Virtues of SIN—Effects of an immigrant workplace introduction program[*]

by

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Abstract

We evaluate an immigrant workplace introduction program aimed at helping individuals considered employable but at the same time expected to experience substantial difficulties in finding work. Using supported employment methods, the SIN program may influence outcomes through several channels. We use individual data and a difference-in-differences approach to estimate the effects of the program. The results suggest that the program increased transitions from unemployment to work experience schemes, and improved future employment probabilities for those who entered these schemes.

Keywords: unemployment, labor market programs, immigrants
JEL-codes: J61, J64, J68

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1   Introduction

The integration of immigrants in the labor market has caused Swedish politicians much agony in recent years. In an international perspective, Sweden stands out as one of the countries with the lowest labor force participation and the highest unemployment levels among the foreign-born (OECD 2005). In 2002, unemployment stood at 4 percent in the native population. At the same time it was 15 percent among those born outside Europe. Also, the chance of moving from unemployment to employment is substantially lower in many immigrant groups in comparison to natives (Åslund & Rooth 2005). It is sometimes argued that simple comparisons to other countries do not account for the fact that Sweden has received a comparatively large fraction of refugees, who generally have worse outcomes than labor migrants (see e.g. Integrationsverket 2006). Still, the fact that a large proportion of the foreign-born population has a disadvantaged labor market position remains.

The Swedish government has adopted a variety of policies and programs to combat the problematic situation. Measures range from special local introduction programs for the recently arrived, via priority regulations at the PES offices, to large-scale neighborhood development programs. This paper evaluates a program on trial since September 2003, which in some respects goes one step further in its interventions. Using "supported employment" methods previously employed for disabled workers, the SIN (Special INtroduction) program targets immigrants and refugees who are considered capable of taking a job immediately, but who are also (at risk of becoming) long-term unemployed.

Once enrolled in SIN, the job searcher is assigned to a special employment officer with considerably fewer clients at his/her hands than what is usual. The work is formally divided into six steps. The first step is to analyze the searcher's merits, potentials and preferences. Then "job gathering" commences, followed by so-called job analysis where it is investigated whether the conceived tasks or workplace need to be adapted in any way. The last three steps are: workplace introduction, employment, and follow-up. We will discuss the methodology further in section 2.

SIN is run in 20 municipalities, many of which are situated in the metropolitan areas of Sweden, where the bulk of the targeted population lives. Previous studies have found that participation is quite selective in terms of being "job ready", i.e. participants should be capable of managing a job and willing to make the move into employment (Hernemar 2004, Lindgren Åsbrink 2005).

These facts have implications for the design of this study of the effects of the program. First, despite the availability of very detailed population micro data on the unemployed, we are unlikely to capture all factors determining participation in SIN. Second, the implementation of the pilot opens up for a difference-in-differences analysis comparing the before-after change in SIN municipalities to other municipalities in the same local labor market. Naturally, this methodology also allows for the possibility that SIN may have affected unemployed non-participants. We will thus estimate the effects of SIN on the population at risk in the participating municipalities, i.e. we apply a reduced form estimator at the municipal level.

We consider two alternative outcomes—employment and open unemployment—and argue that the main effects of SIN may work through different channels: (i) it can affect the hazard from open unemployment to employment orto an intermediate treatment (IMT) (e.g. employment subsidies or work experience schemes); (ii) it may influence the flows back to open unemployment from employment or IMT:s; (iii) it may affect the transitions from IMT:s to employment. The third channel can consist of two parts: a change in the distribution of intermediate states combined with the impact differences between intermediate outcomes (in the absence of SIN), and, secondly, a change in the effects from entering a particular intermediate state on the final outcome.

To get at the mechanisms at work, we estimate several sets of Cox regression models, comparing before-after changes in the SIN locations to the same changes in the non-SIN locations. Our findings suggest that SIN increases the rate of transitions into work experience schemes. Also, entry to work experience schemes is associated with higher chances of becoming employed under SIN than otherwise. SIN does not appear to have affected the flows back to open unemployment from work or from IMT:s. The interpretation of a (statistically and economically) positive significant difference-in-differences estimate on the hazard from unemployment to work is complicated by the fact that analyzing a "fake reform" supposed to have occurred one year before the actual reform yields a similar estimate.

In the next section we give some detail on the design of the SIN program and its implementation. In section 3 we present the data, describe the SIN participants in terms of background characteristics, and sketch what happens when someone enters SIN. Section 4 first discusses some conceptual methodological issues and then outlines the empirical strategy. Section 5 presents the results on the effects of SIN. Section 6 concludes.

2 Program setup and implementation[1]

SIN was introduced in 20 Swedish municipalities on September 1, 2003. Initially it was scheduled to run until December 31, 2005, but the trial period was later extended to December 31, 2006.[2] It is administered by local PES offices which have been granted extra funding for the case workers working with the SIN participants. In 2005, the resources added amounted to a total of SEK 126 million, resulting in some 250 SIN officers (Ams 2006b). According to the December 2005 report from the labor market board, 4,781 individuals entered SIN during 2005 (Ams 2006a). The typical SIN officer appears to have somewhere between 15 and 30 clients. A cautious comparison to other PES officers suggests that this is about one tenth of the normal caseload. Note also that the funding only covers the SIN officers—resources for e.g. employment subsidies or work experience schemes are taken from the regular budget.

2.1 Targeted groups

According to the government bill (Förordning 2003:623), the program may be offered to immigrants or refugees age 20 or above. The individual can be in SIN for a maximum of six months. Under (unspecified) extraordinary circumstances, however, the period can be prolonged. The participants should be reckoned capable of taking a job immediately, but also (at risk of becoming) long-term unemployed. SIN may be granted people who fulfill only the first criterion, but are in (or have completed) a local refugee introduction program. In other words, the rules for program eligibility are very loose since it suffices to (i) be at risk of becoming long-term unemployed, or (ii) to have completed a local introduction program any period of time ago. We will present characteristics of the participants in section 3.2.

Normally, SIN should not be chosen before testing other alternatives (i.e. standard job search assistance, other labor market programs). To assign an in-

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[1] The section draws primarily on Ams (2004a,b)
[2] During 2005, four municipalities were effectively added to the SIN trials. The participating municipality of Eskilstuna then started serving also Flen, Katrineholm, Nyköping and Vingåker. These are small towns compared to most of the other participating locations. We will attend to the expansion in the robustness checks.

dividual to the program, the case worker should have estimated the need for support to be larger than the need for qualified job-matching assistance. The SIN officers are not to be considered just an extra resource for job matching.

2.2 The methods of SIN—in theory

SIN is based on so-called "supported employment", which have been developed for, and previously used in, programs attending to disabled workers (see Antonsson (2003) or Leach (2002) for discussions). After having been judged suitable for SIN, the job searcher is assigned to a SIN officer, and then the process is divided into six steps:

1. Job searcher analysis
2. Job gathering
3. Work analysis
4. Workplace introduction
5. Follow-up
6. Employment

The first step is the "job searcher analysis", in which the case worker interviews and maps the individual's merits, potentials and wishes. If the individual does not have a CV or validated credentials, he or she should be remitted to the regular PES for this purpose. The second step of the process is "job gathering", when the case worker looks for suitable work. The case worker should then inform the prospective employers that the aim of the program is employment, even though the workplace introduction can begin with a trainee position. However, there should be a promise of future employment for a particular workplace to come into question for placement.

Work analysis is the third step, in which the SIN officer investigates whether tasks and work environment suits the participant. If necessary, the officer can discuss possible changes in the tasks with the employer. In this step it is also to be made clear which type of support the officer can give during the workplace introduction. Then, the workplace introduction begins. The introduction is to be performed in close cooperation between the SIN participant, the officer, the employer, colleagues, and union representatives. The officer's presence at the workplace is supposed to facilitate the participant getting started with the job tasks, and to, e.g., help overcoming language barriers or making sure that the participant becomes a part of the workplace community.

When the SIN period is over (usually after six months), the officer performs a follow-up of the assignment. This is considered particularly important since

employment within the program is often on a temporary basis, and there is a wish to make sure that the individual actually gets hired.

2.3   SIN in practice

Some work on SIN has been done previously. First, there are the reports from the labor market board to the central government (e.g. Ams 2005). These generally carry a positive notion of SIN, where it is argued that even though the measure does not in all cases end in employment, the outcomes are good compared to other programs. One thing worth noting is that SIN officers had to be recruited externally in about 70 percent of the cases. According to the labor market board, this meant that SIN was not fully up and running until the fall of 2004 (Ams 2005). We will return to this issue below.

There are also two studies which have performed interviews with participants, officers and (in one study) employers (Hernemar 2005, Lindgren Åsbrink 2006). The overall impression from these interviews is that participation is quite selective. The officers stress the importance of being "job ready". This means having sufficient Swedish language skills, not being in need of any type of rehabilitation and being willing to commute or relocate if necessary. According to these two studies (and to the Ams (2005) report), the main benefit of the program is that the officers have considerably more time with each client, increasing the potential for a successful match. The interviews suggest that the workplace introduction and follow-up have played a smaller part in the implementation. In practice, the officers do thus not appear to stick to the supported employment methodology.

The interviewed employers express the feeling that they can trust that the individuals coming from SIN are really right for the job. Some employers state that it may be easier handling minor conflicts via the officer than with the employee directly. Some officers, however, express the view that it would be very stigmatizing with a strong case worker presence during the introduction, since this would signal low capability of the applicant.[3]

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[3] In fact, the point that supported employment methodology may be stigmatizing and not transferable to the case of immigrants was made at a very early planning stage (see Carling 2003).

3 Data and descriptive statistics

3.1 The data

We use data from the IFAU database covering the entire Swedish population age 16–65. The data contain detailed individual information on registered unemployment spells up to November 15, 2005. The information includes date of unemployment entry, whether the individual is in open unemployment or in some type of program (and if so, which program) at any point in time during registered unemployment, and date of and reason for leaving unemployment (e.g. finding a job, regular education, leaving the work force etc.).

We include individuals who entered open unemployment between January 1, 2000 and November 15, 2005, and who were at least 20 and less than 63 years old at the time of unemployment entry. An individual may have multiple spells. In the baseline analysis, we include individuals who were born outside the Nordic countries. We will, however, discuss robustness checks with varying restrictions on region of birth. There is also a restriction implying that only those who were registered at a PES located in the local labor market of (at least) one of the SIN-participating municipalities are included. This is because we want the comparison group to be subject to the local shocks experienced by the treatment group. Local labor markets are defined by Statistics Sweden based on observed commuting behavior. A local labor market consists of one or (usually) several municipalities. No municipality belongs to more than one local labor market. We use the 2003 definition of local labor market regions.

The analysis focuses on two final outcomes: employment and open unemployment. People are followed for the duration of open unemployment. Then, we register four types of transitions: (i) to (regular) employment (permanent or temporary); (ii) to subsidized employment; (iii) to work experience schemes; (iv) to "other" categories. The fourth type of exit contains those who have deregistered for "unknown" reasons (which is employment in about 50 percent of the cases (see Bring & Carling 2000, Sahin 2003, Forslund et al. 2004), those who have left for regular education or labor market training, and those who become enrolled in other types of labor market programs than the ones covered by (ii) and (iii). We label exit types (ii), (iii) and (iv) intermediate treatments (IMT:s).

Similarly, we monitor transitions from employment or IMT:s back to open unemployment, and from IMT:s to employment. Employment is defined as ei-

ther being registered at the PES in a category indicating regular employment (temporary, part-time, looking for other work), or having "found work" as the latest reason for deregistration.[4]

Table 1 displays some basic facts about the data. There are just below half a million unemployment spells and about 222,000 individuals in the data[5]. The median spell of open unemployment lasts 112 days. On average, each spell was preceded by a total of 247 days in labor market programs and 641 days of open unemployment[6]. The SIN municipalities have "worse" track records: spells last a bit longer and the unemployed have spent more time in unemployment and labor market programs. Note that "SIN" in this context means being registered in a participating municipality. The actual SIN participants (who are a small portion of the total number of individuals) are described in the next section.

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[4] People who find temporary, part-time, or not fully satisfactory work may remain registered at the PES.
[5] Note the difference to *Table 1* which displays the number of individuals conditional on time period and location.
[6] The starting year is 1991.

Table 1 Basic data description

Municipality		Time period (relative to Sep. 1, 2003)		
		Before	After	Total
Non-SIN	# observations	82,921	58,943	141,864
(comparison)	# individuals	48,542	42,771	91,313
	Median spell length	105	103	104
	Average time in LMP	236	216	228
	Average time in open unempl.	582	557	572
SIN	# observations	203,266	142,064	345,330
(treatment)	# individuals	114,056	100,708	214,764
	Median spell length	121	107	115
	Average time in LMP	264	241	254
	Average time in open unempl.	683	651	670
Total	# observations	286,187	201,007	487,194
	# individuals	162,598	143,479	306,077
	Median spell length	116	106	112
	Average time in LMP	256	234	247
	Average time in open unempl.	654	623	641

Notes: SIN municipalities are the ones that participate in the trials starting September 1, 2003. "Spell length" is for completed spells in open unemployment (in days), "time in LMP (unempl.)" is days spent in labor market programs (open unemployment) since August 1991 prior to the start of the current spell. Note that average spell length is longer in the before period, which is partly a result of longer time until censoring on November 15, 2005. The # individuals are conditional on location and time period; an individual may be counted in multiple cells (first spell in each period and location). The total number of individuals observed is 222,269.

3.2 Descriptive statistics

3.2.1 Who participates in SIN?

Let us now describe the SIN participants. *Table 2* below shows that most of the participants were born in Asia, Africa or in European countries outside EU15. Iraq, Iran and former Yugoslavia are the most common countries of birth according to Ams (2005). Note, however, that the numbers are conditional on being born outside the Nordic countries. As a matter of fact, about 5 percent of the participants were born in Sweden. All in all, there are about 7,000 SIN par-

ticipants in the data. The participation rate is between 6 and 7 percent (based on the unemployment inflow in 2003-2004 in the SIN municipalities). With such a low probability of participation, the possibility that people relocate to become eligible for SIN should not be a serious concern for the evaluation. Nevertheless, we performed an analysis of relocations into and out of the (non-)SIN municipalities. The results did not suggest that SIN changed the mobility patterns in any substantial way.

Some of the regulations governing SIN signal that the measure is at least partly intended for recently arrived. It is therefore interesting to find that while 30 percent of the participants have immigrated since the year 2000, approximately half of the sample actually came before 1995.

We also see that there are somewhat more males among the participants, and that the three education categories (primary, secondary, tertiary) each contain roughly one third of the participants. Somewhat noteworthy is the fact that only about one quarter of the people in SIN has an updated plan for action according to the administrative records.[7] Since the SIN methodology stipulates quite extensive considerations of different alternatives available, this seems to be an administrative flaw in the sense that all information is not entered into the registers. The table also shows that 30 percent of the participants are not eligible for any type of unemployment benefits. Typically, these individuals are supported by social assistance.

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[7] Updated means that the plan was created or changed in the calendar year of the start of the unemployment spell linked to SIN.

Table 2 Description of the SIN participants born outside the Nordic countries.

Region of birth	
EU15	2.91
Europe except EU15	27.88
Africa	18.77
North America	1.59
South America	5.55
Asia	42.22
Oceania	0.15
Missing	0.93
Immigration period	
2004–2005	3.93
2000–2003	25.96
1995–1999	19.73
1990–1994	27.76
1985–1989	13.38
1980–1984	4.87
–1979	4.14
unknown	0.23
Male	56.6
Age at unemployment entry	36.7 (9.0)
Level of education	
Primary	31.9
Secondary	36.3
Tertiary	31.8
Fraction with updated action plan	24.6
Coded as not eligible for benefits	30.53
Registered in SIN municipality	97.2
# observations	7,292

Notes: Information on SIN participation and unemployment is taken from separate registers. The dates of SIN start and end do not necessarily comply with the timing of unemployment spells. Time varying variables are therefore measured for the unemployment spell linked to SIN participation according to the following hierarchical criteria: (i) SIN start date is within the spell; (ii) the spell ending at the shortest time before the SIN start date; (iii) the spell beginning at the shortest time after the SIN start date.

Recall that SIN is supposed to target individuals who are believed to have substantial difficulties finding work. Some regulations hint that it may be peo-

ple who have limited labor market experience in Sweden due to short residence (which is true in about a quarter of the cases). Also, SIN should be considered the best alternative and not be chosen before testing other alternatives. Remembering the low employment rates in some groups of foreign-born, one might wonder how the SIN participants compare to other unemployed with similar background. *Table 3* shows characteristics of individuals entering open unemployment during 2003 and 2004, by eventual observation in the SIN program. The non-participants have been re-weighted to conform to the region-of-origin/period-of-immigration distribution of the participants. In other words, the comparison controls for regional background and time spent in Sweden.

The distribution of education is very similar across the two groups—if anything SIN participants are positively selected. The participants have spent considerably more time in open unemployment and labor market programs, which is not surprising given that one can qualify for SIN by being long-term unemployed. What is perhaps more surprising is that the participants are much less likely to be coded "not eligible for any type of benefit". Even though this variable taken from an administrative register does not necessarily reflect actual unemployment benefits (UB) eligibility at a given point in time,[8] the pattern signals that SIN participants may not have less attachment to the labor market than other unemployed with similar backgrounds. This conclusion is also supported by the fact that the 2002 employment and earnings statistics are very similar across the groups. Even though the outcomes are poor on average, there is among the participants clearly a substantial fraction with experience from the Swedish labor market.[9] One could ask why individuals who may have spent more than 20 years in Sweden and who were employed as late as 2002 are in need of a workplace introduction program partly intended to bridge linguistic and cultural gaps.

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[8] The variable is based on which UB fund the individual belongs to. Those who do not belong to any fund or whose benefits have expired should be coded "00" which is what the table entry is based on. However, one can not be certain that this information is updated if, e.g., benefits expire during an unemployment spell.

[9] Not surprisingly, employment rates increase with time spent in Sweden. 47 percent of participants arriving 1980-84 were employed; for those who came earlier the figure was 53 percent.

Table 3 Comparison of SIN participants and non-participants. Weighted by region of origin and immigration period

	SIN participants	Non-participants
Primary education (%)	32.3	33.9
Secondary education (%)	36.2	36.2
Tertiary education (%)	31.4	29.7
Coded as not eligible for benefits (%)	32.4	42.5
Average time in open unempl.	669.6	560.2
Average time in LMP	291.7	213.2
Employment 2002	35.6	36.1
Average earnings 2002	59,200	58,350

Notes: Table presents averages of variables for individuals who entered open unemployment 2003–2004, by SIN participation before Nov 15, 2005. Non-participants have been re-weighted to conform to the region-of-origin/period-of-immigration distribution of the SIN participants. There is a very small number of observations without information on education; this category is excluded from the table and thus the numbers do not add to 100. Measures of employment and average earnings are limited to individuals observed in 2002.

3.3 What happens when someone enters SIN?

Table 4 shows the status of the SIN participants 3 days before the start date of SIN. 70 percent are in open unemployment, whereas about 10 percent are classified as employed. Few are in subsidized employment and some are in work experience schemes. A significant portion is also found in the category "other". Further examination reveals that about half of these individuals are in "matching and guiding activities", one quarter is in preparatory education and about 15 percent is in labor market training.

Table 4 Status 3 days before SIN entry

Status	Freq.	Percent
Unemployed	5,133	70.39
Employed	788	10.81
Subsidized employment	54	0.74
Work experience	340	4.66
Other	977	13.40
Total	7,292	100.00

Notes: 3 observations dropped due to SIN start scheduled after November 15, 2005.

Table 5 then shows the status of the participants on the starting day of SIN, and 3, 14 and 28 days later, in total and by status 3 days before SIN start. Obviously, many individuals leave open unemployment for work experience schemes or regular or subsidized employment. There appears to be transitions just around the SIN start, and then a gradual shift during the first weeks of SIN. After four weeks, 36 percent is in regular employment, 20 percent in subsidized employment and about 23 percent of the participants are in work experience schemes. The latter share is substantially higher just after entry to SIN, suggesting that some participants go via work experience to some form of employment. The share in open unemployment is 11 percent after 28 days in SIN. Of those in the "other" category, about one third is no longer registered at the PES. Among those who are, 38 percent are in "matching and guidance activities", 16 percent are in some form of training, 14 percent are in programs primarily intended for those with (occupational) disabilities, such as wage subsidies or sheltered employment.

There are also movements between the other states. Only half of those who were in a work experience scheme three days prior to SIN remain there on the day of SIN entry. The bulk of these transitions are to some form of employment. 28 days after entry, 35 (24) percent of those who started in work experience are in subsidized (regular) employment.

Table 5 Status on and after SIN entry, by status 3 days before entry

3 days before	Unempl.	Employed	Subs empl.	Work exp	Other
	Day of entry				
Unemployed	16.15	22.09	13.66	40.04	8.07
Employed	0.25	70.43	11.93	14.85	2.54
Subs. empl	0.00	5.56	88.89	1.85	3.70
Work exp	4.41	18.24	27.35	47.35	2.65
Other	5.22	15.35	19.55	22.31	37.56
Total	12.30	26.11	15.46	35.00	11.14
	3 days after entry				
Unemployed	11.57	26.42	14.11	39.63	8.26
Employed	0.76	69.29	12.56	14.59	2.79
Subs. empl.	1.85	5.56	87.04	1.85	3.70
Work exp	4.41	19.71	27.65	45.59	2.65
Other	3.99	19.75	20.57	23.54	32.14
Total	8.98	29.69	15.98	34.77	10.57
	14 days after entry				
Unemployed	11.12	31.05	15.89	33.84	8.09
Employed	2.17	69.68	13.38	12.23	2.55
Subs empl	0.00	7.41	87.04	1.85	3.70
Work exp	5.29	23.24	31.76	36.76	2.94
Other	3.79	23.87	23.26	21.11	27.97
Total	8.82	33.71	17.88	29.70	9.89
	28 days after entry				
Unemployed	13.51	34.57	18.14	26.47	7.31
Employed	3.88	69.73	14.23	9.57	2.59
Subs empl	3.77	9.43	83.02	1.89	1.89
Work exp	8.33	24.40	35.71	27.68	3.87
Other	5.83	25.03	25.66	17.29	26.19
Total	11.13	36.46	20.03	23.28	9.10

The conclusions from this description are the following. First, it is clear that SIN usually means entry into some form of treatment/outcome, i.e. regular or subsidized employment or work experience schemes. Second, the patterns signal that the administrative start dates of SIN most likely do not mark the actual start dates in many cases. Remember that the first two steps of the SIN methodology are to map out the participants capabilities as well as wishes and to find a suitable workplace. We would therefore not expect to see so many transi-

tions into categories indicating being at a workplace around the actual SIN start date. Thus, the SIN start dates must be considered inaccurate in many cases. As a matter of fact, the same is true for the SIN end dates, since these typically are set at the same time as the start dates (many times resulting in a 180-day period). Note however, that the empirical analysis presented below captures a reduced form effect of SIN, which is not based on observations of individual SIN participation.

4 Methodological considerations

We begin this section by outlining the conceptual framework for the effects of SIN, focusing on the different components entailed in the SIN program. Then we discuss how to retrieve these estimates in practice and which identifying assumptions we need to make.
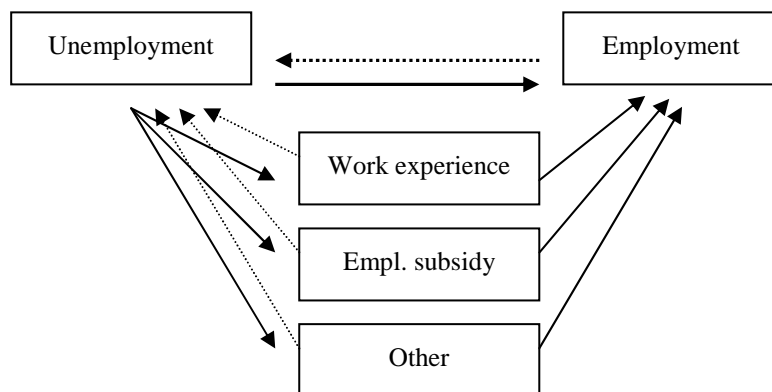
4.1 Conceptual framework

Let us first discuss how SIN may affect the outcomes of interest, here chosen to be employment or alternatively open unemployment. *Figure 1* illustrates how we think about the potential mechanisms at work. All individuals under study start out in open unemployment. Consider first the potential effects going from this state to other states. SIN may affect the individual's probability to move directly from open unemployment to employment. Secondly, it may alter the chances to enter intermediate treatments (IMT:s): work experience, employment subsidies and the summary category other (which includes e.g. labor market training and guidance activities at the PES).

The intermediate treatments may then in turn affect the future labor market opportunities among the unemployed. A change in the distribution of IMT:s towards treatments that improve the chances of entering employment is one potential effect. SIN may also influence the likelihood of moving from a given IMT to employment.

There may also be similar impacts on the flows in the other directions (dashed arrows in the figure): to open unemployment from employment or

from any of the IMT:s respectively. Of course, one could argue that SIN would influence flows between the different IMT:s or the probability to move from employment to an IMT. We argue, however, that the flows described above are the ones of primary interest.[10]

Figure 1 Conceptual framework of the effects of SIN

Following this description, the evaluation of SIN can be split into four steps. First we estimate how SIN affects the hazard (or probability) to employment and to three different IM treatments (employment subsidy, work experience and all other types of activities within the PES). Second, we estimate the hazard back to open unemployment from employment and the three different IMT:s. Third, we consider the impact of SIN on employment after entering any IMT (regardless of which). Fourth, for each IMT we estimate the hazard to employment, i.e. an effect conditional on entering a particular IMT. In the next section we will discuss how to identify the parameters of interest.

4.2   Identification and estimation

If individuals were selected randomly to participate in SIN from the group of eligible individuals, then we could simply estimate the effects using Cox regression models comparing participants to non-participants. The basic estima-

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[10] A sensitivity analysis suggests that SIN did not affect the excluded types of transitions.

tion problem is that we do not have random selection into SIN. The institutional setting suggests that most likely there is substantial selection into SIN at the individual level. We will therefore not base our estimates on individual SIN participation. Instead we will use difference-in-differences (DiD) estimators at the municipal level. Our estimates are therefore the reduced forms effects of entering unemployment in a SIN municipality.

The reason why we do not use a simple before-after estimator (comparing the flows in the SIN municipalities before 1 September 2003 with the flows after) is that there could be changes in macroeconomic conditions that affect the labor market that has nothing to do with SIN. The DiD approach is an attempt to handle this problem by controlling for macroeconomic changes common to all locations in the same labor market area. Needless to say, the identifying assumption is that the development over time in the reference locations capture the counterfactual development in the SIN locations, had SIN not been implemented. We will pay attention to the plausibility of the DiD strategy in the presentation of the results.

4.2.1 DiD estimation

In the DiD estimations, the Cox regressions employed are stratified on municipality. In other words, the estimations include fixed municipal effects. The estimations are performed with the partial maximum likelihood estimator.

Let $Z = 0, 1, 2, 3$ denote employment, employment subsidy, workplace practice and all other intermediate treatments (except open unemployment). The hazard to leave unemployment for $Z = 0, 1, 2, 3$ is then modeled as

$$\lambda^e_i(t,S) = h_0(t,m)\exp(\delta_{z1}D_i(t) + \delta_{z2}S_iD_i(t)), \quad z = 0,...,3 \qquad (1)$$

where $D(t)$ is an indicator variable that is equal to 1 when SIN is in operation, i.e. $D(t) = I(T > Sep.1\ 2003)$, and $h_0(t,m)$, $m = 1,...,M$ is the baseline hazard to $Z = 0, 1, 2$ or 3 for the eligible unemployed in municipal m. The indicator variable S is equal to 1 if the observation is made in a SIN municipality, 0

otherwise. Thus, δ_{z2} measures the effects of SIN on transitions to employment ($z = 0$) or to IMT ($z = 1, 2, 3$).[11]

In the next step we estimate the effects of SIN on the hazard back to open unemployment:

$$\lambda^u_{\ i}(t,S) = h_0^{\ u}(t,m)\exp\left\{\begin{array}{l}\alpha_1 D_i(t) + \alpha_2 S_i D_i(t) + \sum^3_{z=1}\alpha_{z1}(Z_i(t) = z) + \\ \sum^3_{z=1}\alpha_{z2}(Z_i(t) = z)D_i(t) + \sum^3_{z=1}\alpha_{z3}(Z_i(t) = z)S_i + \\ \sum^3_{z=1}\alpha_{z4}(Z_i(t) = z)D_i(t)S_i\end{array}\right\}$$

Here $h_0^{\ u}(t,m)$ is the baseline hazard to unemployment for those employed and in IMT, $z =1, 2, 3$ in municipality m. Thus, α_2 is the effect of SIN on the hazard to unemployment from employment, and $\alpha_{z4}, z = 1,2,3$ estimates the effects within a particular IMT on the transition to open unemployment.

Finally we also estimate the effect of IMT:s on employment

$$\lambda^j_{\ i}(t,S) = h_0^{\ j}(t,m)\left\{\begin{array}{l}\exp(\beta_1 D_i(t) + \beta_2 S_i D_i(t) + \sum^3_{z=2}\beta_{z1}(Z_i(t) = z) + \\ \sum^3_{z=2}\beta_{z2}(Z_i(t) = z)D_i(t) + \sum^3_{z=2}\beta_{z3}(Z_i(t) = z)S_i + \\ \sum^3_{z=2}\beta_{z4}(Z_i(t) = z)D_i(t)S_i\end{array}\right\}$$

Here $h_0^{\ j}(t,m)$ is the baseline hazard to employment for those in IMT, $z =1, 2$ 3 in municipal m. Thus, β_2 captures the effect on the hazard to employment from employment subsidies within SIN and $\beta_{z4}, z = 2,3$ estimates the effects

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[11] Note that the coefficient on S cannot be identified in the stratified regressions.

of SIN operating through work experience and the "other" IMT respectively. Naturally, we can also estimate the model with the different IMT:s counted as one state.

A word of caution regarding how to interpret the different sets of estimates is warranted. The general problem is that there may be heterogeneous treatment effects in the population at risk. Suppose that SIN increases the hazard to employment, and that we see an increased risk of moving back to open unemployment from employment under SIN. Since more people entered employment, the increased flows back to unemployment can be an effect of sample selection, i.e. the ones observed in employment under SIN are different from the ones in employment under the non-SIN regime. A similar case can be made for transitions between the IMT:s and employment.

Of course, altered selection into employment and different IMT:s may be at work even if we would not see any change in the probabilities of entering a particular state. One effect of SIN may simply be to reallocate individuals between different states. At the aggregated municipal level, this is an effect of major interest: does SIN improve the matching between individuals, employers and IMT:s? A drawback is that sample selection may hide individual within IMT effects, which are of course also of interest. We will return to the discussion on the impact of sample selection in the presentation of the results.

5   Empirical results

We begin the presentation of the findings by a very simple description of the probability of being in work 360 days after entering unemployment in the SIN and non-SIN locations by time of entry into unemployment. This type of analysis gives a notion of the total reduced form effects of SIN. We then try to shed some light on different possible mechanisms. First, we study the flows from unemployment to employment or IMT:s. Then we consider the transitions back to unemployment, and finally turn to the impact of SIN on going from IMT:s to employment.
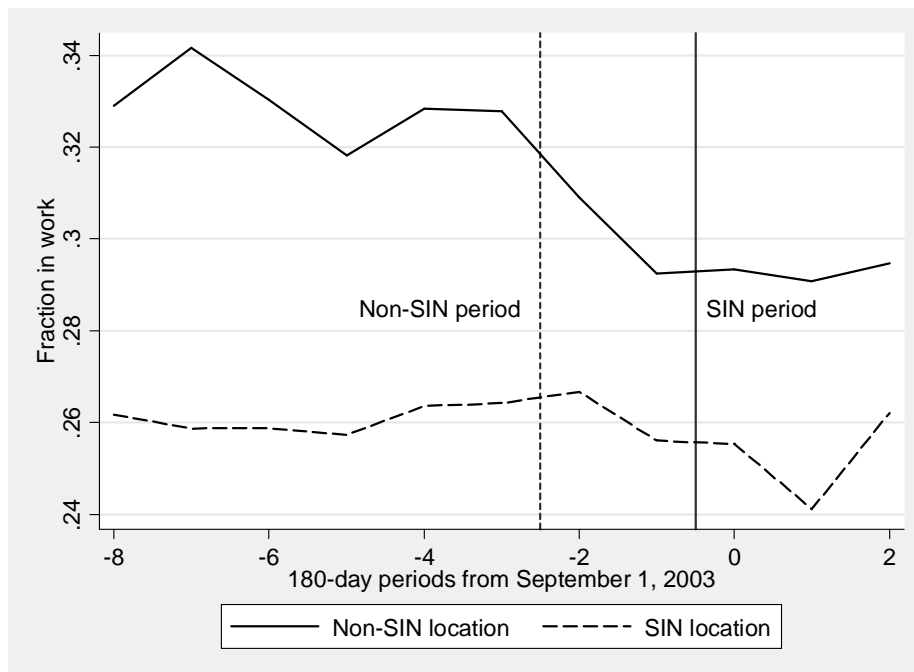
5.1   A first look at the effects of SIN

Figure 2 shows the probability of being in work 360 days after unemployment entry by location and 180-day period relative to September 1, 2003 (period "0" begins on this day). Observations to the left (right) of the left (right) vertical

line are made before (after) the implementation of the SIN program. Observations in between the lines may be partly affected by the SIN program.

In the beginning of the observation period, the probability of being in work is much higher in the non-SIN locations. The gap then narrows as the probability decreases more in the non-SIN locations compared to the participating municipalities. In this figure it is hard to see any clear effects of SIN by comparing the developments after the implementation of the program. There is a dip and then a recovery in the SIN locations. A very benevolent interpretation is that this recovery resulted from the SIN program. However, many of the comparison locations exhibit similar dips and recoveries.

The fact that the development over time differs across types of location is potentially troubling for the DiD approach. We have tried restricting the comparison locations to ones with a development before the SIN program similar to the one in the SIN location. However, such an approach is quite arbitrary compared to the one based on official classifications of local labor markets. Furthermore, the decline in the before period tends to be larger even with the restricted comparisons.

Our strategy in the formal analysis below will be to first perform DiD estimations of the effects of the actual reform. Then, we analyze an imaginary reform supposed to have occurred one year before. In case the imaginary reform yields results that are qualitatively similar to those following the actual reform, we are less inclined to believe in the baseline estimations. In case we see no "effect" in the imaginary analysis, we find the baseline estimates more plausible.

Figure 2 Probability of being in work 360 days after unemployment entry, by location and 180-day period of entry.

Notes: Inflow in period "0" begins on September 1, 2003. The nth periods begin +/– n*180 days before/after this date. Observations to the left (right) of the left (right) vertical line are made before (after) the implementation of the SIN program. Observations in between the lines may be partly affected by the SIN program.

5.2 The impact through different mechanisms

We now turn to present the results regarding the different mechanisms potentially at work. Throughout the presentation of the baseline results, we will discuss the estimates as showing the effects of SIN on the outcomes (i.e. assume that the DiD setup using the basic population restrictions described above retrieves the causal estimates). These assumptions will then be questioned and discussed in the sensitivity analysis of section 5.3.

Figure 3 presents the hazards from unemployment to employment, before and after the SIN reform in the SIN and the comparison locations respectively. The figure tells us three things: (i) the hazard rates are lower in the SIN municipalities both before and after the reform; (ii) the hazards are lower after the reform compared to before; (iii) the decrease between the two time periods ap-

pears to be smaller in the SIN municipalities. This is also the message of the second row of *Table 6*, which displays the estimated effect of SIN on the hazards from open unemployment to employment. According to the DiD setup, there is a positive and significant effect on the outflow: the hazard rate increases by about 12 percent. The table also shows that transitions to all of the three IMT:s were positively affected by SIN. The impact on employment subsidies and "other" is quite moderate, but there is a substantial effect on the probability of entering work experience schemes. *Figure 4* shows that what actually occurred was that a difference present before the SIN reform was eliminated in the post-SIN period. *Figure 5* shows that very little happened to transitions into employment subsidies, whereas *Figure 6* shows a similar decline in the transitions to "other" IMT:s in the SIN and the comparison locations.

Table 6 Effects on transitions from unemployment to employment or intermediate treatments

		Haz. Ratio	Std. Err.	z	P>|z|
Employment	D	0.813	0.007	-24.070	0.000
	SD	1.124	0.012	11.240	0.000
Employment subsidy	D	0.861	0.028	-4.620	0.000
	SD	1.056	0.040	1.430	0.153
Work exp.	D	0.763	0.018	-11.160	0.000
	SD	1.140	0.032	4.580	0.000
Other	D	0.841	0.007	-20.540	0.000
	SD	1.048	0.010	4.740	0.000

Notes: Table shows parameter estimates from Cox hazard regression estimation using the stratified partial maximum likelihood estimator. D measures the general before-after effect; SD is the DiD estimate (the difference in the before-after estimate between the SIN and the non-SIN locations).

The increased outflows from unemployment under SIN do not appear to have increased the flows back to unemployment, as shown by *Table 7*. The only statistically significant differences-in-differences estimate is for employment subsidies. Since there was no significant impact on the flows to this IMT, the somewhat increased return transitions may be of less importance. In terms of levels, *Figure 7* and *Figure 8* show that work experience is the IMT with the

highest hazards back to unemployment, at least during the first 200 days. In general, the patterns are relatively similar in treatment and control locations.

Let us now turn to transitions to employment among those who enter IMT:s. A potential effect of SIN is to provide a better/worse matching between individuals and IMT:s, or to alter the composition of IMT:s. Another type of effect is to alter the outcomes within a particular IMT. The first set of estimates in *Table 8* captures the total effect after moving to any IMT, i.e. the sum of the above-mentioned effects. We see that the point estimate is small and statistically insignificant. The estimates presented in the lower part of the table suggest that the only significant impact under SIN is that the hazard from work experience to employment increased by about 15 percent. *Figure 9* and *Figure 10* suggest that what happened was that the difference between SIN and non-SIN locations diminished. Note also in the figures that the seemingly dramatic changes in the hazards from employment subsidies build on quite a small number of observations.

Table 7 Effects on transitions from employment or intermediate treatments back to unemployment

		Haz. Ratio	Std. Err.	z	P>|z|
Employment	D	.709	.009	-28.080	.000
	SD	.998	.015	-.130	.895
Employment subsidy	D	.784	.038	-4.970	.000
	SD	1.134	.064	2.220	.026
Work exp.	D	1.353	.041	9.920	.000
	SD	.953	.034	-1.320	.186
Other	D	.967	.016	-2.050	.040
	SD	1.015	.020	.740	.458

Notes: Table shows parameter estimates from Cox hazard regression estimation using the stratified partial maximum likelihood estimator. D measures the general before-after effect; SD is the DiD estimate (the difference in the before-after estimate between the SIN and the non-SIN locations).

Do the positive within-effect for work experience and the zero overall DiD estimates of IMT:s make sense? Probably, yes. Work experience has pretty poor outcomes relative to e.g. employment subsidies. SIN meant an increase in the relative share in work experience, which counteracts the positive effects within the work experience IMT. The latter effect is not so surprising: those who enter work experience under the SIN program are to have a promise on employment after finishing. Even though this does not hold in all cases, one

can suspect that SIN makes case workers more restrictive and cautious in granting work experience schemes, and at the same time increases pressure on employers to stick to their promises.

Table 8 Effects on moving from IMT:s to employment

		Haz. Ratio	Std. Err.	z	P>|z|
Any IMT	D	.505	.008	−45.78	.000
	SD	1.026	.018	1.42	
Employment subsidy	D	.574	.025	-12.740	.000
	SD	.961	.049	-.780	.433
Work exp.	D	.663	.024	-11.510	.000
	SD	1.146	.049	3.210	.001
Other	D	.448	.008	-44.260	.000
	SD	1.008	.022	.360	.715

Notes: Table shows parameter estimates from Cox hazard regression estimation using the stratified partial maximum likelihood estimator. D measures the general before-after effect; SD is the DiD estimate (the difference in the before-after estimate between the SIN and the non-SIN locations).

5.3   Sensitivity analysis

We have performed a number of robustness checks to test the plausibility of the results. We begin by presenting a few general specification tests before proceeding to a more fundamental discussion on the difference-in-differences strategy.

5.3.1   General robustness checks

A first type of variation was to investigate the possibility that the population in the treatment and the comparison locations were somehow different, which may make the DiD strategy invalid. The idea is of course that there may be differences across groups in the reaction to local shocks. First, we simply restricted the population to individuals born in the regions most frequent in SIN: Africa, Asia and Europe outside EU15. In a more sophisticated robustness check we then estimated the conditional propensity of entering SIN using a logit regression model with a large set of covariates for the SIN locations. The predictions from this model were then used to remove all comparison observations with a propensity to start SIN that was smaller than the smallest propensity for the SIN participants (846 individuals). For different levels of these pro-

pensities (16 groups) we then estimate separate treatment effects. We also tried including covariates in the regressions, controlling for potential changes in the differences of the population composition. None of these variations made any difference to the results. Dropping observations in the local labor market region where SIN was extended during 2005 did not have any impact on the estimates.

As mentioned in the description of the reform, it has been argued that SIN was not working fully during the first year due to the need for recruiting new officers. We therefore moved the reform date to September 1, 2004 and excluded unemployment entries in the first year of SIN (Sep 2003–Aug 2004). This gave similar but somewhat stronger results in the sense that the positive impact on the transitions to work experience from unemployment was larger, and so was the impact on the flows from work experience to employment. This is vaguely in line with the notion that SIN grew more influential with calendar time.

When we perform the analysis separately by gender, we find that the qualitative patterns are similar, but that the effects appear to be somewhat larger for women than for men. We have also estimated the models separately for some large regions (e.g. the Stockholm local labor market). The qualitative patterns are similar across regions.

Let us also mention that we have approached the SIN reform in many alternative ways and that we have used the stratified Cox regression estimator with different levels of stratification. Estimating differences-in-differences linear probability models for the probability of being in employment at fixed times following entry to unemployment or IMT:s gives qualitatively similar results: an increased probability of being in employment,[12] working at least partly through better performance of the work experience schemes. Using calendar time stratification, propensity scores and calendar time and propensity score as stratification units also give qualitatively the same results.

5.3.2 Analyzing an imaginary reform in September 2002

The empirical strategy builds on the assumption that the only thing separating the development over time in the SIN and the non-SIN locations is the introduction of the SIN program. Put differently, what happened in the control loca-

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[12] Although questionable on the basis discussed in section 5.3.2.

tions would have happened in the treatment locations had the reform not occurred. To test this assumption we analyzed an "imaginary" reform supposed to have taken place at September 1, 2002. To avoid observations affected by the real SIN reform, we censored the data on September 1, 2003.

The results presented in the appendix cast doubt on the impact of SIN on the transitions from unemployment to work. The estimated impact is positive and significant, and of the same magnitude as in the analysis of the real reform. However, we do not find similar patterns for the effects on transitions to work experience, or for the hazard from work experience to employment.

One can discuss the value of this sensitivity analysis. True believers would suggest that it captures a time trend that should be extended into the actual reform period, using a difference-in-difference-in-differences (DDD) strategy. However, the DDD approach means strong assumptions regarding the expected development over a longer period of time. Also, we cannot rule out the possibility that there were e.g. differing institutional changes during the "imaginary reform" period which caused the estimates. Rejecting the baseline results would then be a mistake. Our reading of the results is that one must be very cautious in claiming that SIN directly affected flows from unemployment to work, but that the case is stronger for the effects going through work experience.

5.4   An attempt to compare the costs and benefits of SIN

We will now make some very rough calculations to compare the estimated benefits of SIN to the costs in a public expenditure perspective. There are countless ways of making such calculations, especially considering the many channels through which SIN may work. To get something remotely resembling estimates of the costs per job created, we calculate the costs per job-year added under different assumptions. *Table 9* presents the calculations.

First, let us describe the calculations in the baseline case, where our point of departure is that the only effect worth considering is the increased chance of going from work experience to employment. Thus, for now we assume that there was no change in the transitions to work from unemployment due to SIN (as suggested by the "imaginary reform" analysis). Also, the higher hazard into work experience schemes is ignored. This is correct if we assume that under steady state the number of program slots is fixed. An increased participation in one subpopulation is then matched by crowding out of other unemployed. Note, however, that we include in the table scenarios with a larger number of

jobs created than under the baseline assumptions. One could think of these jobs as a result of an increased number of work experience schemes.

There were about 65,680 periods of unemployment in the SIN locations in 2005 (extending to the full year based on entry patterns in 2004). In the pre-SIN period, 6.4 percent of these ended in work experience, in other words 4,200 observations. Now, the pre-reform hazard rate in *Figure 9* suggests a hazard of approximately 0.0015 per day, leading to 55 percent (= 0.0015*365) or 2,300 individuals finding work in the first year in the absence of SIN. With the estimated 15 percent increase in the hazard following SIN (*Table 8*), the number of added full-year jobs would be 340. We then need to make some assumptions on how long these jobs will last. The pre-reform hazard rate back to unemployment is about 0.002 per day (*Figure 6*), suggesting that jobs can be expected to last for 500 days (= 1/0.002), or 1.37 years.[13] Under these assumptions, the number of created full-year jobs is 466. Related to the annual cost of SIN of 126 million, the cost per job-years added is then about SEK 270,000.

Table 9 Costs per job-year created under different assumptions

Expected days in work: (Hazard back to unemployment)	400 (0.0025)	500 (baseline) (0.0020)	600 (0.0017)
Jobs created:	*Estimated cost per job-year 2005 (SEK)*		
300	383,000	307,000	256,000
340 (baseline)	338,000	270,000	225,000
400	287,000	230,000	192,000
500	230,000	184,000	153,000

Notes: The number of jobs created and the duration of these jobs are based on the observations made in the data. See the text for further descriptions.

What happens to the estimated cost if we alter some of the assumptions? Varying the number of jobs created and the expected duration of each job of

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[13] Remember that the transitions back to unemployment (from work experience and employment) appear to have been unaffected by SIN.

course affects the results; the estimates in *Table 9* range from 153,000 to 383,000. At the lower end this is below the annual cost of financial support per unemployed. At the higher end it is above that level. However, to compute the total fiscal benefits one would need a more complicated analysis (including e.g. added tax revenue due to more people working. Also, we have here not taken into account any dynamic effects on reduced risks of future unemployment, let alone the individual welfare effects of working instead of being unemployed. An answer to whether the program is worth the money spent is thus contingent on assumptions of future labor market outcomes and the valuation of non-monetary gains. To provide such an answer is far beyond the scope of this paper.

6   Concluding remarks

The SIN program targets refugees and immigrants whom are considered employable and ready to take a job immediately, but who are also expected to experience substantial difficulties in finding work. We use detailed population micro data on unemployment spells, intermediate treatments and final outcomes combined with a differences-in-differences strategy comparing SIN municipalities with non-participating locations in the same local labor markets to estimate the effects of the SIN program.

The most robust result is that SIN increased transitions into work experience schemes. Also, going to work experience is associated with higher employment probabilities with SIN than in absence of the program. The impact of SIN on transitions from unemployment to work is muddled by the existence of similar difference-in-differences patterns in the years preceding SIN.

At first glance it may appear that the supported employment approach offers a new—or at least largely unexplored—approach to help job seekers. Available previous studies, however, suggest that the actual implementation builds on better counseling and understanding of each applicant, combined with intensified efforts in finding work for the clients, and careful matching between unemployed individuals and employers. These are all elements of an intensified job-search and matching assistance, which are of course not a new element of active labor market policies. Based on prior knowledge of the effects of matching efforts, we would expect to see positive effects from SIN (see e.g. Blundell et al (2004) and Martin & Grubb (2001)). This is particularly the

case since the resources added are quite significant relative the normal PES level.

From a public governance perspective, the implementation of SIN could be criticized for deviating from the original intentions of a workplace introduction based on supported employment, and for using the SIN officers more or less as extra resources for intensified matching. On the other hand, in the way and at the level implemented in the ongoing trials, the SIN program appears to work relatively well. Departing from the original plan may therefore be considered a smaller problem.

What is open for debate, however, is whether we would see the same effects, should the program be extended to a larger group of individuals (inside or outside the current target group). While theory and previous studies suggest that we should expect positive effects from this type of efforts in the general case, one could argue that successful matching between unemployed individuals and jobs presupposes a large pool of potential participants to choose from, relative to the number of people actually assigned to the program. Also, it is possible that the value of matching efforts is greater for the groups currently targeted due to their relative lack of networks of employer contacts.

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Appendix A

Testing the validity of the results by having an "imaginary" reform in the SIN-municipalities on September 1, 2002. All unemployment spells extending into September, 2003 are removed from the analysis

Table A 1 Imaginary reform 2002: Effects on transitions from unemployment to employment or intermediate treatments

		Haz. Ratio	Std. Err.	z	P>|z|
Employment	D	.927	.011	-6.320	.000
	SD	1.105	.016	6.850	.000
Employment subsidy	D	.942	.045	-1.260	.206
	SD	1.045	.058	.800	.424
Work exp.	D	.525	.020	-16.990	.000
	SD	.990	.045	-.230	.818
Other	D	.896	.011	-9.130	.000
	SD	1.018	.014	1.250	.210

Notes: Table shows parameter estimates from Cox hazard regression estimation using the stratified partial maximum likelihood estimator. D measures the general before-after effect; SD is the DiD estimate (the difference in the before-after estimate between the SIN and the non-SIN locations).

Table A 2 Imaginary reform 2002: Effects on transitions from employment or intermediate treatments back to unemployment

		Haz. Ratio	Std. Err.	z	P>|z|
Employment	D	.558	.008	-39.500	.000
	SD	.976	.017	-1.350	.176
Employment subsidy	D	.979	.043	-.480	.629
	SD	1.067	.056	1.250	.213
Work exp.	D	.869	.051	-2.380	.017
	SD	1.065	.073	.920	.356
Other	D	.950	.019	-2.580	.010
	SD	1.045	.025	1.850	.064

Notes: Table shows parameter estimates from Cox hazard regression estimation using the stratified partial maximum likelihood estimator. D measures the general before-after effect; SD is the DiD estimate (the difference in the before-after estimate between the SIN and the non-SIN locations).

**Table A 3** Imaginary reform 2002: Effects on moving from IMT:s to employ-ment

		Haz. Ratio	Std. Err.	z	P>|z|
Any IMT	D	.296	.005	-67.91	
	SD	.984	.027	-.76	
Employment subsidy	D	.470	.027	-12.930	.000
	SD	1.040	.070	.590	.558
Work exp.	D	.409	.021	-17.310	.000
	SD	.954	.059	-.770	.444
Other	D	.270	.006	-63.840	.000
	SD	.974	.023	-1.080	.279

Notes: Table shows parameter estimates from Cox hazard regression estimation using the strati-fied partial maximum likelihood estimator. D measures the general before-after effect; SD is the DiD estimate (the difference in the before-after estimate between the SIN and the non-SIN locations).
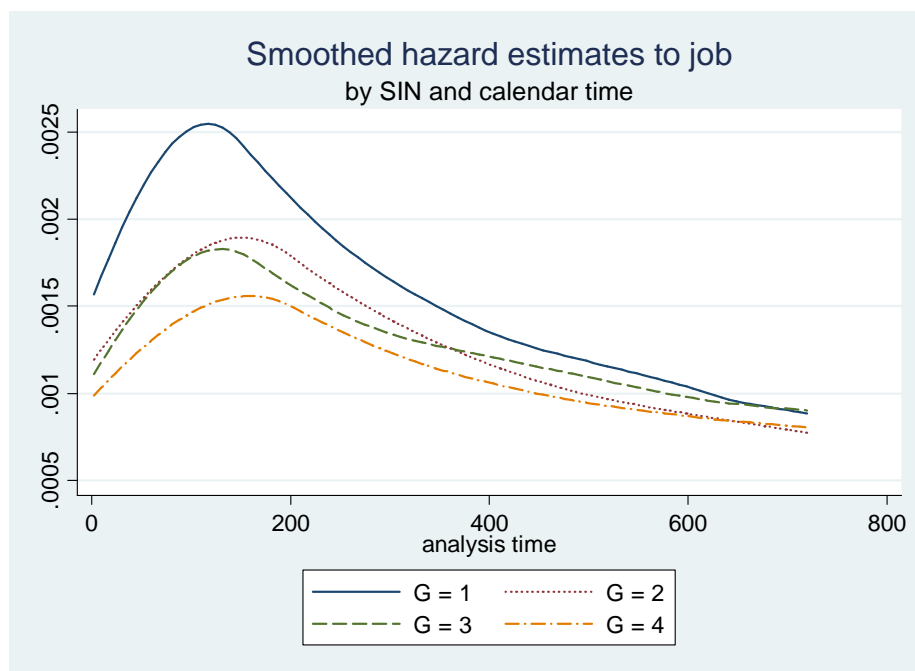
Figure 3 Transitions from unemployment to employment.

G = 1 (not SIN and before September 1 2003), G = 2 (not SIN and after September 1 2003) G = 3 (SIN and before September 1 2003) G = 4 (SIN and after September 1 2003). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.
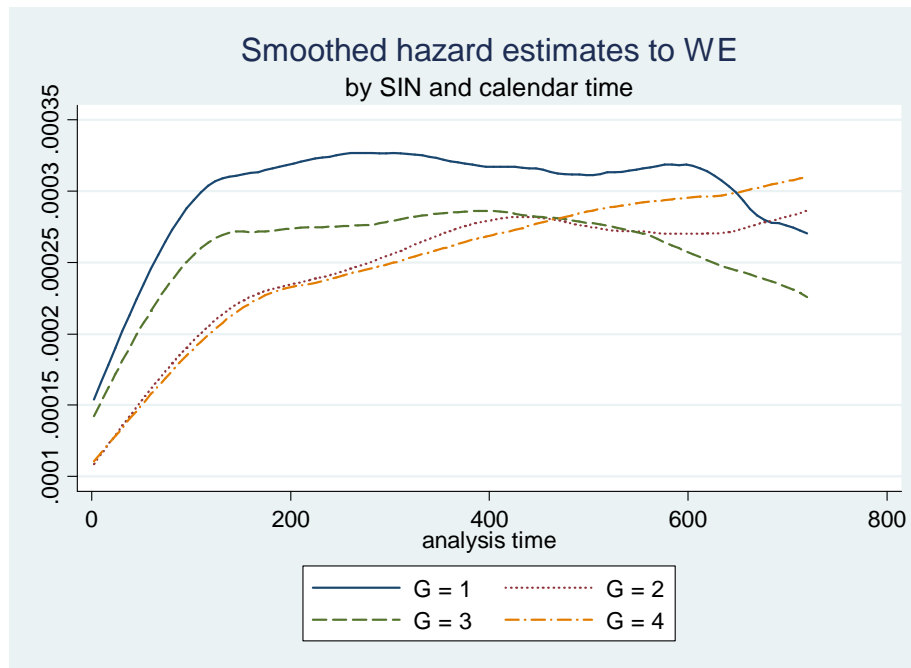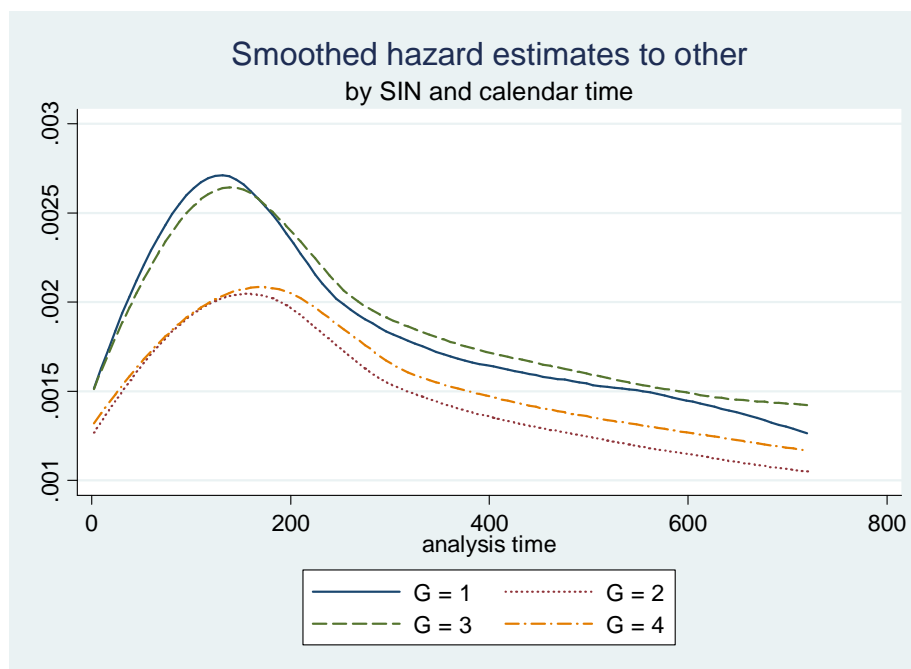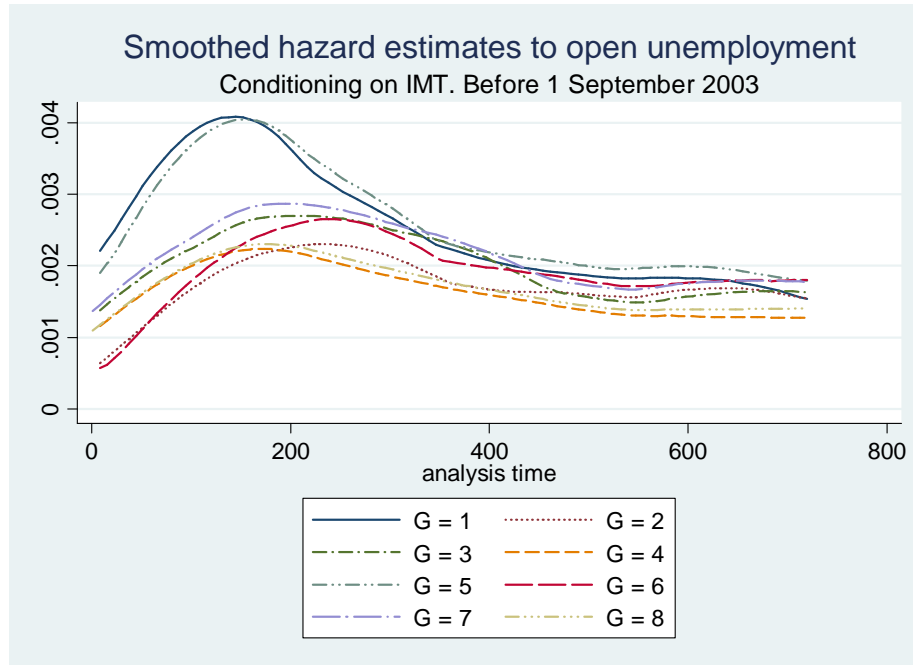
Figure 4 Transitions from unemployment to work experience

G = 1 (not SIN and before September 1 2003), G = 2 (not SIN and after September 1 2003) G = 3 (SIN and before September 1 2003) G = 4 (SIN and after September 1 2003). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.
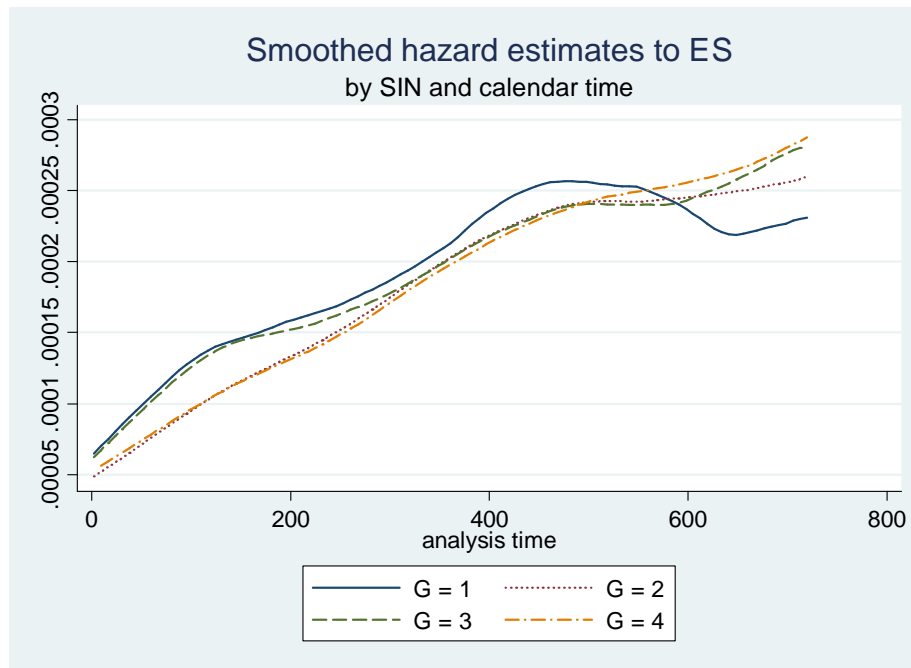
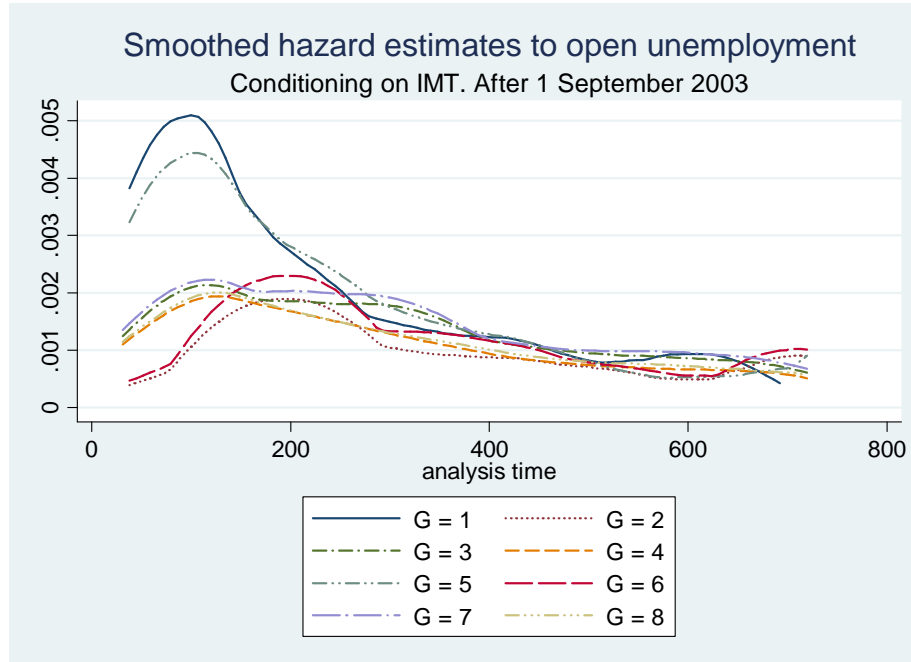**Figure 5** Transitions from unemployment to employment subsidies

G = 1 (not SIN and before September 1 2003), G = 2 (not SIN and after September 1 2003) G = 3 (SIN and before September 1 2003) G = 4 (SIN and after September 1 2003). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.

Figure 6 Transitions from unemployment to "other" IMT:s

G = 1 (not SIN and before September 1 2003), G = 2 (not SIN and after September 1 2003) G = 3 (SIN and before September 1 2003) G = 4 (SIN and after September 1 2003). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.

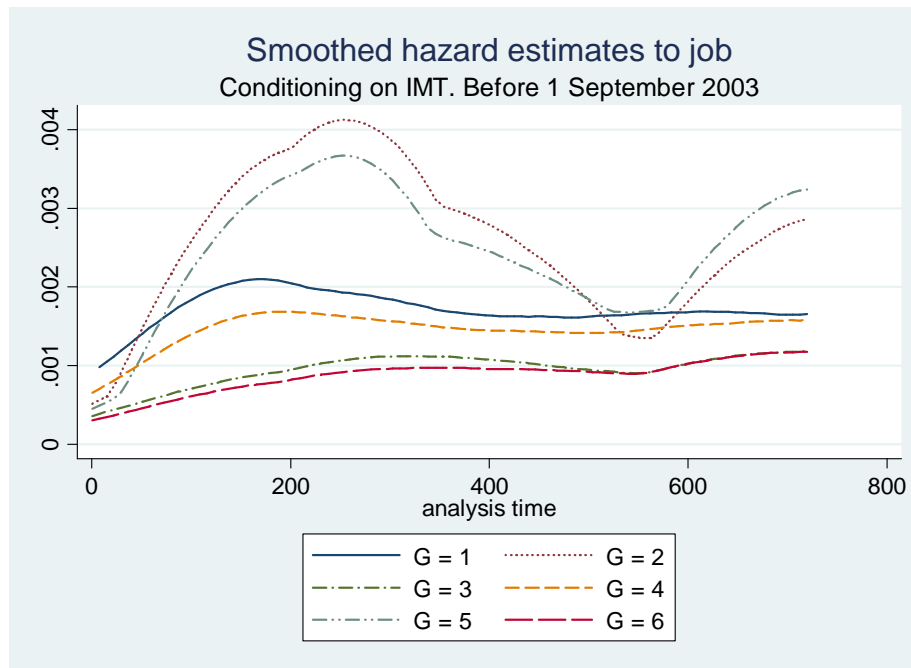**Figure 7** Transitions from IMT to unemployment before September 1, 2003

G = 1 (WE and not SIN), G = 2 (ES and not SIN) G = 3 (other and not SIN) G = 4 (employment and not SIN))¨G = 5 (WE and SIN), G = 6 (ES and SIN) G = 7 (else and SIN) G = 8 (employment and not SIN). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.

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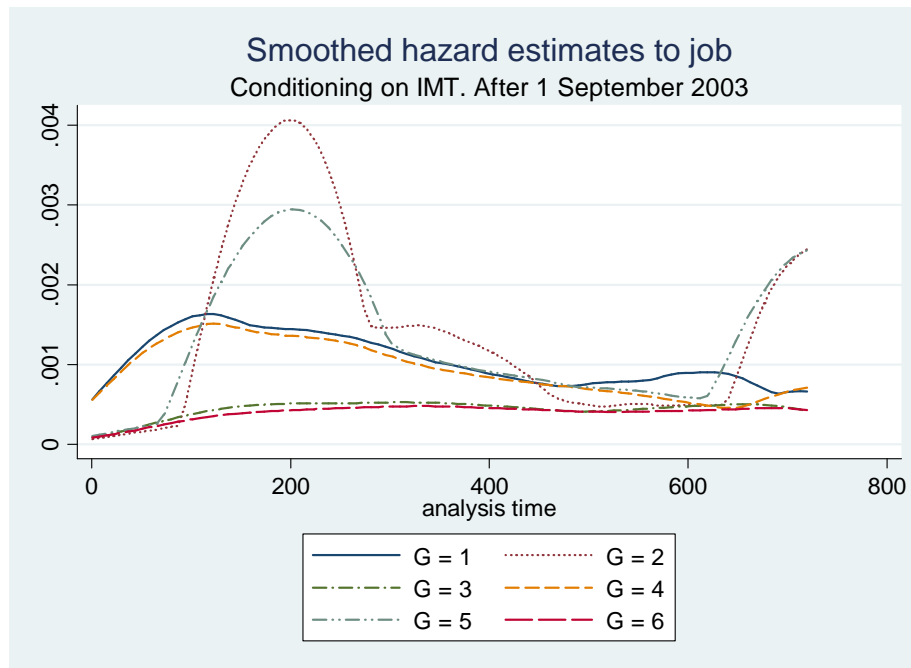
Figure 8 Transitions from IMT to unemployment after September 1, 2003

G = 1 (WE and Not SIN), G = 2 (ES and Not SIN) G = 3 (other and Not SIN) G = 4 (employment and Not SIN)), G = 5 (WE and SIN), G = 6 (ES and SIN) G = 7 (other and SIN) G = 8 (employment and not SIN). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.

Figure 9 Transitions from IMT to employment before September 1, 2003

G = 1 (WE and not SIN), G = 2 (ES and not SIN) G = 3 (other and not SIN) G = 4 (WE and SIN), G = 5 (ES and SIN) G = 6 (other and SIN). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.

Figure 10 Transitions from IMT to employment after September 1, 2003

G = 1 (WE and not SIN), G = 2 (ES and not SIN) G = 3 (other and not SIN) G = 4 (WE and SIN), G = 5 (ES and SIN) G = 6 (other and SIN). Smoothing is performed with the Epanechnikov kernel with the default bandwidth in STATA.