI benefits consist of "Attføringspenger", "Rehabiliteringspenger" and "Tidsbegrenset uføretrygd" and "Arbeidsavklaringspenger". The three former were replaced by the latter in 2010.

[7] Note also that 31% of the welfare recipients are eligible for unemployment benefits. The most likely explanation for why they still do not receive the more generous UI benefit is that they do not fulfill some of the non-income related eligibility requirements, such as being actively looking for a job, or that they are working part time (while receiving some additional income support through welfare benefits).

Table 1: Descriptive Statistics.

	I TDI		II UI		III Welfare	
Individual characteristics						
Age	25.3		24.7		23.0	
Share women (%)	53.0		40.2		52.3	
Immigrant background (%)	17.1		16.7		28.8	
Number of children	1.1		1.3		1.2	
Level of education (%)						
No education/unknown	1.7		1.9		5.3	
Elementary school	53.9		41.8		71.8	
High school	36.3		43.1		18.2	
University/college or higher	8.1		13.3		4.7	
Pre-entry labor market status, eligibility and income						
Sick leave	41.8		0.5		1.1	
Not sick leave, some income	27.2		65.1		16.5	
Education	6.5		3.9		15.3	
Weak or unknown labor market attachment (NEET)	24.6		30.6		67.0	
Eligible UI benefits (%)	76.2		86.0		31.1	
Wage income in 1,000 NOK (123 USD) year prior to entry	256.0	(185.3)	257.6	(164.7)	82.3	(121.5)
Labour market outcomes three years after entry						
Wage income (in 1,000 NOK)	159.5	(198.8)	318.1	(205.5)	163.4	(177.7)
Benefit transfers	142.5	(123.3)	42.4	(76.8)	102.5	(117.6)
NEET (%)	22.0		3.9		18.8	
On DI benefits (%)	6.9		0.1		3.5	
Number of observations	105,998		663,539		374,688	

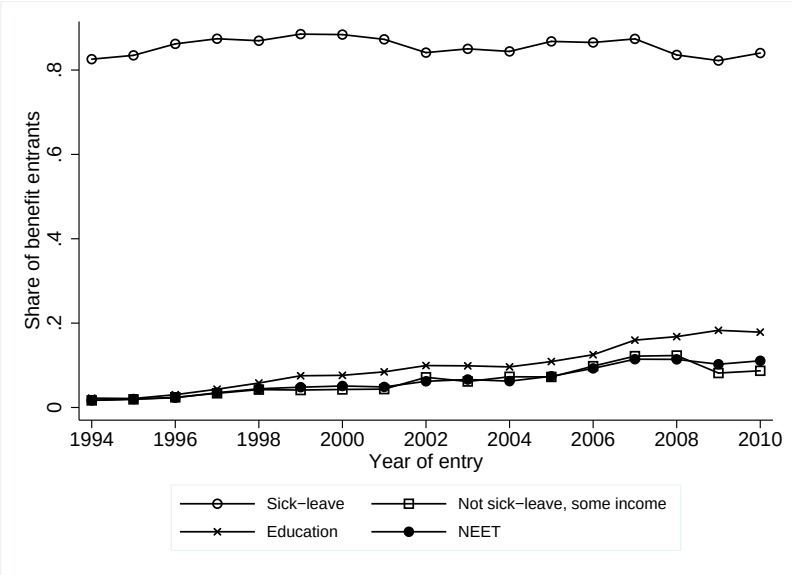
Note: Baseline sample consisting of all TDI, UI and welfare spells of youths aged 18-30 that were started between 1994 and 2010. All time-varying individual characteristics are measured in the year prior to entry. The pre-entry labor market statuses are mutually exclusive (see Appendix A.1 for definitions.) The outcomes are measured in the third year after the calendar year of entry to the benefit system and are described in Section 2.2. Standard deviations in parenthesis.

3 Graphical Evidence

Figure 2 plots the annual shares of the young entrants to the benefit system in the baseline sample who are granted TDI benefits, by pre-entry status. Among the biggest group, namely those entering from sick leave, the share has been fairly constant at around 85% over the last two decades. Among NEET youths, and those entering from education, the shares granted TDI benefits increased by around 550% and 700% from 1994 to 2010. This (arguably) goes against

the hypothesis that the increase in the share of youths on TDI benefits over time is driven by changes in demand, since the change cannot be traced among the biggest group of entrants (in terms of pre-entry status), namely those entering from sick leave.

Figure 2: TDI Receipt by Pre-Entry Status

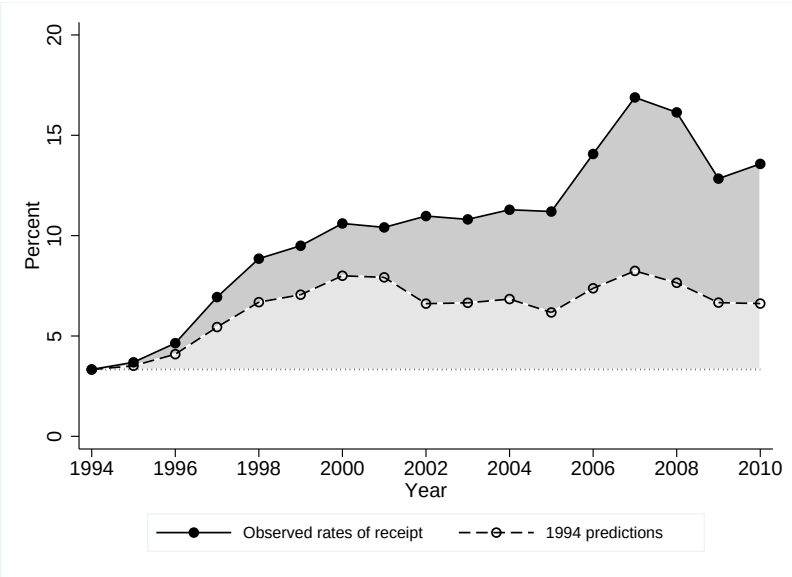


Note: Baseline sample consisting of all TDI, UI and welfare spells of youths aged 18-30 that were started between 1994 and 2010. Pre-entry statuses are defined in Appendix A.1.

Can the increase in the use of TDI benefits be explained by a change over time in the composition of benefit recipients, or by changes in the business cycle that affects young benefit recipients differently depending on their pre-entry status? Figure 3 compares the observed annual shares of benefit recipients who are granted TDI benefits with predicted shares that are meant to capture the shares on TDI benefits, had the TDI screening remained the same as in 1994. The predictions are from a Logit model, estimated on the spells (in the baseline sample) that where started in 1994. An indicator for whether a youth was granted TDI benefits is regressed on the following explanatory variables: pre-entry labor market status, UI benefits eligibility, previous income, gender, age, number of children, level of education, marital status, country background and local unemployment and employment rates (see Appendix A.2 for a description of the Logit model and included variables). The estimated coefficients are used to predict the shares of entrants in the years 1995-2010 who would be granted TDI benefits if they had been subject to the same TDI screening leniency as in 1994, taking into account individual observable characteristics and the local business cycle conditions. The predicted annual shares on TDI benefits are given by

the hollow circles, and the actual shares by the solid circles. The light gray shaded area shows how much of the increase over time in the use of TDI benefits that can be explained by a change in the composition of entrants to the benefit system in terms of observable characteristics and local business cycle conditions. The darker gray shaded area illustrates the part of the increase that is left to be explained either by a change in the composition of youths on unobservable characteristics, or a change in the TDI screening.

Figure 3: Actual and Predicted TDI Receipt Over Time.



Note: Baseline sample of TDI, UI and welfare spells of youths aged 18-30 that were started between 1994 and 2010. The solid circles show the observed annual shares of benefit recipients who are granted TDI benefits. The hollow circles show the annual predicted shares from a Logit model estimated on the spells from the baseline sample that were started in 1994. An indicator for whether a youth was granted TDI benefits is regressed on the following explanatory variables: pre-entry labor market status, UI benefits eligibility, previous income, gender, age, number of children, level of education, marital status, country background and local unemployment and employment rates (see Appendix A.2 for a description of the Logit model and included variables). The estimated coefficients are used to predict the share of entrants in the years 1995-2010 who would be granted TDI benefits if they had been subject to the same TDI screening leniency as in 1994, taking into account individual observable characteristics and the local business cycle conditions.

4 Empirical Strategy

To address concerns of endogeneity in a naïve regression of labor market outcomes on the type of benefit granted, the ideal approach would be to randomize entrants to the benefit system into the different benefit programs. In practice, this is not feasible (nor advisable). Therefore I have looked for variation in the type of benefit granted that breaks the relationship between

benefit receipt and unobserved characteristics that can also be determinants of future labor market outcomes. To this end, I exploit variation in the development of local TDI screening leniency among SSA case workers in different municipalities. This approach is similar to that of Markussen and Røed (2014) who study the effects of a variety of vocational rehabilitation programs in Norway by exploiting local variation in the use of the different programs as a source of randomness in program participation. While they use a time-constant instrument, I exploit differential developments in TDI screening leniency across municipalities over time, allowing me to control for time-constant differences between municipalities, as well as national trends in labor market opportunities of youths.

4.1 Local TDI Screening Leniency

Inflow to disability programs depends not only on formal eligibility criteria, but also on the stringency of the screening process (Johansson et al., 2014). In a study of inflow to the disability insurance (DI) program in Sweden, Johansson et al. document large fluctuations also during periods with no formal changes to eligibility criteria. This has been the case for the TDI program in Norway over the last decades. The TDI screening is performed by case workers at the local SSA offices. They evaluate the applicant's work capacity, by relying on a medical report written by a physician. TDI benefits are more generous than welfare benefits, and they typically have a longer duration than unemployment insurance (UI) benefits. When there is room for discretion in the choice of benefit program, some case workers might be lenient in their TDI screening of applicants, sending too many into TDI out of short-term considerations, without knowledge on the long-term consequences.

There are several reasons why SSA offices might have developed a different TDI screening over time. First, most SSA offices are small: 33% has six or less employees, and 11% three or less employees (Vågeng-utvalget, 2015, p.90). Moreover, at the big offices, case workers often specialize in certain tasks, such as screening to TDI, and they have a high degree of autonomy in the screening process (SINTEF, 2015, p.70). Individual differences in leniency between case workers are therefore likely to be a major driver of the development of local screening cultures. Over the last decades, psychological and muscular and skeletal disorders have become more prevalent, suggesting that the room for individual assessments by SSA case workers has

increased over time. The methodological approach in this paper relates to a number of studies using variation in leniency across public servants. Similar to the current paper, they predict an individuals treatment status by looking at how the public servant treats other individuals. French and Song (2014) and Maestas et al. (2013) study labor supply responses to DI benefits receipt, and Dahl et al. (2014) studies family welfare cultures by exploiting variation in leniency of judges in allowing DI benefits. Doyle (2007, 2008) uses variation in the tendency across case workers to remove children from their homes to study effects of foster care placement, and Duggan (2005) uses differences in prescription leniency across physicians to evaluate effects of increased use of anti-psychotic drugs on other medical expenditures.

Second, TDI and UI benefits are covered directly by the central government budget, while welfare benefits are covered by municipal budgets. Municipalities face binding budgets, and local offices may therefore have an incentive to place youths on UI or TDI programs instead of the welfare program. Since many youths do not qualify for UI benefits, the relevant choice is often between welfare and TDI benefits. Most offices have a mix of state and municipal employees, and shifting youths between programs may be easier when the case workers handling cases related to the central government budget work closely with case workers handling cases related to municipal budgets. In fact, the degree of integration between state and municipal case workers varies across offices (SINTEF 2015, p.325; Furuberg and Myklebø 2013, p.29; Vågeng-utvalget 2015, p.88). Also, the necessity for municipalities to save on their budgets may change over time, due to e.g. the need for building a new school or a sport facility, and can therefore give rise to different screening practices over time.

4.2 Measuring Local TDI Screening Leniency

Since local TDI screening leniency is unobserved, I create a proxy variable. First, for each municipality and year, I measure the share of older entrants to the benefit system (aged 31-67) who is granted TDI benefits as opposed to UI or welfare benefits. To be considered an entrant to the benefit system, an individual cannot have received any type of benefits the previous six months. There is a mechanical correlation between the share of older entrants who is granted TDI benefits and the indicator for whether a youth is granted TDI benefits. This mechanical correlation arises because more people become unemployed in times of cyclical downturns, and

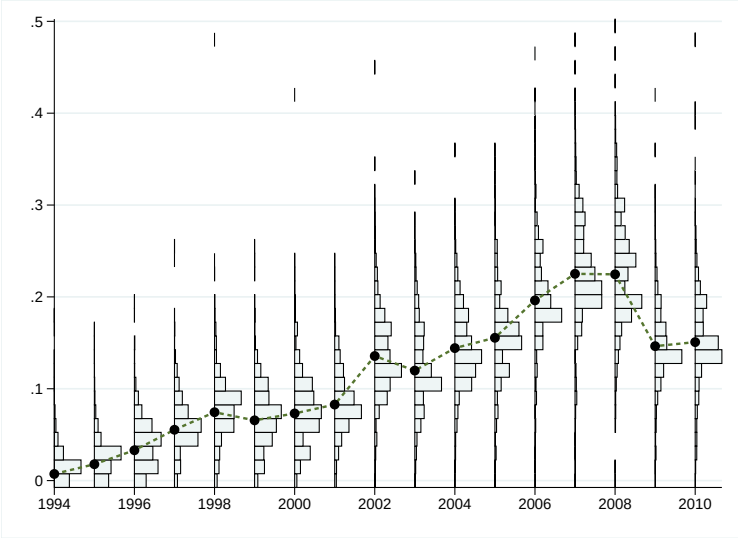
the increased number of people on UI implies a lower share of both older and younger entrants who are granted TDI benefits, irrespective of local screening leniency. To control for local business cycle conditions, I regress the the share of older, new entrants to the benefit system who is granted TDI benefits on a set of dummy variables indicating pre-entry status and UI benefits eligibility (as defined in Appendix A.1), and use the residual from this regression as the instrumental variable. To rule out any remaining correlation between the proxy variable for TDI screening leniency and other time-varying variables affecting labor market outcomes of youths, I perform a number of robustness checks (see Section 6.), and find that the overall results are seem not to be driven by local business cycles. Also, it should be noted that good business cycle conditions, and hence a high value of the proxy for local screening leniency, would tend to bias the results towards favorable labor market effects for youths from being granted TDI.

To construct the instrument, I use older workers instead of a leave-out-mean of young entrants to avoid a (hypothetical) bias from peer effects in benefit uptake.[8] Figure 4 shows the mean and distribution of the proxy for local TDI screening leniency by municipality and year. Similarly to the development for youths that was shown in Figure 1, there has been a substantial increase in the use of TDI benefits among older entrants over the last decades, conditional on background and UI benefits eligibility. In addition, there is considerable variation across the 423 municipalities in the sample.[9]

[8] My estimates would be biased if certain municipalities experience an increase in the demand for TDI benefits from both older and younger benefit entrants, and these entrants at the same time have poor labor market opportunities or a low motivation for work resumption. This possibility will be discussed in more detail in Section 6.1.

[9] The bi-modalities in some of the distributions are a result of some of the big municipalities, such as the capital, having a relatively low share of benefit recipients on TDI compared to UI and welfare benefits. As a robustness check, I re-estimate the model when excluding the big cities. The results are not sensitive to this exclusion (see Column VIII of Table 3.)

Figure 4: Variation in the Proxy for Local TDI Screening Leniency Over Time and Across Municipalities



Note: The means (black dots) and distributions (vertical histograms) of the proxy for local TDI screening leniency by municipality and year. The proxy is constructed by as the residual from a regression of the share of older (aged 31-67), new entrants to the benefit system who is granted TDI on indicators for pre-entry status and UI benefits eligibility (as defined in Appendix A.1). In the Figure, the proxy (residual) has been scaled by the mean share of older entrants who is granted TDI over all municipalities and years.

4.3 The IV Model

The IV model is described by the following two-equation system:

$$TDI_{sjt} = \gamma_0 + \gamma_1 SL_{jt} + \boldsymbol{x}_{\boldsymbol{st}}^{'}\gamma_2 + \boldsymbol{z}_{\boldsymbol{jt}}^{'}\gamma_3 + \boldsymbol{\lambda_t} + \boldsymbol{\rho_j} + u_{sjt}, \tag{1}$$

$$y_{sjt} = \beta_0 + \beta_1 TDI_{sjt} + \boldsymbol{x}_{\boldsymbol{st}}^{'}\beta_2 + \boldsymbol{z}_{\boldsymbol{jt}}^{'}\beta_3 + \boldsymbol{\tau_t} + \boldsymbol{\delta_j} + v_{sjt} \tag{2}$$

where TDI_{sjt} is an indicator for whether the youth with spell s is granted TDI benefits when entering the benefit system, SL_{jt} is the measure of local TDI screening leniency and y_{sjt} is the outcome of interest. Further, let s denote spell, j municipality and t the time of entry. Both equations include a full set of municipality indicators ($\boldsymbol{\rho_j}$ and $\boldsymbol{\delta j}$) and year indicators ($\boldsymbol{\lambda_t}$ and $\boldsymbol{\tau_t}$). The vector $\boldsymbol{x}_{\boldsymbol{st}}^{'}$ contains individual observable control variables including age, gender, level of education, marital status, number of children, and country background. To control for time-changing local business cycle conditions, I also include in $\boldsymbol{x}_{\boldsymbol{st}}^{'}$ a set of controls for UI

benefits eligibility and pre-entry labor market status.[10] The vector, z'_{jt}, includes additional measures capturing local business cycle conditions, such as the share of both younger and older workers on benefits, and the local sick leave rate, local demographics, such as the mortality rate, and a number of alternative local treatment initiatives (see Appendix A.1 for details on all the included control variables). I estimate the parameter of interest, β_1 by 2SLS, where Equation (1) is the First Stage and Equation (2) is the Second Stage. I interpret β_1 as the effect of being granted TDI benefits as opposed to a non-health related benefit on subsequent income and benefit transfers. The parameter captures the effects of all differences between the TDI program and the other programs, including duration, potential treatments and incentives. The standard errors are always clustered at the municipality level, and are robust to heteroscedasticity. To assess the validity of the assumptions of the IV model, I report results from estimations of models when including a gradually richer set of time-varying individual and municipality characteristics. The motivation for including the different control variables is discussed in more detail in Section 6, after presenting the results. Overall, the results are robust to the inclusion of this rich set of controls, however the magnitudes of the estimates change somewhat. The baseline specification therefore includes the full set of time-varying municipality characteristics.

5 Results

Table 2 reports the First Stage and 2SLS estimates of Equations 1-2. All specifications include municipality and year fixed effects, as well as indicator variables for pre-entry labor market status and UI benefits eligibility. Column II includes a vector of individual characteristics such as age, gender, education, income previous year, country background, marital status and number of children. Column III includes controls for both the share of the population of youths, and the share of the population of older inhabitants in the municipality that receive TDI, UI or welfare benefits. Column IV includes a vector of municipal demographic controls, such as the share of the population within five different age groups, the mortality rate, the immigration

[10] I control for pre-entry labor market status and UI benefits eligibility instead of the local unemployment rate measured by the number of people on UI benefits, since the latter is a function of the local TDI screening leniency. When including this potentially endogenous control (or a measure of the local employment rate), conditional on UI benefits eligibility and pre-entry status, the magnitudes of the results increase somewhat. This is as expected since failing to fully control for local business cycle conditions would be expected to bias the results towards more favorable labor market effects for youths, and hence towards zero.

rate, the share of women, the average education level and population size. Column V includes a vector of variables capturing the intensity of use of various vocational rehabilitation and training programs at the SSA office.

Panel (A) reports the First Stage estimates from the regression of an indicator for whether a youth is granted TDI on the proxy for local TDI screening leniency. The statistically significant First Stage implies that the proxy for local TDI screening leniency (that clearly is an imperfect measure) does not capture only random variation between municipalities over time. Moreover, the First Stage is robust to the inclusion of a large number of controls; with a F-statistic of 145 in the baseline specification (Column V). Panel (B) reports results from the 2SLS estimations. The results are striking: Being granted TDI benefits as opposed to UI or welfare benefits decreases earnings three years after entry by around 200,000 NOK (approximately 24,600 USD), and increases benefit transfers by a slightly lower amount. Moreover, the probability of being NEET three years after entry increases by around 30 percentage points. The estimated effect on the probability of being on non-temporary DI benefits is positive, but not statistically significant. When interpreting the results, we should bear in mind that the estimates are local average treatment effects for the population of youths whose benefit receipt would differ according to what local SSA office they reside to and the year of entry to the benefit system. For this type of youths, being granted TDI benefits implies a very limited labor market participation three years after entry. This might reflect lock-in effects of the program since many youths receive TDI benefits for more than three years (Sørbø and Ytterborg, 2015). To investigate this, I extend the analysis to looking at outcomes measured one to seven years after entry.

Table 2: Instrumental Variable Estimates

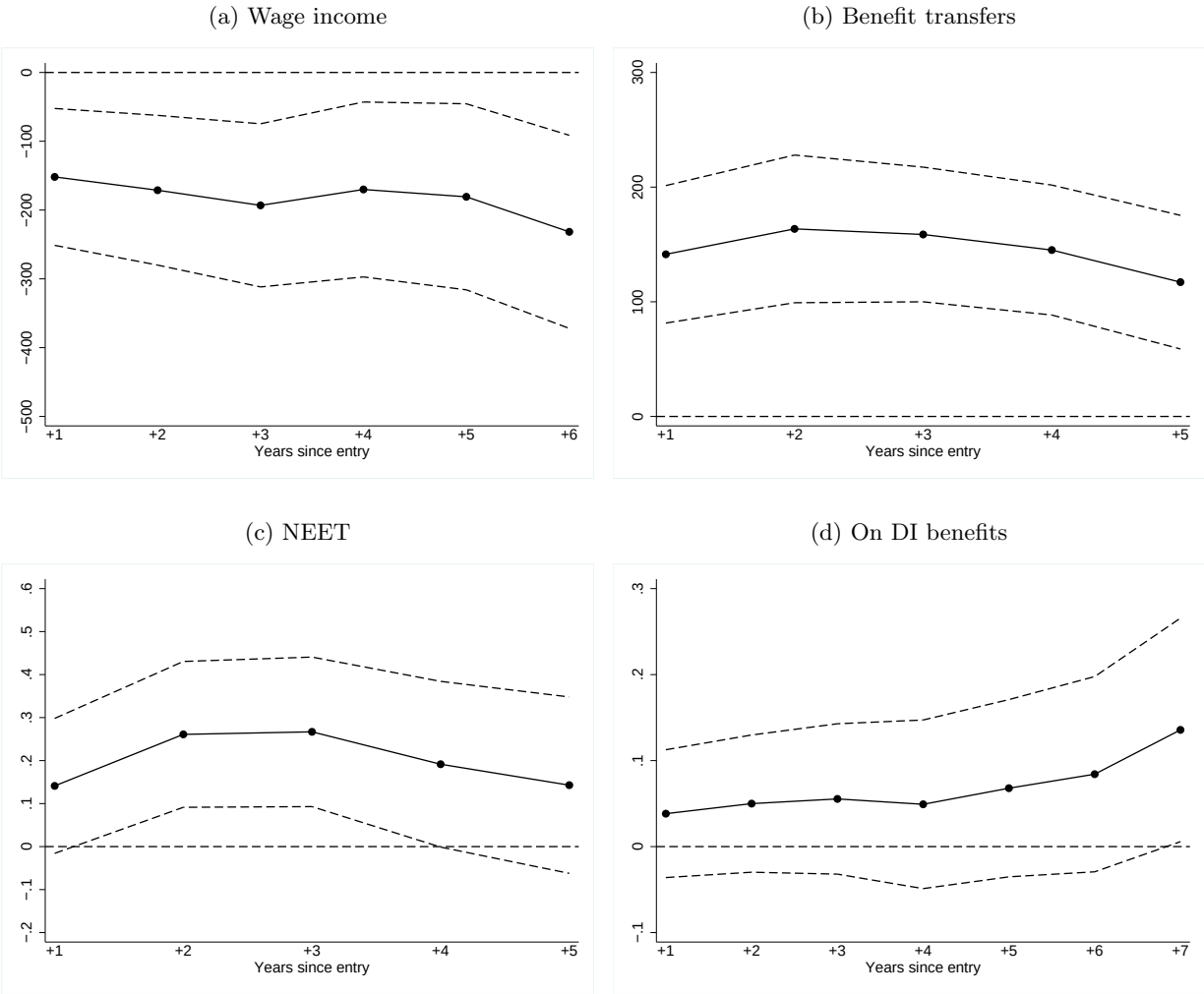
	I	II	III	IV	V	Dep. var. mean	N
Panel A: First Stage							
Proba. temp. dis. benefits	0.166*** [0.011]	0.160*** [0.012]	0.139*** [0.012]	0.138*** [0.012]	0.140*** [0.011]	0.093	1,144,255
F-value (excluded instrument)	225	190	119	138	145		
Panel B: 2SLS							
Wage income	-376.9*** [52.33]	-319.3*** [48.59]	-237.9*** [60.80]	-210.2*** [55.02]	-203.2*** [55.38]	252.7	1,144,255
Benefit transfers	264.2*** [27.81]	210.3*** [26.19]	184.36*** [30.19]	157.40*** [27.31]	157.52*** [27.29]	71.35	1,144,255
NEET	0.377*** [0.650]	0.337*** [0.066]	0.313*** [0.082]	0.302*** [0.080]	0.302*** [0.082]	0.104	1,144,255
On DI benefits	0.142*** [0.040]	0.125*** [0.039]	0.048 [0.043]	0.048 [0.043]	0.052 [0.041]	0.019	1,144,255
Control variables							
Pre-entry status	√	√	√	√	√		
Individual characteristics		√	√	√	√		
Municipality benefit receipt			√	√	√		
Municipality demographics				√	√		
Municipality treatments					√		

* 0.1, ** 0.05, *** 0.01. [Standard errors clustered at the municipality level in brackets]

Note: Main sample of TDI, UI and welfare spells of youths aged 18-30 that were started between 1994 and 2010. Estimates reported for Equations 1-2. All regressions include municipality and year fixed effects. The control variables are described in detail in Appendix A.1. The outcome variables are defined in Section 2.2 and are measured three years after entry to the benefit system.

To estimate longer-term effects, I reduce the sample to spells that were started no later than 2008, that constitute a balanced five-years panel from the date of entry. Benefit transfers and education records are available until 2013, while income records are available until 2014, and DI records until 2015. Hence, for the income and DI benefits receipt outcomes, I can estimate effects up to six and seven years after entry for this sample. All regressions include year and municipality fixed effects, and the full set of municipality level control variables (corresponding to Column V of Table 2). The dynamic pattern of the 2SLS results are presented in Figure 5. The results show long-lasting reductions in wage income, and increased benefit dependency. Panel (d) shows the effects of TDI benefits receipt on the probability of being allowed (non-temporary) DI benefits. There seems to be no effect until seven years after entry to the benefit system, after which the effect is positive and significant. This pattern of estimates corresponds well to characteristics of the system. It usually takes more than five years from an individual enters the benefit system, until DI benefits are granted. In 2002, 67% of the new entrants to the DI program that were below age 40 had been on TDI benefits for ten years prior to being granted DI benefits (Fevang and Røed, 2006). The positive estimate after seven years can reflect either that youths get (more) sick by being on the TDI program, or that screening to non-temporary DI benefits is not only based on objective health assessments, as is the case for the screening to TDI. It should be noted that the decision to grant DI benefits is not made at the local SSA office, but at a more centralized level.

Figure 5: Long-Term Effects



Note: Sample of 1,023,968 spells of youths who enter the benefit system in 2008 or earlier that constitute a balanced panel over at least five years. Estimates and 95% confidence intervals are reported for Equations 1-2. All regressions include municipality and year fixed effects, and the full set of municipality level control variables (corresponding to Column V of Table 2). The control variables are described in detail in Appendix A.1. The vertical axes of panels (a) and (b) are measured in 1,000 NOK, and the vertical axes of panels (c) and (d) are probabilities (1/100 percentage points). Estimated First Stage coefficients (γ_1 in Equation 1) with standard errors in brackets, and F-statistic in parenthesis: 0.135*** [0.011] (143).

To assess whether different youths are affected differently by being granted TDI benefits, I estimate the model separately after dividing the sample along two dimensions. First, I divide by gender since benefit take-up and employment rates tend to differ between men and women. Second, I divide by age since younger and older benefit recipients may have different past labor market experience. The results are presented in Figure 7 in Appendix A.3. Overall, there are no striking differences between women and men, nor young and old. The estimated effects on income are slightly more negative for men than for women, and for older compared to younger

recipients. The effects on the probability of being NEET are stronger for women initially, while for men, they are zero the first year after entry, before they sharply increase up until three years after entry (which is the time when UI benefits are exhausted). Five years after entry, the magnitudes of the estimates are quite similar across genders. The estimated effects on both subsequent benefit transfers and on the probability of being on DI benefits are similar across groups.

6 Threats to a Causal Interpretation

6.1 Omitted Time-Varying Municipality-Level Variables

Even after controlling for pre-entry labor market status and UI benefits eligibility, temporal changes in screening leniency within a municipality may be correlated with omitted time-varying variables that affect labor market outcomes of youths. To assess the importance of such omitted variables, I gradually include a richer set of control variables, and assess how the estimates change (Table 2). Even though the magnitudes decrease somewhat, the signs and the economic significance of the results are robust to the inclusion of a large number of time-varying municipality-level controls. Further, to assess whether local screening leniency is correlated with other types of treatments initiated or prioritized at the local SSA offices, I include a set of variables capturing the intensity of use of various vocational rehabilitation and training programs. Although these controls only capture observable differences in treatment strategies, it is reassuring to see that their inclusion does not alter the results to any noticeable extent. Next, to check for any remaining correlations between the proxy for screening leniency and local business cycle conditions, I include a set of interacted local labor market region and year fixed effects.[11] These interactions absorb all changes over time in, for instance, labor market conditions or health within the regional labor market. This extensive set of conditioning variables absorbs a considerable amount of the variation in the instrument, and consequently, the magnitude and the statistical precision of the estimated coefficients on income-related outcomes decrease somewhat. Nevertheless, the results give the same overall impression (see Column II of Table 3). I also include a set of municipality-specific linear trends. The inclusion of these

[11] Norway is divided into 40 local labor markets, mainly based on commuting zones. See Bhuller (2009) for a description of the region division.

controls increases the magnitudes of the estimates, which might indicate that a model with insufficient controls for local labor market conditions would give results that are biased towards zero (Column III of Table 3). This is as expected, since a downturn of the economy would tend to result in a high share of benefit entrants on UI benefits (which implies a low value of the instrument), as well as poor subsequent labor market outcomes.

Table 3: Instrumental Variable Robustness Estimations

	I	II	III	IV	V	VI	VII	VIII	IX
	Baseline	Reg. labor market x Year FE	Lin. muni. trend	Plant closures	Alternative IV	Family	Migration	Excluding cities	Pre-reform
Panel A: First Stage									
Proba. temp. dis. benefits	0.140***	0.110***	0.111***	0.133***	0.080***	0.147***	0.143***	0.132***	0.114***
	[0.011]	[0.011]	[0.011]	[0.012]	[0.009]	[0.012]	[0.013]	[0.011]	[0.011]
F-value	145	94	103	124	87	100	113	145	111
Panel B: 2SLS									
Income	-203.2***	-129.3***	-205.4***	-192.0***	-161.8**	-172.2***	-205.8***	-179.7***	-187.5***
	[55.38]	[64.96]	[68.21]	[57.04]	[77.32]	[74.10]	[64.72]	[56.57]	[75.87]
Benefit transfers	157.5***	119.5***	161.2***	138.5***	176.5***	102.8***	150.0***	140.9***	138.6***
	[27.29]	[32.55]	[33.49]	[27.46]	[39.19]	[33.85]	[31.49]	[27.54]	[36.27]
NEET	0.302***	0.295***	0.307***	0.296***	0.320***	0.325***	0.289***	0.301***	0.180
	[0.082]	[0.102]	[0.098]	[0.087]	[0.108]	[0.105]	[0.089]	[0.083]	[0.102]
On DI benefits	0.052	0.111*	0.130***	0.078	0.089	0.049	-0.017	0.052	0.059
	[0.041]	[0.055]	[0.051]	[0.045]	[0.056]	[0.047]	[0.048]	[0.042]	[0.054]
N	1,127,282	1,127,282	1,127,282	899,959	1,127,282	693,546	743,479	884,266	968,888

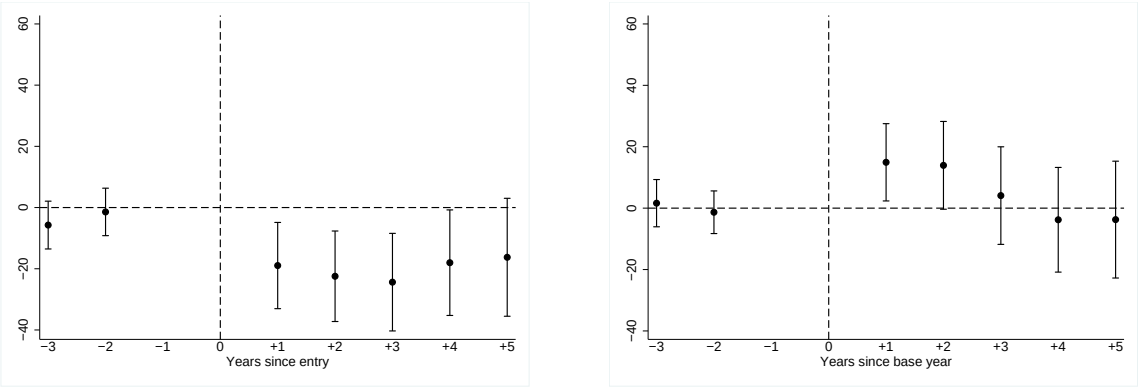
* 0.1, ** 0.05, *** 0.01. [Standard errors clustered at the municipality level in brackets]

Note: Baseline sample: TDI, UI and welfare spells of youths aged 18-30 that were started between 1994 and 2010. All regressions include municipality and year fixed effects, and the full set of municipality level control variables (corresponding to Column V of Table 2). Estimates reported for Equations 1-2. Column I: Baseline results. Column II: Including interacted regional labor market and year fixed effects. Column III: Including linear municipality trends. Column IV: Excluding spells started in a municipality where a major plant closure (a plant of more than 100 employees) took place the same year, or the year previous to entry. Column V: Using an alternative instrument calculated on a sample of older workers aged 40-65 (instead of 30-65). Column VI: Excluding spells of youths with at least one parent who receive health-related benefits some time during the two years previous to entry. Column VII: Excluding spells where the youth change municipality of residence during the two years previous to entry to. Column VIII: Excluding spells of youths living in the four biggest cities of Norway: Oslo, Bergen, Trondheim and Stavanger. Column IX: Excluding spells that were started after the reform that merged the administration of all the benefit programs into one office.

Finally, to assess the importance of remaining unobserved time-varying municipality confounding factors, I run a set of placebo regressions, where I check whether the proxy for local screening leniency is correlated with labor market opportunities of youths living in a given municipality, and who are not in contact with the local SSA office. More concretely, for each spell of young benefit entrants in a given year, I randomly draw a youth who lives in the same municipality, and has the same gender, and approximately the same age and educational attainment, but who has not received any type of benefits the previous three years. If the proxy for local screening leniency is correlated with the local business cycle in a way not accounted for by the included controls, this should show up in the labor market outcomes of this similar, but non-affiliated group of youths. I estimate the Reduced Form of the IV model by regressing the outcome y_{sjt} directly on the instrument, SC_{jt}, where y_{sjt} is wage income one to five years ahead in time. The regression equation also includes the same set of control variables as in the baseline model presented in Column V of Table 2. The results are presented in Panel (b) of Figure 6. For comparison, Panel (a) shows the corresponding Reduced Form estimates for the main sample of benefit recipients. The magnitude of the estimates for the sample of non-recipients is considerably smaller, and except for the estimated effect one year ahead in time, none are statistically significant. The positive and significant effect one year after the base year (of matching) indicates that there is some remaining variation in e.g. local business cycle conditions that is not captured by the included control variables. However, as noted before, these omitted variables seem to bias the results towards zero.

Figure 6: Placebo Regressions of Wage Income on the Local Screening Leniency

(a) Main sample (b) Placebo sample



Note: Panel (a): Sample of 866,852 spells of youths who enter the benefit system between 1996 and 2009 and constitute a balanced sample over nine years. Panel (b): Sample of 866,852 non-benefit recipients matched to the main sample on municipality, gender, age and education level, by nearest neighbor matching. Base year = 0 indicates the year of matching. Estimates (and 95% confidence intervals) are from Reduced Form estimations of the IV model. All regressions include municipality and year fixed effects, and the full set of municipality level control variables (corresponding to Column V of Table 2). The control variables are described in detail in Appendix A.1. The vertical axes are measured in 1,000 NOK (approximately 123 USD).

None of the included control variables, nor robustness checks presented so far can completely rule out that the results are driven by common changes in the demand for TDI benefits across younger and older people within a municipality. As far as I can see, the only way to meet this concern is to address the potential causes for why such local demand could should arise. One cause could be the closure of a major plant that employs both older and younger workers. To assess whether this can be driving the results, I re-run the estimations when excluding spells that are started in a municipality where a major plant was closed in the year of entry, or the year prior to entry to the benefit system. I define a plant as closed down if at least 50% of the employees enter unemployment in the same calendar year. The plant is considered major if it has more than 100 employees. Excluding spells that potentially are affected by plant closures does not alter the results to any noticeable extent (see Column IV of Table 3).[12] Another cause of local demand shocks could be norms or peer effects in benefit claims within a municipality and across generations. There is by now a substantial literature on peer effects in benefit use, however the empirical studies are often challenged by methodological difficulties (as pointed out in Manski (1993)), or focus on ethnic minorities (as for instance in Bertrand et al. (2000)). Studies from Norway include Markussen and Røed (2015) who find peer effects in benefit

[12]This conclusion holds also when using alternative definitions of "plant closure" and "major plant".

program participation. Their findings indicate however, that peer effects mainly occur within age groups. I therefore re-estimate the model when using a proxy for local screening leniency that is calculated on an even older group of entrants, namely those aged 40-65. Although the First Stage coefficient decreases somewhat, the results do not change much using this alternative specification of the instrument (see Column V of Table 3). Another study from Norway that looks at peer effects in program uptake is Dahl et al. (2014), who find that the participation in welfare programs by one generation induces participation of the next generation (while the spill-over effects to close neighbors are negligible). To assess the importance of intergenerational effects, I exclude spells where the youth has at least one parent who received some kind of health-related benefits during the two years prior to benefit uptake of the youth. The exclusion of these spells reduces the magnitude of the results somewhat, indicating that family welfare cultures may be present. Still, the results remain both economically and statistically significant (see Column VI of Table 3).

6.2 The Exclusion Restriction

A second potential concern is the exclusion restriction of the IV model. The proxy for local screening leniency should not affect subsequent labor market outcomes of youths beyond affecting the type of benefit granted. This assumption could be violated if, for instance, the resources of a benefit program is limited, such that a high share of older recipients of TDI benefits would imply less resources to help young recipients return to work. The inclusion of the local program participation controls in Column V of Table 2 should be a good test for this. The set of controls includes the shares of older recipients, both on TDI benefits and on UI benefits, that participate in supported education, regular education, out-placement in regular firms, sheltered employment or targeted training courses. The inclusion of these controls does not affect the results. While these regressions are not an ultimate test of the exclusion restriction, the results at least suggest that competition for resources within a program is of limited importance. Finally, the exclusion restriction is less of a concern with the outcome "on DI benefits", since the decision to grant DI benefits is not made at the local SSA office, and the role of the physician is more prominent than in the process of granting TDI benefits.

Another potential violation of the exclusion restriction arises because the treatment of interest,

being granted TDI benefits, is measured with error. I classify benefit type according to the kind of benefit granted in the first month of entry to the benefit system. However, recipients may be shifted from one type of benefit to another after some time. Of the youths in the sample who start out with UI benefits, less than 1% changes to TDI benefits within three months, and around 3% changes within a year. The corresponding numbers for welfare recipients is 5% and 14%. If individuals who were initially granted another type of benefit are more likely to be transferred to TDI benefits in municipalities with a lenient screening than in municipalities with a strict screening, this would imply a violation of the exclusion restriction, and the scaling of the Second Stage with the First Stage coefficient would be misleading. To investigate the relevance of this, I re-estimate the IV model when varying the time of measurement of type of benefit granted to three and twelve months after entry. The results are not sensitive to the point in time at which I measure the type of benefit granted.

Finally, it should be noted that the exclusion restriction is not necessary for a causal interpretation of the Reduced Form estimates. Ideally, these estimates would give us the effect of being subject to the TDI leniency in the municipality with the most lenient screening compared to the municipality with the strictest screening. However, the true screening leniency of a municipality is measured with error, which implies that the Reduced Form estimates gives a lower bound on this parameter.

6.3 The Composition of Benefit Recipients

A threat to a causal interpretation of the IV estimates are changes in the composition of benefit entrants that are caused by, or correlated with the changes in local screening leniency over time. For instance, a lenient screening may attract some youths into the benefit system, who would otherwise be non-recipients. If this marginal group of benefit recipients is more advantaged (in terms of labor market opportunities) than the group that receives benefits regardless of TDI screening leniency, it would imply that the group of young TDI benefit recipients is, on average, more advantaged (in terms of labor market opportunities) in lenient compared to strict municipalities. Such a selection mechanism into benefit take-up would imply that the results are biased towards zero. In an attempt to address this concern, I control for the share of youths in a municipality and year who receives benefits. This is clearly an imperfect control, since

the composition of benefit recipients may change even if the share of youths on benefits stay constant. Therefore, I run a robustness check where I regress the measure of local TDI screening leniency on past income. Since the main model includes a control for income the year prior to entry, I therefore use wage income measured two and three years prior to entry as outcome variables in the robustness check. Any correlation between the resources of the TDI benefit recipients and the proxy for local TDI screening leniency would likely be revealed from the results from these regressions. The results are presented in Panel (a) of Figure 6, and they show that the effects of local TDI screening leniency on past income are small in magnitude and not statistically significant.

Further, I investigate whether lagged (and current) values of the local screening leniency predict inflow to the sample of benefit recipients. More concretely, I construct a sample consisting of all youths living in a given municipality and year, for each of the years 1994-2010. Next, I regress an indicator variable for entering the benefit system, on the proxy for local screening leniency, as well as one and two-years lagged values. In addition, I include the same set of control variables as in the main model given by Equations 1-2 (except for the controls for the share of young benefit recipients in a municipality and year). Table 4 presents the estimated coefficients (and standard errors) on the screening leniency variables, as well as the other municipality-level control variables.[13] The lagged values of the instrument have no power in predicting inflow to the sample, and hence the results do not support to the hypothesis that a lenient screening attracts youths into the benefit system in the following years. The coefficient on the current level of screening leniency is statistically significant, but small in magnitude compared to e.g. the estimated coefficients for the local sick-leave rate, mortality rate and the share of immigrants. A one percentage point increase in the share of older benefit recipients who are granted TDI benefits (controlling for their pre-entry status and UI benefits eligibility), decreases the probability that a youth is on benefits by 0.019 percentage points. Moreover, the estimate is negative, which goes against the hypothesis that a lenient screening attracts more youths to the benefit system. An alternative explanation for the negative relation between the current value of screening leniency and inflow to the sample is that in an economic upturn, fewer people become unemployed, and this both reduces the inflow to the sample, and gives a higher value of the instrument (the denominator decreases due to fewer older workers on UI).

[13] The coefficients on the individual level controls are not presented out of space concerns.

In the main model (Equation 1-2), this should, at least to some extent, be picked up by the control for the share of youth on benefits. Also, as noted earlier, such a relation would tend to bias the estimates towards zero.

Table 4: Testing for whether (past) screening leniency affects inflow to the sample.

	Coeff.	S.E.
Instrument		
Same period	-0.019***	0.004
One lag	-0.005	0.004
Two lags	0.006	0.005
Municipality level controls		
Sick leave rate	-0.281***	0.054
Mortality rate	-0.306***	0.111
Share women	0.001***	0.000
Share immigrants	0.140***	0.037
Population size	0.000*	0.000
Age composition		
≤ 15		
$16-24$	-0.117	0.146
$25-34$	-0.250*	0.138
$35-44$	-0.272*	0.153
$45-54$	-0.268**	0.138
$55-66$	-0.207	0.132
≥ 67	-0.324***	0.133
Education		
Less than high school	0.032	0.082
High school	0.058	0.073
University	0.058	0.092
Dep. var. mean	0.9	
N	13,060,968	

* 0.1, ** 0.05, *** 0.01. Standard errors are clustered at the municipality level

Note: Population of all youths residing in a municipality a given year over the period 1994-2010. All regressions include municipality and year fixed effects. The control variables are described in detail in Appendix A.1.

Finally, I assess whether the results are biased due to migration of youths to municipalities with a lenient TDI screening, by excluding youths who change municipality of residence during the two years prior to entry to the benefit system. This alternative specification does not alter the results much. If anything, the estimated effects increase in magnitude, indicating that migration tend to bias the results towards zero (Column VII of Table 3).

6.4 Interpreting the IV Estimates

As seen in Table 1, benefit receipt is correlated with observable (and probably also unobservable) characteristics of the recipients. The impact of the TDI screening leniency on individual benefit receipt is therefore likely to be heterogeneous. Using an instrumental variables strategy implies that I am estimating local average treatment effects (Imbens and Angrist, 1994), and the IV estimates should be interpreted as marginal treatment effects for the group of youths whose type of benefit receipt would differ according to what municipality they reside in, and the year of entry to the benefit system. Hence, the results cannot be extrapolated to the population of youths at large. Nonetheless, I argue that the estimated effect is policy relevant, since this is the population of youths whose benefit receipt is most likely to be affected by a more stringent TDI screening.

Given heterogeneous treatment effects, a monotonicity assumption is also necessary for a causal interpretation of the IV estimates. The probability that a young person is granted TDI benefits cannot be decreasing in the proxy for local TDI screening leniency. This assumption could be violated if, for instance, SSA offices operate with a limited number of slots on each benefit program. Reassuringly, this is not the case, and in addition, the case workers do not have data at hand on the current number of welfare recipients (SINTEF, 2015, 98).

7 Conclusion

This paper examines the effect on later labor market attachment of youths of being granted temporary disability insurance (TDI) benefits, as opposed to UI or welfare benefits. Non-employed youths differ in terms of health, labor market experience and motivation for work, and such characteristics may have a bearing both on the probability of being selected to various benefit schemes and on future labor market outcomes. As an attempt to circumvent this selection problem, I use administrative register data on benefit receipt in Norway over the last two decades, and exploit variation in the TDI screening performed by Social Security case workers. Over time there has been a development towards a more lenient TDI screening practice, and this development has been more pronounced in some municipalities than in others.

I argue that this creates some randomness (from the youths' point of view) in the likelihood of being granted TDI benefits. Using local screening leniency as an instrument for TDI receipt, I find large negative effects of being granted TDI benefits on subsequent wage income, and increased benefit dependency for the population of youths whose benefit receipt would differ according to what municipality they live in, and the year of entry to the benefit system.

Since youths are not randomized into different benefit programs, the analysis is based on non-experimental data, and some doubt about the causal interpretation therefore inevitably remains. I argue however, that the most likely sources of bias have been addressed and discarded. Thus my findings show that over the last decades, the Norwegian benefit system has increasingly been "medicalizing" youths by practicing a more lenient TDI screening. This had led to a sharp reduction in later labor market participation of the affected youths, and an increased likelihood of ending up in the usually absorbing state of DI benefits receipt. This study does not assess overall welfare effects, by weighing program costs against potential benefits for the youths. I also leave for future research to identify the mechanisms behind the findings; whether the reduction in later labor market participation is due to differences in the types of treatments offered at the different programs, scarring effects from an increase in the number of years as benefit recipients, or psychological discouragement effects. Nevertheless, assessing the consequences of the institutional change of a more lenient TDI screening is important both since it might not be the best outcome for the youths, and since it certainly is costly for society at large. My findings indicate that the challenge of youths with a weak labor market attachment in industrialized countries should not be met by increasingly granting health-related benefits. In Norway, one quarter of youths who are granted TDI during the time period of study does not qualify for UI benefits. A remedy to avoid medicalization of youths could therefore be to lessen eligibility requirements to UI benefits for youths.

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A Appendix

A.1 Control Variables

Pre-entry status (mutually exclusive categories; included as dummy variables):

(1) On sick leave at least 6 of the 12 months previous to entry.

(2) Previous wage income exceeding the minimum subsistence level defined as two times the Base Amount in the Norwegian pension system (approximately 22,240 USD), and not (1).

(3) Under education at least six of the 12 months previous to entry, and not (1)-(2).

(4) Weak or unknown labor market attachment (NEET), i.e. not (1)-(3).

Eligible for UI benefits (dummy variable):
Eligibility is defined according to the criterion based on previous income contributions. To be eligible, an individual must have had wage earnings of at least 1.5 times the Base Amount in the Norwegian pension system (approximately 16,680 USD) the year previous to entry, or wage earnings of at least three Base Amounts the previous three calendar years.

Individual characteristics (included as dummy variables):
Gender (2 categores).
Age (13 categories).
Number of children (8 categories).
Level of education (8 categories).
Marital status (3 categories).
Country background (4 categories).
Wage income year before entry (included as a linear variable).

Municipality benefit receipt (included as linear and quadratic variables):
Share of youth population (18-30) on TDI, UI or welfare benefits.
Share of older population (31-67) on TDI, UI or welfare benefits.

Sickness absence rate, adjusted for age and sex composition in municipality (included as a linear variable).

Municipality demographics (included as linear variables):

Age: Share of the population in seven age groups: ≤ 15, $16-24$, $25-34$, $35-44$, $45-54$, $55-66$, ≥ 67.

Education: Share of the population in four education groups: Less than high school, high school, lower university levels, higher university levels.

Mortality rate, adjusted for age and gender composition in municipality.

Share of women.

Share of immigrants.

Population size.

Municipality treatment (linear variables): The share of older TDI recipients and of UI benefit recipients who participate in:

(1) Supported education.

(2) Regular education.

(3) Placement in a regular firm.

(4) Sheltered employment.

(5) Targeted training courses.

Local labor market region: Norway is divided into 40 local labor markets based on data on commuting zones (documented in Bhuller (2009)).

A.2 Logit Model

Graph 3 is based on predicted probabilities from the following Logit model.

Let y_{sjt} be the event that the youth with spell s is grated TDI benefits:

$$y_{sjt} = \begin{cases} 1 & \text{if the youths with spell s is granted TDI benefits} \\ 0 & \text{otherwise} \end{cases} \tag{3}$$

The probability of the youth with spell s being granted TDI benefits is given by:

$$P_{sjt} = P(y_{sjt} = 1|X_{sjt}) = F(X'_{sjt}\beta) = \frac{\exp(X'_{sjt}\beta)}{1 + \exp(X'_{sjt}\beta)}, \tag{4}$$

where F(.) if the cdf of the Logistic distribution. The following variables are included in the vector X_{sjt}: Individual characteristics (pre-entry status and UI benefit eligibility as defined in Appendix A.1), and measures of the local unemployment and the local employment rates. The model given by Equations (3)-(4) is estimated by maximum likelihood, where the likelihood function is given by Equation 5.

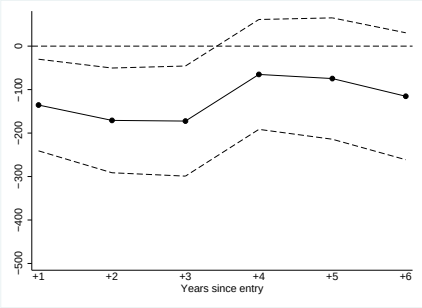
$$L = \prod_{s=1}^{S} P_{sjt}^{y_{sjt}}(1 - P_{sjt}^{y_{sjt}}) \tag{5}$$

A.3 Heterogeneous Effects

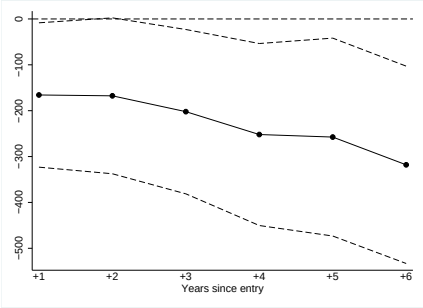
Figure 7: Heterogeneous effects by gender and age

(I) Wage income

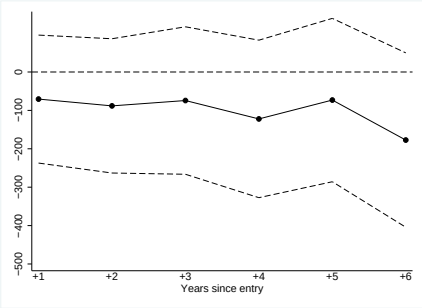
(a) Women



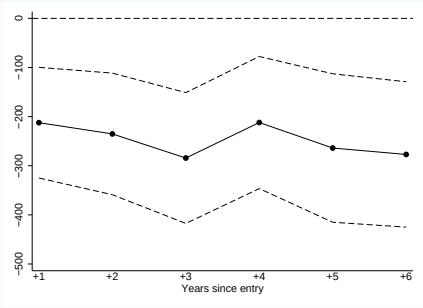
(b) Men



(c) Aged 18-24

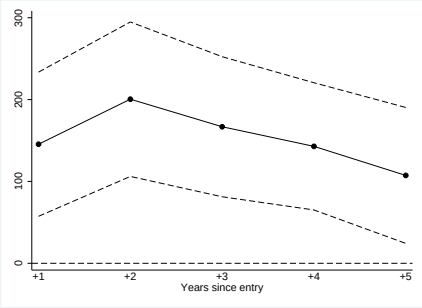


(d) Aged 25-30

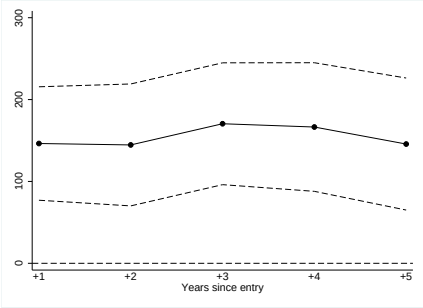


(II) Benefit transfers

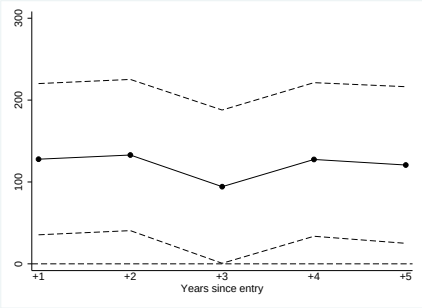
(a) Women



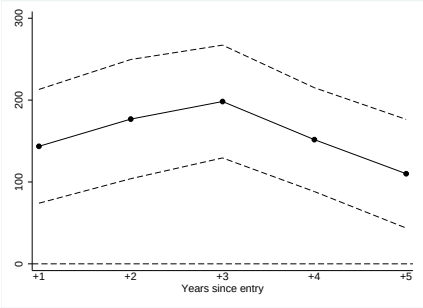
(b) Men



(c) Aged 18-24

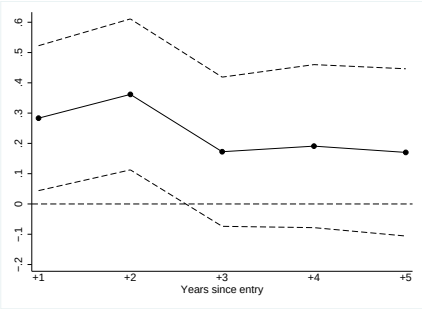


(d) Aged 25-30

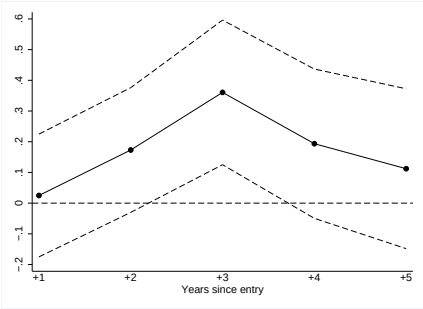


(III) NEET

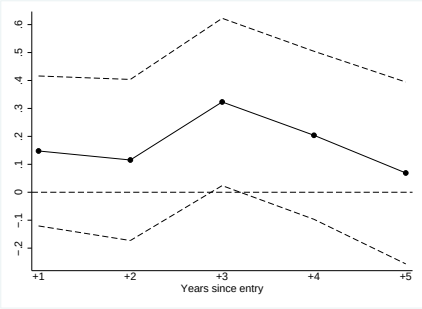
(a) Women



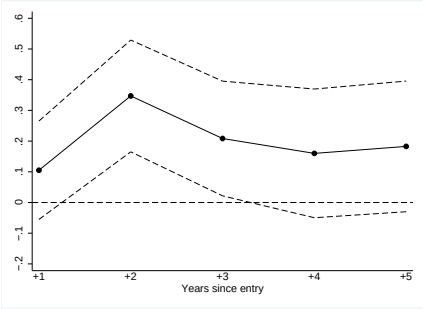
(b) Men



(c) Aged 18-24

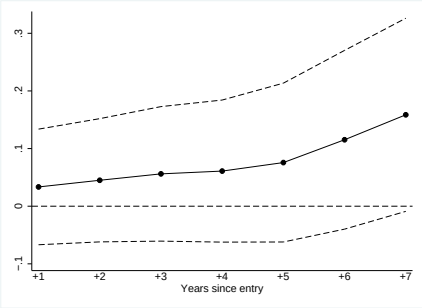


(d) Aged 25-30

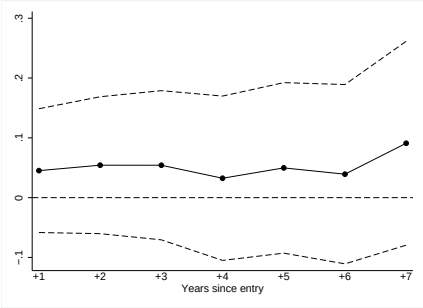


(IV) On DI benefits

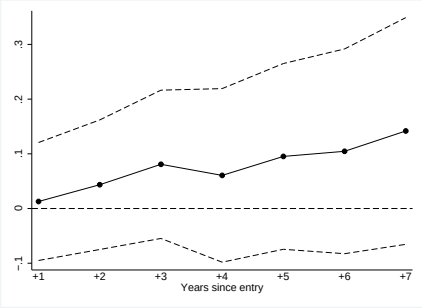
(a) Women



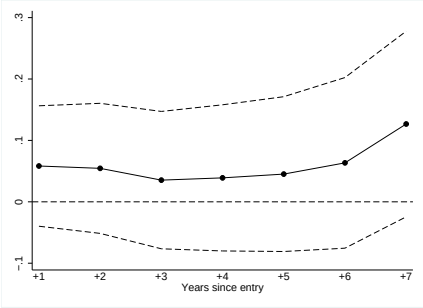
(b) Men



(c) Aged 18-24



(d) Aged 25-30



Note: The sample of 1,023,968 youths entering the benefit system in 2008 or earlier is divided into sub groups along two dimensions, according to the gender and age of the youths. The vertical axes of Panels (I) and (II) are measured in 1,000 NOK, and the vertical axes of Panels (III) and (IV) are probabilities (1/100 percentage points). Estimates (and 95% confidence intervals) reported for Equations 1-2. All regressions include municipality and year fixed effects, and the full set of municipality level control variables (corresponding to Column V of Table 2). The control variables are described in more detail in Appendix A.1. Estimated First Stage coefficients (γ_1 in Equation 1) with standard errors in brackets and the number of observations in parenthesis: Women: 0.140 [0.016] ($N = 469,310$), Men: 0.128 [0.013] ($N = 554,658$), Aged 18-24: 0.103 [0.013] ($N = 544,503$), Aged 25-30: 0.175 [0.018] ($N = 479,465$).