# Can helping the sick hurt the able?
# Incentives, information and disruption in a welfare reform

**Nitika Bagaria (CRA), Barbara Petrongolo (QMUL and CEP)**

**and John Van Reenen (LSE and CEP)**

January 28th, 2016

**Abstract**

Disability rolls have escalated in developed nations over the last 40 years. The UK stands out because the numbers on these benefits stopped rising when a welfare reform ("Jobcentre Plus") was introduced that integrated employment services and benefits for all categories of welfare recipients. The policy sharpened bureaucratic incentives to help disability benefit recipients into jobs relative to unemployment insurance recipients. We exploit the staggered roll-out of the policy across geographical areas to identify treatment effects. In the long-run, the policy raised exits from disability benefits by 6% and lead to an (insignificant) 1% increase in unemployment outflows. This is consistent with a model in which reorganisation helps both groups, but bureaucrats shifted job-brokering efforts towards those on disability benefits and away from the unemployed. Interestingly, the policy had a negative short-run impact on exits for both groups, suggesting important disruption effects. We estimate that it takes about six years for the estimated benefits of the reform to exceed its costs, which may be why welfare reforms are hard to introduce with myopic policy-makers.

**Keywords:** Incentives, public sector, unemployment benefits, performance standards

**JEL Codes:** H51, I13, J18

**Acknowledgements:** We would like to thank the ESRC for funding this research through the Centre for Economic Performance. Richard Disney, Victor Lavy, Alan Manning, Steve Pischke, Bill Wells and participants at seminars in IFS, LSE, NBER and NIESR have given helpful comments. Barra Roantree generously provided some of the estimates of benefit receipt.

## 1. Introduction

Disability rolls have risen almost inexorably in advanced countries over the last forty years (see Autor and Duggan, 2003). In the US, Social Security Disability Insurance cash transfers have tripled from $40bn in 1979 to $124bn in 2010, and their share of total social security payments rose from 10% in 1988 to 20% in 2009 (Autor, 2012). Figure 1 shows that the numbers on the equivalent UK scheme, Incapacity Benefit, also rose fivefold for those of working age, from 400,000 in 1977 to 2 million in 2009.[1] Unlike in the US, however, the growth in the disability rolls in the UK came to a halt in the early 2000s, followed by a slight fall thereafter. This coincided with the introduction of a major welfare reform in 2001 ("Jobcentre Plus"), which integrated job-brokering services and benefit provision for claimants of working age into one organization. The reform also strengthened job search assistance and monitoring for Incapacity Benefit recipients, something that had been in place for unemployment insurance claimants since the mid-1980s (e.g. Van Reenen, 2004).

The reform physically merged the offices that welfare claimants attend to collect benefit cheques, have work-focused interviews, and look for jobs. It simultaneously centralised information via modernised IT systems and reorganised premises, and changed the structure of bureaucratic incentives, without changing the overall financial generosity of the welfare system. In particular, an explicit incentive system was introduced, which awarded about three times as many points for moving a disabled person into work than for moving an unemployed person into work. These points fed into career progression for local bureaucrats.

Our empirical analysis exploits the staggered quasi-random roll-out of the Jobcentre Plus policy across geographical areas, using quarterly administrative data on 406 UK districts over a nine year period.[2]

[1] See Banks et al. (2015) for benefit trends in the UK and related reforms.

[2] UK districts are similar in size to US counties. There are 406 districts in Great Britain, with an average population of 120,000 in the 2001 census.

We identify the policy impact by comparing the change in exit rates for disability and unemployment benefit claimants in districts treated at a point in time to that in districts treated at an earlier or later date. Information on benefit claimants at the district level is provided by the Department for Work and Pensions, and we use quarterly series for stocks, inflows and outflows for various categories of welfare benefits, disaggregated by age and district, and, for the unemployed, by destination (e.g. to employment versus non-participation).

We obtain two main results. First, there were significant organisational disruption costs from the policy, with outflows from disability and unemployment benefits initially declining upon the policy change, and more markedly so for the unemployed than the disabled. Second, in the long-run there are significant positive effects on disabled outflows, whereas unemployment outflows, while still positive, are small and insignificant. These patterns are consistent with a simple model whereby bureaucratic efforts to reduce the disabled rolls increased, while efforts to reduce unemployment rolls decreased. Overlaid on this, there was a long-term positive effect on both groups from the reorganisation through better information, but a short-run negative effect from adjustment costs due to organizational disruption.

Based on our estimates, we provide a simple cost-benefit analysis of Jobcentre Plus that indicates positive net benefits from the policy. However, the presence of significant short-run costs from disruption and sunk set-up costs highlights why such welfare changes are hard to implement in practice. We estimate that it takes about six years for the reform to break even, which is beyond the time horizon of most policy-makers.

This paper links to two main strands of literature. First, the issue of welfare reforms has resurfaced following the Great Recession of 2008-2009. For example, increases in unemployment were much lower than expected in the UK[3] and Germany, and both countries experienced significant welfare reforms prior to the crisis. A body of work in the welfare literature has studied the effects of financial incentives to benefit recipients on the duration of unemployment (e.g. Lalive et al, 2007), while the role of explicit incentives in

[3] On the UK case see Blundell, Crawford and Jin (2014), Gregg, Machin and Salgado (2014) and Pessoa and Van Reenen (2014).

the provision of job placement services is to date less explored. Another strand of this literature has emphasized the interplay between unemployment and disability insurance, as some job losers may turn to disability benefits once they are no longer eligible for unemployment benefits. For the UK, Petrongolo (2009) finds that welfare recipients subject to stricter job search requirements were more likely to start spells on health-related benefits within six months of the end of a claimant unemployment spell, while Mueller et al. (2015) find no evidence that expiry of unemployment benefits raised applications for disability benefits in the US over the Great Recession. By contrast, our work explores links between unemployment and disability insurance stemming from job placement, rather than jobseekers' incentives.

Second, a growing literature has highlighted how incentive systems can be used to improve efficiency (see Bloom and Van Reenen, 2011, and Oyer and Scott, 2011, for surveys), and several papers in this literature have focused on the provision of incentives in the public sector (see inter alia Besley and Ghatak, 2005, for theoretical work, and Lavy, 2002, Burgess et al., 2007, and Baiker and Jacobson, 2007 for empirical applications to various areas of the public sector). Heckman et al. (2002) evaluate performance standards in a scenario close to ours, the provision of Government training programmes, and find that bureaucrats' rewards lead to cream-skimming and deliver significantly different short run and long-run programme impacts. We contribute to this literature by emphasizing the multitasking aspects in the provision of effort in government organisations.

The paper is organised as follows. In section 2 we describe the institutional framework in the UK and the hypotheses we test, in section 3 we outline the data used in the empirical analysis, while in section 4 we report the analysis and results of how the treatment impacts inflows into and outflows from different benefit categories. In section 5 we examine the robustness of our results to different specifications and in section 6 we perform a simple cost-benefit evaluation of Jobcentre Plus. Section 7 concludes.

## 2. Institutional background and identification

*2.1. The Jobcentre Plus system*

There have been major changes in the delivery of public employment and benefit services in the UK between 2001 and 2008. These changes came about against the backdrop of a wider policy emphasis on Welfare-to-Work initiatives that sought to increase labour market activity, aiming for "work for those who can and security for those who cannot" (Hyde et al., 2002). In March 2000, the Prime Minister announced the establishment of the Jobcentre Plus (JCPlus) organization, with the scope to deliver an integrated, work-focused service to both employers and benefit claimants of working age in UK. The creation of JCPlus stemmed from the integration of the Employment Service and Benefits Agency into one organization, combining benefit advice with job placement services. The integration took place in six waves between 2001 and 2008.[4]

Two main changes resulted from the introduction of JCPlus, broadly relating to information and incentives (see Riley et al. 2011 for a more detailed description of the program). On the information side, the integration of employment services and welfare checks under one roof was accompanied by a massive investment in improved information technology (IT) and organisational restructuring. The average size of an office increased as premises were combined, re-built, and offices refurbished. Aggregate floor space decreased by 20%, as did the total staff count, even though operating costs per square meter increased by 12% because of high quality infrastructure and locations. Overall, the sunk costs of re-organization were around £1.8bn, but running costs were reduced by £240m per year (National Audit Office, 2008).

[4] Links between benefit receipt and job search in the UK had been introduced with Restart Programme in 1986 (Dolton and O'Neill, 1996, 2002) and were deepened with the introduction of the Jobseekers Allowance in 1996, and the New Deal for Young People in 1998 (see Manning, 2009, Petrongolo, 2009 and Blundell et al., 2004). Mandatory work-focused interviews were in place for JSA recipients since 1996, well before the introduction of Jobcentre Plus (see Pointer and Barnes, 1997).

The second major change was the introduction of explicit performance targets called "Job Entry Targets". In contrast to the previous system of national-level targets for the number of beneficiaries to place into jobs, under the new regime every benefit officer who helped a benefit claimant into a job was awarded a certain number of explicit Job Entry Targets points varying by the category of the benefit claimant. In addition, there was a district-level target in terms of the number of points to achieve each quarter. These performance standards acted like a benchmark for the managers and mattered for the career prospects of the benefit officers.[5]

The Government's evaluation of JCPlus by Riley et al. (2011), based on local variation in treatment intensity across the 48 Jobcentre Districts in Britain, has found evidence of beneficial long-term impacts of the policy on the outflow rate from welfare benefits in general, and job finding in particular, for nearly all benefit and demographic groups. Their simulations find that the increase in effective labour supply following JCPlus would imply a 0.1% increase in GDP.

### *2.2. Framework*

One can theoretically expect at least three different effects of JCPlus on job placement. Firstly, the physical reorganization, installation of new IT systems and estate rationalization caused disruption. This would lead

[5] The UK welfare system had introduced performance benchmarking since the early 1980s (Propper and Wilson 2003; Bagaria et al, 2013). They have been designed according to targets embodied in the Public Service Arrangements (PSAs) of different government agencies. Makinson (2000) describes the performance standards in the Employment Service, The Benefits Agency, HM Customs and Excise and Inland Revenue. These mostly consisted of national-level targets for the number of beneficiaries to place into jobs, without explicit rewards at the individual or local level. The US welfare system has also introduced elements of performance pay within the recent US Ticket to Work (TTW) Program, providing job placement and ongoing employment support to disability insurance recipients. TTW service providers become eligible for payments from the Social Security as soon as beneficiaries receive earnings above a certain threshold.

to a short-run reduction in the productivity of welfare officers. We expect the adjustment cost effect to be broadly similar across all benefit groups – and we will test this assumption – and to decay over time as officers settle into the new system.

Second, there may be a long-run impact of restructuring and modern IT systems on efficiency. IT replaces routine manual tasks such as recording job entries and keeping records of beneficiaries. Increased automation of services would improve the speed and accuracy with which benefits applications are processed. This reduces operating costs as well as the time officers spend on these back office functions, and enables them to focus on conducting more client-facing job finding interviews. Thus, we expect a long-run increase in job placements for all benefit groups, as the provision of welfare services becomes more efficient. Again, this effect should be broadly similar across all benefit recipients.

Thirdly, the introduction of Job Entry Targets implies a shift in job placement incentives across the two main groups of beneficiaries, in favour of Incapacity Benefit (IB) claimants, at the expense of the Jobseekers Allowance (JSA) claimants. IB and JSA are two main UK systems of disability and unemployment insurance, respectively. Before the introduction of JCPlus, there were broad national level targets for job placements and sub-targets for different benefit categories. For example, in 2001 there was a national target to place 1.36m jobless people into work, accompanied by a sub-target to place 275,000 "disadvantaged" individuals into work.[6] With the JCPlus policy explicit award points were introduced under the new Job Entry Target system that were designed to reflect prioritizing IB claimants. As shown in Table A1, a benefit officer was awarded fifty per cent more points if he/she helped a person on IB into work than a long-term JSA beneficiary, and three times more points relative to a short-term JSA beneficiary.[7] High-power incentives to help IB beneficiaries into jobs were also accompanied by the introduction of mandatory "work-focused" interviews, which were already in place for other categories of benefit claimants

[6] This included those with disabilities, participants in New Deal for Lone Parents, partners of continuously unemployed for 26 weeks, homeless people and qualifying ex-offenders.

[7] We will discuss the other main group, Lone Parents (non-working single moms) in Section 6.

(most notably JSA claimants and lone parents on welfare) before the arrival of JCPlus.

Consider a multi-tasking model with fixed inputs along the lines of Holmstrom and Milgrom (1991). Assume that the JCPlus officers have a given stock of "inputs", and they can apply different amounts of this input to different individual clients, whereby officers' inputs affect clients' outcomes. In our context, the input variable represents staff time for interviews, job placement efforts, and the direct costs of the services provided. After the introduction of the explicit Job Entry Points system, we expect them to reallocate their efforts in favour of IB claimants, with adverse effects on JSA claimants' job finding prospects. Unfortunately, we are not able to measure staff inputs directly, but we can observe participant outcomes, and will relate these to the introduction of JCPlus in a reduced-form set-up.

### *2.3. Identification*

We exploit the staggered roll-out of JCPlus offices across 406 Local Authority Districts in Great Britain to identify the causal impact of the policy. The switch to JCPlus was phased-in over six waves, as illustrated in Figure 2. The figure shows the additional districts covered under each wave. The first wave begun on 1st October 2001 in 32 districts, the second wave in October 2002 with 27 more districts, and by the first quarter of 2008, almost 100% of the country was covered. Figure 3 presents a map of the policy roll out, showing no obvious patterns of geographic clusters that adopted the policy at the same time.

We consider treatment as the "go live" quarter for a district. To address concerns that districts were not randomly assigned into treatment, in Section 4 we allow for differences in pre-treatment trends between treated and non-treated districts for various benefit categories.

There are multiple offices in a district (between 32 and 171), and we also considered exploiting within-district variation from the policy diffusion across offices. However, districts had discretion about JCPlus roll-out across local offices, [8] and we find that although observables cannot predict which districts

[8] National Audit Office (2008) states that **"**Whilst an overall vision of the service improvements was successfully communicated from the centre, the detailed planning of the roll-out was delegated to the districts…. Implementation

are treated in each wave, there appear to be systematic components of office level treatment within districts. For example, we find evidence that districts tended to treat offices with higher JSA outflow first, while this is not true for district level treatment. Further, the points system was formally set at the district level, so this seems the natural level of variation in treatment.

We adopt a difference-in-differences framework to identify the causal impact of JCPlus. Since all districts are treated eventually, effectively we are comparing districts which are treated in a particular year and quarter to those who are treated at a later stage. Our main outcomes are the number of exits from disability and unemployment benefits in each quarter in each district, controlling for existing stocks of claimants.

One potential concern is that jobseekers may be manipulating the benefit category that they apply to, thus affecting the composition of the claimant stock in each clients' group and the corresponding outflow rate from benefits. For instance, benefit applicants may have an incentive to enter the caseload under the IB rules rather than under JSA rules. However, being classified as eligible for IB requires a medical certificate, and conditions for continuous receipt of benefits have been made stricter over time,[9] leaving limited leeway as to what kind of benefits one would be eligible for. A related concern is that the introduction of JCPlus may affect jobseekers' decisions whether to sign-on at all for benefits. To examine these concerns in more detail, we analyse the impact of JCPlus on the inflows into different benefit categories in Section 4.

---

of Jobcentre Plus was a locally driven process".

[9] For instance, in 1999, the Welfare Reform and Pensions Bill introduced 'c*ontinuing assessment of possibility of returning to work'* (Burchardt, 1999), whereby new claimants were required to attend more frequent interviews to assess whether the illness or disability affected the claimant's ability to work and discuss possibilities for returning to work. Personal advisers were also allocated to oversee each claim.

## 3. Data and Empirical Specifications

We use administrative data provided by the UK Department of Work and Pensions, covering the whole population on welfare. Information on existing benefit claims is organized in two main datasets. The first dataset contains monthly information from June 1983 onwards on the stocks, inflows and outflows of unemployment benefits' recipients (JSA), disaggregated by exit destination (work, other benefits, or inactivity). The data is available at the Local Authority District level. There are 406 districts in Great Britain, of which 352 in England, 32 in Scotland and 22 in Wales,[10] with an average population of 120,000 in the 2001 census, which is slightly above the average size of US counties. We further disaggregate claimants by age groups, 18-24 year olds, and 25-59 year olds.[11] The second dataset contains quarterly information from 1999Q3 onwards on other welfare benefits, among which the key disability benefit is IB. The same geographic and age breakdowns are available as for JSA, but given higher time aggregation for IB claims, in our main analysis we aggregate monthly JSA claims at the quarterly level and show JSA estimates on monthly data as a robustness test.

We estimate all specifications on a sample period of nine years, from 1999Q3 to 2008Q2, the quarter before the collapse of Lehman's, which triggered the Great Recession and a huge upsurge of unemployment. Descriptive statistics are presented in Table 1. Columns (1) and (2) refer to national aggregates per quarter, and columns (3) and (4) refer to (unweighted) averages across districts, age groups and quarters. There are nearly one million JSA claimants each quarter during our sample period. The

[10] Local government in England operates under either a single-tier system of unitary authorities and London boroughs, or a two-tier system of counties and district councils. The spatial units in our analysis include the unitary authorities, London boroughs and districts within counties. There are 352 such units in England. Local government in Scotland is organized through 32 unitary authorities. Since 1 April 1996, local government in Wales is organized through 22 single-tier principal areas. The Scottish and Welsh unit areas are also included in our sample.

[11] We have also considered an alternative age cut-off 25-54 for the older age group, and the empirical results were not affected.

quarterly JSA outflow is slightly above the corresponding inflow, as unemployment was falling over the period considered. The average outflow rate from unemployment is about 70% per quarter, corresponding to an expected unemployment duration of 4.3 months. The average stock of IB claims during the same period is just over two million, with much lower turnover than for JSA, implying that IB claims last on average almost nine years (1/(0.028×4)=8.93). Quarterly IB inflows are larger than outflows, as the IB caseload is on average still rising during 1999-2008 (see Figure 1).

### *3.1. Jobcentre Plus and Benefit Flows*

We estimate benefit outflow equations in a difference-in-differences framework. We start with a static specification that identifies the average effect over time following JCPlus introduction:

$$\ln Y^B_{ait} = \beta^B D_{it} + \gamma^B_1 \ln U^B_{ait-1} + \gamma^B_2 \ln U^B_{a'it-1} + \delta^B_{ai} + \delta^B_{at} + \varepsilon^B_{ait}. \qquad (1)$$

where $Y^B_{ait}$ is the number of claimants in age group $a$ leaving the benefit register $B$ (JSA or IB) in district $i$ and quarter $t$. $D_{it}$ is a treatment dummy that turns on in the quarter when the first office in district $i$ is treated. The coefficient of interest $\beta^B$ is identified by the staggered roll-out of JCPlus during six waves, with different districts being treated in each wave. In a robustness test we allow $\beta^B$ to vary across waves, and we show that the policy effect looks remarkably stable across waves when the post-wave window is kept fixed. As noted above, we find that the timing of when a district was treated appeared to be unrelated to observables.

We include as controls the stock of claimants of benefit $B$ at the end of the previous quarter for the own age group, $U^B_{ait-1}$, as well as for the other age group, $U^B_{a'it-1}$ (old/young respectively). Our preferred specifications include a full set of fixed effects (district by age) and age by time dummies, but we also show more restrictive specifications just including separate district, age and time effects. We cluster the standard errors at the district level, at which the policy is defined, but results are robust to alternative ways of dealing with spatial autocorrelation (e.g. Conley, 1999).

While the treatment effect $\beta^B$ in equation (1) is an average over all post-treatment quarters, potential adjustment costs in the implementation of JCPlus suggest that short- and long-run policy effects may indeed differ, thus we next estimate the following dynamic specification of policy impact:

$$\ln Y_{ait}^{B} = \sum_{\tau=1}^{7} \beta_{\tau}^{B} D_{it+\tau} + \beta_{LR}^{B} D_{iLR} + \gamma_1^{B} \ln U_{ait-1}^{B} + \gamma_2^{B} \ln U_{a'it-1}^{B} + \delta_{ai}^{B} + \delta_{at}^{B} + \varepsilon_{ait}^{B} \,. \quad (2)$$

The $D_{it+\tau}$ term is broken down such that $D_{i1}$ is the quarter in which the policy is turned on, $D_{i2}$ is the first quarter after the policy is turned on, and so on. $D_{iLR}$ is the "long-run", defined as eight quarters or more since the policy change. Since the last treatment wave is in 2006Q3, we have at least two years of post-policy experience for all districts. While ending the dynamics after two years is somewhat arbitrary, the treatment coefficients seem stable afterwards and we show that the qualitative results are robust to alternative dynamic specifications (e.g. Table A4)

The presence of adjustment costs would suggest, for either type of benefit, $\beta_t^B < \beta_{t+1}^B$, implying that initial negative disruption unwinds as the new organizational structure settles in. The incentive hypothesis suggests that, in each quarter $t$, the policy effects on IB should be stronger than on JSA i.e. $\beta_t^{IB} > \beta_t^{JSA}$.

## 4. Results

In this section we present both the average impact of JCPlus on the outflow from benefits, as well as its dynamic evolution, and then turn to examining its effect on inflows into benefits.

### *4.1. Basic Results on Outflows from benefits*

Table 2 reports estimates of specification (1) for the log outflow from unemployment (JSA, columns 1-3) and disability rolls (IB, columns 4-6). All regressions control for pre-existing stocks and include age, district and time effects. Column 1 shows that the introduction of JCPlus had on average a negative and significant impact on the unemployment outflow, suggesting that a treated district experiences on average a 1.5% decrease in unemployment outflows. Given an average unemployment outflow of about 650,000 per quarter, this implies just under 10,000 more people staying on unemployment benefits. This overall impact is consistent with both disruption effects and incentives for benefit officers to substitute effort away from the unemployed and towards the disabled. As expected, the lagged stock of own age unemployed claimants enters with a significant positive coefficient, while the stock of the other age group has a negative impact, in line with job competition across age groups. The policy effect remains virtually unchanged when we control for district by age interactions in column 2, as well as age by year interactions in column 3, although the competition effect across age groups becomes small and not significant.

The corresponding regressions of IB outflows in columns 4-6 deliver a positive and weakly significant effect of policy on the disability rolls. Estimates reported in column 4 suggest a 1.7% increase in outflows. Given a sample average outflow of 56,000 people, the introduction of JCPlus drives almost an additional 1,000 people off the IB register each quarter. In more flexible specifications of columns 5 and 6 the treatment effect falls slightly to 1.6% and 1.5% respectively, very close in absolute value to the policy coefficient in unemployment outflow equations.[12] Unlike the JSA outflows, there is no evidence of job competition across age groups. In fact, IB outflows of one age group are positively correlated to the IB stock of the other group.

To distinguish between the transition dynamics into the new regime and long-run policy effects, Table 3 looks more closely at the time structure of treatment, allowing for varying impact of policy by quarter since its introduction as in equation (2). Columns 1-3 for JSA outflows show a consistent dynamic pattern, with rising policy effects over time. The policy impact is negative and significant in the first five quarters since intervention, then ceases to be significant by the sixth and seventh quarters, and turns positive from the eighth quarter. We detect a positive long-run effect of 1.2% on JSA outflows, although this estimate is not significantly different from zero.

In contrast, for IB outflows, although we still detect a negative effect in the first quarter the policy

[12] The results are robust to conditioning on stocks (by age group) of other benefit recipients (i.e. IB and lone parent stocks in JSA outflow equations, JSA and lone parent stocks in IB outflow equations).

is introduced, coefficients turn positive by the second quarter. This positive effect gradually becomes larger and more significant, and the long-run estimate in the most general specification of column 6 implies an extra 6.1% IB recipients leaving the register each quarter, corresponding to roughly 3,400 individuals.

These dynamic responses are presented graphically in Figure 4 and highlight our two main findings. First, the long-run effect of JCPlus is positive for both forms of welfare, but it is clearly much stronger for disability outflows (Panel B) than unemployment outflows (Panel A). Second, there is initially a negative effect for both benefits, but this is much stronger for unemployment than disability benefits.

One explanation for the more positive long-run effect of policy on disability relative to unemployment outflows is the new structure of bureaucratic incentives, whereby officers devote more effort to helping IB recipients into new jobs than JSA recipients after the policy change. Overlaid on this, however, there are (i) an initial disruption effect as new premises and IT systems bed down and (ii) a generally positive effect on both groups from improved organization and information.

An alternative explanation would be that incentives do not matter but somehow the (beneficial) organization and information treatment had a disproportionately larger effect on IB claimants than the unemployed. It is not obvious why this should be the case, but in Section 6 we will look at more refined tests of the incentives hypothesis in two ways. First, we look at policy effects on benefit outflows for a third group of welfare recipients, lone parents, for whom bureaucratic incentives are somewhere in-between those for the other two groups. Second, we exploit further changes in the incentive design, from individual to team level incentives, which were gradually rolled-out from January 2005 in districts that previously adopted JCPlus. Both robustness tests highlight benefit outflow responses in line with the underlying incentive structure.

### *4.2 Pre-policy trends*

A threat to a causal interpretation of our estimates would be the existence of differential pre-policy trends. For example, if districts initially selected for treatment were those in which IB outflows were already increasing (and/or JSA outflows decreasing), we would estimate a positive (respectively, negative) impact of treatment even in the case in which the policy had no real effect. To investigate this we look at pre-treatment trends by estimating the following augmented specification of equation (1):

$$\ln Y_{ait}^{B} = \sum_{k=1}^{K} \beta_k^B D_{it-k} + \beta^B D_{it} + \gamma_1^B \ln U_{ait-1}^{B} + \gamma_2^B \ln U_{a'it-1}^{B} + \delta_{ai}^{B} + \delta_{at}^{B} + \varepsilon_{ait}^{B}. \qquad (3)$$

The first term on the right hand side of equation (3), $\sum_{k=1}^{K} \beta_k^B D_{it-k}$, allows for pre-policy trends. The results are reported in Table 4. Column 1 replicates our baseline specification for JSA outflows (column 3 of Table 2) for reference. The specification of column 2 includes four pre-treatment lags, and the coefficients on the pre-treatment dummies are jointly insignificant (with an F-statistics of 1.88).[13] Columns 3 and 4 of Table 4 report the corresponding specifications for IB, and column 4 shows again no evidence of pre-treatment effects, with an F-statistics of 1.36.

Although these results are reassuring, one caveat is that the individual dummy for the quarter immediately prior to treatment is significant at the 10% level for JSA in column 2. This may be due to the fact that our treatment indicator is based on the true "go live" date of JCPlus and there is likely to be some organizational disruption in advance of that date, which could spill into the previous quarter.[14] This would reduce the benefits of the policy for JSA, but since these are insignificantly positive in the long-run anyway, it makes no substantive difference.

*4.3 Inflow Rates*

While the focus of our analysis is on the intended impact of the introduction of JCPlus on benefit outflows,

[13] In Table A2 of the Appendix we run similar regressions for JSA outflows at the office level and we find that pre-trends are indeed jointly significant, and in particular that offices with lower outflows tend to be treated first. Thus office level variation would not help identify the causal effect of policy. We cannot run a similar analysis at the office level of IB outflows, because the lowest level of disaggregation available for data on IB recipients is the district.

[14] National Audit Office (2008) states that "[JCPlus] introduces a radical shift from the former impersonal surroundings of the Jobcentre and Social Security offices to a modern retail-style environment and has a major impact on the way in which staff interact with customers and hence the quality of service provided."

the policy may have potential side effects on the take-up rate of welfare benefits, as a consequence of changes in treatment of recipients of either type of benefit. The resulting bias in the estimated policy effect depends on underlying selection mechanisms. For example, if reduced incentives to help JSA recipients into work dissuaded less motivated individuals to sign up for unemployment benefits, the estimated policy impact on JSA outflow would be upward biased – and vice versa for IB.

To examine this issue directly we analyse the impact of the JCPlus on inflows into JSA and IB, and estimate a specification similar to equation (1), using the inflow into each benefit category as the dependent variable:

$$\ln(Inflow_{ait}^{B}) = \sum_{\tau=0}^{4} \beta_{\tau}^{B} D_{it+\tau} + \beta_{LR}^{B} D_{iLR} + \rho \ln Pop_{ait} + \delta_{ai}^{B} + \delta_{at}^{B} + \varepsilon_{ait}^{B}, \qquad (4)$$

where $Inflow_{ait}^{B}$ denotes the number of individuals of age $a$, signing-on for benefits of type $B$ in district $i$ in quarter $t$. In the outflow equations we have controlled for the stock of existing benefit claimants, and the corresponding stock in the inflow equations is the age-specific population ($Pop_{ait}$). Ideally, as inflows (mostly) consist of people flowing from employment into welfare, one should control for local employment on the right-hand side, but in the absence of high-frequency employment data at the district level we use the population figures as a proxy.[15]

Estimates of inflow equations are reported in Table 5, distinguishing between long-run effects and transition dynamics. Column 1 shows that, on average, JCPlus had no significant effect on the inflows into JSA, and similarly for IB inflows in column 3. However, when looking at the short-run policy impact in columns 2 and 4, we detect an initial negative and significant impact of JCPlus on inflows into both types of benefits, which becomes positive and insignificant in the long-run.

To address whether this could pose a concern for our results due to changing selection of benefit

[15] We assign the mid-year population estimate from www.nomisweb.com (taken on the 30th of June each year) to all the quarters in the year. Using interpolated quarterly population estimates (from the mid-year estimates) does not change our results.

recipients, we repeat outflow regressions controlling for various lags of the corresponding inflows. The results are reported in Table 6 and represented graphically in Figures 5 and 6 for JSA and IB outflows respectively.[16] Columns 1-3 in Table 6 refer to JSA outflows. Although the coefficients on the inflow variables, whether one or four lags, are positive and significant as one would expect – since more recent welfare recipients are more likely to leave – our main results are robust to their inclusion. To see this, in column 3 we report the estimates of our baseline specification (2) on the same sample as in column 2, in which some observations are lost when we condition on lagged inflows. The long-run effects in columns 1 and 2 are almost identical to those of our baseline specification in column 3, and the dynamic effects are only slightly muted by the inclusion of inflows. Columns 4-6 refer to IB outflows, and all coefficients measuring the impact of policy are both qualitatively and quantitatively similar across specifications. In particular, the long-run positive effect of the policy on IB is still significant and only falls slightly from 0.0547 in column 6 to 0.0503 in column 5.  Hence, despite some initial effects on benefit inflows, any resulting change in claimants' composition does not appear to significantly affect our results.

### *4.4. Outflows to employment versus other destinations*

The JSA (though not the IB) database allows us to disaggregate outflows into alternative destinations, and in particular to look at outflows into work separately from outflows into other states (such as different benefits, training, inactivity, etc.). Table 7 reports results of JSA outflow regressions for alternative destinations, where columns 1 and 2 refer to outflows into work, while columns 3 and 4 refer to other destinations. The broad pattern for either destination looks similar to the overall outflow results, although the estimated effects appear stronger especially in the short run when looking at outflows into work rather than other destinations.

Negative effects on JSA outflows into both work and non-work can be rationalized if one takes into

[16] In alternative specifications, we explicitly control for the duration composition of the stock of benefit claimants at the end of the previous quarter, and find that the baseline outflow results are robust.

account the “stick” (search effort monitoring) and “carrot” (job search assistance) components of the interactions between JSA claimants and dedicated staff at Job Centres. The change in the incentive structure implies that JSA claimants would receive less assistance with the job search process than before, thus lowering their job finding rates, at least in the short run. But insofar as poorer job search assistance also implied less frequent contact with JSA claimants, one may expect looser monitoring and fewer transitions off benefits due to sanctions or discouragement (see also Manning, 2008, and Petrongolo, 2009, for the effects of monitoring on the time spent on JSA benefits).

Another interesting point to be noted about columns 1 and 3 is that the congestion effect stemming from job competition by jobseekers from other age groups is clearly not present in the JSA outflow into other destinations, as the other age group could be competing for jobs in the labour market, but not for other destinations.

Overall, the results in both specifications in Table 7 are comparable to the earlier results on total outflow in Table 2. This reinforces the validity of using total outflow as our dependent variable to proxy for outflow to work.

## 5. Cost-benefit evaluation

For the purpose of our cost-benefit evaluation we consider a policy simultaneously introduced throughout the country, as the staggered roll-out would not offer much general insight into costs and benefits of similar hypothetical policies in other contexts. We conduct two thought experiments. First, we assume away the transitional disruption costs and assume that the steady state is reached immediately upon JCPlus introduction. This gives an idea of the long-run welfare effects of the policy. Second, we explicitly incorporate the dynamic effects reported in Table 3 and illustrate how costs and benefits map out over the transition to the long-run steady state. This produces lower benefits because disruption effects cause an initial increase in the welfare rolls. With discounting, this will reduce the present value of the policy change because the losses – including the initial rise in welfare rolls and set-up costs – are front-loaded, whereas the long-run benefits are more heavily discounted.

Our cost-benefit calculations take into account (i) the savings in administration costs implied by the reorganization of the welfare system; (ii) the increase in output implied by the impact of the policy on job finding; (iii) the net exchequer savings which enter into welfare through a lower deadweight loss taxation (the rest simply being transfers); (iv) the sunk set-up costs. We abstract from the leisure gains and/or psychic losses for being on welfare.

*5.1. Long-term Cost-benefit Evaluation*

The results of the analysis of long-term effects are presented in Table 8. According to audit reports, the annual running costs post-policy were £3.3bn (row 1), about £238m lower than pre-policy (see rows 2 and 3). The long-term impact of JCPlus on job creation is obtained from the long-term estimates reported in columns 3 and 6 of Table 3. Conservatively, we assume that the long-term policy impact on unemployment outflows is zero, as although the point estimate is positive (0.012), it is insignificantly different from zero. We set the long-term impact on IB exits at 0.061. Using this estimate, we obtain the implied steady-state fall in the IB rate (IB stock over population), according to a flow model of IB entry and exit, as shown in Appendix C. Not all of these exits would be into employment. Using the Labour Force Survey (LFS) quarterly panel data for 1998Q2-2002Q2 (pre-policy) we observe that 30% of IB exists are to jobs, while 70% of terminations transit into other benefits or out of the labour force. We also find that 71% of the exits to jobs are full-time while the rest are part-time. We assume that non-employment exits would be to other benefits with cost on average equivalent to IB. This implies that IB spells that do not terminate into employment do not contribute to either job creation or to benefit savings. This is a conservative estimate of policy benefits, as several IB exits will be to states not covered by welfare benefits.

We use wage outcomes as proxies for additional output created, and consider three possible cases for the wages of individuals finding employment after an IB spell: the national minimum wage, the observed mean wage for individuals ending an IB spell in the LFS, and the median wage in the overall wage distribution, obtained from the ASHE 1% sample of taxpayers. The middle case seems the most realistic but the minimum wage and median wage scenarios provide useful lower and upper bounds, respectively. Columns 1 to 3 in Table 8 correspond to the three alternative wage outcomes considered. Row 4 reports weekly earnings for each wage outcome, and row 5 reports the increase in GDP obtained by combining wage levels with job creation resulting from IB exits. The overall GDP gains range between £0.5bn and £1.4bn per year.

Row 6 reports the net gain resulting from a reduced deadweight cost of taxation. This is set to 40% (Gruber, 2011) of the lower net exchequer cost arising from increased tax revenues and lower benefit payments. The mean IB payment in 2000 was £74.71 per week. When an IB recipient finds a job, this benefit saving is accompanied by a change in the tax revenue that depends on the earnings and household composition of the recipient. We used the IFS TAXBEN[17] simulation model to approximate net taxes paid by the 30% of IB exits who found jobs.[18] Combining these elements produces a benefit from a lower deadweight loss between £110m and £200m.

The sum of the three components reported in rows 3, 5 and 6 of Table 8 represents the total annual welfare impact of the policy in the long run. This implies an annual net benefit between £1bn and £2.2bn in 2010 prices (row 8). This benefit needs to be compared to the one-off set-up cost of £2.3bn (row 10), as estimated by audit reports. The policy easily covers the sunk costs of the programme, even on conservative assumptions. If we use the 3.5% social discount rate used by the UK government (HM Treasury, 2003) our cost-benefit analysis implies a net benefit of JCPlus in excess of £25bn, even under the most conservative assumptions about reemployment earnings (row 11).

---

[17] Estimates were provided by Barra Roantree of the Institute for Fiscal Studies using the IFS tax and benefit micro-simulation model, TAXBEN.

[18] We consider two household types, a single adult and a couple with two dependent children, and obtain the associated tax payments. We assume that two thirds of IB exits are represented by single adults, while the remaining third is represented by members of couples with two children consistent with our estimates from the LFS 1998Q2-2002Q2.

*5.2. Cost Benefit Evaluation with Transitional Dynamics*

While previous calculations ignore the transitional dynamics, we now consider the dynamic effects of policy for each quarter since the policy change, using the estimates from columns 3 and 6 of Table 3. In this case we cannot impose the steady-assumptions used to compute the steady-state rise in the number of jobs, as this would be equivalent to assuming that the JSA and the IB rates reach their steady state levels within a quarter. We thus simply obtain the out-of-steady-state number of jobs created as the predicted change in the benefit outflow in the relevant quarter, net of job separations during that quarter. With labour market churning, some of the workers who find jobs separate in subsequent quarters. We estimate these flows from the (pre-policy) LFS panel. For individuals who were on JSA and found jobs 2.3% lost them in the next quarter, and for IB the figure was 0.5%.[19]

The three earnings scenarios, as well as the running costs, are the same as those considered for the long-run analysis of Table 8. We maintain all other assumptions on job finding rates for IB recipients, and, for JSA recipients, we compute on the LFS that 70% of exits were to jobs.[20]

The evolution of costs and benefits over time is represented in Figure 7. The flat, solid line represents the set-up costs, while the three dashed lines represent cumulative benefits since the quarter in which the policy turns on, for the three different levels of earnings. Regardless of the earnings assumptions, flow social benefits eventually exceed the costs so although incorporating dynamics substantially dampens down the net benefits (by almost an order of magnitude), it does not reverse the earlier positive assessment of the program. The present value of the net benefit of the reform is about £2.5-3.8bn, which outweighs the

[19] The job separation rate is obtained as the ratio of inflows into JSA or IB to the employed population of working age. Quantitative results obtained here are very similar to those obtained on an analytical approximation of the change in employment rates during the transition to a new steady state (see Appendix C2).

[20] For the benefit and tax simulation we assume that 70% of JSA exits who find jobs live alone, while 30% live in a couple with two children. For IB, about 67% of those who find jobs live alone, while 33% live in a couple with two children.

£2.3bn sunk cost.

It is worth noting that in the baseline case (middle dashed line in Figure 7), it takes about six years for policy benefits to exceed the set-up costs. This is mainly due to the decline in claimant unemployment outflows during the first two years of the new regime. Only after six years into the new regime are job entry gains sufficient to compensate both the initial job entry losses and the set-up cost. Therefore, although this is a policy which clearly passes the cost-benefit test, a policy maker would be unable to cover the costs of its implementation for six years. Constitutionally, UK general elections are held every five years and the average tenure of a minister is usually only two years. Thus a politician's discount rate would be much higher than the social discount rate, possibly leading to under-investment (see e.g. Aghion et al, 2013).

## 6. Robustness Tests and Extensions

### *6.1. Treatment Effects by Wave*

The policy roll out was introduced in six waves across the country, and our baseline estimates pool variation from all waves for identification. An important issue is whether the effect of treatment is heterogeneous across different waves, in which case the dynamic policy effects that we estimate might instead be due to averaging over heterogeneous effects in earlier and later waves.

To investigate this we estimate equation (1) separately for each wave of the policy roll-out. In order to avoid conflating dynamics effects with heterogeneous wave effects we keep a fixed post-treatment window of one year. The results are reported in Table 9. Although the standard errors are larger as the number of observations is substantially reduced, the estimated treatment effect is remarkably stable across the different waves. Panel A refers to JSA outflows. Compared to the pooled effect reported in Table 2 of -0.015, wave-specific estimates range from -0.010 (wave 5) to -0.020 (wave 1), which is a reasonably tight bound. IB estimates in Panel B are generally higher (a range of 0.011 to 0.032) than the pooled estimate of 0.015, suggesting, if anything, that we might be underestimating the beneficial effects of the programme on IB outflows using the parsimonious specification of equation (1).

*6.2 Lone Parents' Benefits*

Besides JSA and IB, lone parents (overwhelmingly single mothers) on income support are the third largest category on welfare rolls. Table A1 shows that the points awarded to helping a lone parent into work are the same as for IB. However IB, also experienced stronger pressure to find work via work-focused interviews, which were already in place for lone parents on income support. Thus we may expect a weaker policy treatment for lone parents than for IB recipients.[21]

Table 10 reports results of lone parents' welfare outflow equations. In column 1 we estimate the static specification (1) and detect an average decrease of about 1.3% in outflows following the policy. However, when testing for the presence of pre-trends in column 2 (based on the analogue of equation (3)), the F-test on their joint significance rejects the hypothesis of no pre-policy trends (F=3.894), whereas we did not find evidence of differential pre-treatment trends for the JSA or IB outflows (Table 4). We attempt to control for these pre-trends by including district-specific trends in column 3 and the resulting F-test implies that the pre-trends are not jointly significant. Similarly to the JSA results, however, we do find a negative effect one quarter before treatment, consistent with the impact of organizational restructuring which hinders service delivery even before the true "go live" date. When pre-treatment dummies are dropped in column 4, the coefficient on the policy variable is -0.01 and not significantly different from zero, falling about half way between the IB and JSA effects. In column (5) we distinguish between transition dynamics and long-run effects of policy, and find an initial negative impact, which turns positive by quarter 6 and significant in the long run. The long-run effect of 2.5% on lone parents' welfare outflows is smaller than the long-run IB effect of 6.1% but larger than the JSA effect of 1.2% (see Table 3).

Overall, treatment effects of JCPlus on lone parents' welfare exits appear to lie between the effects

[21] There were also many other policies aimed at lone parents during the same time period, including a large increase in the generosity of in-work benefits (the Working Family Tax Credit, similar to EITC) and a voluntary job assistance programme ("New Deal for Lone Parents"), which may contaminate our identification.

of JSA and IB. Similarly as for JSA and IB outflows, we detect an initial negative policy effect for lone parents' outflows, which we interpret as a temporary disruption effect. However, outflows increase in the long run, consistent with the improvement in organization and the new incentive structure.

### *6.3. Spillover Effects*

One potential concern is that, in common with standard difference in differences approaches, our main estimates do not factor in general equilibrium effects of the policy. For example, Crépon et al. (2013) find that there may be unintended negative externalities of active labour market policies as higher exits for one group of welfare recipients may crowd out exits for other groups, especially in depressed labour markets. We examine this idea by looking at outflows in districts next to treated ones, using an augmented version of equation (1):

$$\ln Y_{ait}^{B} = \beta^{B} D_{it} + \mu^{B} NBR_{it} + \gamma_{1}^{B} \ln U_{ait-1}^{B} + \gamma_{2}^{B} \ln U_{a'it-1}^{B} + \delta_{ai}^{B} + \delta_{at}^{B} + \varepsilon_{ait}^{B}. \quad (5)$$

We capture spillovers using a dummy ($NBR_{it}$) that turns one in the quarter when a district's neighbours are treated. We define neighbour districts as those with centroids within 10 km of the centroid of the reference district. The effect of interest is captured by the parameter $\mu^{B}$ and is identified by the fact that different districts had their neighbours treated in different quarters.

The results are shown in Table 11. The sample is now smaller for two reasons. First, estimates are now based on the first five waves only, since all neighbours are treated by the sixth wave. Second, for a few districts all neighbours have centroids further away than 10 km. The baseline impacts on JSA and IB hold true even in this sample, as shown in columns 1 and 3. In column 2, the coefficient on $NBR_{it}$ is positive, consistent with spillovers effects, as the local unemployed find it easier to get jobs due to lower competition from JSA outflows in the treated, surrounding areas. However, the coefficient is only marginally significant, and the main policy effect on JSA outflows remains robust to the inclusion of spillover effects. For IB exits, in column 4, the coefficient on $NBR_{it}$ is positive but far from significant, while the main policy effect remains positive and significant.

We investigated a range of alternative specifications, using other bandwidths for defining neighbours, including the proportion of treated neighbours rather than a discrete dummy for any neighbour treated, weighting by distance, interacting the policy and spillover effects with measures of labor market tightness (using vacancy rates), and interacting the policy effects with lagged stocks of benefit claimants. In no case could we find evidence that the policy had significant effects on other groups.

*6.4. Further Evidence on Response to Incentives: Job Outcome Targets*

Following the rollout of JCPlus and the associated JET incentive scheme, the structure of incentives in Employment Services was further adjusted with the introduction of Job Outcome Targets (JOT) in January 2005. The JOT scheme enriched the monitoring of JCPlus performance, by combining information on claimants flowing off the benefit register with data on their employment status from Her Majesty's Revenue and Customs (HMRC) tax records. However, because of the time-lag involved in collecting tax record information from HMRC, JOT points were recorded with a lag of up to six months[22] and were only measurable at the aggregate team level. This marked a change in performance measurement and management from the existing JET system, which attributed rewards to individual staff members on a daily basis. In particular the shift from individual to team performance evaluation is expected to dilute individual incentives to prioritise IB claimants over JSA claimants (Holmstrom, 1982).[23]

For reporting and rewarding purposes, teams were defined at the district level, and information on individual level performance became unavailable. Thus the number of employees in a district is the

[22] This lag corresponds to the six-month window for setting up a tax account upon starting an employment spell.

[23] Anecdotal evidence suggests that the JOT impacted employee motivation and behaviour. One District Manager described the situation as "It's really hard with regards to JOT, we haven't got a clue; we used to know with JET because you had your daily placing list which detailed each placement inputted the day before and you knew how many points each customer is worth and so you knew where you were in terms of your target. [..] As a manager trying to manage performance it's really difficult." (Based on interviews in Davis, James and Tuohy, 2007).

effective team size which may dilute individual incentives.

Similarly to JCPlus, JOT was gradually introduced across UK districts. Its first wave was started in January 2005, covering 18 districts, comprising 50 offices in total. The program was phased-in in the remaining districts from April 2006. We exploit this staggered rollout to identify the causal impact of team incentives on benefit outflows, using the same difference-in-differences identification as in the rest of the paper. Due to missing information on office size for 68 districts, the analysis that follows will be based on 338 districts.

Table 12 presents regression results. Columns 1 and 2 consider the impact of team work, independent of team size, on JSA and IB outflows, respectively. This is represented by the interaction between the main treatment (i.e. the introduction of JCPlus, denoted by $D_{it}$), and the introduction of JOT (denoted by $D_{it}^{JOT}$). We would expect the effect of the interaction term $D_{it} * D_{it}^{JOT}$ to work in the opposite direction of the main treatment effect $D_{it}$ whenever team work dilutes individual incentives. Indeed this is the case both for JSA and IB outflows, as the introduction of JOT offsets the initial negative and positive impacts, respectively, of JCPlus. Columns 3 and 4 explicitly consider the role of team size by controlling for the district level number of employees in the public employment service ($S_i$). This variable is available for 2004 and 2007, and we only use 2004 data, as post-JOT district size may endogenously respond to the redefinition of incentives. In 2004, the mean district size was 210 and the median was 96. Dilution of individual incentives is now captured by the triple interaction term ($D_{it} * D_{it}^{JOT} * \ln(S_i)$). As expected, the offset of the main policy effect $D_{it}$ increases with team size, for both JSA and IB outflows.[24] For JSA, the team size that perfectly offsets the detrimental impact of JCPlus on benefit outflows is 41 ($-0.0436 + 0.0145 * \ln(41) \cong -0.0103$). For IB, the threshold team size is 28 ($0.0444 - 0.0115 * \ln(28) \cong 0.0060$). Overall the benefit outflow responses reported in Table 12 are consistent with the role of team, as

[24] The coefficient on the double interaction term $D_{it} * D_{it}^{JOT}$ now has the same sign as the main effect, as it represents the impact of "one-man teams", and it thus conceptually similar to the main effect policy effect.

opposed to individual, incentives.

*6.5. Other Robustness Tests*

We have subjected our results to several other robustness tests, some of which we describe below.

*Other programmes.* A concern with our design is that our estimated treatment effects may be potentially confounded by other policies implemented at the same time. The only other important policy targeted at IB claimants we are aware of is the "Pathways to Work" programme, which aimed to help claimants better understand and manage their health conditions and thereby improve their work prospects. It was originally introduced in October 2003 in eight pilot areas, and rolled-out to 14 expansion areas from October 2005.[25] When including post-treatment dummies for the areas affected our baseline estimates are virtually unchanged. For example, using the specification of columns 3 and 6 in Table 2, the estimated long-term impact of JCPlus on JSA outflows changes from a coefficient (standard error) of -0.0152 (0.0055) to -0.0147 (0.0055), and the estimated long-term impact on IB outflows goes from 0.0151 (0.0089) to 0.0149 (0.0087).

*Alternative dynamic specifications.* We explore alternative dynamic specifications in Table A3, by varying the length of the short-run post-policy window, and the estimates obtained confirm the robustness of our main specifications. Short-run estimates of JCPlus are lower than long-run effects, and significantly negative for JSA outflows, and long-run estimates are positive, though only significant for IB outflows.

*Weighting.* To address the concern that our results may be driven by a few small districts, we weight

[25] See Becker et al. (2010). Pilot areas were Bridgend, Gateshead, Somerset, East Lancashire, Essex, Derbyshire and Renfrewshire, Inverclyde, Argyll and Bute. The expansion occurred in three phases: phase 1 from October 2005 (covering Tees Valley, Cumbria, Lancashire West and Glasgow), phase 2 from April 2006 (covering Barnsley, Doncaster & Rotherham, City of Sunderland, County Durham, Lanarkshire & East Dunbartonshire, Liverpool & Wirral, Greater Manchester Central and South West Wales; and phase 3 from October 2006 (covering Eastern Valleys, Greater Mersey and Staffordshire).

observations by the district-level, age-specific benefit caseload in the pre-policy period (1999Q3). Table A4 reports the results for equation (1) using this weighting system. Column 1 shows an average policy effect on JSA outflows of -2.5%, which is somewhat stronger than the baseline results of Table 2. Dynamic effects reported in column 2 are instead very similar to those based on unweighted regressions of Table 3. For IB, the average effect reported in column 3 is weaker than in the unweighted regression and not significant. The short- and long-run effects are however very similar to those obtained on the unweighted regression.

*Estimates at Monthly Frequency.* We are able to estimate JSA (but not IB) outflow equations at the monthly, rather than quarterly, frequency. The dependent variable is now the monthly outflow from JSA, having included the stock at the end of the previous month as a control. Column 1 in Table A5 shows a policy coefficient unemployment outflows of -1.6%, which is very close to the baseline -1.5%. The dynamic results in column 2 are also very similar to the baseline results. These are also represented graphically in Figure A1.

*Heterogeneous Policy Effects.* We investigate whether treatment effects are heterogeneous in interesting ways across different groups. In particular we look at whether the coefficients in columns 3 and 6 of Table 3 differ for welfare recipients of different ages, benefit durations, regions (e.g. London vs. others), and so on. We did not find evidence for much systematic heterogeneity across these groups (results not reported).

## 7. Conclusions

The UK embarked on a major change in the administration of welfare benefits for the unemployed and the disabled in 2001 with the introduction of JCPlus. Bureaucratic incentives to help the disabled into jobs were sharpened, and offices were re-organised to be more efficient. At the same time, the growth of the stock of Incapacity Benefit recipients, which had been rising for 30 years, stopped increasing. By exploiting the staggered introduction of JCPlus across UK districts, we evaluate this policy in the light of a framework encompassing incentives, reorganization and adjustment costs. We show that there are potentially two unintended consequences of the policy change. First, the relative incentives to help the unemployed into jobs fell. Second, the re-organization of the job centres temporarily reduced outflow rates from benefits, likely due to disruption effects.

We found several results that are consistent with the existence of incentive and organization effects. First, we detect an increase in the outflow rates of both the groups of disabled and unemployed in the long-run, but the effects are much larger and only significant for the disabled. Second, there is evidence of important disruption effects, with outflow rates initially falling after the policy change for both groups.

A dynamic cost-benefit analysis of the policy suggests that the short-run costs are easily outweighed by long-run benefits. However, the benefits of the program take time to be visible and this poses a problem for policy-makers whose time horizons may be much shorter than that of a social planner. This highlights the political economy problem at the heart of welfare reform: changes to the administration of the benefit system that have long-run benefits may have significant short-run costs, and this makes it hard to build up a coalition for change.

There are many directions that the work could be taken. To what extent does the increased labour supply lead to lower equilibrium wages (not just due to compositional changes)? Can we unbundle further some of the elements of the policy to distinguish incentives effects from information (which conceivably could be more important for the disabled)? Could similar reforms be effective in other countries that have also seen large increases in the disability rolls? These are areas that we are currently pursuing.

## References

Aghion, Philippe, Tim Besley, John Browne, Francesco Caselli, Richard Lambert, Rachel Lomax, Nick Stern and John Van Reenen (2013) *Investing for Prosperity: Report of the LSE Growth Commission,* LSE: London.

Autor, David (2012) "The Unsustainable Rise of the Disability Rolls in the United States: Causes, Consequences and Policy Options", Brookings Papers.

Autor, David and Mark Duggan (2003) "The rise of disability rolls and the decline in unemployment" *Quarterly Journal of Economics* 188(1), 157-206.

Banks, James, Richard Blundell and Carl Emmerson (2015) "Disability Benefit Receipt and Reform: Reconciling Trends in the United Kingdom", UCL mimeo

Bagaria, Nitika, Novella Bottini and Miguel Coelho (2013), "Human Capital and Economic Growth: A Focus on Primary and Secondary Education in the UK", *LSE Growth Commission Report*.

Barnes Martin and Richard Poynter (1997), *Jobseeker's Allowance Handbook*, Child Poverty Action Group, London.

Beatty, Christina and Steve Fothergill (2009), "Incapacity Benefits in the UK: An Issue of Health or Jobs?" Sheffield Hallam University mimeo.

Becker, Elizabeth, Oliver Hayllar and Martin Wood (2010) "Pathways to Work: programme engagement and work patterns", DWP Research Report 653

Besley, Tim and Maitreesh Ghatak (2005), "Competition and Incentives with Motivated Agents", *American Economic Review* 95(3): 616-636.

Bloom, Nicholas and John Van Reenen (2011), "Human Resource Management and Productivity", *Handbook of Labor Economics Volume 4B* in Orley Ashenfelter and David Card (eds), Chapter 19 1697-1769.

Blundell, Richard, Monica Costa Dias, Costas Meghir and John Van Reenen (2004), "Evaluating the Employment Impact of a mandatory job search assistance programme", *Journal of the European*

*Economics Association,* 2(4): 569-606.

Blundell, Richard, Claire Crawford and Wenchao Jin (2014), "Employment Composition, Hourly Wages and the Productivity Puzzle", *Economic Journal*, 124 (524), 377-407

Burchardt, Tania (1999), "The Evolution of Disability Benefits in the UK: Re-weighting the basket", mimeo.

Conley, Timothy G. 1999. "GMM Estimation with Cross Sectional Dependence." *Journal of Econometrics*, 92(1): 1–45.

Crepon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot and Philippe Zamora (2013), "Do Labour Market Policies have Displacement effects? Evidence from a clustered randomized experiment", *Quarterly Journal of* Economics, 128 (2): 531-380.

David, Sarah, Laura James and Siobhan Tuohy. 2007. "Qualitative Assessment of Jobcentre Plus Delivery of Jobseekers Allowance and New Deal Interventions". DWP Research Report No. 445.

Delong, Brad and Larry Summers (2012), "Fiscal Policy in a Depressed Economy" *Brookings Papers on Economic Activity*, Spring, 233-274.

Dolton, Peter and Denise O'Neill (1996), "Unemployment Duration and the Restart Effect: Some Experimental Evidence", *Economic Journal*, 106: 387-400.

Dolton, Peter and Denise O'Neill (2002), "The Long-Run Effects of Unemployment Monitoring and Work-Search Programs: Experimental Evidence from the United Kingdom", *Journal of Labor Economics,* 20, 381-403.

Gregg, Paul, Stephen Machin and Maina Salgade (2014), "Real Wages and Unemployment in the Big Squeeze", *Economic Journal*, 124(524) 408-432

Gregg, Paul (2009), "Welfare Reform and Lone Parents in the UK", *Economic Journal*, 119:F38-F65.

Gruber, Jonathan (2011), "Public Finance and Public Policy", Worth Publishers.

Heckman, James, Carolyn J. Heinrich, and Jeffrey Smith (2002), "The Performance of Performance Standards", *Journal of Human Resources* 37: 778-811.

HM Treasury (2004) "Opportunity for all: The strength to take the long-term decisions for Britain", Pre-Budget Report, London: HMSO.

HM Treasury (2003) "The Green Book: Appraisal and evaluation in Central Government", London: HMSO.

Holmström, Bengt. 1982. "Moral Hazard in Teams." *The Bell Journal of Economics* 13: 324-340.

Holmström, Bengt and Paul Milgrom (1991), "Multitask Principal-Agent Analyses: Incentive Contracts, Asset Ownership, and Job Design", *Journal of Law, Economics, and Organization* 7(0): 24–52.

Hyde, Mark, John Dixon and Melanie Joyner (2002), "Work for Those Who Can and Security for Those Who Cannot: The United Kingdom's New Social Security Reform Agenda", *International Social Security Review,* 52(4): 69-86.

Johnson, Terry R. and Daniel H. Klepinger (1994), "Experimental Evidence on Unemployment Insurance Work-Search Policies", *Journal of Human Resources,* 29: 695-717.

Lalive, Rafael, Jan van Ours and Josef Zweimüller (2007), "How Changes in Financial Incentives Affect the Duration of Unemployment", *Review of Economic Studies,* 73: 1008-1038.

Layard, Richard (2000), "Welfare to Work and the New Deal", *Business Economist* 31, 3, 28-40.

Manning, Alan (2009), "You Can't Always get What You Want: The Impact of the UK Jobseeker's Allowance", *Labour Economics*, 16: 239-250.

Makinson, John (2000), "Incentives for Change: Rewarding Performance in National Government Networks", Public Services Productivity Panel, HMT.

Mueller, Andreas I.**,** Jesse Rothstein and Till M. von Wachter (2015), "Unemployment Insurance and Disability Insurance in the Great Recession", *Journal of Labor Economics*, forthcoming.

National Audit Office (2008), "The roll-out of the Jobcentre Plus Office network", London: HMSO.

Office for National Statistics, 2011 Census: Digitised Boundary Data (England and Wales), UK Data Service Census Support. Downloaded from: http://edina.ac.uk/census.

Pessoa, Joao and John Van Reenen (2013), "The UK Productivity and Jobs Puzzle: Does the Answer Lie in Labour Market Flexibility?" *Economic Journal*, 124, 433-452

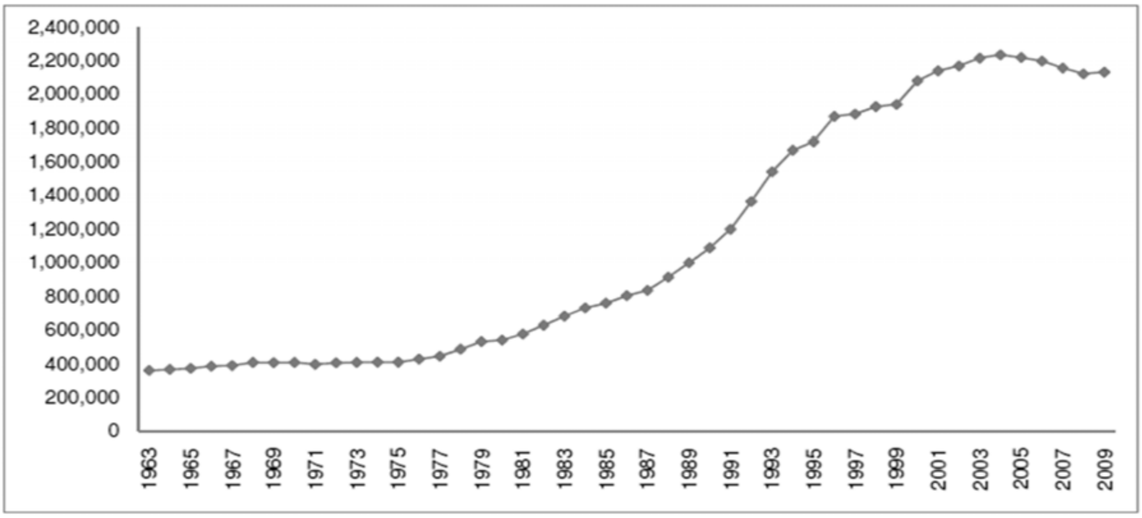
Petrongolo, Barbara (2009), "What Are the Long-Term Effects of UI? Evidence from the UK JSA Reform", *Journal of Public Economics,* 93: 1234-1253.

Propper, Carol and Deborah Wilson, (2003), "The Use and Usefulness of Performance Measures in the Public Sector", CMPO Working Paper 03/073.

Riley, Rebecca, Helen Bewley, Simon Kirby, Ana Rincon-Aznar and Anitha George (2011), *The introduction of Jobcentre Plus: An evaluation of labour market impacts*, DWP Research Report No 9781.

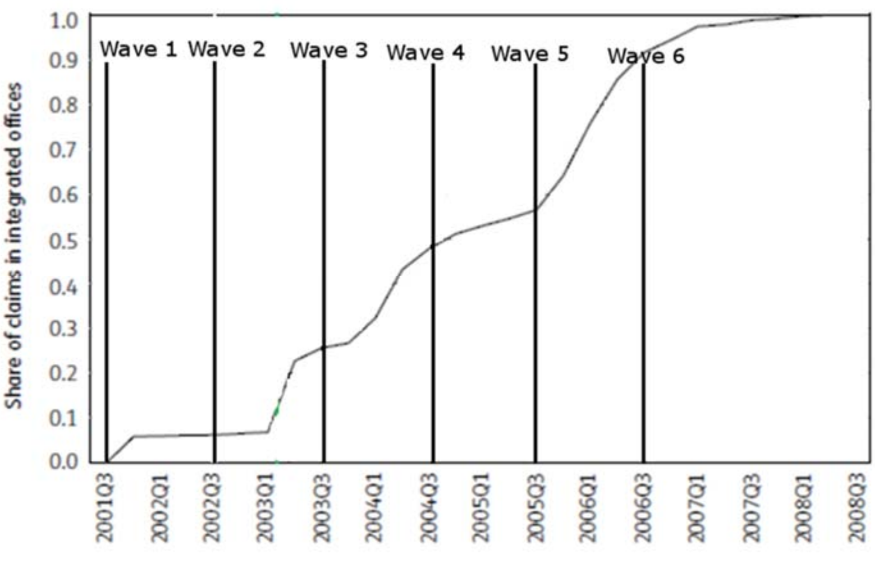
Van Reenen, John (2004) "Active Labour Market Policies and the British New Deal for Youth in Context" in Blundell, R., Card, D. and Freeman, R. *Seeking a Premier Economy*.

**Figure 1**
**Disability claimants (IB) of working age: 1963 -2009**



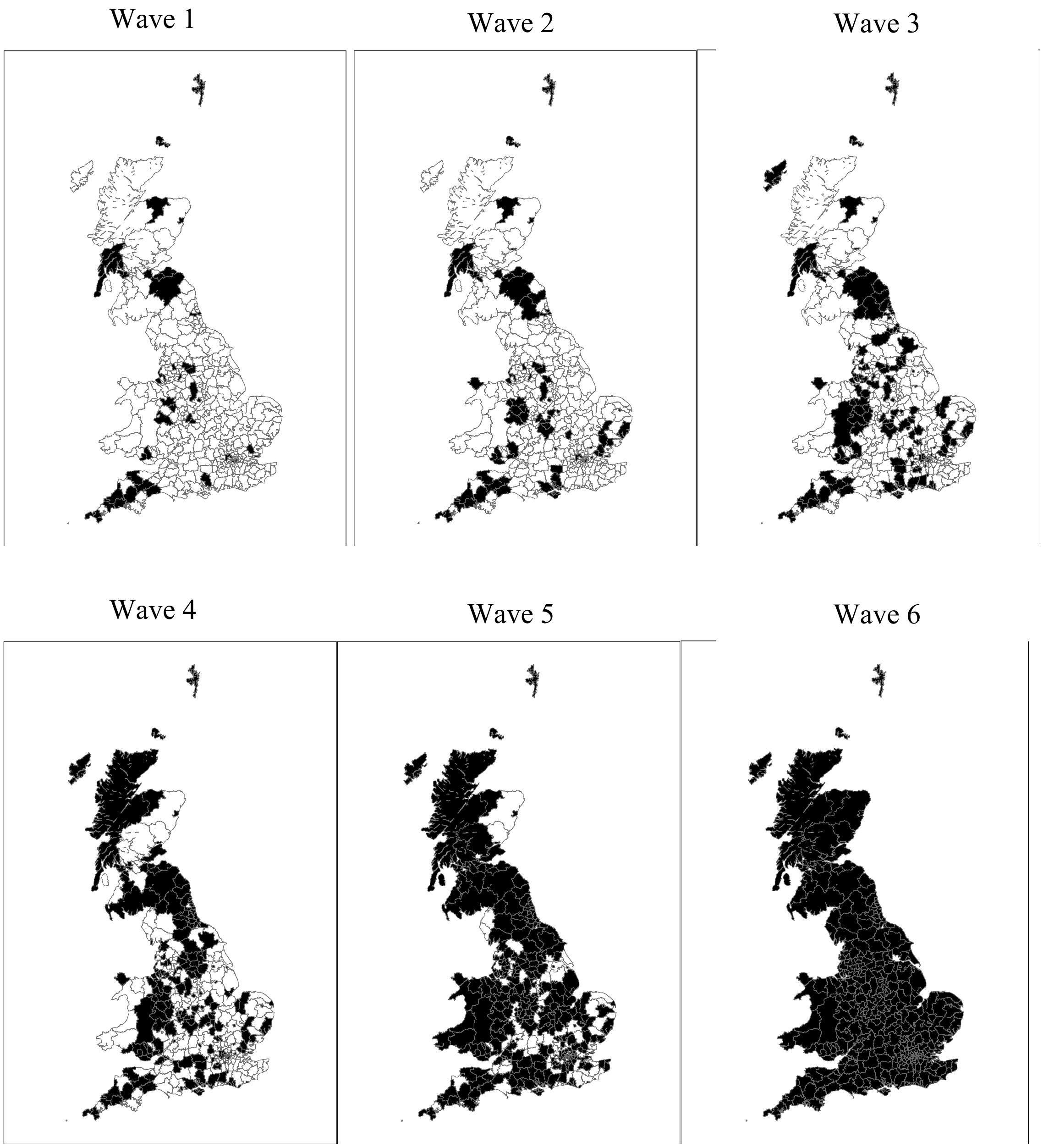

Source: Beatty and Fothergill (2009).

**Figure 2**
**The staggered roll-out of Jobcentre Plus**




Notes. The vertical lines indicate the six waves of the roll-out of the policy (at the start of each wave at least one office switched to the new regime in a district). 32 districts were treat in wave 1; 27 in wave 2; 36 in wave 3; 28 in wave 4; 135 in wave 5; 148 in wave 6. The line shows the proportion of JSA claimants who were affected by the policy (i.e. each office is weighted by the stock of JSA claimants in the quarter that the policy was turned on. Source: Riley et al. (2011)

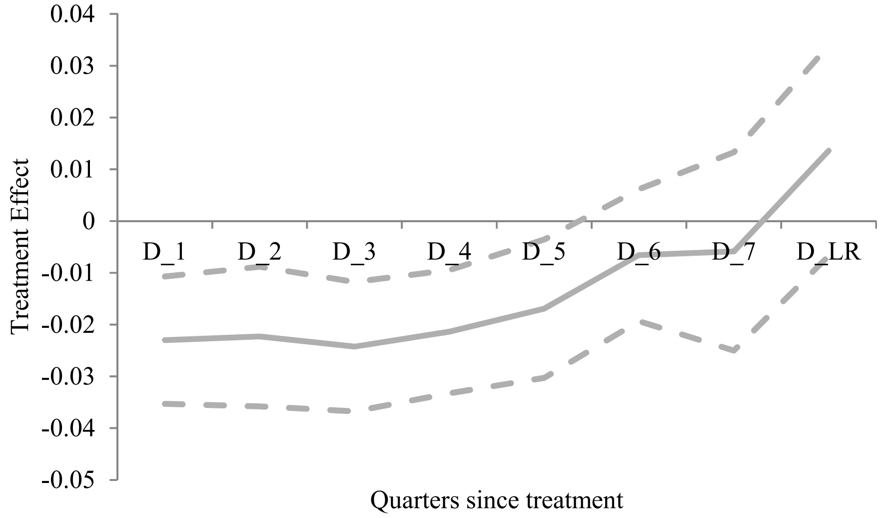
**Figure 3**
**Map of policy diffusion, by wave**



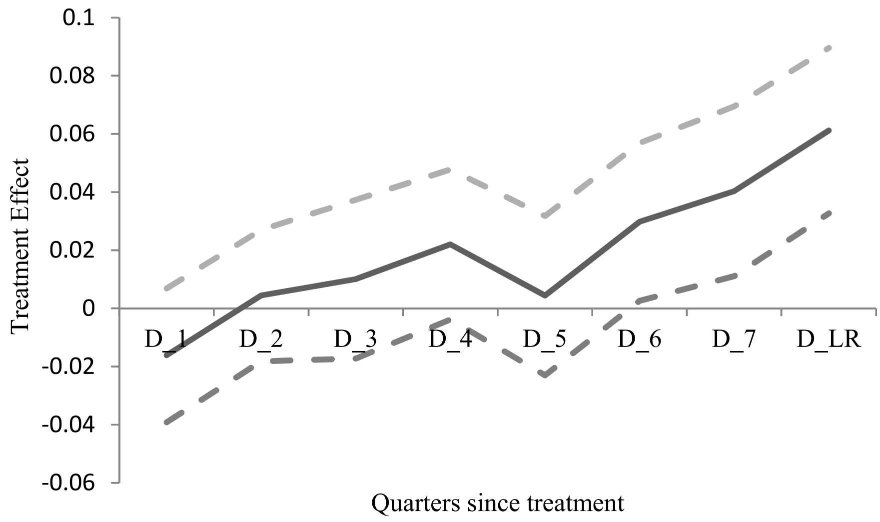

Notes. The maps show additional districts covered under each wave.

**Figure 4**

**Panel A: Dynamic effects of JCPlus on JSA outflows**



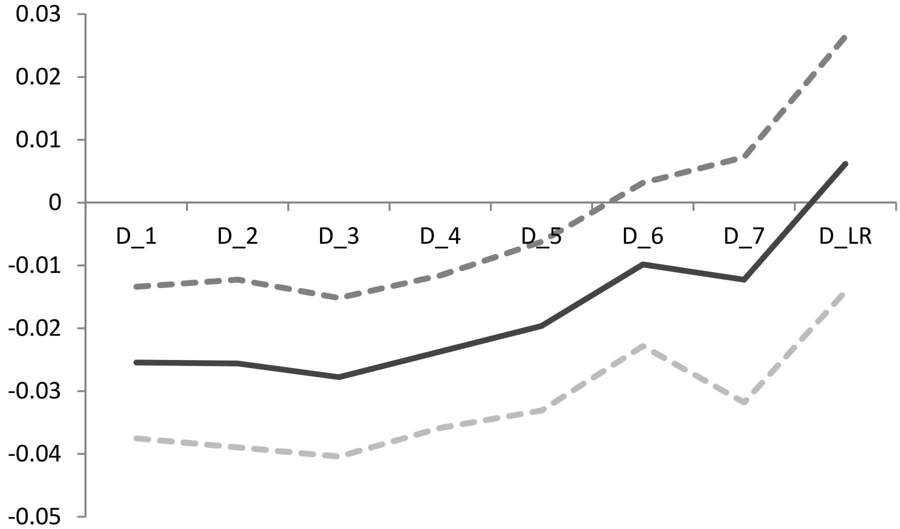

**Panel B: Dynamic effects of JCPlus on IB outflows**



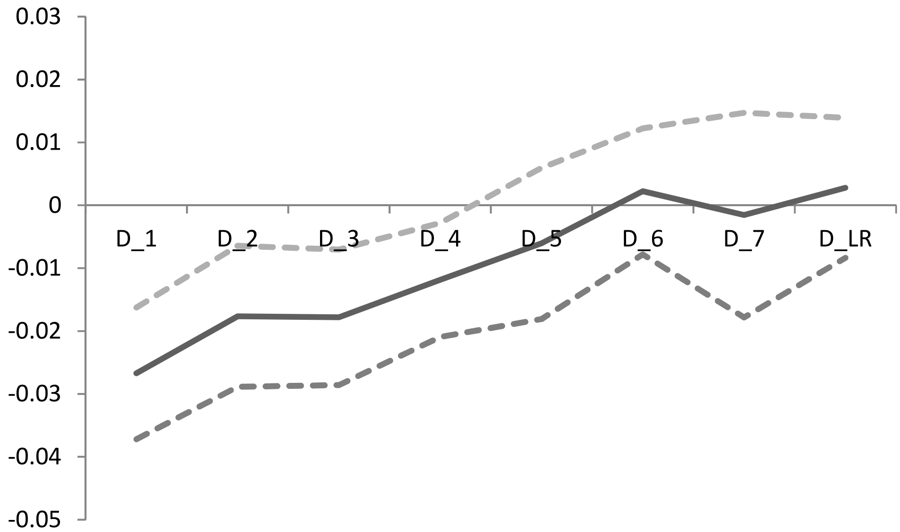

Notes. Panels A and B plot coefficients on $D_{it+\tau}$, using estimates from columns 3 and 6, respectively, of Table 3. Dashed lines represent 95% confidence intervals. See also notes to Table 3 for specifications used.

**Figure 5**
**Dynamic effects of JCPlus on JSA outflows, with and without controls for JSA inflows**

**Panel A: Without controls for JSA inflows**



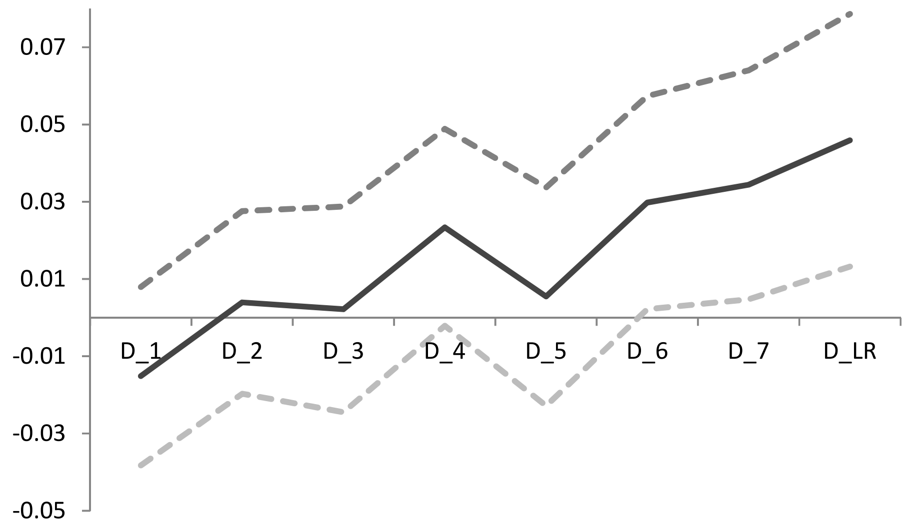

**Panel B: With controls for a 4$^{th}$ order distributed lag of JSA inflows**



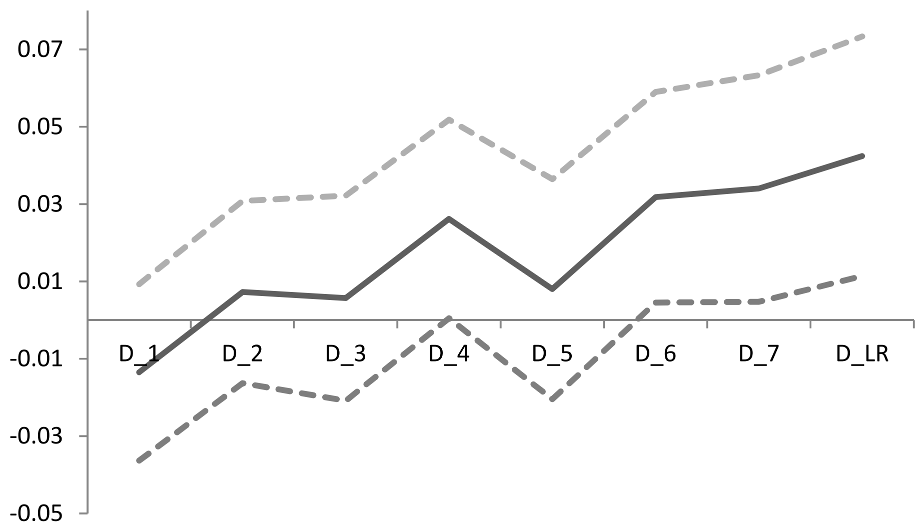

Notes. Panels A and B plot coefficients on $D_{it+\tau}$, using estimates from columns 2 and 3, respectively, of Table 6. Dashed lines represent 95% confidence intervals. See also notes to Table 6 for specifications used.

**Figure 6**
**Dynamic effects of JCPlus on IB outflows, with and without controls for IB inflows**

**Panel A: Without controls for IB inflows**

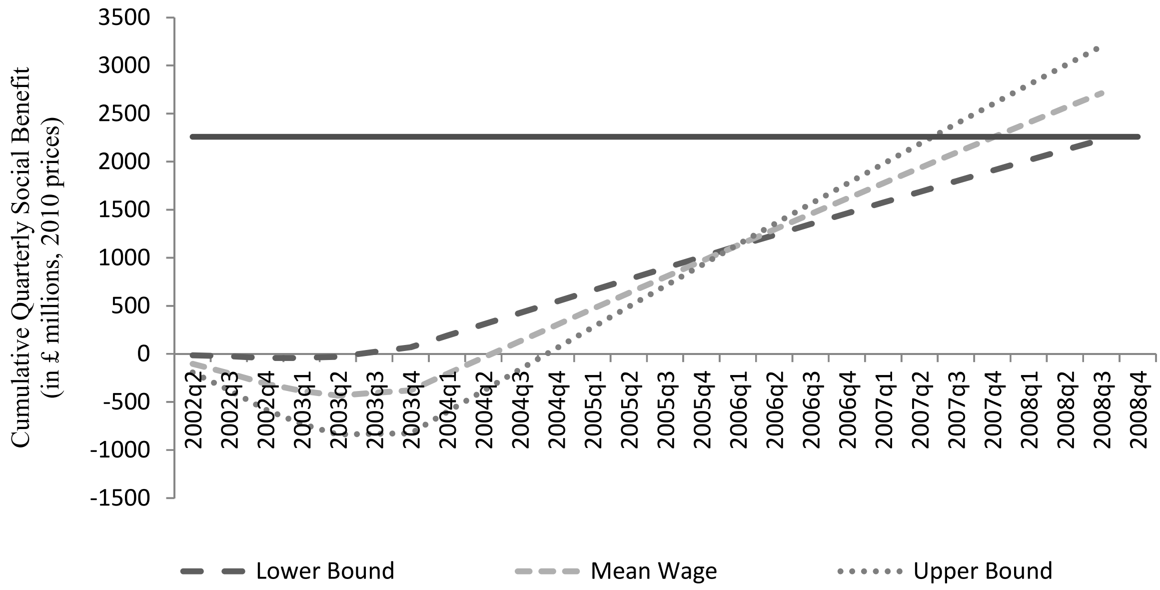



**Panel B: With controls for a 4th order distributed lag of IB inflows**



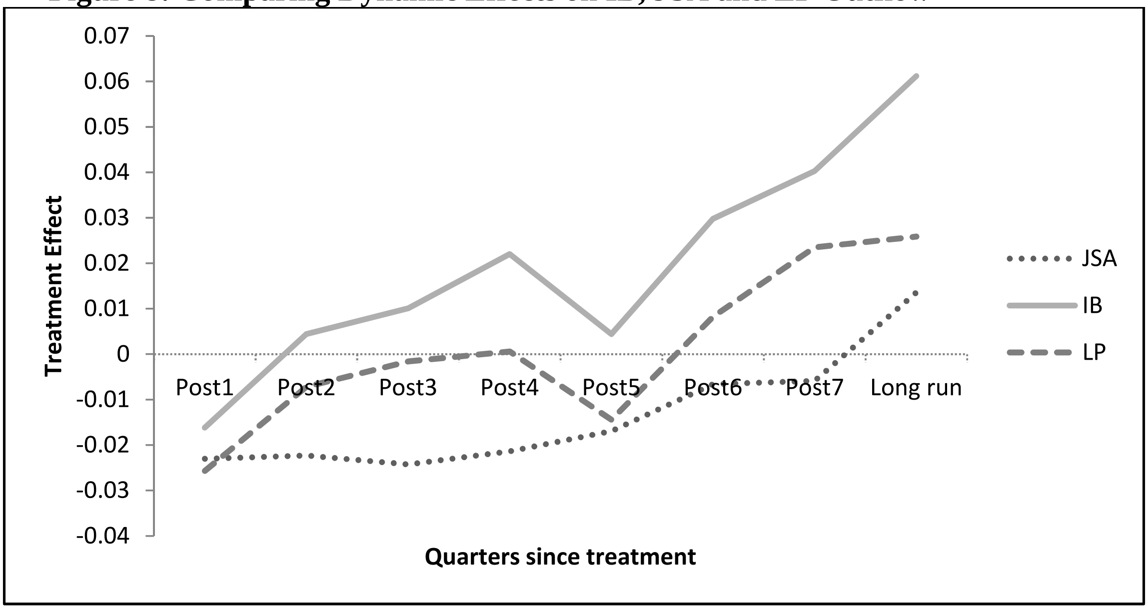

Notes. Panels A and B plot coefficients on $D_{it+\tau}$, using estimates from columns 5 and 6, respectively, of Table 6. Dashed lines represent 95% confidence intervals. See also notes to Table 6 for specifications used.

**Figure 7**
**Cost-benefit evaluation of JCPlus: With transitional dynamics**




Notes. The solid horizontal line represents set-up costs of the policy. The dashed lines represent the cumulative benefit of the policy in each quarter, based on parameter estimates reported in columns 3 and 6 of Table 3 and calculations described in the text.

**Figure 8: Comparing Dynamic Effects on IB, JSA and LP Outflow**




**Notes:** The sample is a panel of 406 districts from 1999Q3 to 2008Q2. The dependent variable is ln (outflow) for IB for the top line. The middle line represents ln (outflow) for LP and the bottom line for JSA. The three lines denote the estimated coefficients for the dynamic specification in equation (2), plotted together for comparison across the three. Standard errors are clustered at the district level.

**Table 1**
**Summary statistics**

| | Quarterly Aggregate | | Average across all district-age-quarter cells | |
|---|---|---|---|---|
| | Mean | SD | Mean | SD |
| | (1) | (2) | (3) | (4) |
| JSA Outflow | 653,819 | 78,049 | 805 | 860 |
| JSA Stock | 939,267 | 115,578 | 1,156 | 1,650 |
| JSA Inflow | 648,957 | 58,156 | 799 | 843 |
| JSA Outflow rate | 0.698 | 0.057 | 0.871 | 0.266 |
| IB Stock | 2,045,210 | 356,417 | 2,567 | 4,259 |
| IB Inflow | 148,318 | 12,125 | 181 | 241 |
| IB Outflow | 56,166 | 11,267 | 70 | 106 |
| IB Outflow rate | 0.028 | 0.0027 | 0.048 | 0.043 |

Notes. The sample period is 1999Q3-2008Q2. Columns (1) and (2) aggregate quarterly stocks and flows at the national level and then take averages over 36 quarters. Columns (3)-(5) report (unweighted) averages of the district-age-quarter cells used in our analysis. The number of cells is 29,232 (36 quarters × 406 districts × 2 age groups). JSA represents unemployment benefits; IB represents disability benefits. Stock variables are measured at the start of each quarter and flow variables are measured over the quarter.

**Table 2**
**Policy effects on outflows from unemployment (JSA) and disability benefits (IB)**

| | Dependent variable: ln(total outflow) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Benefit: | JSA | JSA | JSA | IB | IB | IB |
| $D_{it}$ | -0.0153*** | -0.0152*** | -0.0152*** | 0.0166* | 0.0158* | 0.0151* |
| | (0.0054) | (0.0055) | (0.0055) | (0.0089) | (0.0087) | (0.0087) |
| $\ln(U_{ait-1})$ | 0.7249*** | 0.6355*** | 0.6323*** | 0.2495*** | 0.1462*** | 0.3475*** |
| | (0.0095) | (0.0085) | (0.0100) | (0.0290) | (0.0195) | (0.0314) |
| $\ln(U_{a'it-1})$ | -0.0820*** | 0.0072 | 0.0105 | 0.1705*** | 0.2251*** | 0.0502* |
| | (0.0102) | (0.0086) | (0.0097) | (0.0246) | (0.0196) | (0.0256) |
| District*Age FE | | √ | √ | | √ | √ |
| Age*Time FE | | | √ | | | √ |
| Observations | 29,168 | 29,168 | 29,168 | 26,450 | 26,450 | 26,450 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is equation (1), where the dependent variable is the log of quarterly JSA outflow in columns 1-3, and the log of quarterly IB outflow in columns 4-6. $D_{it}$ takes value 1 in the post-policy period and zero otherwise. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for age, district and time fixed effects. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

**Table 3**
**Dynamic effects of policy**

| | Dependent variable: ln(total outflow) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | JSA | JSA | JSA | IB | IB | IB |
| $D_{it+1}$ | -0.0234*** | -0.0229*** | -0.0230*** | -0.0203* | -0.0154 | -0.0162 |
| | (0.0061) | (0.0062) | (0.0062) | (0.0119) | (0.0117) | (0.0117) |
| $D_{it+2}$ | -0.0230*** | -0.0230*** | -0.0230*** | 0.0052 | 0.0047 | 0.0044 |
| | (0.0068) | (0.0068) | (0.0068) | (0.0119) | (0.0115) | (0.0115) |
| $D_{it+3}$ | -0.0249*** | -0.0249*** | -0.0249*** | 0.0112 | 0.0115 | 0.0101 |
| | (0.0064) | (0.0064) | (0.0064) | (0.0140) | (0.0139) | (0.0139) |
| $D_{it+4}$ | -0.0208*** | -0.0208*** | -0.0208*** | 0.0254* | 0.0223* | 0.0220* |
| | (0.0061) | (0.0062) | (0.0062) | (0.0130) | (0.0132) | (0.0131) |
| $D_{it+5}$ | -0.0166** | -0.0166** | -0.0166** | 0.0044 | 0.0045 | 0.0044 |
| | (0.0068) | (0.0069) | (0.0069) | (0.0143) | (0.0139) | (0.0139) |
| $D_{it+6}$ | -0.0066 | -0.0067 | -0.0067 | 0.0370** | 0.0309** | 0.0298** |
| | (0.0065) | (0.0066) | (0.0066) | (0.0144) | (0.0138) | (0.0138) |
| $D_{it+7}$ | -0.0077 | -0.0076 | -0.0076 | 0.0430*** | 0.0415*** | 0.0403*** |
| | (0.0098) | (0.0099) | (0.0099) | (0.0150) | (0.0150) | (0.0149) |
| $D_{iLR}$ | 0.0117 | 0.0117 | 0.0117 | 0.0646*** | 0.0622*** | 0.0612*** |
| | (0.0104) | (0.0105) | (0.0105) | (0.0150) | (0.0144) | (0.0145) |
| $\ln(U_{ait-1})$ | 0.7237*** | 0.6344*** | 0.6312*** | 0.2589*** | 0.1552*** | 0.3572*** |
| | (0.0095) | (0.0084) | (0.0100) | (0.0288) | (0.0189) | (0.0308) |
| $\ln(U_{a'it-1})$ | -0.0832*** | 0.0060 | 0.0092 | 0.1808*** | 0.2351*** | 0.0595** |
| | (0.0101) | (0.0085) | (0.0096) | (0.0239) | (0.0191) | (0.0255) |
| District*Age FE | | √ | √ | | √ | √ |
| Age*Time FE | | | √ | | | √ |
| Observations | 29,168 | 29,168 | 29,168 | 26,450 | 26,450 | 26,450 |
| F-Test | 4.5560 | 4.4980 | 4.4920 | 4.6310 | 4.3260 | 4.2410 |
| p-value | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0001 | 0.0001 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is equation (2), where the dependent variable is the log of quarterly JSA outflow in columns 1-3, and the log of quarterly IB outflow in columns 4-6. $D_{it+\tau}$ indicates treatment $\tau$ quarters after the policy is introduced. $D_{iLR}$ indicates treatment 8+ quarters after the policy is introduced. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for district, time and age fixed effects. The last row contains the p-value of the F-test for the joint significance of the post-treatment dummies. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

**Table 4**
**Test for pre-treatment trends in benefit outflows**

| | Dependent variable: ln(total outflow) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | JSA | JSA | IB | IB |
| $D_{it-4}$ | | 0.0013 | | 0.0162 |
| | | (0.0065) | | (0.0109) |
| $D_{it-3}$ | | 0.0039 | | -0.0135 |
| | | (0.0063) | | (0.0114) |
| $D_{it-2}$ | | -0.0021 | | 0.0053 |
| | | (0.0068) | | (0.0119) |
| $D_{it-1}$ | | -0.0134$^{*}$ | | -0.0010 |
| | | (0.0079) | | (0.0123) |
| $D_{it}$ | -0.0152$^{***}$ | -0.0168$^{**}$ | 0.0151$^{*}$ | 0.0160 |
| | (0.0055) | (0.0069) | (0.0087) | (0.0100) |
| $\ln(U_{ait-1})$ | 0.6323$^{***}$ | 0.6323$^{***}$ | 0.3475$^{***}$ | 0.3473$^{***}$ |
| | (0.0100) | (0.0100) | (0.0314) | (0.0314) |
| $\ln(U_{a'it-1})$ | 0.0105 | 0.0105 | 0.0502$^{*}$ | 0.0502$^{*}$ |
| | (0.0097) | (0.0097) | (0.0256) | (0.0256) |
| Observation | 29,168 | 29,168 | 26,450 | 26,450 |
| F Test | | 1.8830 | | 1.3560 |
| p-value | | 0.1130 | | 0.2480 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is equation (3), where the dependent variable is the log of quarterly JSA outflow in columns 1 and 2, and the log of quarterly IB outflow in columns 3 and 4. $D_{it-\tau}$ indicates treatment $\tau$ quarters before the policy was introduced. $D_{it}$ takes value 1 in the post-policy period and zero otherwise. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for district, time and age fixed effects as well as age*district and age*time interactions. The last row contains the p-value of the F-test for the joint significance of the pre-treatment dummies. Standard errors are clustered at the district level. $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1.

**Table 5**
**Policy effects on inflows into JSA and IB**

| | Dependent variable: ln(total inflow) | | | |
|---|---|---|---|---|
| | (1) JSA | (2) JSA | (3) IB | (4) IB |
| $D_{it}$ | -0.0063 | | -0.0054 | |
| | (0.0085) | | (0.0072) | |
| $D_{it+1}$ | | -0.0155$^{*}$ | | -0.0175$^{**}$ |
| | | (0.0081) | | (0.0080) |
| $D_{it+2}$ | | -0.0327$^{***}$ | | -0.0179$^{*}$ |
| | | (0.0081) | | (0.0094) |
| $D_{it+3}$ | | -0.0170$^{**}$ | | -0.0068 |
| | | (0.0086) | | (0.0088) |
| $D_{it+4}$ | | -0.0267$^{***}$ | | -0.0064 |
| | | (0.0086) | | (0.0093) |
| $D_{iLR}$ | | 0.0164 | | 0.0043 |
| | | (0.0129) | | (0.0094) |
| $\ln(population_{ait})$ | 0.1441 | 0.1340 | -0.0072 | -0.0134 |
| | (0.1278) | (0.1254) | (0.0610) | (0.0609) |
| Observations | 29,096 | 29,096 | 26,727 | 26,727 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is equation (4), where the dependent variable is the log of quarterly JSA inflow in columns 1 and 2, and the log of quarterly IB inflow in columns 3 and 4. $D_{it}$ takes value 1 in the post-policy period and zero otherwise. $D_{it+\tau}$ indicates treatment $\tau$ quarters after the policy is introduced. $D_{iLR}$ indicates treatment 8+ quarters after the policy is introduced. $population_{ait}$ denotes the mid-year population estimate by age and district. All regressions control for district, time and age fixed effects as well as age*district and age*time interactions. Standard errors are clustered at the district level. $^{***}$ $p<0.01$, $^{**}$ $p<0.05$, $^{*}$ $p<0.1$.

**Table 6**
**Dynamic effects of policy – Controlling for inflows**

| | Dependent variable: ln(total outflow) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | JSA | JSA | JSA | IB | IB | IB |
| $D_{it+1}$ | -0.0235*** | -0.0267*** | -0.0254*** | -0.0156 | -0.0174 | -0.0191 |
| | (0.0054) | (0.0053) | (0.0061) | (0.0118) | (0.0118) | (0.0119) |
| $D_{it+2}$ | -0.0149*** | -0.0176*** | -0.0256*** | 0.006 | 0.0048 | 0.0014 |
| | (0.0057) | (0.0057) | (0.0068) | (0.0117) | (0.0119) | (0.0119) |
| $D_{it+3}$ | -0.0145*** | -0.0178*** | -0.0278*** | 0.0089 | 0.0074 | 0.0039 |
| | (0.0056) | (0.0055) | (0.0064) | (0.0140) | (0.0139) | (0.0139) |
| $D_{it+4}$ | -0.0131*** | -0.0119** | -0.0237*** | 0.0291** | 0.0283** | 0.0255* |
| | (0.0049) | (0.0046) | (0.0062) | (0.0133) | (0.0135) | (0.0134) |
| $D_{it+5}$ | -0.0073 | -0.006 | -0.0196*** | 0.0075 | 0.0062 | 0.0036 |
| | (0.0059) | (0.0061) | (0.0068) | (0.0141) | (0.0142) | (0.0141) |
| $D_{it+6}$ | -0.0021 | 0.0022 | -0.0098 | 0.0334** | 0.0312** | 0.0293** |
| | (0.0053) | (0.0051) | (0.0066) | (0.0138) | (0.0139) | (0.0140) |
| $D_{it+7}$ | -0.0018 | -0.0015 | -0.0123 | 0.0401*** | 0.0362** | 0.0363** |
| | (0.0082) | (0.0083) | (0.0099) | (0.0148) | (0.0150) | (0.0152) |
| $D_{iLR}$ | 0.0064 | 0.0028 | 0.0062 | 0.0589*** | 0.0503*** | 0.0547*** |
| | (0.0068) | (0.0057) | (0.0103) | (0.0144) | (0.0141) | (0.0145) |
| $\ln(Inflow)_{it-1}$ | 0.4252*** | 0.3765*** | | (0.0244) | (0.0286) | (0.0285) |
| | (0.0200) | (0.0147) | | 0.0863*** | 0.0764*** | |
| $\ln(Inflow)_{it-2}$ | | 0.0773*** | | (0.0133) | (0.0132) | |
| | | (0.0109) | | | 0.0544*** | |
| $\ln(Inflow)_{it-3}$ | | 0.0457*** | | | (0.0130) | |
| | | (0.0137) | | | 0.0344*** | |
| $\ln(Inflow)_{it-4}$ | | 0.0827*** | | | (0.0119) | |
| | | (0.0108) | | | 0.0359*** | |
| $\ln(U_{ait-1})$ | 0.3679*** | 0.3323*** | 0.6350*** | | (0.0121) | |
| | (0.0149) | (0.0117) | (0.0105) | 0.3312*** | 0.2775*** | 0.3761*** |
| $\ln(U_{a'it-1})$ | 0.0069 | -0.0109* | 0.0033 | (0.0307) | (0.0344) | (0.0328) |
| | (0.0069) | (0.0059) | (0.0092) | 0.0493** | 0.0231 | 0.0346 |
| Observations | 28,352 | 25,915 | 25,915 | 24,402 | 22,304 | 22,1304 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is equation (2), having added controls for lagged benefit inflows. The dependent variable is the log of quarterly JSA inflow in columns 1-3, and the log of quarterly IB inflow in columns 4-6. $D_{it+\tau}$ indicates treatment $\tau$ quarters after the policy is introduced. $D_{iLR}$ indicates treatment 8+ quarters after the policy is introduced. . $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for age, district and time fixed effects as well as age*district and age*time interactions. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

**Table 7**
**Policy effects on JSA outflows into work and other destinations**

| | Dependent variable: ln(JSA outflow) by destination | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | To work | To work | Not to work | Not to work |
| $D_{it}$ | -0.0244*** | | -0.0169*** | |
| | (0.0065) | | (0.0065) | |
| $D_{it+1}$ | | -0.0320*** | | -0.0402*** |
| | | (0.0074) | | (0.0118) |
| $D_{it+2}$ | | -0.0163** | | -0.0386*** |
| | | (0.0078) | | (0.0125) |
| $D_{it+3}$ | | -0.0375*** | | -0.0355*** |
| | | (0.0090) | | (0.0120) |
| $D_{it+4}$ | | -0.0218** | | -0.0403*** |
| | | (0.0090) | | (0.0119) |
| $D_{it+5}$ | | -0.0351*** | | -0.0253** |
| | | (0.0107) | | (0.0125) |
| $D_{it+6}$ | | -0.0139 | | -0.0238* |
| | | (0.0095) | | (0.0130) |
| $D_{it+7}$ | | -0.0187* | | -0.0122 |
| | | (0.0107) | | (0.0154) |
| $D_{iLR}$ | | -0.0082 | | 0.0084 |
| | | (0.0107) | | (0.0145) |
| $\ln(U_{ait-1})$ | 0.6213*** | 0.6262*** | 0.6305*** | 0.5488*** |
| | (0.0156) | (0.0157) | (0.0121) | (0.0215) |
| $\ln(U_{a'it-1})$ | -0.0313** | -0.0278** | 0.0240** | 0.0319* |
| | (0.0131) | (0.0129) | (0.0110) | (0.0193) |
| Observations | 28,019 | 28,019 | 28,075 | 28,075 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. Columns 1 and 3 are based on specification (1) and columns 2 and 4 are based on specification (2). The dependent variable is the JSA outflow into work in columns 1 and 2; and JSA outflow into other states in columns 3 and 4 (gone abroad, other benefits, education, government training schemes, crime, deceased, other). $D_{it}$ takes value 1 in the post-policy period and zero otherwise. $D_{it+\tau}$ indicates treatment $\tau$ quarters after the policy is introduced. $D_{iLR}$ indicates treatment 8+ quarters after the policy is introduced. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for age, district and time fixed effects as well as age*district and age*time interactions. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

**Table 8**
**Cost-benefit analysis of JCPlus: Long-run evaluation**

| | Re-employment earnings | | |
|---|---|---|---|
| | (1)<br>Lower bound<br>(min wage) | (2)<br>Mean re-employment earnings for IB exits | (3)<br>Upper bound<br>(median wage) |
| 1. Administration cost in old regime (2000 prices) (£m) | 3552 | 3552 | 3552 |
| 2. Administration cost in new regime (2000 prices) (£m) | 3314 | 3314 | 3314 |
| 3. Annual saving in administrative costs (£m) | 238 | 238 | 238 |
| 4. Weekly earnings | 122.00 | 250.0 | 360.0 |
| 5. Increase in GDP from wage income (£m) | 472.11 | 967.44 | 1,393.11 |
| 6. Deadweight gain (40% net exchequer saving) (£m) | 113.68 | 204.34 | 121.79 |
| 7. Annual social benefit (2000 prices) (£m) | 823.79 | 1,409.77 | 1,752.90 |
| 8. Annual social benefit (2010 prices) (£m) | 1,013.14 | 1,733.82 | 2,155.82 |
| 9. PDV of social benefit (£m) | 28,946.93 | 49,537.77 | 61,594.92 |
| 10. Total Setup Cost (2010 prices) (£m) | 2,259.61 | 2,259.61 | 2,259.61 |
| 11. Net benefit (£m) | 26,687.32 | 47,278.15 | 59,335.31 |

Notes. Figures reported represent the long-run cost-benefit analysis of a policy introduced nationally (i.e. transitional dynamics is ignored). The administrative costs are from NAO (2008). The benefits are based on $D_{iLR}$ parameter estimates reported in column 6 of Table 3, and calculations described in the text.

**Table 9**
**Treatment effects by wave**

| | *Wave 1* | *Wave 2* | *Wave 3* | *Wave 4* | *Wave 5* | *Wave 6* | *All waves* |
|---|---|---|---|---|---|---|---|
| | | | Dependent variable: ln(total outflow) from JSA | | | | |
| $D_{it}$ | -0.0196 | -0.0162$^{*}$ | -0.0147$^{*}$ | -0.0121 | -0.0103 | -0.0107$^{*}$ | -0.0152$^{***}$ |
| | (0.0135) | (0.0093) | (0.0078) | (0.0080) | (0.0073) | (0.0062) | (0.0055) |
| $\ln(U_{ait-1})$ | 0.3253$^{***}$ | 0.3407$^{***}$ | 0.3602$^{***}$ | 0.3835$^{***}$ | 0.3916$^{***}$ | 0.3949$^{***}$ | 0.6323$^{***}$ |
| | (0.0182) | (0.0188) | (0.0160) | (0.0158) | (0.0156) | (0.0149) | (0.0100) |
| $\ln(U_{a'it-1})$ | 0.3373$^{***}$ | 0.2825$^{***}$ | 0.2594$^{***}$ | 0.2272$^{***}$ | 0.2248$^{***}$ | 0.2219$^{***}$ | 0.0105 |
| | (0.0195) | (0.0187) | (0.0180) | (0.0210) | (0.0224) | (0.0226) | (0.0097) |
| Observations | 9,727 | 12,967 | 16,207 | 19,448 | 22,688 | 25,928 | 29,168 |
| | | | Dependent variable: ln(total outflow) from IB | | | | |
| $D_{it}$ | 0.0114 | 0.0319 | 0.0315$^{**}$ | 0.0295$^{**}$ | 0.0254$^{**}$ | 0.0171$^{*}$ | 0.0151$^{*}$ |
| | (0.0279) | (0.0202) | (0.0159) | (0.0124) | (0.0108) | (0.0096) | (0.0087) |
| $\ln(U_{ait-1})$ | 0.2221$^{***}$ | 0.1511$^{***}$ | 0.1800$^{***}$ | 0.1624$^{***}$ | 0.1534$^{***}$ | 0.1626$^{***}$ | 0.3475$^{***}$ |
| | (0.0493) | (0.0407) | (0.0355) | (0.0320) | (0.0308) | (0.0284) | (0.0314) |
| $\ln(U_{a'it-1})$ | 1.1741$^{***}$ | 0.7325$^{***}$ | 0.3921$^{***}$ | 0.4031$^{***}$ | 0.4081$^{***}$ | 0.3202$^{***}$ | 0.0502$^{*}$ |
| | (0.2219) | -0.1647 | (0.1307) | (0.1118) | (0.0980) | (0.0836) | (0.0256) |
| Observations | 7,635 | 10,435 | 13,256 | 16,070 | 18,844 | 21,637 | 26,450 |

The specification estimated is equation (1), where the dependent variable is the log of quarterly JSA and IB outflows in the top and bottom panels, respectively. $D_{it}$ takes the value 1 in the post-policy period and zero otherwise. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for age, district and time fixed effects, as well as district*age and age*time interactions. Standard errors are clustered at the district level. $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1

**Table 10**
**Policy effects on welfare outflows for Lone Parents (LP)**

| | Dependent variable: ln(total outflow) | | | | |
|---|---|---|---|---|---|
| | (1) LP | (2) LP | (3) LP | (4) LP | (5) LP |
| $D_{it-4}$ | | -0.0120 | 0.0014 | | |
| | | (0.0112) | (0.0111) | | |
| $D_{it-3}$ | | -0.0302*** | -0.0166 | | |
| | | (0.0103) | (0.0106) | | |
| $D_{it-2}$ | | -0.0140 | -0.0003 | | |
| | | (0.0119) | (0.0117) | | |
| $D_{it-1}$ | | -0.0401*** | -0.0255** | | |
| | | (0.0119) | (0.0121) | | |
| $D_{it}$ | -0.0131* | -0.0270*** | -0.0159* | -0.0096 | |
| | (0.0067) | (0.0087) | (0.0095) | (0.0073) | |
| $D_{it+1}$ | | | | | -0.0265** |
| | | | | | (0.0110) |
| $D_{it+2}$ | | | | | -0.0067 |
| | | | | | (0.0116) |
| $D_{it+3}$ | | | | | -0.0016 |
| | | | | | (0.0107) |
| $D_{it+4}$ | | | | | -0.0020 |
| | | | | | (0.0118) |
| $D_{it+5}$ | | | | | -0.0166 |
| | | | | | (0.0131) |
| $D_{it+6}$ | | | | | 0.0083 |
| | | | | | (0.0115) |
| $D_{it+7}$ | | | | | 0.0211 |
| | | | | | (0.0143) |
| $D_{iLR}$ | | | | | 0.0247** |
| | | | | | (0.0122) |
| $\ln(U_{ait-1})$ | 0.4819*** | 0.4845*** | 0.5529*** | 0.5523*** | 0.5535*** |
| | (0.0634) | (0.0632) | (0.0895) | (0.0895) | (0.0898) |
| $\ln(U_{a'it-1})$ | -0.1921*** | -0.1907*** | -0.1304* | -0.1307* | -0.1302 |
| | (0.0553) | (0.0550) | (0.0790) | (0.0790) | (0.0793) |
| District trends | | | √ | √ | √ |
| Observations | 26,378 | 26,378 | 26,378 | 26,378 | 26,378 |
| F-Test | | 3.894 | 1.757 | | |
| P-value | | 0.0041 | 0.137 | | |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The dependent variable is the log of quarterly LP outflow. $D_{it}$ takes the value 1 in the post-policy period and zero otherwise. $D_{it-\tau}$ and $D_{it+\tau}$ indicate, respectively, treatment $\tau$ quarters before and $\tau$ quarters after the policy is introduced. $D_{iLR}$ indicates treatment 8+ quarters after the policy is introduced. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for age, district and time fixed effects, as well as age*district and age*time interactions. The last row gives the p-value of the F-test for the joint significance of the pre-treatment dummies. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

**Table 11**
**Spillover effects of JCPlus into neighbouring districts**

| | Dependent variable: ln(total outflow) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | JSA | JSA | IB | IB |
| $D_{it}$ | -0.0113 | -0.0123$^{*}$ | 0.0323$^{***}$ | 0.0311$^{***}$ |
| | (0.0074) | (0.0073) | (0.0106) | (0.0106) |
| $NBR_{it}$ | | 0.0150 | | 0.0146 |
| | | (0.0092) | | (0.0126) |
| $\ln(U_{ait-1})$ | 0.6262$^{***}$ | 0.6242$^{***}$ | 0.3514$^{***}$ | 0.3522$^{***}$ |
| | (0.0108) | (0.0110) | (0.0383) | (0.0383) |
| $\ln(U_{a'it-1})$ | 0.0099 | 0.0079 | 0.0673$^{**}$ | 0.0683$^{**}$ |
| | (0.0101) | (0.0102) | (0.0297) | (0.0297) |
| Observations | 22,688 | 22,688 | 20,374 | 20,374 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2006Q2. The estimated specification is equation (5). The dependent variable is the log of quarterly JSA outflow in columns 1 and 2, and the log of quarterly IB outflow in columns 3 and 4. $D_{it}$ takes the value 1 in the post-policy period and zero otherwise. $NBR_{it}$ takes the value 1 when a district whose centroid is within 10 km of district $i$'s centroid is treated and zero otherwise. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for age, district and time fixed effects, as well as age*district and age*time interactions. $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1

**Table 12**
**The impact of Job Outcome Targets and team incentives**

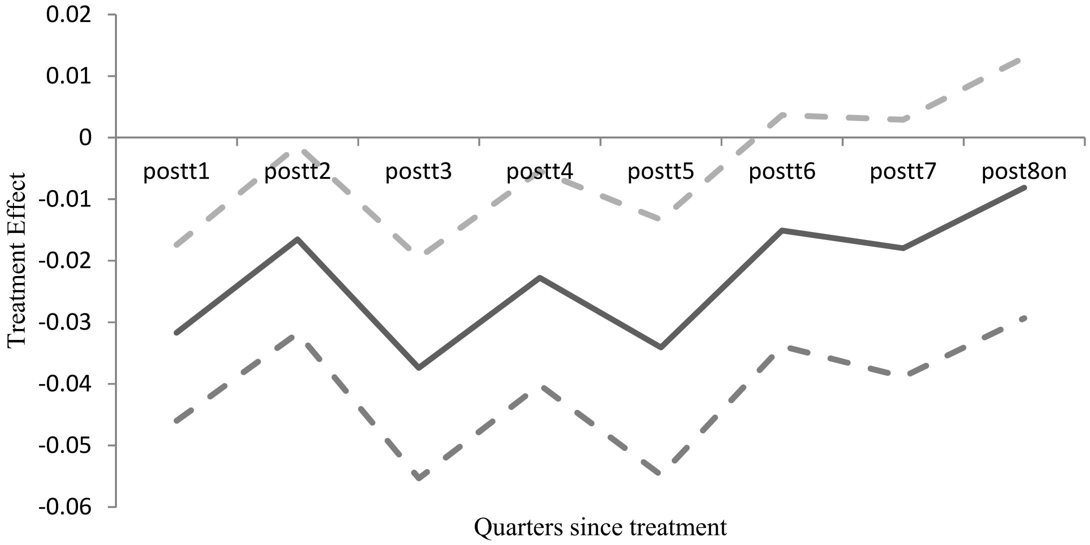
| | Dependent variable: ln(total outflow) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | JSA | IB | JSA | IB |
| $D_{it}$ | -0.0118* | 0.0161 | -0.0103 | 0.0060 |
| | (0.0064) | (0.0098) | (0.0067) | (0.0355) |
| $D_{it} * D_{it}^{JOT}$ | 0.0258** | -0.0104 | -0.0436* | 0.0444 |
| | (0.0103) | (0.0132) | (0.0248) | (0.0300) |
| $D_{it} * \ln(S_i)$ | | | -0.0004 | 0.0019 |
| | | | (0.0029) | (0.0067) |
| $D_{it}^{JOT} * \ln(S_i)$ | | | 0.0031 | 0.0037 |
| | | | (0.0029) | (0.0035) |
| $D_{it} * D_{it}^{JOT} * \ln(S_i)$ | | | 0.0145*** | -0.0115** |
| | | | (0.0052) | (0.0057) |
| Observations | 22,109 | 20,416 | 22,109 | 20,416 |
| p-value | 0.0061 | 0.118 | 0.429 | 0.603 |

Notes. The sample is a panel of 338 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2007Q4. The dependent variable is the log of quarterly JSA outflow in columns 1 and 3, and the log of quarterly IB outflow in columns 2 and 4. $D_{it}$ takes the value 1 in the post-JCPlus period and zero otherwise. $D_{it}^{JOT}$ takes the value 1 in the post-JOT period and zero otherwise. The size variable $S_i$ is the total number of employees in a district. All regressions control for the log of the stock of individuals on same benefits in the same age group at the beginning of the quarter ($\ln(U_{ait-1})$), the corresponding stock for the other age group ($\ln(U_{a'it-1})$), age, district and time fixed effects, as well as age*district and age*time interactions. The last row reports the p-value of the F test for the equality between the effect of JCPlus and total effect of JOT. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

**Online Appendices: Not intended for publication**

## A. Appendix Tables and Figures

**Figure A1: Dynamic Effects on JSA Outflow to Work**




Notes. The figure plot coefficients on $D_{it+\tau}$, using estimates from column 2 of Table 67 Dashed lines represent 95% confidence intervals. See also notes to Table 7 for the specification used.

**Table A1**
**Job Entry Target points (2002-03)**

| Client Group | Points Awarded |
|---|---|
| Disabled people and inactive benefits (IB) | 12 |
| Lone parents (LP) | 12 |
| New Deal 50+, 25+, Young People | 8 |
| Other long term JSA | 8 |
| Short term unemployed JSA | 4 |
| Employed job-entries | 1 |
| Area-based points | 1 |

Notes. The second column lists the number of points awarded to a benefit officer for placing a claimant from the corresponding benefit category into work.

**Table A2**
**Policy effects on the JSA outflows from offices**

| | Dependent variable: ln(total outflow) | |
|---|---|---|
| | (1) | (2) |
| $D_{it-4}$ | | -0.0025 |
| | | (0.0070) |
| $D_{it-3}$ | | -0.0134* |
| | | (0.0079) |
| $D_{it-2}$ | | -0.0182** |
| | | (0.0086) |
| $D_{it-1}$ | | -0.0550*** |
| | | (0.0099) |
| $D_{it}$ | -0.0545*** | -0.0710*** |
| | (0.0056) | (0.0117) |
| $lnU_{ait-1}$ | 0.7410*** | 0.7413*** |
| | (0.0144) | (0.0155) |
| $lnU_{a'it-1}$ | 0.1418*** | 0.1421*** |
| | (0.0167) | (0.0227) |
| Observations | 48,351 | 48,351 |
| F Test | | 17.8300 |
| p-value | | 0.0000 |

Notes. The sample is a panel of 695 offices and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is equation (2), where the dependent variable is the log of quarterly JSA outflow. $D_{it-\tau}$ indicates treatment $\tau$ quarters before the policy is introduced. $D_{it}$ takes the value 1 in the post-policy period and zero otherwise. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for district, time and age fixed effects as well as age*district and age*time interactions. The last row contains the p-value of the F-test for the joint significance of the pre-treatment dummies. Standard errors are clustered at the district level. $^{***}$ p<0.01, $^{**}$ p<0.05, $^{*}$ p<0.1.

**Table A3**
**Alternative dynamics structure of policy effects**

| | Dependent variable: ln(total outflow) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | JSA | JSA | JSA | IB | IB | IB |
| $D_{i1}$ | -0.0274*** | -0.0260*** | -0.0238*** | -0.0209* | -0.0190* | -0.0144 |
| | (0.0065) | (0.0064) | (0.0063) | (0.0114) | (0.0114) | (0.0115) |
| $D_{i2}$ | -0.0282*** | -0.0265*** | -0.0240*** | -0.0041 | -0.0019 | 0.0033 |
| | (0.0072) | (0.0071) | (0.0069) | (0.0115) | (0.0115) | (0.0115) |
| $D_{i3}$ | -0.0308*** | -0.0289*** | -0.0261*** | -0.0027 | -0.0002 | 0.0058 |
| | (0.0067) | (0.0066) | (0.0065) | (0.0131) | (0.0132) | (0.0133) |
| $D_{i4}$ | | -0.0255*** | -0.0222*** | | 0.0079 | 0.0147 |
| | | (0.0065) | (0.0063) | | (0.0121) | (0.0123) |
| $D_{i5}$ | | | -0.0182*** | | | 0.0011 |
| | | | (0.0068) | | | (0.0138) |
| $D_{i6}$ | | | -0.0085 | | | 0.0286** |
| | | | (0.0065) | | | (0.0134) |
| $D_{4LR}$ | -0.0044 | | | 0.0353*** | | |
| | (0.0066) | | | (0.0105) | | |
| $D_{5LR}$ | | 0.0001 | | | 0.0413*** | |
| | | (0.0073) | | | (0.0113) | |
| $D_{7LR}$ | | | 0.0078 | | | 0.0576*** |
| | | | (0.0089) | | | (0.0127) |
| $\ln(U_{ait-1})$ | 0.6474*** | 0.6473*** | 0.6469*** | 0.1511*** | 0.1521*** | 0.1547*** |
| | (0.0084) | (0.0084) | (0.0084) | (0.0180) | (0.0179) | (0.0177) |
| $\ln(U_{a'it-1})$ | -0.0013 | -0.0015 | -0.0019 | 0.2198*** | 0.2209*** | 0.2239*** |
| | (0.0085) | (0.0085) | (0.0085) | (0.0184) | (0.0184) | (0.0183) |
| Observations | 30,788 | 30,788 | 30,788 | 28,074 | 28,074 | 28,074 |
| P-value | 0.0000 | 0.0000 | 0.0000 | 0.0003 | 0.0003 | 0.0000 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2008Q2. The specification estimated is (the analogue of) equation (2), where the dependent variable is the log of quarterly JSA outflow in columns 1-3, and the log of quarterly IB outflow in columns 4-6. $D_{it+\tau}$ indicates treatment $\tau$ quarters after the policy is introduced. $D_{\tau LR}$ indicates treatment $\tau$+ quarters after the policy is introduced. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for district, time and age fixed effects as well as age*district and age*time interactions. The last row contains the p-value of the F-test for the joint significance of the post-treatment dummies. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

**Table A4**
**Outflow regressions weighted by district-level benefit caseload**

| | Dependent variable: ln(total outflow) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | JSA | JSA | IB | IB |
| $D_{it}$ | -0.0251*** | | 0.0091 | |
| | (0.0060) | | (0.0088) | |
| $D_{it+1}$ | | -0.0288*** | | -0.0074 |
| | | (0.0077) | | (0.0089) |
| $D_{it+2}$ | | -0.0317*** | | 0.0029 |
| | | (0.0079) | | (0.0116) |
| $D_{it+3}$ | | -0.0340*** | | -0.0096 |
| | | (0.0080) | | (0.0107) |
| $D_{it+4}$ | | -0.0280*** | | 0.0188 |
| | | (0.0067) | | (0.0132) |
| $D_{it+5}$ | | -0.0283*** | | -0.0100 |
| | | (0.0065) | | (0.0128) |
| $D_{it+6}$ | | -0.0203*** | | 0.0316** |
| | | (0.0067) | | (0.0140) |
| $D_{it+7}$ | | -0.0243** | | 0.0240 |
| | | (0.0095) | | (0.0167) |
| $D_{iLR}$ | | 0.0031 | | 0.0592*** |
| | | (0.0087) | | (0.0148) |
| $\ln(U_{ait-1})$ | 0.5940*** | 0.5898*** | 0.4055*** | 0.3950*** |
| | (0.0144) | (0.0139) | (0.0522) | (0.0502) |
| $\ln(U_{a'it-1})$ | 0.0560*** | 0.0561*** | 0.0106 | 0.0384 |
| | (0.0135) | (0.0135) | (0.0265) | (0.0239) |
| Observations | 29,159 | 29,159 | 26,450 | 26,450 |
| P-value | | 0.0002 | | 0.0000 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from 1999Q3 to 2006Q2. The estimated specification is equation (1) for columns 1 and 3 and equation (2) for columns 2 and 4. The dependent variable is the log of quarterly JSA outflow in columns 1 and 2, and the log of quarterly IB outflow in columns 3 and 4. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for district, time and age fixed effects as well as age*district and age*time interactions. Regressions are weighted by the respective benefit caseload in the district-age group in 1999Q3. The last row contains the p-value of the F-test for the joint significance of the post-treatment dummies. Standard errors are clustered at the district level. Time effects are a separate dummy for each quarter by year pair. *** p<0.01, ** p<0.05, * p<0.1

**Table A5**
**Treatment effects on monthly outflows from JSA**

| | Dependent variable: ln(total outflow) | |
|---|---|---|
| | (1) | (2) |
| $D_{it}$ | -0.0162*** | |
| | (0.0056) | |
| $D_{it+1}$ | | -0.0251*** |
| | | (0.0067) |
| $D_{it+2}$ | | -0.0205*** |
| | | (0.0072) |
| $D_{it+3}$ | | -0.0215*** |
| | | (0.0068) |
| $D_{it+4}$ | | -0.0187*** |
| | | (0.0063) |
| $D_{it+5}$ | | -0.0217*** |
| | | (0.0076) |
| $D_{it+6}$ | | -0.0064 |
| | | (0.0070) |
| $D_{it+7}$ | | -0.0061 |
| | | (0.0088) |
| $D_{iLR}$ | | 0.0067 |
| | | (0.0097) |
| $\ln(U_{ait-1})$ | 0.7099*** | 0.7090*** |
| | (0.0108) | (0.0109) |
| $\ln(U_{a'it-1})$ | -0.0620*** | -0.0629*** |
| | (0.0103) | (0.0102) |
| Observations | 84,202 | 84,202 |

Notes. The sample is a panel of 406 districts and two age groups (18-25 and 26-60) from January 1999 to December 2008. The specifications estimated are equations (1) and (2) in columns 1 and 2, respectively. The dependent variable is the log of monthly JSA outflow. $D_{it}$ takes the value 1 in the post-policy period and zero otherwise. $D_{it+\tau}$ indicates treatment $\tau$ quarters after the policy is introduced. $U_{ait-1}$ denotes the stock of individuals on same benefits in the same age group at the beginning of the quarter, and $U_{a'it-1}$ denotes the corresponding stock for the other age group. All regressions control for district, time and age fixed effects as well as age*district and age*time interactions. *** p<0.01, ** p<0.05, * p<0.1.

**Appendix B: Data Description**

Our empirical analysis combines various data sources.

The design of the policy and the list of districts covered under each wave of the rollout was provided by the Department of Work and Pensions.

1. The Job Seeker's Allowance database is downloaded from www.nomisweb.co.uk and it provides monthly information from 1983 on the stocks, inflows and outflows of recipients' unemployment benefits. The data is available at various geographical levels. We use the data at the Local Authority Districts across Great Britain and there are 406 districts defined on a consistent basis. The data can be disaggregated by age, duration as well as both.

2. Data on other welfare benefits – including Incapacity Benefit (IB) – is sourced directly from the Department of Work and Pensions Tabulation Tool – www.tabulation-tool.dwp.gov.uk/WorkProg/tabtool.html. This provides only quarterly data on the stocks, inflows and outflows of benefit recipients. The data is available for Great Britain (i.e. including England, Wales and Scotland) at the Local Authority Districts level from 1999 Q3 onwards. The 4 quarters in the dataset are defined as February-April, May-July, August-October and November-January. The data can be disaggregated by age and duration. To be consistent across JSA and IB datasets, the monthly JSA information is aggregated to the quarterly level (February-April, May-July, August-October and November-January).

3. We use the quarterly micro-level panel data from the UK Labour Force Survey from 1998 to 2008 to obtain estimates on the household composition of benefit claimants, mean wages, origins of benefit inflows and destination of benefit leavers. The data is securely provided by the UK Data Service under Special Access License. The quartiles of the weekly earnings

distribution were taken from the 2000 New Earnings Survey.

4. We used digitized boundary datasets and geographic look-up tables corresponding to the census geography of Great Britain, provided by the UK Data Service. We used the boundary data in ArcGIS to illustrate the policy rollout and to define the neighbours of districts.

5. Finally, the IFS had generously provided benefits estimates using the IFS tax and benefit micro-simulation model, TAXBEN. In order to estimate the net exchequer cost of benefit claimants, their estimates assumed that the house rent is £44 per week (the average among families receiving income support, jobseeker's allowance or incapacity benefit) and that all disposable income is spent on items subject to the standard rate of VAT and no excise duties.

## Appendix C

*C1. Steady-state change in the IB rate for the Cost-benefit analysis*

In the cost-benefit analysis we have to consider translating our estimates of flow changes into changes in unemployment and welfare stocks. We obtain the steady state change in the IB rate, based on permanent changes in the IB outflow rate following the introduction of Jobcentre Plus.

Assume there are only two states, employment and IB, and denote by $s$ the inflow rate from employment into IB, and by $f$ the outflow rate from IB into employment. In steady state the IB rate is constant, and flow equilibrium implies that the IB rate (as a fraction of the total population) is given by:

$$u = \frac{s}{s+f}$$

The policy has an impact on $f$, leaving $s$ unaffected. The resulting change in the (log) IB rate is given by

$$d \ln u = -(1-u) d \ln f.$$

The implied change in the number of jobs in steady state is given by:

$$\Delta e = -u \, d \ln u = u(1-u) d \ln f \qquad \text{(C1)}$$

According to our estimates, $d \ln f = \beta - (1-\alpha) d \ln u$, where $\beta$ is the treatment effect estimated by diffs-in-diff, and $\alpha$ is the coefficient on the log IB stock. The terms in $u$ on the right-hand side of (B1) are evaluated using the actual IB rate in the pre-policy period.

*C2. Off steady-state approximation*

At each point in time the unemployment rate evolves according to

$$\frac{du_t}{dt} = s_t(1-u_t) - f_t u_t. \qquad \text{(C2)}$$

Solving (B2) forward one period gives:

$$u_t = \gamma_t u_t^* + (1-\gamma_t) u_{t-1}, \qquad \text{(C3)}$$

where $u_t^*$ denotes steady state unemployment and $\gamma_t$ denotes the rate of convergence to it:

$$\gamma_t = 1 - \exp(f_t + s_t).$$

Using a log-linear approximation to (B3) it can be shown that:

$$d \ln u_t = -\gamma_{t-1}(1 - u^*_{t-1}) d \ln f_{t,}$$

where, as above, $d \ln f = \beta - (1 - \alpha) d \ln u$, $u$ is evaluated using the actual IB rate in the previous quarter and $u^*$ is evaluated using the (constant) pre-policy inflow rate into benefits obtained from the Labour Force Survey and the time varying outflow rate from benefits as obtained from our estimates.

While the steady-state result stated above is only used for IB predictions (as the steady-state impact of policy on the JSA outflow is close to zero), the off-steady state results are used to obtain predictions for both the IB and JSA rate during the transition to a new steady state.