



# Regular employment for people with mental illness – An evaluation of the individual placement and support programme

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## 1. Introduction

What could make people with mental illness fit for regular employment, i.e., ordinary paid employment in contrast to sheltered or wage-subsidized employment or even unemployment? This question is of high policy-relevance given that mental illness is one of the leading causes of withdrawal from the labour market in OECD countries (OECD, 2015), as well as a major driver of health care costs (Gustavsson et al., 2011). In particular, many young people are not employed due to mental health problems (Greve and Nielsen, 2013), in fact, mental and behavioural disorders are among the leading causes for years lost to disability among youth in high-income countries (Gore et al., 2011). This fact causes great concern. At the same time, research concludes that work can improve health and prevent disability (Murphy and Athanasou, 1999; Rueda et al., 2012). This has motivated supported employment programmes, where participants are provided individualized support in order to obtain and maintain employment. A meta-analysis within the psychology literature has estimated that the effect of supported employment is twice that of traditional active labour market programmes (Suijkerbuijk et al., 2017), but assessment of long-term effects is generally lacking. In the current study, we analyse the long-term effect of one specific type of supported employment, namely Individual Placement and Support (IPS), offered to people with moderate to severe mental illness. We investigate the effect of IPS using data from a large randomized controlled trial (RCT). IPS has rapidly spread over most of the USA and is now available in at least 19 countries in Asia, Europe, and North America (Drake et al., 2020), and in Norway, the government recently decided to increase funding to make IPS a permanent service by redeploying funds from other vocational services (Government of Norway, 2017).

IPS is a well-defined and manualized method of job support that aims to help people with severe mental illness obtain and maintain regular employment in ordinary jobs, without any use of pre-vocational training (Drake and Becker, 1996). The method follows eight principles: 1) no

exclusion criteria, participation is purely based on the patient's choice, 2) focus is on regular paid employment, 3) job support is integrated with mental health treatment, 4) the job support is guided by patients' preferences, 5) financial counselling is provided, 6) the job search starts rapidly, 7) employment specialists providing the job support engages in systematic job development, and 8) individualized and continuous support is provided (Drake and Becker, 1996). Essentially, what IPS involves is individualized support provided by a "job specialist" that continuously works to secure the patient a job in the ordinary labour market. The support is flexible and takes place wherever the patient prefers, and the goal is to find a job that matches the patient's preferences. Since the idea is to use the ordinary labour market as a training arena, any setbacks are considered valuable experiences, not failures, and, if in demand, support continues even after a job has been obtained.

The purpose of this paper is to provide empirical evidence on the long-term effect and cost effectiveness of IPS compared to relevant "train-and-place" programmes. Traditional labour market programmes usually follow a "train and place" approach, with stepwise services that focus on prevocational training (Corrigan, 2001). While there is a rich literature on the effects of traditional active labour market programmes (see Card et al., 2017 for a review, and Gaure et al., 2012 and Markussen and Røed, 2014 for recent Norwegian analyses), to our knowledge, the economics literature lacks evidence how IPS compares to these programmes. In general, evaluation of rehabilitation strategies is problematic because of potential reversed causation and selection issues, and RCTs are called for (see e.g., Markussen and Røed, 2014; OECD, 2015).

This analysis directly addresses the strong recommendation in the OECD report to do "rigorous evaluation of new intervention programmes ..., ideally including a comparison group and random allocation, systematic data collection and – particularly important – the measurement of longer-term labour market outcomes for people with mental ill-health." (OECD, 2015). Thus, this analysis contributes to the existing literature in several respects. Within the medical literature, several RCTs worldwide report large, positive effects of IPS, however,

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often with relatively short follow-up and likely attrition bias due to self-report (Kinoshita et al., 2013). With one exception (Christensen et al., 2019), this is the first analysis of IPS for severe and moderate mental illness using registry data, thereby avoiding attrition bias as well as recall and justification bias. Registry data facilitates a long follow-up period, and, notably, we examine effects on objective outcomes up to 43 months after baseline, which is beyond timeframes of previous studies including our own (Reme et al., 2019).

This experiment is the largest RCT to date investigating the long-term effects of IPS on regular, unsubsidized employment and health for individuals with moderate to severe mental illness. In this article, we extend considerably on our previous work (ibid) both regarding the timeframe (an extension from 18 to 43 months) and the scope of the effect evaluation, as outlined in the following. Notably, in the current analysis we make an explicit distinction between subsidized and unsubsidized employment, which is important, since the aim of IPS is regular employment. This distinction has not been made clear in the established literature, although subsidized employment might be relevant in several countries. Also, for policy relevance, it is important to assess the net social benefit of the intervention. Answering this research question requires, among other things, data on the magnitude of employment, as well as whether employment is subsidized or not. We therefore study effects on regular employment both at the extensive and intensive margin, using information on labour income and hours worked, and conduct a cost-benefit analysis. We report long-term health effects, using health care utilization data from administrative registries. Furthermore, we investigate potential mechanisms that may explain the effect of the intervention.

## 2. Institutional context

The IPS intervention was developed in the US, where the labour market is quite different from a Norwegian context. Nordic working life is characterized by great autonomy for workers (Esser and Olsen, 2011). Scandinavian countries can be described as small, open economies, characterised by high levels of work participation and technology, small wage differentials and generous welfare states, where wage compression fuels creative destruction, i.e., over time, the work force is moved to the more productive firms, and low-paid, low-productive jobs disappear (Barth et al., 2014). Employees are compensated fully from the first day of sickness absence, and the individual employer bears the financial risk during the first 16 days of a spell. Therefore, one would expect that the high wage level and compressed wage structure in Norway would impose large demands on productivity and high hiring thresholds. If employers - rightfully or not - expect persons with mental illness to be less productive (e.g., less flexible regarding doing different tasks, more sick-listed), then, for a given (high) wage, these job seekers will appear less competitive in the labour market.

The Norwegian Labour and Welfare Administration (Nav) administers a range of welfare state services such as various employment schemes (e.g. work with assistance, traineeship in a sheltered business, wage subsidies), unemployment benefit, sickness benefit, disability and retirement pension, and social assistance. Social assistance is economic transfers that are meant to be transitory and need-based, i.e., not directly dependent upon the individual's labour market status. Individuals receiving social insurance are encouraged to work and can do so while still being entitled to social insurance, although labour income above a certain threshold causes some reduction in social insurance benefits. Many people with mental illness receive secondary psychiatric health care (outpatient services or hospital care). In Norway, these services are financed in the public health care system, with low co-payments, and services are delivered by public clinics and hospitals or private providers on contract with the health authorities. Vocational and health care services are usually offered as separate services with little collaboration across sectors. The IPS intervention is an example of such collaboration, between two independent government bodies: the Directorate of Labour

and Welfare and the Directorate of Health, a collaboration that is rare even in an international context (OECD, 2013).

## 3. The experiment

### 3.1. Design and procedure

Through a multicentre RCT design, IPS was compared to high-quality treatment as usual offered to people with moderate to severe mental illness. Severe mental illness mainly refers to psychotic or bipolar disorder with or without comorbid substance abuse/dependency, while participants categorized as having moderate mental illness primarily suffered from affective disorders, mainly depression and anxiety disorders. Participants were recruited to one of six IPS centres from regional primary and secondary mental health care settings while they were undergoing treatment for their mental disorders. To be eligible for inclusion, participants had to be currently out of the labour market but with an expressed desire to work. The only exclusion criterion was insufficient Norwegian language skills, since such skills were required to answer the questionnaires. Inclusion started on October 1, 2013 and ended on October 31, 2014. 227 and 181 participants were randomized to the treatment and control groups, respectively. The take-up rate was high in the IPS group as all participants had at least some contact with an employment specialist. The take-up rate in the control group was not systematically assessed. Two participants in the IPS group were excluded since they passed away during the first 6 months of the trial (Reme et al., 2019). The detailed study protocol is published and available online (Sveinsdottir et al., 2014). The pre-registered hypothesis was an increase in labour market participation in ordinary paid employment, other primary outcomes were measures of job acquisition, job duration, job intensity, and productivity. In this analysis, we also investigate eight secondary outcomes related to receipt of benefits and health care. All outcomes are presented in section 4.2.

Upon inspection of the registry data, it appeared that 81 participants were already registered as employed at the time of inclusion. We did indeed notice that some of the centres involved in the trial applied the inclusion criteria too liberally, which could explain this high rate of erroneously included participants. However, as the inclusion criteria for the trial was *no employment at baseline*, and since the IPS method was intended for this particular group, we decided to focus on the 327 participants who were not employed at inclusion (184 and 143 in the treatment and control groups, respectively).

### 3.2. Interventions

The treatment group received individual job support from an employment specialist that focused on regular employment, while members of the control group were offered prioritized spots in a vocational rehabilitation scheme. The control group treatment is a high quality version of TAU, because long waiting time is likely to be demotivating and also adds to the negative "lock-in effect" often associated with active labour market programmes: initially, participation in the program is time-consuming and causes participants to spend less time and effort on job search activities than non-participants (see e.g. Card et al. 2017, p.907). The TAU scheme primarily involved work with assistance and/or a traineeship in a sheltered business. While IPS follows a "place-and-train" approach to vocational rehabilitation, the intervention schemes in the control condition were more in line with a "train-and-place" approach (Sveinsdottir et al., 2014). The control interventions, although they also aim to promote regular employment, are based on stepwise approaches including prevocational training and non-competitive work in the process (e.g. interview training, help writing résumés and job applications, unpaid or subsidized apprenticeships, make-work jobs, job clubs, or sheltered workshops). For ethical reasons, the embargo period in the control group only lasted a year. After one year from inclusion in the study, control group participants

could thus cross over and get access to IPS as well. A total of 15 participants from the control group were reported to have crossed over to IPS after the embargo period. (For more information on ethical considerations, see Reme et al., 2019).

### 3.3. Recruitment and randomization

The randomization procedure was computer-generated and stratified by centre. We used a block size of 10 and generated two versions of the randomization list (with 2:1 and 1:1 ratio) for each centre. A 2:1 randomization ratio the first five months of recruitment was applied in order to ensure that the IPS centres could run according to maximal capacity. At each centre, the person responsible for inclusion secured informed consent and emailed the participants' project ID number, gender, and year of birth to the research unit. A blinded technician at the research unit carried out the randomization and returned the result by email or phone.

In the empirical analysis, we will take into account the difference in pace of inclusion between the two groups, shown in Fig. 1, by controlling for calendar time in our estimations.

## 4. Data

### 4.1. Data sources

The project collected information on mental illness severity at baseline, when psychiatric screening was administered for 248 of the 327 participants in our sample. These screening results form the basis for our indicator for severe versus moderate mental illness, in line with the medical IPS evaluation literature. The probability of receiving screening is somewhat higher in the treatment than in the control group, but the difference has weak statistical significance (p-value of 0.093). We have no reason to suspect any systematic pattern: missing psychiatric interviews were mostly attributed to logistic reasons such as time pressure and clinical capacity concerns.

Our primary registry data source is Nav's State Register of Employers and Employees (SREE), which informs us on start and stop dates for each job, as well as contracted hours per week, employer, and industry. Employers are required to report job spells to the SREE, for jobs that exceed 1 h a week (before January 2015, the lower limit was 4 h a week). The outcome variable *employment* equals one if the individual is registered with an active job in the SREE at some point during a calendar month. Furthermore, based on Nav data, we have constructed a set of indicators for receiving social insurance benefits or social assistance. Nav has also provided detailed information on individual participants' use of traditional active labour market programmes (including wage

subsidies), and the standard cost of these programmes, including administrative costs. The Directorate of Labour and Welfare and the Directorate of Health have delivered data on IPS administrative programme costs, which are mainly salaries.

From Statistics Norway, we have received information on annual wage income and the level of completed education by October each year, and from The Norwegian Patient Register, we got information on all visits to secondary mental health care.

### 4.2. Outcomes

The aim of IPS is to help people who are mentally ill obtain and maintain regular employment, therefore, we distinguish between subsidized and unsubsidized employment. The primary outcome we focus on in this analysis is *regular employment*, an indicator which equals one if the individual is registered as employed (i.e., the *employment* indicator defined above equals one) and, notably, received no wage subsidies that month. We also apply a stricter criterium, and investigate *regular employment, half-time or more*.

In order to investigate the number of hours worked in more depth, we construct the outcome *number of man-labour months*. This outcome is a composite measure of the outcome *regular employment* and the extent of part-time work that month (based on the information on contracted hours), summarized over a certain period. We also apply an alternative measure of work intensity, namely *annual labour income* for 2015 and 2016. Job acquisition and job duration are analysed using the outcomes *time till first job* after inclusion, *number of jobs* and *number of employers* during the follow-up period, and *mean job duration*.

The IPS intervention might potentially affect receipt of benefits and the use of mental health care services. To investigate this, we analyse the following eight secondary outcomes: *social assistance*, *unemployment benefit*, *disability pension*, *work assessment allowance*, the summary measure *any benefit*, and (for mental health care) *visits to outpatient health clinics*, *day care visits*, and *inpatient hospital stays*.

Most outcomes are measured on a monthly basis, i.e., the data are organised as a panel with 44 observations per individual (from the month of randomization till month 43, which is the maximum number of months when we observe all participants). For the outcomes *annual labour income* and *number of man-labour months*, we analyse the effect of IPS using one observation per individual. Naturally, the same applies when we report results after 12, 24 or 43 months in the main result table.

### 4.3. Descriptive statistics

The baseline characteristics of the control and treatment group are reported below:

All variables in Table 1 are binary indicators, except information on age, income, and health care utilization. Table 1 shows that the sample is balanced with respect to mental health problem severity and psychiatric health care utilization, age, sex, education, labour income the year prior to first inclusion, employment, receipt of various social insurance benefits, social assistance, and the summary measure "any benefit". More than half of the participants in both groups receive work assessment allowance at inclusion. This benefit, in combination with Nav's vocational rehabilitation programmes, aims at helping individuals find (or return to) employment, but it also serves as a screening device for entry into disability pension.

Descriptive statistics for some key outcomes post inclusion (employment, benefits received, and health care utilization) are presented in Appendix table 1. The outcomes are measured after 12, 24 and 43 months, and we note that the propensity of being employed increases over time, both for employment and regular employment. The increase is largest for the treatment group, indicating a positive effect of the intervention. Benefits received (social insurance uptake and/or social assistance) as well as health care utilization decrease over time, but this decline appears to be approximately equal for the two groups.

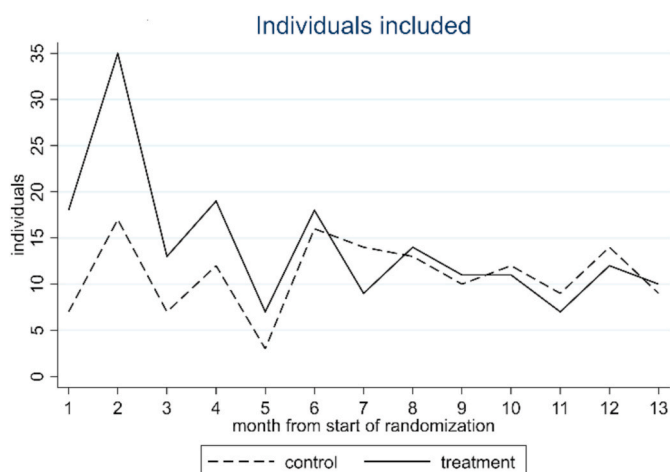


Fig. 1. Inclusion. The first month is October 2013.

**Table 1**  
Means at inclusion.

	Control	Treatment	Difference
Serious mental illness (otherwise moderate) <sup>a</sup>	0.42	0.51	-0.092 (0.154)
Age at inclusion	34.48 <i>10.84</i>	35.27 <i>10.64</i>	-0.789 (0.510)
Woman	0.49	0.5	-0.010 (0.851)
Compulsory schooling only, or missing <sup>b</sup>	0.55	0.57	-0.013 (0.818)
Upper secondary drop-out	0.05	0.03	0.022 (0.300)
Upper secondary completed	0.27	0.21	0.059 (0.210)
Some higher education	0.13	0.20	- 0.068 (0.105)
Wage income 2012 (1000 NOK)	68.3 <i>138.6</i>	79.2 <i>167.12</i>	- 10.9 (0.530)
Employment:1 if days of employment per month >0	0	0	0
Regular employment:1 if days of regular employment per month > 0	0	0	0
Unemployment benefit	0.02	0.04	- 0.022 (0.264)
Work assessment allowance	0.55	0.54	0.014 (0.796)
Disability pension	0.15	0.15	0.000 (0.998)
Social assistance	0.20	0.16	0.038 (0.368)
Any benefit <sup>c</sup>	0.84	0.81	0.029 (0.492)
Outpatient visits per month	1.83 <i>2.88</i>	2.43 <i>3.20</i>	-0.603 (0.078)
Day treatments per month	0.10 <i>1.17</i>	0.04 <i>0.25</i>	0.061 (0.491)
Inpatient stays per month	0.06 <i>0.26</i>	0.06 <i>0.28</i>	-0.004 (0.899)
N	143	184	

p-values in parentheses. For continuous variables, standard deviations are shown in italics.

<sup>a</sup> Psychiatric screening was administered at baseline for 248 individuals (102 and 146 participants in the control and treatment groups, respectively).

<sup>b</sup> Information on education is missing for 7 participants.

<sup>c</sup> The outcome *Any benefit* takes the value 1 if the individual receives one or more of the following transfers: unemployment benefit, work assessment allowance, disability pension or social assistance, 0 otherwise.

## 5. Empirical method

Since the intervention is randomized, its effect can be identified by comparing mean values in the treatment and control groups post inclusion. This effect is to be considered an intention-to-treat effect (ITT), since we do not observe the extent to which they take actively part in the programme. The information on background characteristics is utilized to inspect heterogeneity in the effect of the programme, and as control variables in regression analysis, in order to account for possible remaining heterogeneity between the treatment and the control group.

We estimate the following equation for several outcomes (employment, regular employment, labour income, social insurance uptake, and health care utilization) and for different lengths of the follow-up period (after one year, two years, and 43 months). In most analyses, data are organized as a monthly panel, but for ease of presentation, the time dimension is left out in the notation below: where  $Y$  is the outcome of individual  $i$ ,  $Z$  is an indicator for IPS treatment,  $X$  is a vector of individual characteristics determined prior to randomization, including age, gender, and centre. A potential problem, if labour demand differs by season or year, is that (on average) treatment group participants were recruited earlier than control group participants. In panel data analyses, we control for this by including dummy variables for calendar month

and year. In estimations with one observation per individual, we include month and year of randomization as controls.

The error term  $u$  is adjusted for heteroscedasticity in all analyses. In the panel data regressions, we cluster by individual (since the unit of randomization is the individual, see [Abadie et al., 2017](#)), otherwise we apply robust standard errors (Huber/White estimators). We investigate numerous secondary outcomes (specified in section 4.2), and a Bonferroni adjustment for multiple testing implies a significance level of 0.05/8.

## 6. Results

### 6.1. Employment

In this analysis, we focus on *regular employment*, i.e., having a job while getting no wage subsidies. Our data allows us to distinguish this outcome from the less precise outcome *employment*, which reflects having a job registered with an employer who operates in the ordinary labour market, as opposed to having sheltered employment. A similar definition to our outcome *employment* is used in numerous evaluations of IPS, including our previous work ([Bond et al., 2008](#); [Reme et al., 2019](#)). For comparison and the sake of completeness, we therefore report the effect on both employment and regular employment.

[Fig. 2](#) below gives a visual presentation of how the propensities of employment and regular employment evolve over time, where month 0 represents the month of randomization.

[Fig. 2a](#) and [b](#) clearly demonstrate that after inclusion, the probability of being employed and in regular employment increases in both groups, and considerably more so in the treatment than in the control group. It is also evident that the propensity to receive wage subsidies was higher in the treatment group than in the control group (3.9% versus 1.6% over the 43 months' period), since the difference in means is larger in [Fig. 2a](#) than in [Fig. 2b](#).

In the following, we investigate participants' labour market attachment in more detail, by considering various indicators of labour supply or use of social insurance. We also consider social assistance, although this transfer is transitory, economic assistance and not directly linked to the individual's labour market status. We apply the empirical specification presented in section 5 and report the mean ITT effect from these separate regressions below. The effect of IPS is estimated for four different follow-up periods: after 12, 24 and 43 months, and for all 43 months from randomization.

[Table 2](#) reports a large and statistically significant effect on the indicators for being employed and being in regular employment. This result holds for all four follow-up periods considered. We note that the estimated effect for the stricter outcome measure, regular employment half-time or more, is lower than for being in regular employment. Still, it is statistically significant after 12 and 43 months, as well as over the whole period. The overall result is that - during the first 43 months after inclusion - the propensity to be in regular employment is 8.8 percentage points higher in the treatment than in the control group, and the propensity to be in regular employment with a half-time job or more is 5.0 percentage points higher. These are large effects, given that the means in the control group for these outcomes are 16.5 and 10.7 percent, respectively, in the same period. The estimates for the effect on unemployment benefit or other social security indicators and benefits are close to zero and far from statistical significance. In the following, we inspect whether the effect on regular employment differs according to participants' background characteristics.

### 6.2. Effect heterogeneity

The average effect of IPS, reported in [Table 2](#), may mask substantial effect heterogeneity between subgroups. This is important information for policymaking, if treatment is to be targeted to the groups who benefit the most from it. [Table 3](#) below reports the effect of IPS on regular

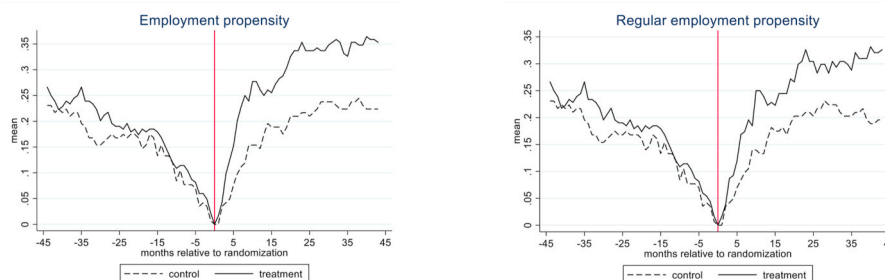


Fig. 2. Fig. 2a (left). The proportion employed, whether subsidized or unsubsidized, before and after the IPS intervention. n = 327. Fig. 2b (right). The proportion in regular, unsubsidized employment, before and after the IPS intervention. n = 327.

Table 2  
Effect on employment-related outcomes.

Dependent variable:	After 12 months	After 24 months	After 43 months	0–43 months
Employment	0.116 (0.008)	0.120 (0.015)	0.133 (0.009)	0.112 (0.001)
Regular employment	0.108 (0.010)	0.096 (0.045)	0.133 (0.007)	0.088 (0.005)
Regular employment, half-time or more	0.068 (0.040)	0.048 (0.240)	0.122 (0.005)	0.050 (0.047)
Unemployment benefit <sup>a</sup>	-0.006 (0.672)	-0.006 (0.539)	0.007 (0.770)	0.007 (0.489)
Work assessment allowance <sup>a</sup>	-0.031 (0.586)	-0.055 (0.337)	-0.051 (0.399)	-0.026 (0.537)
Disability pension	0.006 (0.898)	0.054 (0.253)	-0.010 (0.852)	0.012 (0.759)
Social assistance <sup>a</sup>	0.003 (0.943)	-0.005 (0.883)	-0.012 (0.655)	0.010 (0.643)
Any benefit <sup>a</sup>	-0.034 (0.286)	-0.027 (0.548)	-0.041 (0.487)	0.001 (0.961)
No. of observations	327	327	327	14,388

p-values in parenthesis. All estimations include control for age, gender, year, calendar month, and centre.

The outcomes are indicator variables. Any benefit takes the value 1 if the individual receives one or more of the following transfers: unemployment benefit, work assessment allowance, disability pension or social assistance, 0 otherwise. Clustered standard errors.

<sup>a</sup> For the outcomes unemployment benefit and work assessment allowance, information on all participants is available for 39 months after inclusion, and the column labelled “After 43 months” shows results for 243 participants. For social assistance and any benefit, the corresponding numbers are 38 months and 222 participants.

employment with respect to age, gender, mental health problem severity, and educational level (for participants above the age of 21, who may have obtained all levels of education).

While the effect does not differ between men and women (positive and statistically significant in both subsamples), it varies distinctively according to age, educational level, and mental health problem severity. The effect is largest for participants below the age of 30, individuals who have not completed upper secondary by the age of 21, and those who suffer from severe mental illness. These findings indicate that IPS has the largest impact on individuals who have a particularly weak position in the labour market. We have also tested whether the estimated effect differences between subsamples are statistically significant. However, the estimates of interaction term coefficients are not statistically different from zero, which possibly can be attributed to the low sample size.

Table 3  
Effect on regular employment, subsamples. 43 months follow-up.

Subsample	Number of observations	Number of individuals	Effect estimate
Men	7260	165	0.085 (0.051)
Women	7128	162	0.103 (0.024)
Aged 18–30 at inclusion	5896	134	0.152 (0.004)
Aged >30 at inclusion <sup>a</sup>	8492	193	0.061 (0.121)
Compulsory schooling or upper secondary drop out (aged >21)	7260	165	0.108 (0.007)
Completed upper secondary or some higher education (aged >21)	5632	128	0.033 (0.526)
Severe mental illness <sup>b</sup>	5192	118	0.114 (0.029)
Moderate mental illness <sup>b</sup>	5720	130	0.028 (0.599)

p-values in parenthesis. All estimations include control for age at inclusion, gender, year, calendar month, and centre. Clustered standard errors.

<sup>a</sup> The maximum age at inclusion was 63 years. A separate estimation for the age group 31–50 did not reveal any effect (161 individuals, p-value 0.33).

<sup>b</sup> Psychiatric screening was administered at baseline for 248 individuals.

### 6.3. The magnitude of the effect on regular employment

So far, we have analysed the effect on regular employment at the extensive margin. To better quantify the increase in regular employment, we take into account that far from all employed have full-time positions. Appendix Figure 1 depicts the distribution of weekly work hours given that participants were in regular employment. It shows that in about half of these cases, the workweek was below the standard full-time workweek of 37.5 or 40 h. We exploit the information on contracted hours to calculate - for each individual and each month - man-labour months supplied that month, which equals 0 if not employed, 1 if employed in a full time position, and takes a value in the interval {0,1} if working part-time. This number is then aggregated over months in the first, second, and third year, and over the whole period of 43 months after inclusion. See Table 4.

Table 4 shows positive estimates of the effect of IPS during all intervals in the post-treatment period, and the effect increases over time, although the estimates are not statistically significant for all intervals. Offering IPS rather than (a high quality version of) treatment-as-usual increases the number of man-labour months by about 0.2 during the first year, 0.5 during the second, about the same increase during the third year, and 0.8 during the last 8 months of our observational period, where the increase is highly statistically significant. Taken across the whole period, the number of man-labour months in regular employment increases by about two months compared to the control group.

**Table 4**  
Effect on number of man-labour months in regular employment.

Dependent variable:	Period after randomization				
	First Year (month 0–11)	Second year (month 12–23)	Third Year (month 24–35)	Month 36–43	Over all 43 months
No. of man-labour months	0.245 (0.134)	0.465 (0.150)	0.548 (0.186)	0.807 (0.007)	2.067 (0.047)
No. of obs. (individuals)	327	327	327	327	327

p-values in parenthesis. All estimations include control for age, gender, year and month of randomization, and centre. Robust standard errors.

Another indicator of the magnitude of the rise in regular employment is annual labour income. Pre inclusion, labour income is somewhat higher in the treatment group than in the control group, which is shown in [Appendix figure 2](#) (for 2010–2012), where labour income is measured in 2016 consumer price level. Post inclusion, i.e., for the years 2015 and 2016, there is a difference in the same direction. The unconditional mean labour income in the control group was 33 in 2015 and 55 in 2016, whereas the corresponding average in the treatment group was 55 and 67. In our estimations, we take into account that pace of inclusion differs between the two groups, depicted in [Fig. 1](#). [Table 5](#) shows results after controlling for individual characteristics, month of randomization, and site.

As expected, the treatment group had a higher labour income post inclusion, although the effect estimate is small in economic terms with a weak statistical significance. However, the positive estimates are in line with our previous finding that there was an increase in regular employment for this group compared to the control group.

#### 6.4. Effects on health care utilization

The impact of IPS on health care utilization is of interest for several reasons. Unambiguously, the participants have a health problem at baseline, and changes in health care utilization may be indicative of a health effect. Also, such changes are potentially important from a cost perspective. Secondary mental health care can be categorized at three levels: visits at outpatient clinics, day treatment, and in-patient hospital stays, for the latter, the cost per treatment is considerably higher. In [Appendix table 2](#) we report the effect on health care utilization. The signs of the coefficients indicate a transition towards lower levels of treatment. However, none of the estimates are statistically significant, be it for the whole sample or for those categorized as having severe mental illness at inclusion (not shown).

#### 6.5. Robustness

In all estimations, we control for centre, but the sample size does not allow separate estimations by centre. This could be relevant, since the

**Table 5**  
Effect on labour income.

Dependent variable:	Effect
Labour income 2015, in NOK 1000, 2016-value	16.8 (0.080)
Labour income 2016, in NOK 1000, 2016-value	8.6 (0.487)
No. of observations (individuals).	327

p-values in parenthesis. All estimations include control for age, gender, year and month of randomization, and centre. Robust standard errors.

centres involved in the current study are located in six different counties/municipalities with different labour markets, for instance with regards to centrality, dominant industries, and risk of exposure to economic fluctuations. Previous research on traditional active labour market programmes has indicated that effects are dependent upon macro-economic conditions ([Card et al., 2017](#)). We have tested whether the effect is driven by participants at one particular site. The results from six regressions where we exclude one centre at the time are reported in [Appendix table 3](#), and do not support that the overall effect is site-driven.

#### 6.6. Potential mechanisms

We have learned that the probability of being in regular employment is higher in the treatment group, most clearly demonstrated in [Fig. 2b](#). Several mechanisms may explain the picture given there. First, time till first job is shorter in the treatment group. However, if this is the only mechanism at work, we would expect the IPS effect to fade over time. Our results show the opposite, as reported in [Table 4](#). For maintaining employment in the longer run, finding a good job match is crucial and may require substantial search activity. A good match should then materialize in a long job duration.

Results illuminating these mechanisms are reported in [Table 6](#), for a subsample of participants who were employed at least once post inclusion and who did not receive any wage subsidy in that period (108 individuals). As before, participants are observed until 43 months after inclusion, which means that the data are right censored. We see that treatment group participants get their first job sooner than the control group. They appear to search more, since they hold a higher number of jobs, and with more employers, within the given time period. Furthermore, the job duration, averaged over the participant's jobs, is higher in the treatment group.

#### 6.7. Cost-benefit analysis

In the following, we will make a simple cost-benefit analysis for the 327 unemployed participants included in the programme, i.e., we investigate the extra costs and benefits for the average participant in the IPS group compared to average costs and benefits per person in the control group. Our time horizon is 43 months, i.e., from the first inclusion in Oct 2013 to May 2018. Costs and benefits per IPS participant are given in 1000 Norwegian kroner (NOK), 2016 value, with no discounting. The main items are presented below.

a)	Gain in production value	65
b)	Cost savings, traditional labour market programme costs	211
	Running costs IPS centres	-100
c)	Cost savings, excess burden of taxes	41
	Net social benefit	217

The benefit from IPS arises from three sources: the gain in regular

**Table 6**

Characteristics of regular employment post inclusion, at individual level. Unconditional means with standard deviations in parenthesis. Subsample: participants holding at least one job and receiving no wage subsidies.

Outcomes:	Control	Treated
Time till first job, months	13.79 (9.42)	13.14 (10.91)
Number of jobs	2.00 (1.25)	2.36 (1.55)
Number of employers	1.83 (1.05)	2.04 (1.35)
Mean job duration, days	390.2 (308.7)	443.1 (337.3)
n (individuals)	41	67

employment, cost savings from less use of traditional active labour market programmes, and less burden on public finances. Since we find no statistically significant effects on the use of unemployment benefit, disability pension, work assessment allowance, social assistance or health care utilization, these outcomes are not considered.

The extra gain in production value, compared to TAU, is estimated to be 2 months (Table 4). We proxy the value of an extra man-labour month by the mean gross labour income per man-labour month in 2015 and 2016 among those who were employed (NOK 24.9), i.e., about NOK 65 including social costs. The treatment group makes less use of labour market programmes that are alternative to IPS. This cost saving is about NOK 211, which more than compensates for the running costs of the six IPS centres during a 12 months' treatment period (NOK 100). Furthermore, items a) and b) above have direct and indirect implications for public budgets, and we estimate the corresponding reduction in the excess burden of taxes to be NOK 41 (Direktoratet for økonomistyring, 2018). In total, the net social benefit of IPS is NOK 217 per participant.

We have inspected the sensitivity of this result with respect to the average length of the IPS follow-up period. One of the main principles of IPS is time-unlimited support, and while we have month-by-month data on the duration of TAU treatment, we do not observe the length of the follow-up period for IPS participants. We find that based on information on IPS centres' monthly running costs, the average IPS follow-up period can increase to 34 months before the intervention is not cost-effective compared to TAU.

## 7. Discussion and conclusion

In this paper, we analyse the effect of a vocational rehabilitation programme (IPS) designed to help people with moderate to severe mental illness gain and maintain employment. The IPS programme is compared to high quality usual care, which in most cases involved various employment schemes such as work with assistance and traineeship in a sheltered business. The study is conducted in Norway, which is a high-income country characterized by high job security, low unemployment, and a comprehensive welfare system. Our primary interest was to investigate if IPS increased the probability of regular employment in the long run in unemployed people with mental illness. The combination of our experimental design, an RCT, and the use of administrative registry data is a major methodological asset of this analysis.

We find that IPS did indeed increase the probability of regular employment considerably, by 8.8 percentage points over the 43 months' follow-up period. This is a large increase in absolute terms, and particularly in relative terms, considering the mean propensity in the control group was 16.5 percent in the same period. The absolute effect on man-labour months is small, about 2 months in 43 months, which reflects that, when employed, many participants worked part-time, and a large proportion was not employed at all (36% in the control group and 53% in the treatment group were in regular employment at some point after inclusion). However, the effect is an increase of 45% compared to the mean level in the control group (which is 4.5 man-labour months). The length of the observational period matters: the effect on number of man-labour months in regular employment increases over time.

The effect is particularly large in subsamples of younger individuals (aged 18–30), low educated, and individuals suffering from severe mental illness. In fact, the results were non-significant in the sub-sample suffering from moderate mental illness. Previous studies of similar target groups show conflicting results (Reme et al., 2015; Hellstrom et al., 2017), and we therefore recommend that these findings are replicated in sufficiently powered studies of IPS for people with moderate mental illness. No statistically significant effect on health care utilization (or other secondary outcomes, i.e., social assistance and social insurance receipt) is observed, but the increase in employment did nevertheless not lead to adverse health effects, a concern addressed in Luciano et al. (2016).

When investigating potential mechanisms behind the effect of IPS,

we first note that for the control group participants, more time elapsed before they got their first job. A certain lock-in is to be expected, since the schemes offered to the control group involved elements like testing of work capability. In contrast, one of the principles of IPS – outlined in the introduction – is that job search starts rapidly. Spurring participants to search for and potentially accept job offers can be viewed as a success criterion for the intervention. When evaluating search activity, our results are more suggestive, since we necessarily do not observe search activity as such, only the success of such activity in terms of regular employment. Still, we note that, viewed over a period of three and a half years after inclusion, the treatment group holds more jobs, with more employers, which leads us to hypothesize that they undertake more search activity. Furthermore, one may suspect that such search activity impacts on the receipt of wage subsidies, as well. We observe a higher use of wage subsidies in the IPS group, despite the programme's focus on regular employment. In Norway, wage subsidies are highly accessible and often expected from employers, and we believe this feature, combined with higher search activity in the IPS group, can contribute to explaining this somewhat surprising finding.

Nevertheless, there are indications that the greater job search resulted in better job matches, since average job duration in regular employment is longer in the treatment group. In this respect, we should keep in mind that data on job duration are right-censored, while the effect on regular employment is particularly strong towards the end of the observational period, as reported in Table 4. Therefore, job duration, and the corresponding accumulation of human capital, may be more favourable for the treatment group with an even longer observational period.

All in all, we find that IPS leads to more labour market activity, both in terms of job search, the probability of regular employment, and the magnitude of this employment.

Two caveats are in place. Duration of effect of intervention programmes is of major importance, and differences between the intervention and control group could dilute over time. For our study, the intervention programme continued as ordinary practice after the trial, with the embargo for the control group participants to cross over and receive the intervention ending one year after completed study inclusion. Also, IPS-inspired services were incrementally promoted across all vocational rehabilitation services in this period and were thus increasingly incorporated into usual care. Both factors could imply that some in the control group at some point received the intervention or similar services. Given a true effect of the intervention, such factors could weaken our observed effect estimates over time. Furthermore, the effect of IPS is likely to be underestimated compared to full-scale implementation since, for ethical reasons, the control group was offered a high-quality version of TAU, i.e., they were prioritized into treatment.

On the other hand, we cannot rule out adverse general equilibrium effects, i.e., that IPS had a displacement effect on unemployed, marginalized job seekers not diagnosed with mental disorders and therefore not eligible for treatment, and who would compete with IPS participants in a presumably rather limited labour market (Crépon et al., 2013).

Finally, we argue that external validity is high in this trial. The participating centres were deployed in an ecologically valid setting, the research group was not involved in running the sites, and all the IPS centres scored consistently high on fidelity to the IPS method (Reme et al., 2019).

We conclude that the IPS intervention reaches the aim of increasing employment and is also cost-effective compared to the treatment-as-usual alternative. The results support policy shifts towards integrating work and health services for unemployed people with moderate to severe mental illness. We argue as such that studies based on long-term real-life functional outcomes are of high value and importance in extending the evidence base of approaches. The Organisation for Economic Co-operation and Development has argued for policies that integrate work and health approaches as an important shift to increase

work participation in mental health (OECD, 2015). The IPS programme is an example of such an approach, and our long-term outcome data should be of relevance and encourage further developments.

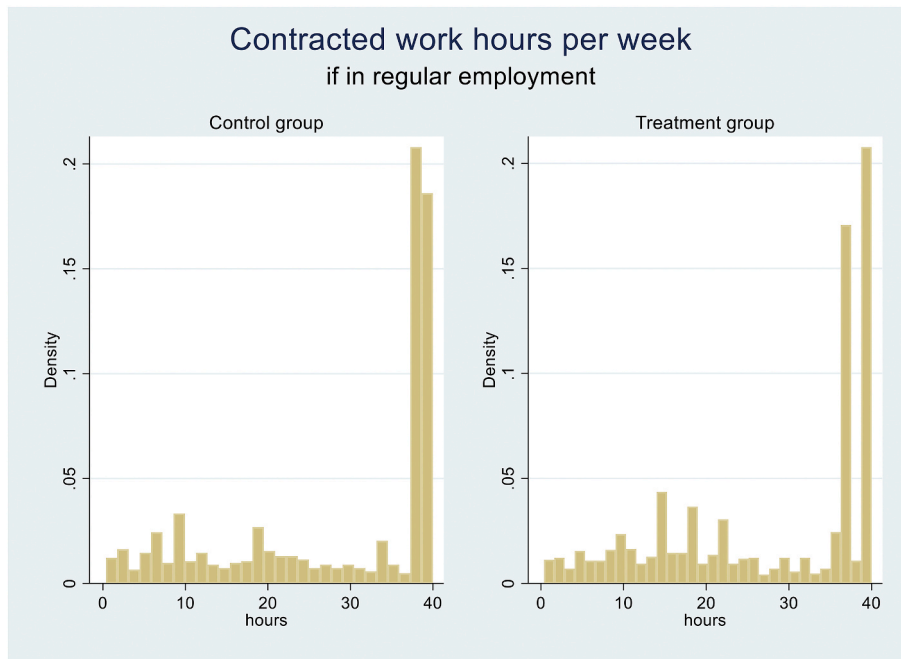
**Author contributions**

Tor Helge Holmås, data set preparation, Formal analysis, Writing – original draft, review & editing, Karin Monstad, data set preparation, Formal analysis, Project administration, Writing – original draft, review & editing, Silje Reme, Funding acquisition, Investigation, Project

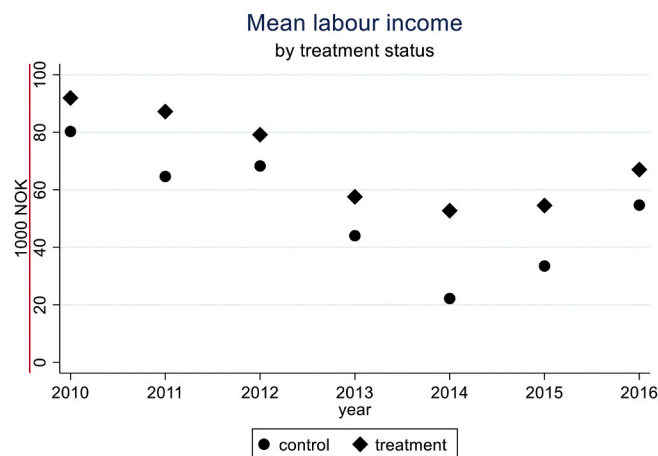
administration, Writing – original draft, review & editing.

**Declaration of competing interest**

None.



**App. Fig. 1.** Weekly work hours given that participants are in regular employment, observed 0–43 months after inclusion. N = 2624.



**App. Fig. 2.** Mean labour income, annual data. 327 individuals.



**App. Table 1**  
Outcomes measured 12, 24, and 43 months after month of randomization, for the control and treatment group. Means and standard deviations in parentheses.

	After 12 months		After 24 months		After 43 months	
	Control	Treatment	Control	Treatment	Control	Treatment
Employment	0.147 (0.355)	0.261 (0.440)	0.217 (0.414)	0.340 (0.474)	0.224 (0.418)	0.353 (0.479)
Regular employment	0.133 (0.341)	0.239 (0.428)	0.210 (0.409)	0.304 (0.461)	0.196 (0.398)	0.326 (0.470)
Any benefit <sup>a</sup>	0.874 (0.333)	0.826 (0.380)	0.790 (0.409)	0.777 (0.417)	0.764 (0.427)	0.730 (0.446)
Outpatient visits per month	1.287 (2.257)	1.565 (2.673)	1.252 (2.764)	1.647 (3.014)	0.622 (1.500)	0.940 (2.252)
Day treatments per month	0	0.011 (0.104)	0	0	0	0.011 (0.147)
Inpatient stays per month	0.070 (0.349)	0.027 (0.163)	0.070 (0.369)	0.033 (0.275)	0.021 (0.186)	0.005 (0.074)
No. of observations	143	184	143	184	143	184

<sup>a</sup> Any benefit takes the value 1 if the individual receives one or more of the following transfers: unemployment benefit, work assessment allowance, disability pension or social assistance, 0 otherwise.

**App. Table 2**

Effect on health care utilization, psychiatric treatment. 43 months' follow-up. Full sample.

Dependent variable:	
Outpatient visits per month	0.159 (0.367)
Day treatments per month	-0.010 (0.554)
Inpatient stays per month	-0.004 (0.683)
No. of observations	14,388

p-values in parenthesis. All estimations include control for age, gender, year, calendar month, and centre. Clustered standard errors.

**App. Table 3**

Effect on regular employment for different subsamples. 43 months' follow-up.

Sample estimated	n	No of individuals	Estimated effect
All centres	14,388	327	0.088 (0.005)
Ex. centre 1	12,276	279	0.079 (0.022)
Ex. centre 2	11,000	250	0.103 (0.003)
Ex. centre 3	11,132	253	0.078 (0.028)
Ex. centre 4	13,200	300	0.099 (0.003)
Ex. centre 5	12,760	290	0.092 (0.007)
Ex. centre 6	11,572	263	0.079 (0.032)

p-values in parenthesis. All estimations include control for age, gender, year, calendar month, and centre. Clustered standard errors.

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