IFAU
Institute for Evaluation of Labour
Market and Education Policy

All aboard? Commuter train access and labor market outcomes

Olof Åslund
Ina Blind
Matz Dahlberg

WORKING PAPER 2015:25

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

IFAU also provides funding for research projects within its areas of interest. The deadline for applications is October 1 each year. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The institute has a scientific council, consisting of a chairman, the Director-General and five other members. Among other things, the scientific council proposes a decision for the allocation of research grants. A reference group including representatives for employer organizations and trade unions, as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

ISSN 1651-1166

All aboard? Commuter train access and labor market outcomes[a]

by

Olof Åslund[b], Ina Blind[c] and Matz Dahlberg[d]

November 9, 2015

Abstract

We investigate the impact of commuter train access on individual labor market outcomes. Our study considers the introduction of a commuter train on a pre-existing railroad in Sweden, considerably decreasing commuting times by public transit and hence increasing access to the regional employment center. Using difference-in-differences matching techniques on comprehensive individual panel data spanning over a decade, our intention-to-treat estimates show that the reform essentially had no impact on the earnings and employment development among the affected individuals.

Keywords: Infrastructure investment, commuting, job access, labor market outcomes
JEL-codes: J22, J63, R23

[a] We thank John Östh for providing a distance table for locations (SAMS) in the local labor market of Uppsala, Eva Jirner for creating the present map, and Åsa Bråmå for creating maps that were used in the study design. We are grateful for comments from Erik Grönqvist, Caroline Hall, Peter Fredriksson, Elisabet Viladecans Marsal and seminar participants at the 2011 European Meeting of the Urban Economics Association in Barcelona and at the 2011 National Conference in Economics in Uppsala. Ina Blind also thanks the Graduate School in Population Dynamics and Public Policy at Umeå University for financial support.
[b] Institute for Evaluation of Labour Market and Education Policy (IFAU) and Department of Economics, Uppsala University. E-mail: olof.aslund@ifau.uu.se
[c] Institute for Housing and Urban Research (IBF) and Department of Economics, Uppsala University. E-mail: ina.blind@ibf.uu.se
[d] IBF and Department of Economics, Uppsala University, and IFAU. E-mail: matz.dahlberg@ibf.uu.se

Table of contents

1 Introduction

The importance of job access via public transit in improving the functioning of the labor market and strengthening the economic position of workers, is a topic receiving considerable political attention. The infrastructural investments required are substantial and relatively easy to compute. The gains are harder to estimate, and knowledge, for example about the labor market impact of changes in job access and commuting times, is limited. However, recent theoretical work points to the importance of transport modes for generating differences in economic outcomes across groups (Gautier & Zenou, 2010) and some studies argue that the availability of public transit is a key determinant for cross-group differences in geographical distribution (Glaeser, Kahn, & Rappaport, 2008).

We investigate these issues, studying the individual labor market effects of a commuter train considerably decreasing commuting times by public transit to the employment center for those living close to the pre-existing railroad on which the commuter train was introduced. *Upptåget* (the case we study), was inaugurated in the early 1990s, connecting locations north of the city of Uppsala, Sweden, to the local center and further to the greater Stockholm area. We argue below that the institutional features suggest that the case is well suited for overcoming many of the methodological challenges typically present in this type of research.[1] While the location of the train was governed by a pre-existing railroad, the timing was related to a legal change. The train altered commuting opportunities and travel times for some areas, while leaving conditions unchanged for other areas included in the same local labor market. It can also be argued that—at least in a European context—we address the more policy-relevant margin: the effects of improving public transport rather than introducing it in a context where only private transport has been available previously.

Theory suggests a number of reasons why commuting opportunities may affect the employment and wages of individual workers. The literature is described in more detail in the next section, but let us here only point out a few potential mechanisms. First, shorter travel times or less expensive commutes may increase the optimal job search area (Gobillon, Selod, & Zenou, 2007) and may also decrease the reservation wage

[1] Our approach is in line with the suggestions made by Gibbons and Machin (2006), in their literature study on transport and labor market linkages, on how to deal with the problems of endogeneity and to identify a causal impact of transport or transportation policy on labor market outcomes.

(Brueckner & Zenou, 2003; Coulson, Laing, & Wang, 2001), leading to decreased unemployment. In other words, the effective labor market is increased, which should improve matching. Long commutes may also affect the productivity of workers, either because the commute itself requires a lot of effort (Zenou, 2002), or because the commute affects the flexibility between work and other commitments (Ross & Zenou, 2008). This may make employers reluctant to hire people living too far away, or induce workers to shirk, which increases the risk of unemployment.

Although there is a large number of empirical studies on the impact of job access on labor market outcomes following Kain (1968), it is only relatively recently that transport modes and transport infrastructure have been taken into account in this line of research.[2] There are thus some studies on the importance of car ownership or car access (e.g., Gurley & Bruce, 2005; Ong, 2002; Raphael & Rice, 2002; Shen & Sanchez, 2005) and a few studies focusing on job access by public transit. Some US studies suggest no or little relation between job access by public transit and employment (e.g., Cervero, Sandoval, & Landis, 2002; Sanchez, Shen, & Peng, 2004); whereas others find a positive association (e.g., Kawabata, 2003; Ong & Houston, 2002; Sanchez, 1999).

Evidence regarding the importance of job access by public transit is thus mixed and pertains mainly to the US. It is possible that the effects of new public transit infrastructure are different in Europe where the public transit network in and around cities is generally more extended than in the US. Matas, Raymond and Roig (2010) study the importance of job access by public transit in Barcelona and Madrid, Spain, and find a positive effect on women, primarily among the low-educated.

We use comprehensive, individual panel data for the years 1985–1996, including detailed geographical information on residential and workplace location, as well as on labor market outcomes. We combine a difference-in-differences approach with matching methods to compare the development of labor market outcomes for individuals living in treated and non-treated areas before the introduction of the train.

[2] Kain (1968) suggested that the high unemployment rate of African-Americans in US metropolitan areas was aggravated by the movement of low-skilled jobs from the central cities to the suburbs, worsening job access for African-American workers constrained to central cities by housing market discrimination (the spatial mismatch hypothesis). Since the study by Kain (1968) a large number of empirical studies have been carried out which attempt to test the relation between job access and labor market outcomes in general and the spatial mismatch hypothesis in particular. The collected evidence suggests that poor access to jobs does indeed lead to worse labor market outcomes (for literature surveys see Gobillon et al., 2007; Ihlanfeldt, 2006; Ihlanfeldt & Sjoquist, 1998; Zenou, 2009).

We find that the introduction of the commuter train essentially had no significant effects on employment probability or labor earnings for those individuals living in the treated area before the new commuter train was introduced. A large set of robustness checks and supplementary analyses confirm the impression that getting access to the commuter train did not significantly alter the labor market development of the treated individuals.

The rest of the paper is organized as follows: in Section 2 we briefly present some theories as to why job access can matter for individual labor market outcomes. Section 3 describes the development of the commuter train *Upptåget* and defines the treatment group and the potential control group. Section 4 explains the empirical strategy and presents the data used as well as some descriptive statistics. The results from the empirical analysis are presented in Sections 5 and 6, and Section 7 concludes.

2 Theories

The introduction of the commuter train Upptåget considerably decreased commuting times by public transit from the stations towards the employment center of Uppsala city and further south towards Stockholm, and thus led to improved job access close to the stations. What does theory lead us to expect about the effect of the commuter train on individuals' labor market outcomes?

In the standard urban economic model developed by Alonso (1964), Mills (1967), and Muth (1969), housing and land prices decline with distance from the central business district (CBD) to compensate individuals for longer commutes. In this monocentric urban model, high-income workers consume more land and therefore choose to live where land is cheap, i.e., far from the CBD, while poor workers live close to the CBD.[3] In the model and versions thereof, for example including different transport modes (LeRoy & Sonstelie, 1983) and decentralized or multi-centric employment (e.g., White, 1976), the labor market is fully competitive, productivity and wages are given and there is no unemployment. Thus, although the models include a relation between job access and income, length of commute cannot affect individuals' labor market outcomes.

[3] The key condition for this is that the elasticity of land with respect to income is greater than the elasticity of the value of time with respect to income (see Becker, 1965). The validity of this condition has been questioned, see e.g. LeRoy and Sonstelie (1983) and Glaeser, Kahn and Rappaport (2008).

In the middle of the 1990s efforts began to combine urban economic models with labor economic theories and develop models in which workers' location (land market), as well as wages and unemployment (labor market) are determined in equilibrium (for a synthesis, see Zenou, 2009). While most of these models do not take transport modes into account, they may still be relevant at least to the extent that people rely on public transit.

One branch of this literature introduces spatial frictions to efficiency wage models (see e.g. Brueckner & Zenou, 2003; Ross & Zenou, 2008; Zenou, 2002, 2009; Zenou & Smith, 1995).[4] In some of these models work effort and thus productivity is allowed to vary with the length of commute, either because the commute itself requires a lot of effort (Zenou, 2002), or because the commute affects the flexibility between work and other commitments (Ross & Zenou, 2008). The implications of this for employment and wages depend on the ability of employers to observe workers' commuting costs and to anticipate workers' behavior. Within these models, the new and faster commuting opportunity could reduce work related fatigue and increase workers' leisure time, thus making people with longer commutes more productive at work. This could in turn make employers more willing to hire workers living far away/reduce the workers' risk of being caught shirking and with that the number of unemployment spells. Increased leisure time and a reduction of commuting related fatigue could have some effect even on people who did not rely on public transit but on car before the introduction of Upptåget. Switching from driving a car to riding a train would allow them to rest, work or do some errands (e.g. send mails, pay bills, make phone calls) during the commute.

Another branch of the urban labor economics literature introduces spatial frictions to search-matching models (see e.g., Gobillon et al., 2007; Smith & Zenou, 2003; Wasmer & Zenou, 2002, 2006).[5] Studies in this vein suggest that the introduction of the commuter train could help people to higher search efficiency and search intensity, increasing their employment probabilities and probabilities of finding better paying jobs.

A spatial search-matching model that includes mode-choice is provided by Gautier and Zenou (2010). In the model, because of initial wealth differences, whites can buy cars while ethnic minorities have to rely on public transit. Since the set of jobs that can

[4] For the initial efficiency wage model see Shapiro and Stiglitz (1984).
[5] For the initial search-matching model see Mortensen and Pissarides (1999) and Pissarides (2000).

be reached by car is larger than the set that can be reached by public transport, whites find jobs more quickly and experience shorter unemployment spells. Furthermore, a worker's bargaining position depends on what employers know or suppose about car ownership among white and ethnic minorities (statistical discrimination), resulting in higher wages for whites. In this model, better public transport such as the commuter train *Upptåget* should reduce differences in labor-market outcomes between whites and ethnic minorities.

To the extent that workers' residential locations are fixed, there are also models where workers may refuse jobs involving commutes that are too long because commuting to that job would be too costly in view of the proposed wage (Brueckner & Zenou, 2003; Coulson et al., 2001). This can depress both wages and employment rates in areas where the number of jobs are few relative to the labor pool. The new and faster commuting opportunity could here allow people to accept jobs that they would previously not have accepted, positively affecting their employment and earnings. These models could be most relevant for groups with stronger residential constraints, e.g., people with low incomes for whom credit constraints can limit residential choices and immigrants for whom different types of discrimination can be limiting (see e.g. Ahmed & Hammarstedt, 2008, for evidence on discrimination on the Swedish rental market).

In the long run it is possible that better labor market outcomes brought about by Upptåget are amplified if the train helps people's careers take off. However, it is also possible that people trade off the improved commuting opportunities for other things, e.g., larger housing further away. It is also possible that with time there is increased job competition from people moving into the settlements with stations. Competition for jobs close to the stations could also come from people who now reverse commute from Uppsala city to the other station settlements. Theoretically, a large in-migration could also raise housing costs and force some people to move and give up the improved job access. In practice, however, there is little direct pressure on housing costs for those who own their housing and since rents are regulated in Sweden and location and job access are not very important in the rent setting, the changes in housing costs were probably also small for people renting their housing. On the other hand, the improved commuting opportunities could retain or attract firms to the station settlements, thus further increasing job access in these places.

There is thus some uncertainty regarding the direction of the effects, even though most mechanisms would indicate that better access to jobs would theoretically mean better labor market outcomes. What is more unclear, however, is the magnitude (or even presence) of the empirical impact. We essentially do not know how people value or are able to take advantage of a given decrease in expected commuting time. The empirical study performed below aims to provide some information on this topic, which is of core relevance for policy in the area.

3 Upptåget and the research design

We employ a quasi-experimental research design that builds on the introduction of a commuter train, Upptåget, to the Swedish city of Uppsala. Our definition of treatment and control groups is based on residential location in 1989; before public discussion on the commuter train began, and two years before the trains started running. We compare those who then lived in the part of the local labor market of Uppsala where the commuter train was introduced, to individuals who lived in two other parts of the same local labor market, but which were not subject to changes in transport infrastructure. That is, we will conduct an intention-to-treat (ITT) analysis. To control for observed and unobserved differences between the group of treatment and control individuals unrelated to the commuter train, the analysis is conducted using a difference-in-differences matching estimator. Below, we first give some institutional detail and describe the decision process and the implementation of the commuter train. We then proceed to a more detailed description of the research design.

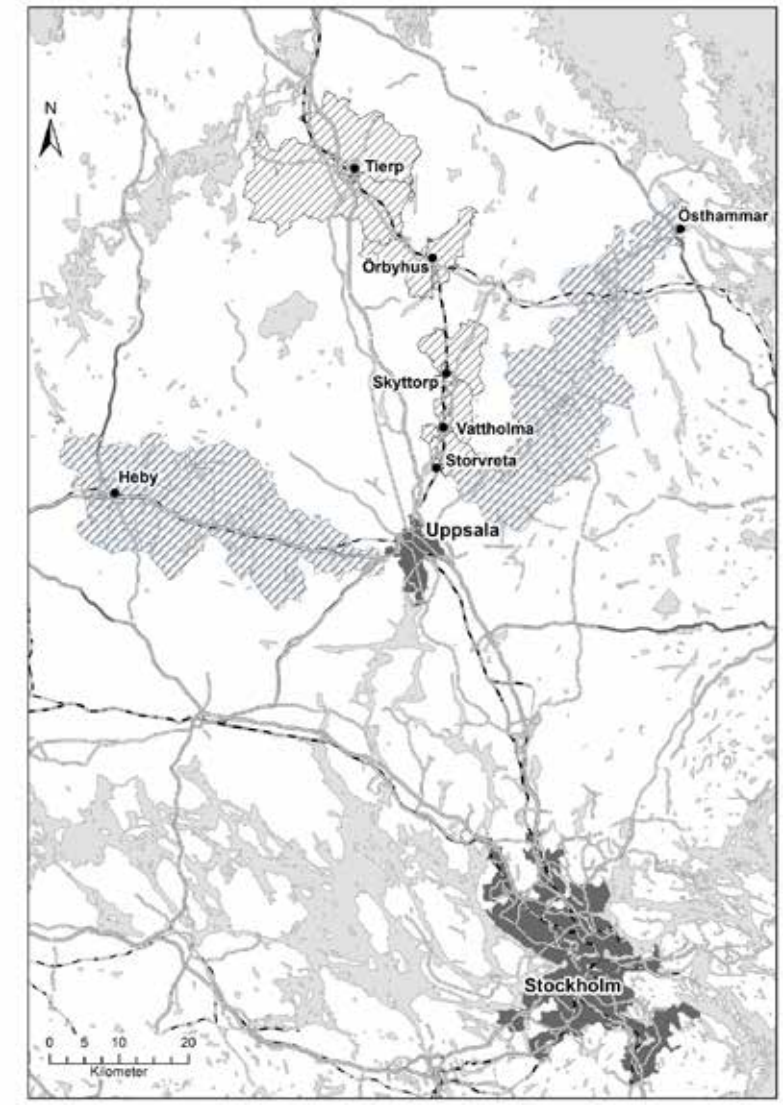
3.1 Upptåget

The central node of the local labor market of Uppsala is Uppsala city, which is the main destination for labor commuting from the surrounding municipalities of Enköping, Heby, Tierp, and Östhammar.[6] Upptåget runs between Uppsala city and the principal settlement in Tierp municipality, Tierp town, 54 km north, see Figure 1. Before the introduction of Upptåget, all public transit within the municipalities of Heby, Tierp, and Östhammar, as well as between these municipalities and Uppsala municipality was by

[6] The local labor markets are defined by Statistics Sweden based on commuting patterns. From 1996, Uppsala municipality and with it the municipalities of Enköping, Heby, Tierp, and Östhammar came to belong to the local labor market of Stockholm. However, Uppsala city continued to be the main destination for labor commuting from the municipalities of Enköping, Heby, Tierp, and Östhammar.

bus.[7] The only exception was a long-distance train with stops in Tierp town and in Uppsala city. Public transit by road was coordinated and purchased by Upplands Lokaltrafik (UL), a firm jointly owned by Uppsala county council and the municipalities in the county.[8]

Figure 1: The Upptåget, Heby, and Östhammar-corridors



Note: The shaded (north-bound) area between Uppsala and Tierp constitutes the "treatment corridor". The shaded area between Uppsala and Heby (west-bound) and between Uppsala and Östhammar (northeast-bound) constitutes the "control-corridors". When inaugurated in 1991, Upptåget stopped in Storvreta, Vattholma, Skyttorp, and Örbyhus between the end stations of Uppsala and Tierp. The map was created by Eva Jirner.

[7] The information on the public transit network in this and the following paragraphs is from timetables and annual reports from UL unless otherwise indicated. We thank Mats O Karlsson, member of Uppsala County Council 1988-2006 for checking the accuracy of the information. Timetables from June 1986 and onwards can be consulted at the Uppsala County Council archive, although some years are missing.

[8] Before 2007 the municipality of Heby belonged to Västmanland county and not to Uppsala county, so public transit within Heby municipality was not organized by UL.

The first concrete plans for Upptåget seem to have been outlined in 1988 and were accompanied by a trial trip in the same year (see e.g. Upplands Lokaltrafik (UL),1988 and Arbetsgruppen Projekt Upptåget, 1988a). In a study preceding the train (Arbetsgruppen Projekt Upptåget, 1988a) it can be read that two elements brought to the fore the interest for a commuter train: in an analysis on express buses from the northern part of Uppsala county towards Uppsala city it was found that a commuter train on already existing railroad tracks would not be more expensive while it would radically shorten commuting times; and government bill 1987/88:50 opening for counties to take own initiatives regarding train services by renting railroads from the administering State organization (Banverket).

It was first suggested that the train should start running in the autumn of 1990 (Arbetsgruppen Projekt Upptåget, 1988b). However, this proved impossible since another project was also planned on the same link (a high-speed train between Stockholm and Sundsvall) making double tracks and some other track work necessary (see Arbetsgruppen Projekt Upptåget, 1988b and UL annual report 1989/1990). In the summer/autumn 1990, the necessary decisions regarding the division of operation and capital costs for the train between Uppsala County Council, Tierp municipality and Uppsala municipality were reached (Documents Uppsala County Council meeting, 1990 & UL annual report 1989/1990). Around the same time, UL announced that they intended the train to start running in August 1991, but that this depended on the progress of the track work. We have not been able to track down at what time the inauguration date was finally fixed, but in accordance with the aim of UL, Upptåget was inaugurated in August 1991. At this first stage there were 15 trips per weekday between Tierp and Uppsala (eight from Tierp to Uppsala and seven in the opposite direction), with stops in the settlements of Örbyhus, Skyttorp, Vattholma, and Storvreta (see Figure 1 for the locations of these stations). From January 1994 Upptåget also stopped in the locality of Tobo.[9]

[9] The next step in the development of Upptåget was official discussions, starting in 1994, about a continuation of Upptåget northwards towards the city of Gävle. In 1999, however, it became clear that the track work needed for the northward continuation could be conducted no earlier than 2005/2006. In 2002 official discussions also began about a southwards continuation of Upptåget towards Arlanda airport and the northern parts of Stockholm. In 2004, a decision was finally reached that the northward and southward continuations should be inaugurated in August 2006, which was also achieved. Given that these further developments of Upptåget were at most at the discussion stage during the time period we are studying (1985-1996), we do not think that they affected the studied outcomes.

Upptåget did not directly replace a particular bus line but stopped at places previously served by three other bus lines. These bus lines also served places not crossed by Upptåget and were to a large extent maintained after the introduction of Upptåget. After the inauguration of Upptåget, the track work continued between Tierp and Uppsala until 1997, and the number of trips and speed were steadily increased over the 1990s, despite occasional delays due to the track work.

An examination of timetables for public transit suggests a substantial reduction in travel times. In 1990, i.e., before Upptåget started running, the fastest bus transit between Tierp town and Uppsala city took 65 minutes, while Upptåget took 47 minutes in 1992, and 40 minutes in 1996. For Skyttorp (located approximately in the middle of the route), travel times were more than cut in half; the corresponding figures were 42, 20, and 18 minutes respectively.[10] Here it can also be noted that the fare for a ride on Upptåget between any of the stations and Uppsala city was the same as for a bus ride[11], and no more expensive than corresponding bus rides to Uppsala city from other parts of the Uppsala local labor market.

From the investigations preceding the inauguration of Upptåget (Arbetsgruppen Projekt Upptåget, 1988a, 1988b) it appears that it was thought that Upptåget would help handle regional imbalances, counter the county's dependence on Stockholm, and create an integrated county with its own identity. The regional imbalances mentioned are the strong growth of job opportunities and population in Uppsala municipality, with ensuing pressure on housing provision and municipal services, and the stagnating number of job opportunities and population decrease in the municipalities of Tierp and Älvkarleby (the municipality just north of Tierp municipality), with ensuing under-use of existing municipal infrastructure. It was hoped that Upptåget would give the inhabitants in the northern parts of the county access to a larger labor market and that the population pressure on Uppsala could be distributed over the other stations.[12] However, Upptåget was also seen as one of several means to alleviate the problems of

[10] Both before and after the introduction of Upptåget the fastest public transit between Tierp town and Uppsala city was the long distance train that took about 40 minutes. However, the number of connections with the long distance train were few.

[11] The fare for a ride with Upptåget was the same as the fare for a ride with regional bus. However, before 1994, the fare for a ride with intra-city bus between Storvreta and Uppsala was lower than for a ride with Upptåget between Storvreta and Uppsala.

[12] Other benefits hoped for from Upptåget mentioned in the investigations were decreased commuting by car, a larger housing choice especially for two-earner households, and a higher share of people with higher education in the northern part of the county thanks to better access to the higher education institutions in the city of Uppsala.

traffic congestion in the inner city of Uppsala. It was estimated that Upptåget could replace 10 percent of the buses entering the inner city, which would also help postpone an expansion of the bus terminal. It was regarded as more reasonable to make better use of the land already reserved for public transit, i.e., the railroad tracks crossing Uppsala city, than to convert new land within Uppsala city to accommodate public transit.

In conclusion, the introduction of Upptåget seems to be well suited to use in a quasi-experimental approach to study the effect of improved commuting opportunities and thereby job access on individuals' labor market outcomes. The introduction was not primarily motivated by the labor market outcomes in the areas served, although there were hopes that the train would help workers in stagnating areas. Instead, the stretch covered by Upptåget was governed by existing railroad tracks, and the timing was related to a legal change. Furthermore, population pressure and congestion problems in Uppsala city seem to have been as important for the introduction of Upptåget as the labor market outcomes in the areas served. Some other elements add to the suitability of using Upptåget in a quasi-experimental setting. The time period between the idea and the realization of Upptåget was relatively short (3.5 years) and characterized by uncertainty about the launch date, making anticipatory migration less likely. Also, changes in commuting times between the stations and Uppsala city were large, and there are potential control groups who lived in areas in the same local labor market but were not subject to changes in transport infrastructure (see section 3.2).

3.2 Treatment and control groups

In this study we consider the treatment group to be individuals who at the end of 1989 lived in a SAMS (Small Area for Market Statistics) with a population center within 4,500 meters of one of the stations served by Upptåget, and more than 10,000 meters from the central parts of Uppsala city.[13] The SAMS classification was created by Statistics Sweden to satisfy demand for small area statistics from users other than municipalities. The objective was to create fairly homogeneous residential areas of about 1000 inhabitants each, implying that the classification divides Sweden into about 9,000 units.[14] The SAMS have been used frequently in Swedish studies as the formal

[13] However, we have excluded the station locality of Tobo, despite the fact that it lies 4400 m from one of the other stations, since the Upptåget commuter train did not stop in Tobo until 1994.

[14] In larger municipalities, the SAMS classification is based on municipal subdivisions used for intra-municipal and sometimes regional planning and administration and in smaller municipalities it is based on election districts. The

division closest to neighbourhoods. The choice of 4,500 meters is somewhat arbitrary but captures the areas where Upptåget came to be the main public transport mode. The (end of) year 1989 is before major decisions about the commuter train were taken (which was in the summer of 1990) and before one could tell for sure if/when the train would come into being. We therefore think it is unlikely that the people who in 1989 lived close to an Upptåget station had chosen to do so because of the train. At the same time, 1989 is close enough to the decisions for most people not to have moved before the decisions were reached, meaning that most of the people who lived close to an Upptåget station in 1989 actually received the offer of improved commuting opportunities.

As potential control group we have chosen individuals from two other parts of the local labor market of Uppsala. These areas were not subject to changes in transport infrastructure, but also exhibit frequent commutes to Uppsala city. The first is the corridor between the principal settlement in Heby municipality, Heby town, and Uppsala, where a commuter train on existing railroad tracks was discussed at the same time as Upptåget but not put into practice.[15] The second is the corridor between the principal settlement in Östhammar municipality, Östhammar town, and Uppsala.[16] We thus have a "treatment corridor" north of Uppsala, and two "control corridors" to the west and the northeast respectively. Analogous to the proximity to stations for the treatment group, we define the potential control group to be individuals who at the end of 1989 lived in SAMS with population centers within 4,500 meters of the main road between Uppsala and Heby on one hand and between Uppsala and Östhammar on the other, and more than 10,000 meters from the central parts of Uppsala city.[17] The "treatment" and "control" corridors are shown by the shaded areas in Figure 1.

The stretch between Uppsala and Tierp, where Upptåget was introduced, and the stretches between Uppsala and Heby and Uppsala and Östhammar respectively are

SAMS classification came into use in 1994 and has remained unchanged since then apart from minor adjustments, for example to adapt the SAMS borders to municipal borders. Information from before 1994 can be located to a SAMS by use of the more precise coordinates that real estates have in Sweden. For more information, see Statistics Sweden (2005).

[15] The railroad tracks between Heby town and Uppsala city were used for long distance trains that did not stop in Heby town or anywhere between Heby town and Uppsala city. In the investigation preceding Upptåget it was found that half-hour traffic Heby-Uppsala would be very uneconomic and require four trains, while half-hour traffic Tierp-Uppsala would only require three trains.

[16] Henceforth, Tierp, Östhammar, and Heby will be shorthand for the towns with the same name.

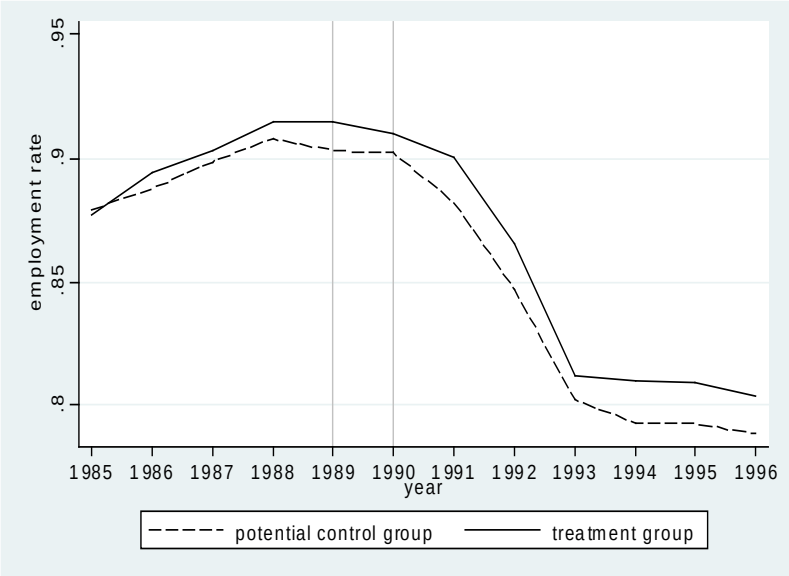
[17] The largest part, 95 percent (88 percent), of the individuals in the potential control group in the end of 1989 lived in a SAMS with a population center no further than 4500 meters (2000 meters) from one of the settlements along the main roads.

countryside with some smaller settlements. In 1990, Tierp had around 5,000 inhabitants, Heby around 2,500 inhabitants, and Östhammar around 6,000 inhabitants, while Uppsala city had around 110,000 inhabitants (Statistics Sweden, 1992). During the period we use in our analysis, 1985–1996, regional buses were the only type of public transit in the corridor between Uppsala and Heby and between Uppsala and Östhammar. In 1997 some long-distance/regional trains between Linköping and Uppsala, the "UVEN trains", began stopping in Heby and another settlement in the corridor between Heby and Uppsala (Morgongåva). The number of connections were few, and the frequency and maintenance not in the hands of Heby municipality or Uppsala County Council. Nevertheless, to avoid the risk that the UVEN trains could influence our estimates, we have chosen 1996 as the last year in which we investigate the effects of the introduction of Upptåget. We think that a period of 6.5 years from the offer of improved commuting opportunities, whereof Upptåget was up and running 5.5 years, should be enough to detect effects from the train on the labor market outcomes of the treatment individuals.

Here it can also be mentioned that to our knowledge the road network in and around the Upptåget corridor, the Heby corridor, and the Östhammar corridor remained largely unchanged from 1985 until well after 2000.[18]

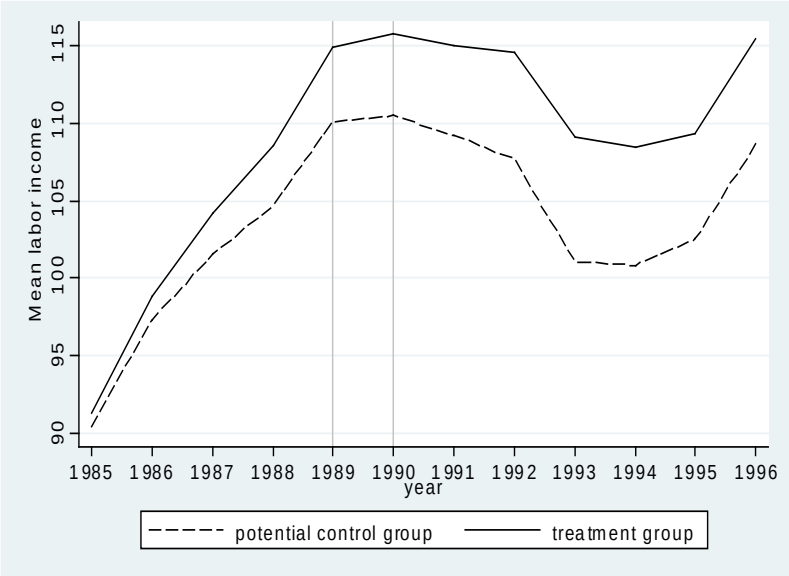
[18] From the late 1990s, plans for a new section of the European E4 route northwards from Uppsala were outlined, but construction did not start until 2002 and it was only in 2007 that it was entirely ready for use.

Figure 2: Employment rate for the treatment group and the potential control group



Notes: The measures refer to individuals aged 22-57 in 1989 and for whom we have data for at least the years 1987-1989. The vertical line for 1989 shows when the individuals were selected. The vertical line for 1990 shows the last year before Upptåget started running.

Figure 3: Mean annual labor earnings for the treatment group and the potential control group (1000 SEK in 1989 prices)



Notes: The measures refer to individuals aged 22-57 in 1989 and for whom we have data for at least the years 1987-1989. The vertical line for 1989 shows when the individuals were selected. The vertical line for 1990 shows the last year before Upptåget started running.

Figure 2 and Figure 3 show the raw employment rate and the mean annual earnings from labor for the treatment group and the potential control group for the period 1985–1996 (see Appendix A for variable definitions). From the figures it can be seen that both in the treatment group and the potential control group, the employment rate and the

mean labor income decreased markedly at the beginning of the 1990s—Sweden went from a booming economy in the late 1980s to a deep recession in the early 1990s. Furthermore, it can be seen that although the employment rate and the mean labor income were similar in the treatment group and the potential control group in 1985, the trends in the two groups before the introduction of the commuter train Upptåget were somewhat different. To handle the different trends, we therefore combine a difference-in-differences approach with a matching strategy in order to obtain treatment and control groups that are balanced in terms of labor market history as well as in terms of age, sex, education, birth region, and industry. The empirical strategy is further described in the next section.

4 Empirical strategy and data

4.1 Intention to treat

There are several well-known methodological problems associated with empirical investigations of the effect of commuting opportunities and job access on labor market outcomes. First, endogeneity can be a problem. Economic theory suggests that individuals simultaneously choose their job, residential location, and commuting behavior. Self-selection and unobserved heterogeneity, e.g. unobserved productivity such as motivation or perseverance may be the reason that individuals living in places with better commuting opportunities/job access have better labor market outcomes. As noted by Åslund, Östh, and Zenou (2010), residential sorting can also lead researchers to understate the impact of job access, e.g. if residential amenities are better in locations with worse job access, or if the low-skilled are forced to live close to jobs due to transportation restrictions. Similarly, workers with jobs or higher earnings may choose residential locations with poor job access in order to consume larger amounts of housing at a lower price, as hypothesized by the standard urban economic model.

Furthermore, as noted by Ihlanfeldt (2006), there may also be reversed causality running from labor market outcomes to job access, so that the better labor market outcomes of workers in some areas attract firms to these locations, implying better job access there. With regards to transport links and commuting opportunities there could be reversed causality running from labor market outcomes to the introduction of transport links or other changes in commuting opportunities. Policy makers could

improve commuting opportunities from areas where workers have bad labor market outcomes to help these areas. Alternatively, policy makers could improve commuting opportunities from areas where workers have good labor market outcomes to further strengthen these areas or, by improving public transit, to decrease commuting by car. Also, profit-maximizing transport companies could choose to provide more services to areas with high employment rates and high incomes, implying more trips and the possibility to set higher fares.

To obtain a better estimate of the effect of public transit infrastructure on individual labor market outcomes, as explained in section 3.2, we compare the outcomes of individuals who in 1989 lived in proximity to the pre-existing railroad upon which Upptåget began running with the outcomes of individuals in the same local labor market who in 1989 lived in places that were not subject to changes in transport infrastructure. Given the institutional setting described in section 3.1, we think it is a reasonable assumption that these individuals did not choose their 1989 location based on the train. Conditional on a set of control variables (see section 4.2), the introduction of the commuter train thus provides variation in (offered) commuting opportunities exogenous to individuals' labor market outcomes. Furthermore, the labor market outcomes of the 1989 individuals do not seem to have been very important for the location and timing of the train. Studying the 1989 individuals thus alleviates both omitted variable bias and the problem of reversed causality.

In our analysis, the individuals in the treatment and potential control groups are traced forward to 1996 regardless of where they lived in other periods. We thus perform a reduced-form analysis, allowing the introduction of the commuter train to influence labor market outcomes through any channel. In other words, we estimate the intention-to-treat (ITT) effect, which here is the effect of being offered improved commuting opportunities by public transit, regardless of whether an individual actually came to live in proximity to an Upptåget station once the train was up and running. From a policy perspective, this parameter should be of direct interest.

4.2 Methodology

As explained above, the individuals in the treatment and potential control groups did not choose whether or not they would be offered improved commuting opportunities, which should alleviate omitted variable bias. Nevertheless, it turns out that the two groups are

rather different with respect to observed characteristics in 1989 (see Table 1, the rows "unmatched"), and potentially still with respect to unobserved characteristics. Given that, for example, people with different ages and education levels can be expected to have different developments of employment and labor income, this could help explain the diverging trends between the treatment and potential control groups before treatment assignment (as shown in Figure 2 and Figure 3). For the exogeneity assumption to hold, it thus seems important to control for these differences when estimating the ITT-effect (the effect of being offered improved commuting opportunities by public transit). To estimate the ITT we therefore use a difference-in-differences matching estimator (DIDM) (Heckman, Ichimura and Todd, 1997).[19] This type of estimator is analogous to the standard difference-in-differences (DID) regression estimator, but does not impose functional form restrictions in estimating the conditional expectations of the outcome variable, and reweights the observations according to the weighting function implied by the matching estimator.[20] The matching thus ensures that the treatment and control group are balanced in terms of observed characteristics, while the DID approach controls for unobserved but temporally invariant characteristics remaining after matching. [21]

To be precise, we use a DID propensity score matching estimator. The estimator requires that:

$$E(Y_{0t} - Y_{0t'} | P, Z = 1) = E\left(Y_{0t} - Y_{0t'} | P, Z = 0\right) \quad (1)$$

where Y_0 is outcome conditional on non-assignment to treatment, t and t' are time periods before and after the treatment assignment respectively, P is the propensity score i.e., the probability of treatment assignment, Z is treatment assignment status, with $Z=1$ for the treatment group, and $Z=0$ for the control group. The estimator also requires that a match can be found for each individual in the treatment group:

$$\Pr\left(Z = 1 | X\right) < 1 \quad (2)$$

where X is a set of observable conditioning variables. Equation (2) must hold in both period t and period t'.

[19] The description of difference-in-difference matching estimators in this and following paragraphs relies heavily on Smith and Todd (2005).
[20] Matching techniques are traditionally used to overcome selection bias in non-experimental settings. Here, however, we mainly use matching to balance the treatment and potential control groups in terms of observed characteristics.
[21] For comparison, in the results section 5.1 we also show some results using only a matching estimator and only a difference-in-differences estimator.

The difference-in-differences propensity score matching estimator we use is constructed in the following way: first, the propensity score is estimated using a logistic model. Second, nearest neighbor matching, with replacement and ties, on the propensity score is used to match each treatment group individual to an individual in the potential control group. Third, the difference in outcome between the treatment and matched control groups after treatment assignment is compared to the mean difference in outcome between the treatment and matched control groups for 1985-1989. The estimator can be written as:

$$\hat{a}_{DIDM} = \frac{1}{n_1}\left\{\sum_{i \in I_1 \cap S_P}\left(Y_{1ti} - \overline{Y_{0t'i}}\right) - \sum_{j \in I_0 \cap S_P} W(i,j)\left(Y_{0tj} - \overline{Y_{0t'j}}\right)\right\} \quad (3)$$

where *i* and *j* denote individuals, I_1 denotes the set of individuals in the treatment group, I_0 the set of individuals in the potential control group, S_P the region of common support, n_1 the number of persons in the set $I_1 \cap S_p$, $W(i,j)$ are weights given by the nearest neighbor propensity score matching, and $Y_{0t'i}$-bar and $Y_{0t'j}$-bar are the average outcome 1985-1989 for individual *i* and *j* respectively[22].[23]

How to specify the propensity score is not obvious, and the specification of the propensity score could be important. For example, Smith and Todd (2005) and Heckman et al. (1997) find that, in their data, which variables are included in the estimation of the propensity score can make a substantial difference to the performance of an estimator. To choose the specification of the propensity score we use a version of an algorithm for stepwise regression proposed in Imbens and Rubin (2014). Briefly described, we start by estimating a logistic model with only a constant and then iteratively try adding variables to the model, first linear variables and then interaction variables, from a set of *X*. Variables are selected for inclusion in the propensity score depending on the likelihood ratio test statistic. The set of *X* consists of pre-treatment

[22] We think that taking the average over 1985-1989 gives a better estimate of unobserved, temporally invariant characteristics than using a single year.

[23] In the estimations, we use the robust standard errors derived by Abadie and Imbens (Abadie & Imbens, 2006, 2011, 2012) with two matches. It should be noted that these standard errors do not correct for potential correlations in unobserved shocks across individuals. Taking account of clustering in matching models is not a straightforward task (see Hanson & Sunderam, 2012, for a discussion about this). Hanson & Sunderam (2012) propose a version of the Abadie and Imbens standard errors in the presence of clustering. Like the Abadie and Imbens standard errors, the proposition of Hanson and Sunderam builds on matching within the treatment group, with the difference that the matching is done between different clusters, e.g., SAMS, within the treatment group. So far, the Hanson and Sunderam way of estimating the standard errors has not been much used in applied work. One reason for this might be that there is a trade off in their way of estimating the standard errors; matching across clusters decreases the potential clustering problems but might, at the same time, also decrease the match quality and thereby the overall quality of the standard errors. For the estimations, we have used the Stata 13 command "teffects psmatch".

assignment variables that we think may affect the development of labor market outcomes even in the absence of the introduction of the commuter train: sex, birth region, dummy variables for age group in 1989 and education level in 1990.[24] Given that the deep recession that Sweden experienced in the early 1990s, mirrored in Figure 2 and Figure 3, affected some industries more than others, the set of *X* also contains dummy variables for the broadly defined industry in which an individual worked in 1989. Finally, the set *X* also includes pre-treatment assignment, i.e., 1985-1989, labor income and employment status, to handle differences in trends not accounted for by the other variables in *X*. See Table 1 for a list of the *X* variables.[25]

We estimate Equation (3) for the whole treatment group as well as for a range of sub-groups, each time re-estimating the propensity scores for the sub-group under study.[26]

4.3 Data

The study uses population-wide longitudinal register data, compiled for research purposes by Statistics Sweden, and held by IFAU. Among other things, the data contain rich and detailed information on demographic characteristics, income, employment, and education. For this study, it is particularly important that we also have access to geographic information on the workplace and residential location of each individual. This information is available at the SAMS level (see discussion on SAMS in Section 3.2). The individual's locations are measured at the end of each year.

The individuals who lived in the Upptåget corridor in 1989, i.e., the treatment group, and the individuals who lived in the Heby corridor and the Östhammar corridor in 1989,

[24] The reason for using education level in 1990, which is not strictly a pre-treatment assignment variable, is that the 1990 data on education can be thought to be of higher quality than the 1989 data on education. The reason for the difference between the 1989 and 1990 data on education is that the 1989 data only rely on administrative registers while the 1990 data are supplemented with information from the 1990 census. Information on education in 1989 is missing for about 3% (4%) of the individuals in the treatment group (the potential control group), while education in 1990 is missing for less than 1% of the individuals in both the treatment and potential control groups. Given the choice between using incomplete data, not conditioning on education at all, and using education in 1990 which could to some extent be influenced by treatment assignment (but not by treatment), we have chosen the latter option.

[25] The variables labor income in 1985, 1986, 1987, 1988, and 1989 are tried separately as are the variables employment in 1985, 1986, 1987, 1988, and 1989. The dummy variables for 1990 education level, age group in 1989, birth region, and industry in 1989 respectively, are tried as a group, i.e., either dummy variables for all education levels are included or no dummy variables for education level are included. A linear variable/group of linear variables is included if its likelihood ratio test statistic is larger than the likelihood ratio test statistic for the other tried linear variables/groups of linear variables and larger than 1 ("Clin" in Imbens, 2014). Concerning the interaction variables, only interaction between the linear variables/groups of linear variables selected for inclusion in the propensity score are tried. Interaction variables/groups of interaction variables are included if its likelihood ratio test statistic is larger than for the other tried interaction variables/groups of interaction variables and larger than 3 ("Cqua" in Imbens, 2014). The choices of Clin and Cqua are somewhat arbitrary; Clin is the same as in Imbens 2014 while Cqua is set slightly higher than in Imbens 2014 to limit the number of interaction variables included. The labor income variables are tried in levels and not in natural logarithms in the algorithm.

[26] The densities of the estimated propensity score for the groups analyzed are given in Appendix B.

i.e., the potential control group, are traced backwards to 1985 and forward to 1996 regardless of where they lived in years other than 1989. We only consider the individuals who were of employable age (18-64) over the whole period 1985-1996, i.e., who were at least 22 and no older than 57 at the end of 1989. Furthermore, in order to have some pre-treatment information for the matching and difference-in-differences analysis, we limit our sample to individuals for whom we have data for at least the years 1987-1989, i.e., who lived in Sweden during that period.[27] The number of individuals aged 22-57 living in the Upptåget corridor in late 1989 was 7,989, and of these 7,934 lived in Sweden in 1987-1989. The corresponding number for the Heby and Östhammar corridors together are 11,493 and 11,341. Concerning attrition, people should only disappear from the original data set if they die or leave Sweden. We have coded individuals missing in a given year as not being employed and as receiving no labor income that year. Of the individuals in the data for 1989, less than 1 percent were missing from the data for 1985 and just above 2 percent from the data for 1996.

Table 1 summary statistics

		mean		% reduction		t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.50	0.48	4.2		2.85	0.004
	Matched	0.50	0.50	-0.1	97	-0.08	0.937
age group 1989							
20-29 years	Unmatched	0.17	0.20	-7.5		-5.06	0
	Matched	0.17	0.18	-2.3	68.8	-1.5	0.135
30-39 years	Unmatched	0.32	0.30	4.7		3.24	0.001
	Matched	0.32	0.32	0.2	96.5	0.1	0.919
40-49 years	Unmatched	0.36	0.34	5.5		3.73	0
	Matched	0.36	0.36	1.1	79.6	0.7	0.487
50-57 years	Unmatched	0.15	0.17	-5.2		-3.51	0
	Matched	0.15	0.14	0.8	83.8	0.54	0.588
education 1990							
10 years or	Unmatched	0.29	0.34	-10.3		-7	0
less	Matched	0.29	0.28	2.4	76.7	1.55	0.121
secondary	Unmatched	0.47	0.48	-2.6		-1.77	0.077
	Matched	0.47	0.47	-0.7	73.7	-0.43	0.667
tertiary	Unmatched	0.24	0.18	15.1		10.41	0
	Matched	0.24	0.25	-1.6	89.5	-0.94	0.345
missing	Unmatched	0.01	0.01	-0.8		-0.56	0.578
	Matched	0.01	0.01	-1.6	-93.7	-0.97	0.33
birth region							
Sweden	Unmatched	0.94	0.92	10		6.7	0
	Matched	0.94	0.94	1.8	82.1	1.21	0.228
western	Unmatched	0.04	0.07	-11.3		-7.55	0
country	Matched	0.04	0.05	-1.9	82.7	-1.35	0.178
non-western	Unmatched	0.01	0.01	0.8		0.55	0.583
country	Matched	0.01	0.01	0	100	0	1

[27] To be able to examine how the very youngest (those aged 19-21 in 1989) are affected by the introduction of Upptåget, we also carry out another selection of individuals in the analysis of sub-populations (see Section 5.2; last panel in Figure 8).

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
industry 1989							
manufacturing	Unmatched	0.16	0.18	-5.9		-4.02	0
	Matched	0.16	0.17	-1.3	77.9	-0.84	0.402
construction	Unmatched	0.07	0.07	1.4		0.93	0.35
	Matched	0.07	0.07	0.7	52.3	0.41	0.684
trade	Unmatched	0.10	0.09	2.4		1.65	0.098
	Matched	0.10	0.10	-0.7	69.2	-0.46	0.647
education	Unmatched	0.06	0.05	3.8		2.6	0.009
	Matched	0.06	0.07	-2	48.2	-1.17	0.241
health care	Unmatched	0.12	0.09	8.2		5.68	0
	Matched	0.12	0.12	0.1	99	0.05	0.96
other types of	Unmatched	0.07	0.08	-4.9		-3.29	0.001
care	Matched	0.07	0.07	0	99	0.03	0.974
public admin-	Unmatched	0.06	0.04	6.7		4.64	0
istration	Matched	0.06	0.05	1.8	73.5	1.07	0.287
other	Unmatched	0.21	0.25	-9.2		-6.23	0
	Matched	0.21	0.20	0.8	91.8	0.49	0.623
unknown/did	Unmatched	0.17	0.15	3.9		2.66	0.008
not work	Matched	0.17	0.16	0.7	82.2	0.43	0.669
employed	Unmatched	0.88	0.88	-0.8		-0.56	0.579
1985	Matched	0.88	0.87	2.8	-246.2	1.74	0.083
employed	Unmatched	0.89	0.89	2.1		1.42	0.156
1986	Matched	0.89	0.89	1.7	20.5	1.05	0.296
employed	Unmatched	0.90	0.90	1.5		1.03	0.304
1986	Matched	0.90	0.90	1.5	-1.8	0.97	0.334
employed	Unmatched	0.91	0.91	2.4		1.64	0.101
1988	Matched	0.91	0.91	1.2	48.4	0.79	0.43
employed	Unmatched	0.91	0.90	3.9		2.67	0.008
1989	Matched	0.91	0.91	2.9	26.5	1.83	0.067
ln labor in-	Unmatched	3.62	3.62	0		-0.03	0.976
come 1985	Matched	3.62	3.53	2.9	-6599.2	1.8	0.072
ln labor in-	Unmatched	3.75	3.73	0.7		0.48	0.631
come 1986	Matched	3.75	3.70	1.7	-141.7	1.06	0.291
ln labor in-	Unmatched	3.83	3.78	1.8		1.26	0.209
come 1987	Matched	3.83	3.81	0.9	50.5	0.57	0.566
ln labor in-	Unmatched	3.92	3.84	3.1		2.13	0.033
come 1988	Matched	3.92	3.89	1.3	58.2	0.82	0.41
ln labor in-	Unmatched	3.99	3.84	5.4		3.7	0
come 1989	Matched	3.99	3.96	1.3	76.8	0.82	0.414
Individuals in sample	Unmatched	7933	11341				

Notes: a) Western countries include: Finland, Norway, Denmark, Iceland, Ireland, Great Britain, the Netherlands, Belgium, France, Monaco, Luxembourg, Lichtenstein, Spain, Portugal, Andorra, Germany, Switzerland, Italy, San Marino, Vatican City State, Austria, Greece, Canada, the USA, Australia, Papua New Guinea, New Zealand and other Oceanian countries.

b) To be able to take logarithms, individuals with zero labor income were ascribed 1 SEK.

c) The full sample includes one more individual with a covariate, working in education in 1989 combined with education for 1990 missing, which predicts treatment perfectly. This individual is therefore dropped before the analysis.

Table 1 presents some summary statistics for the treatment group, the potential control group and the matched control group.[28] Table 1 shows for each variable the mean in the treatment group, the potential (unmatched) control group, and the matched control group; the t-test for equality of means in the unmatched and matched samples, and the

[28] Corresponding tables for the sub-groups can be found in Appendix C.

percentage standardized bias in the unmatched and matched samples along with the percentage reduction in absolute bias between the samples.[29] The standardized bias is a scale and sample size free way of assessing overlap (Imbens 2014).[30]

From Table 1, it can be seen that the standardized biases indicate that there are some substantial differences in the average covariate values between the treatment and the unmatched control group. This is especially the case for education: the percentage standardized bias for tertiary education (at most 10 years of education) is 15.1 (-10.3). It is also the case for birth region where the percentage standardized bias for being born in Sweden (a western country) is 10 (-11.3). There are also some differences with respect to the industry in which the treatment and unmatched control groups worked in 1989: the percentage standardized bias for health care (public administration) (manufacturing) is 8.2 (6.7) (-5.9). Furthermore, there are some differences with respect to age groups where the absolute values of the percentage standardized bias are between 4.7 and 7.5, and for gender where the percentage standardized bias is 4.2. With respect to employment in 1985-1989, the differences in standardized bias between the treatment and control groups were relatively small in the unmatched sample; the absolute values of the standardized bias were between 0.8 and 3.9. The same holds for ln labor income 1985-1989.

In the matched sample the absolute values of the percentage standardized bias are always under 3 percent and for most covariates smaller than in the unmatched sample. One exception is "education 1990 missing" where the absolute value of the percentage standardized bias has increased from 0.8 to 1.6. The bias can, however, be considered small also in the matched sample. Another exception is employment, where the absolute value of the percentage standardized bias is higher in the matched sample than in the unmatched sample in 1985, although still under 3 percent. More important, however, is

[29] The standardized percentage bias, suggested by Rosenbaum & Rubin (1985) is the percentage difference of the sample means in the treatment group and the control group (unmatched or matched) as a percentage of the square root of the average of the sample variances in the treatment and control groups, $((X_1-X_0)/(0.5*(V_1(X)+V_0(X))^{1/2})*100$, where $V_1(X)$ is the mean (variance) in the treatment group and $V_0(X)$ the analogue for the control group. This measure seems to go under different names. Imbens (2014) calls it normalized differences and Smith & Todd (2005) standardized differences.

[30] As Imbens (2014) explains, the t-statistic for testing the null hypothesis that a difference is zero may be large in absolute value simply because the sample is large and, as a result, small differences between the two samples' means are statistically significant even if they are substantively small. Large values for the standardized bias, in contrast, indicate that the average covariate values in the two groups are substantially different. There are no clear indications of what is a "large" difference, but in his applications Imbens (2014) seems to consider a standardized bias of above 10 percent as substantial, whereas Caliendo & Kopeinig (2008) write that in most empirical studies a standardized bias below 3 or 5 percent is seen as sufficiently small.

the fact that the trends in the treatment and matched control groups are similar: in both the treatment and matched control group the employment rate increased by 0.04 from 1985 to 1989 while the increase in the unmatched control group was only 0.02. For labor income, the absolute values of the percentage standardized bias for labor income are higher in the matched than in the unmatched sample in 1985 and 1986, although once again under 3 percent. Furthermore, the trends in the treatment and matched control groups are similar: mean ln labor income increased by 0.38 in the treatment group and 0.42 in the matched control group while the increase in the unmatched control group was 0.23. In sum, the matching strategy we employ seems to do a good job in creating a sample that is well balanced in terms of age, education, sex, birth region, and 1989 industry, and in ensuring that the developments in employment and labor income between the treatment and the matched control group were similar prior to treatment assignment.

5 Baseline results

In this section we present the "intention to treat" (ITT) estimates, i.e., the effects of the introduction of Upptåget on the employment status and earnings of the individuals who lived close to the railroad tracks just before the commuter train was instigated. The first section presents the mean effects in the whole population, the second section presents the mean effects in different subpopulations, and the final section provides some sensitivity analyses of the baseline results.

5.1 ITT estimations for the whole population

Figure 4 presents the ITT estimates on the employment probability and on the natural logarithm of labor income for the whole studied population living in the treated area in the year before it was revealed that the commuter train was going to be instigated (1989—the year indicated by the first vertical line in the figure; the second vertical line indicates the last year before the commuter train started operating (1990)).[31] The year-specific estimates, linked by the solid line, are obtained from the matched difference-in-differences estimator given in equation (3). That is, each year specific-estimate shows the difference in outcome between the treatment group and the matched control group in

[31] To be able to take logarithms, individuals with zero labor income were ascribed 1 SEK.

that year, over and above the mean difference in outcome 1985-1989. The dashed lines show the 95 percent confidence interval.

From the figure, it can first be noted from the pre-trends that the matching procedure does a good job in balancing the observations in the treatment and control groups. The point estimates are insignificant and close to zero in the whole pre-period, indicating that the development of employment probability and annual labor earnings among the individuals in the treated area are very similar to their matched "twins" in the control areas before the news about the future commuter train was released. This was not unexpected given the summary statistics in Table 1.

Second, it seems like the introduction of the commuter train had, on average, no effects on employment probability and labor income for those individuals that were intended to be treated. For earnings, the point estimates are insignificant at the five percent significance level for all years. For the employment probability, the point estimates are insignificant for all years but one (it is barely significant at the five percent significance level in 1993). The only significant ITT estimate is negative, and indicates a 1.5 percentage point decrease in the employment probability in 1993.[32] We consider the general message from Figure 4 to be that for the overall population, the introduction of the commuter train had very little impact on the employment probabilities and earnings among treated workers.

Figure 4: Effects on employment probability and labor earnings for the whole population

Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see Equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 7933 treatment individuals and 11341 potential control individuals.

[32] Of course, we cannot rule out that the significant estimate is obtained by chance; since we estimate many point-estimates, some of them will, by chance, turn out to be falsely significant.

It can be instructive to compare the matched difference-in-differences estimates with the estimates obtained when using only a matching estimator (Figure 5) or only a difference-in-differences estimator (Figure 6). From Figure 5 it can be seen that the point estimates from the matching estimator follows the same patterns as the point estimates from the matched difference-in-differences estimator (cf. Figure 4). However, in the pre-treatment period the point estimates from the matching estimator lies further from zero than the point-estimates from the matched difference-in-differences estimator, indicating that there could be unobserved differences between treated and untreated observations important to take into account. Furthermore, the 95 % confidence interval is somewhat tighter from the matched difference-in-differences estimator than from the matching estimator.

Likewise, the pre-treatment estimates for the difference-in-differences estimator (Figure 6) show – as expected given the differences observed in Table 1 and Figures 2 and 3 – that the observations in the treatment and control groups are not well balanced before treatment. In particular, there seems to be a positive pre-treatment trend, with significant differences in the last years of the pre-treatment period for labor earnings, which cast serious doubt on the positive and significant point estimates observed in the post-treatment period.

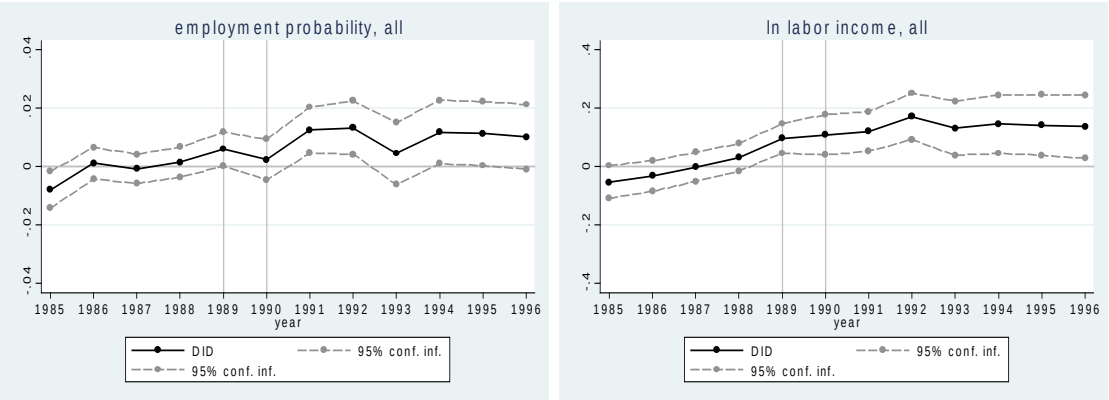
Hence, the matched difference-in-differences estimator yields more reliable pre-treatment trends than the matching and the difference-in-differences estimators and should thus provide the most reliable estimates of the effects of the introduction of the commuter train on the affected individuals' labor market outcomes.

Next we will examine whether some sub-populations are more affected than others.

Figure 5: Effects on employment probability and labor earnings for the whole population when using only a matching estimator

Note: The figures show matching estimates: The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year. The sample used for the estimations contains 7933 treatment individuals and 11341 potential control individuals.

Figure 6: Effects on employment probability and labor earnings for the whole population when using only a difference-in-differences estimator



Note: The figures show difference-in-differences estimates: The point estimate for each year shows the difference in mean outcome between the treatment group and the *un*matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 7934 treatment individuals and 11341 control individuals.

5.2 ITT estimates for subpopulations

As discussed in the theoretical section we would expect the introduction of a commuter train to have larger effects on labor market outcomes among the groups that rely more on public transit and/or have stronger residential constraints, e.g., ethnic minorities and people with low incomes. In this section we will therefore report results from estimations on separate sub-populations to examine whether there are any heterogeneous effects. The sub-groups we consider are women (Figure 7), different age-groups (Figure 8), individuals who had low income before the introduction of the train (Figure 9), and different birth-regions (Figure 10). Generally speaking, two common themes emerge from the estimates in these figures. First, the pre-trends also look

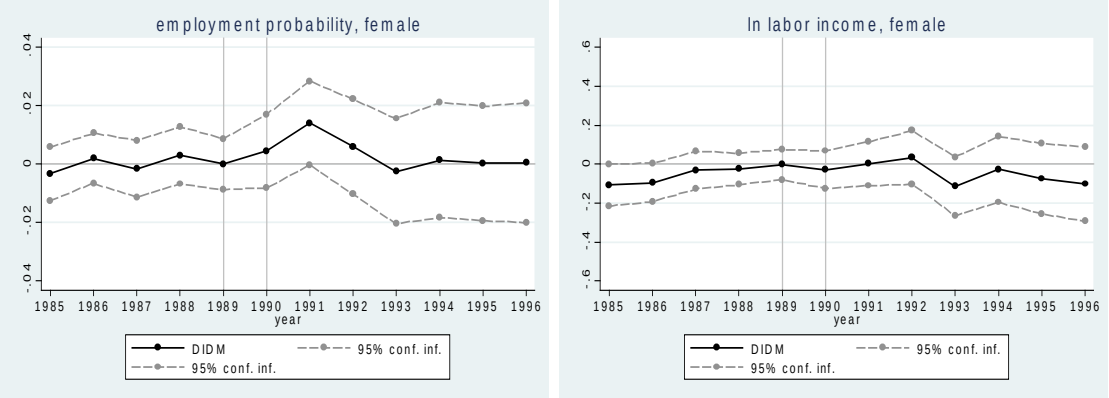
reasonable for the sub-groups, providing further support for the assumption that we have a good comparison group when using the matched difference-in-differences estimator. Second, there is very little support for the hypotheses that the commuter train increases employment probability and earnings from labor among these sub-groups.

For individuals age 22–29 in 1989 in the treated areas, there is a tendency to a negative development relative to their counterparts in the control group (cf. the panels in Figure 8).[33] However, only a few of the estimates are statistically significant. When we focus on those very young at the time of treatment, standard errors become large, and there is not as clear a trend in the point estimates. As for the scattered positive estimates found in other groups (women, age 40–49), we do not interpret this as considerable evidence on any impact.

For those born in a non-western country (cf. the last panel in Figure 10), the point estimates indicate a fairly stable, positive and large effect of the commuter train on employment as well as on earnings, but there is too much uncertainty in the estimates to be able to draw any clear conclusions (an uncertainty that probably stems from the fact that the group is fairly small; there are 94 individuals in the treatment group). One reason for highlighting the estimated pattern for the non-western group is that this is perhaps the group where we would *a priori* be most likely to find an impact: employment outcomes are poor, meaning that only a few people entering employment may make a difference, and access to alternative transport (car ownership, co-driving, etc.) may be lower.

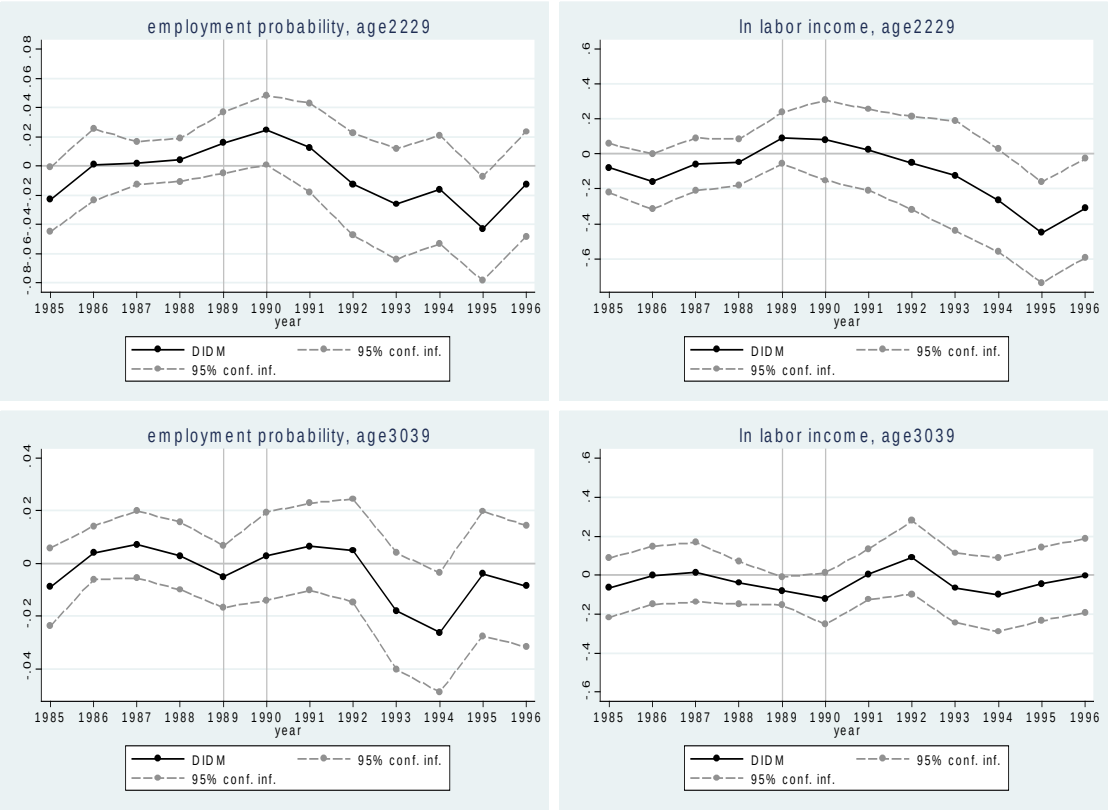
[33] Note that in the last panel in Figure 8, we carry out a selection of individuals other than in the baseline analysis to be able to examine how the very youngest (those aged 19-21 in 1989) are affected by the introduction of Upptåget.

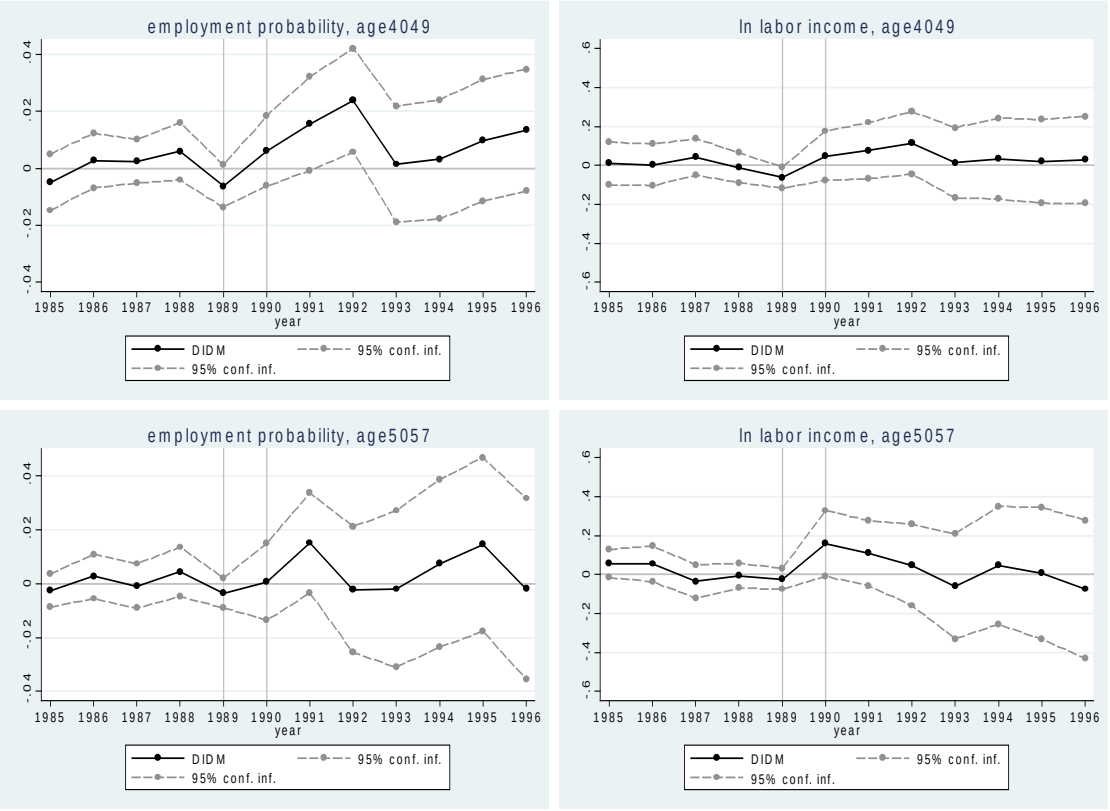
Figure 7: ITT estimates on employment probability and labor earnings: women



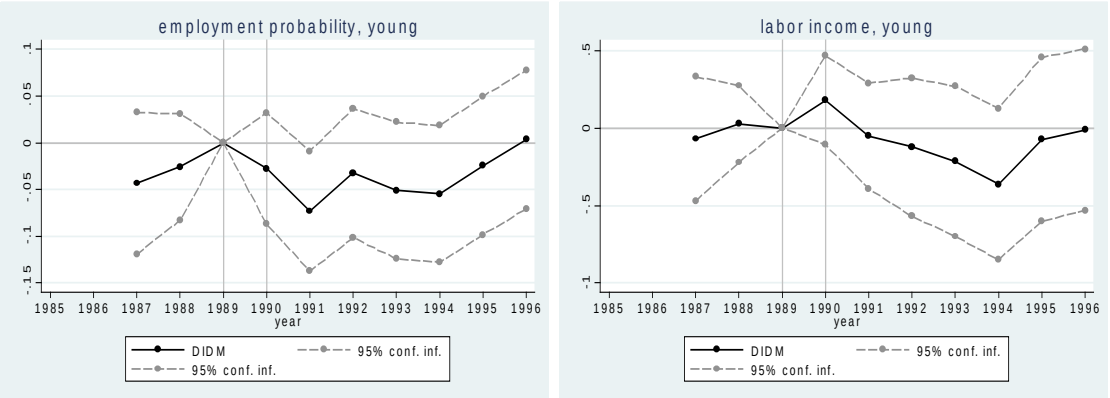
Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 3954 treatment individuals and 5419 potential control individuals.

Figure 8: ITT estimates on employment probability and labor earnings: by age



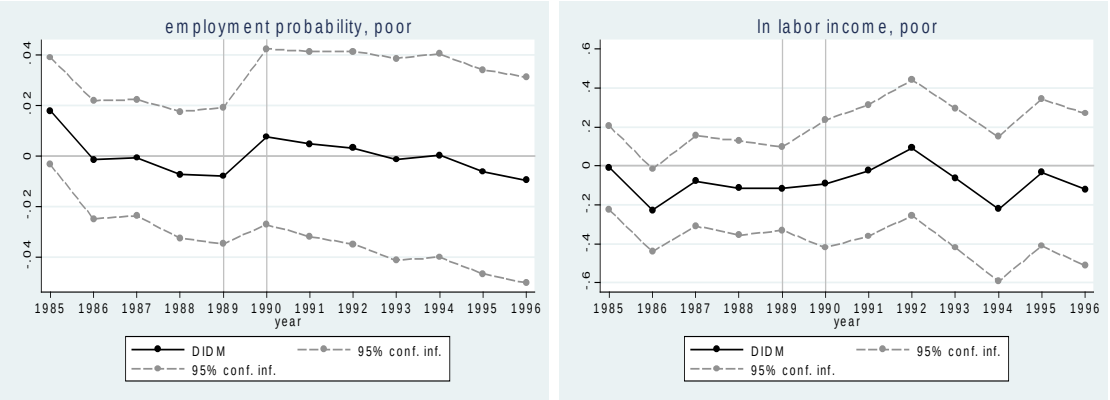


Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations for age 2229 contains 1369 treatment individuals and 2282 potential control individuals. The corresponding figures for age 3039 (4049) [5057] are 2526 (2859) [1152] and 3362 (3800) [1857] respectively.



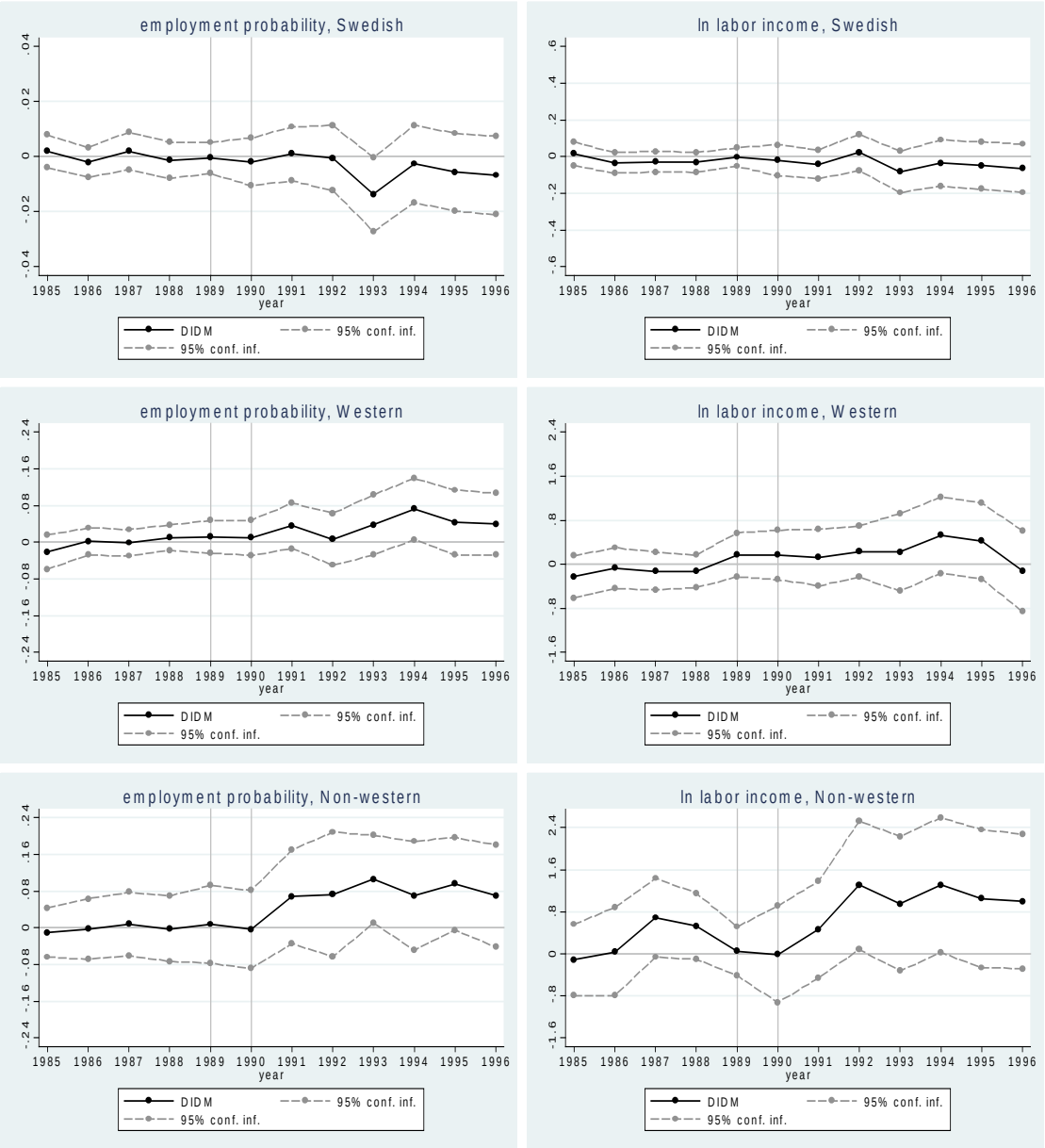
Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1989. The sample used for the estimations contains 647 treatment individuals and 897 potential control individuals. Young is defined as being 19-21 years old in 1989. For young people, the labor income and employment history variables in the set of *X* used in the algorithm to choose the propensity score only include values for 1988 and 1989.

Figure 9: ITT estimates on employment probability and labor earnings: for those with low incomes in 1989



Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 1484 treatment individuals and 2472 potential control individuals. An individual is defined as having low income if his or her labor income in 1989 was less than 50 percent of the median taxable income in 1989 among those for whom the Swedish Tax Agency had information (82% of the population) (see Statistics Sweden: Statistical Yearbook 1992, Table 221 "Income-earners by total net income and age").

Figure 10: ITT estimates on employment probability and labor earnings: by birth-region



Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations in the Swedish (Western) [Non-Western] subsample contains 7477 (351) [94] treatment individuals and 10401 (782) [125] potential control individuals.

6 Robustness checks and further analysis

In this section, we will conduct two further analyses. First, in Section 6.1, we perform some sensitivity checks by (i) dropping the observations from an area (Tierp) where commuting time was less affected than in other treated areas and (ii) dropping the observations from an area (Storvreta) that was already more integrated with Uppsala

city before the introduction of Upptåget. Then, in Section 6.2, we examine what effects the introduction of the commuter train had on commuting behavior. Finally, in Section 6.3, we check whether there are any indications of heterogeneous labor market shocks (i.e., shocks that hit the treatment area more strongly or differently than the control areas).

6.1 Robustness checks: treatment intensity

All individuals in the treatment area were offered proximity to the commuter train Upptåget and thereby improved commuting opportunities by public transit. While the commuter train was substantially faster than bus connections, the decrease in travel time by public transit was not the same everywhere in the Upptåget corridor. For the train stations between Tierp and Uppsala, travel times by public transit to Uppsala were approximately cut in half, which in 1996 represented an absolute gain of between 17 and 36 minutes depending on station. From Tierp, on the other hand, Upptåget decreased travel time to Uppsala by less than 30 percent compared to bus in 1992 and by less than 40 percent in 1996. Furthermore, the fastest public transit between Tierp and Uppsala was, both before and after the introduction of Upptåget, a long distance train that took about 40 minutes. It can therefore be argued that Upptåget had less of an effect on travel time by public transit between Tierp and Uppsala, even though it indeed increased the number of fast connections.[34]

Likewise, it can be argued that Storvreta, which is the Upptåget station closest to Uppsala, might have been less intensively treated than the other settlements since it was already more integrated with Uppsala before the introduction of Upptåget.

In this section we will therefore examine whether the results are sensitive to excluding either Tierp or Storvreta from the analyses.[35]

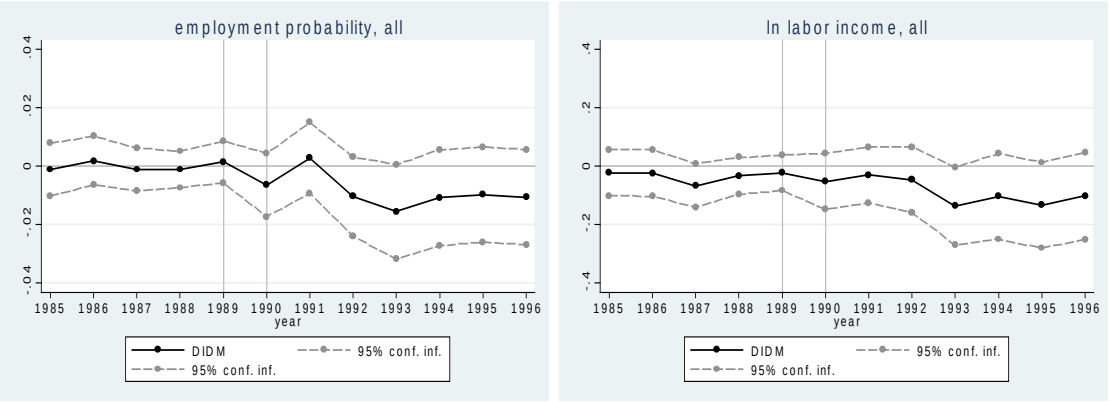
The results when using a sample of all individuals but excluding those who lived in Tierp (Storvreta) and corresponding control areas in 1989 are presented in Figure 11 (Figure 12). When comparing with the baseline results (cf. Figure 4), it is clear that we reach very similar conclusions; the pre-trends look reasonable, and there are essentially

[34] In 1989 long distance trains did 8 trips per weekday from Tierp to Uppsala. In 1992, Upptåget alone accounted for 8 trips, which had increased to 16 in 1996.

[35] We have also estimated models in which we make other exclusions, such as using only the individuals living in Uppsala municipality but not in Storvreta (and hence also excluding Tierp) and using only the individuals living in the municipalities Tierp, Heby, and Östhammar; none of these alterations changes the conclusions. These results are available upon request.

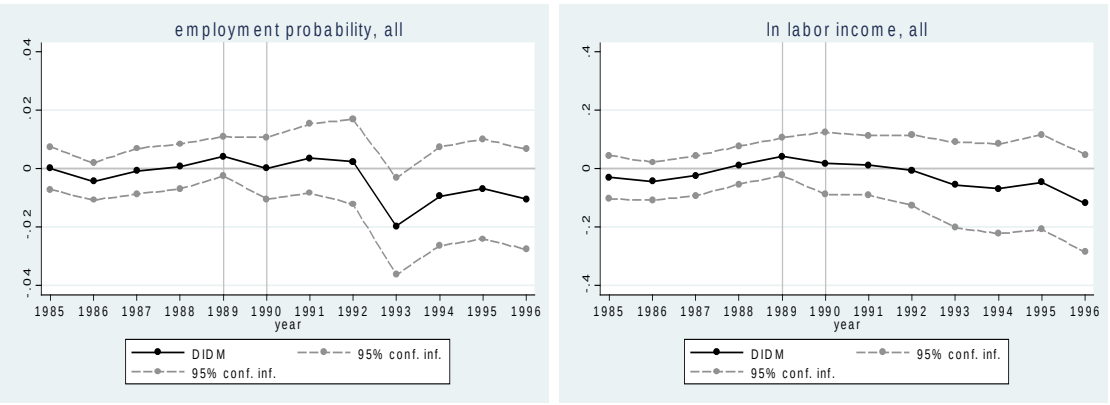
no significant effects (either statistically or economically) from the commuter train on the individuals' employment probability or labor earnings.

Figure 11: ITT estimates on employment probability and labor earnings: whole sample but excluding Tierp



Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 5085 treatment individuals and 8729 potential control individuals.

Figure 12: ITT estimates on employment probability and labor earnings: whole sample but excluding Storvreta



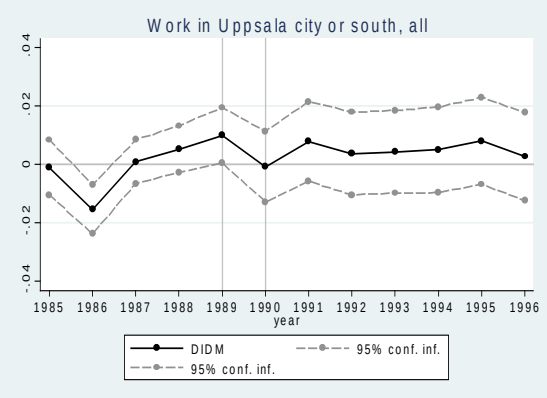
Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 5127 treatment individuals and 10462 potential control individuals.

6.2 Commuting patterns

Even though there appears to be little effect on employment and earnings among those who got access to the commuter train, it is possible that people altered their commuting behavior. To investigate this, we performed an analysis similar to those above, but with the probability of working in Uppsala city or further south (it should be recalled that the treatment and control areas are all somewhat north of the city, whereas the commuter-

receiving Stockholm region is to the south). We found very little impact on this probability; again suggesting small effects of the reform (cf. Figure 13).

Figure 13: ITT estimates on probability to work in Uppsala city or south, whole sample



Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the for groups 1985-1989. The sample used for the estimations contains 7933 treatment individuals and 11341 potential control individuals.

6.3 Heterogeneous labor market shocks

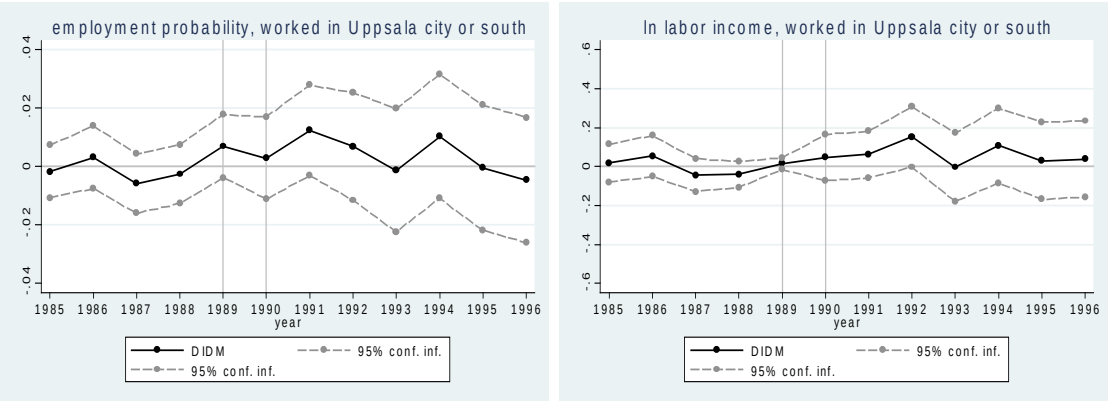
The identifying assumption in the analysis presented above is that without the introduction of the commuter train, the development of employment and earnings in the treatment group would have been the same as in the control group. As discussed above, several facts support this assumption: the institutional details and our study design suggest that we should not worry about self-selection into locations; the treatment and control areas are all part of a local labor market sharing the same employment center; the pre-reform comparisons indicate that the matched sample contains individuals with comparable development and responses to economic fluctuations (where the matching is also conducted on the pre-reform industry that the individuals worked in).

However, it should be noted that if there are geographically heterogeneous economic shocks over treatment and control areas that are not fully captured by the research design, we risk confusing the impact of the train with changes that would have happened anyway. The zero effect could then, for example, be the sum of a negative local labor market shock and a positive effect of the train. This is in principle an untestable assumption; we can never fully rule out the possibility that the treated areas are affected by different shocks than the control areas, even though the factors mentioned above point in another direction.

However, one way to check whether there are any indications of unaccounted for, heterogeneous, labor market shocks is to examine if the estimated effects for the treated individuals who were employed in the treatment area are different from those for the treated individuals who were employed outside the treatment area. The presumption is that, in the presence of heterogeneous negative labor market shocks in the treatment area, the treated individuals working in the treatment area would be more negatively affected than the treated individuals working outside the treatment area.

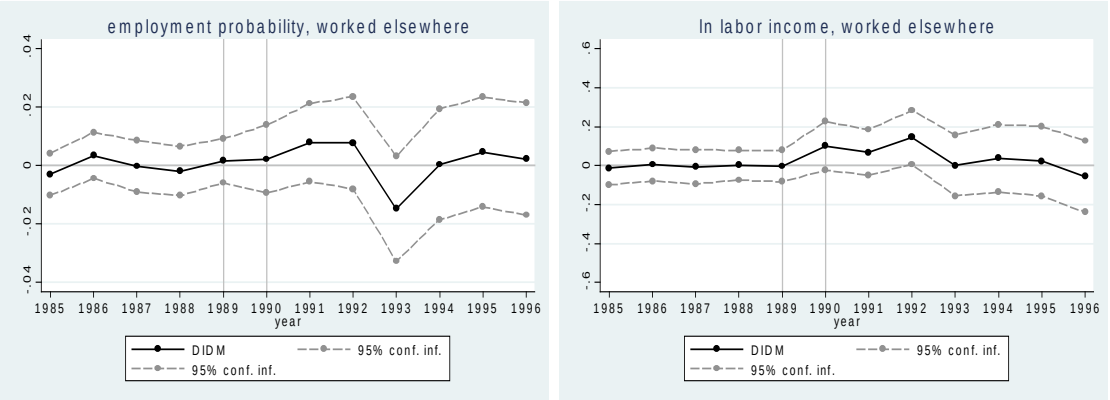
To examine this, we divide the treated individuals into those working in Uppsala or further south, towards the Stockholm region (these are the individuals working outside the treatment area) and those not working in Uppsala or further south (these are the individuals that mainly work close to the home, in the treatment area). As is clear from Figure 14 and Figure 15, the estimated coefficients for these two groups are similar and not significantly different from each other in any of the time periods; for none of the groups can we reject the null hypothesis that Upptåget had no effect on the treated individuals' labor market outcomes. This strengthens the assumption that, given the research design, there were no labor market shocks that affected the treatment area more strongly or differently than the control areas.

Figure 14: ITT estimates on employment probability and labor earnings: part of sample who worked in Uppsala city or south in 1989 (i.e., outside the treatment area)



Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 3441 treatment individuals and 3288 potential control individuals.

Figure 15: ITT estimates on employment probability and labor earnings: part of sample who did not work in Uppsala city or south in 1989 (i.e., mainly within the treatment area).



Note: The figures show the difference-in-differences matching estimates, $\alpha\text{-hat}_{DIDM}$ (see equation (3)): The point estimate for each year shows the difference in mean outcome between the treatment group and the matched control group that year, above the difference in mean outcome between the groups for 1985-1989. The sample used for the estimations contains 4485 treatment individuals and 8051 potential control individuals.

7 Concluding discussion

In this paper, we have investigated the importance of improved public transit for individual labor market outcomes. The introduction of a commuter train, Upptåget, between Uppsala and Tierp (54 km north of Uppsala) in Sweden in the early 1990s meant that individuals living in some areas were offered considerably decreased commuting times by public transit and increased job access to the regional labor center in Uppsala, whereas other individuals competing for jobs in the same local labor market did not experience a similar change.

We argue that institutional features suggest that the setting is suitable for evaluating the labor market impact of transport opportunities: timing was affected by a change of national law, the stretch was determined by a pre-existing railroad, and the time between the first discussion and implementation was relatively short. Our empirical analysis uses detailed longitudinal individual data to compare the development for individuals who lived in treated and non-treated areas the year before the information about the new commuter train was released. The intention-to-treat (ITT) estimates are obtained through a matched difference-in-differences estimator.

Our results suggest that the introduction of the commuter train essentially had no significant effects (either statistically or economically) on the employment probability or labor earnings for those individuals who lived in the treated area before the new

commuter train was announced. The only potential exception to this result is for the group of individuals who were born in a non-western country. For this group, the patterns of the ITT point estimates tend to a positive and large effect on both their employment probability and their labor earnings. However, since the non-westerners constitute a fairly small group in the studied area, there is a large uncertainty in the point estimates and the results should be interpreted cautiously. Nevertheless, the results are interesting in that the group is also one where we could expect greater effects according to theory: the economic position is on average poor, and access to alternative transport (car, co-driving etc.) is likely to be lower than for the average worker. This is a group that warrants more analysis in future studies.

We can only speculate on the reasons for the absence of empirical effects, despite rather clear theoretical effects. For the average worker, one could perhaps argue that it is reasonable to find limited effects in a context where public transport is also available prior to the introduction of the train, and considering that many individuals in the treated areas use private transport to get to work. We do of course not know whether effects would be more present in a context where public transportation was provided to a market with initially no or very limited public transportation. On the other hand, the type of case we study is a very common example facing policy makers.

When analyzing the costs and benefits of major infrastructural investments such as railroads and commuter trains, there are of course aspects other than increased employment and higher earnings to take into account. Less time spent on commutes is arguably also a welfare gain for those whose job and wage prospects are not affected at all. But our analysis provides a piece of the puzzle that is to a large extent missing in previous research and which is essential to any cost-benefit analysis. However, it is also important to acknowledge that we restrict our attention to the individual consequences for workers directly exposed to the reform. From a societal perspective, the effects on in- and out-migration and the regional economic impact are probably at least as relevant. This is a topic of another paper.

References

Abadie, A., & Imbens, G. W. (2006). Large sample properties of matching estimators for average treatment effects. *Econometrica, 74*(1), 235-267.

Abadie, A., & Imbens, G. W. (2011). Bias-corrected matching estimators for average treatment effects. *Journal of Business and Economic Statistics, 29*(1), 1-11.

Abadie, A., & Imbens, G. W. (2012). *Matching on the estimated propensity score*. Manuscript. Retrieved from *http://www.hks.harvard.edu/fs/aabadie/pscore.pdf* 2015-06-15.

Ahmed, A. M., & Hammarstedt, M. (2008). Discrimination in the rental housing market: A field experiment on the Internet. *Journal of Urban Economics, 64*(2), 362-372.

Alonso, W. (1964). *Location and land use: toward a general theory of land rent*, Harvard University Press. Cambridge, MA.

Arbetsgruppen Projekt Upptåget. (1988a). Upptåget - En förstudie.

Arbetsgruppen Projekt Upptåget. (1988b). Upptåget - förslag till länspendel Uppsala-Tierp.

Åslund, O., Östh, J., & Zenou, Y. (2010). How important is access to jobs? Old question—improved answer. *Journal of Economic Geography, 10*(3), 389-422.

Becker, G. S. (1965). A theory of the allocation of time. E*conomic Journal, 75*(299), 493-517.

Brueckner, J. K., & Zenou, Y. (2003). Space and unemployment: The Labor market effects of spatial mismatch. *Journal of Labor Economics, 21*(1), 242-262.

Caliendo, M. & Kopeinig, S. (2008). Some practical guidance for the implementation of propensity score matching. *Journal of Economic Surveys, 22*(1), 31-72.

Cervero, R., Sandoval, O., & Landis, J. (2002). Transportation as a stimulus of welfare-to-work. *Journal of Planning Education and Research, 22*(1), 50-63.

Coulson, N. E., Laing, D., & Wang, P. (2001). Spatial mismatch in search equilibrium. *Journal of Labor Economics, 19*(4), 949-972.

Gautier, P. A., & Zenou, Y. (2010). Car ownership and the labor market of ethnic minorities. *Journal of Urban Economics, 67*(3), 392-403.

Gibbons, S., & Machin, S. (2006). *Transport and labour market linkages: empirical evidence, implications for policy and scope for further UK research*: Commissioned for the Eddington Study.

Glaeser, E. L., Kahn, M. E., & Rappaport, J. (2008). Why do the poor live in cities? The role of public transportation. *Journal of Urban Economics, 63*(1), 1-24.

Gobillon, L., Selod, H., & Zenou, Y. (2007). The mechanisms of spatial mismatch. *Urban Studies, 44*(12), 2401-2427.

Gurley, T., & Bruce, D. (2005). The effects of car access on employment outcomes for welfare recipients. *Journal of Urban Economics, 58*(2), 250-272.

Hanson, S. G., & Sunderam, A. (2012) The variance of non-parametric treatment effect estimators in the prescence of clustering. *Review of Economics and Statistics, 94*(4), 1197-1201.

Heckman, J., Ichimura, H., & Todd, P.E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training program. *Review of Economic Studies, 64*(4), 605-654.

Ihlanfeldt, K. R. (2006). A primer on spatial mismatch within urban labor markets. In R. J. Arnott & D. P. McMillen (Eds.), *A companion to urban economics* (pp 404-417). Boston: Blackwell Publishing.

Ihlanfeldt, K. R., & Sjoquist, D. L. (1998). The spatial mismatch hypothesis: a review of recent studies and their implications for welfare reform. *Housing Policy Debate, 9*(4), 849-892.

Imbens, G. (2014). Matching methods in practice: three examples. NBER Working Paper no. 19959.

Imbens, G., & Rubin, D. (2015). *Causal Inference in Statistics, Social, and Biomedical Sciences*. Cambridge University Press.

Kain, J. F. (1968). Housing segregation, negro employment, and metropolitan decentralization. *Quarterly Journal of Economics, 82*(2), 175-197.

Kawabata, M. (2003). Job access and employment among low-skilled autoless workers in US metropolitan areas. *Environment and Planning A, 35*(9), 1651-1668.

LeRoy, S. F., & Sonstelie, J. (1983). Paradise lost and regained: Transportation innovation, income and residential location. *Journal of Urban Economics, 13*(1), 67-89.

Matas, A., Raymond, J.-L., & Roig, J.-L. (2010). Job Accessibility and Female Employment Probability: The Cases of Barcelona and Madrid. *Urban Studies, 47*(4), 769-787.

Mills, E. S. (1967). An aggregative model of resource allocation in a metropolitan area. *American Economic Review, 57*(2), 197-210.

Mortensen, D. T., & Pissarides, C. A. (1999). New developments in models of search in the labor market. In D. Card & O. Ashenfelter (Eds.), *Handbook of labor economics* (Vol. 3, pp. 2567-2627). Amsterdam: Elsevier Science.

Muth, R. F. (1969). *Cities and housing*: University of Chicago press.

Ong, P. M. (2002). Car ownership and welfare to work. *Journal of Policy Analysis and Management, 21*(2), 239-252.

Ong, P. M., & Houston, D. (2002). Transit, employment and women on welfare. *Urban Geography, 23*(4), 344-364.

Pissarides, C. A. (2000). *Equilibrium unemployment theory*. Cambridge, MA: MIT press.

Raphael, S., & Rice, L. (2002). Car ownership, employment, and earnings. *Journal of Urban Economics, 52*(1), 109-130.

Rosenbaum, P. & Rubin, D. (1985). Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *American Statistician, 39*(1), 33-38.

Ross, S. L., & Zenou, Y. (2008). Are shirking and leisure substitutable? An empirical test of efficiency wages based on urban economic theory. *Regional Science and Urban Economics, 38*(5), 498-517.

Sanchez, T. W. (1999). The connection between public transit and employment. *Journal of the American Planning Association, 65*(3), 284-296.

Sanchez, T. W., Shen, Q., & Peng, Z.-R. (2004). Transit mobility, jobs access and low-income labour participation in US metropolitan areas. *Urban Studies, 41*(7), 1313.

Shapiro, C., & Stiglitz, J. E. (1984). Equilibrium unemployment as a worker discipline device. *American Economic Review, 74*(3), 433-444.

Shen, Q., & Sanchez, T. W. (2005). Residential location, transportation, and welfare to work in the United States: A case study of Milwaukee. *Housing Policy Debate, 16*(3-4), 393-431.

Smith, J. A., & Todd, E. P. (2005). Does matching overcome LaLonde's critique of nonexperimental estimators? *Journal of Econometrics*, 125, 305-353.

Smith, T. E., & Zenou, Y. (2003). Spatial mismatch, search effort, and urban spatial structure. *Journal of Urban Economics, 54*(1), 129-156.

Statistics Sweden. (1992). Statistisk Årsbok 1992 [Statistical Yearbook 1992]. Örebro.

Statistics Sweden. (1992). *Tätorter 1990 - Befolkning och areal i tätorter och glesbygd - Reviderade uppgifter.* Statistiska meddelanden Na 38 SM 9201 [*Localities 1990 - Population and area in urban and rural areas - Revised figures.* Statistical reports Na 38 SM 9201]. Örebro.

Statistics Sweden. (2005). *Geografin i statistiken - regionala indelningar i Sverige.* Meddelanden i samordningsfrågor för Sveriges officiella statistik 2005:2 [*Geography in statistics - regional divisions in Sweden.* Reports on Statistical Co-ordination for the Official Statistics of Sweden 2005:2]. Örebro.

Upplands Lokaltrafik (UL). Annual report 1989/1990.

Upplands Lokaltrafik (UL). Time-tables 1986-2008.

Upplands Lokaltrafik (UL). (1988). Minutes board meeting February 11, 1988.

Uppsala county council. (1990). Documents County Council meeting September 24, 1990.

Wasmer, E., & Zenou, Y. (2002). Does city structure affect job search and welfare? *Journal of Urban Economics, 51*(3), 515-541.

Wasmer, E., & Zenou, Y. (2006). Equilibrium search unemployment with explicit spatial frictions. *Labour Economics, 13*(2), 143-165.

White, M. J. (1976). Firm suburbanization and urban subcenters. *Journal of Urban Economics, 3*(4), 323-343.

Zenou, Y. (2002). How do firms redline workers? *Journal of Urban Economics, 52*(3), 391-408.

Zenou, Y. (2009). *Urban Labor Economics*. Cambridge: Cambridge University Press.

Zenou, Y., & Smith, T. E. (1995). Efficiency wages, involuntary unemployment and urban spatial structure. *Regional Science and Urban Economics, 25*(4), 547-573.

Appendix A: Definition of some variables

Employment: Employment is based on the official annual employment statistics and refers to status during November each year. A person is classified as employed if he or she did paid work for at least one hour per week. If data for an individual is missing in a given year, we consider the individual as not employed in that year.

Labor income, labor earnings, earnings (the words are used interchangeably): Annual earnings from work, including self-employment and employer's income, in 1000 SEK in 1989 prices. If data for an individual is missing in a given year, we consider the individual to have no labor income in that year.

"western countries": Finland, Norway, Denmark, Iceland, Ireland, Great Britain, the Netherlands, Belgium, France, Monaco, Luxembourg, Lichtenstein, Spain, Portugal, Andorra, Germany, Switzerland, Italy, San Marino, Vatican City State, Austria, Greece, Canada, the USA, Australia, Papua New Guinea, New Zealand and other Oceanian countries.

Low income in 1989: An individual is defined as having low income in 1989 if his or her labor income in 1989 was less than 50 percent of the median taxable income in 1989 among those for whom the Swedish Tax Agency had information (82% of the population) (see Statistical Yearbook 1992, Table 221 "Income-earners by total net income and age").

Young: Being 19-21 years old in 1989.

Appendix B: propensity score densities of estimated propensity scores

Figure B 1: The whole sample

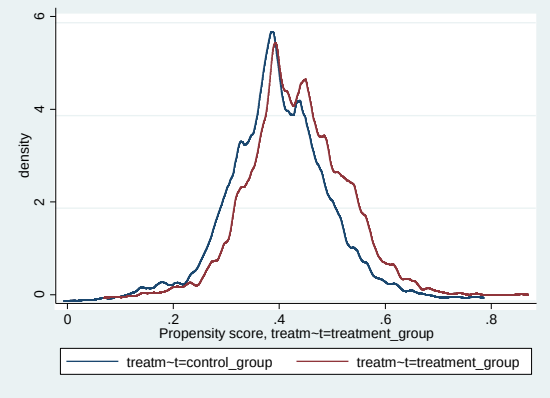


Figure B 2: Women

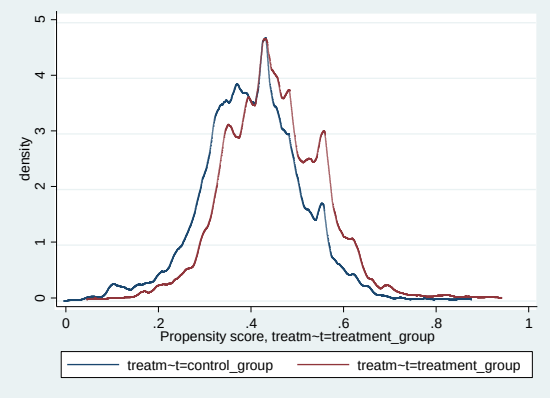


Figure B 3: Age 22-29 years in 1989

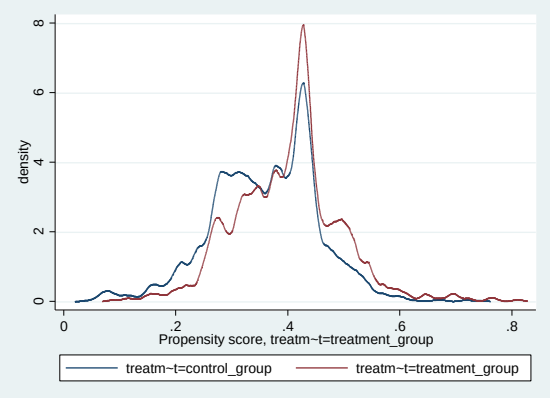


Figure B 4: Age 30-39 years in 1989

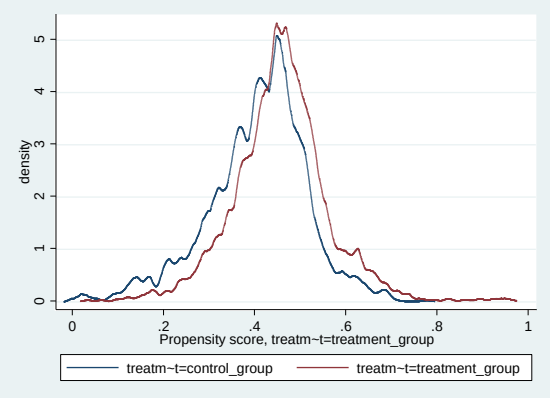


Figure B 5: Age 40-49 years in 1989

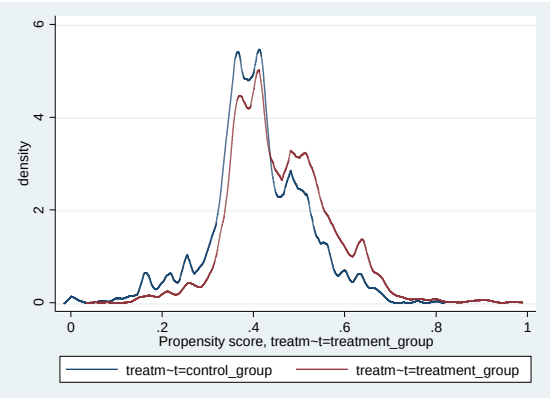


Figure B 6: Age 50-57 years in 1989

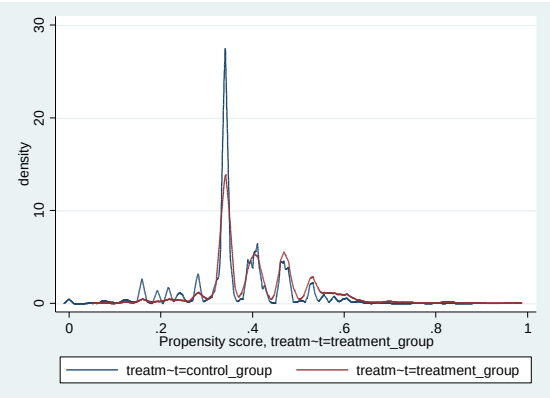


Figure B 7: Young

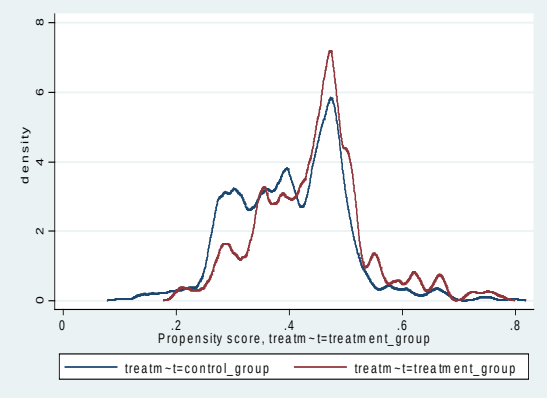


Figure B 8: Low income in 1989

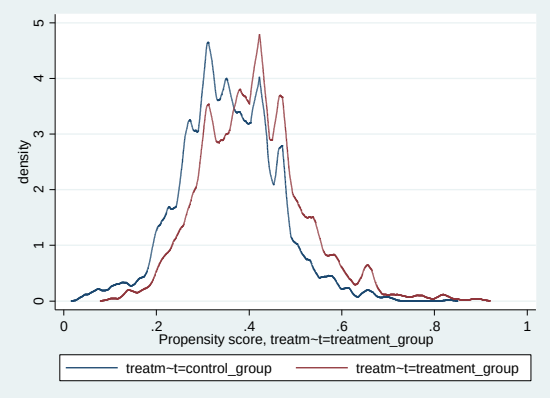


Figure B 9: Born in Sweden

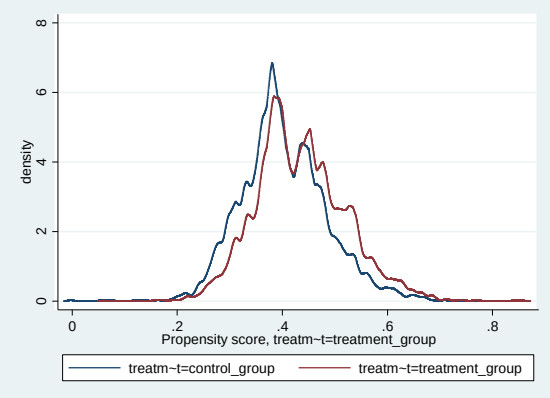


Figure B 10: Born in a western country

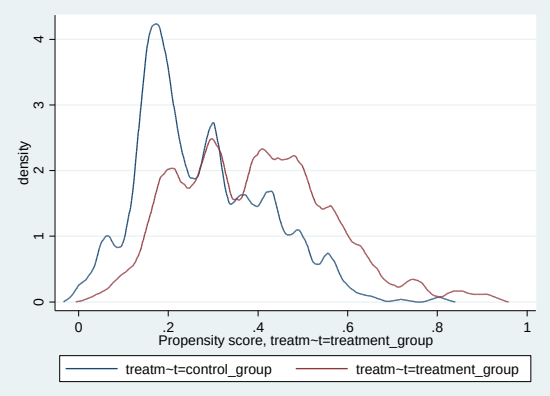


Figure B 11: Born in a non-western country

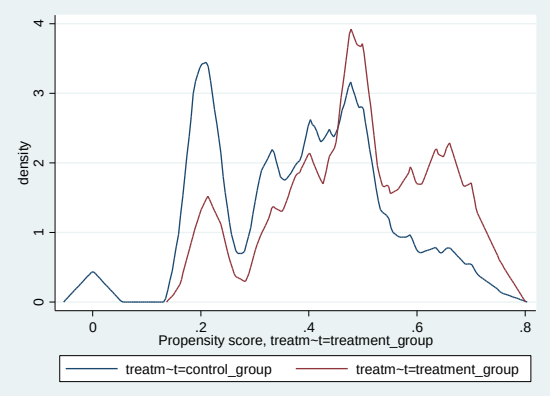


Figure B 12: Without Tierp

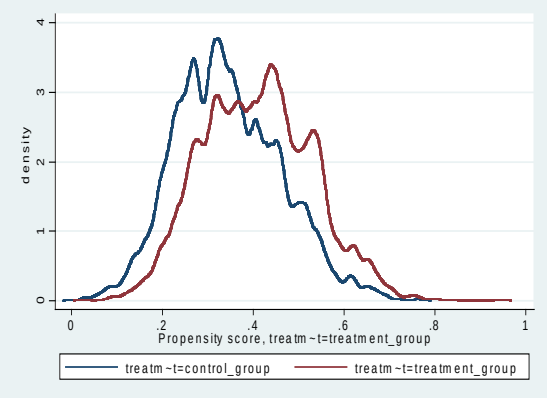


Figure B 13: Without Storvreta

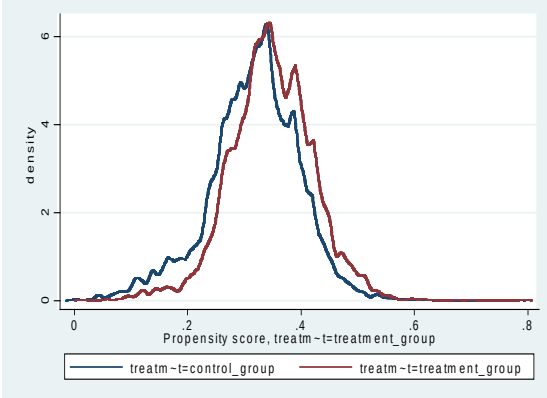


Figure B 14:
Worked in Uppsala city or south

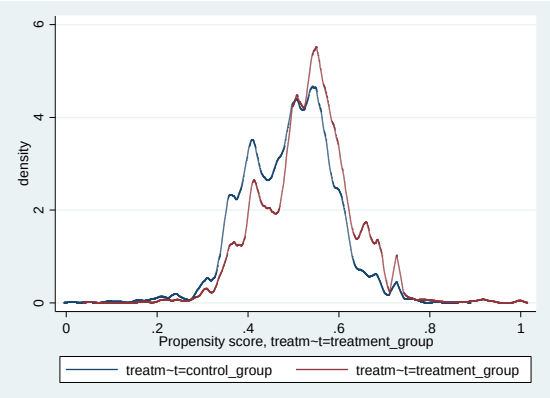
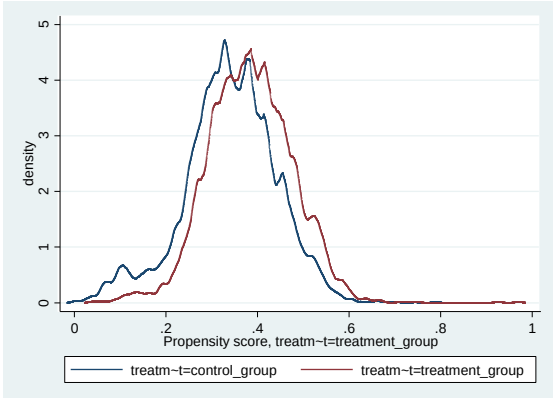


Figure B 15:
Worked elsewhere



Appendix C: Summary statistics for sub-populations

Table C 1: Women

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
age group 1989							
20-29 years	Unmatched	0.17	0.20	-6.8		-3.26	0.001
	Matched	0.17	0.17	0.5	92.3	0.24	0.81
30-39 years	Unmatched	0.33	0.31	4.2		2.03	0.042
	Matched	0.33	0.32	2.2	47.8	0.98	0.326
40-49 years	Unmatched	0.35	0.33	5.4		2.57	0.01
	Matched	0.35	0.37	-2.2	59.2	-0.96	0.337
50-57 years	Unmatched	0.14	0.16	-5.3		-2.51	0.012
	Matched	0.14	0.14	-0.6	89.3	-0.26	0.797
education 1990							
10 years or	Unmatched	0.27	0.32	-10.8		-5.12	0
less	Matched	0.27	0.26	1.1	89.6	0.51	0.611
secondary	Unmatched	0.47	0.48	-1.4		-0.67	0.502
	Matched	0.47	0.48	-1.5	-4.5	-0.65	0.514
tertiary	Unmatched	0.25	0.20	13.5		6.51	0
	Matched	0.25	0.25	1	92.4	0.44	0.66
missing	Unmatched	0.00	0.01	-0.8		-0.37	0.714
	Matched	0.00	0.01	-2.8	-270.4	-1.18	0.237
birth region							
Sweden	Unmatched	0.94	0.91	8.2		3.9	0
	Matched	0.94	0.94	-0.7	91.8	-0.32	0.746
western	Unmatched	0.05	0.07	-9		-4.24	0
country	Matched	0.05	0.05	3	66.4	1.52	0.13
non-western	Unmatched	0.01	0.01	0.2		0.09	0.931
country	Matched	0.01	0.02	-5.2	-2789.4	-2.08	0.038
industry 1989							
manufacturing	Unmatched	0.08	0.12	-13.3		-6.27	0
	Matched	0.08	0.08	-0.8	93.7	-0.41	0.685
construction	Unmatched	0.01	0.01	1.9		0.91	0.362
	Matched	0.01	0.01	0.3	86.7	0.11	0.914
trade	Unmatched	0.08	0.08	-1.3		-0.63	0.53
	Matched	0.08	0.07	3.5	-166.4	1.62	0.106
education	Unmatched	0.08	0.08	2.3		1.08	0.279
	Matched	0.08	0.09	-1.2	46.3	-0.53	0.599
health care	Unmatched	0.20	0.17	8.7		4.16	0
	Matched	0.20	0.21	-3.5	60	-1.47	0.141
other types of	Unmatched	0.12	0.15	-8.2		-3.87	0
care	Matched	0.12	0.12	0.5	93.6	0.24	0.807
public admin-	Unmatched	0.06	0.05	4.8		2.32	0.021
istration	Matched	0.06	0.06	-0.6	88.1	-0.24	0.81
other	Unmatched	0.16	0.18	-7		-3.32	0.001
	Matched	0.16	0.15	0.5	92.3	0.25	0.804
unknown/did	Unmatched	0.21	0.17	11.7		5.61	0
not work	Matched	0.21	0.20	1.7	85	0.75	0.455
employed	Unmatched	0.84	0.85	-3.1		-1.48	0.139
1985	Matched	0.84	0.85	-2.4	23.6	-1.05	0.296
employed	Unmatched	0.86	0.85	0		0.02	0.986
1986	Matched	0.86	0.86	-0.9	-2455.2	-0.42	0.677
employed	Unmatched	0.87	0.87	0.3		0.15	0.884
1986	Matched	0.87	0.87	-2	-569.7	-0.92	0.359
employed	Unmatched	0.89	0.88	1.6		0.78	0.437
1988	Matched	0.89	0.89	-0.7	56.4	-0.32	0.749
employed	Unmatched	0.89	0.87	3.2		1.55	0.122
1989	Matched	0.89	0.89	-1.6	49.6	-0.75	0.454
ln labor in-	Unmatched	2.98	3.07	-2.9		-1.41	0.159
come 1985	Matched	2.98	3.08	-3.1	-6.7	-1.4	0.163
ln labor in-	Unmatched	3.14	3.19	-1.4		-0.68	0.495
come 1986	Matched	3.14	3.23	-2.8	-94.7	-1.24	0.214

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
ln labor in-	Unmatched	3.27	3.26	0.4		0.2	0.841
come 1987	Matched	3.27	3.30	-0.8	-89.6	-0.35	0.724
ln labor in-	Unmatched	3.40	3.33	2.3		1.11	0.267
come 1988	Matched	3.40	3.42	-0.6	74.5	-0.27	0.791
ln labor in-	Unmatched	3.47	3.33	4.8		2.3	0.022
come 1989	Matched	3.47	3.47	0.2	96.6	0.08	0.94
Individuals in sample	Unmatched	3954	5419				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 2: Age 22-29 years in 1989

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.49	0.46	5.1		1.5	0.133
	Matched	0.49	0.47	2.8	45.9	0.73	0.467
education 1990							
10 years or	Unmatched	0.19	0.20	-2.7		-0.79	0.428
less	Matched	0.19	0.20	-2.6	5.1	-0.68	0.5
secondary	Unmatched	0.67	0.67	-1.7		-0.5	0.615
	Matched	0.67	0.67	-0.6	63.9	-0.16	0.871
tertiary	Unmatched	0.14	0.12	5.2		1.52	0.129
	Matched	0.14	0.13	3.9	25.1	1	0.316
missing	Unmatched	0.00	0.00	3.4		1.04	0.298
	Matched	0.00	0.00	0	100	0	1
birth region							
Sweden	Unmatched	0.97	0.94	13.1		3.71	0
	Matched	0.97	0.97	1.5	88.9	0.44	0.658
western	Unmatched	0.02	0.05	-15.8		-4.39	0
country	Matched	0.02	0.02	-0.4	97.4	-0.14	0.892
non-western	Unmatched	0.01	0.01	2.2		0.66	0.507
country	Matched	0.01	0.01	-2.4	-7.1	-0.56	0.576
industry 1989							
manufacturing	Unmatched	0.22	0.20	4.6		1.36	0.174
	Matched	0.22	0.22	0	100	0	1
construction	Unmatched	0.09	0.08	4.5		1.32	0.185
	Matched	0.09	0.09	-2.7	40.5	-0.66	0.507
trade	Unmatched	0.11	0.11	2.3		0.68	0.494
	Matched	0.11	0.11	0.9	60.1	0.24	0.809
education	Unmatched	0.02	0.01	2.6		0.78	0.436
	Matched	0.02	0.01	4	-52.1	1.08	0.282
health care	Unmatched	0.11	0.10	3.8		1.13	0.259
	Matched	0.11	0.09	4.4	-13.8	1.15	0.252
other types of	Unmatched	0.06	0.08	-5.8		-1.68	0.092
care	Matched	0.06	0.05	5.1	12.6	1.48	0.14
public admin-	Unmatched	0.03	0.03	1.8		0.52	0.604
istration	Matched	0.03	0.03	0	100	0	1
other	Unmatched	0.19	0.25	-13.6		-3.93	0
	Matched	0.19	0.21	-3.5	73.9	-0.96	0.338
unknown/did	Unmatched	0.17	0.15	4		1.18	0.239
not work	Matched	0.17	0.18	-3.4	16	-0.85	0.393
employed	Unmatched	0.78	0.80	-5		-1.46	0.144
1985	Matched	0.78	0.80	-4.8	3	-1.26	0.208
employed	Unmatched	0.83	0.83	-1.1		-0.33	0.74
1986	Matched	0.83	0.82	1.1	2.9	0.29	0.775
employed	Unmatched	0.87	0.86	4.1		1.19	0.236
1986	Matched	0.87	0.87	1.5	63.1	0.4	0.69
employed	Unmatched	0.88	0.89	-3.6		-1.07	0.285
1988	Matched	0.88	0.87	2.3	36.8	0.58	0.561
employed	Unmatched	0.88	0.89	-0.6		-0.19	0.85
1989	Matched	0.88	0.86	6	-824	1.5	0.134

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
ln labor in-	Unmatched	3.16	3.33	-6.8		-2.02	0.044
come 1985	Matched	3.16	3.15	0.1	98.9	0.02	0.985
ln labor in-	Unmatched	3.44	3.60	-6.5		-1.93	0.053
come 1986	Matched	3.44	3.52	-3	54.2	-0.77	0.442
ln labor in-	Unmatched	3.59	3.62	-1.2		-0.35	0.725
come 1987	Matched	3.59	3.57	0.9	23.3	0.24	0.811
ln labor in-	Unmatched	3.62	3.76	-5.6		-1.65	0.099
come 1988	Matched	3.62	3.59	1.4	75.4	0.34	0.734
ln labor in-	Unmatched	3.70	3.74	-1.7		-0.5	0.614
come 1989	Matched	3.70	3.52	6.8	-292.7	1.67	0.096
Individuals in sample	Unmatched	1369	2282				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 3: Age 30-39 years in 1989

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.52	0.51	3.3		1.24	0.215
	Matched	0.52	0.51	1.4	56.9	0.5	0.617
education 1990							
10 years or	Unmatched	0.21	0.27	-12.5		-4.7	0
less	Matched	0.21	0.23	-4	67.9	-1.46	0.145
secondary	Unmatched	0.51	0.52	-2.3		-0.86	0.39
	Matched	0.51	0.51	0.1	96.5	0.03	0.978
tertiary	Unmatched	0.28	0.21	15.9		6.08	0
	Matched	0.28	0.26	4.2	73.7	1.43	0.152
missing	Unmatched	0.00	0.01	-4.6		-1.72	0.086
	Matched	0.00	0.01	-1.6	65.8	-0.63	0.531
birth region							
Sweden	Unmatched	0.94	0.92	7.5		2.82	0.005
	Matched	0.94	0.93	3.1	58.6	1.15	0.252
western	Unmatched	0.04	0.06	-8.8		-3.32	0.001
country	Matched	0.04	0.05	-1.6	82.3	-0.6	0.548
non-western	Unmatched	0.02	0.01	0.8		0.3	0.766
country	Matched	0.02	0.02	-3.5	-352.1	-1.16	0.245
industry 1989							
manufacturing	Unmatched	0.14	0.17	-8.1		-3.06	0.002
	Matched	0.14	0.14	0.5	93.3	0.2	0.84
construction	Unmatched	0.07	0.07	-1.8		-0.7	0.486
	Matched	0.07	0.07	0	100	0	1
trade	Unmatched	0.09	0.09	0.8		0.31	0.758
	Matched	0.09	0.09	0.3	65.8	0.1	0.922
education	Unmatched	0.05	0.05	0.6		0.23	0.816
	Matched	0.05	0.04	2.2	-264.9	0.81	0.42
health care	Unmatched	0.13	0.10	8.6		3.3	0.001
	Matched	0.13	0.14	-1.6	81.5	-0.54	0.592
other types of	Unmatched	0.08	0.09	-2.4		-0.92	0.356
care	Matched	0.08	0.08	0.1	94.2	0.05	0.959
public admin-	Unmatched	0.06	0.05	6.5		2.49	0.013
istration	Matched	0.06	0.06	1.5	76.2	0.53	0.6
other	Unmatched	0.21	0.24	-6.4		-2.41	0.016
	Matched	0.21	0.23	-2.8	55.4	-1.02	0.308
unknown/did	Unmatched	0.16	0.14	6		2.27	0.023
not work	Matched	0.16	0.15	1.6	73.9	0.54	0.588
employed	Unmatched	0.88	0.88	0.7		0.25	0.799
1985	Matched	0.88	0.89	-4.1	-511.9	-1.5	0.134
employed	Unmatched	0.90	0.89	2.6		1	0.319
1986	Matched	0.90	0.90	-0.1	95.1	-0.05	0.963
employed	Unmatched	0.90	0.90	1.3		0.51	0.61
1986	Matched	0.90	0.90	0.9	31.5	0.33	0.743

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
employed	Unmatched	0.92	0.91	2.4		0.93	0.354
1988	Matched	0.92	0.92	-0.6	76.7	-0.21	0.835
employed	Unmatched	0.93	0.92	2.9		1.11	0.269
1989	Matched	0.93	0.93	-3.5	-20.8	-1.32	0.186
ln labor in-	Unmatched	3.50	3.52	-0.9		-0.35	0.724
come 1985	Matched	3.50	3.57	-2.7	-189.6	-0.98	0.325
ln labor in-	Unmatched	3.65	3.63	0.6		0.22	0.826
come 1986	Matched	3.65	3.66	-0.5	8.2	-0.19	0.849
ln labor in-	Unmatched	3.72	3.71	0.2		0.06	0.95
come 1987	Matched	3.72	3.72	0	85.9	0.01	0.993
ln labor in-	Unmatched	3.89	3.82	2.7		1.02	0.306
come 1988	Matched	3.89	3.94	-2	24.8	-0.74	0.457
ln labor in-	Unmatched	4.03	3.96	2.8		1.06	0.288
come 1989	Matched	4.03	4.13	-3.9	-39.5	-1.46	0.144
Individuals in sample	Unmatched	2526	3362				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 4: Age 40-49 years in 1989

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.49	0.47	4.5		1.8	0.071
	Matched	0.49	0.48	2.6	42	0.98	0.328
education 1990							
10 years or	Unmatched	0.31	0.37	-11.3		-4.56	0
less	Matched	0.31	0.30	2.7	75.9	1.06	0.289
secondary	Unmatched	0.40	0.42	-4		-1.6	0.111
	Matched	0.40	0.39	1.5	62.2	0.57	0.57
tertiary	Unmatched	0.29	0.21	17.1		6.97	0
	Matched	0.29	0.30	-4.5	73.9	-1.6	0.111
missing	Unmatched	0.00	0.00	-1.2		-0.46	0.644
	Matched	0.00	0.00	-1.7	-47.2	-0.63	0.531
birth region							
Sweden	Unmatched	0.94	0.91	9.3		3.73	0
	Matched	0.94	0.94	-1.7	81.4	-0.73	0.468
western	Unmatched	0.05	0.08	-10.2		-4.08	0
country	Matched	0.05	0.05	0.3	97.2	0.12	0.905
non-western	Unmatched	0.01	0.01	0.7		0.28	0.783
country	Matched	0.01	0.01	3.9	-474.5	1.61	0.107
industry 1989							
manufacturing	Unmatched	0.14	0.18	-9.5		-3.81	0
	Matched	0.14	0.13	4.3	54.8	1.74	0.082
construction	Unmatched	0.07	0.06	1.9		0.76	0.45
	Matched	0.07	0.06	2.3	-21.4	0.86	0.39
trade	Unmatched	0.09	0.08	4.2		1.7	0.09
	Matched	0.09	0.08	4.3	-2.9	1.63	0.104
education	Unmatched	0.08	0.07	6.9		2.79	0.005
	Matched	0.08	0.08	2	71.1	0.72	0.471
health care	Unmatched	0.12	0.08	10.6		4.33	0
	Matched	0.12	0.13	-5.4	49.3	-1.85	0.064
other types of	Unmatched	0.05	0.08	-8.8		-3.53	0
care	Matched	0.05	0.06	-1.6	82.5	-0.63	0.53
public admin-	Unmatched	0.07	0.05	8.8		3.58	0
istration	Matched	0.07	0.08	-2.7	69.6	-0.91	0.361
other	Unmatched	0.22	0.26	-9.4		-3.78	0
	Matched	0.22	0.23	-2.1	78.1	-0.8	0.426
unknown/did	Unmatched	0.16	0.15	2.7		1.11	0.268
not work	Matched	0.16	0.16	-1	64.5	-0.36	0.718
employed	Unmatched	0.92	0.92	-0.7		-0.29	0.775
1985	Matched	0.92	0.92	-2.2	-207.5	-0.83	0.405

		mean		% reduction		t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
employed	Unmatched	0.92	0.92	2		0.8	0.422
1986	Matched	0.92	0.92	0.6	69	0.24	0.813
employed	Unmatched	0.93	0.93	-0.7		-0.3	0.762
1986	Matched	0.93	0.92	0.5	28.5	0.2	0.841
employed	Unmatched	0.94	0.93	3.4		1.37	0.169
1988	Matched	0.94	0.93	1.9	43.2	0.74	0.457
employed	Unmatched	0.93	0.92	5.3		2.12	0.034
1989	Matched	0.93	0.94	-2.8	46.1	-1.16	0.247
ln labor in-	Unmatched	3.95	3.86	3.4		1.36	0.173
come 1985	Matched	3.95	3.93	0.6	81.7	0.23	0.814
ln labor in-	Unmatched	4.03	3.95	3		1.19	0.233
come 1986	Matched	4.03	4.03	0.3	90.3	0.11	0.914
ln labor in-	Unmatched	4.14	4.04	3.9		1.57	0.116
come 1987	Matched	4.14	4.09	1.9	51.1	0.71	0.476
ln labor in-	Unmatched	4.21	4.07	5.6		2.24	0.025
come 1988	Matched	4.21	4.22	-0.2	95.6	-0.1	0.924
ln labor in-	Unmatched	4.28	4.07	8.3		3.31	0.001
come 1989	Matched	4.28	4.34	-2.2	73.1	-0.91	0.364
Individuals in sample	Unmatched	2859	3800				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 5: Age 50-57 years in 1989

		mean		% reduction		t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.48	0.47	3		0.79	0.43
	Matched	0.48	0.47	3	-2.4	0.73	0.467
education 1990							
10 years or	Unmatched	0.51	0.57	-11.9		-3.19	0.001
less	Matched	0.51	0.50	2.8	76.6	0.67	0.505
secondary	Unmatched	0.31	0.30	2.9		0.77	0.444
	Matched	0.31	0.31	-0.9	67.1	-0.22	0.822
tertiary	Unmatched	0.17	0.12	12.8		3.46	0.001
	Matched	0.17	0.17	-1.7	86.5	-0.39	0.699
missing	Unmatched	0.01	0.01	1.3		0.34	0.732
	Matched	0.01	0.01	-3.9	-204.2	-0.82	0.412
birth region							
Sweden	Unmatched	0.93	0.90	12.4		3.24	0.001
	Matched	0.93	0.94	-2.8	77.3	-0.77	0.44
western	Unmatched	0.06	0.09	-14		-3.64	0
country	Matched	0.06	0.05	1	93	0.27	0.786
non-western	Unmatched	0.01	0.01	2.8		0.76	0.445
country	Matched	0.01	0.00	5.7	-104.4	1.51	0.132
industry 1989							
manufacturing	Unmatched	0.18	0.20	-4.5		-1.19	0.233
	Matched	0.18	0.19	-2	55.6	-0.48	0.629
construction	Unmatched	0.05	0.05	2.6		0.69	0.492
	Matched	0.05	0.06	-0.8	69.5	-0.18	0.856
trade	Unmatched	0.09	0.08	3.5		0.95	0.344
	Matched	0.09	0.08	3.6	-3.4	0.88	0.381
education	Unmatched	0.08	0.08	2.1		0.56	0.573
	Matched	0.08	0.08	1	54.1	0.23	0.818
health care	Unmatched	0.09	0.07	7.1		1.93	0.054
	Matched	0.09	0.09	-0.6	91.1	-0.14	0.885
other types of	Unmatched	0.06	0.06	-0.6		-0.17	0.868
care	Matched	0.06	0.06	-0.4	41.9	-0.09	0.93
public admin-	Unmatched	0.05	0.04	3		0.82	0.414
istration	Matched	0.05	0.05	-1.7	44.7	-0.38	0.701
other	Unmatched	0.19	0.23	-10.3		-2.72	0.007
	Matched	0.19	0.19	0	100	0	1

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
unknown/did	Unmatched	0.20	0.19	3.5		0.94	0.347
not work	Matched	0.20	0.20	0.7	81.2	0.16	0.876
employed	Unmatched	0.89	0.91	-4		-1.07	0.285
1985	Matched	0.89	0.90	-2	49.3	-0.48	0.633
employed	Unmatched	0.90	0.90	0.6		0.16	0.872
1986	Matched	0.90	0.90	-0.3	52.6	-0.07	0.945
employed	Unmatched	0.89	0.90	-1.9		-0.52	0.604
1986	Matched	0.89	0.89	-1.4	27.4	-0.34	0.736
employed	Unmatched	0.90	0.88	4.4		1.16	0.246
1988	Matched	0.90	0.89	0.3	93.7	0.07	0.946
employed	Unmatched	0.88	0.86	5.8		1.53	0.127
1989	Matched	0.88	0.89	-2.1	63.9	-0.52	0.601
ln labor in-	Unmatched	3.64	3.67	-1		-0.26	0.795
come 1985	Matched	3.64	3.59	1.7	-74.2	0.39	0.694
ln labor in-	Unmatched	3.69	3.65	1.4		0.36	0.719
come 1986	Matched	3.69	3.64	1.6	-19	0.38	0.704
ln labor in-	Unmatched	3.66	3.63	0.8		0.21	0.835
come 1987	Matched	3.66	3.70	-1.3	-61.3	-0.3	0.762
ln labor in-	Unmatched	3.64	3.51	3.9		1.04	0.298
come 1988	Matched	3.64	3.65	-0.3	92.4	-0.07	0.942
ln labor in-	Unmatched	3.56	3.30	7.2		1.91	0.056
come 1989	Matched	3.56	3.59	-0.8	89.5	-0.19	0.849
Individuals in sample	Unmatched	1152	1857				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 6: Young

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.47	0.47	0.8		0.15	0.881
	Matched	0.47	0.47	-0.9	-18.1	-0.16	0.87
education 1990							
10 years or	Unmatched	0.12	0.16	-11.7		-2.24	0.025
less	Matched	0.12	0.09	7.2	38.8	1.44	0.15
secondary	Unmatched	0.79	0.74	12.3		2.38	0.018
	Matched	0.79	0.82	-6.6	46.5	-1.27	0.204
tertiary	Unmatched	0.09	0.10	-4		-0.77	0.442
	Matched	0.09	0.08	1.1	73.2	0.2	0.842
missing	Unmatched	-	-				
	Matched	-	-				
birth region							
Sweden	Unmatched	0.99	0.98	8.2		1.55	0.122
	Matched	0.99	0.99	-2.6	68.1	-0.63	0.526
western	Unmatched	0.01	0.02	-8.2		-1.55	0.122
country	Matched	0.01	0.01	2.6	68.1	0.63	0.526
non-western	Unmatched	-	-				
country	Matched	-	-				
industry 1989							
manufacturing	Unmatched	0.22	0.23	-3.1		-0.6	0.552
	Matched	0.22	0.21	3	3.7	0.54	0.587
construction	Unmatched	0.08	0.08	1.4		0.28	0.781
	Matched	0.08	0.07	4	-178.9	0.73	0.465
trade	Unmatched	0.16	0.13	10.4		2.04	0.042
	Matched	0.16	0.17	-2.2	79	-0.37	0.71
education	Unmatched	0.02	0.01	7.1		1.42	0.155
	Matched	0.02	0.00	10.9	-53	2.15	0.032
health care	Unmatched	0.09	0.10	-1.6		-0.31	0.756
	Matched	0.09	0.10	-4.2	-163.9	-0.75	0.454
other types of	Unmatched	0.09	0.08	4.3		0.84	0.401
care	Matched	0.09	0.08	2.8	35.8	0.49	0.623

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
public admin-	Unmatched	0.01	0.02	-4.8		-0.91	0.363
istration	Matched	0.01	0.03	-9.6	-100.8	-1.59	0.113
other	Unmatched	0.18	0.23	-10.5		-2.02	0.043
	Matched	0.18	0.21	-6.1	41.7	-1.12	0.263
unknown/did	Unmatched	0.14	0.13	1.6		0.3	0.764
not work	Matched	0.14	0.13	4	-161.3	0.74	0.46
employed	Unmatched	0.10	0.14	-10.3		-1.98	0.048
1985	Matched	0.10	0.13	-8.2	20.2	-1.5	0.134
employed	Unmatched	0.29	0.34	-10.7		-2.06	0.04
1986	Matched	0.29	0.31	-5.1	52.1	-0.93	0.354
employed	Unmatched	0.48	0.52	-8.5		-1.65	0.099
1986	Matched	0.48	0.50	-4.4	48	-0.8	0.426
employed	Unmatched	0.73	0.69	7.8		1.5	0.134
1988	Matched	0.73	0.73	-1	86.8	-0.19	0.851
employed	Unmatched	0.79	0.78	1		0.19	0.847
1989	Matched	0.79	0.77	5.2	-421.6	0.92	0.357
ln labor in-	Unmatched	-3.30	-2.39	-20.7		-4.01	0
come 1985	Matched	-3.30	-2.51	-17.9	13.6	-3.24	0.001
ln labor in-	Unmatched	0.18	1.12	-25.8		-5.07	0
come 1986	Matched	0.18	1.12	-25.8	0	-4.71	0
ln labor in-	Unmatched	2.05	2.41	-12.8		-2.49	0.013
come 1987	Matched	2.05	2.06	-0.4	97.2	-0.06	0.951
ln labor in-	Unmatched	3.18	3.31	-6.3		-1.23	0.218
come 1988	Matched	3.18	3.10	4.2	33.9	0.7	0.486
ln labor in-	Unmatched	3.64	3.59	2.2		0.42	0.673
come 1989	Matched	3.64	3.58	2.9	-35.3	0.52	0.6
Individuals in							
sample	Unmatched	647	897				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 7: Low income in 1989

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.74	0.69	10.3		3.12	0.002
	Matched	0.74	0.72	5.5	46.3	1.53	0.127
age group 1989							
20-29 years	Unmatched	0.27	0.27	-0.2		-0.05	0.963
	Matched	0.27	0.27	-1.7	-984.2	-0.45	0.65
30-39 years	Unmatched	0.33	0.30	5.2		1.59	0.111
	Matched	0.33	0.33	0	100	0	1
40-49 years	Unmatched	0.23	0.25	-5.1		-1.53	0.125
	Matched	0.23	0.23	0	100	0	1
50-57 years	Unmatched	0.17	0.17	-0.5		-0.15	0.882
	Matched	0.17	0.17	2	-301.5	0.54	0.59
education 1990							
10 years or	Unmatched	0.36	0.38	-4.2		-1.27	0.205
less	Matched	0.36	0.35	2.9	29.8	0.81	0.42
secondary	Unmatched	0.48	0.48	-1.1		-0.34	0.731
	Matched	0.48	0.48	-0.5	52.3	-0.15	0.883
tertiary	Unmatched	0.15	0.12	8.5		2.62	0.009
	Matched	0.15	0.15	-2.2	74.2	-0.56	0.573
missing	Unmatched	0.02	0.02	-2.2		-0.67	0.504
	Matched	0.02	0.02	-3	-35.9	-0.81	0.418
birth region							
Sweden	Unmatched	0.91	0.89	5.2		1.56	0.12
	Matched	0.91	0.91	0.2	95.7	0.06	0.95
western	Unmatched	0.07	0.08	-6.4		-1.91	0.056
country	Matched	0.07	0.07	-0.3	96	-0.07	0.942
non-western	Unmatched	0.03	0.02	0.9		0.26	0.794
country	Matched	0.03	0.03	0	100	0	1

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
industry 1989							
manufacturing	Unmatched	0.09	0.09	-1.1		-0.35	0.728
	Matched	0.09	0.09	-1.2	-2.6	-0.32	0.749
construction	Unmatched	0.02	0.02	-2.5		-0.77	0.444
	Matched	0.02	0.02	-2.3	11	-0.62	0.537
trade	Unmatched	0.07	0.07	0.4		0.11	0.915
	Matched	0.07	0.09	-6.8	-1835.9	-1.75	0.08
education	Unmatched	0.04	0.04	0.1		0.02	0.985
	Matched	0.04	0.04	-0.4	-488.6	-0.1	0.924
health care	Unmatched	0.11	0.09	6.5		1.99	0.047
	Matched	0.11	0.11	-2	68.5	-0.53	0.599
other types of	Unmatched	0.08	0.09	-3.5		-1.06	0.287
care	Matched	0.08	0.06	7.3	-108.4	2.18	0.03
public admin-	Unmatched	0.02	0.02	3.5		1.07	0.286
istration	Matched	0.02	0.03	-2.7	20.9	-0.68	0.497
other	Unmatched	0.15	0.18	-9.4		-2.83	0.005
	Matched	0.15	0.14	0.9	90.3	0.26	0.795
unknown/did	Unmatched	0.42	0.39	5.4		1.65	0.098
not work	Matched	0.42	0.40	2.2	59.5	0.6	0.551
employed	Unmatched	0.64	0.70	-11.5		-3.52	0
1985	Matched	0.64	0.62	4.4	61.3	1.18	0.238
employed	Unmatched	0.65	0.69	-7.1		-2.18	0.029
1986	Matched	0.65	0.65	0.4	95	0.1	0.923
employed	Unmatched	0.64	0.68	-8.9		-2.71	0.007
1986	Matched	0.64	0.64	0.5	94.2	0.14	0.891
employed	Unmatched	0.62	0.65	-4.7		-1.45	0.148
1988	Matched	0.62	0.63	-0.9	81.2	-0.24	0.809
employed	Unmatched	0.58	0.59	-2.3		-0.7	0.482
1989	Matched	0.58	0.59	-1	58.5	-0.26	0.794
ln labor in-	Unmatched	1.18	1.67	-11.3		-3.48	0.001
come 1985	Matched	1.18	1.13	1.1	90.3	0.29	0.771
ln labor in-	Unmatched	1.12	1.55	-9.5		-2.91	0.004
come 1986	Matched	1.12	1.29	-3.8	59.5	-1.04	0.298
ln labor in-	Unmatched	1.05	1.34	-6.4		-1.94	0.052
come 1987	Matched	1.05	1.07	-0.4	93.1	-0.12	0.906
ln labor in-	Unmatched	0.80	0.98	-4		-1.23	0.219
come 1988	Matched	0.80	0.85	-1.3	68.6	-0.34	0.731
ln labor in-	Unmatched	0.30	0.30	0		-0.01	0.991
come 1989	Matched	0.30	0.36	-1.3	-3391.4	-0.36	0.718
Individuals in sample	Unmatched	1484	2472				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 8: Born in Sweden

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.49	0.48	3.7		2.45	0.014
	Matched	0.49	0.49	1.9	48.2	1.18	0.239
age group 1989							
20-29 years	Unmatched	0.18	0.21	-7.5		-4.9	0
	Matched	0.18	0.18	0.7	90.5	0.45	0.653
30-39 years	Unmatched	0.32	0.30	4.3		2.87	0.004
	Matched	0.32	0.32	-1.1	74	-0.68	0.494
40-49 years	Unmatched	0.36	0.33	5.3		3.52	0
	Matched	0.36	0.36	0.8	84.2	0.51	0.609
50-57 years	Unmatched	0.14	0.16	-4.4		-2.91	0.004
	Matched	0.14	0.15	-0.4	89.9	-0.28	0.781
education 1990							
10 years or	Unmatched	0.29	0.33	-9.1		-5.98	0
less	Matched	0.29	0.29	-0.6	93.3	-0.38	0.705

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
secondary	Unmatched	0.47	0.48	-3.3		-2.2	0.028
	Matched	0.47	0.47	-0.4	88.7	-0.23	0.819
tertiary	Unmatched	0.24	0.18	14.3		9.54	0
	Matched	0.24	0.23	1.3	91	0.75	0.452
missing	Unmatched	0.00	0.00	0.8		0.51	0.61
	Matched	0.00	0.01	-0.8	-1.7	-0.45	0.65
industry 1989							
manufacturing	Unmatched	0.16	0.17	-2.6		-1.72	0.086
	Matched	0.16	0.16	0.8	68.1	0.52	0.606
construction	Unmatched	0.07	0.07	2		1.3	0.195
	Matched	0.07	0.07	0.2	92	0.09	0.925
trade	Unmatched	0.10	0.09	1.4		0.9	0.366
	Matched	0.10	0.10	-1.6	-20.5	-0.99	0.324
education	Unmatched	0.06	0.05	3.4		2.24	0.025
	Matched	0.06	0.06	0.2	94.9	0.1	0.919
health care	Unmatched	0.12	0.09	7.9		5.26	0
	Matched	0.12	0.12	-2.3	70.5	-1.34	0.18
other types of	Unmatched	0.06	0.08	-5.6		-3.68	0
care	Matched	0.06	0.06	0.9	84.3	0.57	0.568
public admin-	Unmatched	0.06	0.05	6.2		4.13	0
istration	Matched	0.06	0.06	0.9	85.5	0.52	0.602
other	Unmatched	0.21	0.25	-10.9		-7.15	0
	Matched	0.21	0.21	-0.6	94.2	-0.4	0.687
unknown/did	Unmatched	0.16	0.15	4.2		2.76	0.006
not work	Matched	0.16	0.16	1.8	57.6	1.07	0.285
employed	Unmatched	0.88	0.89	-1.1		-0.73	0.464
1985	Matched	0.88	0.88	1.3	-21	0.81	0.419
employed	Unmatched	0.90	0.89	2.2		1.42	0.155
1986	Matched	0.90	0.90	0.1	95.9	0.05	0.957
employed	Unmatched	0.91	0.91	1.1		0.72	0.471
1986	Matched	0.91	0.90	1.5	-36.4	0.91	0.362
employed	Unmatched	0.92	0.91	2.2		1.45	0.147
1988	Matched	0.92	0.92	0.4	83.8	0.22	0.825
employed	Unmatched	0.92	0.91	3.5		2.31	0.021
1989	Matched	0.92	0.92	0.7	81.1	0.42	0.678
ln labor in-	Unmatched	3.69	3.69	-0.3		-0.18	0.855
come 1985	Matched	3.69	3.64	1.6	-480.9	0.96	0.338
ln labor in-	Unmatched	3.82	3.81	0.7		0.48	0.632
come 1986	Matched	3.82	3.83	-0.3	65.3	-0.15	0.878
ln labor in-	Unmatched	3.89	3.86	1.3		0.85	0.397
come 1987	Matched	3.89	3.89	0	99.7	0	0.998
ln labor in-	Unmatched	3.97	3.90	2.8		1.85	0.064
come 1988	Matched	3.97	3.97	-0.1	95	-0.09	0.931
ln labor in-	Unmatched	4.03	3.90	5		3.28	0.001
come 1989	Matched	4.03	4.01	1	80.5	0.61	0.544
Individuals in sample	Unmatched	7477	10401				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 9: Born in a western country

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.58	0.51	15.9		2.47	0.014
	Matched	0.58	0.62	-8	49.4	-1.08	0.28
age group 1989							
20-29 years	Unmatched	0.08	0.15	-22		-3.26	0.001
	Matched	0.08	0.07	0.9	95.9	0.14	0.887
30-39 years	Unmatched	0.32	0.27	11.1		1.75	0.08
	Matched	0.32	0.30	5.6	49.6	0.73	0.463
40-49 years	Unmatched	0.42	0.36	11.9		1.87	0.062

		mean		% reduction		t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
	Matched	0.42	0.42	0	100	0	1
50-57 years	Unmatched	0.18	0.22	-10		-1.54	0.125
	Matched	0.18	0.21	-7.1	29.1	-0.95	0.343
education 1990							
10 years or	Unmatched	0.30	0.42	-24.9		-3.82	0
less	Matched	0.30	0.28	5.4	78.4	0.75	0.454
secondary	Unmatched	0.44	0.42	3.4		0.54	0.592
	Matched	0.44	0.44	0	100	0	1
tertiary	Unmatched	0.25	0.15	24.1		3.88	0
	Matched	0.25	0.27	-6.4	73.2	-0.77	0.44
missing	Unmatched	0.01	0.00	8.1		1.41	0.16
	Matched	0.01	0.01	0	100	0	1
industry 1989							
manufacturing	Unmatched	0.20	0.37	-39.1		-5.87	0
	Matched	0.20	0.16	9	76.9	1.39	0.166
construction	Unmatched	0.02	0.05	-15.8		-2.29	0.022
	Matched	0.02	0.03	-3.1	80.1	-0.51	0.614
trade	Unmatched	0.07	0.04	11.3		1.84	0.066
	Matched	0.07	0.05	9.8	13.9	1.27	0.204
education	Unmatched	0.05	0.03	10.9		1.79	0.074
	Matched	0.05	0.08	-12.6	-15.7	-1.36	0.175
health care	Unmatched	0.15	0.08	19.6		3.2	0.001
	Matched	0.15	0.14	0.9	95.4	0.11	0.915
other types of	Unmatched	0.07	0.07	1.4		0.23	0.821
care	Matched	0.07	0.08	-3.3	-128.4	-0.42	0.674
public admin-	Unmatched	0.03	0.02	9.3		1.53	0.126
istration	Matched	0.03	0.07	-23	-146.8	-2.2	0.028
other	Unmatched	0.20	0.16	11		1.75	0.081
	Matched	0.20	0.19	2.2	79.9	0.28	0.777
unknown/did	Unmatched	0.20	0.17	7.6		1.2	0.232
not work	Matched	0.20	0.20	0.7	90.4	0.09	0.925
employed	Unmatched	0.81	0.85	-11		-1.74	0.082
1985	Matched	0.81	0.84	-9.2	16.2	-1.21	0.227
employed	Unmatched	0.83	0.87	-11.7		-1.86	0.063
1986	Matched	0.83	0.84	-3.2	72.7	-0.41	0.685
employed	Unmatched	0.84	0.87	-6.4		-1	0.317
1986	Matched	0.84	0.86	-4	36.5	-0.53	0.597
employed	Unmatched	0.86	0.88	-4.9		-0.78	0.437
1988	Matched	0.86	0.87	-1	79.9	-0.13	0.898
employed	Unmatched	0.86	0.87	-1.9		-0.29	0.772
1989	Matched	0.86	0.86	-0.4	77.5	-0.05	0.956
ln labor in-	Unmatched	2.85	3.30	-12.8		-2.03	0.043
come 1985	Matched	2.85	3.06	-6.1	52.5	-0.78	0.436
ln labor in-	Unmatched	2.97	3.50	-15.4		-2.47	0.014
come 1986	Matched	2.97	3.02	-1.6	89.4	-0.2	0.839
ln labor in-	Unmatched	3.11	3.45	-9.9		-1.56	0.118
come 1987	Matched	3.11	3.22	-3.3	66.7	-0.43	0.67
ln labor in-	Unmatched	3.20	3.52	-9.4		-1.49	0.136
come 1988	Matched	3.20	3.32	-3.4	63.3	-0.44	0.658
ln labor in-	Unmatched	3.35	3.40	-1.6		-0.24	0.809
come 1989	Matched	3.35	3.17	5.4	-248.2	0.68	0.494
Individuals in sample	Unmatched	351	782				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 10: Born in a non-western country

		mean			% reduction	t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.50	0.46	8.8		0.64	0.521
	Matched	0.50	0.44	12.1	-37.9	0.83	0.407
age group 1989							
20-29 years	Unmatched	0.14	0.14	0.7		0.05	0.961
	Matched	0.14	0.10	11.6	-1640.5	0.85	0.399
30-39 years	Unmatched	0.41	0.40	3		0.22	0.825
	Matched	0.41	0.41	0.8	73.6	0.05	0.957
40-49 years	Unmatched	0.32	0.30	3.3		0.24	0.812
	Matched	0.32	0.34	-5.3	-62.2	-0.36	0.722
50-57 years	Unmatched	0.13	0.16	-9.2		-0.67	0.505
	Matched	0.13	0.15	-5.5	40.2	-0.38	0.702
education 1990							
10 years or	Unmatched	0.22	0.31	-20		-1.46	0.147
less	Matched	0.22	0.22	0	100	0	1
secondary	Unmatched	0.38	0.40	-3.5		-0.25	0.8
	Matched	0.38	0.38	0	100	0	1
tertiary	Unmatched	0.35	0.21	32.1		2.38	0.018
	Matched	0.35	0.35	0	100	0	1
missing	Unmatched	0.04	0.08	-15.6		-1.12	0.264
	Matched	0.04	0.04	0	100	0	1
industry 1989							
manufacturing	Unmatched	0.18	0.26	-20		-1.45	0.148
	Matched	0.18	0.18	0	100	0	1
construction	Unmatched	0.01	0.01	2.7		0.2	0.84
	Matched	0.01	0.01	0	100	0	1
trade	Unmatched	0.04	0.02	10.3		0.77	0.442
	Matched	0.04	0.04	0	100	0	1
education	Unmatched	0.03	0.06	-11.7		-0.84	0.4
	Matched	0.03	0.03	0	100	0	1
health care	Unmatched	0.03	0.04	-4.3		-0.31	0.754
	Matched	0.03	0.03	0	100	0	1
other types of	Unmatched	0.15	0.09	18.8		1.4	0.162
care	Matched	0.15	0.15	0	100	0	1
public admin-	Unmatched	0.01	0.01	2.7		0.2	0.84
istration	Matched	0.01	0.01	0	100	0	1
other	Unmatched	0.28	0.23	10.2		0.75	0.454
	Matched	0.28	0.28	0	100	0	1
unknown/did	Unmatched	0.27	0.28	-3.1		-0.23	0.819
not work	Matched	0.27	0.27	0	100	0	1
employed	Unmatched	0.63	0.50	25		1.83	0.069
1985	Matched	0.63	0.64	-2.9	88.5	-0.2	0.841
employed	Unmatched	0.68	0.57	23.4		1.7	0.09
1986	Matched	0.68	0.69	-1.3	94.3	-0.1	0.924
employed	Unmatched	0.72	0.64	17.9		1.3	0.194
1986	Matched	0.72	0.72	1	94.3	0.07	0.943
employed	Unmatched	0.80	0.76	9.1		0.66	0.508
1988	Matched	0.80	0.80	-1.4	84.3	-0.1	0.919
employed	Unmatched	0.79	0.75	8.3		0.61	0.544
1989	Matched	0.79	0.78	0.9	89.7	0.06	0.952
ln labor in-	Unmatched	0.95	-0.20	22.2		1.62	0.107
come 1985	Matched	0.95	1.07	-2.5	89	-0.18	0.861
ln labor in-	Unmatched	1.15	-0.09	24		1.74	0.083
come 1986	Matched	1.15	1.12	0.6	97.7	0.04	0.968
ln labor in-	Unmatched	2.09	0.56	31.8		2.3	0.022
come 1987	Matched	2.09	1.41	14	56	0.98	0.326
ln labor in-	Unmatched	2.84	1.78	25.6		1.85	0.066
come 1988	Matched	2.84	2.33	12.4	51.5	0.88	0.38
ln labor in-	Unmatched	2.92	2.47	11.7		0.85	0.394
come 1989	Matched	2.92	2.88	1	91.4	0.07	0.944
Individuals in sample	Unmatched	94	125				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 11: Without Tierp

		mean		% reduction		t-test	
Variable	**Sample**	**Treatment**	**Control**	**% bias**	**|bias|**	**t**	**p>|t|**
female	Unmatched	0.50	0.47	5.3		2.98	0.003
	Matched	0.50	0.49	1.3	76	0.63	0.526
age group 1989							
20-29 years	Unmatched	0.15	0.20	-14.6		-8.16	0
	Matched	0.15	0.14	2.4	83.3	1.33	0.182
30-39 years	Unmatched	0.36	0.30	12.1		6.9	0
	Matched	0.36	0.36	0.3	97.2	0.17	0.869
40-49 years	Unmatched	0.38	0.33	9.6		5.44	0
	Matched	0.38	0.39	-2.7	71.6	-1.35	0.178
50-57 years	Unmatched	0.12	0.17	-13.5		-7.52	0
	Matched	0.12	0.12	0.6	95.4	0.34	0.735
education 1990							
10 years or	Unmatched	0.26	0.35	-20.6		-11.54	0
less	Matched	0.26	0.26	0.8	96.2	0.41	0.683
secondary	Unmatched	0.44	0.47	-5.1		-2.88	0.004
	Matched	0.44	0.45	-0.8	83.7	-0.42	0.675
tertiary	Unmatched	0.29	0.17	29.2		16.95	0
	Matched	0.29	0.29	0	100	0	1
missing	Unmatched	0.00	0.01	-2.8		-1.58	0.115
	Matched	0.00	0.00	0.8	72.2	0.46	0.647
birth region							
Sweden	Unmatched	0.93	0.91	7.5		4.2	0
	Matched	0.93	0.93	1.4	81.2	0.75	0.453
western	Unmatched	0.05	0.07	-9.7		-5.39	0
country	Matched	0.05	0.05	-0.9	90.7	-0.5	0.62
non-western	Unmatched	0.02	0.01	2.9		1.68	0.094
country	Matched	0.02	0.02	-1.3	54.3	-0.62	0.536
industry 1989							
manufacturing	Unmatched	0.13	0.19	-16.4		-9.1	0
	Matched	0.13	0.13	0.6	96	0.36	0.721
construction	Unmatched	0.06	0.07	-2.6		-1.45	0.148
	Matched	0.06	0.07	-1.1	57.5	-0.56	0.577
trade	Unmatched	0.10	0.10	0.4		0.25	0.804
	Matched	0.10	0.09	2.5	-461.6	1.25	0.21
education	Unmatched	0.06	0.05	5		2.84	0.004
	Matched	0.06	0.06	-1.4	72.3	-0.65	0.514
health care	Unmatched	0.12	0.08	12.2		7.08	0
	Matched	0.12	0.12	1.6	86.7	0.77	0.444
other types of	Unmatched	0.07	0.08	-2.3		-1.3	0.192
care	Matched	0.07	0.08	-2.3	2.3	-1.14	0.254
public admin-	Unmatched	0.06	0.04	10.9		6.38	0
istration	Matched	0.06	0.06	1.9	82.7	0.87	0.382
other	Unmatched	0.22	0.23	-1.8		-1.01	0.31
	Matched	0.22	0.23	-1.2	31.7	-0.62	0.536
unknown/did	Unmatched	0.17	0.17	1.6		0.89	0.374
not work	Matched	0.17	0.17	-0.5	69.8	-0.24	0.813
employed	Unmatched	0.89	0.88	2.9		1.63	0.103
1985	Matched	0.89	0.89	-1.9	34.3	-0.99	0.323
employed	Unmatched	0.90	0.89	3.7		2.11	0.035
1986	Matched	0.90	0.90	-1	73.2	-0.52	0.601
employed	Unmatched	0.91	0.90	3.3		1.85	0.065
1986	Matched	0.91	0.92	-2.1	37.1	-1.08	0.28
employed	Unmatched	0.92	0.91	4		2.26	0.024
1988	Matched	0.92	0.92	-2.2	46.2	-1.14	0.254
employed	Unmatched	0.92	0.90	7.4		4.12	0
1989	Matched	0.92	0.92	-1.2	83.3	-0.66	0.507
ln labor in-	Unmatched	3.72	3.59	4.5		2.55	0.011
come 1985	Matched	3.72	3.75	-1.2	73.4	-0.62	0.533
ln labor in-	Unmatched	3.84	3.71	4.5		2.57	0.01
come 1986	Matched	3.84	3.87	-1.2	72.9	-0.64	0.524

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
ln labor in-	Unmatched	3.92	3.75	6.2		3.47	0.001
come 1987	Matched	3.92	3.99	-2.8	53.8	-1.51	0.13
ln labor in-	Unmatched	4.00	3.81	7.4		4.18	0
come 1988	Matched	4.00	4.04	-1.6	78	-0.87	0.386
ln labor in-	Unmatched	4.08	3.79	10.6		5.92	0
come 1989	Matched	4.08	4.11	-1.2	88.4	-0.67	0.505
Individuals in sample	Unmatched	5085	8729				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 12: Without Storvreta

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.49	0.48	2.4		1.41	0.158
	Matched	0.49	0.49	0.3	87	0.16	0.874
age group 1989							
20-29 years	Unmatched	0.20	0.21	-2		-1.15	0.251
	Matched	0.20	0.20	-0.3	85.2	-0.15	0.882
30-39 years	Unmatched	0.29	0.30	-0.8		-0.47	0.64
	Matched	0.29	0.29	-0.1	83.9	-0.07	0.948
40-49 years	Unmatched	0.34	0.33	2.1		1.23	0.22
	Matched	0.34	0.34	1.1	46.8	0.56	0.574
50-57 years	Unmatched	0.16	0.16	0.4		0.26	0.794
	Matched	0.16	0.17	-0.9	-113.5	-0.48	0.633
education 1990							
10 years or	Unmatched	0.33	0.35	-2.4		-1.38	0.169
less	Matched	0.33	0.33	1	57.9	0.5	0.615
secondary	Unmatched	0.49	0.48	0.4		0.24	0.813
	Matched	0.49	0.49	-1.6	-296.5	-0.81	0.418
tertiary	Unmatched	0.17	0.16	2.5		1.46	0.145
	Matched	0.17	0.17	1.3	49.5	0.63	0.529
missing	Unmatched	0.01	0.01	-0.2		-0.12	0.908
	Matched	0.01	0.01	-1.7	-759.9	-0.82	0.411
birth region							
Sweden	Unmatched	0.95	0.91	15		8.41	0
	Matched	0.95	0.95	-0.1	99.5	-0.05	0.963
western	Unmatched	0.04	0.07	-13.7		-7.69	0
country	Matched	0.04	0.04	-0.6	95.7	-0.34	0.733
non-western	Unmatched	0.01	0.01	-5.7		-3.2	0.001
country	Matched	0.01	0.00	1.7	70.7	1.09	0.275
industry 1989							
manufacturing	Unmatched	0.20	0.19	2		1.18	0.237
	Matched	0.20	0.20	-0.1	95.1	-0.05	0.961
construction	Unmatched	0.08	0.07	5.1		3.02	0.003
	Matched	0.08	0.08	1.8	64.7	0.88	0.376
trade	Unmatched	0.08	0.09	-0.7		-0.41	0.682
	Matched	0.08	0.09	-1.6	-129.7	-0.81	0.42
education	Unmatched	0.06	0.05	3.5		2.09	0.037
	Matched	0.06	0.06	-2.3	33.6	-1.12	0.262
health care	Unmatched	0.10	0.09	3.4		2.01	0.045
	Matched	0.10	0.10	0.5	86.2	0.23	0.816
other types of	Unmatched	0.06	0.08	-5.4		-3.1	0.002
care	Matched	0.06	0.07	-1.1	80.2	-0.56	0.578
public admin-	Unmatched	0.05	0.04	2.6		1.54	0.123
istration	Matched	0.05	0.05	-2.6	0	-1.25	0.211
other	Unmatched	0.20	0.25	-11.1		-6.44	0
	Matched	0.20	0.19	1.7	84.8	0.9	0.371
unknown/did	Unmatched	0.17	0.15	4.8		2.83	0.005
not work	Matched	0.17	0.16	1.4	69.9	0.72	0.472
employed	Unmatched	0.86	0.88	-3.8		-2.26	0.024

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
1985	Matched	0.86	0.86	0.6	84.8	0.29	0.774
employed	Unmatched	0.89	0.89	0.6		0.33	0.743
1986	Matched	0.89	0.89	-0.8	-43	-0.41	0.683
employed	Unmatched	0.90	0.90	-0.4		-0.25	0.802
1986	Matched	0.90	0.90	0.3	25.1	0.16	0.872
employed	Unmatched	0.91	0.91	1.1		0.62	0.535
1988	Matched	0.91	0.91	0.9	17.5	0.44	0.657
employed	Unmatched	0.91	0.90	2		1.19	0.235
1989	Matched	0.91	0.90	2.1	-1	1.04	0.299
ln labor in-	Unmatched	3.49	3.59	-3.6		-2.14	0.033
come 1985	Matched	3.49	3.50	-0.6	84.6	-0.28	0.782
ln labor in-	Unmatched	3.64	3.70	-2.1		-1.26	0.208
come 1986	Matched	3.64	3.67	-1	51.2	-0.53	0.599
ln labor in-	Unmatched	3.72	3.76	-1.2		-0.72	0.469
come 1987	Matched	3.72	3.73	-0.4	68.5	-0.19	0.845
ln labor in-	Unmatched	3.81	3.80	0.1		0.03	0.973
come 1988	Matched	3.81	3.78	0.9	-1488.9	0.45	0.652
ln labor in-	Unmatched	3.86	3.81	1.8		1.07	0.283
come 1989	Matched	3.86	3.81	1.9	-4.8	0.97	0.332
Individuals in sample	Unmatched	5127	10462				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 13: Worked in Uppsala city or further south

		mean			% reduction	t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.47	0.44	5.5		2.25	0.024
	Matched	0.47	0.47	1	81.9	0.41	0.681
age group 1989							
20-29 years	Unmatched	0.16	0.23	-19.6		-8.07	0
	Matched	0.16	0.16	-1.3	93.6	-0.56	0.575
30-39 years	Unmatched	0.36	0.32	7.6		3.11	0.002
	Matched	0.36	0.35	1.4	81.4	0.58	0.562
40-49 years	Unmatched	0.38	0.32	12.5		5.12	0
	Matched	0.38	0.40	-3.1	75.1	-1.26	0.207
50-57 years	Unmatched	0.10	0.12	-5.7		-2.32	0.02
	Matched	0.10	0.09	4.1	27.1	1.82	0.068
education 1990							
10 years or	Unmatched	0.22	0.26	-8.4		-3.45	0.001
less	Matched	0.22	0.20	4.6	45.7	1.98	0.048
secondary	Unmatched	0.45	0.50	-9.8		-4.01	0
	Matched	0.45	0.48	-4.7	51.7	-1.96	0.05
tertiary	Unmatched	0.32	0.24	18.9		7.74	0
	Matched	0.32	0.32	0.3	98.3	0.13	0.897
missing	Unmatched	0.00	0.00	0.3		0.13	0.896
	Matched	0.00	0.00	4.9	-1448.5	2.72	0.007
birth region							
Sweden	Unmatched	0.93	0.93	0.4		0.17	0.865
	Matched	0.93	0.94	-2.5	-509.5	-1.07	0.282
western	Unmatched	0.05	0.06	-1.6		-0.66	0.51
country	Matched	0.05	0.05	2.6	-61	1.12	0.262
non-western	Unmatched	0.02	0.01	2.1		0.85	0.397
country	Matched	0.02	0.02	0.5	77.3	0.19	0.85
industry 1989							
manufacturing	Unmatched	0.13	0.13	-1.2		-0.51	0.613
	Matched	0.13	0.13	0.7	44	0.29	0.772
construction	Unmatched	0.07	0.08	-5.2		-2.14	0.032
	Matched	0.07	0.07	1.4	72.9	0.62	0.535
trade	Unmatched	0.12	0.13	-2		-0.81	0.415
	Matched	0.12	0.12	1.6	20.3	0.67	0.503

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
education	Unmatched	0.04	0.03	4.7		1.91	0.056
	Matched	0.04	0.03	3.7	21.6	1.5	0.135
health care	Unmatched	0.16	0.15	3.6		1.47	0.141
	Matched	0.16	0.16	-0.6	82	-0.26	0.793
other types of	Unmatched	0.06	0.06	2.7		1.09	0.275
care	Matched	0.06	0.07	-2.8	-6.8	-1.12	0.261
public admin-	Unmatched	0.07	0.06	6.7		2.74	0.006
istration	Matched	0.07	0.08	-2.4	64.6	-0.91	0.361
other	Unmatched	0.26	0.28	-3.8		-1.57	0.116
	Matched	0.26	0.27	-2.6	31.8	-1.09	0.276
unknown/did	Unmatched	0.09	0.09	-0.9		-0.36	0.72
not work	Matched	0.09	0.08	3	-243.3	1.28	0.201
employed	Unmatched	0.91	0.89	6.3		2.6	0.009
1985	Matched	0.91	0.91	0.5	92.2	0.21	0.831
employed	Unmatched	0.94	0.92	5.2		2.13	0.033
1986	Matched	0.94	0.93	2.5	52.1	1.06	0.291
employed	Unmatched	0.94	0.94	1.9		0.78	0.438
1986	Matched	0.94	0.95	-1.1	41	-0.48	0.634
employed	Unmatched	0.96	0.95	2.3		0.93	0.35
1988	Matched	0.96	0.96	0.3	87.7	0.12	0.905
employed	Unmatched	0.96	0.95	3.3		1.35	0.177
1989	Matched	0.96	0.95	5	-50.5	2.02	0.044
ln labor in-	Unmatched	4.08	3.98	4.8		1.97	0.049
come 1985	Matched	4.08	4.07	0.5	88.6	0.23	0.819
ln labor in-	Unmatched	4.22	4.13	4.4		1.8	0.071
come 1986	Matched	4.22	4.17	2.3	46.6	0.96	0.336
ln labor in-	Unmatched	4.33	4.27	3.1		1.26	0.206
come 1987	Matched	4.33	4.38	-2.8	10.8	-1.18	0.237
ln labor in-	Unmatched	4.43	4.39	2.8		1.16	0.246
come 1988	Matched	4.43	4.48	-2.9	-1.9	-1.23	0.219
ln labor in-	Unmatched	4.64	4.56	9.1		3.73	0
come 1989	Matched	4.64	4.63	1	89	0.43	0.665
Individuals in sample	Unmatched	3441	3288				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Table C 14: Worked elsewhere

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
female	Unmatched	0.52	0.49	5.6		2.99	0.003
	Matched	0.52	0.50	4.4	20.2	2.1	0.035
age group 1989							
20-29 years	Unmatched	0.19	0.19	-0.9		-0.47	0.637
	Matched	0.19	0.18	1.2	-36.2	0.57	0.567
30-39 years	Unmatched	0.29	0.29	0.4		0.19	0.847
	Matched	0.29	0.28	0.9	-161.2	0.44	0.657
40-49 years	Unmatched	0.35	0.34	1.2		0.63	0.53
	Matched	0.35	0.35	-1.4	-16.3	-0.64	0.521
50-57 years	Unmatched	0.18	0.18	-1		-0.52	0.602
	Matched	0.18	0.18	-0.6	34.5	-0.3	0.762
education 1990							
10 years or	Unmatched	0.34	0.37	-5.9		-3.13	0.002
less	Matched	0.34	0.34	0.4	92.8	0.2	0.841
secondary	Unmatched	0.48	0.47	1.3		0.67	0.501
	Matched	0.48	0.47	0.3	75	0.15	0.882
tertiary	Unmatched	0.18	0.15	6		3.27	0.001
	Matched	0.18	0.18	-0.6	90.1	-0.28	0.782
missing	Unmatched	0.01	0.01	-0.8		-0.44	0.663
	Matched	0.01	0.01	-1.5	-86.9	-0.71	0.478
birth region							

		mean		% reduction		t-test	
Variable	Sample	Treatment	Control	% bias	|bias|	t	p>|t|
Sweden	Unmatched	0.95	0.91	15.9		8.23	0
	Matched	0.95	0.96	-1.8	88.9	-1.01	0.314
western	Unmatched	0.04	0.08	-16.4		-8.42	0
country	Matched	0.04	0.04	1.9	88.4	1.12	0.265
non-western	Unmatched	0.01	0.01	-1.8		-0.95	0.343
country	Matched	0.01	0.01	0	100	0	1
industry 1989							
manufacturing	Unmatched	0.19	0.20	-4.5		-2.41	0.016
	Matched	0.19	0.18	0.6	87.5	0.27	0.786
construction	Unmatched	0.07	0.06	4.4		2.37	0.018
	Matched	0.07	0.07	-0.1	97.9	-0.04	0.967
trade	Unmatched	0.07	0.07	0.9		0.46	0.644
	Matched	0.07	0.07	-0.3	70	-0.12	0.904
education	Unmatched	0.08	0.06	6.6		3.62	0
	Matched	0.08	0.08	0.2	97.4	0.08	0.937
health care	Unmatched	0.08	0.07	5.4		2.93	0.003
	Matched	0.08	0.08	-0.2	96.9	-0.08	0.939
other types of	Unmatched	0.07	0.09	-7		-3.68	0
care	Matched	0.07	0.07	-0.7	89.3	-0.37	0.709
public admin-	Unmatched	0.05	0.04	4.5		2.46	0.014
istration	Matched	0.05	0.05	1.3	70.9	0.6	0.548
other	Unmatched	0.17	0.23	-16.7		-8.8	0
	Matched	0.17	0.16	0.5	97	0.26	0.798
unknown/did	Unmatched	0.23	0.18	12.4		6.72	0
not work	Matched	0.23	0.23	-1	91.9	-0.45	0.652
employed	Unmatched	0.85	0.87	-6.9		-3.75	0
1985	Matched	0.85	0.85	0.3	96.3	0.12	0.906
employed	Unmatched	0.86	0.87	-3.1		-1.68	0.092
1986	Matched	0.86	0.86	2.2	30.3	1	0.317
employed	Unmatched	0.87	0.88	-3.1		-1.66	0.097
1986	Matched	0.87	0.87	1.1	63.3	0.52	0.601
employed	Unmatched	0.88	0.89	-2.4		-1.28	0.202
1988	Matched	0.88	0.88	0.6	73.3	0.29	0.769
employed	Unmatched	0.88	0.88	-0.9		-0.51	0.612
1989	Matched	0.88	0.87	1.7	-82.8	0.8	0.422
ln labor in-	Unmatched	3.26	3.47	-6.5		-3.54	0
come 1985	Matched	3.26	3.25	0.3	94.9	0.15	0.879
ln labor in-	Unmatched	3.40	3.57	-5.4		-2.93	0.003
come 1986	Matched	3.40	3.37	0.9	83.2	0.42	0.677
ln labor in-	Unmatched	3.46	3.58	-4		-2.19	0.029
come 1987	Matched	3.46	3.44	0.5	86.9	0.24	0.807
ln labor in-	Unmatched	3.53	3.61	-2.7		-1.45	0.148
come 1988	Matched	3.53	3.50	0.8	68.7	0.39	0.699
ln labor in-	Unmatched	3.50	3.55	-1.7		-0.9	0.367
come 1989	Matched	3.50	3.48	0.6	61.9	0.3	0.767
Individuals in sample	Unmatched	4485	8051				

Notes: To be able to take logarithms individuals with labor income 0 were ascribed 1 SEK.

Publication series published by IFAU – latest issues

Rapporter/Reports

2015:1 Albrecht James, Peter Skogman Thoursie and Susan Vroman ”Glastaket och föräldraförsäkringen i Sverige”

2015:2 Persson Petra ” Socialförsäkringar och äktenskapsbeslut”

2015:3 Frostenson Magnus ”Organisatoriska åtgärder på skolnivå till följd av lärarlegitimationsreformen”

2015:4 Grönqvist Erik and Erik Lindqvist ”Kan man lära sig ledarskap? Befälsutbildning under värnplikten och utfall på arbetsmarknaden”

2015:5 Böhlmark Anders, Helena Holmlund and Mikael Lindahl ”Skolsegregation och skolval”

2015:6 Håkanson Christina, Erik Lindqvist and Jonas Vlachos ”Sortering av arbetskraftens förmågor i Sverige 1986–2008”

2015:7 Wahlström Ninni and Daniel Sundberg ” En teoribaserad utvärdering av läroplanen Lgr 11”

2015:8 Björvang Carl and Katarina Galic´ ”Kommunernas styrning av skolan – skolplaner under 20 år”

2015:9 Nybom Martin and Jan Stuhler ”Att skatta intergenerationella inkomstsamband: en jämförelse av de vanligaste måtten”

2015:10 Eriksson Stefan and Karolina Stadin ”Hur påverkar förändringar i produktefterfrågan, arbetsutbud och lönekostnader antalet nyanställningar?”

2015:11 Grönqvist Hans, Caroline Hall, Jonas Vlachos and Olof Åslund ”Utbildning och brottslighet – vad hände när man förlängde yrkesutbildningarna på gymnasiet?”

2015:12 Lind Patrik and Alexander Westerberg ”Yrkeshögskolan – vilka söker, vem tar examen och hur går det sedan?

2015:13 Mörk Eva, Anna Sjögren and Helena Svaleryd ”Hellre rik och frisk – om familjebakgrund och barns hälsa”

2015:14 Eliason Marcus and Martin Nilsson ”Inlåsningseffekter och differentierade ersättningsnivåer i sjukförsäkringen”

2015:15 Boye Katarina ”Mer vab, lägre lön? Uttag av tillfällig föräldrapenning för vård av barn och lön bland svenska föräldrar”

2015:16 Öhman Mattias ”Smarta och sociala lever längre: sambanden mellan intelligens, social förmåga och mortalitet”

2015:17 Mellander Erik and Joakim Svärdh ”Tre lärdomar från en effektutvärdering av lärarstödsprogrammet NTA”

2015:18 Regnér Johan and Martin Söderström ”Tidiga insatser för arbetssökande med hög risk för långtidsarbetslöshet”

2015:19 Blind Ina, Matz Dahlberg and Olof Åslund ”Som på räls? Förbättrade pendlingsmöjligheter och individens arbetsmarknad”

Working papers

2015:1 Avdic Daniel “A matter of life and death? Hospital distance and quality of care: evidence from emergency hospital closures and myocardial infarctions”

2015:2 Eliason Marcus “Alcohol-related morbidity and mortality following involuntary job loss”

2015:3 Pingel Ronnie and Ingeborg Waernbaum “Correlation and efficiency of propensity score-based estimators for average causal effects”

2015:4 Albrecht James, Peter Skogman Thoursie and Susan Vroman “Parental leave and the glass ceiling in Sweden”

2015:5 Vikström Johan “Evaluation of sequences of treatments with application to active labor market policies”

2015:6 Persson Petra “Social insurance and the marriage market”

2015:7 Grönqvist Erik and Erik Lindqvist “The making of a manager: evidence from military officer training”

2015:8 Böhlmark Anders, Helena Holmlund and Mikael Lindahl “School choice and segregation: evidence from Sweden”

2015:9 Håkanson Christina, Erik Lindqvist and Jonas Vlachos “Firms and skills: the evolution of worker sorting”

2015:10 van den Berg Gerard J., Antoine Bozio and Mónica Costa Dias “Policy discontinuity and duration outcomes”

2015:11 Wahlström Ninni and Daniel Sundberg “Theory-based evaluation of the curriculum Lgr 11”

2015:12 Frölich Markus and Martin Huber “Direct and indirect treatment effects: causal chains and mediation analysis with instrumental variables”

2015:13 Nybom Martin and Jan Stuhler “Biases in standard measures of intergenerational income dependence”

2015:14 Eriksson Stefan and Karolina Stadin “What are the determinants of hiring? The role of demand and supply factors”

2015:15 Åslund Olof, Hans Grönqvist, Caroline Hall and Jonas Vlachos “Education and criminal behaviour: insights from an expansion of upper secondary school”

2015:16 van den Berg Gerard J. and Bas van der Klaauw “Structural empirical evaluation of job search monitoring”

2015:17 Nilsson Martin “Economic incentives and long-term sickness absence: the indirect effect of replacement rates on absence behavior”

2015:18 Boye Katarina “Care more, earn less? The association between care leave for sick children and wage among Swedish parents”

2015:19 Assadi Anahita and Martin Lundin “Tenure and street level bureaucrats: how assessment tools are used at the frontline of the public sector”

2015:20 Stadin Karolina “Firms’ employment dynamics and the state of the labor market”

2015:21 Öhman Mattias “Be smart, live long: the relationship between cognitive and non-cognitive abilities and mortality”

2015:22 Hägglund Pathric, Per Johansson and Lisa Laun “Rehabilitation of mental illness and chronic pain – the impact on sick leave and health”

2015:23 Mellander Erik and Joakim Svärdh “Inquiry-based learning put to test: long-term effects of the Swedish science and technology for children program”

2015:24 Norén Anna “Childcare and the division of parental leave”

2015:25 Åslund Olof, Ina Blind and Matz Dahlberg “All aboard? Commuter train access and labor market outcomes”

Dissertation series

2014:1 Avdic Daniel “Microeconometric analyses of individual behaviour in public welfare systems”

2014:2 Karimi Arizo “Impacts of policies, peers and parenthood on labor market outcomes”

2014:3 Eliasson Tove “Empirical essays on wage setting and immigrant labor market opportunities”

2014:4 Nilsson Martin “Essays on health shocks and social insurance”

2014:5 Pingel Ronnie “Some aspects of propensity score-based estimators for causal inference”

2014:6 Karbownik Krzysztof “Essays in education and family economics”