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Causal effects of subsidized career breaks

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WORKING PAPER 2005:17

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ISSN 1651-1166

Causal effects of subsidized career breaks*

by

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June 30, 2005

Abstract

The paper uses a quasi-experimental situation to analyze the effects of career interruptions on future labor market outcomes. Data are generated by a Swedish program that granted career breaks to applicants until funds where exhausted. Comparing approved and declined (due to lack of funds) applications allows us to derive "pure" effects of interrupted career that are not confounded by selection or omitted variables. The results show no significant effects on working hours but give some support for increased retirement probabilities among the oldest workers. The average wage effect is negative and in the order of 3 percent 1–2 years after the break. Further evidence suggests that one reason for the large negative wage effects may be related to changes in jobs and tasks.

JEL Codes: J31, J22, J24, J26
Keywords: Career interruptions, labor supply, wages, natural experiment

* We wish to thank seminar participants at IFAU, Per-Anders Edin, Magnus Gustavsson and Eva Mörk for helpful comments and the Ministry of Industry, Employment and Communications for financing our surveys, Intervjubolaget and ARS for executing the surveys, and Katalin Bellaagh, Bo Melin and Lotta Nylén for helpful discussions on the survey design.
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1 Introduction

A large part of the work-force in most countries experience interruptions during their labor market careers. Career breaks may occur for several reasons such as unemployment, sickness or parental leave. There is a large literature on the effects of unemployment on future wages and employment. There is also a literature studying other forms of career interruptions where a main focus has been the issue of how maternal leave schemes affect the gender wage gap. This paper supplements these literatures by providing quasi-experimental evidence on how publicly funded career breaks influence future labor market outcomes in Sweden.

There are several reasons why career interruptions may affect future wages. Most obviously, an interruption means forgone experience and thus lost accumulation of skills (Mincer, 1974). Moreover, Edin and Gustavsson (2005) show that unemployed people perform worse on formal tests than they did before the unemployment spell started, suggesting that human capital can be *lost* during periods of inactivity. On the other hand, it should not be ruled out *a priori* that career breaks can be spent accumulating useful human capital. It has also been suggested that career interruptions may send negative signals to employers; people that take e g parental leave, may be considered as less motivated than others (see e g Albrecht et al, 1999). This effect is likely to be stronger in cases where the leave is "unexpected"; e g when fathers take parental leave. Apart from these direct effects, there may also be indirect effects through reduced investments in on-the-job training before the leave.

We are interested in estimating the "pure" effects of subsidized career interruptions, that is the effect for a person who all else equal, receives a career break. This implies that we aim to estimate the total effects from lost experience *and* other effects associated with intermittencies. The ideal situation for identifying the pure effect would be one of random assignment between a continued and an interrupted career. Obviously, such situations do not commonly occur, which is why auxiliary identifying assumptions typically are needed to solve problems of identification.

There are three main problems that need to be addressed: *Unobserved heterogeneity* problems arise if the people taking breaks have specific unobserved characteristics that also affects the outcomes. One way of solving this problem is to rely on unobserved components models, such as fixed- or random effects models. *Anticipation* problems arise if, for example, wages are

lower already before an interruption as suggested by e g Gronau (1988). This would lead models relying on assumptions of permanent differences to underestimate negative wage effects of career interruptions.[1] Finally, problems with *time varying selection* occur if the career breaks are associated with external events that have an independent effect on the future outcome. In fact, this can rarely be ruled out *a priori*: unemployment is associated with job loss, parental leave with having children, and sick leave spells with poor health, all of which may have a direct effect on future wages.

A substantial part of the empirical literature on career interruptions has focused on the effects of unemployment. The applied methods range from OLS (Ellwood, 1982), matching (Eliasson & Storrie, 2004) and sibling comparisons (Skans, 2004) to the use of aggregate instruments (Gregg, 2001) and distributional assumptions (Heckman & Borjas, 1980). All of these methods present attempts to separate unobserved heterogeneity from causal effects. In general, the results suggest that unemployment is negatively related to future labor market performance.

Another strand of the literature looks at the effects of maternal leave on future wages. Some examples include, Arun et al (2004), Baum (2002), Corcoran & Duncan (1979), Corcoran et al (1983), Gronau (1988), Gupta & Smith (2002), Kim & Polachek (1994) and Waldfogel (1997, 1998a, 1998b). Stafford & Sundström (1996) looks the effects of maternity leave using Swedish data. The typical study uses either cross sectional variation, relying on observed characteristics for identification, or fixed (or random) effects estimation and finds negative effects on future wages from women's career interruptions.

There is also a small literature on the effects of sick leave spells on future labor market performance. Hesselius (2004) uses fixed effects estimation on Swedish data and finds a wage penalty of around 5 % for one year's absence.

Finally there are a couple of papers that tries to simultaneously identify the effects of different forms of career interruptions. These papers include Phipps et al (2001) for Canada and Albrecht et al (1999) that studies Swedish data using cross section and panel regressions. The results suggest that unemployment is associated with a much larger wage penalty than parental

[1] Note the similarity to what is referred to as "Ashenfelter's dip" (see Ashenfelter, 1978) in the evaluation literature.

leave, and that fathers are more penalized than mothers after being on parental leave.

This paper studies the effects of participating in a Swedish subsidized career break scheme. We compare the participants with those whose applications were rejected due to insufficient funds. Apart from providing an evaluation of a career-break policy, it is worth noting that this "quasi-experimental" setting provides a rare opportunity to identify casual effects of career interruptions since the rejected applicants provide a comparison group which is likely to have comparable unobserved characteristics up to the point of application. Thus, the identification problems faced by previous studies are not likely to confound the results in this case.

When studying the effects on workers actually taking career interruptions the focus has been on the effects on wages.[2] We believe that there are reasons to explore the causal effects on labor supply as well. There are at least two direct mechanisms through which labor supply may be affected. First, as has been argued by the proponents of the subsidized career break scheme studied in this paper, it is possible that a break may reduce the risk of subsequent sickness, such as for example due to stress related illnesses. Similar effects can influence the chosen age of retirement; if a subsidized leave is a substitute for early retirement, it may have positive effects on the labor supply in the future. Second, it is conceivable that the preference for leisure (or similarly, the stock of information that affects the value of leisure) is increased by a career break. In addition to these direct mechanisms, there may be feedback effects on hours worked through the demand side. If wages are reduced, for example, hours are likely to be affected.

The results show some indications of an increased probability of retirement for those aged at least 60 but there are no significant effects on the labor supply of the average participant, who on average took a 10 month leave, or any changes in the propensity to call in sick. The career breaks did, however, result in a substantially lower wage (3 %). Also, in contrast to what is the case for hours worked, there is significant heterogeneity in the wage results. Workers with less experience and higher previous incomes appear to be more affected.

[2] The main exception is the literature on the effects of unemployment where future employment probabilities is a common outcome variable. Ruhm (1998) also show evidence suggesting that subsidized parental leave schemes may affect women's incentives to participate in the labor force.

We also find that career breaks increase the probability of changing both employer and tasks; however, these career moves do in general appear to have been in a negative direction.

The paper is structured as follows. *Section 2* presents the career break scheme under study. *Section 3* presents that data and methods. Results on hours are presented in *Section 4* and results on wages in *Section 5*. *Section 6* concludes.

2 The career break scheme

This paper studies the effects of subsidized career interruptions using data from a pilot scheme that ran in 12 Swedish municipalities between February 1st 2002 and December 31st 2004. The Green Party of Sweden put subsidized career breaks on top of their agenda in their campaign for the general election of 2000, and the pilot was part of a broader budget compromise between the Green party, the governing Social Democratic party and the Left party during the fall of 2001. This process was repeated during the fall of 2004, and from January 1st 2005, the program is available nationally.[3]

In total 80 (out of 288) municipalities announced their interest to participate in the pilot. The 12 municipalities that were chosen to participate in the pilot were not chosen randomly; according to a press-release from the ministry of Industry, Employment and Communication they were chosen so as to represent the Swedish "regional diversity". In effect, the far north and the far south, as well as (parts of) the two major Swedish metropolitan areas, are all represented among the chosen municipalities.

The career break program subsidized workers on a 3 to 12 months leave during which they are granted 85 % of their unemployment insurance (UI). Since the UI replacement-rate is 80 %, the subsidy amounts to 68 % of previous earnings. However, the Swedish UI-system has two different maximum levels, or "caps", resulting in a maximum compensation at 13,662 SEK (€1,500) per month during the first 100 days and 12,716 SEK (€1,390) per month thereafter (numbers are for 2002).

[3] Similar "programs" are or have been in effect in Belgium, Denmark and Finland.

There are no restrictions on how the time is spent during the break, with the exception of a requirement not to work during the leave. The only conditions under which a person on leave is allowed to work is if the worker is continuing with a minor secondary job or becomes self-employed.

According to the survey in Lindqvist (2004), 55 % of the people on leave were in some form of "recreation", 15 % were caring for family members whereas 22 % were in some form of education and 6 % ran their own firm during their leave. It can be noted that 75 % of those in education did not expect that this schooling would result in any changes in the wage, suggesting that at least part of the education was consumption rather than investment.

To qualify for the subsidy during the pilot an employee had to have at least two years of consecutive employment at an establishment situated in one of the 12 municipalities. The establishment could be either in the private or the public sector. Furthermore, the subsidy is only granted if the employer approves of the career break and consent to hiring a previously unemployed replacement worker.[4] The replacement worker does not have to work at the same position as the person on leave.

It is not obvious why employers approve of subsidized breaks. Survey results suggest they mainly did this to accommodate the employees' wishes (Lindqvist, 2004). However, we know nothing about employers (or employees) in cases where the employers disapproved of the leave.

After receiving the employer's approval, an employee can apply for a subsidized leave at the local Public Employment Service (PES). The PES has a fixed budget (proportional to the size of the labor force in the municipality) for each year.[5] During the pilot the (national) budget was expected to finance 2,000 12-months long interruptions per year. Valid applications were approved until the funds were expired. There were two different applications of this principle; in some municipalities preference were given according to the date the application arrived at the PES, in others according to the preferred starting date of the leave (Fröberg et al, 2003).

In 10 of the 12 municipalities there were more applications than available funds. The fact that the timing of the application determined whether or not an

[4] In practice, however, half of the replacement workers were not unemployed when hired. Also, half of the replacement workers had worked at the establishment previously (Lindqvist, 2004). For an evaluation of the effects on the replacement workers, see Larsson et al (2005).

[5] After the national implementation in January 2005 the budget allows for 12,000 one year long breaks each year.

application was approved generates the quasi-experimental setting that we exploit in this paper. One complicating factor is that one third of the people whose applications were declined received a break later on during the pilot. This was possible since the budget was set on an annual basis so that a second chance appeared later. We will address this problem by using the initial decision as an instrument for whether a person actually went on a leave.

3 Data and methods

We use data on people starting their career interruptions between the start of the pilot in February 2002 and March 31st 2003; information on these individuals were made available from the National Labour Market Board. Furthermore, we collected all rejected applications from the different PES-offices in June 2003. For ease of exposition we will refer to the two groups as assigned (approved applications) and not assigned (rejected applications).

Data on both groups were gathered through telephone surveys on two different occasions.[6] The first survey (henceforth referred to as "Survey 1") during the fall of 2003 focused on background questions and questions regarding what the respondents did during their leave or the time-period corresponding to the leave (Lindqvist, 2004). The second survey ("Survey 2"), during February 2005, asked questions regarding employment, working hours and wages. These responses are used as measures of outcomes.

3.1 Non-responses

Since we use survey data there are issues of non-responses. 72 % of the 3,323 people we contacted for the first survey responded. The main reason for non-responses was failure to contact the respondent either because of incorrect phone numbers or since the respondent was not accessible during the survey period (a more detailed analysis of reasons for non-responses is available upon request). Of those responding to the first survey 73 % also responded to the second survey. Thus, in total we have 53 %, or 1,747 individuals, that responded to both surveys.

[6] The surveys were executed by two different independent contractors, "Intervjubolaget" and "ARS-research".

We dropped 135 observations who stated other reasons than 'lack of funds at the PES' for not being assigned. The most common other reasons were 'no suitable replacement worker was found' or 'unknown reason'.[7] We also drop a very small number of observations with missing background information. Therefore, we have 1,605 observations in our used sample.

Table 1 shows the number of responses to the different surveys by age, gender and registration at the PES offices; the only background information we have for all individuals. We use indicators of registration at the PES 2–4 years before the (intended) start of career break since 2 years of consecutive employment with the same employer was a prerequisite for the application.[8]

[7] We have estimated our models also including those stating that their applications were rejected for 'unknown reason' but none of the results were affected.

[8] The survey data is consistent with this requirement for all but a very small number of individuals.

Table 1 Responses to survey 1 and 2

	Initial population	Responses Survey 1	Responses Surveys 1 & 2	Used sample
Assigned				
All	2,001	1,478	1,110	1,106
Age				
<40	0.216	0.190	0.188	0.186
40-55	0.437	0.438	0.445	0.447
>55	0.347	0.372	0.367	0.367
Female	0.719	0.749	0.777	0.777
*Status 730– 1,460 days before start**				
Unemployed	0.016	0.013	0.013	0.013
Temporary or subsidized employment	0.036	0.034	0.037	0.037
On-the job search	0.010	0.012	0.013	0.013
Not assigned				
All	1,322	908	637	499
Age				
<40	0.250	0.221	0.203	0.212
40-55	0.431	0.445	0.454	0.463
>55	0.319	0.334	0.344	0.325
Female	0.750	0.776	0.818	0.836
*Status 730– 1,460 days before start**				
Unemployed	0.020	0.017	0.017	0.017
Temporary or subsidized employment	0.066	0.061	0.065	0.069
On-the job search	0.007	0.008	0.010	0.009

Note: The reason for the relatively low number of "not assigned" people in the last column is that 135 observations who stated that their application was rejected due to not fining a suitable replacement worker were dropped. *Fraction of days. Employment status is calculated two years before since employment is required during the final two years, see Appendix A for details.

3.2 Validity of the "quasi-experiment"

Figure 1 shows the distribution of (intended) start dates for assigned and not assigned individuals. The purpose of the figure is to show that assignment was indeed determined by the timing of the application. The first panel is for all municipalities, but since every municipality had there own queue, there is some overlap between the groups. The second panel is for Gothenburg (which is by

far the largest participating municipality, see Appendix B) where we more clearly can see a separation between the groups. It should also be noted that Gothenburg was one of the municipalities where the application date (which we do not have data on), rather than the start date, was used to determine which applications should be approved (see Fröberg et al, 2003).

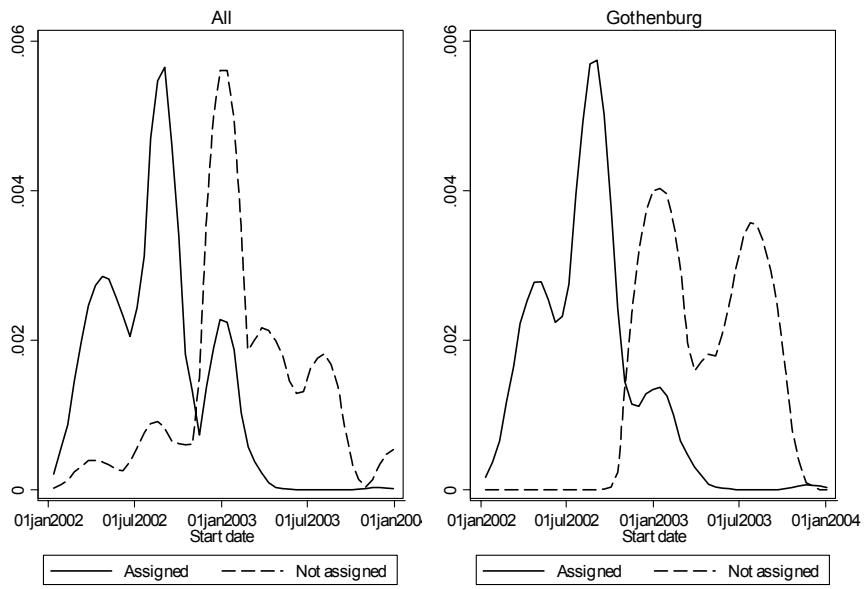

Figure 1 Start dates for assigned (actual dates) and not assigned (intended dates).

Table 2 Characteristics of assigned and not assigned

	Assigned	Not assigned	Municipality corrected difference	Municipality and start corrected difference
Women	0.777	0.836	-0.068***	-0.045
Age				
<40	0.186	0.212	-0.038*	-0.014
40-55	0.447	0.463	-0.007	0.054
>55	0.367	0.325	0.045	-0.040
Married (includes cohabiting)	0.810	0.848	-0.004	-0.043
Having children	0.495	0.555	-0.078***	-0.036
Education				
Primary (< 10 years)	0.149	0.090	0.055***	0.007
Upper secondary	0.392	0.411	-0.000	-0.023
Some tertiary	0.459	0.499	-0.054*	0.016
Experience	25.1	23.1	2.696***	0.914
Tenure	16.6	15.8	1.886***	0.312
Full time employee	0.718	0.713	0.008	0.011
Public employee	0.613	0.715	-0.100***	-0.106***
Monthly pre-tax earnings (SEK) [a]	17,158	17,512	-563**	264
Employment status 730- 1,460 days before start (fraction of days) [b]				
Unemployed	0.013	0.017	-0.006	-0.006
Temporary or subsidized employment	0.037	0.069	-0.029***	-0.010
On-the job search	0.013	0.009	0.003	0.009
Treated	1.000	0.327	0.663***	0.709***
Number of observations	1,106	499	1,605	1,605

Note: See Appendix A for details about variable definitions and Appendix B for further comparisons between the two groups. Corrected differences are from regressions on municipality dummies, in the last column interacted with a linear function in the (intended) start date. [a] See Appendix B for the distribution. [b] Employment status is calculated 2 years before since employment is required during the final two years. * (**, ***) significant at the 10 % (5 %, 1 %) level.

Our identifying assumption is that the timing of the application is excludable from the outcome equation at least conditional on our covariates. This means that whether the application appeared before or after the budget was expired is not directly correlated with the outcomes.

The identifying assumption can, of course, not be tested directly. An informal "test" of the assumption is however provided by looking at whether the two groups differ with respect to *observed* characteristics. Some evidence from Survey 1 is presented as descriptive statistics in *Table 2*.[9] Before looking at the differences it is worth noting that there is a very high share of women and public employees among the applicants. Also the numbers for age and experience are quite high relative to the work force in general. A more detailed inspection of the most frequent occupations shows that e g assistant nurses, mail delivery persons and pre-school teachers are highly overrepresented (see Lindqvist, 2004). Overall the evidence suggests that the take up of the policy is mainly in occupations with low wage dispersion (even by Swedish standards).

Differences between assigned (column 1) and not assigned (column 2) individuals are in most cases small but do exist. However, some differences in the raw data are expected since the municipality distribution may differ. In the third column we therefore present differences after controlling for differences between municipalities. We find that some of the differences are significant; most notably the assigned have somewhat more experience and tenure but lower earnings before their application. They are also to lesser extent public employees. These differences are somewhat worrying since they suggest that there may be differences between the early applicants and the later applicants even within municipalities.

On account of the found differences we perform a further analysis by introducing a municipality specific linear function of the start date, thus creating a "regression-discontinuity" situation (this strategy is also explored in the empirical analysis below). After introducing the linear start-date variable, only the dummy for being a public employee is significant and some of the differences, such as the pre-application earnings, changes signs. Thus, the evidence show that, controlling for a linear function of start date, there are no substantial observed differences between the assigned and the not assigned.

[9] Appendix A presents details regarding variable definitions.

Due to the evidence presented above, we introduce both the observed characteristics and a linear function of the start dates in our empirical analysis (see *Section 3.4* below for a further discussion)

Appendix B presents further comparisons and the municipality distribution. The multivariate relationships are shown in the results sections below.

3.3 Outcomes

Our focus is in this paper is on how career breaks affects hours and wages. The survey respondents were asked to specify the number of hours they worked during the previous week. If they could not give a specific number, they were asked to reply within specified intervals (0, 1–14, 15–24, 25–35, 36+). In the cases were respondents used the intervals we use the most frequent response within the interval instead (1, 8, 20, 30 and 40). If they worked less than 36 hours they were also asked whether they would have preferred to work more and if not so, why.

Depending on the type of contract, people were asked to report their monthly or hourly wages. We convert the responses to monthly wages by multiplying hourly wages by 165 (following SCB, 2004) and by correcting monthly wages for part time work. For 93 % of the sample the estimate is based on a monthly wage. Those with atypical contracts (e g self-employed) were asked about their earnings during the last month, which was corrected for part time employment when applicable. To avoid inducing systematic wage differences from differences on contract type, we include dummies for wages based on hourly wages or monthly earnings in our wage regressions.[10]

Data also contain a number of additional outcome variables, such as employment, unemployment, retirement, the self assessed expected retirement age, sick leaves and whether the person changed jobs or tasks during the last three years (which, effectively means from before the application). Definitions of these variables can be found in *Appendix A*. Descriptive statistics are presented in *Table 3* below. The differences between the columns should not be given a direct causal interpretation since the municipality distribution differs between the assigned and the not assigned. We return to analyze corrected differences in *Section 4* and *Section 5* below.

[10] These dummies do not affect the results.

Table 3 Outcomes for assigned and not assigned

	Assigned	Not assigned
Hours worked last week	26.3 (17.1)	28.4 (16.4)
Employed	0.622	0.651
Unemployed	0.089	0.082
Times on sick leave last 3 weeks	0.484 (0.817)	0.459 (0.798)
Retired if age 60+	0.252	0.125
Full time retired if age 60+	0.286	0.125
Expected retirement age if age 50+	63.2 (2.318)	63.3 (2.286)
Wages	19,745 (3,773)	20,177 (3,701)
Log wages	9.874 (0.180)	9.897 (0.174)
Same job	0.874	0.953
Same task	0.798	0.877
Change in qualifications*	0.330	0.491

Note: Standard deviations in parenthesis. See Appendix A for details about variable definitions. *Only for those that changed tasks, equal to 1 if more qualified task, 0 if equally qualified and –1 if less qualified. Standard deviations are in parenthesis.

3.4 Empirical model

We use initial assignment as an instrument for treatment (i.e. for taking a career-break). However, it should be noted that each municipality had their own budget, and thus their own "experiment". We are therefore studying 10, rather then just one, experiments.

Thus, we include dummies for each municipality (*M*) in our model which explains outcomes (denoted by *Y*) by a dummy for treatment (*T*). Since we found (observable) differences between the assigned and the not assigned in *Table 2* above, our model also include an interaction between municipality and a linear function of the (intended) start date (*S*) as well as a set of covariates (*X*):

$$Y_i = \alpha + \lambda T_i + M_i \mu^M + S_i M_i \gamma^M + X_i \beta^X + \varepsilon_i . \qquad (1)$$

Note that the inclusion of the (intended) start date gives the analysis a "regression-discontinuity" design: We are properly identifying the causal effects of the career break if all (potential) differences between the assigned and the not assigned either are captured by the *X*-variables *or* depend on the start date but are captured by a linear function. Note that the linear start date variables for each municipality effectively are identified from differences *within* each group (assigned and not assigned).

The comparisons of raw means in *Table 2* and *Appendix B* show that the assigned and the not assigned differ in some of the observable aspects, but that very few of the differences remain after controlling for municipality and a linear function of the start date. Thus, as far as this is true also for any unobserved differences, the model should properly identify the causal effects of the career breaks.

Equation (1) is estimated using initial assignment (*A*) as an instrument. Since both the instrument and the explanatory variables are binary it is straightforward to interpret the results as "local average treatment effects" (see Imbens & Angrist, 1994). This means that the estimates measure the effects of a career break for compliers. In this case, compliers are those that (would have) remained at work unless receiving a career break at their first application.

In some specifications we estimate a model using initial assignment as the covariate:

$$Y_i = \alpha + \lambda A_i + M_i \mu^M + S_i M_i \gamma^M + X_i \beta^X + \varepsilon_i . \qquad (2)$$

Equation (2) estimates the direct effect of assignment, which typically is referred to as the "intention to treat effect" in the evaluation literature.

4 Working hours

We start by exploring the effects on hours worked and then turn to an analysis of how wages were affected in the next section. *Figure 2* shows histograms of hours worked for the assigned and the not assigned separately. The picture does not suggest large differences between the two groups.

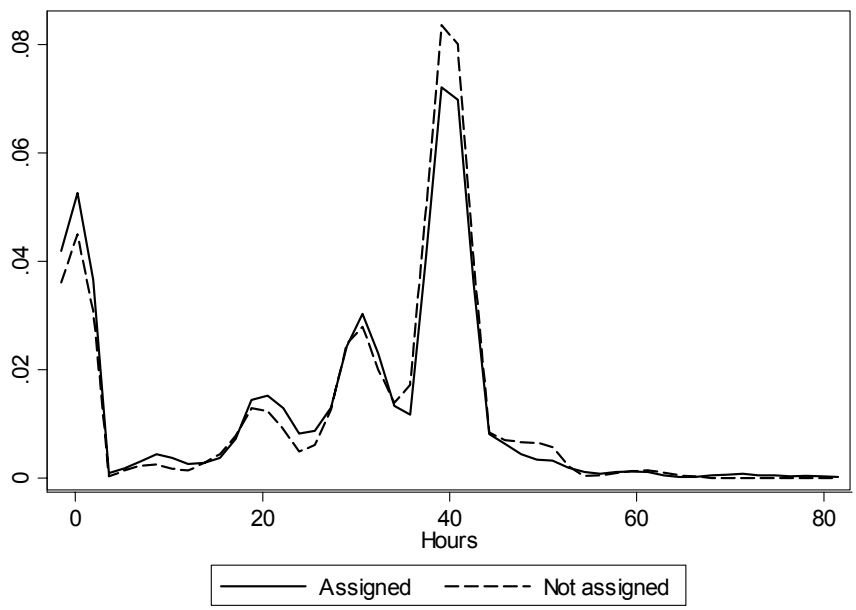

Figure 2 Hours worked in previous week for assigned and not assigned (kernel density).

We continue by looking at estimates from our empirical model based on equation (1) where the dependent variable is weekly hours of work. Later we turn to other indicators of labor supply.

Table 4 Effects on weekly hours worked

Variable	First stage	Intention to treat	IV	IV
	(1)	(2)	(3)	(4)
Assignment			-1.674	-1.465
			(1.792)	(1.904)
Treatment (career break)	0.708***	-1.186		
	(0.025)	(1.267)		
Age	-0.043	-11.829***	-11.901***	
	(0.037)	(2.694)	(2.698)	
Age2	0.001	0.306***	0.307***	
	(0.001)	(0.057)	(0.057)	
Age3	-0.000	-0.002***	-0.002***	
	(0.000)	(0.000)	(0.000)	
Male	0.014	4.326***	4.349***	
	(0.017)	(1.049)	(1.049)	
Less than upper secondary	-0.018	0.369	0.339	
	(0.020)	(1.329)	(1.330)	
Some tertiary or more	-0.025*	1.591*	1.549*	
	(0.014)	(0.922)	(0.922)	
Pre-application experience	0.004	-0.182	-0.175	
	(0.004)	(0.245)	(0.246)	
Squared/100	-0.005	0.084	0.076	
	(0.007)	(0.453)	(0.455)	
Pre-application tenure	0.001	0.191	0.192	
	(0.003)	(0.183)	(0.183)	
Squared/100	-0.003	-0.436	-0.441	
	(0.007)	(0.465)	(0.466)	
Full time employed	-0.001	4.694***	4.693***	
	(0.018)	(1.086)	(1.088)	
Pre-application income	0.004	0.917***	0.923***	
	(0.005)	(0.331)	(0.332)	
Squared/100	-0.000	-0.002***	-0.002***	
	(0.000)	(0.001)	(0.001)	
Public employee	0.004	-0.941	-0.934	
	(0.013)	(0.892)	(0.893)	
N	1,605	1,605	1,605	1,605
R2	0.62	0.17	0.17	0.02

Note: Instrument is assignment. All regressions control for municipality (12 dummies). All except last column also control for (intended) start date interacted with municipality; unemployment, temporary employment and on-the job search (2-years before); marital status and having children. Income is monthly earnings in 1,000 SEKs (≈100 Euros). All control variables refer to before the (intended) leave. Robust standard errors in parentheses. * (**, ***) significant at the 10 % (5 %, 1 %) level.

The results, displayed in the first column of *Table 4*, show that assignment is a strong predictor of treatment as expected. None of the other variables significantly predict treatment, except for tertiary education which is significant only at the 10 % level. Overall, this further supports our notion that the quasi-experiment is valid. Looking at the effects on hours worked we see that all point estimates are negative but highly insignificant. The estimates are very similar if we include or exclude the covariates. It could be noted that including the covariates increases the R-squared from 2 % to 17 % without affecting the estimates of interest, suggesting once again that assignment is uncorrelated with (at least the observed) characteristics of the applicants.

Table 5 Other indicators of labor supply

	Working 36 hours or more	Employed	Not working	Unemployed	Times on sick leave (3 months)
With controls	-0.067	-0.032	0.023	0.004	-0.009
	(0.050)	(0.049)	(0.046)	(0.031)	(0.123)
R2	0.19	0.16	0.13	0.03	0.07
Without controls	-0.068	-0.011	0.033	0.012	-0.044
	(0.044)	(0.042)	(0.036)	(0.023)	(0.081)
R2	0.01	0.01	0.01	0.00	0.02
N	1,605	1,605	1,605	1,605	781
	Aged 60 or more in outcome year (2005)				Aged 50+
	Full time retired	Full or part time retired	Not working	Hours of work	Expected retirement age
With controls	0.214*	0.218*	0.230*	-2.658	-0.619
	(0.112)	(0.114)	(0.138)	(5.111)	(0.428)
R2	0.31	0.29	0.32	0.31	0.12
Without controls	0.163*	0.212**	0.169	-4.399	-0.386
	(0.091)	(0.093)	(0.109)	(3.888)	(0.319)
R2	0.02	0.02	0.08	0.06	0.03
N	270	270	270	270	745

Note: Hours and employment information refers to the previous week. All regressions include municipality dummies. Controls are age (with square and cube), male, primary school, some tertiary or more, pre-application experience (with square), pre-application tenure (with square), full time employed, income (with square), public employee; unemployment, temporary employment and on-the job search (2-years before); married and having children. All these covariates refer to before the application. Robust standard errors are in parenthesis. * (**, ***) significant at the 10 % (5 %, 1 %) level.

We have also experimented with estimating the effect on hours worked for various sub-samples according to for example gender, age, presence of

children, education, sector, experience and tenure without finding any significant effects, neither positive nor negative. The same general picture arises from quantile regressions, all estimates are found to be insignificant.

In the upper part of *Table 5* we show some results from experiments with alternative measures of labor supply. First, we show results for a dummy if working 36 hours or more and again we find a negative but insignificant result. We also look at the probability of being full time employed (working more 36 hour or being absent for cause) and the probability of being unemployed. Even though the estimates point in the direction of a negative effect on labor supply they are all insignificant, thus we conclude that the overall evidence does not give any conclusive support for changes in the labor supply.

Our estimates do not suggest that there are any effects on the propensity to take sick leave after break. The effect on the number of times on sick leave during the last 3 months is negative, but highly insignificant. This analysis is complicate by the fact that some of the non-compliers where in the process of completing their career breaks during the window during which we measure the sick leave propensity. We have tried various ways to address this, but regardless of "solution" the result is insignificant.[11] The presented estimates condition on being employed in December.

In the second part of *Table 5* we present evidence on retirement decisions. The model for effects on the probability of retiring is estimated for those over age 60 (only 3 people in the sample did retire before this age, all at age 57). The resulting estimates suggest that a career break increases the probability of retirement by 21 % with a p-value of 0.058.[12] Note that age is a crucial covariate in this case and it thus seems more likely to trust the specification including age (with square and cube). In the raw data we see that 25 % among the 206 assigned aged 60 or more did retire, the corresponding numbers for the 64 individuals that were not assigned is 12 %. This suggests rather strong effects, but it should be noted that the numbers are based on small samples. Further results, show no effect for average hours worked during last week for this group. The table also show the results from a more speculative analysis based on *expected* retirement age. In this case, we do not find any significant

[11] We have excluded those on career break in December 2004 and tried restricting the sample to those working at least one hour in December 2004, or conditional of being employed in December 2004. In all of these variations we have estimated models with and without controls for previous sick leaves. All estimates have been highly insignificant.

[12] The p-value is 0.014 if we study the direct (intention to treat) effect using a Probit model.

effects, regardless of which age group we study (the table shows the results for ages 50+). To sum up the evidence on labor supply for the oldest workers, we find some indications of increased retirement, but the evidence is not significant for all indicators. Even a conservative interpretation disapproves with the hypothesis of increases in the retirement age put forward by the proponents of the career break scheme.

5 Wages and careers

Turning to wages we follow the career interruption literature and estimate a wage regression and incorporate the subsidized leave. Naturally, this specification does not exclude the possibility of previous career breaks, in fact such breaks are likely to have occurred for most of the sample since the majority are women and many have children. The estimates aim to capture the "marginal" contribution of an additional career break.

Figure 3 show the distributions of log wages for assigned and not assigned individuals. The figure shows some separation between the two distributions, but as noted above the municipality distribution differs between the groups.

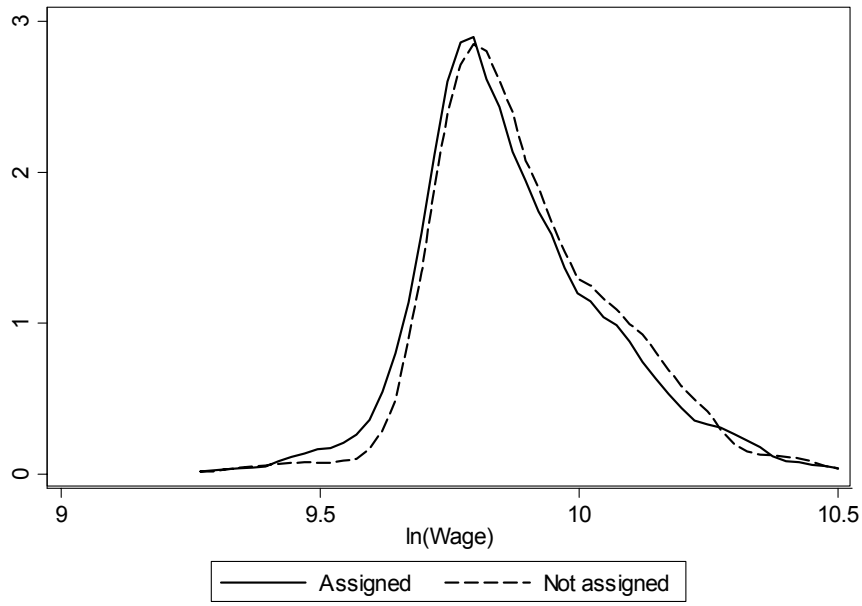

Figure 3 Wages for assigned and not assigned (kernel densities).

Table 6 Effects on log wages

Variable	First stage	Intention to treat	IV	IV
	(1)	(2)	(3)	(4)
Assignment	0.696***	-0.022**		
	(0.027)	(0.010)		
Treatment (career break)			-0.031**	-0.040**
			(0.015)	(0.016)
Age	-0.026	-0.024	-0.025	
	(0.043)	(0.026)	(0.026)	
Age2	0.001	0.001	0.001	
	(0.001)	(0.001)	(0.001)	
Age3	-0.000	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	
Male	0.011	0.022**	0.022**	
	(0.019)	(0.010)	(0.010)	
Less than upper secondary	-0.024	-0.010	-0.010	
	(0.022)	(0.013)	(0.013)	
Some tertiary or more	-0.033**	0.060***	0.058***	
	(0.016)	(0.008)	(0.008)	
Pre-application experience	0.002	0.003	0.003	
	(0.005)	(0.002)	(0.002)	
Squared/100	0.002	-0.004	-0.004	
	(0.008)	(0.004)	(0.004)	
Pre-application tenure	0.000	-0.002	-0.002	
	(0.003)	(0.002)	(0.002)	
Squared/100	-0.001	0.005	0.005	
	(0.009)	(0.004)	(0.004)	
Full time employed	0.003	-0.075***	-0.075***	
	(0.021)	(0.012)	(0.012)	
Pre-application income	-0.004	0.005	0.005	
	(0.011)	(0.008)	(0.008)	
Squared/100	0.000	0.000***	0.000***	
	(0.000)	(0.000)	(0.000)	
Public employee	0.001	-0.014*	-0.014*	
	(0.015)	(0.008)	(0.008)	
N	1,375	1,375	1,375	1,375
R2	0.61	0.49	0.49	0.06

Note: Instrument is assignment. All regressions control for municipality (12 dummies) and wage type. All except column (4) also control for (intended) start date interacted with municipality; unemployment, temporary employment and on-the job search (2-years before); marital status and having children. Income is monthly earnings in 1000 SEKs (≈100 Euros). All control variables refer to before the (intended) leave. Robust standard errors in parentheses. * (**, ***) significant at the 10 % (5 %, 1 %) level.

Note that when studying the effect on wages the experimental set up is not quite as clear as when studying hours. The reason is that we are unable to measure the wage effects on those that do not work. However, given that the effect on labor supply is insignificant, this issue does not appear to be important.

Table 6 shows the results from different estimations related to our IV model. The first stage regression differ marginally from the analysis on hours since the sample is different (we lose 230 observations without wages). As before however, the control variables are insignificant with the exception of tertiary education.

When estimating the wage equations it is worth noting that the control variable estimates may differ from standard wage regression results for several reasons. First, the sample differ, we do not estimate the wage effect for a random sample of workers. For example, due to the cap in the subsidy (see *Section 2*), it is likely that the wage level is one of the main determinants of who applies for the break. Thus we have a much more compressed wage distribution which may explain the modest estimate for the gender dummy. Second, we introduce previous earnings as a covariate, thus we are controlling for much of the cross sectional variation in wages. Third, we introduce experience at the same time as age (as well as tenure); this is mostly to keep the same model as when studying the labor supply effects (where age certainly has an independent effect).

When studying the effects of career breaks on monthly wages we find negative and significant estimates. The intention to treat effect is 2.2 % and the resulting IV-estimate is 3.1 %. If not controlling for any covariates except municipality and wage type, we find a slightly larger estimate of 4.0 %. Note that the estimates of interest are fairly similar even though this simpler model only explains 6 % of the wage variation whereas the model with covariates explains roughly half of the variation in wages. Thus, the differences in observables between the assigned and the not assigned appear to be of only minor importance for the outcome. Nevertheless, the fact that including covariates reduces the estimates somewhat may be of some concern (although the difference is far from significant). In *Table 7* we thus show estimates from additional models to see how different sets of covariates affects the result. We estimate four different models: the raw mean effect (first column), the model including only municipality dummies (second column), a model including also the covariates (third column) and a model including covariates and municipal-

ity-specific start date effects (last column). Including municipality dummies increases the estimates and including the covariates reduces them, while the start date plays no role. The results are all negative and significant. We also show linear wage effects that follow the same pattern: a career break reduces average monthly wages by around 600 SEK (or €54). For robustness we have also looked at the median (intention to treat) effect using a MAD-estimator, the estimate was significant and in the order of the mean effect (as is indeed suggested by figure 3 above).

Table 7 Robustness of the instrumental variables model

	(1)	(2)	(3)	(4)
Log wages	-0.028*	-0.040**	-0.024**	-0.031**
	(0.015)	(0.016)	(0.012)	(0.015)
R2	0.03	0.06	0.49	0.49
N	1,375	1,375	1,375	1,375
Linear wage effect (SEK)	-519.742	-815.244**	-423.719*	-570.246*
	(322.143)	(348.046)	(238.977)	(313.702)
R2	0.03	0.06	0.50	0.50
N	1,375	1,375	1,375	1,375
Municipality dummies	No	Yes	Yes	Yes
Other Covariates	No	No	Yes	Yes
Start date (by Municipality)	No	No	No	Yes

Note: Wage type dummies for whether the wage comes from a monthly wage, an hourly wage or from monthly income are included in all regressions. Other covariates are age (with square and cube), male, primary school, some tertiary or more, pre-application experience (with square), pre-application tenure (with square), full time employed, income (with square), public employee; unemployment, temporary employment and on-the job search (2-years before); married, and having children. All these covariates refer to before the application. Robust standard errors are in parenthesis. * (**, ***) significant at the 10 % (5 %, 1 %) level.

5.1 Effects on jobs and careers

Given that most of the people taking the subsidized career breaks have occupations with relatively modest wage dispersion, it is perhaps surprising that we find such large negative wage effects. In order to further our understanding of the mechanisms at hand we estimate the effects on the probability of remaining at the same job and doing the same tasks as three years ago. Effectively, this means as before the application.

Results presented in *Table 8* show that career breaks increase the probability of changing jobs. We also experimented with estimating the wage effects separately for those changing jobs, and those remaining at the same job. This is highly speculative since the people changing jobs in the two groups may be very different. With this caveat in mind, the estimates suggest that the career movers without the break were much more successful than the movers coming from a break: we find a very large and significant estimate for the (small) population that changed jobs or tasks, and a much smaller effect for those that did not.

Table 8 Effects of career breaks on jobs and tasks

	Dependent variable				
	Same job	Wage effect if same job	Wage effect if changed job	Same task	Change in qualifications
Controlling for covariates and start date	-0.070*	-0.015	-0.139**	-0.084*	-0.266
	(0.037)	(0.016)	(0.056)	(0.046)	(0.193)
R2	0.09	0.51	0.58	0.08	0.28
Not controlling for covariates and start date	-0.095***	-0.037**	-0.105**	-0.084**	-0.228
	(0.025)	(0.017)	(0.053)	(0.034)	(0.155)
R2	0.04	0.05	0.24	0.03	0.11
N	1,375	1,237	138	1,375	243

Note: Wage type dummies are for whether the wage comes from a monthly wage, an hourly wage or from monthly income. Other covariates are age (with square and cube), male, primary school, some tertiary or more, pre-application experience (with square), pre-application tenure (with square), full time employed, income (with square), public employee; unemployment, temporary employment and on-the job search (2-years before); married and having children. All these covariates refer to before the application. Robust standard errors are in parenthesis. * (**, ***) significant at the 10 % (5 %, 1 %) level.

There is also a negative effect on the probability of doing the same task as before the application. For the people who change tasks we estimate a crude model explaining the direction of changes in the required qualifications. We generate a variable with the value 1 if the new task is more qualified (according

to the respondent), 0 if equally qualified, and -1 if less qualified. Estimates provided in Table 8 are insignificant[13] but suggest that there is a move towards (relatively) less qualified tasks for the people on career breaks. Inspection of the raw data shows that the assigned both have fewer cases of upward mobility and more cases of downward mobility.

5.2 Heterogeneity

Since the policy investigated in this paper is fairly particular, it raises the issue of whether the results generalize to other populations. Furthermore, we may be interested in studying whether some groups are more affected than others. To investigate these issues we have estimated the model for various subgroups.

The estimates are displayed in *Table 9*. In all cases we show the group with the largest estimated effect. The estimates are not very precise since the sample sizes are relatively small. However, in general it appears as if the characteristics that are underrepresented in the population under study (for example high previous earnings, male, short experience etc) compared to the average labor force also are those that generate relatively large effects.

This picture is hardly surprising; if we believe that employees and/or employers are rational and react on the expected effects on wages (or productivity) we should see an overrepresentation of characteristics associated with smaller effects among the applicants. It is thus likely that the overall estimates present a lower bound of effects for a random worker taking a career break.

[13] Estimates are significant if we only include observations where the level of qualifications changed.

Table 9 Heterogeneity of instrumental variables estimates

Restriction	Controls	No controls	N	Fraction of total sample
Male	-0.078*	-0.036	283	0.206
	(0.042)	(0.042)		
Age < 45	-0.044*	-0.031	486	0.353
	(0.024)	(0.024)		
Experience <20	-0.063**	-0.034	419	0.305
	(0.026)	(0.025)		
Tenure <10	-0.040	-0.019	356	0.259
	(0.032)	(0.032)		
Tertiary education	-0.031	-0.064***	668	0.486
	(0.020)	(0.023)		
Income > 17,000 SEK	-0.059***	-0.065***	691	0.503
	(0.021)	(0.021)		
Private sector	-0.054*	-0.023	483	0.351
	(0.029)	(0.031)		
Married	-0.040***	-0.032*	1,130	0.822
	(0.015)	(0.018)		
No children	-0.034	-0.061**	637	0.463
	(0.023)	(0.025)		
Working in Gothenburg	-0.073*	-0.101**	463	0.337
	(0.044)	(0.042)		
Municipality dummies	Yes	Yes		
Other Covariates	Yes	No		
Start date (by Municipality)	Yes	No		

Note: Wage type dummies for whether the wage comes from a monthly wage, an hourly wage or from monthly income are included in all regressions. Other covariates are age (with square and cube), male, primary school, some tertiary or more, pre-application experience (with square), pre-application tenure (with square), full time employed, income (with square), public employee; unemployment, temporary employment and on-the job search (2-years before); married and having children. All these covariates refer to before the application. Robust standard errors are in parenthesis. * (**, ***) significant at the 10 % (5 %, 1 %) level.

A further question regarding the interpretation of the results has to do with how the time on leave is spent. Since we do not know what the workers in the comparison group would have done if they had been granted a leave this is a difficult question to answer. However, in order to get some flavor of this we tried estimating a model explaining post-leave wages by the covariates and indicator-variables for time spent in some form of education (23 %) or self-employment (4 %) during the leave. The model only used the people actually taking career breaks and the results, should thus be interpreted with care. The results showed no significant differences depending on time use, the point estimate for self employment was positive (4 %) with a p-value of 0.14. For education the estimate was basically zero and very imprecise. This may reflect selection or that the types of education pursued during the leave was mainly a form of consumption.

6 Concluding remarks

The paper studies the effects on labor supply and wages of being granted a subsidized career break of 3-12 months. The results does not indicate that taking a career break increases future labor supply, the only (marginally) significant results found points in the direction of an increased retirement probability for older workers.

The results on wages are more clear-cut: Hourly wages are reduced by approximately 3 % by the career break. This is in the order of a yearly wage increase,[14] but perhaps surprisingly large given that females, older workers and public employees are overrepresented in the studied population. However, further results suggest that the career breaks led to more changes in jobs and tasks and that these changes were in a negative direction. This suggests that the effects, at least partly, are driven by a less favorable career development, rather than just the wage within a given job. Since the evidence also show that the career breaks increased the probability of changing jobs it is possible that there will be a catch-up over time as tenure is acquired in the new jobs. However, studying this would require a much longer follow up period than is currently available.

[14] According to Statistics Sweden the average monthly wages grew by 3.2 % between 2002 and 2003, see SCB (2004).

When interpreting these results it is important to keep in mind that we are identifying the effect of being granted a career break; any effects through signaling towards the own employer (although perhaps not towards alternative employers) will be washed away since both the treatment and the comparison group advertised their willingness to take a break. Also, characteristics that we typically associate with small returns to experience, such as being female, working in the public sector and having long experience are overrepresented among the workers applying for the career breaks. Thus, it is likely that the presented estimates are lower bounds for the effects a randomly selected worker would experience if taking a career break.

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Appendix A: Variable definitions

Table A1 Variable definitions (continues on next page)

Variable	Source	Comment/restrictions
Assigned/ not assigned	PES register and applications	PES registered the assigned in official registers. Applications for those not assigned from PES offices were gathered manually.
Reason for not assigned	Survey 1: What was the reason for not being granted a leave?	Remain in data if answering 'PES ran out of money' (84 %). Otherwise dropped. Main other reasons are 'not finding a suitable replacement worker' and 'unknown reason'.
Treated	PES registers	All treated are registered.
Municipality of workplace.	PES registers or Survey 1 if assigned. From application if not assigned.	In some cases imputed from area codes of phone numbers.
Actual Start date	PES registers	
Intended Start date (if not assigned)	1) Applications 2) Survey 1 Q: When did you intend to start you leave? 3) Imputed from mean among not assigned in municipality	Not all applications included (readable) start dates. Not all respondents remembered their intended start dates or understood the question correctly.
Monthly Wage	Survey 2: Q1: Monthly wage, hourly wage or other contract? Q2: What was your monthly (hourly) wage (or earnings)? Q3: (If not hourly wage) Is that for full time? Q4: (If not full time) For which fraction of full time was your contract last week?	Based on monthly wage (93 %), hourly wage or monthly earnings (if contract other than monthly or hourly wage). Monthly wage and earnings corrected for part time. Hourly wage multiplied by 165 (following SCB, 2004). Kept if >10,000 & <40,000: drops 17 observations.
Weekly hours	Survey 2 Q1: How many hours did you work last week? Q2: (if not able to answer, [imputed value]): Was it 0 [0], 1–14 [8], 15–24 [20], 25–35 [30], 36 or more [40].	If reason for working less than 36 hour is "being on career break" (14 cases) then use reply from "How many hours did you work the first week in December?"

Table A1 Variable definitions (continued)

Variable	Source	Comment/restrictions
Same job	Survey 2 Q: Are you currently working for the same organization or firm as you did three years ago?	
Same task	Survey 2 Q: Are your main tasks the same as they were three years ago?	
More or less qualified tasks	Survey 2 Q: (if changed tasks) Are your current tasks more qualified, equally qualified or less qualified than the old ones?	
Unemployment (outcome)	Survey 2 Q: (if working less than 36 hours) If your own or another employer had offered you to work more, would you have done so?	Unemployed if wishing to work more
Employed	Survey 2 Q: (if working less than 36 hours and not unemployed) what was the main reason for not wishing to work more?	Employed if either working 36 hours + or absent due to vacation, scheduled leave or due to parental leave or sickness (own or child's).
Retired	As above	If answering "retired".
Preferred retirement age	Survey 2 Q: At which age would you like to retire??	Actual retirement age if retired.
Sick leave (outcome)	Survey 2 Q: How many times have you been on sick leave during the last three months?	
Pre application labor income	Survey 1 Q: What was your monthly labor income (before taxes) before applying?	Imputed from the covariates included in the IV model if no reply (50 cases).
Experience/Tenure	Survey 1 Q: How many years of work experience do you have?	'…with the employer from which you applied for a leave' for tenure.
Unemployment (history)	PES registers (730-1,460 days before start date)	Includes both open unemployment and training programs
Temporary employment (history)	As above	Registered in temporary employment, part time unemployed, subsidized employment etc
On–the–job search (history)	As above	

Note: Survey 1 in fall of 2003, survey 2 in February 2005. Some questions have been abbreviated in the table.

Appendix B: Comparisons of assigned and not assigned

Table B1 Municipality distribution of applications

	Assigned	Not assigned
Botkyrka	0.058	–
Gällivare	0.024	0.108
Göteborg (Gothenburg)	0.374	0.232
Hultsfred	0.025	0.046
Hällefors	0.011	0.042
Katrineholm	0.044	–
Landskrona	0.052	0.028
Lund	0.119	0.076
Piteå	0.070	0.182
Strömsund	0.020	0.052
Västerås	0.159	0.174
Åmål	0.045	0.060
Number of observations	1,105	500

Table B2 Children (before application)

	Assigned	Not assigned
Having children	0.495	0.556
1 child 0-6	0.117	0.148
2 children 0-6	0.045	0.052
>=3 children 0-6	0.007	0.010
1 child 7-16	0.152	0.192
2 children 7-16	0.163	0.150
>=3 children 7-16	0.036	0.036
1 child 17 or older	0.116	0.154
2 children 17 or older	0.025	0.034
>=3 children 17 or older	0.005	0.006
Number of observations	1,105	500

Table B3 Earnings (before application)

	Assigned	Not assigned
Monthly pre-tax earnings (SEK)		
Mean	17,157	17,513
Median	17,000	17,500
<10 000	0.033	0.034
10 000-14 999	0.206	0.204
15 000-19 999	0.524	0.508
20 000+	0.236	0.254
Number of observations	1,105	500
Household income (if married or cohabiting)		
Mean	36,821	36,057
Median	35,000	35,000
<20 000	0.060	0.044
20 000 – 29 999	0.171	0.184
30 000 – 39 999	0.395	0.429
40 000 – 49 999	0.252	0.236
50 000+	0.122	0.106
Number of responses	820	385

Table B4 Sick-leaves (before application)

	Assigned	Not assigned
Number of sick leave spells 12 months before application	1.381	1.428
0	0.424	0.380
1	0.271	0.292
2-5	0.256	0.290
>5	0.049	0.038
Number of sick leave days 12 months before application	14.794	12.768
0	0.424	0.380
<14 days	0.353	0.416
14-28 days	0.097	0.112
1-3 months	0.074	0.052
> 3 months	0.052	0.040
Number of responses	1,105	500

Table B5 Length of break (actual and intended)

	Assigned	Not assigned
Average career break length (months)	9.9	9.1
Median career break length (months)	12	12
Number of responses	1,105	500

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