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**What Are the Long-Term Effects of UI?
Evidence from the UK JSA Reform**

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Abstract
This paper investigates long-term returns from unemployment compensation, exploiting variation from the UK JSA reform of 1996, which implied a major increase in job search requirements for eligibility and in the related administrative hurdle. Search theory predicts that such changes should raise the proportion of non-claimant nonemployed, with consequences on search effort and labor market attachment, and lower the reservation wage of the unemployed, with negative effects on post-unemployment wages. I test these ideas on longitudinal data from Social Security records (LLMDB). Using a difference in differences approach, I find that individuals who start an unemployment spell soon after JSA introduction, as opposed to six months earlier, are 2.5-3% more likely to move from unemployment into Incapacity Benefits spells, and 4% less likely to have positive earnings in the following year. This latter employment effect only vanishes four years after the initial unemployment shock. At the same time, earnings for the treated individuals seem to be lower than for the non treated, but the confidence intervals around these estimated effects are quite large to exclude a wider variety of scenarios. These results suggest that while tighter search requirements were successful in moving individuals off unemployment benefits, they were not successful in moving them onto new or better jobs, with fairly long lasting unintended consequences on a number of labor market outcomes.

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1 Introduction

Despite a substantial literature on the impact of unemployment insurance (UI) on the duration of unemployment and re-employment rates,[1] less is known on its long-term effects on work careers, starting with the first job following an unemployment spell. But the channels through which UI affects the process of return to work, mainly job search effort and reservation wages, are clearly also likely to have an impact on the quality of post-unemployment jobs and in general on future work careers. For example it may be argued that more generous UI gives workers the opportunity of not simply accepting the first job offer that comes along, but of waiting for a good job, that provides the best match for their skills. Indeed the theoretical literature contains a number of papers pointing out that UI may have beneficial effects, mainly by encouraging workers to wait for high-productivity jobs in an environment with search frictions and heterogeneous jobs.[2]

This paper provides new evidence on the long-term returns from UI, exploiting variation from the UK Jobseekers' Allowance (JSA) reform of 1996. The JSA was introduced in October 1996 to replace the previous Unemployment Benefit/Income Support system. This was a major reform to the UK system of welfare benefits for the unemployed, and it was generally perceived as a toughening of the unemployment compensation regime. Indeed, one of the most important changes with respect to the previous system was a substantial rise in search requirements for eligibility and in the related administrative hurdle. There is now broad consensus on the strong positive effects of the JSA on the claimant outflow rate. In particular, the months following JSA introduction coincided with a record fall in the number of unemployment benefit claimants.

In this paper I explore the link between tighter search requirements and a number of post-unemployment outcomes, including future employment rates, weeks worked, earnings and new benefit spells. The impact of higher search requirements on average search intensity is theoretically ambiguous, as some will search more intensively to meet the requirements, while others may consider the requirements too burdensome and give up search (see Manning, 2005), with an ambiguous impact on the exit rate into new jobs. But the introduction of stricter eligibility criteria unambiguosly reduces utility during job search, with negative effects on reservation wages and post-unemployment wages, and raises the share of non-claimants in the nonemployment stock, thus possibly raising the take-up rate of other kinds of benefits.

I use a difference in differences approach to estimate the effects of unemployment compensation on subsequent careers. I compare long-term outcomes for cohorts of unemployment entrants before and after JSA introduction in October 1996. As these two cohorts may differ in seasonal factors,

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[1] See, among others, Atkinson and Mickewright (1991) and Meyer (1995) for extensive surveys of nonexperimental and experimental studies, respectievely, and Lalive, van Ours and Zweimüller (2007) for more recent evidence.

[2] See Diamond (1981), Acemoglu (2001), Acemoglu and Shimer (1999, 2000), and Marimon and Zilibotti (1999).

I construct similar reference cohorts for 1997, and then look at difference in differences across cohorts and years.

There is an aspect of the JSA rules that makes this procedure non-standard, namely that when the JSA was introduced, the new eligibility requirements applied not only to the new claimant inflow, but to the existing stock of unemployed claimants as well, so there is no major discontinuity to expect between labor market outcomes of workers who became unemployed just before and just after JSA introduction. But the distance between the start date of an unemployment claimant spell and the date of JSA introduction is indicative of the spell's probability of being treated, and this will be the basis of my identification. I include in my treatment group all spells started in the three months after JSA introduction, for which treatment probability is equal to one, and in the control group all spells started six to three months before JSA introduction, for which treatment probability is positive but strictly less than one, as some of them may have ended before being subject to JSA rules. Using control and treatment groups defined this way poses a number of issues and requires robustness tests that will be dealt with in detail below.

My empirical analysis leads to three main findings. First, JSA has had a strong, positive and significant impact on the outflow from claimant benefits for the individuals affected, but a null or even negative impact on weeks worked one year later. While the reform successfully managed to move claimants off benefits, it had a much more limited impact in getting them onto new, lasting jobs. Second, I find that JSA has had a negative and significant impact on the probability of positive earnings in the years following an unemployment shock. This effect is about 4% in the first year, and is gradually reabsorbed in the next three years. Post-unemployment annual and weekly earnings (conditional on employment) also seem to be somewhat reduced by the JSA, but the corresponding effects are often do not reach standard significance levels. Third, while JSA has moved individuals off unemployment-related benefits, it has increased the incidence of other benefits, most notably incapacity benefits. Starting a spell soon after JSA introduction, as opposed to six months earlier, implies an increase of 2.5-3% in the probability of claiming incapacity benefits six months after unemployment exit.

The related literature contains a number of papers that look at different aspects of the relationship between the generosity of unemployment compensation and subsequent earnings and job stability. This literature started in the 1970s with studies that exploited individual variation in UI replacement ratios in order to study the impact of UI generosity on post-unemployment wages (see Ehrenberg and Oaxaca, 1976; Burgess and Kingston, 1976; Holen, 1977; and Classen, 1977), and contains some recent contributions that use quasi-experimental evidence to quantify its impact on both post-unemployment earnings and job stability (see, among others, Card, Chetty and Weber, 2007, and Van Ours and Vodopivec, 2006). The majority of studies in this literature tend to find

zero or very modest effects of UI generosity on the quality of post-unemployment jobs, across a variety of institutional backgrounds and econometric methods.[3]

My work complements existing evidence on post-unemployment impact of UI with three main contributions. First, I use social security data containing complete labor market histories, which provide a more long-term perspective on the impact of UI than previously addressed in the literature. The large sample size of the data used also allows me identify the effects of interest using narrowly defined cohorts of unemployment entrants and to test for anticipation effects of the JSA. Second, UI systems have several institutional features, and I estimate the effects of major changes in job search requirements, while most of the previous literature focused on the effects of either changes in UI benefit levels or in their maximum duration. As it will be illustrated below, an increase in search requirements is predicted to lower reservation wages even when the actual level of benefits received remains unchanged. Third, I consider a new potential dimension of the long-term effects of UI, namely the start of other benefit spells. This completes the picture of what happens beyond the current unemployment spell, and has consequences for the impact of JSA on total benefit expenditure.

The paper is organized as follows. The next session discussed related work. Section 3 describes the JSA features that are going to be relevant in my analysis. Section 4 proposes a simple job search model to represent the likely effects of JSA. Section 5 describes the data set used. Section 6 presents my methodology and some preliminary evidence. Section 7 presents my findings on the effect of JSA on a number of post-unemployment outcomes. Section 8 finally concludes.

2   Related work

This work is related to two main strands of literature on welfare reforms, namely the large existing literature on the impact of tighter job search requirements for UI eligibility, and the much less abundant literature on the long-term effects of UI generosity.

Evidence on the impact of job search requirements on the time spent of benefits is relevant to the analysis of the paper, as this would naturally represent a kind of first stage for more long-term effects of UI. For instance, if time on benefits did not respond to the tightening of search requirements, it would be hard to expect much effect of this on the quality of post-unemployment jobs. There now exists a large body of experimental work on the effects of increased enforcement of search requirements, based on a number of US social experiments carried out in the late 1970s and 1980s. Meyer (1995) provides an extensive survey and evaluation of these experiments, and finds that the adopted combinations of search requirements and assistance was largely successful

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[3]Exceptions are Ehrenberg and Oaxaca, (1976), Burgess and Kingston (1976), Holen, (1977) and Centeno (2004).

in reducing the number of weeks on benefits. At the same time, the impact on weeks worked tends to be less clear-cut, quantitatively weaker and often imprecisely estimated, suggesting that not all transitions off benefits represents new hires. More closely related to this paper, Johnson and Klepinger (1994) find that job seekers assigned to a standard search requirements treatment spend on average shorter time on UI benefits and earn about 3% less in the first quarter following UI than a control group with no search requirements at all.[4]

For the UK there has been a randomized experiment in 1986, the so-called Restart Programme, which randomly assigned claimants who had spent twelve months of benefits (later reduced to six) to treatment consisting in counseling and tighter enforcement of eligibility requirements, and was essentially a precursor to the JSA. The Restart seems to have significantly increased the exit rate from unemployment (Dolton and O'Neill, 1996) and to have had beneficial long-term employment effects for men treated, though not for women (Dolton and O'Neill, 2002).

A UK-based study of JSA may contribute to the evidence provided by the mostly US-based experimental studies in a number of ways. First, it seems that the JSA had a stronger bite on the claimant unemployment outflow than most US experiments, and thus one may expect that findings from the US social experiments may not necessarily generalize to other scenarios. Second, most US experiments involve combinations of search requirements and counselling services, and it may be difficult to determine the relative merits of different measures. Finally, the use of social security data in the evaluation of the JSA provides a more long-term perspective on the impact of UI rules and on a wider variety of outcome measures than typically studied in existing experiments.

The existing literature on the impact of the generosity of UI on post-unemployment outcomes is not as large and less conclusive. Early studies from the 1970s tend to identify the effect of UI on post-unemployment earnings by exploiting individual variation in the replacement ratio. Among these, Ehrenberg and Oaxaca (1976) look at the effect of the UI replacement ratio on the change in earnings before and after unemployment using data from the US National Longitudinal Survey, and find that a 25% increase in the replacement ratio yields a 7% increase in post-unemployment wages for older men, with lower or non significant effects for other demographic groups. Burgess and Kingston (1976) and Holen (1977) follow a similar approach on Service to Claimants data, and estimate that an extra dollar in weekly benefits raises post-unemployment annual earnings by 25 and 36 dollars, respectively. In contrast, Classen (1977) finds no significant effect of UI on earnings using data on claimants from the Continuous Wage and Benefit History.

It can be argued that exploiting individual variation in the replacement ratio is not ideal as this may be correlated with some unobserved individual characteristics, and Cox and Oaxaca

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[4]More recent evaluations of US randomized experiments tend to find negative effects of tighter search requirements on UI duration (see for example Klepinger et al., 1997), although in some cases the estimated effect is at most quite small (Ashenfelter et al., 1999). See also the recent survey by Fredriksson and Holmlund (2006).

(1990) who review this literature tend to dismiss positive findings, and conclude that "one can find no compelling evidence in support of the proposition that UI increases wages because of better matches and increased job stability" (p. 236).

Related studies in the more recent literature are sparse, and tend to conclude that the earnings effects of UI are non-significant or at best very modest. Addison, McKinley and Blackburn (2000) use data from Displaced Worker Surveys and only find (weak) evidence of a favorable impact of UI on post-unemployment earnings when comparing recipients and non-recipients, and even in this case the estimates obtained are substantially smaller than those obtained by earlier studies who found evidence of positive effects. Belzil (2001) and Juraida (2002) look at post-unemployment job duration as a measure of job quality using cohorts of Canadian and US displaced workers respectively. While Belzil finds no causal impact of UI benefit duration on post-unemployment job duration, Juraida finds that UI eligibility actually increases the probability of future layoffs. Card et al. (2007) exploit discontinuities in severance payments and UI benefit entitlement in Austria, based on previous employment history, and find no beneficial effects of either transfer on post-unemployment earnings or job stability. Similar results are obtained by Van Ours and Vodopivec (2006), who exploit the change introduced by a Slovenian UI reform that substantially reduced the potential benefit duration. Finally, Paserman (2007) estimates a structural job search model, and finds that changes in the level of benefits have negligible impact on re-employment wages, and only affect job finding rates via search intensity.

While the driving variation analyzed by all papers in this literature consists of changes on the level and/or in the potential duration of benefits, I will mostly study the impact of changes in job search requirements, as implied by the JSA reform. As shown in Section 3, these requirements can have an effect on the workers' reservation wages even when the actual level of benefit perceived remains unchanged. Moreover, a tightening of search requirements may raise the number of claimants who leave unemployment without finding a job, and such transitions into "non-claimant" nonemployment may have more severe consequences on re-employment outcomes, as they typically imply stronger detachment from the labor market than claimant nonemployment.

3 The UK Jobseeker's Allowance

The JSA was introduced on 7 October 1996 in order to replace the existing system of Unemployment Benefits (UB) and Income Support (IS). UB represented unemployment insurance, was based on previous social security contributions, and was not means tested. IS was an unemployment assistance scheme that was means tested. The JSA has a contributory component (contJSA),

which replaced UB, and a means tested component (incJSA), which replaced IS.[5]

In both the old and the new regime the means-tested component of unemployment compensation was much more important than the contributory component, simply because the majority of unemployment claimants have insufficient social security contributions to be eligible for UB or contJSA, whether at all or in its full duration. For example, only 15% of the ongoing claimant unemployment spells in April 1996 were covered by UB. In my data I cannot distinguish between contJSA and incJSA, but Manning (2005) computes on Labour Force Survey data that in February 1997 again 15% of JSA recipients were receiving contJSA.

The features of JSA that are relevant for this study are the changes introduced with respect to the previous UB/IS system, and the transitional arrangements for individuals receiving either UB or IS when JSA came into action.[6] JSA introduction implied some changes in the duration and level of benefits. UB had a maximum entitlement period of 12 months, and this was halved to 6 months under JSA. In 1996 UB was £48.25 per week for single persons, with a £29.75 adult dependant supplement, while IS was £47.90 for single persons aged 25+, £37.90 for single persons aged 18-24,[7] and £75.20 for couples in which at least one spouse was aged 18+. Thus UB and IS payments were very similar except for young people, who received about 20% less under IS than UB. When JSA was introduced it was initially payable at exactly the same rates and conditions as IS. Thus the only category who would see their benefits cut in the new JSA regime consists on youths who were eligible for UB under the old regime. But because the proportion of UB recipients was low, this change had an arguably limited impact. Nevertheless, most of the results below are presented separately for the 16-24 and the 25-64 year old groups.

The most significant break with respect to the previous UB/IS regime was represented by the substantial increase in job search requirements for eligibility and in the related administrative hurdle. Claimants have to sign a Jobseeker's Agreement in which they agree to actively seek work and commit to a number of specific search steps in order to find work, like how may employers at least they are going to contact every week, or how many times at least they are going to contact a Jobcentre. They are required to keep a detailed diary of search steps undertaken, such as each phone call made to a potential employer. The search diary is then checked against the initial agreement at fortnightly interviews with the Employment Service, or more often if a claimant is suspected of fraud. Claimants may be "directed" by the Employment Service staff to take specific steps, and if a claimant is still unemployed after 13 weeks, he is required to broaden his search

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[5]After JSA introduction there is still a benefit called IS, but it is not job-search related, and provides means-tested welfare to selected demographic groups, most notably lone parents and carers of dependants with disabilities.

[6]A very detailed description of institutional and administrative aspects of the JSA is contained in the Jobseeker's Handbook by Pointer and Barnes (1997). The pre-existing UB/IS system is covered by Finn et al. (1996).

[7]16 and 17 year olds were also eligible for the £37.90 IS rate if living away from their parents or qualified for a disability premium; otherwise were entitled to a £28.85 reduced rate.

and may not turn down job offers outside his main occupation (although it can be argued that these measures are hardly enforceable, in so far one has control on job offers received). Failure to meet the above requirements is threatened with temporary sanctions or disqualification.

Although the new JSA rules fit in a trend of tighter eligibility for unemployment compensation, started in 1986 with the Restart Programme for those unemployed longer than twelve months, JSA introduction represented a marked change in entitlement rules and in required interaction with the Employment Service.

As this work is mostly going to focus on cohorts of unemployment entrants during the year of JSA introduction, transitional arrangements from the UB/IS system to the JSA are going to play an important role in my choice of methodology. During the pre-JSA period, all UB spells started on or before 8 April 1996 and before 7 October 1996 had a maximum 6 (instead of 12) months entitlement at the UB rate. More importantly, all existing UB and IS spells as of 7 October 1996 are transferred to the JSA system, and claimants had to fill a Jobseeker's Agreement soon after 7 October, and "were treated as having made a Jobseekers' Agreement until the date in which an actual Agreement is made" (Finn, Murray and Donnelly, 1996, p.64), using information provided in their initial UB or IS form. The retroactive applicability of JSA was very much in the spirit to sanction "those who were not previously assiduous in their job search or were claiming fraudulently" (Rayner et al, 2000, p1).

The JSA has been generally perceived as a major reform of the UK welfare system for the unemployed, and some of its effects can be easily grasped by looking at time series of seasonally adjusted flows in and out of registered unemployment, shown in Figure 1. Soon after JSA introduction, there was a marked increase in the claimant outflow, with little or no impact on the inflow into the claimant register. As Figure 2 shows, this translated into a more rapid decline in the unemployment stock, which was already falling in the months preceding the reform. Indeed official evaluations of the JSA carried by the then Department of Social Security (now Department for Work and Pension) agree in documenting a very strong impact of the JSA on the flow off the unemployment claimant register[8]. More recently, McVicar (2006) studies a case of excused signing (and thus zero monitoring of search effort) within the JSA, during refurbishment of benefit offices in Northern Ireland. He finds that periods with no monitoring strongly reduce the exit rate from benefits. However, optimistic conclusions on job search and employment effects of search monitoring do not seem to be granted. Manning (2005) finds in fact that the JSA did not result in an overall increase in job search effort, nor in higher job-finding rates. The next section illustrates how these developments may in turn result in lower post-unemployment earnings and/or higher labor force exits.

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[8]See for example Rayner et al. (2000) and Smith et al. (2000).

4   A simple job search model

A simple job search model is a useful framework to illustrate the likely impact of higher job search requirements on post-unemployment outcomes. The model is in the wage posting tradition, with an exogenous wage distribution and endogenous search effort, as in Mortensen (1986). With respect to Mortensen (1986), I assume that only the unemployed search for jobs, as this is the key aspect affected by the JSA reform, while employed job search, though empirically important, would not affect the relevant predictions of the model.

Individuals are infinitely lived, and maximize lifetime utility in continuous time. They can be either employed or unemployed. When unemployed, they are paid unemployment compensation b, and spend job search effort s, assuming for the moment that b is not conditional on s. Search effort in turn costs $c(s)$ and generates job offers at rate $\lambda(s)$. It is typically assumed that search costs are convex in effort, while returns are concave, thus $c'(s) > 0$, $c''(s) > 0$, $\lambda'(s) > 0$, and $\lambda''(s) < 0$. Offers are random draws from an exogenous, known distribution $F(w)$. When employed, individuals are paid a wage w and face an exogenous risk of job loss δ.

Given this environment, the unemployed pick an optimal job search effort s, and a reservation wage w_R, representing the lowest acceptable wage offer. The flow value of unemployment and employment can be written as follows, respectively:

$$rU = \max_{s, w_R} \left\{ b - c(s) + \lambda(s) \int_{w_R} [W(w) - U] \, dF(w) \right\} \tag{1}$$

$$rW(w) = w + \delta[U - W(w)], \tag{2}$$

where r represents the intertemporal discount rate.

The reservation wage is defined by $rW(w_R) = rU$, i.e. it is the level of the wage that makes employment equally valuable as unemployment, and given (1) this also implies $rU = w_R$, i.e. the flow value of unemployment is equal to the reservation wage. Using integration by parts to rewrite (1), and noting that $W'(w) = 1/(r + \delta)$, the reservation wage is implicitly defined by

$$w_R = rU = \max_s \left\{ b - c(s) + \frac{\lambda(s)}{r + \delta} \int_{w_R} [1 - F(w)] \, dw \right\}. \tag{3}$$

Search effort is set optimally at the level that equates the marginal costs of search with the marginal benefits, represented by the higher arrival rate of offers, times the associated net gain with respect to unemployment:

$$c'(s^*) = \frac{\lambda'(s^*)}{r + \delta} \int_{w_R} [1 - F(w)] \, dw. \tag{4}$$

The key issue here is how w_R and s^* respond to changes in the institutional environment, which for the moment is simply summarized into the parameter b. The reservation wage clearly

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increases with w_R, as b directly affects unemployment income, which is forgone when one finds a job. In particular:

$$
\begin{aligned}
\frac{dw_R}{db} &= r\frac{dU}{db} = 1 - \frac{\lambda(s)}{r+\delta}\left[1 - F(w_R)\right]\frac{dw_R}{db} \\
&= \frac{r+\delta}{r+\delta+\lambda(s)\left[1 - F(w_R)\right]} > 0
\end{aligned}
\tag{5}
$$

A rise in b affects s^* via its effect on w_R, and in particular it lowers s^* because by raising w_R it lowers the net returns to job search. Formally:

$$
\frac{ds^*}{db} = \frac{\lambda'(s^*)\int_{w_R}\left[1 - F(w)\right]dw}{r+\delta+\lambda(s^*)\left[1 - F(w_R)\right]}\left[\frac{\lambda''(s^*)}{r+\delta}\int_{w_R}\left[1 - F(w)\right]dw - c''(s^*)\right]^{-1} < 0.
$$

The rate at which the unemployed find work is $h = \lambda(s^*)[1 - F(w_R)]$. Thus a reduction in b raises the job finding rate via both an increase in job search effort (and thus in the arrival rate of offers) and a reduction in the reservation wage (and thus a fall in the rejection rate). At the same time, it lowers the expected post-unemployment wage, $E(w|w > w_R)$.

Graphically, this can be seen by drawing indifference curves in the s, b space (as also done by Manning, 2005). Utility while unemployed monotonically increases in b, while it increases in s for $s < s^*$ and decreases in s for $s > s^*$:

$$
r\frac{dU}{ds} = \frac{r+\delta}{r+\delta+\lambda(s)\left[1 - F(w_R)\right]}\left\{\frac{\lambda'(s)}{r+\delta}\int_{w_R}\left[1 - F(w)\right]dw - c'(s)\right\}.
\tag{6}
$$

The indifference curves thus look like those represented in Figure 3, which also depicts the effect of a fall in b. Higher curves are associated with higher levels of utility.

But as argued above, a change in the level of benefits was probably not the main feature of the JSA, which instead mostly implied a change in search requirements for eligibility. Imagine now that unemployment benefits are paid at the full rate for s equal to or higher than some threshold $\underline{s}$, and at some lower rate otherwise. The introduction of JSA implies an increase in such threshold.

Consider an individual who has indifference curves as those represented in Figure 4. The increase in requirements from $\underline{s}_1$ to $\underline{s}_2$ would raise his optimal search effort from s_1^* to the corner solution $s_2^* = \underline{s}_2$, and he would move on to a lower indifference curve, characterized by lower utility and lower reservation wages. His job finding rate is thus higher, and these are precisely the "intended" consequences of the JSA. Consider now an individual whose initial search effort is lower, as illustrated in Figure 5, such that he barely meets the more lenient requirements, i.e. $s_1^* = \underline{s}_1$. With the new requirements he would actually reduce his search effort. In other words, not only would he not meet the new requirements $\underline{s}_2$, but also it would no longer be worthwhile for him to keep his search effort as high as $\underline{s}_1$, thus $s_2^* < s_1^*$. With lower reservation wages and

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lower search effort, the effect of the increase in search requirements on the job finding rate of the unemployed is ambiguous. These are "unintended" consequences of the JSA, emphasized by Manning (2005) as one of factors why the JSA was not really successful at moving the unemployed on to new jobs.

This framework delivers two main results that are going to be relevant for the empirical analysis that follows. First, in any UI regime, workers with $s^* \geq \underline{s}$ are going to be formally classified as UI claimants, while those with $s^* < \underline{s}$ are non-claimants. Thus changes in $\underline{s}$ affect the composition of the nonemployed between claimants and non-claimants. To see this, note that changes in $\underline{s}$ do not affect optimal search intensity for workers with either very high initial search effort, i.e. $s_1^* \geq \underline{s}_2$, or very low initial search effort, i.e. $s_1^* < \underline{s}_1$. The former will be UI claimants in both regimes, while the latter will always be non-claimants. But workers who pick initial search effort in the middle range $\underline{s}_1 \leq s_1^* < \underline{s}_2$ are affected by the change in search requirements. All of them are initially claiming UI: some of them will find it optimal to search harder when $\underline{s}$ is raised (as in Figure 4), and keep claiming UI; while others will reduce search effort (as in Figure 5), and stop claiming UI in the new regime. This implies that an increase in $\underline{s}$ will raise the share of non-claimants among the nonemployed population. They can be either non-claimant unemployed[9] or nonparticipants, and may or may not receive benefits that are not job search related.

Second, whether optimal search effort increases (Figure 4) or decreases (Figure 5), utility enjoyed when unemployed unambiguously falls as a consequence of an increase in $\underline{s}$, and this holds even when the actual level of benefits received remains unchanged. This happens because some cash payments that were initially made to the unemployed without too much questioning are now made conditional on substantial search effort, with some associated costs. Thus one would expect that an increase in $\underline{s}$ lowers reservation wages and the quality of post-unemployment jobs.

If on top of higher search requirements, the level of benefits is also falling, as it was the case for workers aged 18-24 who were receiving UB before JSA introduction, this generates a stronger fall in the reservation wage (as shown in Figure 3), and lowers the incentive to raise search effort and meet the higher requirements, thus raising the proportion of workers who reduce search effort as a consequence of JSA introduction. Thus any fall in the level of benefits would simply reinforce the effects of tighter search requirements on both post-unemployment wages and outflows into non-claimant unemployment.

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[9]To fall in this category, a worker may not meet the JSA search requirements but meet instead the ILO unemployment definition, which classifies as unemployed those who have not worked more than one hour during the reference period but who are "available for and actively seeking work".

5 Data

The data used in this paper are drawn from the Lifetime Labour Market Database (LLMDB), administered by the Department for Work and Pensions. The LLMDB represents a 1% random sample of social security records in Great Britain. Individuals covered are those whose National Insurance numbers end in two given digits. The LLMDB provides a rich set of information on labor histories of selected individuals from 1978 onwards. In particular, I will use information on benefit spells and dependent labor income.

The LLMDB provides start and end dates of benefit spells, together with their type. Types include job-search related benefits, like UB and IS in the old system and JSA in the new system; health related benefits, like Incapacity Benefits (IB) or the Disability Living Allowance (DLA); in-work benefits, like the Working Family Tax Credit (WFTC); retirement pensions; maternity allowances; and a few others.

All information on benefit spells is in principle available since 1978, but the quality of benefit spells data until 1995 is poorer than for the later period. For example, fewer benefit spells seem to be recorded for the earlier period, and a relatively large proportion of them has missing end dates, or imputed end dates.

I use unemployment claimants spells for 1996 and 1997. The LLMDB reports 45,982 unemployment benefit spells started by British males between 1 January 1996 and 31 December 1997. According to the UK Official Labour Market Statistics (Nomis), the male unemployment inflow in the same period was 4,616,199. When LLMDB sampling is taken into account, the figures stemming from the two sources are closely comparable.

Having said this, even in the post 1995 period, the accuracy of information on end dates of spells is less accurate than that on start dates. In particular, the IS end dates have very strong quarterly spikes. This happens because all relevant information about IS spells is collected quarterly by the Department for Work and Pensions; thus if an individual features in the sample with an ongoing IS spell at the start of a given quarter, but has disappeared from the sample at the end of the quarter, he is assigned an imputed completion date corresponding to the middle date of the quarter. As typically IS spells follow UB spells, this bunching problem is going to produce spikes in the end dates of unemployment spells in my sample in the pre-JSA regime. For this reason I choose to minimize the use of the end date of spells in my empirical analysis, and all selection criteria used are based on spells start dates.

In the pre-JSA regime I construct unemployment spells by linking together UB and IS spells that (partly) overlap, and UB and IS spells that do not overlap but have a maximum two weeks window between the end date of the former and the start date of the latter. This is because a spell

out of benefits of less than two weeks is highly unlikely to represent a short job spell, and thus for my purpose the corresponding benefit spells sequence best represents a single unemployment spell. Also, and more importantly, bureaucratic procedures may require some time to move a claimant from unemployment insurance to unemployment assistance benefits, and this may explain some short gap between benefit spells. However, my estimation results were not sensitive to shortening such window to 7 or zero days.

Information on employment and income is provided by fiscal years. Fiscal years in the UK start on 6 April of a given year and end on 5 April of the following year, and in what follows all annual indicators reported refer to fiscal, rather than calendar years. Employment and income are represented by annual weeks worked and annual pre-tax pay, respectively. Both measures are available from 1978 onwards. However, it should be noted that while from 1999 onwards the number of weeks worked is reported directly within each National Insurance file, this has been estimated by the Department for Work and Pensions for the period 1978-1998 using information on known periods of nonemployment and self-employment. When applied to the post 1999 period, this methodology reproduces fairly accurately the actual measure of weeks worked available (Needham, 2007).

Income data from the LLMDB have two main shortcomings. First, the LLMDB does not currently contain employment spells dates, but it reports the number of employment spells recorded in a given year, so that it is possible to know how many jobs someone has held in a given year, with the associated weeks worked and pay, but it is not possible to know their start and end dates, nor their chronological order. This implies that the best measure of wages from this survey is average weekly wages over a fiscal year.

Second, the LLMDB does not provide information on weekly hours worked. This is mostly a drawback for the analysis of female wages, given that the incidence of part-time work among British women was fairly high (around 42% according to the Labour Force Survey) during my period of observation. Thus I limit my analysis to British males.

Figure 6 display raw data on employment and earnings from the LLMDB between 1978 and 2003. The average number of weeks worked in a year declined steadily in the sample period, while both annual and weekly earnings increased. Weekly earnings were increasing at an average rate of 5.8% per year during the sample period, and this corresponds to an average 1.2% real growth.[10] It is worthwhile to notice the blip in annual weeks worked in 1997-1998 and the dip in annual earnings in 1996. The apparent anomalies could be potentially explained by the fact that recording methods changed in 1997, and the LLMDB was moved on to a new National Insurance computerized system. The move from the old to the new system may in part explain the observed

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[10]Using the retail price index from the Office for National Statistics.

changes in variables of interest between 1996 and 1997.

6 Methodology and preliminary evidence

In order to assess the long-term effects of JSA exposure on job quality, one needs to take into account the retroactive nature of the reform, which applied to all unemployment claimant spells as of 7 October 1996, including those started during the previous UB/IS regime. In particular, this feature rules out major discontinuities in the relationship between the start date of an unemployment spell and future outcomes. I will thus choose to compare outcomes for cohorts of unemployment entrants that are close enough in entry dates to be reasonably similar in aggregate factors, but far enough to have significantly different probabilities of being treated by the JSA.

For a treatment group I use claimant unemployment spells for males aged 16-64, started in the three months following JSA introduction, and precisely between 7 October 1996 and 5 January 1997. All these spells are subject to the JSA rules. For a control group I use spells started six to three months before JSA introduction, that is between 8 April and 7 July 1996. These spells are initially not subject to JSA rules, but eventually become subject if they last beyond 7 October 1996. Thus the distinction between treatment and control is based on different intentions to treat.

There are a number of issues to be discussed to understand how good a control group this would be. First, individuals in the control group are not be treated initially, but become treated if they last beyond 7 October 1996. Thus the most direct interpretation of the resulting estimates is would provide the effect of being treated by JSA, as opposed to not being treated in first 3-6 months of unemployment. But further assumptions would be needed to allow a more general interpretation. For example, if the treatment probability were randomly distributed among individuals in the control group, conditional on observable characteristics, then the issue would be simply one of adequately re-scaling the obtained effect of JSA. For example, in my sample this probability happens to be almost exactly 50%, and thus the coefficients obtained on these treatment and control groups should be multiplied by two.

But the probability of being treated in the control group depends on the timing of job finding, and this is in general affected by unobserved characteristics that define someone's employability, such as motivation, ability, search effort etc. If the less-employable are also the less able in the labor market, individuals who end up being treated in the control group have lower average unobserved ability. Thus, what matters for the direction of the associated bias is whether the JSA is going to have a stronger impact on post-unemployment earnings for the more or the less able workers. If the former is true, the estimated effect of the JSA obtained on these treatment and control groups overestimates the true effect, once scaling has been taken into account. If the latter is true, as it is

plausible, one obtains an underestimate of the true effect. My estimates control for detailed past employment histories, which should act as a good proxy for a number of relevant unobservables (see Card and Sullivan, 1988). As a robustness check, I will also perform a test solely based on the short-term unemployed, so that the control group only contains spells that ended before JSA introduction.

Second, I select control and treatment groups on the timing of job loss, and more precisely, on the timing of signing-on for unemployment benefits. One may worry about strategic behavior in the time of signing-on in the presence of anticipatory effects of JSA. And in principle individuals may try to alter the signing-on behavior in the face of JSA by (i) signing-on earlier than they would have done without the JSA; (ii) signing-on later; (iii) not signing-on at all. But how likely is this kind of strategic behavior prior to JSA introduction? It may be argued that trying to sign-on (shortly) earlier does not avoid treatment, as JSA is retroactive; signing-on later simply implies loss of unemployment income, thus is clearly not optimal; and finally not signing-on at all implies again loss of unemployment income: if one really dreads the prospect of the JSA interview it is optimal to sign-on initially and then not show up for the first JSA interview.

Some indirect evidence on this can be grasped by looking at Figure 7, which gives the number of claimant unemployment spells started between 1 January 1996 and 31 December 1997, and shows no sign of any unusual behavior in the unemployment inflow around the time of JSA introduction. Figure 8 provides a closer snapshot of the two months around JSA introduction. This reveals a marked weekly pattern in starting dates, with Mondays being by far the busiest days, and the frequency of new spells declining monotonically during the week, but again there is no evidence of bunching of new spells shortly before or after 7 October.

It would be interesting to be able to observe the same kind of evidence in the unemployment outflow, but as alrealy noted in Section 5 the LLMDB data are not ideal for this purpose, due to heavily bunched ending dates of IS spells, which produce sizeable spikes in the end dates of claimant unemployment spells, as shown in Figure 9. But official labor market data reported in Figure 1 show no unusual behavior in the unemployment outflow just before JSA introduction, with a strong fall immediately afterwards.

Finally, treatment and control groups are certainly going to be different as far as seasonal factors are concerned. For this reason I construct treatment and control groups for the same dates in 1997, and estimate the effect of JSA on future outcomes using a difference in differences approach. I estimate an equation of the form

$$y_i = \beta_0 + \beta_1 C_i^{96} + \beta_2 T_i + \beta_3 \left(C_i^{96} * T_i \right) + \gamma X_i + \varepsilon_i \tag{7}$$

where y_i represents an outcome variable, X_i is a vector of individual characteristics, C_i^{96} is a

dummy variable for the 1996 cohort, T_i denotes treatment, and their interaction picks the effect of JSA. Specification (7) is going to deliver an unbiased estimate for the coefficient of interest, β_3, if

$$
\begin{aligned}
E(\varepsilon_i | C_i^{96} &= 0, T_i = 0, X_i) - E(\varepsilon_i | C_i^{96} = 0, T_i = 1, X_i) = \\
E(\varepsilon_i | C_i^{97} &= 0, T_i = 0, X_i) - E(\varepsilon_i | C_i^{97} = 0, T_i = 1, X_i).
\end{aligned}
$$

In other words, as treatment and control groups are selected on the basis of their date of job loss, the underlying identifying assumption is that the correlation between the timing of job loss and unobservables, if any, be the same across the two cohorts. This assumption is likely to be violated if there are strong reasons to expect strategic signing-on timing, but I have argued above that this is unlikely. Also, it would be worrying if control and treatment had markedly different observables before JSA introduction, and this can be checked by looking at their employment and earnings histories.

Descriptive statistics for treatment and control groups are reported in Table 1 and Figures 10-13. Table 1 reports information on age and the current unemployment spell. There are around 4,000 spells in each of the groups (treat and control in 1996 and 1997 cohorts). These groups are very similar in their age, but differ in the duration of their current spell and in its destination. The control group in the 1996 cohort tends to have longer spells than the three other groups, and this is the main effect emphasized by the official evaluations of the JSA, although not with a difference in differences approach. This group also has a lower probability to experience new benefit spells in the near future.

More detailed information on annual earnings and weeks worked for the treatment and the control groups is presented in Figures 10-13. Figure 10 gives the proportion of men with positive earnings in each year for treatment and control groups in both the 1996 and 1997 cohorts. The vertical line in correspondence of 1996 represents the introduction of JSA. This coincides with the reference unemployment spell for the 1996 cohort, while the reference unemployment spell for the 1997 cohort takes place one year later. Overall, the fraction of men with positive earnings rises for all groups by over 30 percentage points during the 10 years prior to JSA treatment, and this is mostly an age effect, as the sample is relatively young. After treatment, the trend in such fraction flattens out or even declines. It is also worthwhile to notice that the proportion with positive earnings has a spike in the year of treatment, simply telling that the reference unemployment spell tends to follow in most cases a period of paid employment.

Figure 11 reports the average number of annual weeks worked, conditional on working. In this case the pre-shock trends are falling, and in particular individuals start experiencing negative employment shocks around 6 years before the reference unemployment spell. After the JSA shock,

weeks worked increase, but it seems that part of the increase is due to the 1997-1998 blip in weeks worked in the main database (see Figure 6). Figures 12 and 13 report log annual and weekly earnings, respectively. Both measures of earnings decline sharply in the year of job loss, but otherwise they follow a generally upward trend.

An interesting feature that stands out from Figures 10-13 is that pre-treatment trends are in general very close for treatment and control groups in both cohorts. More importantly, the associated difference in differences is never significantly different from zero for any of the variables considered in the pre-treatment period. Figures 14-17 plot the difference in differences for the same variables represented in Figures 10-13, where year 0 corresponds to 1996 for the 1996 cohort, and to 1997 for the 1997 cohort. In Figure 14 a probit version of equation (7) is estimated; while Figures 15-17 are based on OLS. The solid lines represent the point estimates (and, specifically, marginal effects in Figure 14), and the dashed lines represent the 90% and 95% confidence intervals, showing that, for the four labor market indicators considered, all point estimates lie within the 90% interval in the pre-treatment period. Recall that in order to consistently estimate β_3 one needs that any difference in unobservables between the treatment and control groups be the same across the two cohorts. Using work histories as a proxy for individual unobservables, the evidence presented in Figures 14-17 is in line with my identifying assumption.

It should finally be noted that some of the trends in Figures 10-13 seem to diverge after JSA introduction, and in some cases more for the 1996 than the 1997 cohort, as also shown by point estimates in Figures 14-17 for the post-treatment period. This is indicative of potential JSA effects on future outcomes. The next section will provide more detailed results on these effects.

7 Results

7.1 Employment and earnings

I start by presenting evidence on the effects of JSA on the probability of leaving the unemployment claimant register. Not only was this the main effect emphasized by the official evaluations of the JSA, but also it could be the main channel through which one can expect more long-term effects.

I thus estimate a duration model of exit from unemployment, using a specification analogous to (7), except that the duration model is non-linear. The results of the Cox proportional hazard model are presented in the upper panel in Table 2, where the coefficients reported refer to the interaction between the 1996 cohort and treatment, and thus are supposed to pick the effect of JSA. All specifications also include separate dummy variables for treatment and the 1996 cohort. The standard errors are clustered at the individual level, to cater for individuals with multiple spells in this sample.

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In the regression summarized in column 1, no other regressors are included, and the estimates show a 10% increase in the unemployment exit hazard as a result of JSA. Column 2 also controls for age, age squared, and past employment history (i.e. whether the individual had a claimant unemployment spell in the previous two years, the total number of weeks worked and annual earnings in each of the previous five years and their square).[11] As expected from the evidence presented in Table 1 and Figures 6-9, the inclusion of covariates hardly affects the results. The next four columns show results for the young (aged 16-24) and the adult sample (aged 25-64) separately. The JSA effect is still positive for both groups, but it is stronger for the adult sample, while it does not reach the standard significance level for the young.

However, as information on ending dates of spells is heavily bunched at quarterly frequencies, a continuous time duration model is probably not the best way to describe unemployment exit. Another way to look at the effect of JSA on the outflow from the unemployment register consists in comparing the fraction of the control group who are still claiming upon JSA introduction, i.e. on 6 October 1996, with the fraction of the treatment group who, by symmetry, are still claiming on 6 March 1997, as control and treatment groups are selected as entering unemployment six months apart. This method has also the advantage of excluding a direct JSA effect on the exit probability of the control group. The DID results from a probit model are presented in the lower panel of Table 2, and show again a significant JSA effect on the probability to leave the register, which is now higher for the young than the adult subsample.

Table 2 thus replicates the main result of the JSA evaluation literature, namely its strong and significant impact on the exit rate from unemployment. But moving claimants off benefits may not be equivalent to moving them on to new jobs. The LLMDB does not allow me to fully characterize unemployment destinations, because it does not contain information on dates of employment spells, but I can use information on weeks worked and earnings for the fiscal year after treatment (and for later years) in order to assess the impact of JSA on both employment and post-unemployment earnings.

Figures 18-21 present estimates of the effect of JSA on post-unemployment outcomes. Year 1 thus corresponds to 1997 for the 1996 cohort and to 1998 for the 1997 cohort. These estimates are analogous to the post-treatment estimates presented in Figures 14-17, but unlike in Figures 14-17 they control for a number of observable characteristics, including the pre-treatment trends, and also they distinguish between the young and the prime-age sample. Figure 18 shows the effect of JSA on the probability of having positive earnings in the five years after the reference unemployment spell for the whole sample and for the two age subgroups. JSA implied a reduction of 4% in the probability of positive earnings in the year after the shock for the whole sample,

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[11]Extending employment and earnings histories 10 instead of 5 years back produced virtually identical results.

and this effect is statistically significant at the 1% level. The JSA effect is roughly halved in the second and third year (and it is only significant at the 10% level), and it finally tails off. For those aged 16-24 the JSA effect is stronger to start with, and again is reabsorbed within the next three years. For the prime age subsample the effect is weaker, and becomes non significantly different from zero from the second year onwards. Registering for unemployment benefits soon after JSA introduction, as opposed to six months earlier, implies thus a significant fall in the future employment probability, and this effect is fairly long-lived especially for the youths.

Information on the actual number of weeks worked, conditional on having positive earnings, is presented in Figures 19. The effect of JSA on weeks worked tends to be moderate and is almost never significant for all age groups. Thus it seems that JSA mostly affected re-entry into employment, without much of an effect on the number of weeks worked for those who did re-enter employment.

Figure 20 present evidence on (log) annual earnings. Estimated effects are everywhere negative, again stronger for the younger subsample, although they are often not significantly different from zero. Figure 21 finally present estimates for weekly earnings, which are the variable most closely related to post-unemployment job quality in this dataset. While the effect of JSA on weekly earnings of the older subsample is quite close to zero and never significant throughout the post-unemployment period, the corresponding effect for the younger sample is negative and significant in years 2-4 after the shock. It is probably hard to reconcile such decline in weekly earnings with the direct impact of JSA, because if anything one would expect an immediate effect in the first year after the reference unemployment spell, which is gradually reabsorbed as individuals who are initially mismatched search on-the-job for better matches. Some explanation of this behavior may be related to the employment selection effects of JSA. Figure 18 has shown that the JSA had an important initial impact on the proportion of individuals in work, which fades gradually over the next five years, as the treated catch up with the non-treated in their employment levels. Thus the employment stock may be of relatively high quality among the treated initially, because only the most able have initially found work, and then quality declines as the less-able among the treated find work. This selection mechanism may help explain why one does not find a JSA effect initially, but finds instead a negative effect in the following years.

In summary, the most important effect of JSA on this sample is to reduce the probability to have positive earnings after an unemployment shock, with more moderate and less precise effects on the level of both annual and weekly earnings, and with almost no effect on the number of weeks worked for those who re-enter employment. All effects tend to be stronger and more precisely estimated for the younger sample. One explanation could be that for youths eligible for UB, JSA introduction meant both an increase in search requirements, and a reduction in the benefit

level, with amplified effects on post-unemployment outcomes. But as argued in Section 3 this explanation is unlikely, as the proportion of individuals eligible for UB only represents a minority of observations. The other explanation is that the impact of search requirements alone may be stronger for the youths, and this view seemed to be in line with the introduction of the New Deal for Young People in April 1998, which combined JSA search requirements with intensive help with job search (see Van Reenen, 2003).

7.2   Future benefit spells

Previous estimates show that the JSA raised the unemployment outflow, but at the same time also raised the probability of not working at all in the following year, so one may wonder what happens to individuals who leave the unemployment register but do not get jobs. One possibility is that they may apply for and obtain other benefits, which are not conditional on active job search.

To answer this question I use information on different types of benefit spells contained in the LLMDB. The UK welfare system, like most systems, includes several types of benefits, that can be related to job search, income, health, work etc. For example, during the six months preceding JSA introduction, between April and September 1996, the LLMDB registers about 77,000 benefit spells. The most important category among these is represented by unemployment benefits, which account for about three quarters of ongoing spells. The next category is represented by health-related benefits, including Incapacity Benefits and the Disability Living Allowance. IB can be claimed by individuals who are unable to work because of ill health or a disability, and accounts for about 20% of benefit spells in the pre-JSA period. The DLA is a benefit for individuals who need personal care due to mental or physical disabilities, and accounts for 10% of spells. Finally come in-work benefits, represented by the Working Family Tax Credit, which includes just below 4% of benefit spells. One year later, that is between April and September 1997, there are about 63,000 benefit spells in the LLMDB. The importance of unemployment benefits has declined to about 50%, and that of health-related benefits has increased to 24% for IB, and to 14% for DLA. In-work benefits have also slightly risen to just over 5%.

To look at the impact of the JSA on unemployment exits into other benefits, I estimate a probit version of equation (7), where the dependent variable is equal to 1 if an individual is receiving benefits of any type within 1 month or within 6 months of the end of the reference unemployment spell, and zero otherwise. I then distinguish between new spells on IB, and new claimant unemployment spells, whether on JSA, UB or IS. Destinations into other benefit categories are not separately estimated because they represent a very small minority of my sample (see also descriptive statistics presented in Table 1).

Table 3 reports results for a one-month window. When controlling for characteristics, individ-

uals treated by the JSA are on average 3.1% more likely to experience a new benefit spell within one month of completing their current spell, and this effect is significant at the 5% level. Most of the impact happens among the younger subsample, in which 4.5% of individuals experience new benefit spells; while for the older subsample this figure falls to 2.4% and it is not significantly different from zero. The positive impact of JSA on exit into new benefit spells is explained in roughly equal parts by spells on IB and new claimant unemployment spells, although the corresponding effects only tend to be significant for new spells on IB.

Six months later the situation is more clear-cut, as shown in Table 4. Individuals affected are 3.4% more likely to be benefit recipients. The top panel of Figure 18 above has shown that the JSA increases by 4.3% the probability of not working in the following year, and the estimates of Table 4 tell that the bulk of such rise in nonemployment is explained by a higher take-up rate of new benefits. The same observation holds looking at each age subsample separately. Among new benefits, the biggest component is represented by IB. This last piece of evidence fits in the increasing trend in take-up rates of IB in the UK and is consistent with the widespread view that individuals who had lowest re-employment rates were actually advised by the Employment Service to apply for IB (see Nickell and Quintini, 2002).

7.3   Robustness tests

The adopted definition of control and treatment groups, as well as some features of the data, require a number of robustness checks. First, as noted in Figure 2, the unemployment inflow frequency has a marked weekly pattern, and this may reflect the timing of initial benefit payments, rather than the date a job loser initially approached the Employment Service. I thus converted the benefit spells data from daily into weekly, by moving each start date to the previous and following Mondays in turn, and constructed treatment and control groups in the same way as explained in Section 6. The estimates obtained on this new sample were virtually identical to those obtained on the original one.

Second, as treatment and control groups are selected according to their date of job loss for two consecutive years, one may worry about interactions between seasonal factors and year effects. For example, if the labor market were in general tighter in the fall (when the treatment is selected) than in the spring (when the control is selected), and this effect were stronger in 1997 than in 1996, one could potentially predict poorer lower relative re-employment prospects for the treatment group in 1996 as a consequence of macroeconomic effects. Evidence on macroeconomic effects can be provided by the monthly vacancy to unemployment ratio, which is typically used as a measure of labor market tightness. This ratio increases roughly monotonically in Britain between January 1996 and December 1997, and thus shows no evidence of different seasonal patterns in 1996 and

1997. As a final check, I repeated the main estimates controlling for the value of labor market tightness in the month of job loss, and the results stayed largely unchanged.

Fourth, I worked with a sample in which the control group has by definition a zero treatment probability, by focusing on the short-term unemployed only. That is, I include in the control group spells started between 8 April and 7 July 1996, and ended before 7 October 1996. None of these can be treated by the JSA. For symmetry I include in the treatment group spells started between 7 October 1996 and 5 January 1997, and ended before 7 April 1997, and then repeat exactly the same procedure for entrants in spring and fall 1997. Of course it must be noted that the short-term unemployed are a non-random sample of the unemployed population, and also that the treatment effect needs not be the same across unemployment duration groups. But, in a benchmark scenario in which the short-term unemployed are a random sample of the unemployed (conditional on characteristics), and the JSA effect is homogeneous across duration groups (again, conditional on characteristics), we would expect the estimated JSA effect on this sample to be twice as large as the one obtained in the original sample, in which the control group in the 1996 cohort turns out to have a 50% treatment probability.

The results on post-unemployment work and earnings for the short-term unemployed are represented in Figures 22-25, where for simplicity only results on the two age subsamples are reported. For example, Figure 22 shows that the JSA effect on the probability of positive earnings starts off at about 20% for 16-24 year olds, and at about 6.5% for 25-64 year olds. The corresponding effects for the main sample are 6.3% and 3.8% respectively (middle and bottom panel of Figure 18). Stronger JSA effects than on the main sample are also found in Figures 23-25. In particular, the JSA impact on annual earnings is now initially significant for both age samples (Figure 24), and the JSA impact on weekly earnings becomes significant for 25-64 year olds (Figure 25).

Table 5 represents the impact of JSA on exits into new benefit spells within either one or six months from the completion of the current unemployment spell. Again all effects are stronger than those reported in Table 3 and 4 for the main sample.

The final robustness test consists in a falsification check, based on treatment and control groups for 1997 and 1998, constructed in the same way as I previously did for 1996 and 1997. If my previous estimates identify the effect of JSA, one should obtain no significant effects of an interaction term between the treatment and the 1997 cohort on this new sample, for any of the post-unemployment outcomes considered. This is indeed what I obtain, as shown in Figures 26-29 for post-unemployment employment and earnings; and in Table 6 for exits into other benefit spells.

8 Conclusions

This paper has investigated the post-unemployment effects of higher job search requirements, exploiting variation provided by the introduction of the UK JSA in October 1996. In a simple job search framework, one expects that tighter requirements for UI eligibility lower the reservation wage, with negative consequences on the quality of post-unemployment jobs, and raises the fraction of non-claimant nonemployed.

Using administrative longitudinal data on spells on unemployment benefits and earnings, I find that JSA has had a positive and significant impact on the claimant unemployment exit rate, as well as on exits into other benefits, and a negative and significant impact on the probability of working in the year after the unemployment spell. Starting a spell soon after JSA introduction, as opposed to six months earlier, raises the likelihood of a spell on Incapacity Benefits by about 2.5-3%, and lowers the likelihood of positive earnings by about 4%. At the same time, earnings for the treated individuals seem to be lower than for the non treated after an unemployment shock, but the confidence intervals around these estimated effects are quite large to exclude a wider variety of scenarios. Overall, all the estimated effects tend to be stronger for the 16-24 than the 25-64 years old sample.

A possible interpretation is that tighter search requirements implied by the JSA indeed moved claimants off unemployment benefits, without really raising job finding rates. Among claimants treated by the JSA, those who found jobs quickly did not see their fortunes much changed with respect to the previous regime, as implied by the absence of significant effects on weeks worked and earnings in the year following job loss. But those who left the unemployment register without finding a job were more likely to start spells on benefits that were not search related, and thus to spend lower search effort and become more detached from the labor market than before the JSA, with farily long-lasting effects on their employment rates. According to my estimates, the JSA implied a net loss in (unconditional) weeks worked and earnings with respect to the previous system during about three years after a job loss.

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[35] Van Reenen, J. (2003) "Active Labour Market Policies and the British New Deal in Context" NBER Working Paper 9576.

Table 1
Characteristics of treatment and control groups

	Control		Treatment	
1996 cohort	mean	st.d.	mean	st.d.
Age	32.5	11.8	33.6	12.1
Duration of current spell (days)	200.6	304.1	177.1	277.0
% on new benefit spell within 1 month of completion	0.153		0.190	
Of which: on new claimant unemployment spell	0.073		0.088	
on incapacity benefits	0.051		0.063	
on other benefits	0.027		0.027	
% on new benefit spell within 6 months of completion	0.455		0.508	
Of which: on new claimant unemployment spell	0.376		0.406	
on incapacity benefits	0.075		0.096	
on other benefits	0.039		0.044	
Number of spells	4004		4327	
1997 cohort	Mean	St.d.	Mean	St.d.
Age	32.9	11.8	33.5	11.9
Duration of current spell (days)	174.4	270.6	172.3	263.1
% on new benefit spell within 1 month of completion	0.202		0.207	
Of which: on new claimant unemployment spell	0.094		0.098	
on incapacity benefits	0.070		0.067	
on other benefits	0.028		0.028	
% on new benefit spell within 6 months of completion	0.513		0.528	
Of which: on new claimant unemployment spell	0.406		0.417	
on incapacity benefits	0.104		0.100	
on other benefits	0.041		0.046	
Number of spells	4217		4310	

Notes. The sample includes men aged 16-64. Source: LLMDB

Table 2
Impact of JSA on claimant outflow

	Cox proportional hazard model					
	Ages 16-64		Ages 16-24		Ages 25-64	
JSA	0.099***	0.106***	0.063	0.083	0.104***	0.108***
[s.e]	[0.031]	[0.031]	[0.055]	[0.054]	[0.038]	[0.038]
Observations	16423	16423	5320	5320	11103	11103
Other controls	No	Yes	No	Yes	No	Yes
	Whether still claiming at the end of the semester					
	Ages 16-64		Ages 16-24		Ages 25-64	
JSA	-0.149***	-0.154***	-0.180***	-0.190***	-0.127***	-0.130***
[s.e]	[0.014]	[0.014]	[0.023]	[0.023]	[0.017]	[0.017]
Observations	16858	16858	5499	5499	11359	11359
Other controls	No	Yes	No	Yes	No	Yes

Notes. Estimation methods: Cox proportional hazard model in the upper panel and probit model in the lower panel (marginal effects reported). JSA=C^{96}*Treat, and regressions also control for treatment and C^{96} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. Standard errors are clustered at the individual level and reported in brackets.

Table 3
The impact of JSA on exit into other benefit spells within 1 month

	Any benefit spell					
	Ages 16-64		Ages 16-24		Ages 25-64	
JSA	0.034***	0.031**	0.048**	0.045**	0.026*	0.024
(s.e)	(0.013)	(0.013)	(0.021)	(0.021)	(0.016)	(0.016)
	New claimant unemployment spell					
	Ages 16-64		Ages 16-24		Ages 25-64	
JSA	0.018*	0.016	0.029	0.029	0.013	0.01
(s.e)	(0.010)	(0.010)	(0.018)	(0.018)	(0.012)	(0.012)
	Spell of Incapacity Benefits					
	Ages 16-64		Ages 16-24		Ages 25-64	
JSA	0.017**	0.015*	0.027**	0.024*	0.011	0.01
(s.e)	(0.008)	(0.008)	(0.013)	(0.013)	(0.010)	(0.010)
Observations	16858	16858	5499	5499	11359	11359
Other controls	no	yes	no	yes	no	yes

Notes. The sample includes the 1996 and 1997 cohorts. The outcome variable is 1 if a new benefit spell has started within 1 month since the end of the current spell. JSA= C^{96}*treat, and regressions also control for treatment and C^{96} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. Estimation method: probit (marginal effects are reported). Standard errors clustered at the individual level.

Table 4
The impact of JSA on exit into other benefit spells within 6 months

Age sample	16-64	16-24	25-64	16-64	16-24	25-64
Any benefit spell						
JSA	0.038**	0.034**	0.045*	0.037	0.034*	0.029
(s.e)	(0.015)	(0.015)	(0.026)	(0.027)	(0.019)	(0.019)
New claimant unemployment spell						
JSA	0.020	0.017	0.037	0.031	0.013	0.007
(s.e)	(0.015)	(0.015)	(0.027)	(0.027)	(0.018)	(0.018)
Spell of Incapacity Benefits						
JSA	0.027***	0.024**	0.030**	0.026*	0.025**	0.022*
(s.e)	(0.010)	(0.010)	(0.015)	(0.015)	(0.013)	(0.012)
Observations	16858	16858	5499	5499	11359	11359
Other controls	no	yes	no	yes	no	yes

Notes. The sample includes the 1996 and 1997 cohorts. The outcome variable is 1 if a new benefit spell has started within 6 months since the end of the current spell. JSA= C^{96}*treat, and regressions also control for treatment and C^{96} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. Estimation method: probit (marginal effects are reported). Standard errors clustered at the individual level

Table 5
The impact of JSA on exit into other benefit spells within 1 month
Short-term unemployed only

	Any benefit spell			
	Within 1 month		**Within 6 months**	
	Ages 16-24	Ages 25-64	Ages 16-64	Ages 25-64
JSA	0.078***	0.056***	0.041	0.038
(s.e)	(0.029)	(0.020)	(0.035)	(0.024)
	New claimant unemployment spell			
	Within 1 month		**Within 6 months**	
	Ages 16-64	Ages 25-64	Ages 16-64	Ages 25-64
JSA	0.030	0.019	0.004	0.021
(s.e)	(0.023)	(0.015)	(0.035)	(0.024)
	Spell of Incapacity benefits			
	Within 1 month		**Within 6 months**	
	Ages 16-64	Ages 25-64	Ages 16-64	Ages 25-64
JSA	0.048**	0.031**	0.065***	0.030**
(s.e)	(0.019)	(0.013)	(0.024)	(0.015)
Observations	3453	6762	3453	6762
Other controls	yes	yes	yes	yes

Notes. The sample includes the short-term unemployed from 1996 and 1997 cohorts. Short-term unemployed are defined as those who exit unemployment by the beginning of October (respectively April) if in the treatment (respectively control) group. The outcome variable is 1 if a new benefit spell has started within 1 or 6 month since the end of the current spell. JSA= C^{96}*treat, and regressions also control for treatment and C^{96} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. Estimation method: probit (marginal effects are reported). Standard errors clustered at the individual level.

Table 6
The impact of JSA on exit into other benefit spells
Falsification test on 1997 and 1998 cohorts

	Any benefit spell			
	Within 1 month		Within 6 months	
	Ages 16-24	Ages 25-64	Ages 16-64	Ages 25-64
JSA	0.030	-0.009	-0.037	-0.008
(s.e)	(0.021)	(0.015)	(0.027)	(0.019)
	New claimant unemployment spell			
	Within 1 month		Within 6 months	
	Ages 16-64	Ages 25-64	Ages 16-64	Ages 25-64
JSA	0.013	-0.008	-0.040	-0.009
(s.e)	(0.017)	(0.011)	(0.027)	(0.018)
	Spell of Incapacity benefits			
	Within 1 month		Within 6 months	
	Ages 16-64	Ages 25-64	Ages 16-64	Ages 25-64
JSA	0.012	0.001	0.004	0.001
(s.e)	(0.012)	(0.010)	(0.014)	(0.012)
Observations	5366	11303	5366	11303
Other controls	yes	yes	yes	yes

Notes. The sample includes the 1997 and 1998 cohorts. The outcome variable is 1 if a new benefit spell has started within 1 or 6 month since the end of the current spell. JSA= C^{97}*treat, and regressions also control for treatment and C^{97} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. Estimation method: probit (marginal effects are reported). Standard errors clustered at the individual level.

Figure 1:
Flows out of and into the claimant unemployment register
Data seasonally adjusted. Source: NOMIS
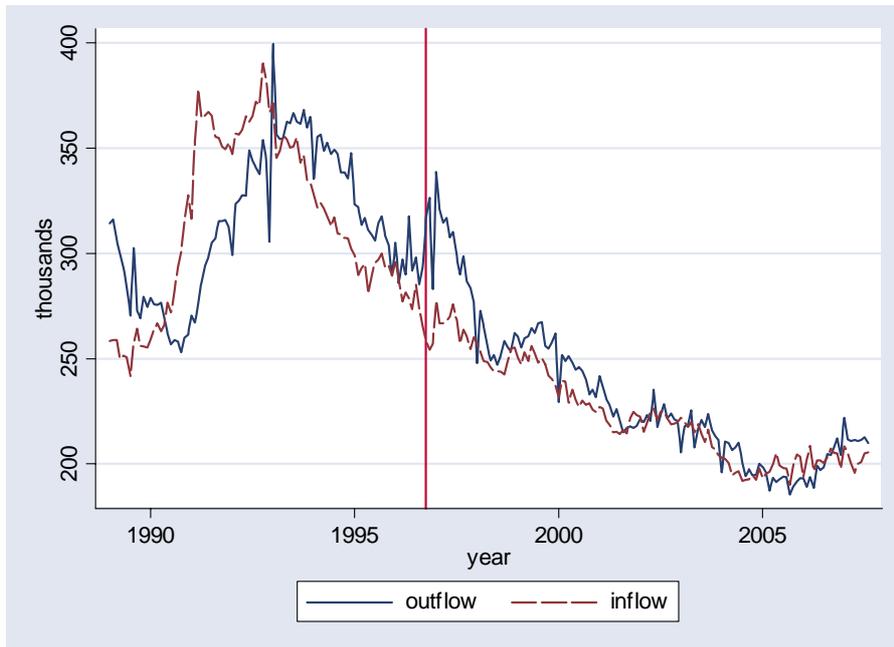
Figure 2:
The claimant unemployment count
Data seasonally adjusted. Source: NOMIS
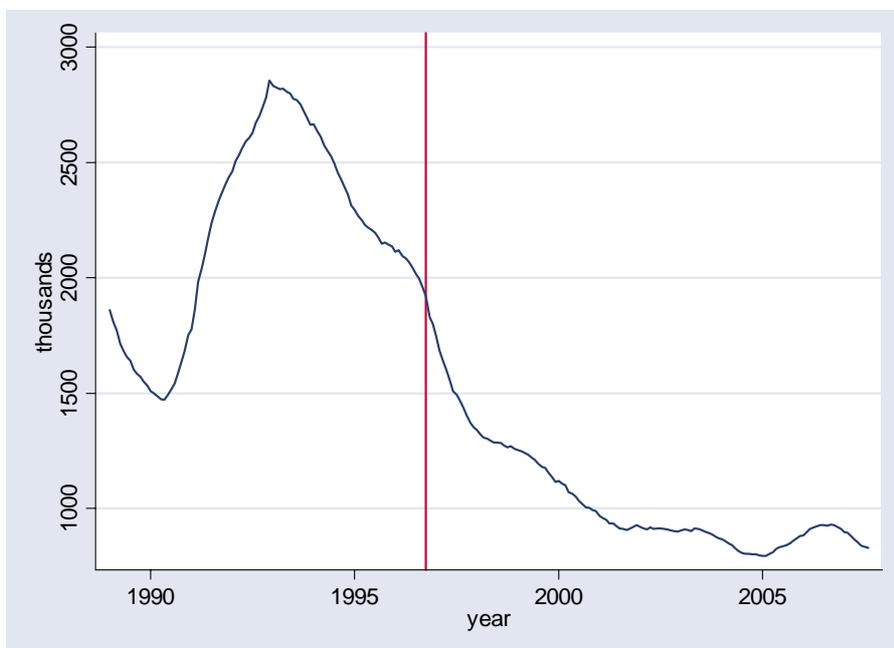
Figure 3
Indifference curves between benefit levels and search effort
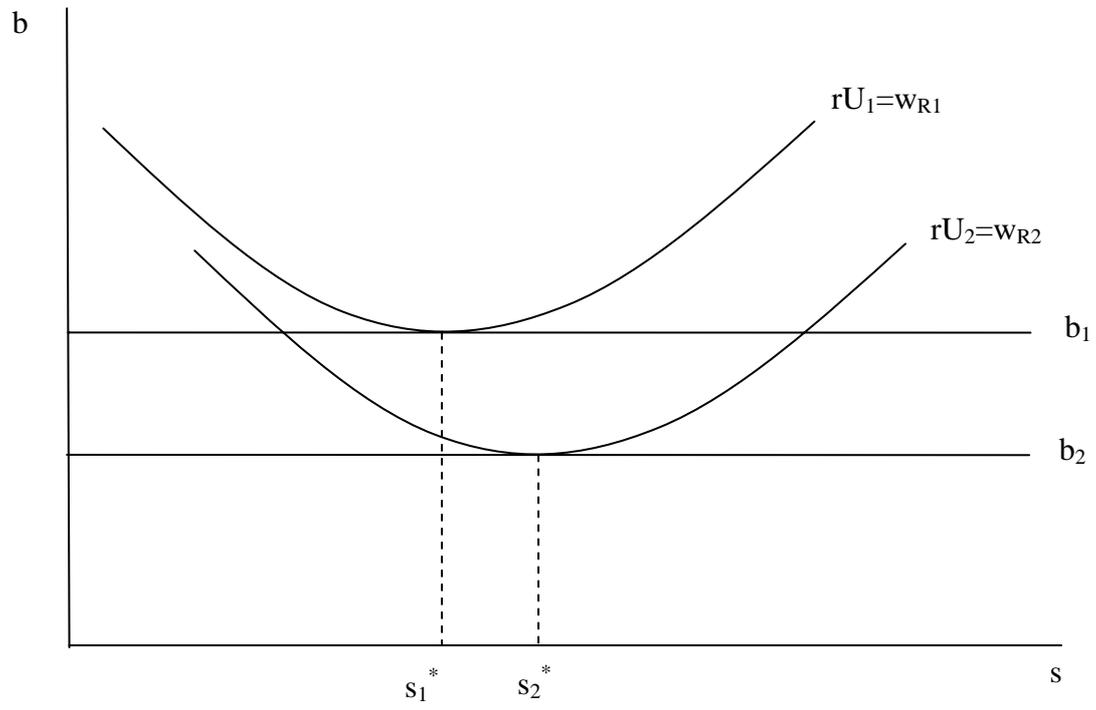
Figure 4
Intended consequences of tighter eligibility requirements

b

$rU_1=w_{R1}$

$rU_2=w_{R2}$

$\underline{s}_1$ s_1^* $s_2^* = \underline{s}_2$ s
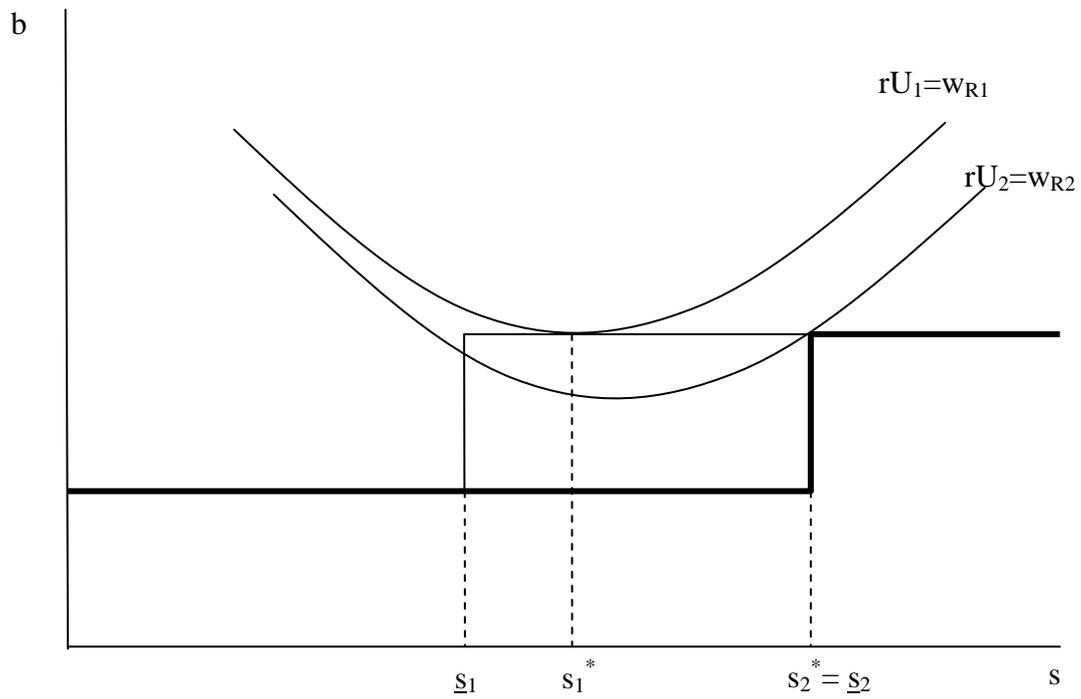
Figure 5
Unintended consequences of tighter eligibility requirements

b

$rU_1=w_{R1}$

$rU_2=w_{R2}$

s_2^* $s_1^* = \underline{s}_1$ $\underline{s}_2$ s
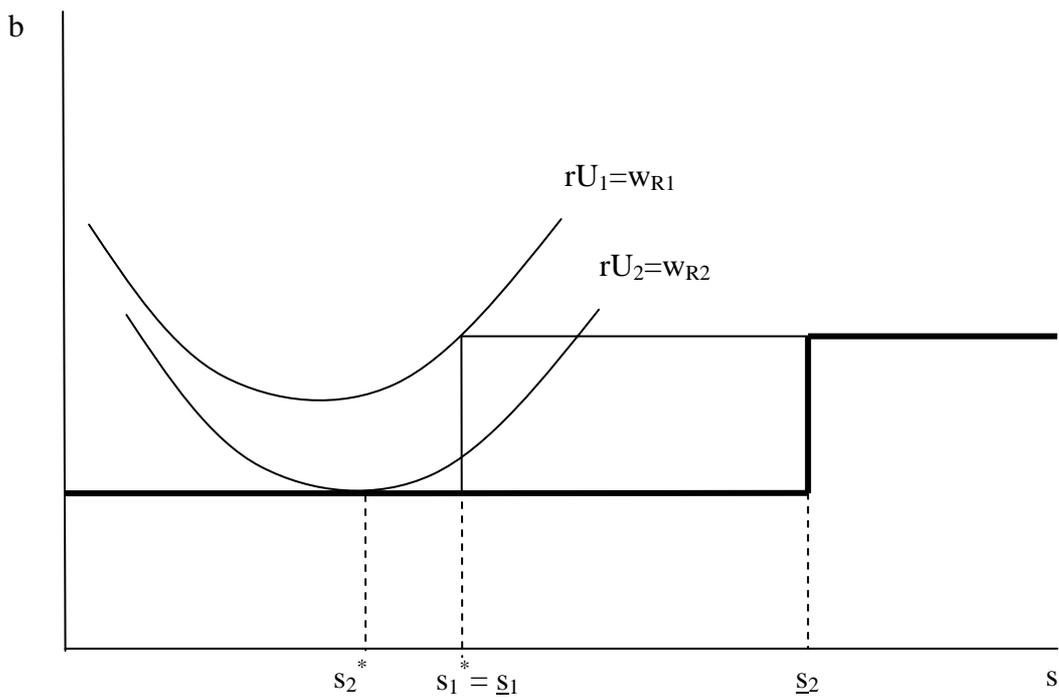
Figure 6
Raw employment and earnings time series from the LLMDB
Sample: males aged 16-64.
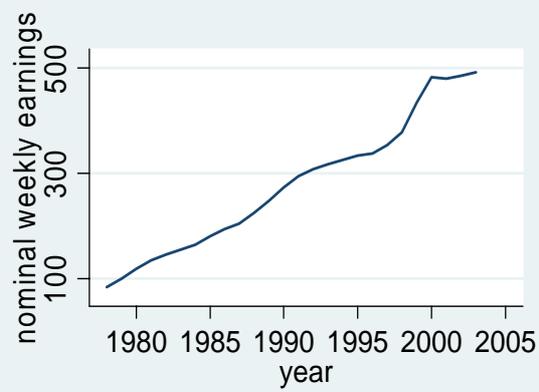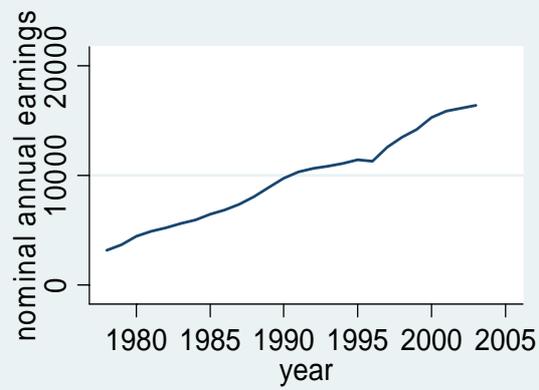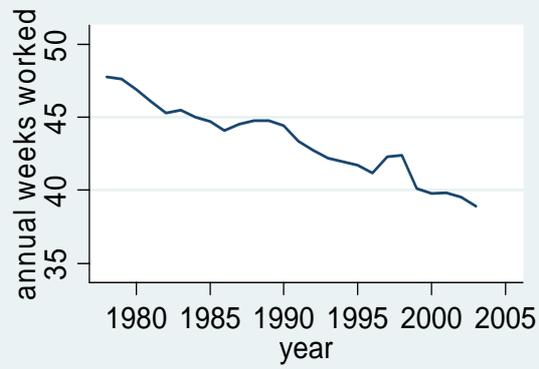
Figure 7
Claimant unemployment spells started 1 Jan n1996-31 Dec 1998
Source: LLMDB
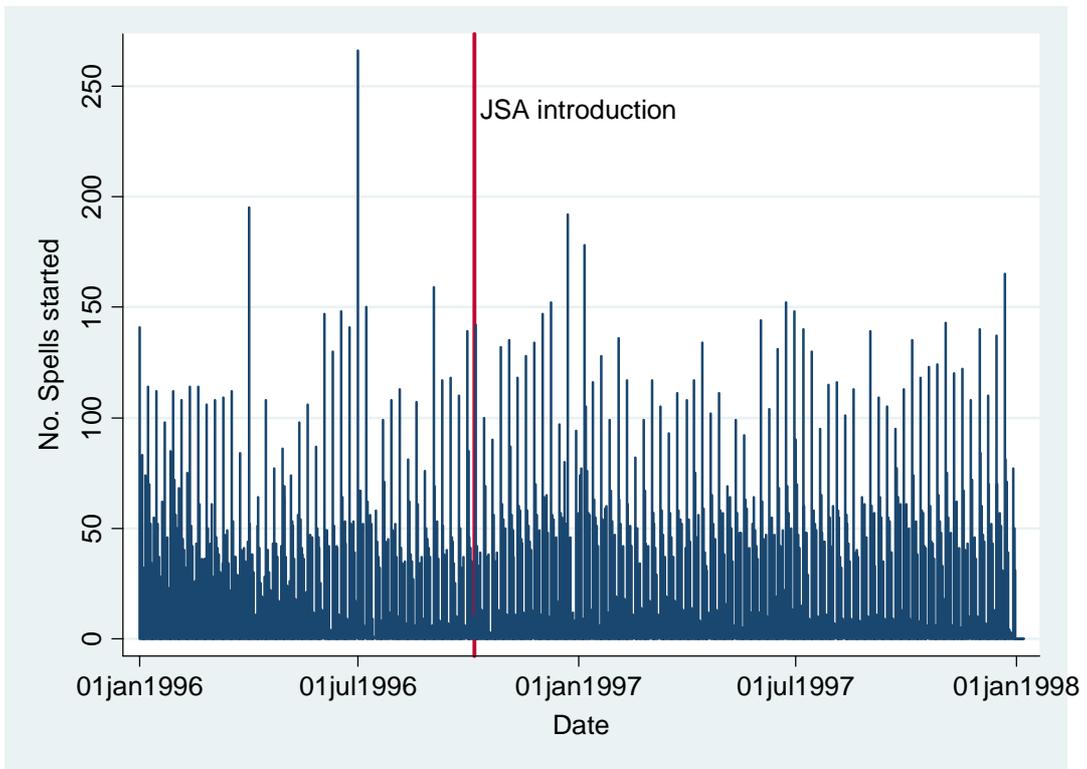
Figure 8
Claimant unemployment spells started 1 Sept -31 Oct 1996
Source: LLMDB
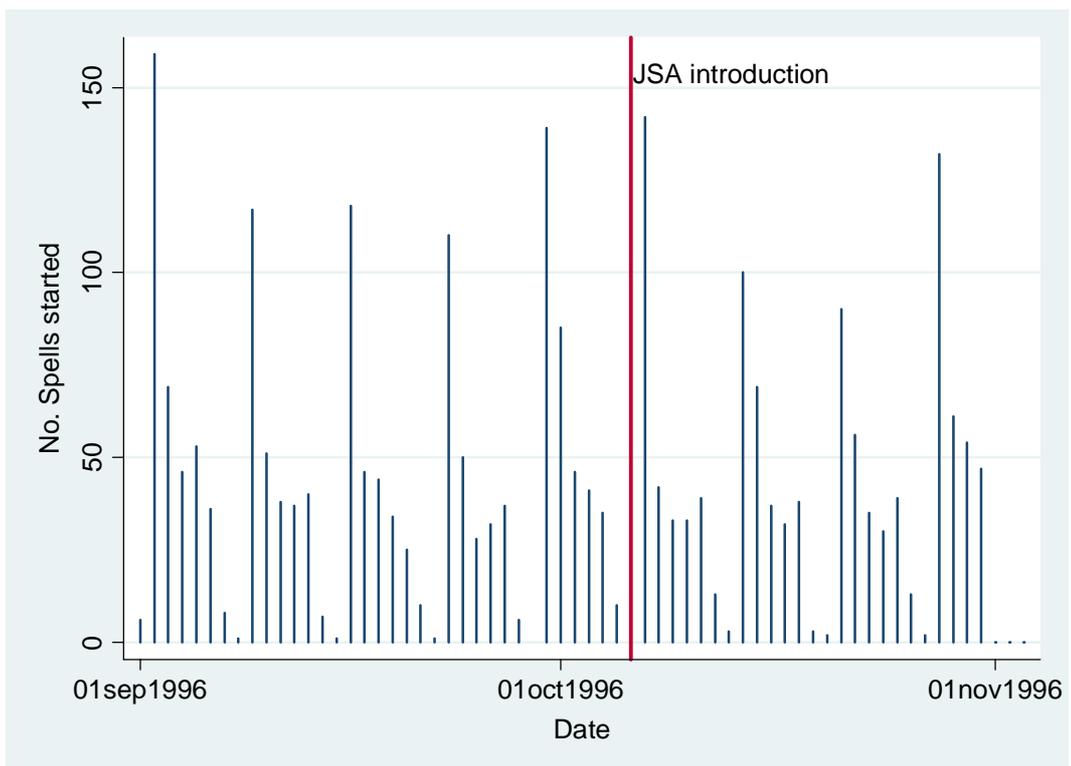
Figure 9
Claimant unemployment spells ended 1 Jan 1996-31 Dec 1998
Source: LLMDB
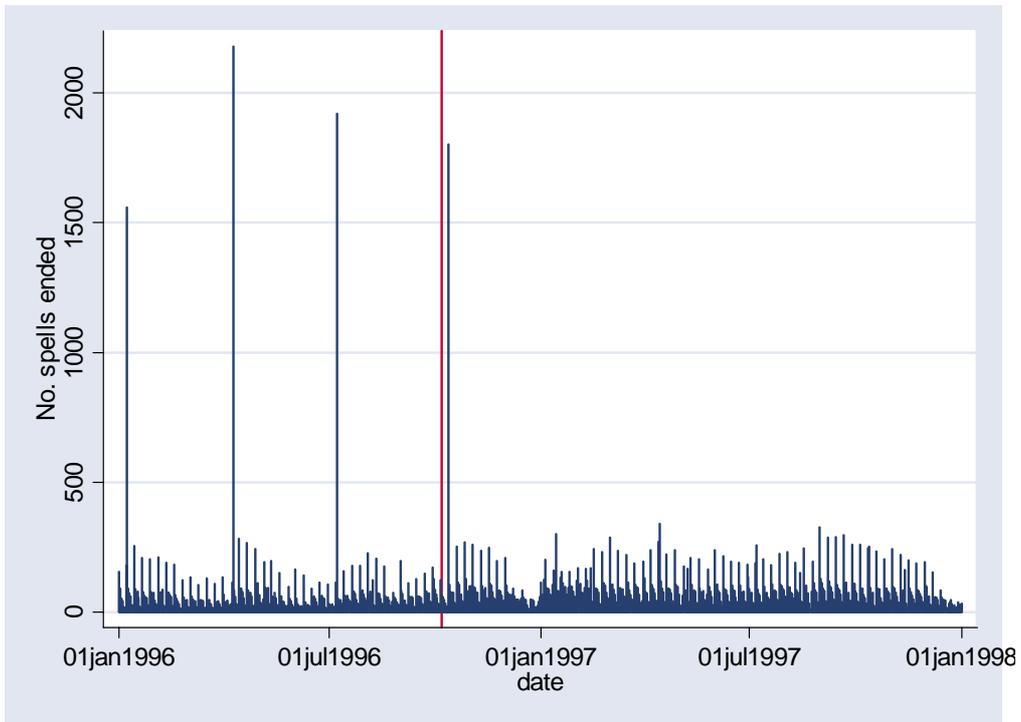
Figure 10
The proportion of individuals with positive earnings in control and treatment groups
Sample: Males 16-64. Source: LLMDB
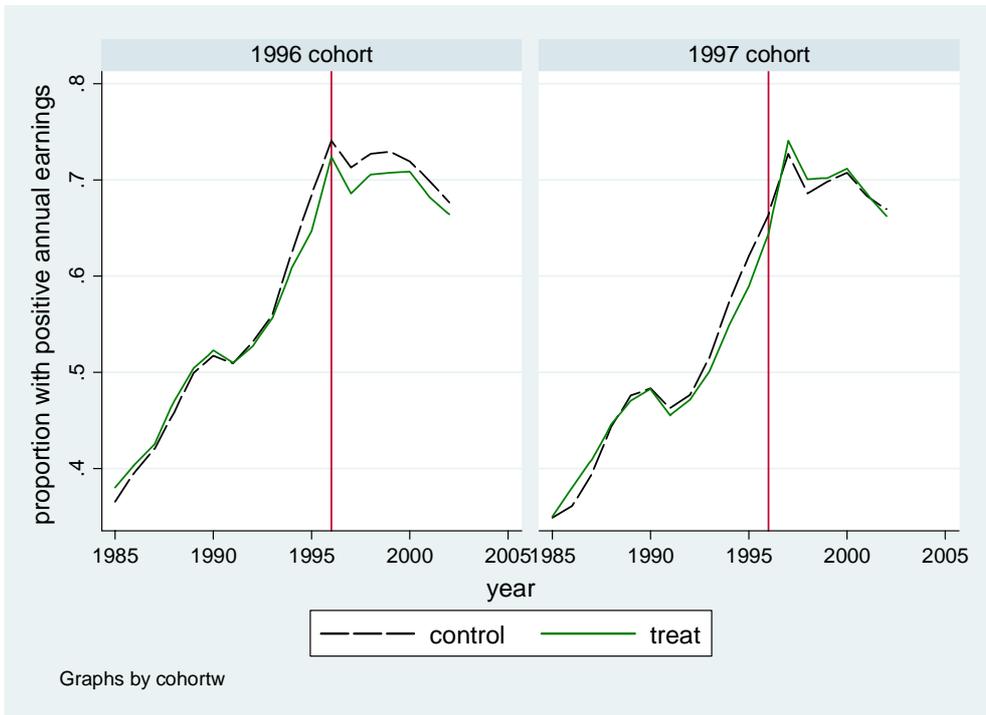
Figure 11
Average number of weeks worked among the employed in control and treatment groups
Sample: Males 16-64. Source: LLMDB
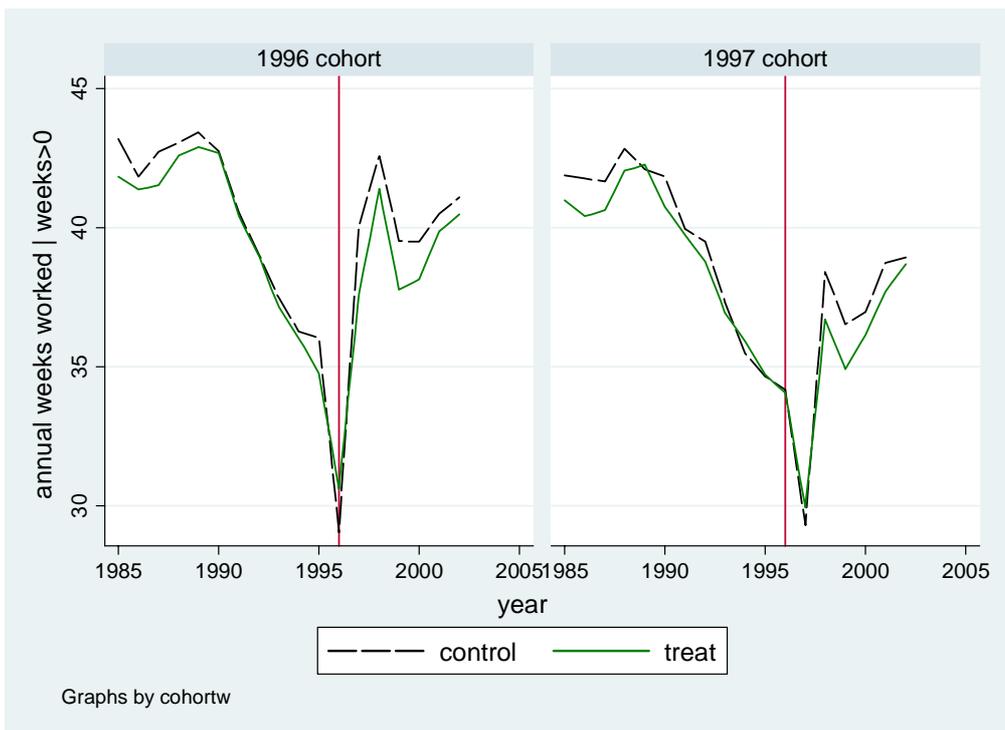
Figure 12
Log annual earnings in control and treatment groups
Sample: Males 16-64. Source: LLMDB
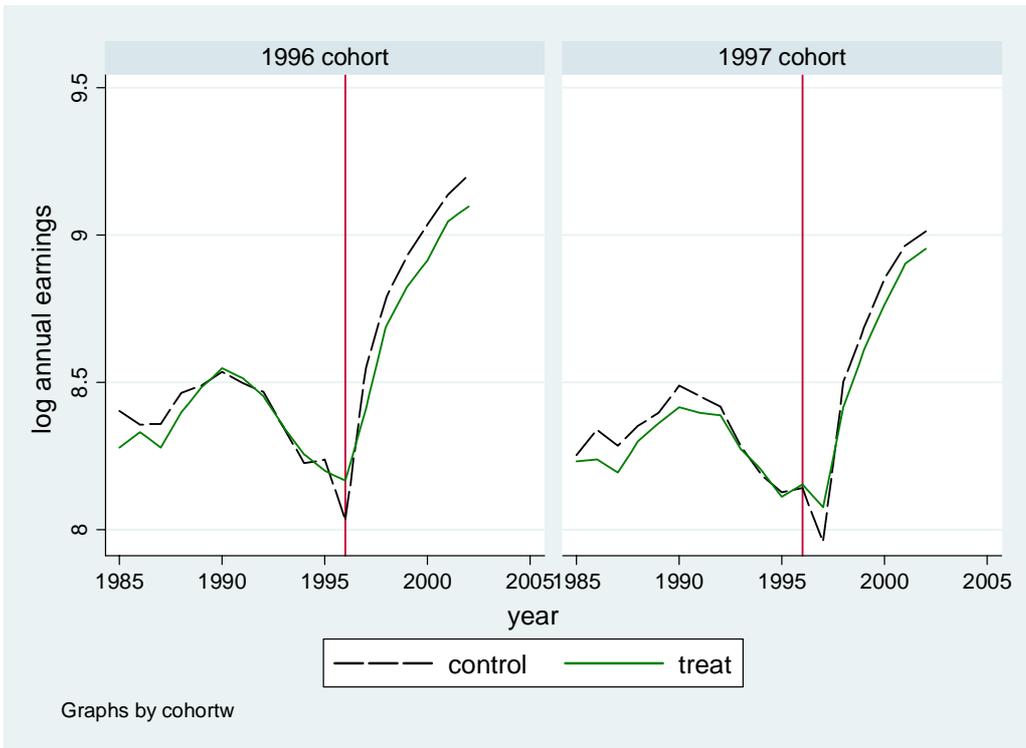
Figure 13
Log weekly earnings in control and treatment groups
Sample: Males 16-64. Source: LLMDB
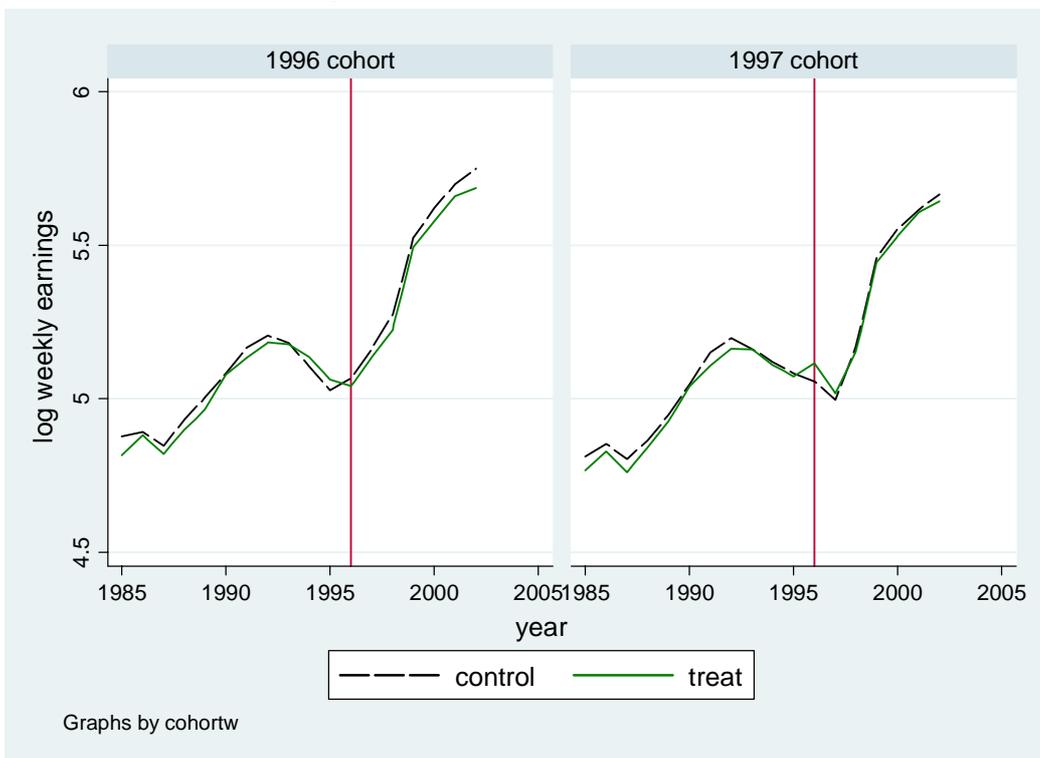
Figure 14
DID estimates of the probability to have positive earnings
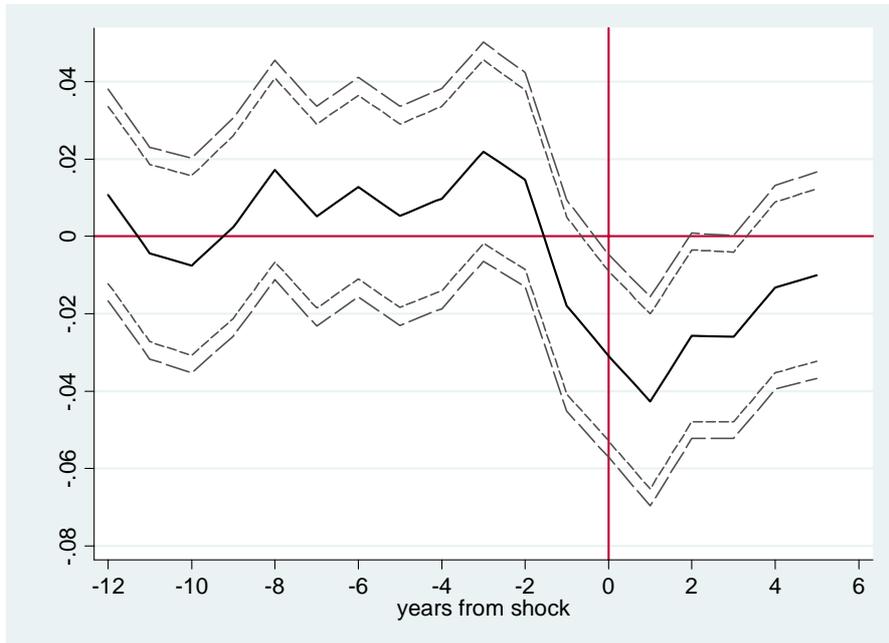
Figure 15
DID estimates of the number of weeks worked among the employed
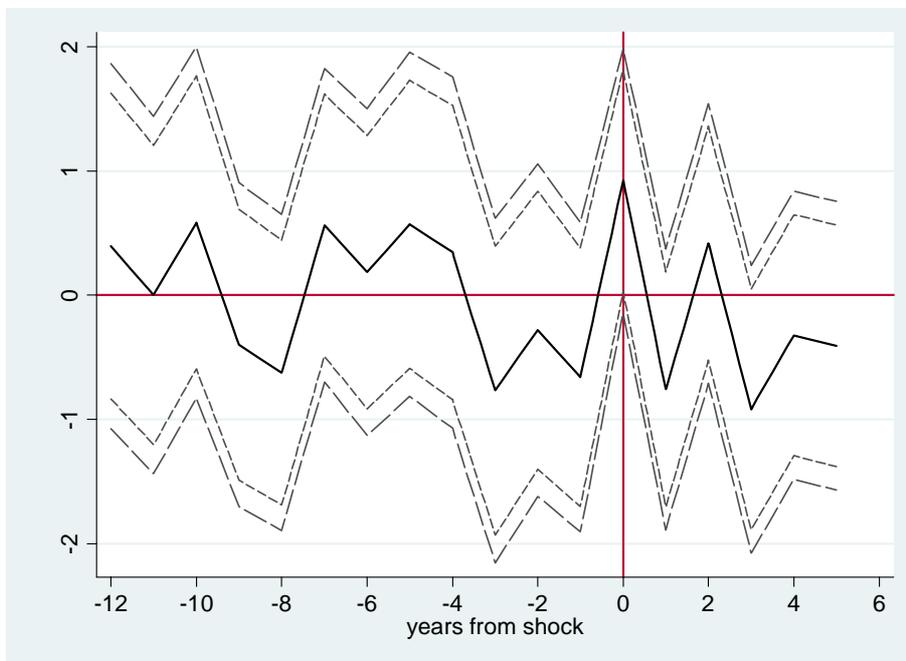
Figure 16
DID estimates of log annual earnings
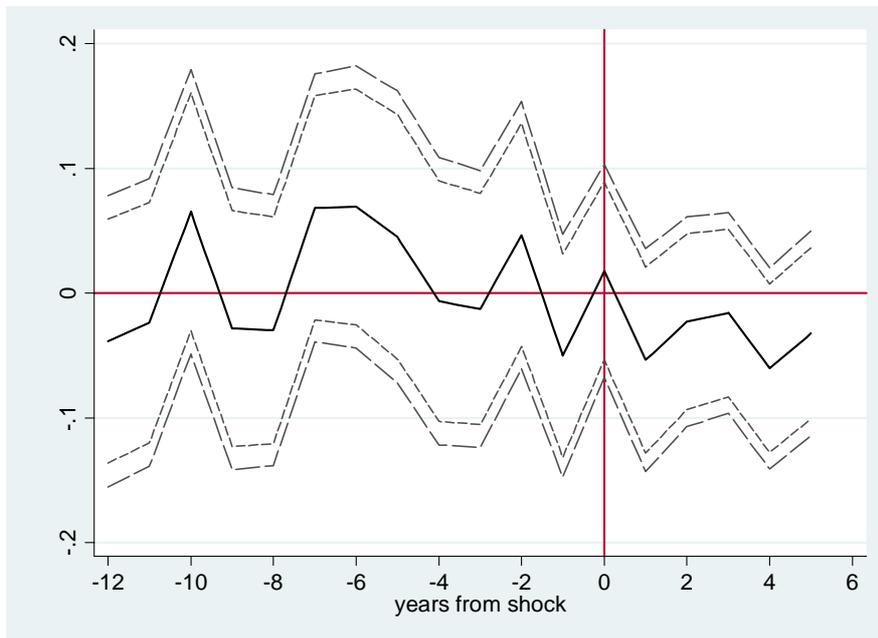
Figure 17
DID estimates of log weekly earnings
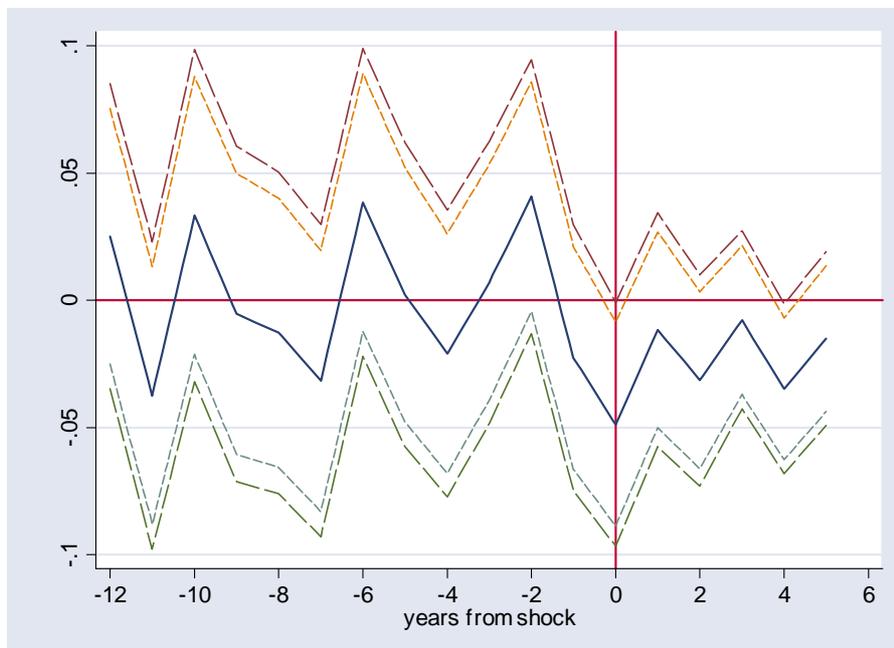
Notes to Figures 14-17. The solid line represents marginal effects (Figure 14) or coefficients (Figures 15-17) on the C^{96}*Treat interaction (see equation (7)), and regressions also control for treatment and C^{96} separately. The dashed lines represent 90% and 95% confidence intervals. Time zero denotes 1996 for the 1996 cohort; and 1997 for the 1997 cohort.
Sample: Males 16-64. Source: LLMDB.

Figure 18
The impact of JSA on the probability of positive earnings
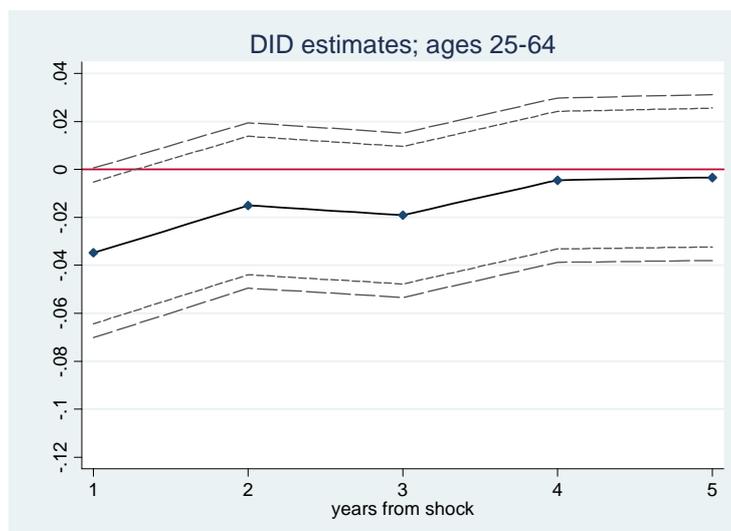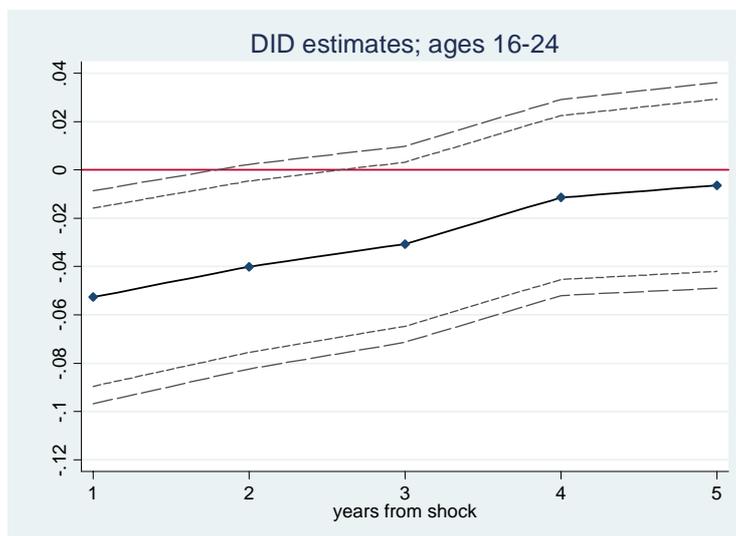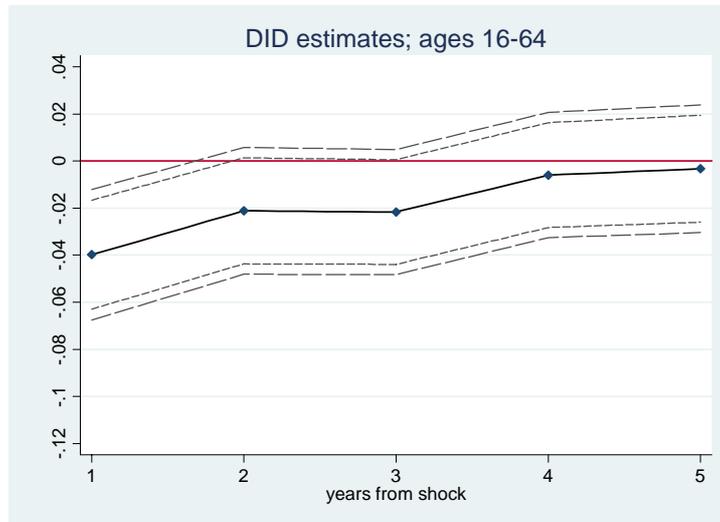
Figure 19
The impact of JSA on the number of weeks worked among the employed
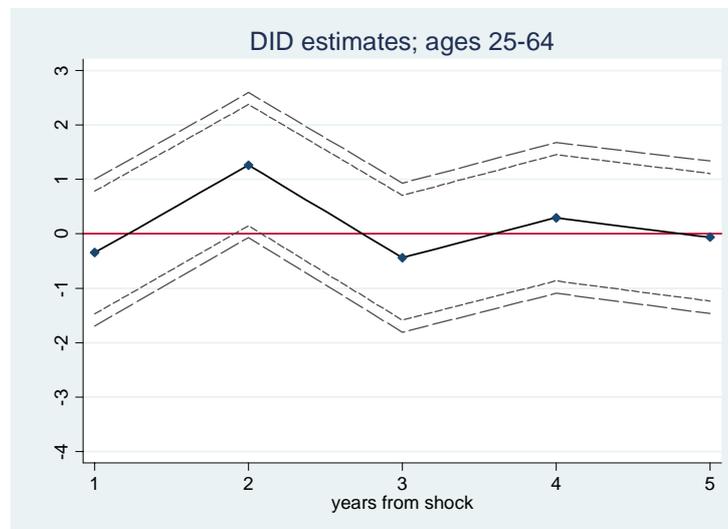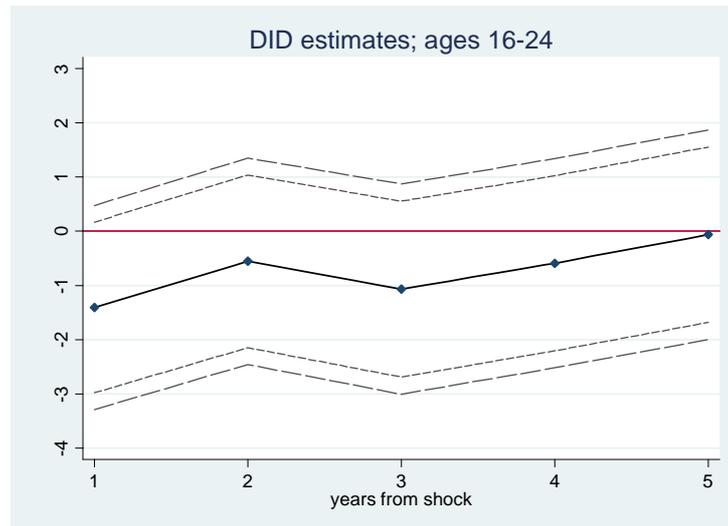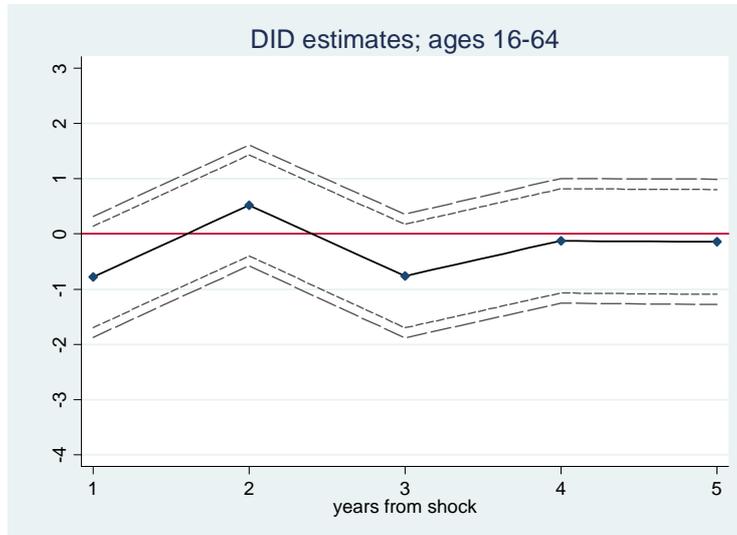
Figure 20
The impact of JSA on log annual earnings

DID estimates; ages 16-64

years from shock

DID estimates; ages 16-24

years from shock

DID estimates; ages 25-64

years from shock
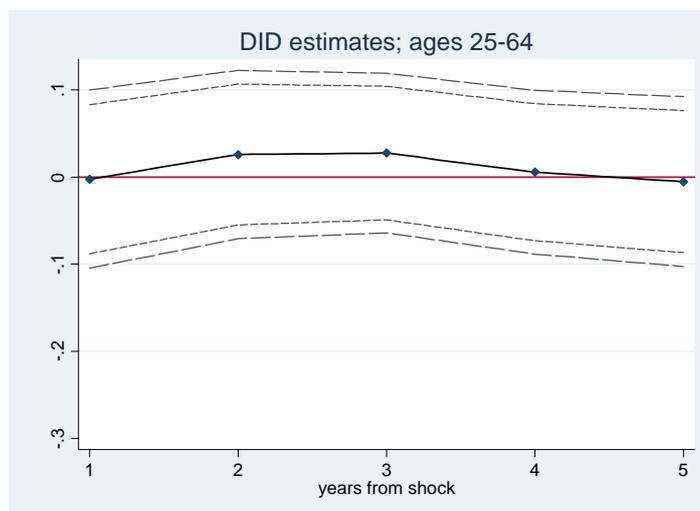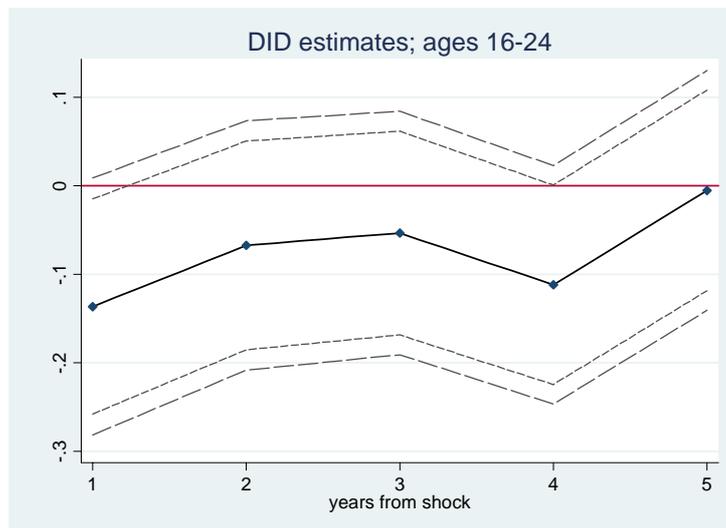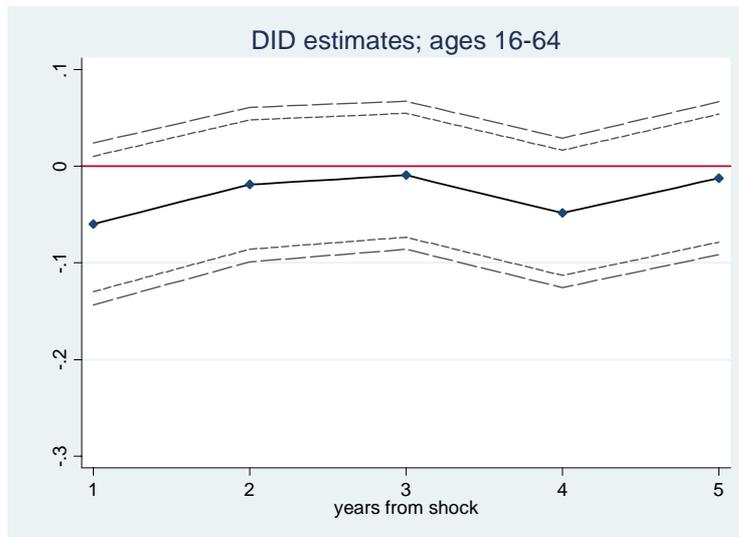
Figure 21
The impact of JSA on log weekly earnings

DID estimates; ages 16-64

DID estimates; ages 16-24

DID estimates; ages 25-64
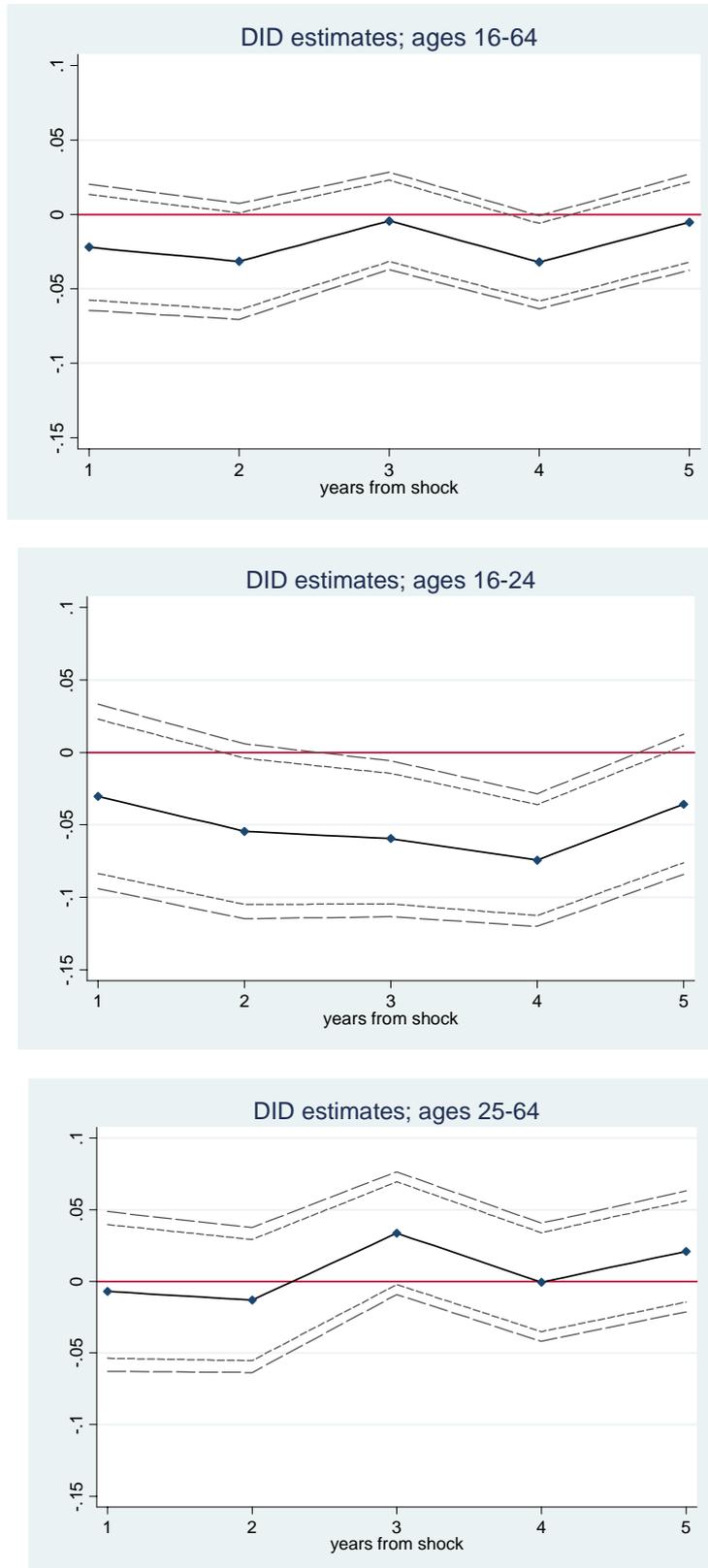
Notes to Figures 18-21. The solid line represents marginal effects (Figure 18) or coefficients (Figures 19-21) on to the C^{96}*Treat interaction term (see equation (7)), and regressions also control for treatment and C^{96} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. The dashed lines represent 90% and 95% confidence intervals, based on standard errors clustered at the individual level. Year 1 denotes 1997 for the 1996 cohort; and 1998 for the 1997 cohort. Source: LLMDB.: Males 16-64. Source: LLMDB

Figure 22
The impact of JSA on the probability to have positive earnings
Sample: short-term unemployed
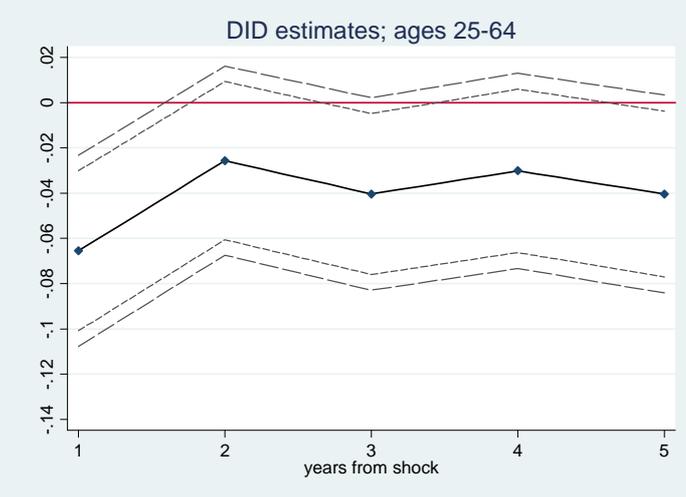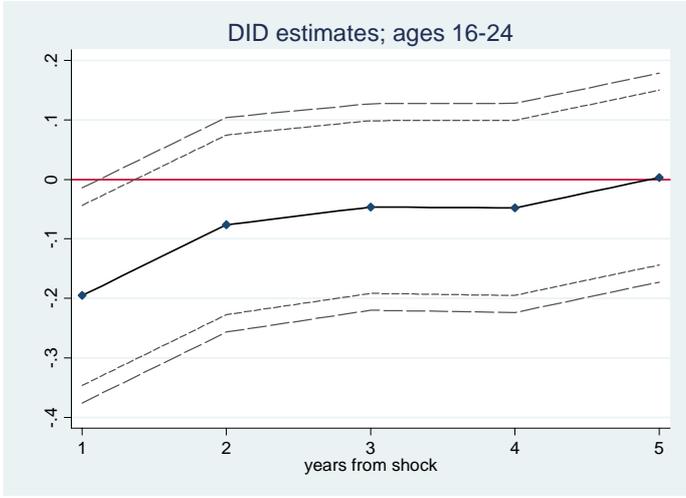
Figure 23
The impact of JSA on the number of weeks worked among the employed
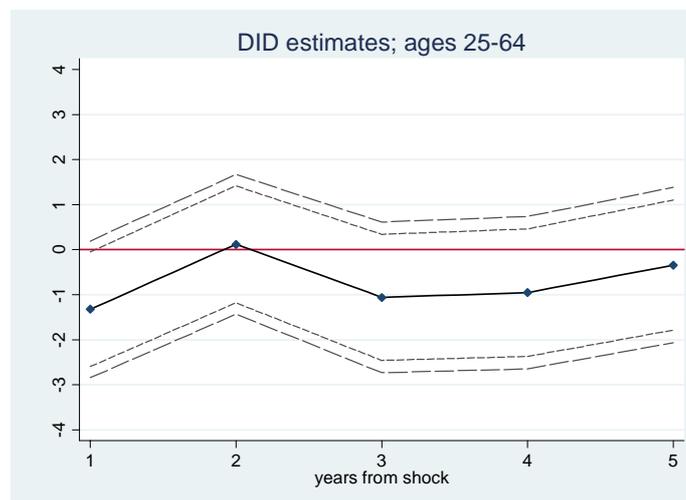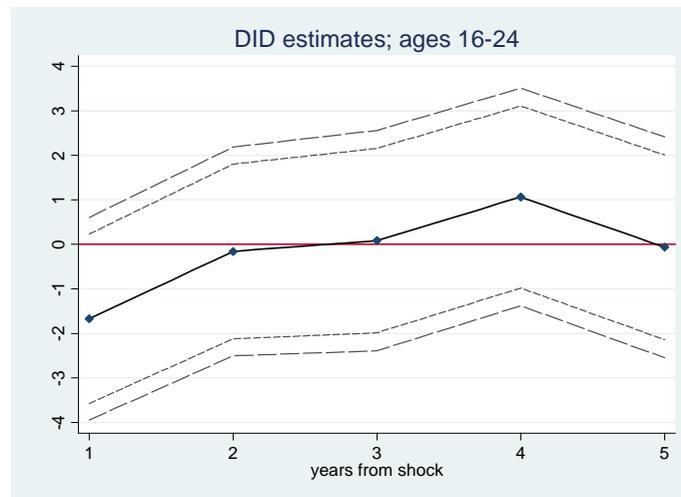**Sample: short-term unemployed**

Figure 24
The impact of JSA on log annual earnings
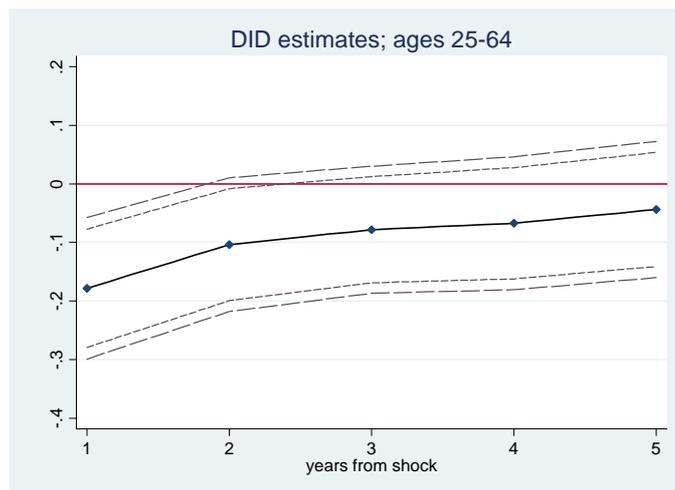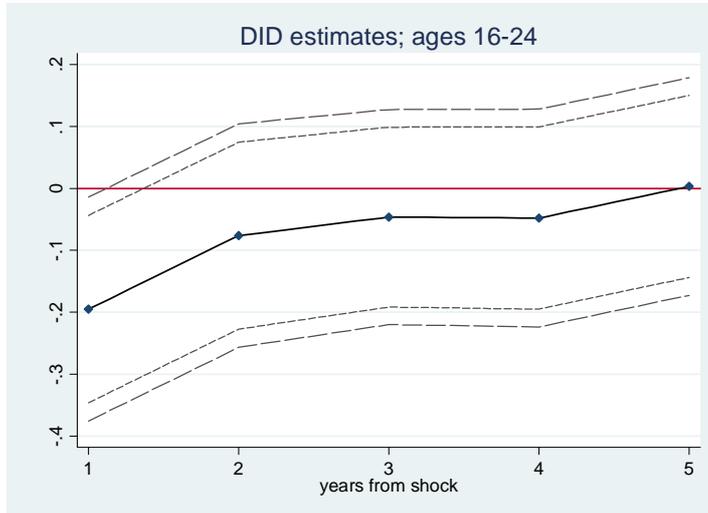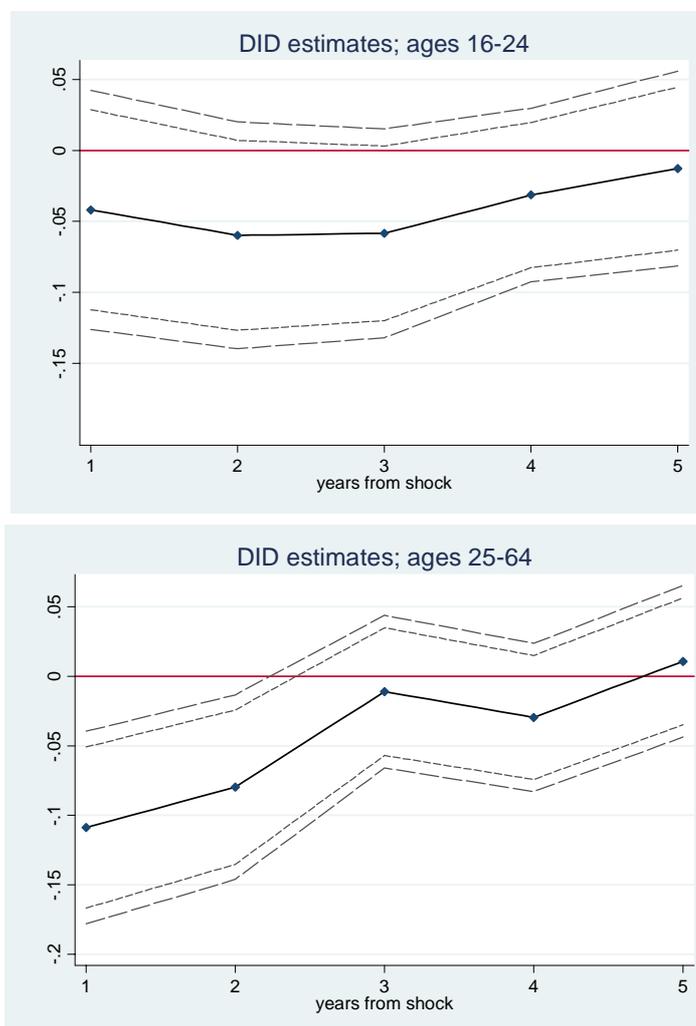**Sample: short-term unemployed**

DID estimates; ages 16-24

DID estimates; ages 25-64

Figure 25
The impact of JSA on log weekly earnings
Sample: short-term unemployed

DID estimates; ages 16-24

years from shock

DID estimates; ages 25-64

years from shock

Notes to Figures 22-25. The sample includes the short-term unemployed from 1996 and 1997 cohorts. Short-term unemployed are defined as those who exit unemployment by the beginning of October (respectively April) if in the treatment (respectively control) group. The solid line represents marginal effects (Figure 22) or coefficients (Figures 23-25) on to the $C^{96}*Treat$ interaction term (see equation (7)), and regressions also control for treatment and C^{96} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. The dashed lines represent 90% and 95% confidence intervals, based on standard errors clustered at the individual level. Year 1 denotes 1997 for the 1996 cohort; and 1998 for the 1997 cohort. Source: LLMDB.

Figure 26
The impact of JSA on the probability to have positive earnings
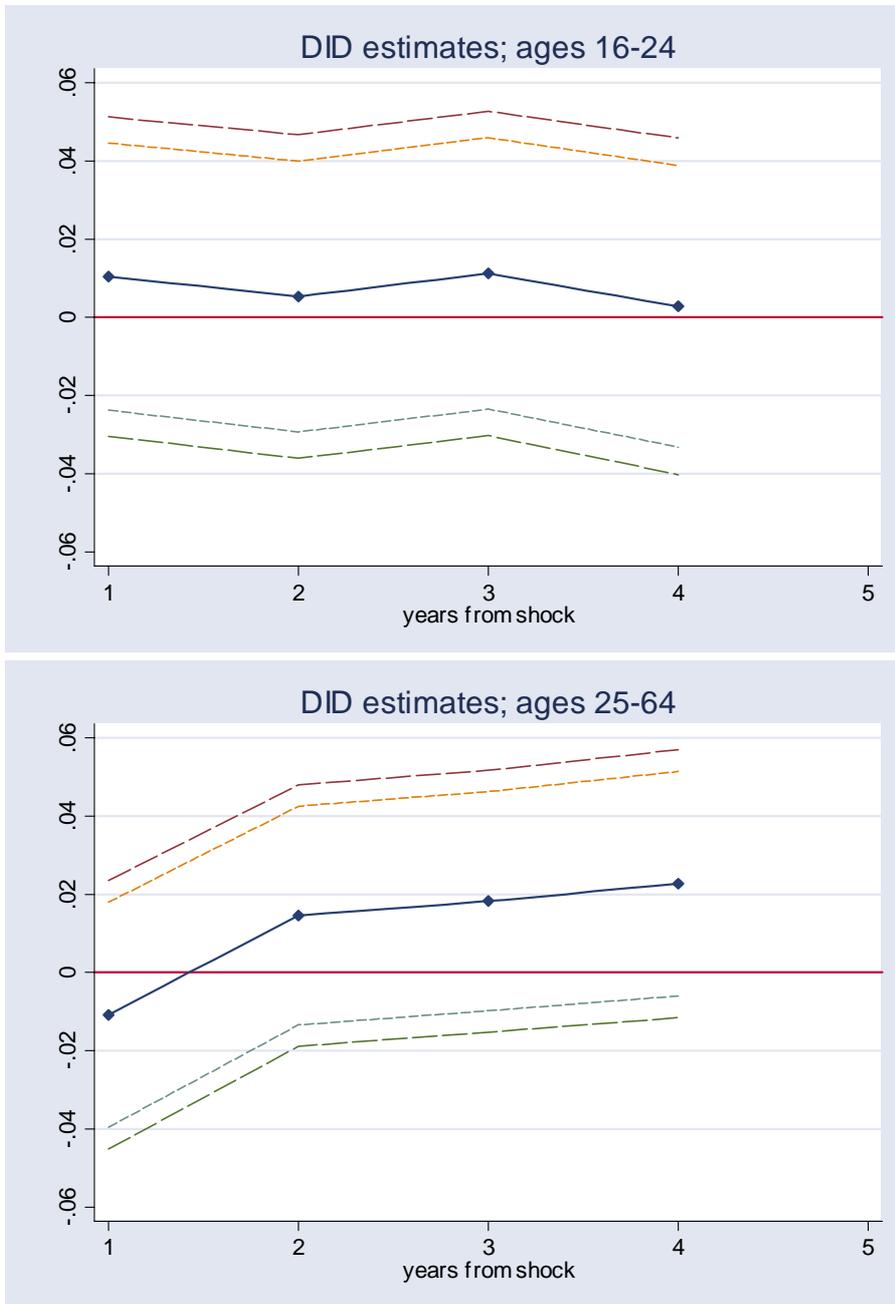**Falsification test on 1997 and 1998 cohorts**

Figure 27
The impact of JSA on weeks worked for the employed
Falsification test on 1997 and 1998 cohorts

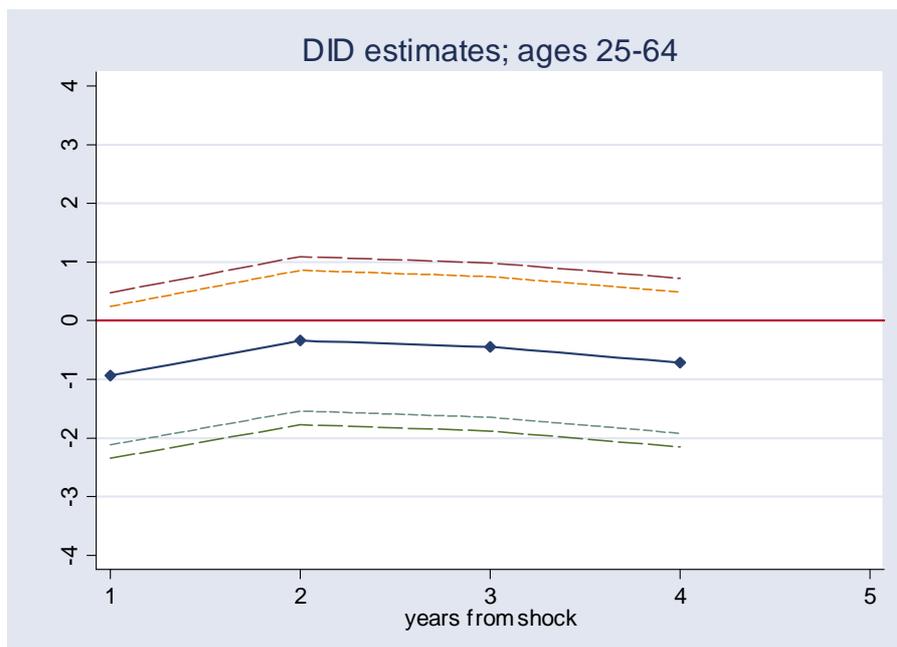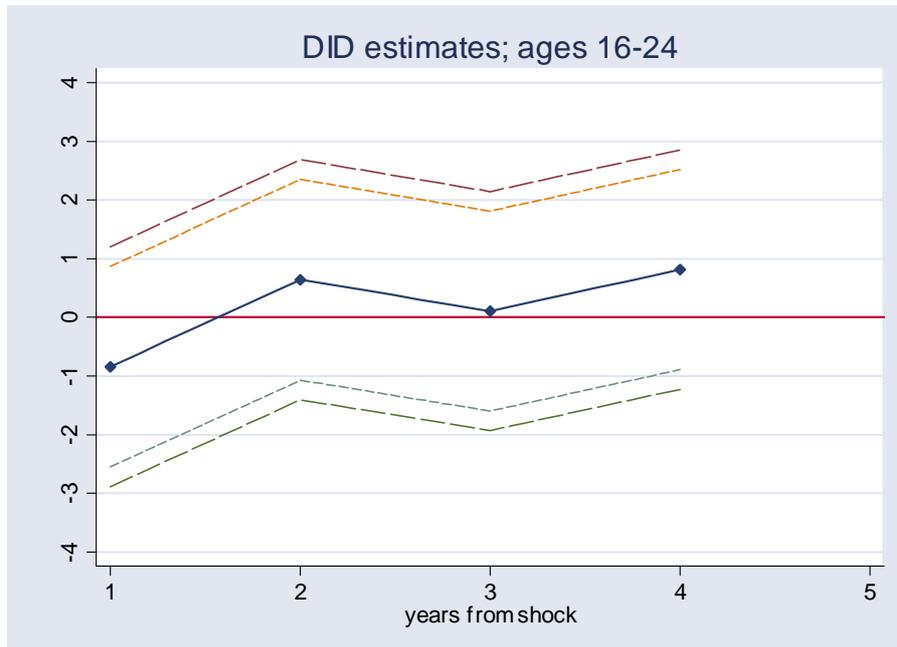DID estimates; ages 16-24

DID estimates; ages 25-64

Figure 28
The impact of JSA on log annual earnings
Falsification test on 1997 and 1998 cohorts

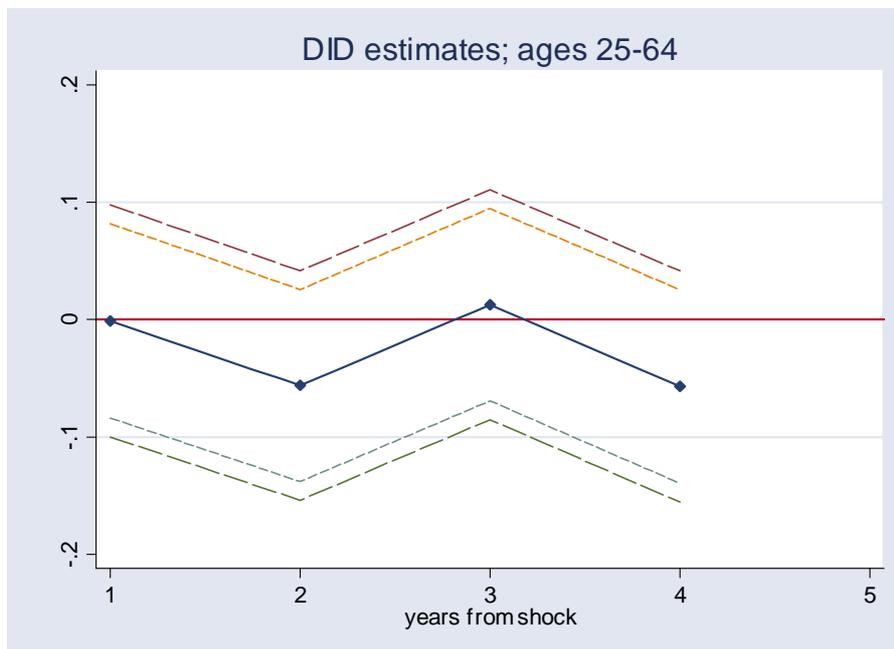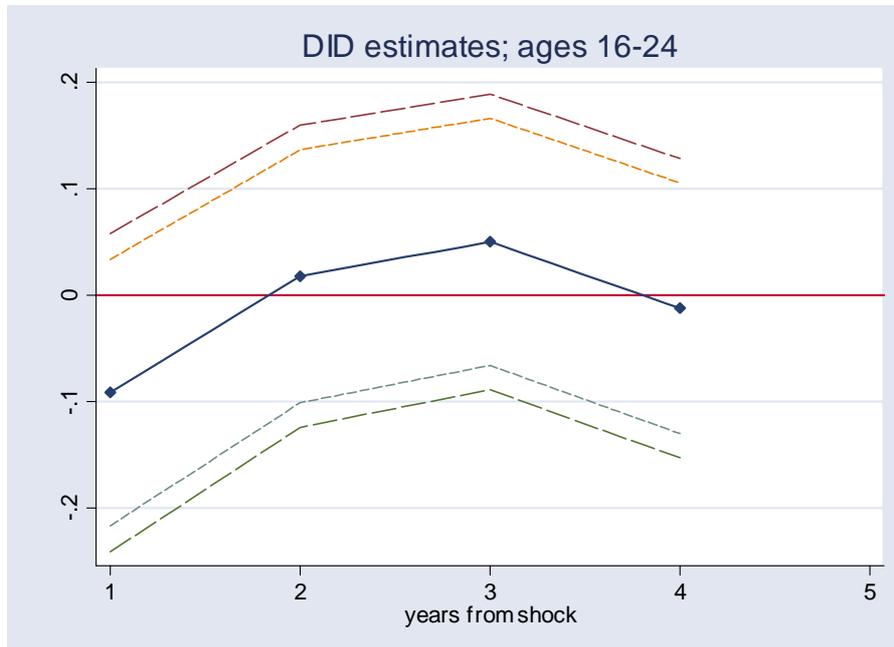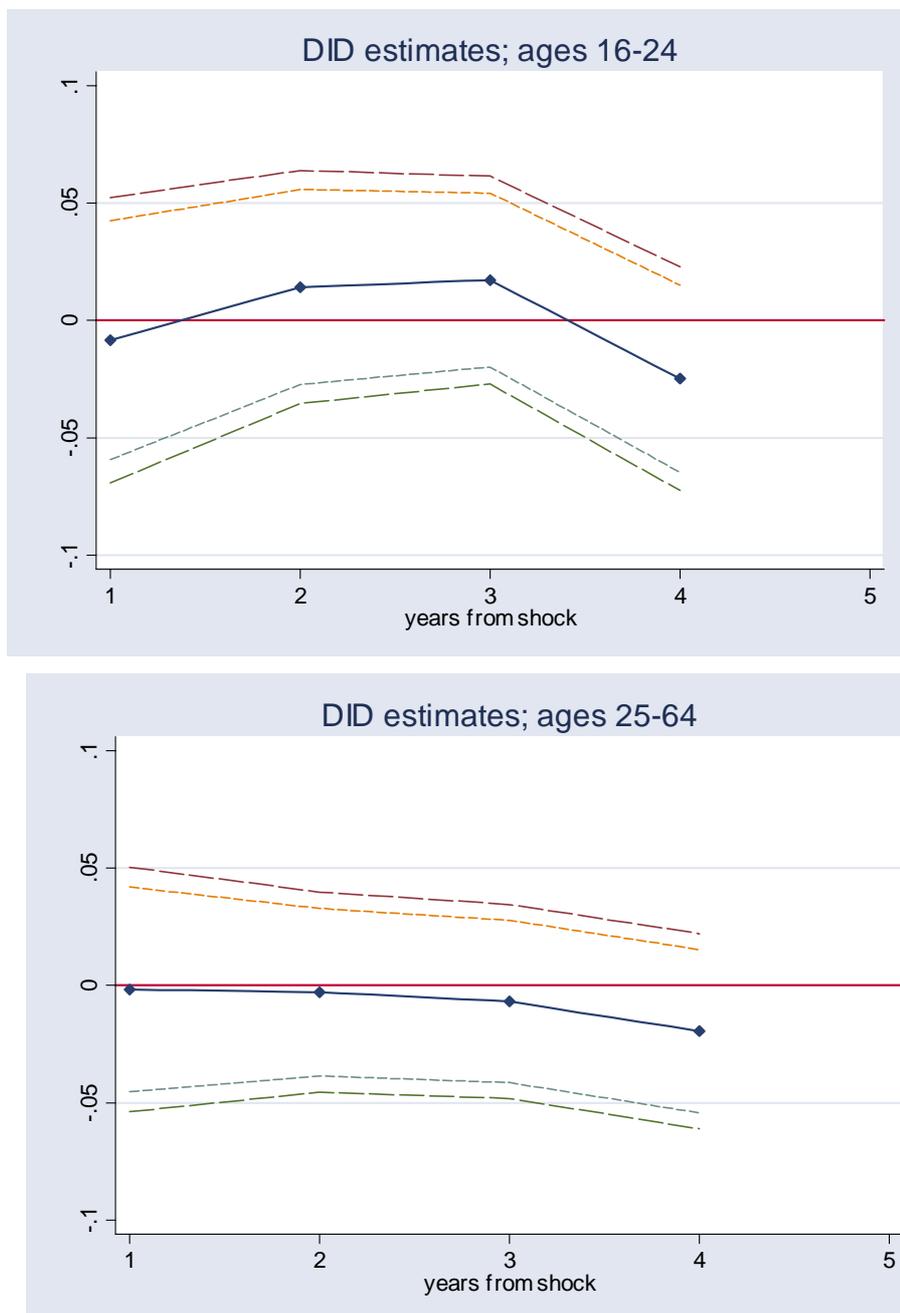DID estimates; ages 16-24

DID estimates; ages 25-64

Figure 29
The impact of JSA on log weekly earnings
Falsification test on 1997 and 1998 cohorts

Notes to Figures 26-29. The sample includes the 1997 and 1998 cohorts. The solid line represents marginal effects (Figure 26) or coefficients (Figures 27-29) on to the C^{97}*Treat interaction term (see equation (7)), and regressions also control for treatment and C^{97} separately. Other controls included are: age, age squared, whether had an unemployment spell in the previous 2 years, total weeks worked in each of the past 5 years and their square, total earnings in each of the past 5 years and their square. The dashed lines represent 90% and 95% confidence intervals, based on standard errors clustered at the individual level. Year 1 denotes 1998 for the 1997 cohort; and 1999 for the 1998 cohort. Source: LLMDB.

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