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Are temporary work agencies stepping-stones into regular employment?

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Abstract

This paper estimates the causal effect of temporary work agency (TWA) employment on the subsequent probability of employment in the regular labor market. The main purpose is to estimate the stepping-stone effect separately for natives and immigrants, where the latter group potentially benefits the most from TWA employment. Since no quasi-experiment is available, individual Differences-in-Differences and matching is used to deal with the potential selection bias. The results point at a negative regular employment effect, which slowly fades away over a couple of years. Thus no evidence of a stepping-stone effect is found. When conditioning on immigrants this negative effect is absent. A long-run significant effect is found on overall employment probability (including TWA employment), there is even a long-run positive effect on annual earnings (mainly driven by women). Unemployment probabilities decreased, however the results in the estimation were less stable over time compared to the employment estimates, suggesting that the TWAs might keep individuals from exiting the labor market. Stratification on gender showed that the negative regular employment effect on women persisted for two more years compared to men.

Keywords: Temporary work agencies, stepping-stone, labor market, matching
JEL-codes: J15, J6, J4

1 Introduction

The growth of temporary working agencies (TWA:s) in Sweden and Europe has been rapid since the 1990's and the sector is still expanding in Sweden. The 2011 level of penetration[1] was about 1.4% (Bemanningsföretagen, 2011) which amounted to an all-time high; 62,863 employees (yearly full time equivalents). Thus, it is interesting to investigate whether this development has been of any advantage for the unemployed in terms of increased transition rates into regular employment.

Undoubtedly the deregulation of the market in 1993 was a major contributor to the rapid development since it made TWAs legal, only prohibiting agencies to charge employees for their services and imposing a 6 months TWA contracting stop if a job position has been terminated. The driving force behind the temporary work industry (TWI) is primarily the demand side of the labor market. Increased competitive pressure has forced employers to change their organizational structure towards the "lean" production fashion: The permanent work force is adjusted to the minimum production levels and increased demand is met with atypical employment such as temporary workers or workers hired through TWA.

The rationale for hiring through a TWA instead of recruiting a regular temporary employee is that there are costs associated with hiring and firing which can be mitigated by the TWAs. Furthermore, the tasks required at a company might not comprise enough to constitute even a part-time position. TWAs has the advantage of bundling together different tasks into one or several employment positions. Since recruiting is the TWAs main function it is argued that they have the economics of scale advantage which would imply that they are more efficient in both duration until sealing an employment contract and the quality of the match. This matching efficiency is a theoretical result by Neugart and Storrie (2006) and also claimed by the TWI itself. Empirically, there is however only inconclusive evidence, these findings will be summarized in Section 2.

When client firms hire staff from a TWA they assume – after accounting for hiring and firing costs – that the worker provided is the best possible match and that their own effort in recruiting would not be able to compete with the TWA's outcome. One hypothesis is that the TWAs increase the probability for a worker to gain permanent employment in the regular labor market by increasing the human capital, signaling working ambitions, expanding the worker's network and serving as a cheap screening device. The latter two effects are especially vital for immigrants since they are likely subject to statistical discrimination. The type of screening service that a TWA in effect offers is likely one of the most powerful remedies against statistical discrimination since the client firm will be able to observe the real productivity without hiring the worker. Since employers usually have a hard time to adequately assess an immigrant's abilities, education and skills acquired in a different environment, TWAs could be a remedy working as a cheap probation device where the uncertainty and risk has been incorporated into the TWA itself. It is also reasonable to believe that the immigrant's working network is weaker than natives' and just getting a job helps building up country specific human capital such as language skills and deeper knowledge of how the labor

[1]TWA employment as a share of total employment. There is reason to believe that the figure is an understatement since it is survey-based and non-repliers is more common among the agencies.

market works.[2] Another reason to further investigate the effect on immigrants is their overrepresentation in the industry (Andersson Joona and Wadensjö, 2010). All these things taken together lend reason to believe that immigrants may specially benefit from TWA employment. The opposite sign is of course also possible due to stigmatization. In this paper I will estimate the causal impact of employment in a TWA on the medium and long-run transition rate from unemployment to regular labor market employment, with extra focus on non-western immigrants.[3]

The paper is organized as follows. Section 2 reviews previous work done in the field and Section 3 describes a theoretical framework to pinpoint the parameter of main interest. Section 4 describes the data and the definition of the treatment group. Section 5 presents the estimation framework and outlines the matching estimation. The results from the various estimations are reported in Section 6, both for the matched and the unmatched sample, ending with a brief summary of the robustness check. Section 7 concludes.

2 Previous empirical work

Andersson *et al.* (2007) focus on how low income earners subsequent wage outcomes are affected by taking a job at a TWA. They find that the effect is positive but only if the workers find a stable employment afterwards. Ichino *et al.* (2008) use data from a custom made survey in two Italian regions. Collecting a wide variety of variables, relying on the CIA (explained in section 5.1) and utilizing a propensity score matching design (which have been the most common approach in this field) they get quite credible results pointing at a positive effect from TWAs in making the transition into regular employment.

Autor and Houseman (2010) takes advantage of a quasi-experimental setting provided by the 'Work First' strategy where US welfare recipients are randomly assigned into *nonprofit* contractors with differing job placement rates.[4] They find no or slightly negative effects of working as an indirect-hire in terms of subsequent earnings and employment outcomes. One must bear in mind that these results are mainly internally valid, they apply to the lowest socio-economic group, i.e. welfare recipients, and to the US labor market which is vastly different from the European labor market. Lane *et al.* (2003) find positive result in the US for workers at risk of being on public assistance, the design is based on defining counterfactuals and utilizing matching estimation. Since the research design of Autor and Houseman (2010) is more credible when it comes to inference, one might suspect that Lane *et al.* (2003) suffers from selection-bias which of course is of great concern since I do not have exogenous selection into treatment either. In Summerfield (2009) matching techniques are once again employed, the results indicate that a beneficial effect of TWAs may exist for women currently out of the labor force and trying to re-enter, while negative effects were found for men on regular employment in the long-run (4 years).

[2] Bennmarker *et al.* (2009) find significant positive effects for immigrants on time in employment after participating in a private job placement agency compared to the public employment service, indicating that immigrants might benefit more from private options, arguably due to the increased access to norms and networks on the Swedish labor market

[3] Non-western immigrants: Born in Africa, South America, Asia, Soviet Union or other European countries (i.e. excluding the Nordic countries and EU15)

[4] Autor has used the Work First program to research the TWI before, see e.g. Autor and Houseman (2005).

Garcia-Perez and Munoz-Bullon (2005) use a switching regression model which allows for self-selection. Their findings suggest that the ”low qualified” group is negatively self-selected and that ”highly qualified” workers benefit (in terms of a permanent contract) the most from TWAs.[5] Amuedo-Dorantes *et al.* (2008) compare regular temp workers to TWA workers posterior likelihood of being hired on a permanent basis by using matching on annual register data. They find a negative relationship but their research design is flawed since they identify their treatment group by future outcomes (i.e. not ever taking a TWA job) which biases the estimates downwards (in this case), as pointed out by Fredriksson and Johansson (2003). Kvasnicka (2008) uses high frequency register data and employ matching techniques to estimate a causal effect. Contrary to all of the other mentioned studies, no discernible effects on employment or unemployment where found.

In the Swedish setting, the research is scarce on this question and causal inference is yet inconclusive. Andersson and Wadensjö (2004) focus on the TWA's stepping-stone role for non-European immigrants. Their findings (based on register data) show that immigrants - in relation to natives - more often leave a TWA for another type of employment. This could be interpreted in favor of the stepping-stone hypothesis. The causal inference is however weak and mainly relies on reasoning based on *probit* correlations.

In a recent study, Jahn and Rosholm (2012) investigate the TWAs effect on the duration until exit from unemployment into regular employment for immigrants. They find significant and positive results when measuring in-treatment effects but nothing significant when examining the post-treatment effect. However, when dividing the sample into smaller groups such as non-western immigrants they found a post-treatment effect but no in-treatment effect.[6] The study was performed in Denmark implying that the results might be applicable to the Swedish labor market since they are not that different from each other.

There is evidently no real consensus in the field, the results span from positive, none to negative, underscoring that this is a relatively unexplored research topic.

3 Theory

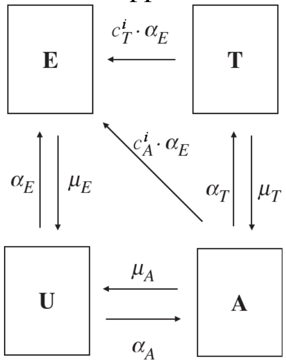
Since the question at hand concerns matching on the labor market it is useful to turn to the equilibrium unemployment model presented in Mortensen and Pissarides (1994). The more precise analysis of the problem has been done by Neugart and Storrie (2006) using an augmented Mortensen-Pissarides matching model. I will use their model to show what parameter I will try to estimate empirically.

Figure 1 sketches the model. E is regular employment, U is unemployment, A is employed at a TWA but not at work at any client firm and T is employed by TWA at work on behalf of a client firm. α_J (where J=E,U,A) are the exogenous rates at which a job offer arrives, while μ_J are the separation rates. Superscript i stratifies between immigrants (*im*) and natives (*na*) and the

[5]Qualification is measured as required qualification for the current job position. Thus it is not a perfect measure of individual skill.

[6]The definition of *non-western immigrants* in Jahn and Rosholm (2012) is much more narrow than in this paper, thus the results are not completely comparable.

Figure 1: Labor market flow chart, applied from Neugart and Storrie (2006)



factor c is the matching effect of the TWA, where $c > 1$ implies increased matching efficiency. If we expect the TWAs to increase job-searchers possibilities to get into regular employment, then $\frac{c_T^i + c_A^i}{2} > 1$. The hypothesis that immigrants gain more than natives through the cheap screening process implies $c_T^{im} > c_T^{na}$.[7] In the empirical estimation, I will however estimate the joint added effect of being employed at a TWA: $\delta^i = c_T^i + c_A^i - 2$ where c_A^i is not an effect of screening. Since the pool of workers in A is small due to internal efficiency by the TWAs, as shown by Kvasnicka (2003), this will not influence the estimates much. Neugart and Storrie's (2006) theoretical result suggests that c_T had to be above 1 otherwise the emergence of TWI would not have taken place, i.e. the matching efficiency is a prerequisite for their own existence.

4 The Data

Even though the TWI is growing rapidly it still does not constitute more than approximately 1.4% of the labor force. Therefore, I will have to use large datasets such as register databases in order to retrieve a sufficiently large sample. I have access to the composite register data used in Andersson and Wadensjö (2004). The main part of the data comes from *Longitudinal integration database for health insurance and labor market studies* (LISA) provided by Statistics Sweden. The composite panel database is balanced and give information of e.g. age, gender, place of birth, education, place of residence, employment status, annual income, days in unemployment etc. for about 7.3 million individuals 16-64 years old covering the years 1997 to 2008, making this study the largest in the TWA field up to date.[8] Since the research question at hand is whether TWAs works as a stepping-stone out of unemployment into regular employment, identification of the population is based on the unemployment status in November 2001.[9] This means that the sample under study is those unemployed or in a labor market program in November 2001. Also individuals older than 55 in 2001 are pruned out of the data in order to reduce the probability that their subsequent labor

[7]The screening hypothesis does not say anything about $c_A^{im} \gtreqless c_A^{na}$ but the expanding of the network hypothesis suggests a favor for the immigrants.

[8]The entire database presently holds annual registers since 1990 and includes all individuals 16 years of age and older that were registered in Sweden as of December 31 for each year.

[9]To increase computational efficiency, the control group was a 20% random sample, drawn from the population (excluding individuals in TWAs 2001) before any other restriction was put upon the group.

market outcome is affected by any early retirement plan.[10] Taking treatment is then defined as being registered at a TWA in November 2002. Subsequent years are recorded as labor market outcomes (see Table 1 or Appendix C for a description of the outcomes) or effect of treatment. The control group are those not joining a TWA in November 2002 (when unemployed in 2001), entry in TWA 2003 and onwards is allowed since I do not want to condition on future outcomes.[11]

Table 1: Definition of labor market outcomes[a]

Outcome 1	Annual probability of regular employment
Outcome 2	Annual probability of unemployment
Outcome 3	Annual probability of TWA employment
Outcome 4	Annual probability of employment (TWA or regular)
Outcome 5	Annual earnings

[a] The outcomes are not mutually exclusive

One drawback of the data is that administrative staff is not separately coded with the workers out for hire. The administrative staff is however a small share of TWA employment and the issue should not affect the results in any significant way.[12] Another caveat is that the data frequency is low, relying on annual observations make the treatment definition somewhat imprecise due to the absence of employment status information between the pre-treatment year 2001 and the treatment year 2002. However this will most likely only affect precision and not bias the estimates. Another drawback with low frequent data is that we cannot observe the in-treatment effect (the contemporary effect of treatment), any treatment effect taking place within a year will not be recorded due to the data structure.

When relying on a selection on observables design it is crucial to obtain all relevant pre-treatment observables that might be correlated with both the outcome and the selection into treatment equation. The most vital observable is previous unemployment duration since this is highly correlated with both the outcome and the selection equation. Due to the long period covered in the data much credibility is gained since the parallel trends assumption can be tested thoroughly. Moreover, deducing the long-run effect can be done in a more convincing manner than solely relying on a permanent employment indicator since we can follow the individuals over several years into post-treatment.

[10] In effect meaning that they are at most 63 in the last year, 2008.

[11] Examples of counterfactual outcomes in 2002 could be; taking up studies, getting a regular job or continue their unemployment spell etc.

[12] This data problem is common to all register based TWA research.

Table 2: Summary statistics for 2001, full sample

	Treatment group		Differences in mean	
	Control	**Treatment**	**Coefficient**	**T-statistic**
Gender				
Male	57.0	65.1	0.081	(17.03)***
Female	43.0	34.9	-0.0816	(-17.03)***
Aggregate days in unemployment 1998-2001			**-62.43**	**(-16.27)*****
0	0.8	0.6		
1-30	2.2	1.4		
31-90	8.0	11.4		
91-182	14.4	17.2		
183-274	12.3	15.1		
275-364	11.2	14.6		
365-730	37.6	32.2		
731-1094	12.8	7.2		
1095-1457	0.4	0.2		
Age groups				
16-20	8.7	12.3	0.022	(8.42)***
21-25	15.4	23.6	0.055	(14.50)***
26-30	15.3	18.8	0.057	(14.05)***
31-35	14.5	15.0	0.017	(4.65)***
36-40	14.9	10.8	-0.018	(-5.56)***
41-45	12.0	8.8	-0.036	(-12.37)***
46-50	10.6	6.0	-0.041	(-15.77)***
51-55	8.6	4.7	-0.040	(-18.31)***
Country of birth				
Sweden	73.9	75.0	.012	(2.69)***
Nordic countries (except Sweden)	3.2	2.8	-0.004	(-2.34)***
EU12	0.9	0.6	-0.003	(-3.79)***
Other European countries	7.0	6.7	-0.003	(-1.23)
Africa	2.2	2.2	-0.000	(-0.23)
North America	0.4	0.4	0.000	(0.72)
South America	1.5	1.7	0.002	(1.44)
Asia	10.7	10.3	-0.004	(-1.25)
Oceania	0.0	0.0	-0.000	(-9.95)***
Soviet Union	0.2	0.2	0.001	(1.15)
Highest education level				
Primary school less than 9 years	3.9	1.5	-0.024	(-17.80)***
Primary school 9 (10) years	17.9	14.4	-0.019	(-4.87)***
Upper secondary 2 years or less	34.0	27.5	-0.065	(-14.49)***
Upper secondary 2 years or more	27.2	35.5	0.072	(15.75)***
Higher education less than 3 years	8.6	10.5	0.016	(4.98)***
Higher education 3 years or more	7.3	9.9	0.023	(7.80)***
Post-graduate education	0.2	0.2	-0.001	(-4.59)***
Unknown	1.0	0.6	-0.003	(-3.89)***
Resides in a large city				
Stockholm	12.3	19.4	0.076	(18.98)***
Gothenburg	17.0	23.7	0.060	(14.37)***
Malmoe	15.4	10.9	-0.045	(-14.60)***
Other	55.4	46.0	-0.091	(-18.26)***
Number of observations	25,158	953	26,111	

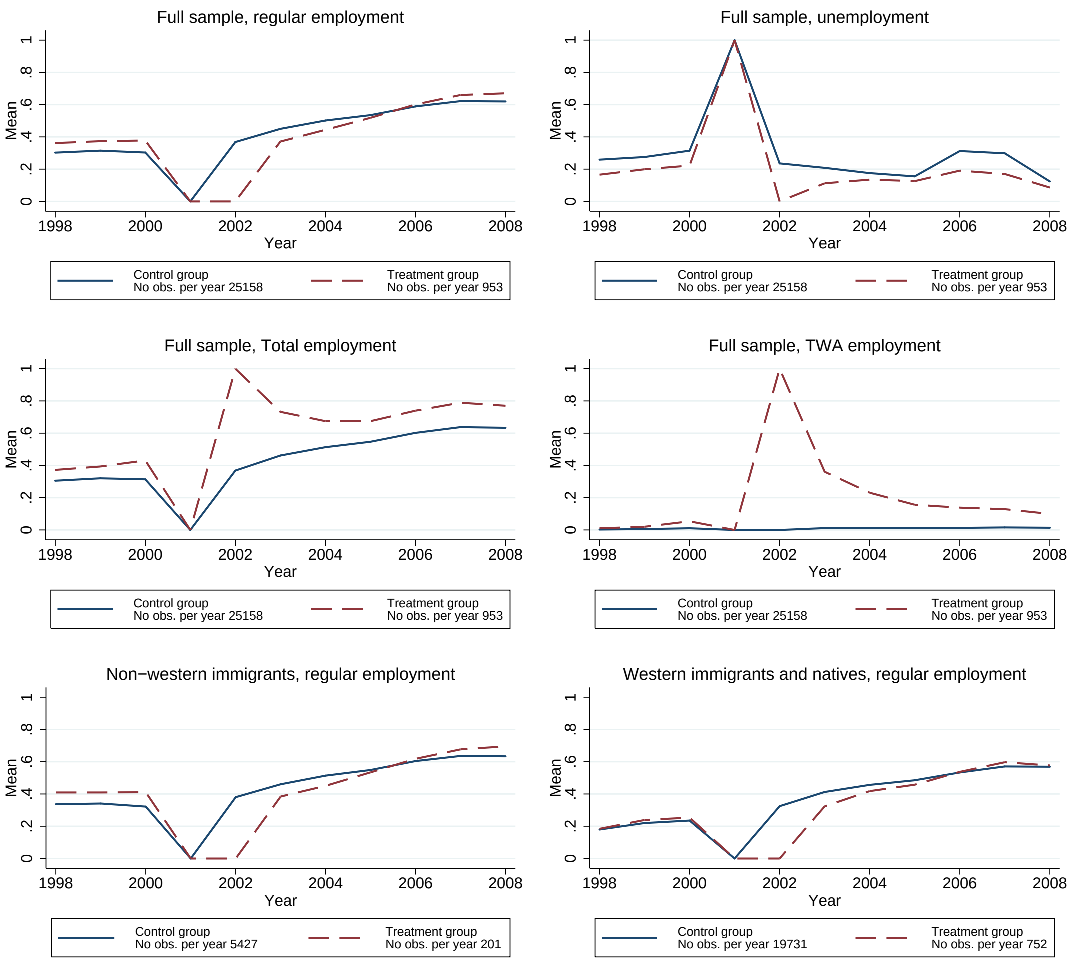
T-statistic in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2 presents descriptive statistics for selected variables and regression based t-tests for the treatment and control group. The two groups are not balanced in most dimensions. The treatment group is younger, has higher education, has on average been unemployed less, resides in

Stockholm and Gothenburg to a greater extent and consists of a higher share of males and natives. The latter two facts run counter to cross-sectional summary statistics of the population in 2002 where the opposite is true (Andersson and Wadensjö, 2004). This is a consequence of the sample selection since I actually identify observations by flow rather than stock in this paper.

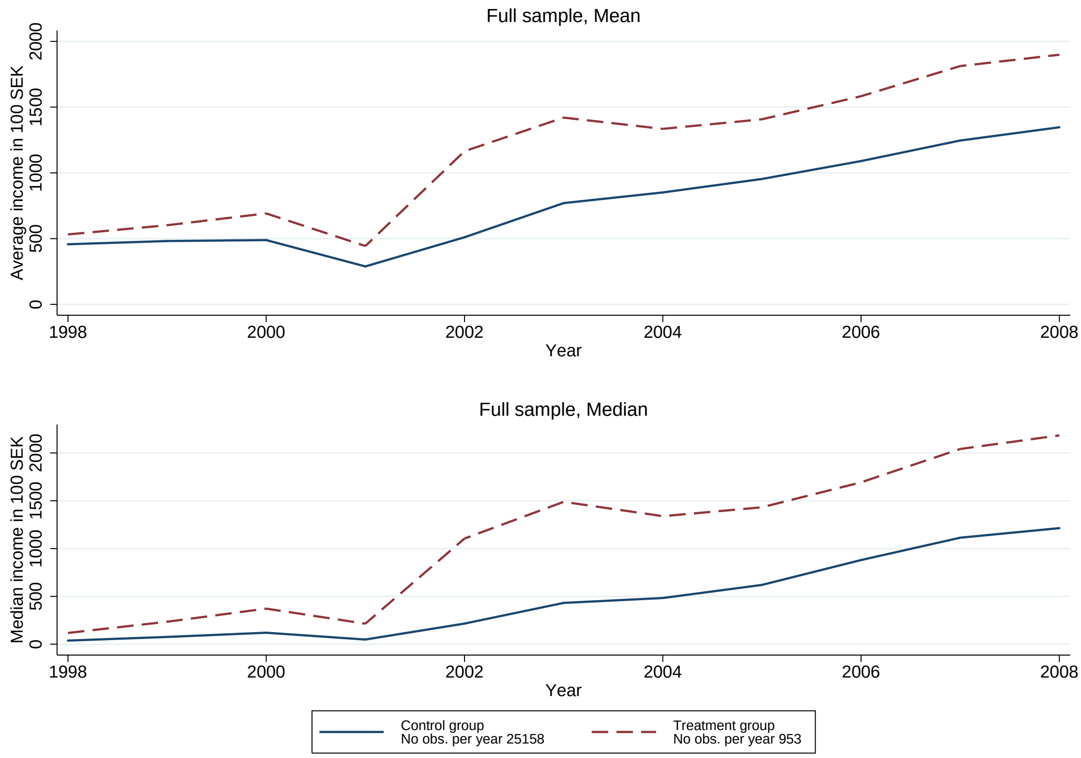
Controlling for the observed characteristics observed in Table 2 parametrically by an OLS means that obtaining unbiased results heavily relies on correct model specification.

Figure 2: Summary of outcomes



In Figure 2 we can follow different employment outcomes over different sample groups. The plots show no visible positive effect on the subsequent outcome after treatment by just comparing means. However, when examining income means and medians in Figure 3, we seem to detect a positive income effect, although a slight divergence in earnings is revealed in the early years.

Figure 3: Annual earnings



5 Methodology

When trying to estimate causal effects the main issue is to deal with selection bias caused by omitted variables. The goal is to estimate the *treatment effect* (TE):

$$\delta^i = Y_{1i} - Y_{0i} \tag{1}$$

where δ^i denotes the causal effect of interest and Y_{1i}, Y_{0i} denote the potential outcomes. Since we cannot observe more than one outcome per individual, it is impossible to estimate δ^i. We then turn to the following equation instead

$$E[Y_{1i}|D=1] - E[Y_{0i}|D=0]. \tag{2}$$

The problem is that those not taking treatment (the control group) might be inherently different and expecting a different outcome if taking treatment than the treatment group. This can be shown formally by adding and subtracting the counterfactual $E[Y_{0i}|D=1]$ and rearranging:

$$\underbrace{E[Y_{1i}|D=1] - E[Y_{0i}|D=1]}_{\delta \text{ or ATT}} + \underbrace{\left(E[Y_{0i}|D=0] - E[Y_{0i}|D=1]\right)}_{\text{potential selection bias}}. \tag{3}$$

The causal effect estimated is the *average treatment effect on the treated* (ATT). Solving or at least mitigating the selection bias is the key to obtain unbiased estimates (Angrist and Pischke, 2009). An ideal randomized experiment would imply that the last bracket equals zero thus solving the problem. Since I do not have access to a randomized experiment (or a quasi-experiment) two approaches will be used: Individual fixed effects and matching, both relying on the *conditional independence assumption* (CIA). The CIA is further described in the following subsection. Below I discuss how these methods deal with the selection issues.

5.1 Differences-in-Differences

An ideal econometric approach would have been to identify an exogenous variation into taking treatment or not, that would have ensured causal inference. Since the focus is on the causal effect of TWA employment (taking treatment) on the subsequent regular employment probability I will turn to the difference-in-difference model. When estimating this model it is crucial that; (i) both groups exhibit parallel trends, (ii) selection into treatment is conditionally random or at least not correlated with the outcome and (iii) nothing else that affects the outcome variable occurs at the same time as the treatment timing. Since I have not been able to identify an exogenous instrument that selects individuals into treatment I instead control for individual effects (exploiting the panel data structure) which might be correlated with both selection into treatment and the outcome. However, individual trends cannot be captured by this way of modeling. Another way to deal with this endogeneity is to employ matching techniques which relies on the CIA. It states that conditional on observable characteristics treatment is as good as randomly assigned, more formally:

$$D|X \quad \perp \quad Y_{0i} \tag{4}$$

Where D is getting treatment, X is a set of confounders and Y_{0i} the potential outcome if not taking treatment. A rich set of observables are as previously stated crucial in order to claim that the CIA holds and thus that the conditional differences-in-differences (cDiD) is valid. More specifically, observables such as previous labor market performance prior to treatment might control for the unobservable characteristics that cause a selection bias. One way of applying the matching approach is to weight the most crucial variables in the estimation in order to balance the two groups such that they look very similar along observable dimensions. This can be done by *coarsened exact matching* (CEM) where reweighing is done individually on the different confounders depending on relevance.[13]

The outcome variable of main interest is the long-run probability of getting employed in the regular sector, relying on employment status in several periods afterwards, will basically capture this long-run performance on the labor market.[14] Defining the outcome as $P(Y = 1)$, the probability of getting employed in the regular sector. An initial problem is that $P(Y = 1)$ is not observed

[13] The technique is outlined in Iacus *et al.* (2008) and Blackwell *et al.* (2009).

[14] Estimations will also be performed on unemployment, overall employment and annual earnings. But the focus will be on regular employment for simplicity.

in the pre-treatment period of 2001 since the criteria in this year is that the individuals under study should be unemployed. However we do not need outcome data for 2001 as long as we have outcomes for earlier years such as 2000 and 1999. 1998 will be used as the reference year, the parallel trends assumption can be tested using data for 1999 and 2000.[15] The most basic specification of a DiD model looks like:

$$Y_{i,t} = \alpha + \gamma_t + \phi d_i + \delta D_{i,t} + \beta' \mathbf{X}_{i,t} + \nu_{i,t}. \tag{5}$$

Where α denotes the intercept, γ_t a set of time dummies, $d_i = 1$[if in treatment group] and $D_{it} = 1$[if getting treatment]. The timing of the treatment is chosen to 2002 to ensure a long follow-up. $\mathbf{X}_{i,t}$ is a set of confounders and $\nu_{i,t}$ is the error term. Since matching is performed before estimating the equation we will omit the treatment group dummy (d_i) since its coefficient will be zero if the matching was successful. Including a group dummy would, in that case, only inflate the standard errors while not contributing to the model. A basic individual Differences-in-Differences model (iDiD) would only be slightly modified, where the difference is that we include individual dummies (a_i), instead of a group dummy, which takes care of the individual heterogeneity and may reduce standard errors. Joining a TWA after the treatment year by either the control or treatment group is permitted and thus selection is orthogonal to future outcomes. Violating this and in effect condition on future outcomes would lead to a bias of the estimated effect (Fredriksson and Johansson, 2003).

Both groups are identified by being unemployed in 2001; thus a form of matching on pre-treatment labor market outcomes is already performed here. The control group is defined as not joining a TWA in 2002 and instead engage in something else or stay unemployed. The counterfactual path is then, e.g., taking up studies, dropping out of the labor force, taking a regular job etc. No restrictions were put upon any outcomes from 2003 to 2008. To further investigate how the effect propagates in time, $\delta D_{i,t}$ will be replaced by an interaction between a set of time dummies and the treatment group. Including interactions also for the pre-treatment years can strengthen the parallel trends assumption since conditional on group (or individual) effects and time effects, past treatment $T_{i,t}$ will predict future outcome, $Y_{i,t}$, but future treatment, $T_{i,t}$, will not show any significant effect. This is a type of causality testing but it cannot however totally rule out the possibility of a selection bias. The main iDiD and cDiD[16] models estimated in this paper will be specified as

$$Y_{i,t} = \gamma_t + a_i + \sum_{\rho=1999}^{2000} \delta_\rho T_{i,\rho} + \sum_{\tau=2002}^{2008} \delta_\tau T_{i,\tau} + \beta' \mathbf{X}_{i,t} + \nu_{i,t} \tag{6}$$

and

$$Y_{i,t} = \gamma_t + \sum_{\rho=1999}^{2000} \delta_\rho T_{i,\rho} + \sum_{\tau=2002}^{2008} \delta_\tau T_{i,\tau} + \beta' \mathbf{X}_{i,t} + \nu_{i,t}. \tag{7}$$

[15]This time span might be considered a bit short, but I have also been running a probit on the entire working population and predicting values for 2001, they all support the parallel trends assumption together with the years 1999 and 2000, however since that approach is unorthodox it has not been included and the model does not rely on these results at all.

[16]*Conditional Differences-in-Differences*, i.e. DiD performed on a matched data set.

Where $T_{i,t} = 1[\text{if treated}]$, given that there is no anticipation effects (by construction impossible in this setting) the coefficient of the leads (δ_ρ) should be zero (i.e. parallel trends) strengthening our causal link hypothesis. Since matching is performed before estimating the equation we omit the treatment group dummy d_i in equation (6) since its coefficient will be zero if the matching was successful.

The reason for using both iDiD and cDiD (to be described in the following section) is that we can expect iDiD to give more precise estimates by construction, compared to a regular DiD by controlling for individual effects rather than two group effects. Also, controlling for unobserved and observed heterogeneity with fixed effects does not prune out observations like *coarsened exact matching* (CEM)[17] does; CEM might result in very few observations. On the other hand cDiD can by balancing the two groups mitigate the bias occurring when for instance the two groups have different age compositions which can give rise to diverging income progressions (steeper for younger people). By well-balanced groups it is also more convincing to point at the control groups' outcome as actual counterfactual outcomes since they are the same in all observed aspects. The drawback then is of course the low number of observations that arise due to tight matching criteria. Contrasting these two methods to each other will also give the reader a feel of how big the self-selection bias might be in this application.

5.2 Matching

Matching is a technique to overcome the selection-bias threatening causal inference. The approach is however not uncontroversial. Evidence pointing in favor of the technique comes from e.g. Dehejia and Wahba (1999) where they report a successful non-experimental analysis on the data in LaLonde (1986); using matching they replicate the experimental impact estimates. Smith and Todd (2005) criticizes Dehejia and Wahba (1999), but conclude however that the matching techniques is best put in a DiD-design which is what is done in this paper (*conditional DiD*). A principle conceptual difference between regular regression estimations and matching estimation is that it gives the researcher greater flexibility in choosing how to aggregate heterogeneous effects especially when using the specific technique; *coarsened exact matching*. Since previous work show that the impact TWAs has on individuals differ greatly among groups, this is of great importance. Due to the explicit and easily manipulated weighting procedure, which is in the hands of the researcher instead of implicitly in the estimator (as in OLS), matching makes it easier to estimate the interesting parameters such as the ATT in a stratified way (Cobb-Clark and Crossley, 2003).

The basic idea with matching estimators is that we try to find a "twin" for each individual taking treatment. This is done by matching on observable characteristics. The idea is that if the individuals are very similar in observables that are related to the outcome and selection process, the risk of them being different in unobservables that is correlated with outcome and selection is reduced or even eliminated. In practice we explicitly try to calculate the counterfactual untreated outcome $E[Y_{0i}]$. Compare eq. (1) with the following equation

[17] The matching technique is described in section 5.2.

$$\delta_i^* = Y_{1i} - E[Y_{0i}]. \tag{8}$$

Matching estimations rely on the CIA as discussed earlier. Furthermore, Rosenbaum and Rubin (1983) noted that an additional condition was needed, *common support*: If we define $P(x)$ as being the probability of getting treatment (D) for an individual with characteristics x, then the common support condition requires $0 < P(x) < 1, \quad \forall x$. This is also called the *overlap condition* and it rules out perfect predictability of D given x, without this assumption we have no information to construct our counterfactuals. CEM takes care of this by construction.

Coarsened exact matching is a member of the Monotonic Imbalance Bounding (MIB) class of matching methods (further described in Iacus *et al.* (2008)). It is a method of pre-processing data which deals with the 'curse of dimensionality'[18] by coarsening continuous data into bins where the researcher by in depth knowledge of the variables at hand can determine the size of the bins to preserve information and maximize number of matches. When the continuous variable is coarsened into bins, matching will take place on the respective stratas and then observations are finally re-weighted according to the size of their stratas. The bin width can be constant (ε_j) within the variable j or it can be varying within each variable, ε_j^v, where v is the cut-off points. Then basically any type of regression can be performed including the new weights on the uncoarsened data. If matching is exact in a variable – which is done in e.g. educational level – then this confounder is not needed in the regression since balancing is perfect, unless the variable is time variant. If matching only is exact on coarsened values and/or is time variant, then the confounder should be included in the regression to control for the within-bin correlation which most likely will be very small if the bin width (ε_j) is tightly defined.

The rationale for using CEM instead of e.g. *propensity score matching* is because the technique is more transparent, straightforward, by construction deals with the *common support*, gives priority to balancing (thus reducing bias and model dependence) to variance (high precision), meets the congruence principle, is computationally efficient and reduces sensitivity to measurement error (the latter will lead to biased estimates of the ATT, see Iacus *et al.* (2008)). In Figure 7 and 8 Appendix B two kernel density plots over aggregated days in unemployment from 1998 to 2001 and two histograms over age are graphed before and after matching to give a visual representation of what is going on in the matching process. The treatment group has a higher density over the left region of *days in unemployment* and vice versa over the right region, this skewness is adjusted through the matching. A similar adjustment takes place in the variable age, where the treatment group has a lower density in the left region and vice versa in the right. Notably the sample under study becomes a quite young sample compared with the population. Since balancing the two groups to each other change the average sample characteristics compared to the population, we in effect measure the *sample average treatment effect on the treated* (SATT) when applying CEM.

Matching was performed exactly in 2001 – unless other is specified – on gender, level of education and marital status, coarsened exact match on aggregate days in unemployment from

[18] Matching on a continuous variable will in effect rule out any matches.

1998 to 2001[19] ($\varepsilon_{unemp.}$ = 2 days), age (ε_{age} = 5 years), annual earnings in 2000 ($\varepsilon^{v}_{earnings}$ where v= [0, 5 000, 10 000, 15 000, 20 000, 25 000, 30 000, 35 000, 40 000, 60 000, 100 000, 200 000]). For non-western immigrants the income distribution was completely different and the following break points where chosen to get a sufficient amount of matches, v= [0, 3 000, 10 000, 50 000]. Number of children over age groups ($\varepsilon^{v}_{children_{age}}$ where v=[0.5, 1.5, 2.5] and age=[0-3, 4-6, 7-10, 11-15, 16-17, 18+]). When matching the non-western sample, *marital status* was not included since it reduced the number of matches and did not help to establish parallel trends. *Region of birth* was not included since it reduced the number of matches severely; if *region of birth* is included in the regression F-test cannot reject that the coefficients are zero (5% significance level).[20]

6 Results

This section will begin with multiple cross-sectional regressions measuring how subsequent labor market outcome varies over time from year 2002, then follow up with the iDiD design. This will be contrasted by the results obtained after the pre-processing matching technique *coarsened exact matching* resulting in a cDiD.

6.1 Unmatched results

I first use OLS to estimate

$$Y_{t,i} = \alpha_t + \beta_t (TWA)_{2002,i} + \gamma_t' \mathbf{X}_{t,i} + u_{t,i} \quad \text{for} \quad t = 2003:2008 \tag{9}$$

where Y is either regularly employed, unemployed or employed. Thus each line of Table 3 is a separate regression measuring the effect of joining a TWA in year 2002 on subsequent labor market outcomes.

The estimates in Table 3 hint at a locking-in effect during the first years which fades away and becomes insignificant in 2006 and 2007 though still negative. For unemployment, we find the same mechanic decrease of unemployment in the first year. Those who joined a TWA have 6.3 percentages points lower risk of unemployment in 2003, but already in 2004 the estimate becomes insignificant. In 2006 and 2007 the estimates once again turn significant. It would, however, be a far stretch to draw any inference about that since earlier insignificant estimates suggest that we are most likely picking up something unobserved systematic, e.g. the business cycle. The overall probability of employment rises by 0.22 and then hovers around 0.10 implying that treatment raises participants' overall employment rate. The non-western immigrants section of the table show similar results but with more unfavorable figures compared to the full sample in all aspects. These results will be contrasted against the more causally robust cDiD and iDiD estimates.

Making use of the individual DiD design we estimate the equation for the full sample and the

[19]Serves as a measure of labor market attachment.

[20]Four region of birth dummies in a joint hypothesis test give `P-value > 0.05` and inclusion of them actually increases the standard errors on the treatment variables while leaving the estimates unaffected.

Table 3: Multiple OLS-regressions
INDEPENDENT VARIABLE: TWA

	Full sample			Non-western immigrants		
	(1)	(2)	(3)	(4)	(5)	(6)
OUTCOME YEAR	P(Regular emp.)	P(Unemployment)	P(Employment)	P(Regular emp.)	P(Unemployment)	P(Employment)
2003	-0.128***	-0.063***	0.217***	-0.155***	-0.013	0.148***
	(0.016)	(0.011)	(0.015)	(0.035)	(0.027)	(0.034)
2004	-0.094***	-0.018	0.121***	-0.105***	0.026	0.073**
	(0.016)	(0.011)	(0.015)	(0.035)	(0.028)	(0.034)
2005	-0.051***	-0.010	0.090***	-0.076**	0.004	0.061*
	(0.016)	(0.011)	(0.015)	(0.036)	(0.027)	(0.034)
2006	-0.026*	-0.072***	0.095***	-0.035	-0.052*	0.062*
	(0.016)	(0.013)	(0.014)	(0.036)	(0.031)	(0.033)
2007	-0.005	-0.078***	0.104***	-0.021	-0.080***	0.083***
	(0.015)	(0.012)	(0.013)	(0.035)	(0.030)	(0.032)
2008	0.007	-0.013	0.089***	-0.039	0.010	0.051
	(0.015)	(0.009)	(0.013)	(0.036)	(0.024)	(0.033)
Observations	26,111	26,111	26,111	5,628	5,628	5,628
Control variables	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Control variables: Number of children over six different age groups and dummies for; gender, world region of birth, parents born abroad, labor market region, educational level, marital status, year of arrival, age groups, social welfare benefit received current year. For non-western immigrants the dummies for parents born abroad and some of the world region of birth dummies were dropped due to collinearity.

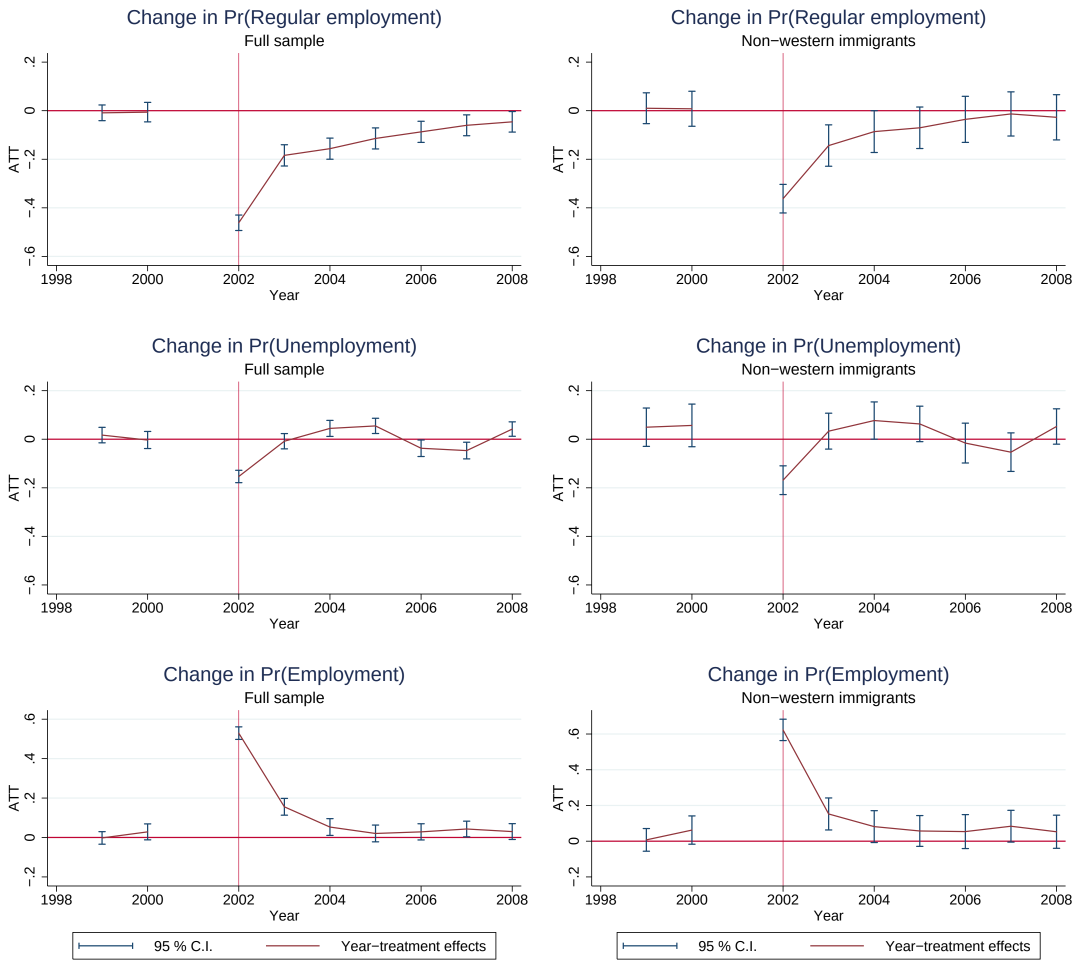
subsample *non-western immigrants*.[21] The estimated coefficients i.e. the *average treatment effects on the treated* (ATT) are plotted in Figure 4 and the corresponding Table 7 is found in Appendix A. The estimates exhibit a similar pattern as in Table 3 but overall they are more unfavorable for the stepping-stone hypothesis.[22]

Regular employment exhibit a substantial locking-in effect followed by what could be explained as a TWA-stigmata (i.e. having worked at a TWA might signal bad ability and regular employers shy away from these employees) but not quite as bad for non-western immigrants. As for unemployment it shows no significant effect in 2003 but then follows a two year increasing probability of unemployment followed by a decrease, contrasting against the naive OLS results in Table 3 which in all periods were negative (though insignificant at times). A possible explanation

[21] Stratification is also done by gender but no large diverging results were found, the estimates can be found in Table 8 Appendix A.

[22] Standard errors were clustered on individual level to account for possible serial-correlation.

Figure 4: iDiD, employment status



to why the unemployment probabilities exhibit a cyclical pattern is that it follows the actual unemployment fluctuations in the real economy. This implies that the treatment group might be weaker in the labor market, i.e. negative self-selection might be an issue. The results hardly varies when conditioning on *non-western immigrants*, though one could argue that they do not suffer any or less of a TWA-stigmata since the post-treatment estimates on regular employment are lower than for the full sample and revert back to insignificance more quickly. The most striking feature of the plots is that once you enter a TWA the probability of being regularly employed is reduced for at least 7 years. Overall employment status is however not negatively affected, if anything there is a positive effect (though only significant at a 5%-level for 2003 and 2004). For instance, joining a TWA in 2002 resulted in a 15.6 percentage points decrease of probability of being regularly employed in 2004, but in the same year the overall probability of being employed increased by 5.3 percentage points. Thus it might be interesting to take a closer look at the income progression that one would expect from joining a TWA. Previous studies usually show that the working conditions in TWAs are worse and that the salary is lower compared to those with regular employment contracts (Jahn, 2008).

When performing the individual DiD estimation on income, the parallel trends assumption is in the danger zone. Figure 3 suggests a small diverging trend from 1998 to 2001; the individual FE:s do not control for the differences in the pre-existing trend. This is picked up by the iDiD estimator in Figure 10 Appendix B and suggests that the estimates are unreliable. In the following section, matching will prove itself useful compared to fixed effects in mitigating these sorts of problems.

6.2 Matched results

To measure the overall imbalance in the unmatched dataset which is visible in Table 2 we can use the $\mathcal{L}_1$ statistic introduced by (Iacus *et al.*, 2008) as a comprehensive measure of global imbalance. Starting by discretizing the continuous variables by using a pre-defined binning-algorithm[23] and binning the variables by the researcher's choice. Comparison will be made between the two approaches using the following statistic

$$\mathcal{L}_1(f,g) = \frac{1}{2}\sum_{\ell_1\cdots\ell_k} |f_{\ell_1\cdots\ell_k} - g_{\ell_1\cdots\ell_k}|$$

where k is the number of dimensions (or variables), f is the treated, g is the control and ℓ is the respective variable imbalance. $f_{\ell_1\cdots\ell_k}$ is then the k-dimensional relative frequency for the treated. $\mathcal{L}_1 = 0$ means perfect balance and $\mathcal{L}_1 = 1$ means perfect imbalance. The measure by itself is not that informative but computing the pre-matching $\mathcal{L}_1$ and comparing it to the post-matching $\mathcal{L}_1$ will show if the matching was successful.

The first balancing is done on the full sample where pre-matching $\mathcal{L}_1 = 0.99$ and post-matching $\mathcal{L}_1 = 0.86$. The summary statistics reported in Table 4 exhibit a clear improvement relative to Table 2. Since matching on *aggregate days in unemployment* was performed at 2 days precision – and not in the intervals specified in the table – the balanced result cannot be entirely visible in the table (though it can be viewed in Figure 7 in Appendix B). *Country of birth* is not perfectly balanced since no matching was made on that variable. Still, improvement has been made; only the *Nordic countries* are unbalanced in the treatment group's favor (2 percentage points more in the treatment group). The fact that balancing on some variables give rise to balance on other observable variables credits the plausibility of the CIA assumption, since it is not unreasonable to believe that unobserved characteristics also might become balanced. The post balancing result for the subsample *non-western immigrants* are shown in Table 5. Balance is not perfect in the mean *Aggregate days in unemployment* but the difference is insignificant. Balancing has not been performed on *country of birth* or *year of arrival*, yet they still balanced after matching. The $\mathcal{L}_1$ statistic equals 1.0 for pre-matching and 0.80 post-matching, an even better improvement than for the full sample matching. If we compare the $\mathcal{L}_1$ statistics and the matched tables to the unmatched it is clear that improvements have been made. However, the common support condition implies a substantial reduction in the number of observations which affects the precision of the estimates.

[23] The default coarsening algorithm by the matching software is *Scott break method*: $\varepsilon_{scott} = 3.5\sqrt{\bar{s}_n^2}\, n^{-1/3}$ where n denotes sample size and $\sqrt{\bar{s}_n^2}$ the sample standard deviation (Scott, 1992).

Table 4: Summary statistics for 2001, matched sample

	Group		Differences in mean	
	Control	**Treatment**	**Coefficient**	**T-statistic**
Gender				
Male	69.9	69.9	0.000	(0.00)
Female	30.1	30.1	0.000	(0.00)
Aggregate days in unemployment 1998-2001	**332**	**331**	**-0.790**	**(-0.07)**
0	0.4	0.4	0.4	
1-30	2.9	1.6	2.7	
31-90	10.1	12.3	10.4	
91-182	19.1	18.1	18.9	
183-274	16.5	15.4	16.4	
275-364	14.0	13.9	14.0	
365-730	29.1	31.3	29.4	
731-1094	7.5	6.5	7.4	
1095-1457	0.4	0.4	0.4	
Age groups	**30**	**30**	**0.043**	**(0.09)**
16-20	12.9	12.9	-0.000	(-0.00)
21-25	30.1	30.1	0.000	(0.00)
26-30	21.0	21.0	-0.000	(-0.00)
31-35	13.6	13.6	-0.000	(-0.00)
36-40	7.2	7.2	-0.000	(-0.00)
41-45	7.4	7.4	-0.000	(-0.00)
46-50	3.8	3.8	-0.000	(-0.00)
51-55	4.0	4.0	-0.000	(-0.00)
Country of birth				
Sweden	82.6	82.1	-0.006	(-0.29)
Nordic countries (except Sweden)	1.1	3.3	0.0214	(2.75)***
EU12	0.4	0.5	0.002	(0.40)
Other European countries	4.9	4.0	-0.009	(-0.79)
Africa	2.4	1.4	-0.009	(-1.31)
North America	0.4	0.5	0.001	(0.34)
South America	1.1	1.1	-0.000	(-0.00)
Asia	7.0	7.1	0.001	(0.07)
Oceania	0.0	0.0	-0.000	(-1.00)
Soviet Union	0.1	0.0	-0.001	(-1.41)
Highest education level				
Primary school less than 9 years	0.5	0.5	0.000	(0.00)
Primary school 9 (10) years	12.7	12.7	0.000	(0.00)
Upper secondary 2 years or less	28.6	28.6	0.000	(0.00)
Upper secondary 2 years or more	42.0	42.0	0.000	(0.00)
Higher education less than 3 years	7.8	7.8	0.000	(0.00)
Higher education 3 years or more	8.2	8.2	0.000	(0.00)
Unknown	0.2	0.2	0.000	(0.00)
Resides in a large city				
Stockholm	15.0	15.0	0.000	(0.00)
Gothenburg	22.1	22.1	0.000	(0.00)
Malmoe	10.5	10.5	-0.000	(-0.00)
Other	52.4	52.4	-0.000	(-0.00)
Number of observations	3,488	552	4,040	

Robust standard errors
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Summary statistics for 2001, non-western immigrants matched sample

	Group		Differences in mean	
	Control	**Treatment**	**Coefficient**	**T-statistic**
Gender				
Male	82.7	82.7	-0.000	(-0.00)
Female	17.3	17.3	0.000	(0.00)
Aggregate days in unemployment 1998-2001	**405**	**385**	**-19.30**	**(-0.60)**
0	0.2	1.0		
1-30	2.2	0.0		
31-90	6.2	8.2		
91-182	16.9	12.2		
183-274	14.1	18.4		
275-364	10.0	11.2		
365-730	38.2	40.8		
731-1094	11.3	8.2		
1095-1457	1.0	0.0		
Age groups	**31**	**31**	**0.270**	**(0.27)**
16-20	9.2	9.2	-0.000	(-0.00)
21-25	24.5	24.5	0.000	(0.00)
26-30	13.3	13.3	-0.000	(-0.00)
31-35	25.5	25.5	0.000	(0.00)
36-40	11.2	11.2	0.000	(0.00)
41-45	14.3	14.3	0.000	(0.00)
46-50	2.0	2.0	0.000	(0.00)
Country of birth				
Other European countries	27.2	32.7	0.055	(0.90)
Africa	18.1	13.3	-0.048	(-1.23)
South America	6.1	7.1	0.011	(0.001)
Asia	46.9	46.9	0.001	(0.01)
Soviet Union	1.8	0.0	-0.018	(-1.54)
Highest education level				
Primary school less than 9 years	4.1	4.1	-0.000	(-0.00)
Primary school 9 (10) years	16.3	16.3	0.000	(0.00)
Upper secondary 2 years or less	22.4	22.4	-0.000	(-0.00)
Upper secondary 2 years or more	36.7	36.7	-0.000	(-0.00)
Higher education less than 3 years	8.2	8.2	0.000	(0.00)
Higher education 3 years or more	9.2	9.2	0.000	(0.00)
Unknown	3.1	3.1	0.000	(0.00)
Year of arrival				
1936-1965	0.4	0.0	-0.004	(-1.20)
1966-1972	1.6	1.0	-0.005	(-0.37)
1973-1985	18.5	17.3	-0.012	(-0.24)
1986-1993	51.0	42.9	-0.082	(-1.27)
1994-2001	28.4	38.8	0.103	(1.66)*
Resides in a large city				
Stockholm	34.7	34.7	0.00	(0.00)
Gothenburg	27.8	27.8	0.00	(0.00)
Malmoe	6.9	6.9	-0.00	(-0.00)
Other	30.6	30.6	0.00	(0.00)
Number of observations	275	98	373	

Robust standard errors
*** p<0.01, ** p<0.05, * p<0.1

In Figure 5 the different *sample average effects on the treated* (SATT) are plotted over time, the estimates are displayed in Table 9 Appendix A. For the full sample, the probability of regular employment is not significantly different from zero in 2007 while the iDiD estimates were always significantly negative (Table 4).[24] Stratification on gender shows that women endure negative regular employment estimates until 2006 while men's estimates only remain significant until 2004, see Figure 9 Appendix B. Another change that has taken place – compared to the iDiD estimates – is in the regular employment probability for non-western immigrants where there is no evidence of a negative significant effect, which is in line with the theory of a more favorable subsequent labor market outcome for immigrants than for natives, however this is rather an absence of adverse effects than a prevailing positive effect. Notably, the standard errors are large and the estimated insignificant effect for 2003 might just be due to imprecision caused by the reduced amount of observations.

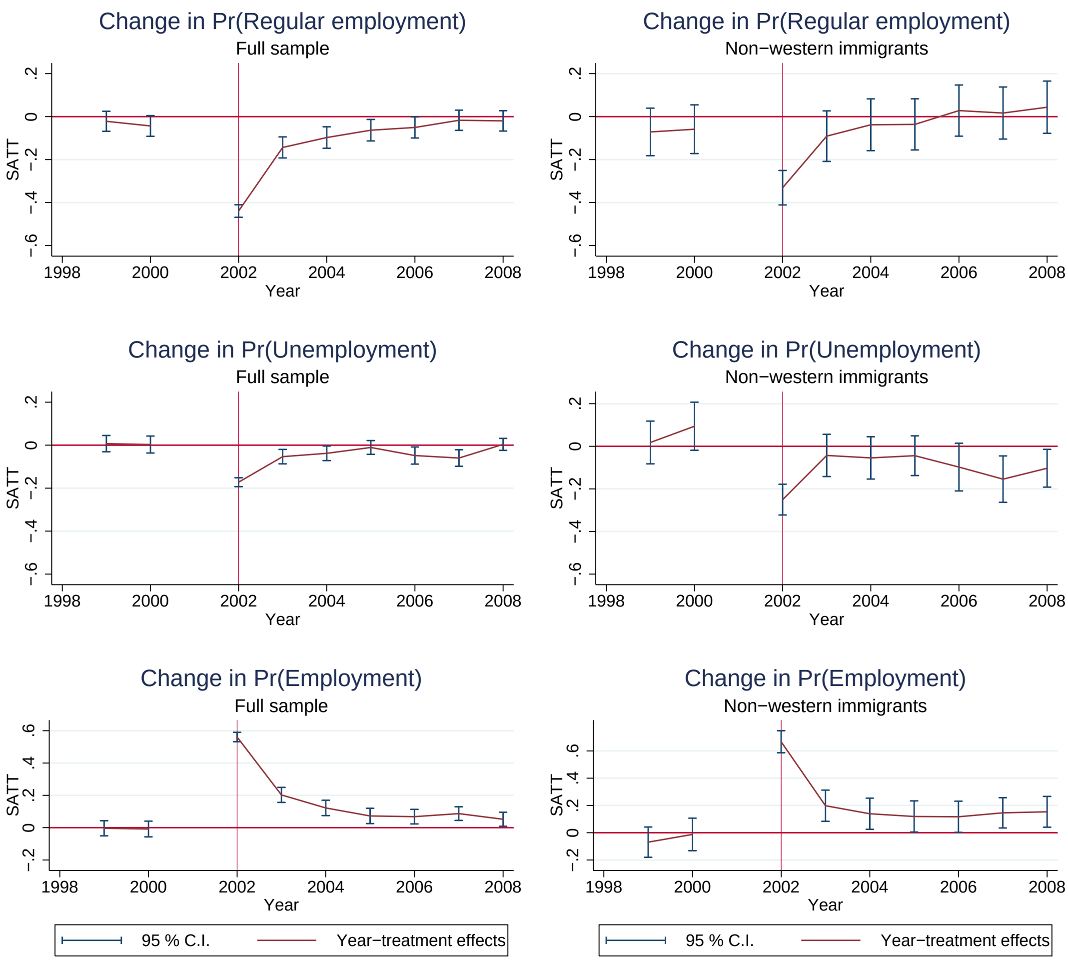
The estimates for unemployment (Figure 5 and Table 9 Appendix A) are in agreement with the overall employment results, by never getting anywhere near significantly positive. The cyclical pattern of unemployment has been reduced by the matching process thus showing a favorable image for TWA's effect on the transition out of unemployment. Apart from 2005, until 2007 all estimates are significantly negative, hovering around 0.04 to 0.08. For instance, joining a TWA in 2002 reduced the probability of getting unemployed by 4 percentage points in 2004. Compared to the iDiD estimates where the unemployment significantly fluctuated around zero, the cDiD estimates do not. If negative self-selection was the reason for this pattern then it seems like the matching has mitigated the problem, making these estimates more reliable. The unemployment estimates for non-western immigrants exhibit an insignificant positive trend before taking treatment, the in-treatment effect is then significantly negative, followed by four consecutive years of negative insignificant estimates and then of two years significantly negative estimates. The pattern displayed in the *non-western immigrants'* panel is similar to the full sample panel even though – maybe due to imprecision – more estimates are insignificant and also the impact is larger. For instance; in 2007 taking treatment lead to a 15.4 percentage points drop in probability of being unemployed for non-western immigrants and 8.7 percentage points for the full sample.

The overall employment (Figure 5) has also changed significantly from the iDiD estimates, the panel show clear cut evidence of a long-run change in probability of employment for both the full sample and the immigrant subset. Given that the matching successfully eliminated the self-selection bias, we can causally interpret this as joining a TWA increases one's probability of employment in the long run by approximately 7 to 9 percent points in general and 12 to 15 percent points for non-western immigrants.

The coarsened exact matching technique also showed itself useful in the income equation where the process purged out individuals with a diverging pre-treatment income trends and reduced the standard errors. A time trend has also been included in the earnings regression apart from the other confounders included in the employment status cDiD. Figure 6 (and Table 10 in Appendix A) report the estimates. There are no subsequent adverse earnings effect by joining a TWA. In fact the income progression seem to benefit from TWA employment, supporting the descriptive results

[24] Standard errors were clustered on individual level to account for possible serial-correlation.

Figure 5: cDiD, employment status



in Figure 3. When stratifying on gender (Figure 6 and Table 10 in Appendix A) it seems like the effect is mostly driven by women. Since the parallel trends assumption cannot be empirically supported in the non-western immigrants sample the estimates are unreliable.

The estimated effects can at first sight seem a bit large, but there is more than a wage effect induced by TWA employment driving these estimates. To disentangle the wage effect of TWA we can decompose the earnings in the following simple equation: $earnings = P(employment) \times wage \times (annual) hours$. Taking the natural logarithm and differencing gives

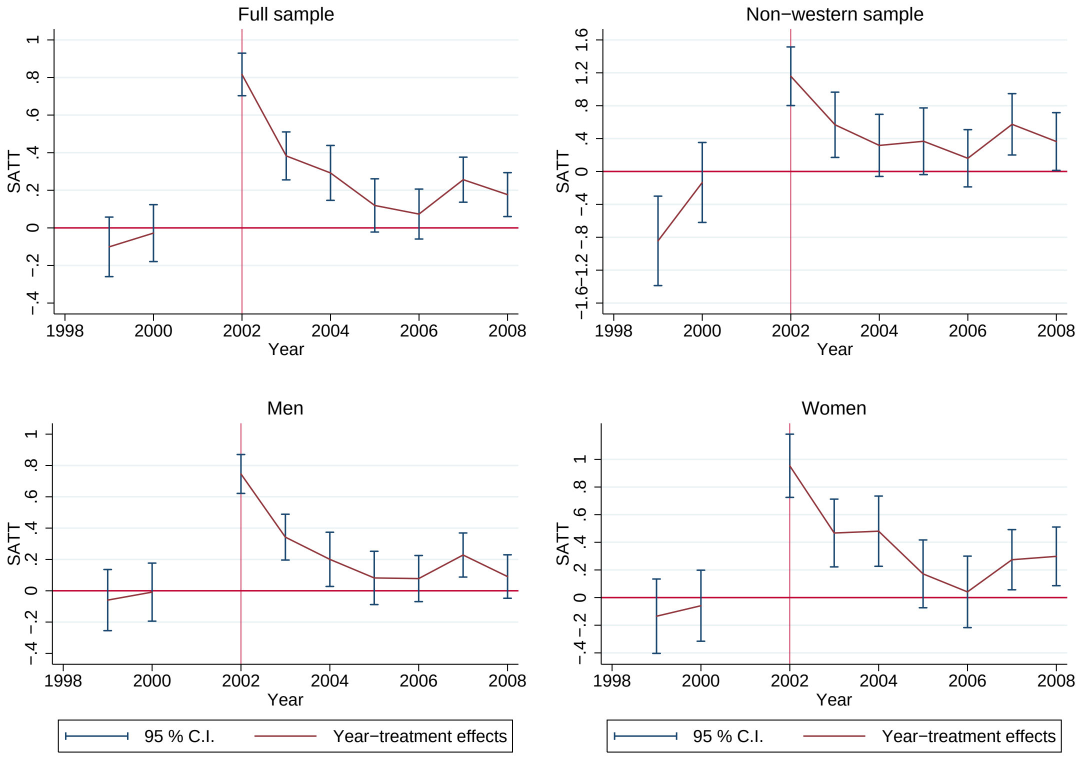
$$\Delta \ln earnings = \Delta \ln employment + \Delta \ln wage + \Delta \ln hours$$

where

$$\Delta \ln employment \approx \frac{\Delta employment}{employment}.$$

Using the full sample estimates from 2008 to exemplify: $\Delta \widehat{employment} = 0.056$ (Table 9 Appendix

Figure 6: cDiD, percentage change in earnings



A), $\Delta \ln \widehat{earnings} = 0.177$ (Table 10 Appendix A) and the average employment rate ($\overline{employment_{2007}} = 0.643$).

$$0.177 = \frac{0.056}{0.643} + \Delta \ln wage + \Delta \ln hours$$

Then subtracting the increased earnings effect stemming from the increased probability of being employed

$$0.177 - 0.087 = \Delta \ln wage + \Delta \ln hours$$
$$0.09 = \Delta \ln wage + \Delta \ln hours.$$

Meaning that the treatment group on average increased their annual earnings by 9% in 2008 compared to those not taking treatment when accounting for employment effects. How much of this that is an effect of increased working hours rather than a wage increase is unfortunately not possible to determine with the available data but presumably the hours effect dominates. One has to keep this equation in mind when interpreting the plots.

6.3 Robustness check

To make sure the results does not hinge on the specific timing of treatment (2002) a type of robustness check were performed: All equations were re-estimated with 2004 as treatment year instead of 2002. Neither the iDiD nor the cDiD estimator showed sensitivity to the treatment timing. The overall pattern was unchanged in all estimations. Two changes are worth noting though; the unemployment estimates for the cDiD shifted down a few points, making the estimates significant at all post-treatment years. Lastly the earnings estimates shifted upwards, resulting in significantly positive estimates for all panels in the post-treatment years.

By this I conclude that the reported results most likely are robust since the pattern and the estimates hardly varied when switching treatment year.

7 Summary

This paper investigates how TWAs affect the subsequent labor market outcomes for unemployed in terms of employment status and income. The outcomes has been re-estimated on non-western immigrants and stratified on gender to control for heterogeneous effects. The selection bias associated with TWA studies has been tackled by individual Differences-in-Differences (iDiD) and conditional Differences-in-Differences (cDiD). The study was concluded with a robustness check were sensitivity to treatment year was tested. Both estimators were found to be robust.

The most solid result that can be drawn from the estimations is that joining a TWA (taking treatment) decreases the probability of getting a regular job (TWAs excluded) for years to come in general but not for non-western immigrants. Conversely the effect on overall employment (TWAs included) has a long-run positive effect when using the matched estimator (only 3 years with the iDiD). The estimates for unemployment are only marginally significant at times and fluctuate around zero when estimating an iDiD, but when matching the groups we found evidence of a positive transition rate out of unemployment. When stratifying on gender, women showed stronger and more persistent negative regular employment effects even though the other outcomes did not diverge much, suggesting that women tend to stay longer periods in TWA employment. When turning to the income estimations the treatment group seem to have gained a bit from the TWA in the long-run. Stratification on gender showed that the result is mainly driven by women.

The evidence provided in this thesis does not support the stepping-stone hypothesis since regular employment is negatively affected or not affected at all in the medium and long run. It might on the other hand work as a way to escape unemployment which especially if you are a women might benefit your future income (though not clear if it is an effect of increased working hours or wage). The TWAs also had a clear long-run effect on the employment probabilities. Compared to the unemployment estimates the employment estimates were larger and more stable over time, suggesting that the TWAs keeps individuals from exiting the labor market. This effect together with the increased long-run earnings effect puts the TWI in a quite favorable light. The biggest difference between the full sample and the non-western immigrants sample is in the regular employment outcome, where the latter group does not seem to ”get stuck” in the TWA to the same extent as the full sample. However both in the iDiD and in the cDiD standard errors are quite large and the lack of a negative regular employment effect might be just out of imprecision.

It should here be emphasized that the results are valid for people in unemployment, whether they are valid for a weaker subset (e.g. social assistance recipients) or a stronger subset (e.g.

students) is an open question. External validity is also affected by the matching process which distorts the sample's characteristics to some extent. In this case for instance, the results are first and foremost applicable to a younger subset of the population.

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A Appendix: Tables

Table 6: Summary statistics for 2001, non-western immigrants

	Group		Differences in mean	
	Control	**Treatment**	**Coefficient**	**T-statistic**
Gender				
Male	58.5	76.1	0.177	(5.73)***
Female	41.5	23.9	-0.177	(-5.73)***
Aggregate days in unemployment 1998-2001	**447**	**396**	**-50.898**	**(-2.87)***
0	0.8	0.5		
1-30	1.6	0.5		
31-90	6.6	9.0		
91-182	11.5	13.4		
183-274	11.2	13.9		
275-364	10.0	13.4		
365-730	41.4	37.8		
731-1094	16.3	11.4		
1095-1457	0.6	0.0		
Age groups	**36**	**32**	**-3.905**	**(-6.04)***
16-20	4.1	10.9	0.069	(3.11)***
21-25	10.9	19.4	0.085	(3.01)***
26-30	13.2	12.4	-0.008	(-0.32)
31-35	17.3	18.9	0.016	(0.56)
36-40	19.2	15.9	-0.033	(-1.26)
41-45	17.4	14.4	-0.030	(-1.18)
46-50	11.6	5.0	-0.066	(-4.14)***
51-55	6.3	3.0	-0.033	(-2.66)***
Country of birth				
Other European countries	32.5	31.8	-0.007	(-0.21)
Africa	10.4	10.4	0.001	(0.03)
South America	6.9	8.0	0.010	(0.53)
Asia	49.4	48.8	-0.007	(-0.18)
Soviet Union	0.7	1.0	0.003	(0.36)
Highest education level				
Primary school less than 9 years	10.7	5.5	-0.052	(-3.14)***
Primary school 9 (10) years	15.8	15.4	0.004	(-0.16)
Upper secondary 2 years or less	25.6	22.9	-0.027	(-0.90)
Upper secondary 2 years or more	23.7	30.8	0.071	(2.15)**
Higher education less than 3 years	9.4	10.4	0.010	(0.46)
Higher education 3 years or more	11.3	10.9	-0.003	(-0.14)
Post-graduate education	0.6	1.0	0.004	(0.60)
Unknown	2.9	3.0	0.001	(0.11)
Year of arrival				
1936-1965	0.4	0.0	-0.004	(-4.59)***
1966-1972	2.8	0.5	-0.023	(-4.20)***
1973-1985	19.9	14.9	-0.050	(-1.95)*
1986-1993	48.5	52.2	0.038	(1.06)
1994-2001	28.4	32.3	0.039	(1.17)
Resides in a large city				
Stockholm	20.3	32.3	0.121	(3.61)***
Gothenburg	22.4	28.9	0.065	(1.99)**
Malmoe	21.0	10.0	-0.111	(-5.06)***
Other	36.3	28.9	-0.075	(-2.29)**
Number of observations	5,427	201	5,628	

Robust standard errors
*** p<0.01, ** p<0.05, * p<0.1

Table 7: iDiD coefficient estimates corresponding to Fig. 4

VARIABLES	(1) Regular employment Full sample	(2) Unemployment Full sample	(3) Employment Full sample	(4) Regular employment non-western immigrants	(5) Unemployment non-western immigrants	(6) Employment non-western immigrants
Treatment × 1999	-0.009	0.017	-0.002	0.010	0.050	0.007
	(0.017)	(0.016)	(0.016)	(0.032)	(0.040)	(0.032)
Treatment × 2000	-0.006	-0.003	0.028	0.008	0.057	0.062
	(0.021)	(0.018)	(0.021)	(0.037)	(0.045)	(0.040)
Treatment × 2002	-0.461***	-0.153***	0.529***	-0.362***	-0.169***	0.623***
	(0.016)	(0.013)	(0.016)	(0.030)	(0.030)	(0.031)
Treatment × 2003	-0.184***	-0.008	0.156***	-0.144***	0.033	0.152***
	(0.022)	(0.016)	(0.022)	(0.043)	(0.038)	(0.046)
Treatment × 2004	-0.156***	0.045***	0.053**	-0.086**	0.077**	0.082*
	(0.022)	(0.017)	(0.022)	(0.044)	(0.039)	(0.045)
Treatment × 2005	-0.114***	0.055***	0.021	-0.070	0.063*	0.057
	(0.022)	(0.016)	(0.022)	(0.044)	(0.037)	(0.044)
Treatment × 2006	-0.087***	-0.037**	0.028	-0.036	-0.016	0.054
	(0.022)	(0.017)	(0.021)	(0.048)	(0.042)	(0.048)
Treatment × 2007	-0.060***	-0.047***	0.043**	-0.013	-0.053	0.084*
	(0.022)	(0.018)	(0.020)	(0.046)	(0.040)	(0.045)
Treatment × 2008	-0.046**	0.042***	0.030	-0.027	0.052	0.053
	(0.022)	(0.015)	(0.021)	(0.048)	(0.037)	(0.047)
Observations	261,110	261,110	261,110	56,280	56,280	56,280
R-squared	0.424	0.264	0.431	0.422	0.248	0.428
Control variables	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Control variables ($\mathbf{X}_{i,t}$): Dummies for labor market region, education level, educational orientation, marital status, age groups, social welfare benefit received current year.
Standard errors were clustered on individual level to account for possible serial-correlation.

Table 8: iDiD, stratified on gender

VARIABLES	(1) Regular employment Women	(2) Unemployment Women	(3) Employment Women	(4) Regular employment Men	(5) Unemployment Men	(6) Employment Men
Treatment × 1999	-0.031	0.031	-0.024	0.004	0.010	0.010
	(0.028)	(0.026)	(0.028)	(0.020)	(0.021)	(0.020)
Treatment × 2000	-0.033	-0.005	-0.002	0.006	-0.002	0.042
	(0.034)	(0.027)	(0.033)	(0.025)	(0.024)	(0.026)
Treatment × 2002	-0.502***	-0.113***	0.488***	-0.437***	-0.174***	0.554***
	(0.028)	(0.019)	(0.028)	(0.020)	(0.017)	(0.020)
Treatment × 2003	-0.200***	-0.002	0.166***	-0.176***	-0.010	0.148***
	(0.038)	(0.024)	(0.035)	(0.027)	(0.021)	(0.027)
Treatment × 2004	-0.177***	0.060**	0.060*	-0.145***	0.039*	0.048*
	(0.037)	(0.025)	(0.036)	(0.027)	(0.022)	(0.027)
Treatment × 2005	-0.124***	0.059**	0.022	-0.109***	0.056***	0.019
	(0.039)	(0.026)	(0.037)	(0.027)	(0.020)	(0.027)
Treatment × 2006	-0.110***	-0.055**	0.030	-0.075***	-0.023	0.027
	(0.038)	(0.027)	(0.035)	(0.027)	(0.022)	(0.026)
Treatment × 2007	-0.068*	-0.065**	0.059*	-0.056**	-0.032	0.034
	(0.037)	(0.027)	(0.033)	(0.027)	(0.023)	(0.026)
Treatment × 2008	-0.021	0.016	0.061*	-0.058**	0.060***	0.015
	(0.036)	(0.022)	(0.033)	(0.027)	(0.020)	(0.026)
Observations	111,591	111,591	111,591	149,519	149,519	149,519
R-squared	0.426	0.260	0.433	0.426	0.267	0.433
Control variables	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Control variables ($\mathbf{X}_{i,t}$)*:* Dummies for labor market region, education orientation level, marital status, age groups and social welfare benefit received current year.
Standard errors were clustered on individual level to account for possible serial-correlation.

Table 9: cDiD coefficient estimates corresponding to Fig. 5

VARIABLES	(1) Regular employment Full sample	(2) Unemployment Full sample	(3) Employment Full sample	(4) Regular employment Non-western immigrants	(5) Unemployment Non-western immigrants	(6) Employed Non-western immigrants
Treatment × 1999	-0.022	0.007	-0.004	-0.071	0.018	-0.069
	(0.024)	(0.019)	(0.024)	(0.056)	(0.051)	(0.057)
Treatment × 2000	-0.043*	0.003	-0.008	-0.059	0.094	-0.012
	(0.025)	(0.020)	(0.025)	(0.058)	(0.058)	(0.061)
Treatment × 2002	-0.439***	-0.172***	0.561***	-0.331***	-0.250***	0.667***
	(0.015)	(0.011)	(0.015)	(0.041)	(0.037)	(0.041)
Treatment × 2003	-0.143***	-0.053***	0.203***	-0.091	-0.043	0.198***
	(0.025)	(0.017)	(0.024)	(0.060)	(0.051)	(0.058)
Treatment × 2004	-0.097***	-0.038**	0.122***	-0.038	-0.054	0.139**
	(0.025)	(0.017)	(0.024)	(0.061)	(0.051)	(0.058)
Treatment × 2005	-0.063**	-0.011	0.073***	-0.036	-0.044	0.119**
	(0.025)	(0.016)	(0.024)	(0.061)	(0.048)	(0.058)
Treatment × 2006	-0.050**	-0.048**	0.068***	0.028	-0.097*	0.117**
	(0.025)	(0.020)	(0.023)	(0.061)	(0.057)	(0.058)
Treatment × 2007	-0.017	-0.060***	0.087***	0.017	-0.154***	0.146**
	(0.024)	(0.020)	(0.022)	(0.062)	(0.056)	(0.057)
Treatment × 2008	-0.020	0.004	0.052**	0.044	-0.103**	0.153***
	(0.024)	(0.014)	(0.022)	(0.062)	(0.045)	(0.058)
Observations	40,400	40,400	40,400	3,730	3,730	3,730
R-squared	0.126	0.056	0.140	0.182	0.101	0.230
Control variables	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Control variables ($\mathbf{X}_{i,t}$): Age, number of kids over age groups, and dummies for; region of birth, year of arrival, educational orientation and level, social welfare benefit received current year, and a linear time trend. For the non-western immigrants' sample region of birth was not included, but dummies for marital status where added.

Standard errors were clustered on individual level to account for possible serial-correlation.

Table 10: cDiD coefficient estimates corresponding to Fig. 6

Percentage change in earnings

VARIABLES	(1) Full sample	(2) Women	(3) Men	(4) Non-western immigrants
Treatment × 1999	-0.101	-0.135	-0.059	-0.844***
	(0.081)	(0.137)	(0.100)	(0.277)
Treatment × 2000	-0.028	-0.059	-0.009	-0.133
	(0.077)	(0.131)	(0.095)	(0.248)
Treatment × 2002	0.816***	0.954***	0.746***	1.158***
	(0.058)	(0.117)	(0.063)	(0.182)
Treatment × 2003	0.383***	0.467***	0.342***	0.568***
	(0.065)	(0.125)	(0.075)	(0.203)
Treatment × 2004	0.292***	0.481***	0.201**	0.317
	(0.075)	(0.130)	(0.088)	(0.193)
Treatment × 2005	0.119*	0.172	0.082	0.368*
	(0.072)	(0.125)	(0.087)	(0.207)
Treatment × 2006	0.074	0.041	0.078	0.161
	(0.068)	(0.132)	(0.075)	(0.177)
Treatment × 2007	0.256***	0.274**	0.228***	0.574***
	(0.061)	(0.111)	(0.072)	(0.190)
Treatment × 2008	0.177***	0.298***	0.091	0.364**
	(0.060)	(0.108)	(0.071)	(0.179)
Observations	28,891	7,431	21,460	2,443
R-squared	0.219	0.193	0.256	0.274
Control variables	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Control variables ($\mathbf{X}_{i,t}$)*:* Age, number of kids over age groups and dummies for; region of birth, year of arrival, educational orientation, social welfare benefit received current year and level and a linear time trend.

Standard errors were clustered on individual level to account for possible serial-correlation.

B Appendix: Figures

Figure 7: Kernel density plot over aggregate days in unemployment, 1998-2001

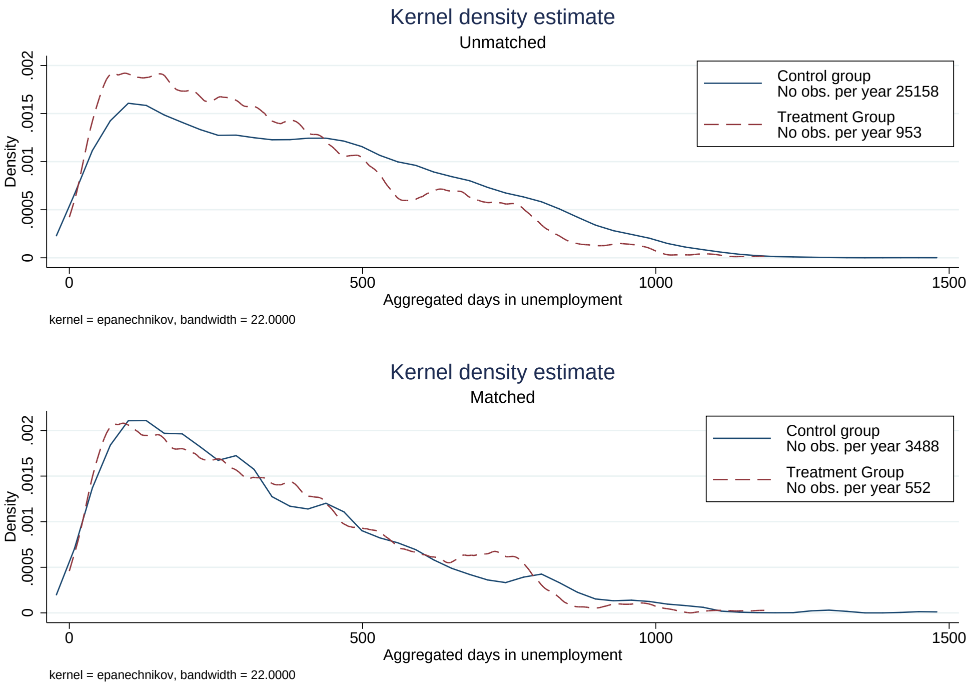


Figure 8: Histogram over age

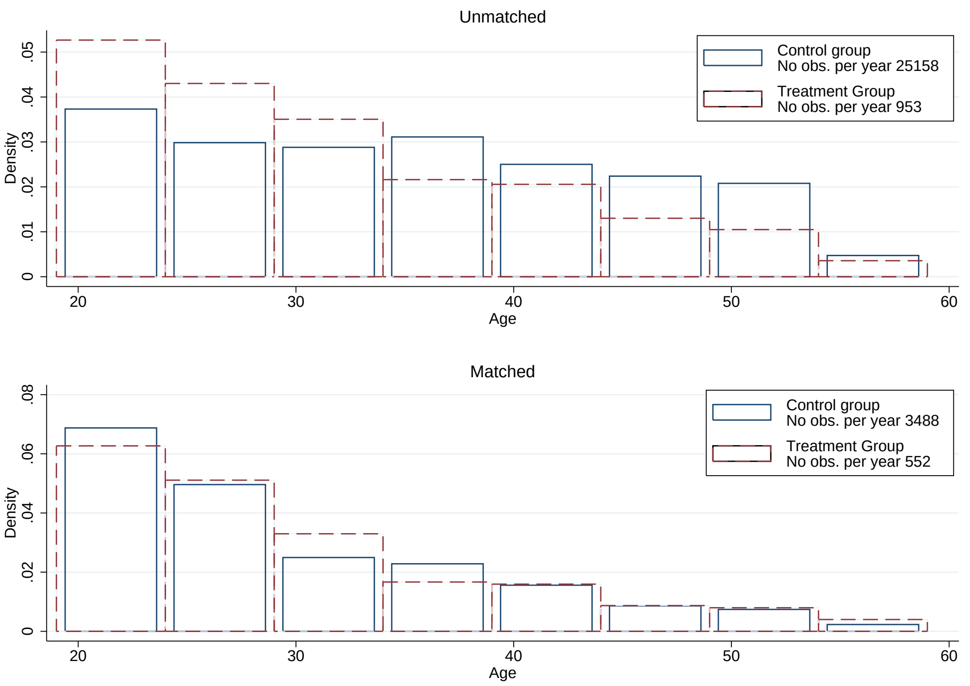


Figure 9: Employment status stratified on gender, matched DiD

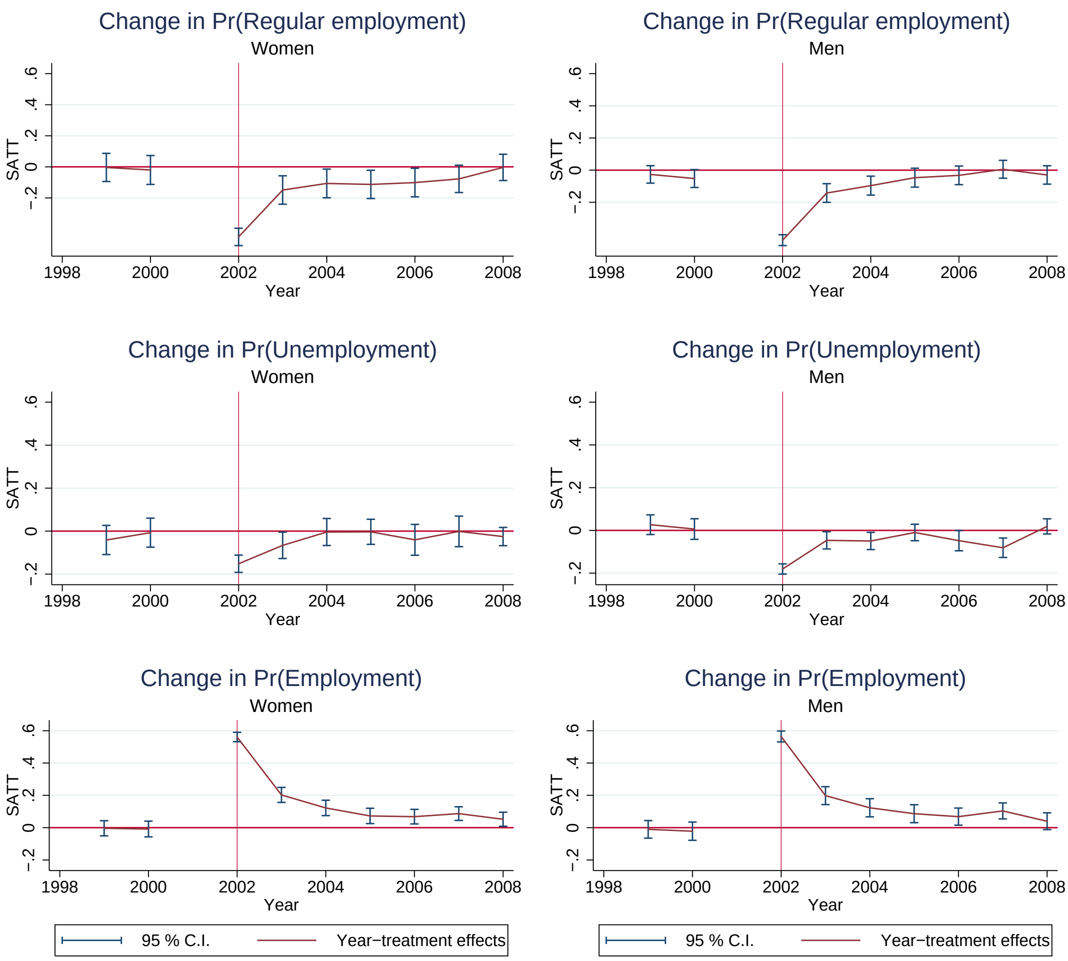
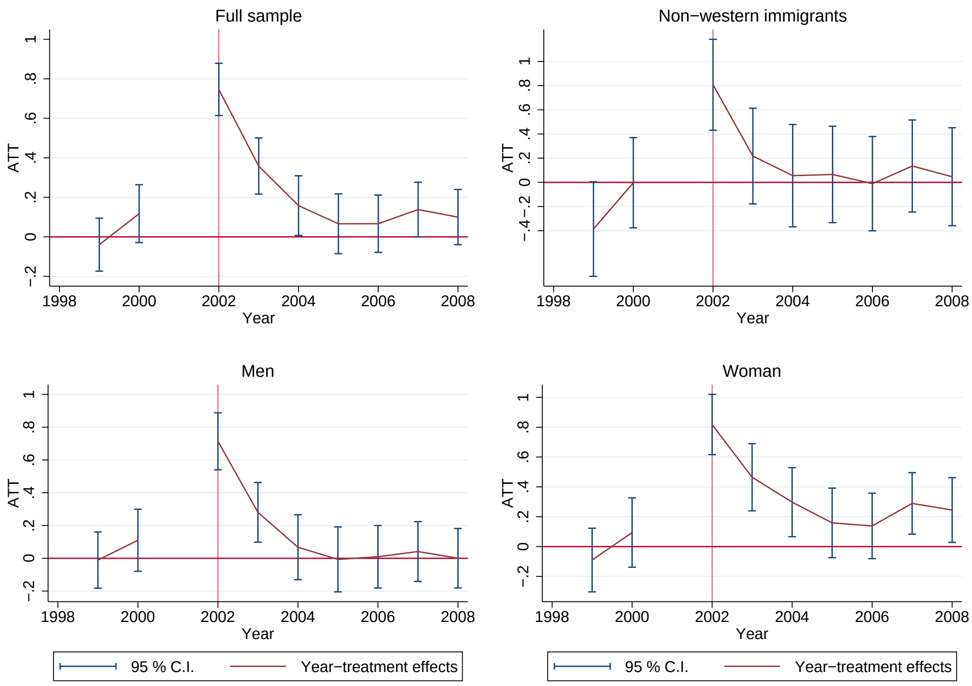


Figure 10: iDiD, percentage change in earnings



C Appendix: Description of outcome variables

Regular employment defined as all working in November in the employment register. The official definition of being employed in RAMS[25] closely tries to follow the ILO definition, meaning that if any income-generating labor has been performed during the week of measurement that is regarded as being employed (includes income from own business). In addition to this all TWA workers are excluded to define *regular* employment.

Unemployment defined as searching for job at the unemployment offices at the end of November including those registered in a labor market program.

TWA employment defined as working at a TWA in November. The TWA definition is number 78 in the SNI 2007.[26]

Total employment defined as regular employment but not excluding TWA employment.

Earnings defined as total gross annual reported income from work, recorded by the tax office.

[25]Register based labor market statistics
[26]SNI 2007 Standard for Swedish industrial classification 2007

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