Issues

- Linking with our original motivation on increase in youth unemployment in mid 2000s
- Career prospects of managers and targets. Any qualitative anecdotes we can offer to back up assertions on e.g. p. 3 on basis on JC+ reports?

Incentives, Disruption and Jobs: Evidence from a Public Employment Service Reform

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Abstract

We distinguish three effects of welfare reforms. First, changes in bureaucratic incentives to move targeted welfare recipients into jobs; second, often unintended substitution effects due to multi-tasking: bureaucrats will substitute effort away less targeted groups; third, organizational disruption effects as new systems are put in place. We identify these effects by examining the dynamics of outflows across different welfare claimants before and after a major UK policy change ("Job Centre Plus") that shifted relative incentives of benefit officers to place disabled people into jobs compared with the unemployed. The policy was staggered across different areas in 6 waves over a 5 year period. We find that the policy significantly increased outflows from disability benefits, but *reduced* the outflow rate from unemployment benefits in the short run. In the long-run, however, both groups benefit but the disabled outflows rose by more (6%) than for the unemployed (1%). We perform a cost-benefit analysis showing that a social planner would favour immediate introduction of the policy: benefits outweigh costs by around £8bn in present value terms. However, a myopic policy maker would not favour the policy as social costs exceed benefits until five years after introduction. This illustrates the difficult political economy of welfare reform and other public investments that have a pay-off longer than election cycles.

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

JEL Codes:

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1. Introduction

The Great Recession has led to large increases in the numbers claiming unemployment benefits and other forms of welfare across the OECD. This has made the issue of structural reforms to improve welfare to work transitions of utmost importance to mitigate the hysteresis effects that damaged many economies after major recessions in the 1980s and the Great Depression. The rise in unemployment has been much less than expected in the UK[4] and Germany, and both countries experienced significant welfare reforms prior to the crisis. Did these reforms help?

A growing literature has highlighted how incentive systems can be used to improve efficiency, particularly in the public sector (see Bloom and Van Reenen, 2011, for a survey). Heckman et al. (2011) investigate incentive structures that reward bureaucrats based on measured outcomes to find that these systems affect which individuals get included in the government programs and their short run impacts do not always predict their long run impacts on participants. One mechanism for these impacts has been how bureaucrats alter their behaviour in response to incentive structures and performance measures. The incentive system could lead bureaucrats to cream-skim and re-allocate resources which may be socially suboptimal.

In this paper we focus on the impacts of bureaucratic incentives and organizational restructuring through the design in 2001 of Jobcentre Plus, an executive agency that has gradually become the key provider of job placement services and benefit advice in the UK. The introduction of Jobcentre Plus implied a major reorganization of the welfare system for jobseekers, as public job placement services (formerly run by the Employment Service) and benefit services (run by the Benefits Agency) were integrated into one single organization.

Another major change was the introduction of explicit performance targets that rewarded benefit officers according to job placement. The UK welfare system had introduced performance benchmarking since the early 1980s (Propper and Wilson 2003; Bagaria et al, 2013),[5] which mostly consisted of national-level targets for the number of

[4] On the UK case see Blundell, Crawford and Jin (2013); Gregg, Machin and Salgade (2013) and Pessoa and Van Reenen (2013).

[5] 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.

beneficiaries to place into jobs, without explicit rewards at the individual or local level. Jobcentre Plus radically reformed the performance evaluation of the welfare system, by introducing a new regime of Job Entry Targets, in which every benefit officer who helped a benefit claimant into a job was awarded a certain number of *explicit* Job Entry Target 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 performance benchmark for the managers and mattered for the career prospects of the officers. As the new incentive scheme established higher reward points for helping Incapacity benefit (IB) claimants into new jobs than for helping the unemployed on Jobseekers' Allowance (JSA), officers would potentially reallocate effort across categories of clients.

In our empirical analysis we exploit the staggered roll-out of Jobcentre Plus across UK districts. 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 case of JSA claims, disaggregated by reason for exit. The availability of information on exits from various benefit categories during a long time horizon after the introduction of Jobcentre Plus allows us to separately identify short-run and long-run effects of the reorganization, as well as the permanent differential effects of the new incentive structure.

We find two main results. First, there were significant organizational disruption costs from the policy, with outflows from disability and unemployment benefits initially declining after the introduction of the policy, and more markedly so for JSA than IB recipients. Second, in the long-run there were significant positive effects on the outflows from IB, while the effects on JSA outflows, though positive, are modest and not significantly different from zero. These patterns are consistent with a simple story whereby the change in incentives to benefit officers increased their efforts to reduce the IB rolls, but decreased their efforts to reduce JSA rolls. However, there was also an organizational change which although positive for both groups in the long run, caused transitory organizational disruption which hurt both welfare groups' job finding rates in the short-run. We present a cost-benefit analysis that supports the change showing that in steady state the present value of social benefits outweighs costs by around £8bn. However, the presence of significant short-run costs highlights why such welfare changes are hard to implement. The short-run time horizons of politicians would militate against the desire to implement such structural labour market reforms. In our cost-benefit analysis, flows of net social benefits do not become positive until five years after the policy is introduced.

Our paper builds on a growing strand of literature that studies the effects of incentive pay in the public sector (e.g. Besley and Ghatak, 2005). We contribute to this literature by emphasizing the multitasking aspects in the provision of effort in local government organizations and specifically we exploit differential incentives in job placements and data on the outflows from incapacity and unemployment benefits over a long horizon after policy change. Our work is also closely related to the literature on the effectiveness of welfare systems in developed countries (see for example Meyer, 1995, for a survey of findings from social experiments). A body of work in this literature has studied the effects of financial incentives to benefit recipients on the duration of unemployment (see Lalive et al. 2007 for recent evidence), while the role of explicit incentives in the provision of job placement services is to date largely unexplored. Our paper contributes to fill this gap by providing one of the very first evaluations of the introduction of explicit incentives to bureaucrats in welfare systems.

The paper is organized 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 inflow into and outflow 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. The Institutional Framework

2.1 The Jobcentre Plus system

There were major infrastructure changes in the delivery of public employment and benefits services in the UK between 2001 and 2008. The change was part of a wider policy emphasis on Welfare-to-Work initiatives[6] that sought to make sure that those on unemployment benefits were active in the labour market. In March 2000, the Prime Minister announced the establishment of a new organization – Jobcentre Plus – that would deal with people of working age, to deliver a single, work-focused, integrated service to both employers and benefit claimants of working age in UK. Since October 2001, the Employment Services (ES) and Benefits Agency (BA) were integrated into one organization. The new organization which combined benefit advice with job placement services was called Jobcentre Plus. With a budget of £1.9 billion for the roll-out, the integration took place between 2001 and 2008.

What did this mean for the delivery of services to benefit claimants? There were two main changes that took place as part of the Jobcentre Plus package. The first involved changes in the physical organization of offices as the two types of services (Employment Services and Benefit Services) were now delivered under one roof. The integration of the two departments was accompanied by the introduction of modernized Information Technology (IT) systems and organizational restructuring. The number of offices was reduced from 1500 separate offices to 865 combined offices across the UK. There were staff reductions in the range of 15,000, rationalization of estates, and office refurbishments. Though the rationalization reduced floor space of the Jobcentre Plus estate by 20%, it increased operating costs per square meter by 12% because of high quality infrastructure and locations[7].

The second major change was the introduction of explicit performance targets called Job Entry Targets (JET). As opposed to the previous system of national-level targets for the number of beneficiaries to place into jobs, under the regime of Job Entry Targets, every officer who placed a benefit claimant into a job was awarded a certain number of *explicit* Job Entry Target 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 performance benchmark for the managers and mattered for the career prospects of the benefit officers.

[6] The guiding principle of welfare reform was "work for those who can and security for those who cannot" (Hyde et al., 2002).
[7] Source: National Audit Office, "The Roll-out of the Jobcentre Plus Office Network", 2008.

2.2 Model

Theoretically, we distinguish between three different effects on benefit officers' job placement activity as a result of the introduction of the Jobcentre Plus. Firstly, the physical reorganization, installation of new IT systems and estates rationalization would have an immediate disruptive effect, as the provision of services would have been disrupted and unavailable for some periods of time during the initial phase of the introduction of Jobcentre Plus. This would lead to a reduction in the productivity of the officers in the short-run. We expect the disruption effect to be broadly similar across all benefit groups. This effect should decay over time as officers settle into the new system.

We distinguish this from the second hypothesis about the long-run effect of system restructuring and modern IT systems on efficiency. Benefit officers are now able to use the new IT systems which facilitate various 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 were 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, in the long run, we expect an 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 effect of the Job Entry Targets implies a shift in bureaucratic incentives in favour of Incapacity Benefit claimants. Before the introduction of Jobcentre Plus, 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 (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) into work.

The explicit award points under the new Job Entry Targets (JET) system were designed to reflect organizational priorities towards the Incapacity Benefit claimants. As shown in Table A1, a benefit officer was awarded fifty per cent more points if he/she placed a person on Incapacity Benefit[8] into a job than a long-term JSA beneficiary, and three times

[8] We discuss the other group, Lone Parents in Section 5 and the Appendix.

more points relative to a short-term JSA beneficiary. Given that the benefit officers had to achieve a quarterly target number of points, this should incentivize them to focus on placing the IB claimants.

The model we have in mind is a simple multi-tasking model with fixed inputs along the lines of Holmstrom and Milgrom (1991). Assume that the Jobcentre officers have a given stock of "inputs". They can apply different amounts of this "input" to different individual clients (benefit claimants). These inputs affect the outcomes experienced by claimants. In our context, the input variable represents staff time for interviews and the direct costs of the services provided. After the introduction of the explicit weighting system, we expect them to re-organize the manner in which they allocate their efforts[9]. They would now focus more on and increase effort in placing Incapacity Benefit claimants. Given a fixed stock of "inputs", this should adversely affect the job placement efforts and outcomes for those on Job Seeker's Allowance claimants. Thus, Jobcentre Plus is likely to have a larger impact on IB recipients than JSA recipients.

2.3 Identification

We exploit the staggered roll-out of the Jobcentre Plus offices across Local Authority Districts in the UK to identify the causal impact of the policy. The switch to Jobcentre Plus was phased in over six waves, as illustrated in Figure 1. The first wave begun on 1st October 2001 in 32 districts. The second wave began in October 2002 with 27 more districts offering a fully integrated work and benefits service, and by the first quarter of 2008, most of the Jobcentre Plus offices had gone live. Figure 2 presents a map of the policy roll out, showing no obvious patterns of geographic clusters that adopted the policy at the same time.

We use the "True Go Live" date for offices within a district. The Jobcentre Plus programme was rolled out at the office level and each district has several offices.[10] We define our treatment indicator such that a district is "policy on" when at least one office in that district was treated in that quarter. Other offices could have been treated at a later date and indeed it could take as long as three years for all offices to be covered. But we conduct our analysis at the district level as opposed to the office level for several reasons. Firstly, the

[9] Unfortunately, we're not able to measure staff inputs, but we can observe participant outcomes. So, in a sense this is a reduced form estimation.
[10] This ranges from a minimum of 32 to a maximum of 171.

new quarterly targets under the Job Entry Target system were at the district, rather than office level. Secondly, the plans to implement the Jobcentre Plus programme were drawn up at the district level making the office roll out endogenous.[11] With targets to meet each quarter, it is likely that a district will first treat the "good" offices i.e. those that have a higher benefit outflow rate to begin with. In the Appendix Table A2 we show that this is a problem.

We use a difference-in-differences framework to identify the causal impact of Jobcentre Plus. 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. We call the former treated and the latter non-treated. Of course one concern is that districts selected into treatment were not randomly assigned into treatment. This would confound our causal interpretation. To examine this, we check for differences in pre-trends between the treated and non-treated for various benefit categories, as discussed further in section 4.

We measure bureaucratic outcomes by the number of job placements from each benefits register each quarter in each district, for the two benefit registers – Incapacity Benefit and Job Seeker's Allowance. To measure the impact of Jobcentre Plus, we estimate standard benefit outflow equations for these two key groups. We further disaggregate outflows by age groups - young and old. The young are defined as being aged between 18 to 24 years and the older group consists of people aged 25-59 years. We also examine other age splits as robustness checks (Table A6).

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 choose to enter the caseload as a disabled person rather than under Job Seeker's Allowance. In the UK context, it is rather difficult to be classified as disabled to claim Incapacity Benefit since it requires a medical certificate and the conditions required to qualify for

[11] 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 of Jobcentre Plus was a locally driven process" and that "Localised planning allowed Jobcentre Plus to make early progress with the roll-out, as the districts which were ready first could be scheduled for early roll-out".

receiving Incapacity Benefit have been made stricter over time.[12] A related concern is that the introduction of Jobcentre Plus may affect jobseekers' decisions whether to sign-on at all for benefits. To examine this further, we also analyse the impact of the Jobcentre Plus on the *inflows* into the different categories in section 4.

3. Data

We use two sets of administrative panel data provided by the UK Department of Work and Pensions that cover the welfare population. The first is the Job Seeker's Allowance (JSA, henceforth) database. It contains ongoing, monthly information from 1983 on the stocks, inflows and outflows of recipients of JSA unemployment benefits. The data is available at the Local Authority District across Great Britain and there are 406 districts defined on a consistent basis.[13] The data can be disaggregated in various ways, and we focus on cuts by age (alternative age categories are shown in table A6).

The second dataset contains *quarterly* data from 1999Q3 at the district level on other welfare benefits including the key disability benefit, Incapacity Benefit (IB, henceforth). To be consistent across the two datasets, we aggregate the monthly JSA data to the quarterly level, but use the monthly information as a robustness check. We estimate all our specifications on a consistent time period of nine years (36 quarters) 1999Q3 to 2008Q2, the quarter before the collapse of Lehman's which triggered the Great Recession and a huge upsurge of unemployment.

For the JSA dataset, but unfortunately not IB, we also have some information on destinations after leaving JSA and use this to examine whether those who left as a result of the policy obtained jobs, as opposed to exit the labor force.

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[12] For instance, in 1999, the Welfare Reform and Pensions Bill introduced '*continuing assessment of possibility of returning to work*' (Burchardt, 1999). While the criteria for benefit receipt remained unaltered, the significant change was the collection of additional information focussing on the abilities of the claimant at intervals and the allocation of a personal adviser to oversee each claim. New claimants were also required to attend an interview at the beginning of the claim, and any time thereafter, to discuss possibilities for returning to work. More recently, applicants to IB will have to go to a Work Capability Assessment during the first 13 weeks of IB. This was aimed to see if the illness or disability affected the claimant's ability to work.

[13] 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.

Descriptive statistics are 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. The aggregate outflow rate from JSA is 70% consisting of an average outflow of 653,819 per quarter and a stock of 939,267 per quarter in the country as a whole. Inflows were a bit lower than this (648,957 per quarter) as unemployment was falling over the sample period. Outflow rates for IB were much lower at 3% per quarter: as is well known far fewer people leave the stock of disability rolls than unemployment.

3.1. Jobcentre Plus and Benefit Flows

We estimate standard benefit outflow equations in a difference-in-differences framework. We start by estimating a static model to estimate the *average* effect arising from the change in bureaucratic incentives. The base specification is:

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

where Y_{ait}^{B} is the outcome measure in terms of the logs of number of people in age group a leaving the benefit register B (JSA or IB) in district i at time (year-quarter) t. D_{it} denotes a treatment dummy which turns on in the quarter when the first office in district i is treated. The coefficient is identified by the fact that the policy was rolled out in six waves with different districts being treated in each wave. One robustness test we consider is to allow β^{B} being different in each wave and showing that the effect looks remarkably similar across waves when the post-wave window is kept fixed. As noted above, we found that the timing of when a district was treated appeared to be unrelated to observables, whereas the timing of when a particular office within a district was treated was systematic (see robustness test results in Table A2).

We include as controls the stock of claimants of benefit B at the end of the previous quarter for the own age group, U_{ait-1}^{B} as well as other age groups, a' (old/young respectively in the baseline specification). Our preferred specifications include a full set of fixed effects (district by age group) 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 which is the level at which the policy is defined.

In equation (1) the treatment effect is summarized by the coefficient β^{B} which is an average over all the post-treatment quarters. Our adjustment cost theory, however, suggests that there should be a distinct dynamic pattern of change with the positive policy effects being dampened at first by organizational disruption. Hence, we allow the policy effect to be different depending on how many quarters have elapsed since the policy.

$$\ln Y_{ait}^{B} = \sum_{\tau=1}^{7} \beta_{\tau}^{B} D_{it+\tau} + \beta_{>7}^{B} D_{it>7} + \gamma_{1}^{B} \ln U_{ait-1}^{B} + \gamma_{2}^{B} \ln U_{a'it-1}^{B} + \delta_{ai}^{B} + \delta_{at}^{B} + \varepsilon_{ait}^{B}$$

The $D_{it+\tau}$ term is broken down by period after the policy begins so D_{i1} is the quarter when the policy is turned on, D_{i2} is the first quarter after the policy is in effect and so on. $D_{it>7}$ is the "long-run" defined as 8 quarters or more after the policy is in place. Since the last district to have the policy was Wave 6 in 2006Q3 we have at least two years of post-policy experience for all districts. Ending the dynamics after two years is somewhat arbitrary, but we show that the qualitative results are robust to alternative dynamic specifications in the Appendix (e.g. Table A4)

The disruption hypothesis suggests that the $\beta_3^B > \beta_2^B > \beta_1^B$ and so on. In other words, the initial disruption effects unwind as the new organizational structure settles down. The incentives hypothesis suggests that the positive effects on IB should be greater than JSA i.e. $\beta^{IB} > \beta^{JSA}$. A stronger version in the dynamic specification would be $\beta_{\tau}^{IB} > \beta_{\tau}^{JSA}$ for all τ.

4. Results

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

4.1. Basic Results on Outflows from benefits

In column (1) of Table 2 we estimate equation (1) where the dependent variable is the ln(total outflow) from unemployment (JSA claimants). Our controls include the stock of unemployed claimants at the end of the previous quarter by age group, time (quarter by year), age and district dummies. The coefficient on the post-policy dummy is negative and significant with a value of -0.0153 suggesting that a district treated with the Jobcentre Plus experiences, on average, a 1.5% *decrease* in unemployment outflows. Given the average outflow of 653,819 per quarter this implies 9,807 more people staying on unemployment benefits. This overall impact is consistent with the idea of disruption effects and/or that the job entry point system gave incentives for benefit officers to substitute effort away from the unemployed and towards the disabled. The lagged stock of own age unemployed claimants enters with a significant positive coefficient as expected (the larger stock the larger the flow, all else equal). The coefficient on the stock of the other age group is negative, implying some substitution effects due to job competition. In column (2), we include a full set of fixed effects (district by age dummies) and again find a significant negative coefficient on the post-policy variable and this result remains basically unchanged when we also include age interacted with time effects in column (3).[14]

We repeat the same specifications in columns (4)-(6) of Table 2 for the (log) outflow from IB, and condition on the lagged IB stocks on the right hand side.[15] In column (4) we estimate the specification analogous to column (1) and find a positive and weakly significant coefficient of 0.0166 on the post-policy dummy. This suggests about a 1.7% increase in total outflows, which given a sample average outflow of 56,166 people means an additional 954 fewer people off the IB register. In column (5), we include a full set of fixed effects and in column (6) we include age*time dummies. The treatment effect falls slightly to 0.0151.

The specifications in Table 2 just look at the post-policy period as a whole without examining the dynamics of the policy effect. The theory suggested that there would be short-run negative effects arising from organizational disruption costs. Table 3 probes the dynamics more carefully allowing a differential effect in each of the quarters after the policy switches on (up to the eighth quarter after introduction) as in equation (2). We use the same specifications as Table 2. Interestingly, the coefficients show a consistent dynamic pattern, being negative in the quarter immediately after the policy's introduction, but then becoming more positive over time. We detect negative impacts on JSA outflows for the first 5 quarters, but these cease to be significant by quarters 6 and 7 and actually turn positive for quarter 8 and beyond. This suggests that after two years there is a positive effect of 1.2% on

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

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

outflows due to the policy, although this effect is not significantly different from zero. In contrast, for IB outflows, although we find a negative effect in the first quarter after the policy is introduced, this turns positive by the second quarter. This positive effect gradually becomes larger and more significant until in the long-run it reaches a level of 0.061 in our most general specification of column (6).

These dynamic responses are presented graphically in Figures 3 and 4 and highlight our two main findings. First, the long-run effect is positive for both forms of welfare, but it is clearly much stronger for disability benefits than unemployment benefits. Second, there is initially a negative effect for both benefits of the policy change.

The interpretation of our results is that the more positive effect of the policy on IB compared to JSA is driven by the job point system, such that officers devote more effort to helping the disabled into new jobs than the unemployed after the policy change. Consistent with the theory, agents act on these incentives. Overlaid on this, however, is an initial disruption effect as buildings and new systems bed down. This depresses all outflows and is an adjustment cost of the policy.

An alternative explanation would be that incentives do not matter but somehow the organizational change had a disproportionately larger effect on IB claimants than the unemployed. It is not obvious why this should be, but we will look at a more refined test of the incentives hypothesis involving a third group of welfare recipients (lone parents) where bureaucratic incentives are somewhere in-between those for the other two groups. We find results again consistent with the incentive hypothesis when looking at outflows from benefits for this third group (see Section 5).

4.2. Magnitude of the outflow effects

Using the information on average benefit payouts, we perform some simple back of the envelope calculations. Consider the steady state/long-run effects first. For JSA the long-run coefficient implies 7,650 fewer individuals on the unemployment rolls each quarter and fiscal savings of £5.1m using an average weekly payout of £55.4. For IB the long-run coefficient is 0.0612 and the average weekly payout is £74.71. [16] This implies an extra 3,437 people leaving per quarter and savings of £3.1m. Putting this all together implies a saving of

[16] The weekly payouts are average JSA and IB payments, respectively, recorded in 2001.

about £33m (= (5.1m+3.1m)*4) per annum.

There are other benefits as well as lower exchequer costs of course. First, some of these individuals will go into jobs and this will increase output as well as providing tax revenue. Secondly, to the extent that there are hysteresis effects, less human capital will be lost to the economy (e.g. Delong and Summers, 2012). Third, there may be other benefits to getting people into work through less anti-social behaviour. Fourth, happiness research suggests many psychic benefits from having a job, although classically we would have to also consider disutility from loss of leisure.

Although in the long-run the policy has clear benefits we have also shown there are short-run disruption costs. In the first quarter, for example, outflows from JSA and IB both fall, so in the short-run the costs clearly outweigh the benefits. Hence, whether the policy is attractive from a welfare point of view depends on the social discount rate. In section 6 we present a full welfare analysis which shows that for reasonable value of this, social benefits outweigh the costs, but unfortunately policy-makers' time horizons might be shorter.

4.3 Pre-policy trends?

An identification threat to any difference-in-differences estimation is 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):

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

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 comparison. In column (2) we include four pre-treatment lags and note that the coefficients on the pre-treatment dummies are jointly insignificant (F-test =1.88). We perform the same specifications for IB in columns (3) and (4) and again find no evidence of pre-treatment effects.

Although this is 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 could be due to the fact that our treatment indicator is based on the true "go live" date of Jobcentre Plus and there is likely to be some organizational disruption in advance of that date, which could spill into the previous quarter.[17] This would reduce the benefits of the policy.

4.4 Inflow Rates

Our analysis focuses on the intended Jobcentre Plus outcome to increase the outflow rates from benefits, but a possible side effect is that the inflow rate into benefits also changes as a consequence of the policy change. For example, individuals may have an incentive to apply for IB rather than JSA if they perceive the new regime as "tougher" for JSA claimants, thereby inducing a change in size and composition of IB and JSA stocks and in the corresponding outflow rates. The resulting bias in the estimated policy effect is hard to sign. One would expect an upward bias in the estimated effect of the policy on the JSA outflow (and a downward bias for the IB outflow) if the dissuaded individuals were less motivated in their job search and more weakly attached to the labour force – and vice versa.

To examine this issue directly we analyse the impact of the Jobcentre Plus on *inflows* into JSA and IB. We estimate a specification similar to equation (1) but use the inflow into each benefit category as the dependent variable instead of the outflow:

$$\ln(INFLOW_{ait}^{B}) = \sum_{\tau=0}^{7} \beta_{\tau}^{B} D_{it+\tau} + \beta_{>7}^{B} D_{it>7} + \rho \ln Pop_{ait} + \delta_{ai}^{B} + \delta_{at}^{B} + \varepsilon_{ait}^{B} \qquad (4)$$

In the outflow equation we 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 to unemployment, 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.[18]

[17] National Audit Office (2008) states that **"**It 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 staff interact with customers and hence the quality of service provided."

[18] We assign the mid-year population estimate from www.nomisweb.com (taken on the 30th of June each year)

In column (1) of Table 5 we show that, on average, Jobcentre Plus had no significant effect on the inflows into JSA. Along similar lines, column (3) shows no evidence of significant average effects on inflows into IB benefits. However, when we look at the dynamic impact on inflows in columns (2) and (4), we find that the policy had initially a negative and significant impact on inflows into both benefits, and these effects turn positive (though not significant) for the JSA after five quarters, while for IB they drop very close to zero after three quarters.

To address whether this could be a concern for our results, we repeat outflow regressions controlling for various lags of the corresponding inflows. The results are reported in Table 6, where all specifications include fixed effects for both district*age and age*time interactions.[19] Columns (1)-(3) refer to JSA outflows. Although the coefficients on the inflow variables, whether one or four lags, are positive and significant as one would expect, our main results are robust to their inclusion. To see this, in column (3) we report our baseline equation (2) on the same sample as column (2), as some observations are lost when we condition on lagged inflows. The long-run effects in columns (1) and (2) are almost identical to those in our baseline specification of column (3), and the dynamic effects only slightly muted by the inclusion of inflows. Columns (3)-(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 (column (6)) to 0.0503 in column (5). Hence, although inflows do change, any change in composition arising from this does not appear to substantially affect our results. Figures 5 and 6 illustrate this graphically.

4.5. Outflows to employment vs. other destinations

The JSA (but not the IB) database allows us to disaggregate the 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.) The results of this analysis are reported in Table 7, where columns (1) and (2) refer to outflows into work, while

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to all the quarters in the year. Using interpolated quarterly population estimates (from the mid-year estimates) does not change our results.

[19] 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.

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 account the "stick" (job search monitoring) and "carrot" (search effort assistance) components of the interactions between JSA claimants and dedicated staff at Job Centres. The change in the incentive structure implied 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. Robustness and Extensions

5.1 Robustness Tests

This section reports the results of a number of robustness tests, which consistently confirm our previous results on disruption and incentive effects of Jobcentre Plus.

Disaggregating the treatment effect by wave

As discussed above, the roll out of the policy was introduced in six staggered waves across the country. Our baseline estimates exploit all waves for identification, but an important issue is whether there is any heterogeneity in the treatment effect across different waves. For example, a legitimate worry could be that the dynamic effects that we identify in Figures 3 and 4 may be instead due to averaging over different effects in earlier and later waves.

To investigate this we estimated equation (1) separately for each wave of the policy roll-out. In order to avoid conflating the dynamics with wave effects we kept to a fixed post treatment window of 4 quarters. The results are reported in Table 8. 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 we estimated in Table 2 of -0.015, wave-specific estimates range from -0.10 (wave 5) to -0.20 (wave 1) which is a reasonably tight bound. IB estimates in Panel B are generally higher (a range of 0.011 to 0.32) than the pooled estimate of 0.015, suggesting, if anything, that we might be underestimating the positive effect of the programme using the parsimonious specification of equation (1).

We also considered alternative specifications such as restricting the comparison areas to those that had not been treated (Table A3), which lead to similar results.

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 9 delivers an average effect of Jobcentre Plus on JSA outflows of -1.6%, which is very close to the 1.5% fall reported in column (3) of Table 2. Column (2) reports dynamic effects we look 8 quarters after treatment and find that the JSA effect is negative initially.

Other dynamic patterns reported in Table A4 again confirm the robustness of our main specifications.

Weighting

One further worry is that our results are driven mainly by a few districts and would not be representative of a homogeneous treatment affect across districts. To address this concern, we weight observations by the district level age-specific benefit caseload in 1999 quarter 3, much before Jobcentre Plus was implemented. We use the age specific caseloads as some districts may have a larger proportion of older benefit claimants than others, and we need to account for these potential compositional differences across districts. Table A5 reports the results for specification (1) using this weighting system, instead of assuming equal weights as before. Column (1) now delivers an average treatment for JSA of 2.5%, while the corresponding estimate for the unweighted regression was 1.5%. This suggests that the policy effect is larger in larger districts. Dynamic effects reported in column (2) are instead very similar to those of Table 3, column (3).

For IB, the average effect reported in column (3) for IB is now much lower than in the unweighted regression, and statistically insignificant at 0.9%. We interpret this result as showing that smaller districts seemed to be more affected by the treatment for IB. The short and long run effects of the treatment are however very similar to those from the unweighted regression reported in Table 3, column (6).

5.2 Lone Parents' Benefits

Besides JSA and IB, Lone Parents on income support (LP) are the third big category on the welfare rolls. The introduction of job entry points increased bureaucratic incentives to place IB recipients into work and reduced the incentives to place JSA recipients into work. From Table A1 we see the points awarded to LP is similar to IB, so one would also expect a positive effect on this group.

There are two problems, however, with simply looking at the LP group as another test. First, the policy change was more intense for the IB group than the LP group. This is because Jobcentre Plus introduced mandatory "work focused" interviews for IB claimants, but not for LP,[20] and one may argue that the impact of incentives on job placements would only work via repeated interaction between benefit recipients and providers. Secondly, there were other policy interventions affecting the LP group around the time period we focus on, including a large increase in the generosity of in-work benefits for lone parents (initially called Working Families Tax Credits) and the "New Deal for Lone Parents", a job assistance programme. These changes may contaminate our tests, and indeed we find evidence for pre-sample trends for this group.

In column (1) of Table 10 we estimate equation (1) for the LP outflow, and obtain an

[20] Increasing the link between benefits and work search had occurred since Restart Programme in 1986 (Dolton and O'Neill, 1996, 2002) and were deepened with the introduction of the Jobseekers Allowance in 1996, and under the New Deal for Young People in 1998 (see Manning, 2009, Petrongolo, 2009, Blundell et al, 2004, and Van Reenen, 2004 for a history of these developments). In particular, mandatory work-focused interviews were in place for JSA recipients since 1996, well before the introduction of Jobcentre Plus (see Pointer and Barnes, 1997).

average decrease of about 1.3% in outflows from LP, an effect that lies between the positive effect on IB and the more negative effect on JSA. However, when testing for the presence of pre-trends in column (2) based on the analogue of equation (3), we find that the joint F-test rejects the hypothesis of no pre-policy trends (F=3.894). Whereas we did not find evidence of differential pre-treatment trends for JSA and IB, we cannot reject such hypothesis for LP.

We attempt to control for these pre-trends by including district-specific trends in column (3) of Table 10, and the joint F-test for pre-trends is now insignificant. Similar to the IB and JSA, however, we do find a negative effect one quarter before treatment, consistent with the impact of organizational restructuring which impedes service delivery even before the true "go live" date. When pre-treatment dummies are dropped in column (4), the average treatment effect is -0.0096 and insignificant. This value falls about half way between the IB and JSA effects, as we would expect. In column (5) we estimate a dynamic specification, and find an initial negative impact, which becomes positive by quarter 6 and is significantly positive in the long run. The long-run effect of 2.5% is smaller than the long-run IB effect of 6.1% but larger than the JSA effect of 1.2% (Figure 7).

Overall, the treatment effects on LP appear to lie between the effects of JSA and IB. Just like the other benefits there appears to be an initial negative effect which we interpret as an organizational disruption effect. However, in the long-run there is a positive improvement consistent with an improvement in incentives.

6. Cost-benefit evaluation

Finally, we perform a cost-benefit evaluation of the introduction of Jobcentre Plus. We consider the immediate introduction of the policy over the whole of Great Britain, rather than the staggered roll-out. We consider two scenarios. First, an analysis as if we immediately went to the long-run steady state and second, an analysis that incorporates the transitional dynamics that we have shown to be so important.

We take into account four elements of impact: (i) the savings in administration costs implied by the reorganization of the welfare system; (ii) the increase in GDP 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.

6.1 Steady State Cost Benefit Analysis

The results of the steady-state analysis are represented in Table 11. The Jobcentre Plus administration cost in steady state is £3,314m which implies a saving of over £200m compared to the previous regime, as shown in rows 1-3. The steady-state impact of Jobcentre Plus on job creation is obtained from the long-run estimates reported in columns (3) and (6) of Table 3. Conservatively, we assume that the long-run impact of Jobcentre Plus on JSA exits 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 as 0.061. Not all of these exits will be to jobs. Using the LFS quarterly panel data 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 assume these 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 and is again a conservative estimate of policy benefits, as many IB exits will be to lower levels (indeed zero) of welfare.

We consider three possible wage outcomes (our proxy for additional output) for individuals finding employment after a spell on IB: the minimum wage, the empirical mean wage for individuals ending a benefit spell (again from LFS), and the median wage in the overall wage distribution (from ASHE). The middle case seems the most realistic but the minimum and median wage scenarios provide useful lower and upper bounds, respectively. Columns (1) to (3) of Table 11 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 £26m and £77m p.a.

Row 6 reports the net gain resulting from a reduced deadweight cost of taxation. This is set to 30%[21] 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 the household composition of the recipient. We used the IFS

[21] This is again conservative and follows Layard (2000). If we took the higher deadweight losses used say by Feldstein, policy benefits would be even greater.

TAXBEN[22] simulation model to approximate these additional taxes and benefits for the 30% of IB exits who found jobs.[23] Putting all these together produces a benefit from a lower deadweight loss between £4m and £25m.

The combination of the three components reported in rows 3, 5 and 6 of Table 11 represent the total annual benefit implied by the policy in steady state. This is an annual net benefit of £270m-£340m a year. Its present discounted value is around £10bn (reported in row 8) based on a 3% social discount rate.

This benefit must be compared to the total sunk set-up cost of Jobcentre Plus, which was equal to £1,859m. At the mean re-employment wage, our cost-benefit analysis implies a net benefit of Jobcentre Plus in excess of £8bn. But even in the most conservative, minimum wage scenario, the net benefit is about £7bn (row 10). The most important component of such net benefit is the saving in running costs (row 3). The bottom line is that Jobcentre Plus is a very attractive policy from a social welfare perspective.

6.2 Cost Benefit Analysis with transitional dynamics

We now generalize our analysis to take into account the transitional dynamics, which allow for the initial fall in job-finding rates for JSA and, to a lesser extent, for IB. To this purpose, we use all estimates of the dynamic effects of policy from columns (3) and (6) of Table 3, for each quarter (1 to 8+) since the policy change.

The three earnings scenarios, as well as the running costs, are the same as those considered for the steady state analysis of Table 11. However, we now need to track GDP and net exchequer gains or losses for each quarter since the policy change, for both JSA and IB recipients. With discounting, this will reduce the present value of the policy change because the losses are front-loaded. We keep to all the other assumptions on job finding rates for IB and again use the empirical data in the LFS that shows that for JSA recipients, 70% of exits where to jobs.[24]

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

[23] 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.

[24] For the benefit and tax simulation we assume that 70% of JSA exitors who find jobs live alone, while 30%

The evolution of costs and benefits over time is represented in Figure 8. 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 dampens down the benefits, it does not do so by much. Under the assumption that the steady state benefits of the reform in term of job placement continue indefinitely into the future, the present value of the net benefit of the reform is about 3.1 billion, 4 billion and 5 billion in the low, baseline and high wage scenario respectively, which easily outweighs the £2bn sunk cost, just as it did in the steady state calculations.

An important point to note, however, is that in the baseline case (middle dashed line in Figure 8), net social costs exceed benefits after a full *five years* since policy introduction. These costs are almost entirely driven by sizeable declines in benefit outflows for JSA recipients during the first two years of the new regime. Only after five years since the policy change are job entry gains sufficient to compensate both the initial job entry losses and the set-up cost.

6.3 Discussion of cost benefit and political economy of reform

Our calculations show that the policy is clearly a desirable one in terms of social welfare. There are short-run costs, most obviously in the set up costs, but as our econometric estimates show, also in initial disruption which causes declines in outflows for all groups and particularly for JSA where bureaucratic incentives have weakened. These short-run costs are swamped by the long-run gains, but will be felt very keenly by policy makers who introduce an expensive reform and see little clear social gain for many years.

In the British system there must be an election every five years. The minister running a department can expect to be in post for less than two years. Even for a purely benign politician this will induce some myopia especially when combined with vigorous criticism in the media and by opposition politicians. Welfare policies like Jobcentre Plus are like classic investment decisions. Since a politician’s discount rate will be much higher than the

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live in a couple with two children

social discount rate we should expect systematic under-investment in such policies. This is indeed what we seem to have experience in terms of public investment in the UK (Aghion et al, 2013) and welfare reform is an illustration of this problem.

Given this, how is welfare reform ever possible? Sometimes a crisis hits forcing radical reforms. But perhaps the large majorities enjoyed by Prime Minister Tony Blair in 1997 and 2001 enabled the government to pursue longer run policies, at least in welfare reform where there was a large degree of cross-party consensus.

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 Jobcentre Plus. Bureaucratic incentives to place the disabled into jobs were sharpened and offices were re-organised to be more efficient (e.g. in their use of IT). We evaluate the success of this, but note two unintended consequences. First, the relative incentives to place the unemployed (JSA claimants) into jobs fell. Second, the re-organization of the job centres can be expected to have disruption costs which reduce outflow rates temporarily and are an additional (and generally ignored) cost of the change.

We found several striking results that are consistent with the existence of incentive and organization effects. First, we find an increase in the outflow rates of the disabled and unemployed in the long-run (after two years), but the effects are much larger and only significant for the disabled. In the long-run outflows from IB were 6.1% higher and JSA outflows 1.2% higher. Second, there is evidence of important disruption effects with outflow rates falling significantly around the time of re-organization for both types of benefit claimants and then improving as the changes bed down".

A quantitative simulation of the policy suggests that on reasonable assumptions the programme not only pays for itself, but passes a cost-benefit calculation as the short-run costs of disruption and administration are outweighed by the long-run benefits. However, the benefits of the program do take some time to be visible and this poses a problem for policy-makers whose time horizons may be much shorter than a social planner. This reveals the political economy problem at the heart of welfare reform: changes to the administration of the benefit system that have long-run benefits have significant short-run costs and this makes it hard to build up a coalition for change.

There are many directions we want to take this work. In particular, we are concerned about some of the general equilibrium impacts of the policy. For example, Crepon et al. (2012) find that there can be unintended negative externalities as a result of performance standards. Could the increased outflows in the treated areas be at the expense of lower outflows in the non-treated areas? To what extent does the increased labour supply lead to lower equilibrium wages (not just due to compositional changes)? These are areas we are actively engaged in exploring.

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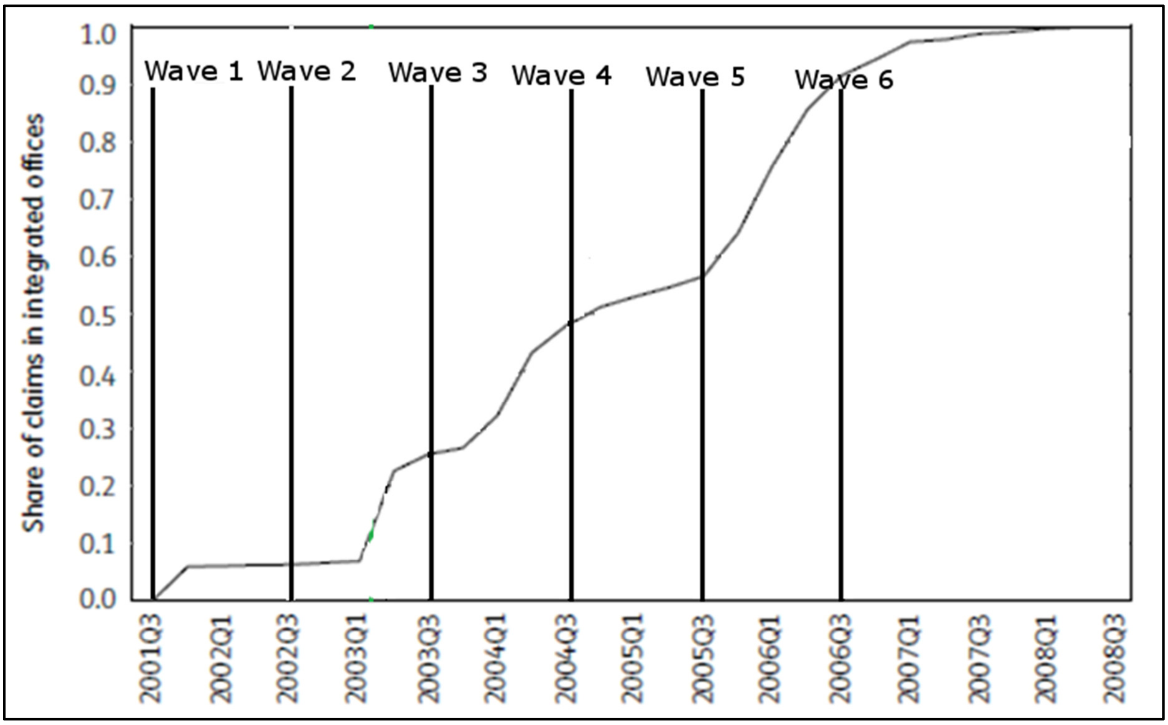
Figure 1: The Staggered Roll-out of the "Jobcentre Plus" Policy



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). In Wave 1 there were 32 districts, in Wave 2 there were 27 districts, in Wave 3 there were 36 districts, in Wave 4 there were 28 districts, in Wave 5 there were 135 districts and in Wave 6 there were 148 districts. 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: NIESR, 2011.

Figure 2
Spatial Map of Diffusion, by Wave

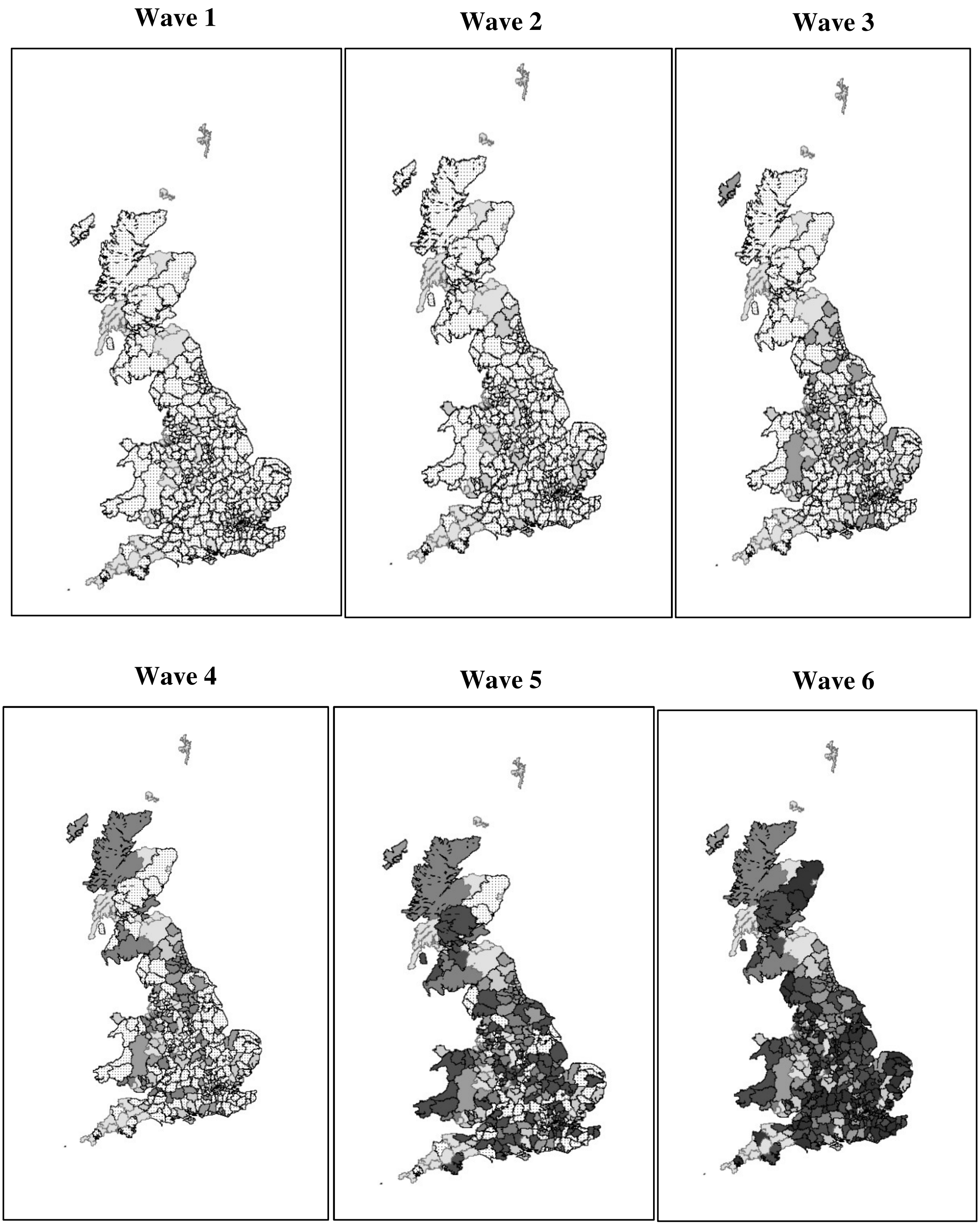
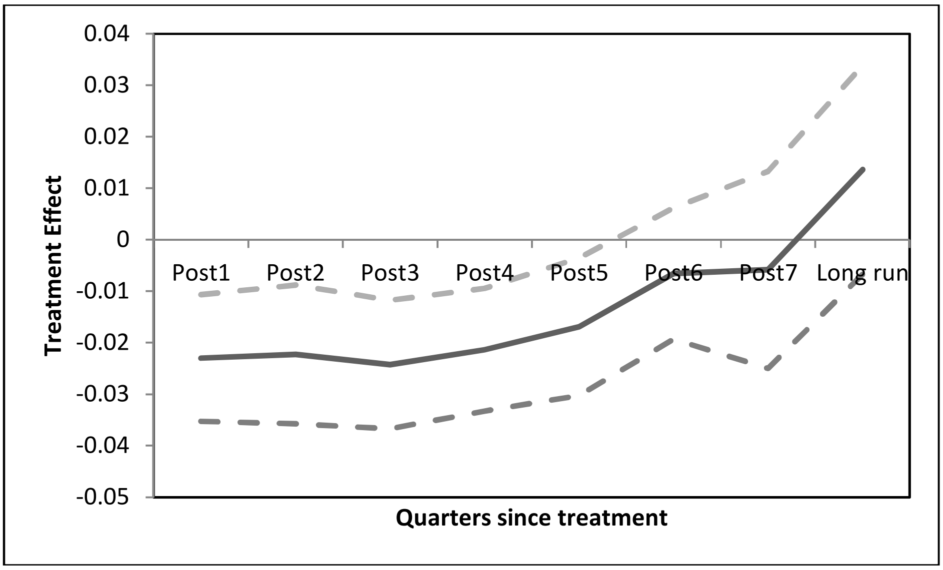
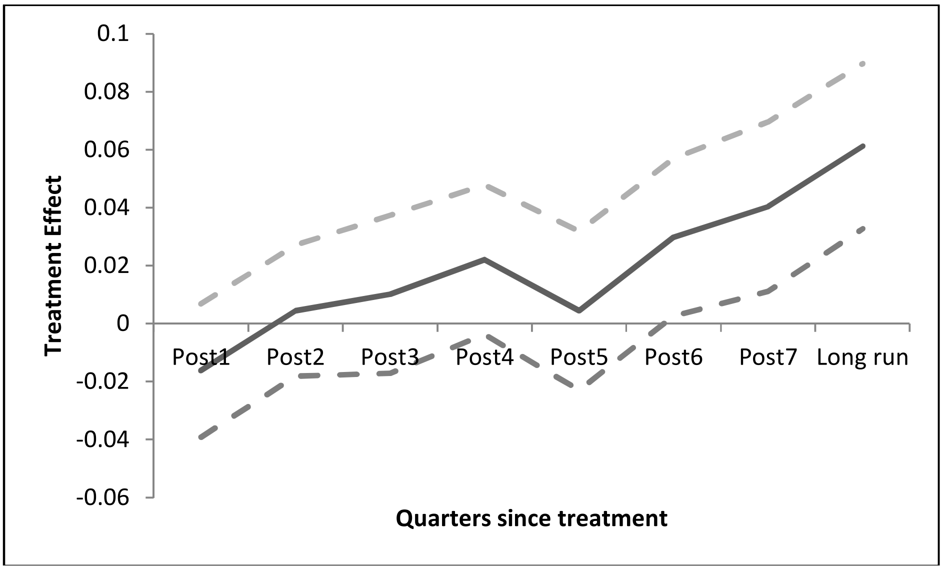


Figure 3: Dynamic Effects on JSA Outflow



Notes: The sample is a panel of 406 districts from 1999Q3 to 2008Q2. The dependent variable is ln(outflow) for JSA. Standard errors are clustered at the district level. The middle line denotes the estimated coefficients for the dynamic specification in equation (2). The top and bottom lines denote the 95% confidence intervals.

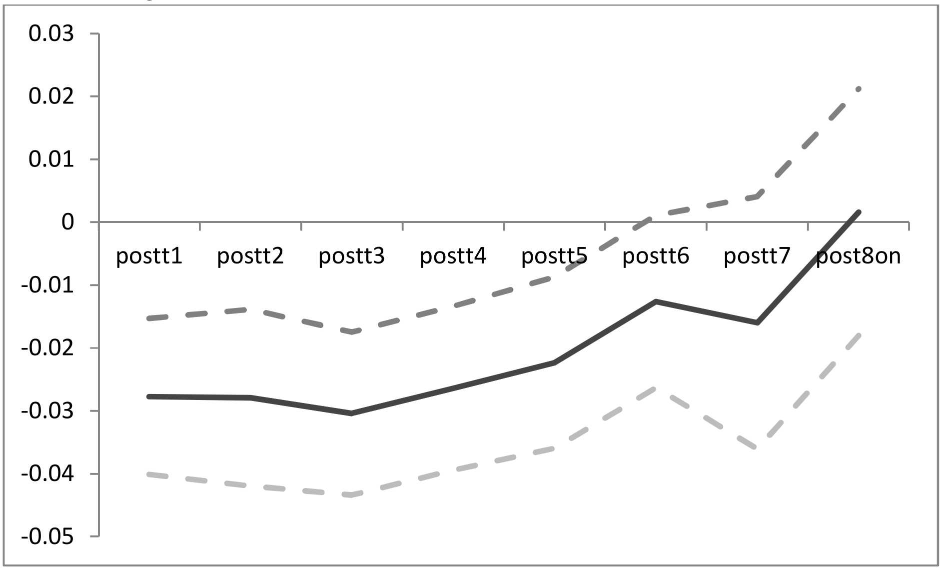
Figure 4: Dynamic Effects on IB Outflow



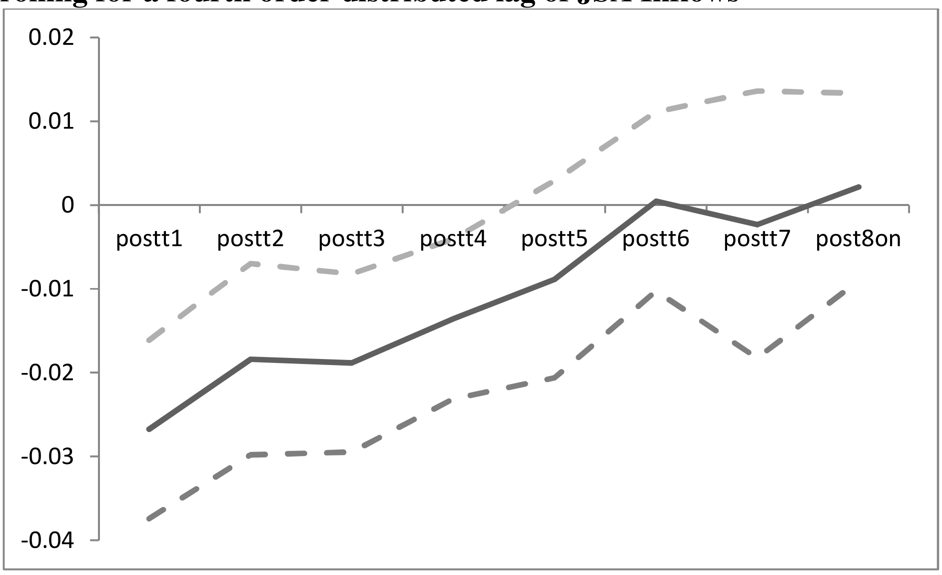
Notes: The sample is a panel of 406 districts from 1999Q3 to 2008Q2. The dependent variable is ln(outflow) for IB. Standard errors are clustered at the district level. The middle line denotes the estimated coefficients for the dynamic specification in equation (2). The top and bottom lines denote the 95% confidence intervals.

Figure 5: Dynamic Effects on JSA Outflows, with and without JSA inflow controls

A: No controls for JSA Inflows



B: Controlling for a fourth order distributed lag of JSA Inflows



Notes: These are the coefficients in Table 6 column (2) and column (3). The dependent variable is ln(outflow) for JSA. Standard errors are clustered at the district level. The middle line denotes the estimated coefficients and the top and bottom dashed lines denote the 95% confidence intervals.

Figure 6: Dynamic Effects on IB Outflows, with and without IB inflow controls

Figure A: No controls for IB Inflows

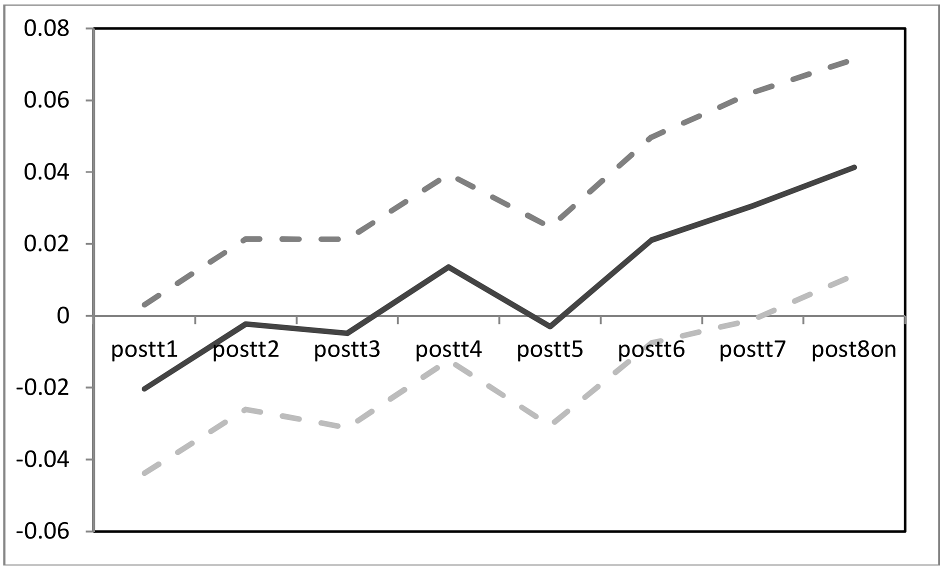
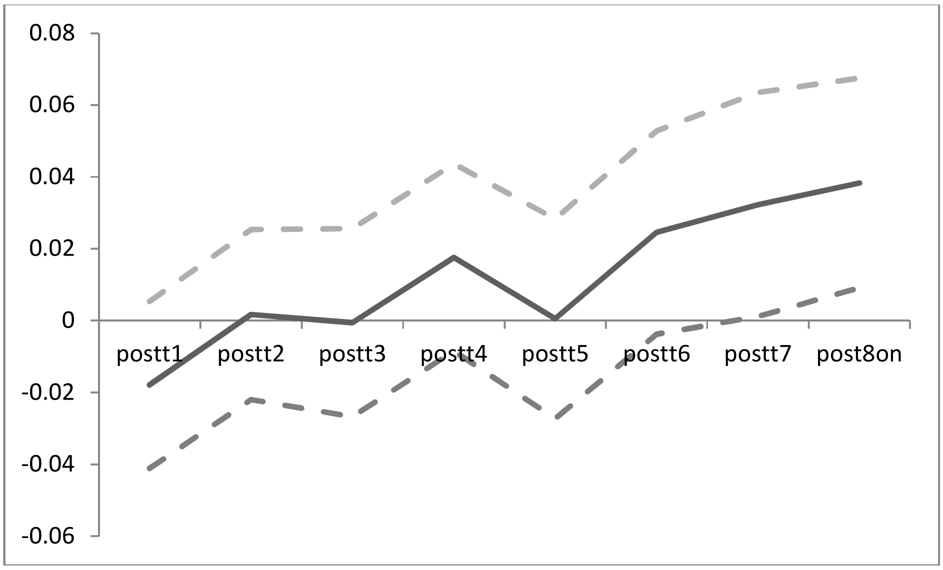
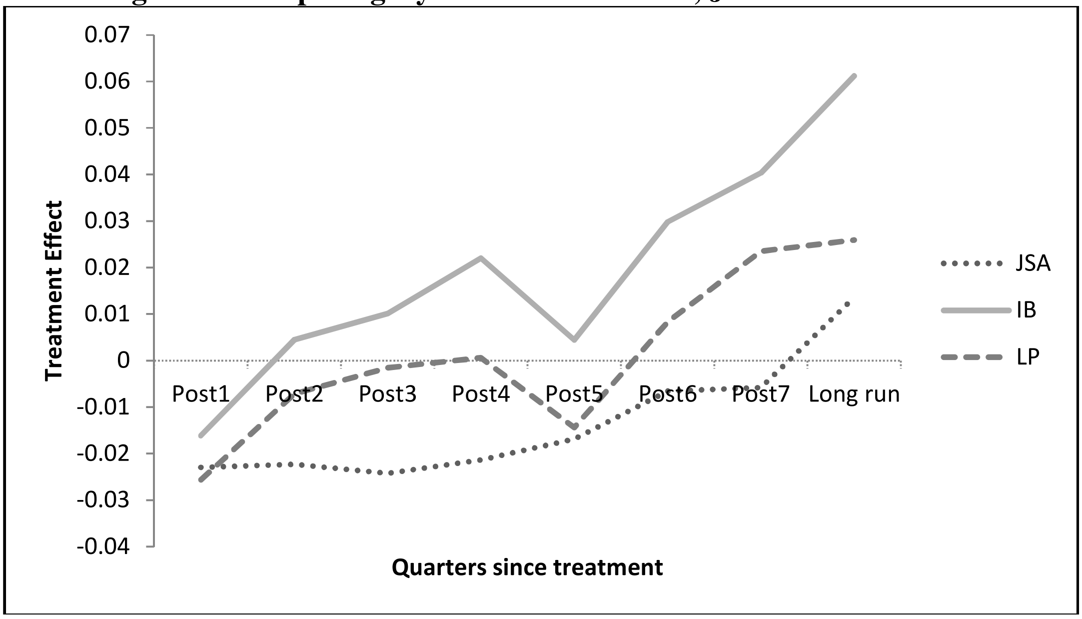


Figure B: Controlling for a fourth order distributed lag of IB Inflows



Notes: These are the coefficients in Table 6 column (5) and column (6). The dependent variable is ln(outflow) for IB. Standard errors are clustered at the district level. The middle line denotes the estimated coefficients and the top and bottom dashed lines denote the 95% confidence intervals.

Figure 7: 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.

Figure 8: Cost-benefit analysis of Jobcentre Plus: Dynamic evaluation

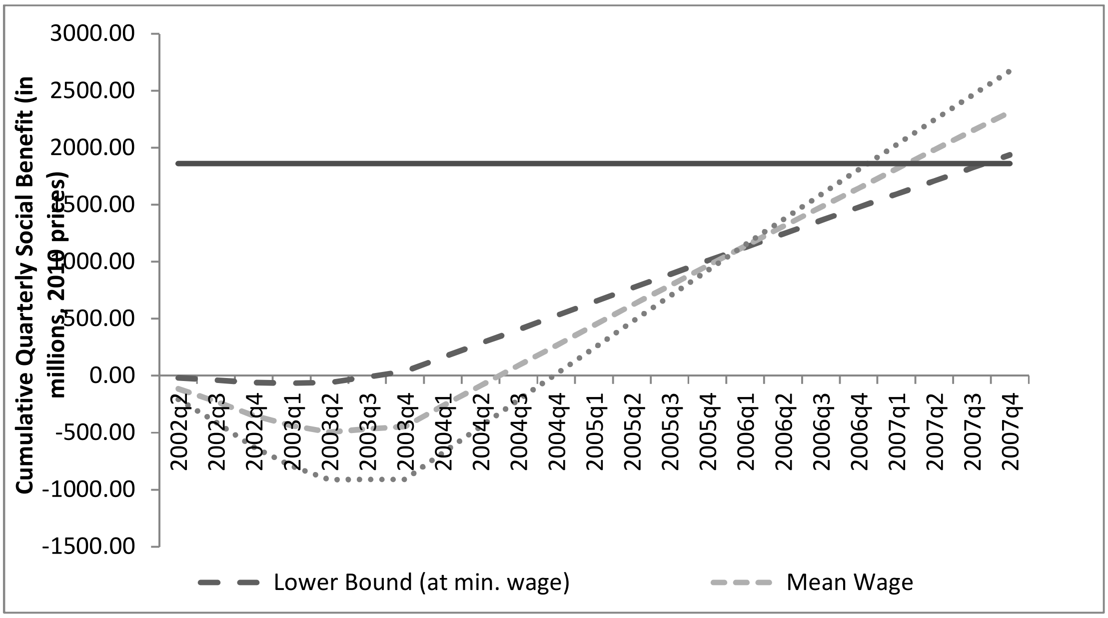


Table 1: Summary Statistics

	Quarterly Aggregate		Unweighted average across all district-age-quarter cells		
	Mean	SD	Mean	SD	Obs
	(1)	(2)	(3)	(4)	(5)
JSA Outflow	653,819	78,049	805	860	29,232
JSA Stock (t-1)	939,267	115,578	1,156	1,650	29,232
JSA Outflow rate (outflow(t)/stock(t-1))	0.698	0.057	0.871	0.266	29,232
JSA Inflow	648,957	58,156	799	843	29,232
JSA Outflow to Work	288,037	61,225	353	384	29,343
JSA Monthly Outflow	216,250	38,567	266	290	84,448
JSA Monthly Stock	916,954	92,173	1,129	1,609	84,448
IB Outflow	56,166	11,267	70	106	29,232
IB Stock (t-1)	2,045,210	356,417	2,567	4,259	29,232
IB Outflow rate (outflow(t)/stock(t-1))	0.028	0.0027	0.048	0.043	29,232
IB Inflow	148,318	12,125	181	241	29,232

Notes: These are descriptive statistics across all districts in our sample for the UK over the period 1999Q3-2008Q2. The first two columns aggregate stocks and flows to the year-quarter level and then average over the 36 quarters in our sample. The last 3 columns present the unweighted average of the district-age-quarter unit of observations used in our analysis.

Table 2: Policy Effects on the JSA and IB Outflows

	Log(Total Outflow)					
	(1) JSA	(2) JSA	(3) JSA	(4) IB	(5) IB	(6) IB
$Post_{it}$	-0.0153***	-0.0152***	-0.0152***	0.0166*	0.0158*	0.0151*
	(0.0054)	(0.0055)	(0.0055)	(0.0089)	(0.0087)	(0.0087)
lnU_{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)
$lnU_{a'it-1}$	-0.0820***	0.0072	0.0105	0.1705***	0.2251***	0.0502*
	(0.0102)	(0.0086)	(0.0097)	(0.0246)	(0.0196)	(0.0256)
N	29,168	29,168	29,168	26,450	26,450	26,450
District*Age FE	NO	YES	YES	NO	YES	YES
Age*Time FE	NO	NO	YES	NO	NO	YES

Notes: Each column estimates equation (1) with the dependent variable as the log of the outflow from benefit during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1, 2 and 3, the dependent variable is ln(outflow) for JSA. In columns 4, 5 and 6, the dependent variable is ln(outflow) for IB. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Dynamic Policy Effects on the JSA and IB Outflows

Dependent Variable: Log(Total Outflow)						
	(1)	(2)	(3)	(4)	(5)	(6)
	JSA	JSA	JSA	IB	IB	IB
$\boldsymbol{Post_1}$	-0.0234***	-0.0229***	-0.0230***	-0.0203*	-0.0154	-0.0162
	(0.0061)	(0.0062)	(0.0062)	(0.0119)	(0.0117)	(0.0117)
$\boldsymbol{Post_2}$	-0.0230***	-0.0230***	-0.0230***	0.0052	0.0047	0.0044
	(0.0068)	(0.0068)	(0.0068)	(0.0119)	(0.0115)	(0.0115)
$\boldsymbol{Post_3}$	-0.0249***	-0.0249***	-0.0249***	0.0112	0.0115	0.0101
	(0.0064)	(0.0064)	(0.0064)	(0.0140)	(0.0139)	(0.0139)
$\boldsymbol{Post_4}$	-0.0208***	-0.0208***	-0.0208***	0.0254*	0.0223*	0.0220*
	(0.0061)	(0.0062)	(0.0062)	(0.0130)	(0.0132)	(0.0131)
$\boldsymbol{Post_5}$	-0.0166**	-0.0166**	-0.0166**	0.0044	0.0045	0.0044
	(0.0068)	(0.0069)	(0.0069)	(0.0143)	(0.0139)	(0.0139)
$\boldsymbol{Post_6}$	-0.0066	-0.0067	-0.0067	0.0370**	0.0309**	0.0298**
	(0.0065)	(0.0066)	(0.0066)	(0.0144)	(0.0138)	(0.0138)
$\boldsymbol{Post_7}$	-0.0077	-0.0076	-0.0076	0.0430***	0.0415***	0.0403***
	(0.0098)	(0.0099)	(0.0099)	(0.0150)	(0.0150)	(0.0149)
$\boldsymbol{Post_{8LR}}$	0.0117	0.0117	0.0117	0.0646***	0.0622***	0.0612***
	(0.0104)	(0.0105)	(0.0105)	(0.0150)	(0.0144)	(0.0145)
$\boldsymbol{lnU_{ait-1}}$	0.7237***	0.6344***	0.6312***	0.2589***	0.1552***	0.3572***
	(0.0095)	(0.0084)	(0.0100)	(0.0288)	(0.0189)	(0.0308)
$\boldsymbol{lnU_{a'it-1}}$	-0.0832***	0.0060	0.0092	0.1808***	0.2351***	0.0595**
	(0.0101)	(0.0085)	(0.0096)	(0.0239)	(0.0191)	(0.0255)
N	29,168	29,168	29,168	26,450	26,450	26,450
District*Age FE	NO	YES	YES	NO	YES	YES
Age*Time FE	NO	NO	YES	NO	NO	YES
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: Each column estimates equation (2) with the dependent variable as the log of the outflow from benefit during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1, 2 and 3, the dependent variable is ln(outflow) for JSA. In columns 4, 5 and 6, the dependent variable is ln(outflow) for IB. 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: Pre-treatment Trends in Benefit Outflow

	Log(Total Outflow)			
	(1) JSA	(2) JSA	(3) IB	(4) IB
$\boldsymbol{Post_{-4}}$		0.0013		0.0162
		(0.0065)		(0.0109)
$\boldsymbol{Post_{-3}}$		0.0039		-0.0135
		(0.0063)		(0.0114)
$\boldsymbol{Post_{-2}}$		-0.0021		0.0053
		(0.0068)		(0.0119)
$\boldsymbol{Post_{-1}}$		-0.0134*		-0.0010
		(0.0079)		(0.0123)
$\boldsymbol{Post_{it}}$	-0.0152***	-0.0168**	0.0151*	0.0160
	(0.0055)	(0.0069)	(0.0087)	(0.0100)
$\boldsymbol{lnU_{ait-1}}$	0.6323***	0.6323***	0.3475***	0.3473***
	(0.0100)	(0.0100)	(0.0314)	(0.0314)
$\boldsymbol{lnU_{a'it-1}}$	0.0105	0.0105	0.0502*	0.0502*
	(0.0097)	(0.0097)	(0.0256)	(0.0256)
N	29,168	29,168	26,450	26,450
District*Age FE	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES
F Test		1.8830		1.3560
p-value		0.1130		0.2480

Notes: Each column estimates equation (3) with the dependent variable as the log of the outflow from benefit during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1 and 2, the dependent variable is ln(outflow) for JSA. In columns 3 and 4, the dependent variable is ln(outflow) for IB. 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: Analysis of Benefit Inflows

	Log(Total Inflow)			
	(1)	(2)	(3)	(4)
	JSA	JSA	IB	IB
$\boldsymbol{Post}$	-0.0063		-0.0054	
	(0.0085)		(0.0072)	
$\boldsymbol{Post_1}$		-0.0155*		-0.0175**
		(0.0081)		(0.0080)
$\boldsymbol{Post_2}$		-0.0327***		-0.0179*
		(0.0081)		(0.0094)
$\boldsymbol{Post_3}$		-0.0170**		-0.0068
		(0.0086)		(0.0088)
$\boldsymbol{Post_4}$		-0.0267***		-0.0064
		(0.0086)		(0.0093)
$\boldsymbol{Post_{5LR}}$		0.0164		0.0043
		(0.0129)		(0.0094)
Ln(population)	0.1441	0.1340	-0.0072	-0.0134
	(0.1278)	(0.1254)	(0.0610)	(0.0609)
N	29,096	29,096	26,727	26,727
District*Age FE	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES

Notes: All columns estimate equation (4) with the dependent variable as the inflow into benefits during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1-4, the dependent variable is ln(inflow). Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Dynamic Policy Effects on JSA and IB Outflows controlling for inflows

	Log(Total Outflow)					
	(1)	(2)	(3)	(4)	(5)	(6)
	JSA	JSA	JSA	IB	IB	IB
$\boldsymbol{Post_1}$	-0.0235***	-0.0267***	-0.0254***	-0.0156	-0.0174	-0.0191
	(0.0054)	(0.0053)	(0.0061)	(0.0118)	(0.0118)	(0.0119)
$\boldsymbol{Post_2}$	-0.0149***	-0.0176***	-0.0256***	0.006	0.0048	0.0014
	(0.0057)	(0.0057)	(0.0068)	(0.0117)	(0.0119)	(0.0119)
$\boldsymbol{Post_3}$	-0.0145***	-0.0178***	-0.0278***	0.0089	0.0074	0.0039
	(0.0056)	(0.0055)	(0.0064)	(0.0140)	(0.0139)	(0.0139)
$\boldsymbol{Post_4}$	-0.0131***	-0.0119**	-0.0237***	0.0291**	0.0283**	0.0255*
	(0.0049)	(0.0046)	(0.0062)	(0.0133)	(0.0135)	(0.0134)
$\boldsymbol{Post_5}$	-0.0073	-0.006	-0.0196***	0.0075	0.0062	0.0036
	(0.0059)	(0.0061)	(0.0068)	(0.0141)	(0.0142)	(0.0141)
$\boldsymbol{Post_6}$	-0.0021	0.0022	-0.0098	0.0334**	0.0312**	0.0293**
	(0.0053)	(0.0051)	(0.0066)	(0.0138)	(0.0139)	(0.0140)
$\boldsymbol{Post_7}$	-0.0018	-0.0015	-0.0123	0.0401***	0.0362**	0.0363**
	(0.0082)	(0.0083)	(0.0099)	(0.0148)	(0.0150)	(0.0152)
$\boldsymbol{Post_{8LR}}$	0.0064	0.0028	0.0062	0.0589***	0.0503***	0.0547***
	(0.0068)	(0.0057)	(0.0103)	(0.0144)	(0.0141)	(0.0145)
$\boldsymbol{ln(Inflow)_{-1}}$	0.4252***	0.3765***		(0.0244)	(0.0286)	(0.0285)
	(0.0200)	(0.0147)		0.0863***	0.0764***	
$\boldsymbol{ln(Inflow)_{-2}}$		0.0773***		(0.0133)	(0.0132)	
		(0.0109)			0.0544***	
$\boldsymbol{ln(Inflow)_{-3}}$		0.0457***			(0.0130)	
		(0.0137)			0.0344***	
$\boldsymbol{ln(Inflow)_{-4}}$		0.0827***			(0.0119)	
		(0.0108)			0.0359***	
$\boldsymbol{lnU_{ait-1}}$	0.3679***	0.3323***	0.6350***		(0.0121)	
	(0.0149)	(0.0117)	(0.0105)	0.3312***	0.2775***	0.3761***
$\boldsymbol{lnU_{a'it-1}}$	0.0069	-0.0109*	0.0033	(0.0307)	(0.0344)	(0.0328)
	(0.0069)	(0.0059)	(0.0092)	0.0493**	0.0231	0.0346
N	28352	25915	25915	24402	22304	22304
District*Age FE	YES	YES	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES	YES	YES

Notes: All columns estimate equation (2) with the dependent variable as the outflow from benefits during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1-3, the dependent variable is ln(outflow) for JSA and the dependent variable in columns 4-6 is the ln(outflow) for IB. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Dynamic Effects on the Outflow to Work

Log(Outflow by destination)				
	(1)	(2)	(3)	(4)
	To Work	To work	Not to work	Not to work
$Post$	-0.0244***		-0.0169***	
	(0.0065)		(0.0065)	
$Post_1$		-0.0320***		-0.0402***
		(0.0074)		(0.0118)
$Post_2$		-0.0163**		-0.0386***
		(0.0078)		(0.0125)
$Post_3$		-0.0375***		-0.0355***
		(0.0090)		(0.0120)
$Post_4$		-0.0218**		-0.0403***
		(0.0090)		(0.0119)
$Post_5$		-0.0351***		-0.0253**
		(0.0107)		(0.0125)
$Post_6$		-0.0139		-0.0238*
		(0.0095)		(0.0130)
$Post_7$		-0.0187*		-0.0122
		(0.0107)		(0.0154)
$Post_{8LR}$		-0.0082		0.0084
		(0.0107)		(0.0145)
lnU_{ait-1}	0.6213***	0.6262***	0.6305***	0.5488***
	(0.0156)	(0.0157)	(0.0121)	(0.0215)
$lnU_{a'it-1}$	-0.0313**	-0.0278**	0.0240**	0.0319*
	(0.0131)	(0.0129)	(0.0110)	(0.0193)
N	28,019	28,019	28,075	28,075
District*Age FE	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES

Notes: Column 1 estimates equation (1) and column 2 estimates equation (2), both with the dependent variable as the log of the outflow from benefit to work during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1, 2 and 3, the dependent variable is log(outflow to work) for JSA. 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 8: Treatment effect in individual waves

WAVE	1	2	3	4	5	6	Baseline
			Panel A: Log(Total Outflow) from JSA				
$\boldsymbol{Post_{it}}$	-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)
$\boldsymbol{lnU_{ait-1}}$	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)
$\boldsymbol{lnU_{a'it-1}}$	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)
N	9727	12967	16207	19448	22688	25928	29168
			Panel B: Log(Total Outflow) from IB				
$\boldsymbol{Post_{it}}$	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)
$\boldsymbol{lnU_{ait-1}}$	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)
$\boldsymbol{lnU_{a'it-1}}$	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)
N	7635	10435	13256	16070	18844	21637	26450

Notes: The dependent variable is the log of the outflow from benefit during a year-quarter. All regressions control for interacted district-age and interacted age-time fixed effects. The regressions restrict the post-treatment period to 4 quarters after each wave. The sample is a panel of 406 districts for each wave. In the upper panel the dependent variable is log(outflow) for JSA. In the lower panel, the dependent variable is the log(outflow) from IB. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Treatment effects on Monthly Outflows from JSA

Log(Total Outflow)		
	(1) JSA	(2) JSA
$\boldsymbol{Post_{it}}$	-0.0162*** (0.0056)	
$\boldsymbol{Post_1}$		-0.0251*** (0.0067)
$\boldsymbol{Post_2}$		-0.0205*** (0.0072)
$\boldsymbol{Post_3}$		-0.0215*** (0.0068)
$\boldsymbol{Post_4}$		-0.0187*** (0.0063)
$\boldsymbol{Post_5}$		-0.0217*** (0.0076)
$\boldsymbol{Post_6}$		-0.0064 (0.0070)
$\boldsymbol{Post_7}$		-0.0061 (0.0088)
$\boldsymbol{Post_{8LR}}$		0.0067 (0.0097)
$\boldsymbol{lnU_{ait-1}}$	0.7099*** (0.0108)	0.7090*** (0.0109)
$\boldsymbol{lnU_{a'it-1}}$	-0.0620*** (0.0103)	-0.0629*** (0.0102)
N	84,202	84,202
District*Age FE	YES	YES
Age*Time FE	YES	YES

Notes: Column 1 estimates equation (1) and column 2 estimates equation (2), both with the dependent variable as the log of the outflow from benefit during a year-month. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from January 1999 to December 2008. The dependent variable is log(outflow) for JSA. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Treatment Effect on Lone Parents

	Log(Total Outflow)				
	(1) LP	(2) LP	(3) LP	(4) LP	(5) LP
$\boldsymbol{Post_{-4}}$		-0.0120	0.0014		
		(0.0112)	(0.0111)		
$\boldsymbol{Post_{-3}}$		-0.0302***	-0.0166		
		(0.0103)	(0.0106)		
$\boldsymbol{Post_{-2}}$		-0.0140	-0.0003		
		(0.0119)	(0.0117)		
$\boldsymbol{Post_{-1}}$		-0.0401***	-0.0255**		
		(0.0119)	(0.0121)		
$\boldsymbol{Post_{it}}$	-0.0131*	-0.0270***	-0.0159*	-0.0096	
	(0.0067)	(0.0087)	(0.0095)	(0.0073)	
$\boldsymbol{Post_{1}}$					-0.0265**
					(0.0110)
$\boldsymbol{Post_{2}}$					-0.0067
					(0.0116)
$\boldsymbol{Post_{3}}$					-0.0016
					(0.0107)
$\boldsymbol{Post_{4}}$					-0.0020
					(0.0118)
$\boldsymbol{Post_{5}}$					-0.0166
					(0.0131)
$\boldsymbol{Post_{6}}$					0.0083
					(0.0115)
$\boldsymbol{Post_{7}}$					0.0211
					(0.0143)
$\boldsymbol{Post_{8LR}}$					0.0247**
					(0.0122)
$\boldsymbol{lnU_{ait-1}}$	0.4819***	0.4845***	0.5529***	0.5523***	0.5535***
	(0.0634)	(0.0632)	(0.0895)	(0.0895)	(0.0898)
$\boldsymbol{lnU_{a'it-1}}$	-0.1921***	-0.1907***	-0.1304*	-0.1307*	-0.1302
	(0.0553)	(0.0550)	(0.0790)	(0.0790)	(0.0793)
N	26,378	26,378	26,378	26,378	26,378
District*Age FE	YES	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES	YES
District Specific Trend	NO	NO	YES	YES	YES
F-Test		3.894	1.757		2.137
P-value		0.00408	0.137		0.0389

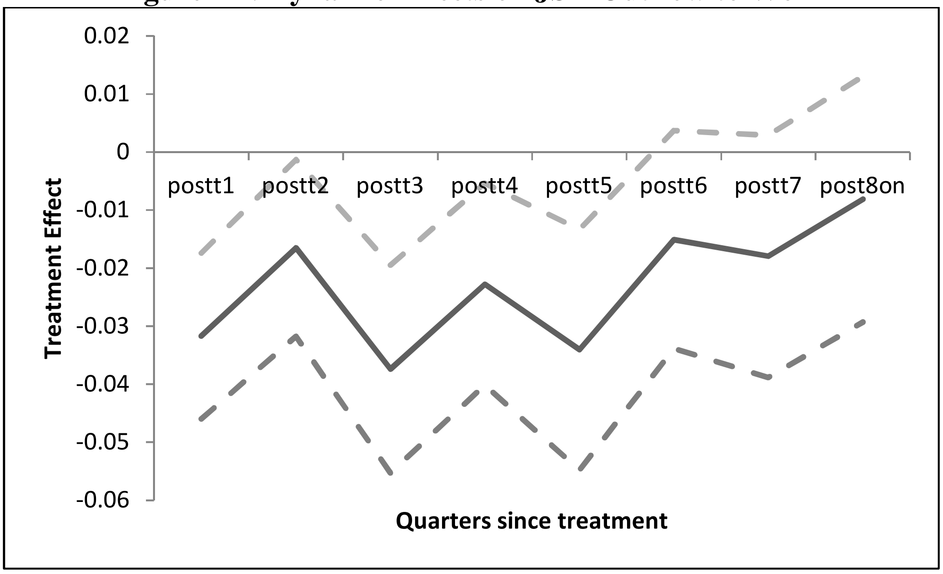
Notes: The dependent variable is the log of the outflow from benefit during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. The dependent variable is ln(outflow) for LP. The last row contains the p-value of 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 11
Cost-benefit analysis of Jobcentre Plus: Steady-state evaluation

	Re-employment earnings		
	Lower bound (min wage)	Mean re-employment earnings after benefits	Upper Bound (median wage)
1. Administration cost in old regime (mln £)	3552	3552	3552
2. Administration cost in new regime (mln £)	3314	3314	3314
3. Annual saving in administrative costs (mln £)	238	238	238
4. Weekly earnings (ASHE 2000)	122.00	250.0	360.0
5. Increase in GDP from wage income (mln £)	26.17	53.62	77.21
6. Deadweight gain (mln £) (30% net exchequer saving)	4.04	7.26	24.98
7. Annual social benefit	268.21	298.88	340.19
8. PDV of social benefit	8940.17	9962.64	11339.56
9. Total JCP Setup Cost	1859	1859	1859
10. Net benefit	7081.17	8103.64	9480.56

Appendices

Figure A1: Dynamic Effects on JSA Outflow to Work



Notes: The sample is a panel of 406 districts from 1999Q3 to 2008Q2. The dependent variable is log(outflow) for JSA. Standard errors are clustered at the district level. The middle line denotes the estimated coefficients for the dynamic specification in equation (2). The top and bottom lines denote the 95% confidence intervals.

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 in column (1) into jobs.

Table A2: Policy Effects on the JSA and IB Outflows from Offices

Log(Total Outflow)		
	(1) JSA	(2) JSA
$\boldsymbol{Post_{-4}}$		-0.0025
		(0.0070)
$\boldsymbol{Post_{-3}}$		-0.0134*
		(0.0079)
$\boldsymbol{Post_{-2}}$		-0.0182**
		(0.0086)
$\boldsymbol{Post_{-1}}$		-0.0550***
		(0.0099)
$\boldsymbol{Post_{it}}$	-0.0545***	-0.0710***
	(0.0056)	(0.0117)
$\boldsymbol{lnU_{ait-1}}$	0.7410***	0.7413***
	(0.0144)	(0.0155)
$\boldsymbol{lnU_{a'it-1}}$	0.1418***	0.1421***
	(0.0167)	(0.0227)
N	48,351	48,351
District*Age FE	YES	YES
Age*Time FE	YES	YES
F Test		17.8300
p-value		0.0000

Notes: The above table is limited to the JSA due to data availability. The first column estimates equation (1) and column (2) estimates equation (3) with the dependent variable as the log of the outflow at the office level from JSA during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 695 offices districts from 1999Q3 to 2008Q2. 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: Treatment effect in Individual waves (dropping previous waves)

WAVE	1	2	3	4	5	6	Stacked
	Panel A: Log(Total Outflow) from JSA						
$Post_{it}$	-0.0196	-0.0314	-0.024	-0.0234	-0.0373	-0.0323	-0.0333
	(0.0135)	(0.0200)	(0.0129)	(0.0183)	(0.0129)	(0.0185)	(0.0073)
lnU_{ait-1}	0.3253	0.341	0.3688	0.388	0.3924	0.387	0.3816
	(0.0182)	(0.0199)	(0.0160)	(0.0167)	(0.0167)	(0.0166)	(0.0159)
$lnU_{a'it-1}$	0.3373	0.2838	0.2582	0.2219	0.2127	0.205	0.2182
	(0.0195)	(0.0197)	(0.0174)	(0.0188)	(0.0194)	(0.0202)	(0.0181)
N	9727	11943	13848	14889	15801	15817	20216
	Panel B: Log(Total Outflow) from IB						
$Post_{it}$	0.0114	-0.0336	-0.0015	-0.0073	-0.0052	-0.0288	-0.0118
	(0.0279)	(0.0376)	(0.0245)	(0.0202)	(0.0228)	(0.0213)	(0.0137)
lnU_{ait-1}	0.2221	0.1464	0.1601	0.1575	0.132	0.1214	0.1593
	(0.0493)	(0.0419)	(0.0384)	(0.0361)	(0.0364)	(0.0354)	(0.0331)
$lnU_{a'it-1}$	1.1741	0.5537	0.356	0.4087	0.4283	0.4151	0.3894
	(0.2219)	(0.1658)	(0.1341)	(0.1219)	(0.1203)	(0.1072)	(0.1055)
N	7635	9556	11209	12016	12594	12331	16533

Notes: The dependent variable is the log of the outflow from benefit during a year-quarter. All regressions control for interacted district-age and interacted age-time fixed effects. The regressions restrict the post-treatment period to 4 quarters after each wave and drop districts treated in previous waves. In the upper panel the dependent variable is log(outflow) for JSA. In the lower panel, the dependent variable is the log(outflow) from IB. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Specification test for dynamic structure

	Log(Total Outflow from Benefit)					
	(1)	(2)	(3)	(4)	(5)	(6)
	JSA	JSA	JSA	IB	IB	IB
$\boldsymbol{Post_1}$	-0.0274***	-0.0260***	-0.0238***	-0.0209*	-0.0190*	-0.0144
	(0.0065)	(0.0064)	(0.0063)	(0.0114)	(0.0114)	(0.0115)
$\boldsymbol{Post_2}$	-0.0282***	-0.0265***	-0.0240***	-0.0041	-0.0019	0.0033
	(0.0072)	(0.0071)	(0.0069)	(0.0115)	(0.0115)	(0.0115)
$\boldsymbol{Post_3}$	-0.0308***	-0.0289***	-0.0261***	-0.0027	-0.0002	0.0058
	(0.0067)	(0.0066)	(0.0065)	(0.0131)	(0.0132)	(0.0133)
$\boldsymbol{Post_4}$		-0.0255***	-0.0222***		0.0079	0.0147
		(0.0065)	(0.0063)		(0.0121)	(0.0123)
$\boldsymbol{Post_5}$			-0.0182***			0.0011
			(0.0068)			(0.0138)
$\boldsymbol{Post_6}$			-0.0085			0.0286**
			(0.0065)			(0.0134)
$\boldsymbol{Post_{4LR}}$	-0.0044			0.0353***		
	(0.0066)			(0.0105)		
$\boldsymbol{Post_{5LR}}$		0.0001			0.0413***	
		(0.0073)			(0.0113)	
$\boldsymbol{Post_{7LR}}$			0.0078			0.0576***
			(0.0089)			(0.0127)
$\boldsymbol{lnU_{ait-1}}$	0.6474***	0.6473***	0.6469***	0.1511***	0.1521***	0.1547***
	(0.0084)	(0.0084)	(0.0084)	(0.0180)	(0.0179)	(0.0177)
$\boldsymbol{lnU_{a'it-1}}$	-0.0013	-0.0015	-0.0019	0.2198***	0.2209***	0.2239***
	(0.0085)	(0.0085)	(0.0085)	(0.0184)	(0.0184)	(0.0183)
N	30,788	30,788	30,788	28,074	28,074	28,074
District*Age FE	YES	YES	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES	YES	YES
P-value	0.0000	0.0000	0.0000	0.0003	0.0003	0.0000

Notes: The dependent variable is the log of the outflow from benefit during a year-quarter. All regressions control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2008Q2. In columns 1, 2 and 3, the dependent variable is log(outflow) for JSA. In columns 4, 5 and 6, the dependent variable is log(outflow) for IB. 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 A5: Robustness to Weighting by District Level Benefit Caseload

	Log(Total Outflow from Benefit)			
	(1)	(2)	(3)	(4)
	JSA	JSA	IB	IB
$\boldsymbol{Post_{it}}$	-0.0251***		0.0091	
	(0.0060)		(0.0088)	
$\boldsymbol{Post_1}$		-0.0288***		-0.0074
		(0.0077)		(0.0089)
$\boldsymbol{Post_2}$		-0.0317***		0.0029
		(0.0079)		(0.0116)
$\boldsymbol{Post_3}$		-0.0340***		-0.0096
		(0.0080)		(0.0107)
$\boldsymbol{Post_4}$		-0.0280***		0.0188
		(0.0067)		(0.0132)
$\boldsymbol{Post_5}$		-0.0283***		-0.0100
		(0.0065)		(0.0128)
$\boldsymbol{Post_6}$		-0.0203***		0.0316**
		(0.0067)		(0.0140)
$\boldsymbol{Post_7}$		-0.0243**		0.0240
		(0.0095)		(0.0167)
$\boldsymbol{Post_{8LR}}$		0.0031		0.0592***
		(0.0087)		(0.0148)
$\boldsymbol{lnU_{ait-1}}$	0.5940***	0.5898***	0.4055***	0.3950***
	(0.0144)	(0.0139)	(0.0522)	(0.0502)
$\boldsymbol{lnU_{a'it-1}}$	0.0560***	0.0561***	0.0106	0.0384
	(0.0135)	(0.0135)	(0.0265)	(0.0239)
N	29,159	29,159	26,450	26,450
District*Age FE	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES
P-value		0.0002		0.0000

Notes: Columns 1&3 estimate equation (1) and columns 2&4 estimate equation (2), both with the dependent variable as the log of the outflow from benefit during a year-quarter. All regressions are weighted by the particular benefit caseload in the district-age group in 1999Q3 (prior to treatment). All estimations control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2007Q4. In columns 1 and 2 the dependent variable is log(outflow) for JSA. In columns 3 and 4, the dependent variable is log(outflow) for IB. 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 A6: Robustness to Different Age Groups (18-54 year olds instead of 18-59)

	Log(Total Outflow from Benefit)			
	(1)	(2)	(3)	(4)
	JSA	JSA	IB	IB
$\boldsymbol{Post_{it}}$	-0.0155***		0.0143*	
	(0.0056)		(0.0087)	
$\boldsymbol{Post_1}$		-0.0223***		-0.0109
		(0.0062)		(0.0118)
$\boldsymbol{Post_2}$		-0.0241***		0.0081
		(0.0070)		(0.0119)
$\boldsymbol{Post_3}$		-0.0265***		0.0068
		(0.0064)		(0.0134)
$\boldsymbol{Post_4}$		-0.0210***		0.0177
		(0.0062)		(0.0125)
$\boldsymbol{Post_5}$		-0.0166**		0.0044
		(0.0070)		(0.0140)
$\boldsymbol{Post_6}$		-0.0069		0.0276*
		(0.0067)		(0.0141)
$\boldsymbol{Post_7}$		-0.0078		0.0434***
		(0.0100)		(0.0155)
$\boldsymbol{Post_{8LR}}$		0.0123		0.0515***
		(0.0106)		(0.0176)
$\boldsymbol{lnU_{ait-1}}$	0.6303***	0.6291***	0.3474***	0.3554***
	(0.0101)	(0.0100)	(0.0314)	(0.0309)
$\boldsymbol{lnU_{a'it-1}}$	0.0112	0.0099	0.0502*	0.0581**
	(0.0098)	(0.0097)	(0.0256)	(0.0256)
N	29,159	29,159	26,450	26,450
District*Age FE	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES
P-value		0.0000		0.0094

Notes: Columns 1&3 estimate equation (1) and columns 2&4 estimate equation (2), both with the dependent variable as the log of the outflow from benefit during a year-quarter. The age groups considered in these regressions are young (18-24) and old (25-54) as opposed to the definition of the older group as 25-59 year olds in all previous tables. All estimations control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2007Q4. In columns 1 and 2 the dependent variable is log(outflow) for JSA. In columns 3 and 4, the dependent variable is log(outflow) for IB. 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 A7: Robustness to Redefining Treatment Dummy

	Log(Total Outflow from Benefit)			
	(1)	(2)	(3)	(4)
	JSA	JSA	IB	IB
$\boldsymbol{Post_{it}}$	-0.0167***		0.0114	
	(0.0055)		(0.0082)	
$\boldsymbol{Post_1}$		-0.0060		0.0122
		(0.0064)		(0.0115)
$\boldsymbol{Post_2}$		-0.0241***		-0.0122
		(0.0066)		(0.0117)
$\boldsymbol{Post_3}$		-0.0243***		0.0099
		(0.0071)		(0.0125)
$\boldsymbol{Post_4}$		-0.0265***		0.0082
		(0.0069)		(0.0135)
$\boldsymbol{Post_5}$		-0.0226***		0.0181
		(0.0066)		(0.0125)
$\boldsymbol{Post_6}$		-0.0186**		0.0010
		(0.0073)		(0.0147)
$\boldsymbol{Post_7}$		-0.0089		0.0271*
		(0.0069)		(0.0142)
$\boldsymbol{Post_{8LR}}$		0.0074		0.0541***
		(0.0103)		(0.0155)
$\boldsymbol{lnU_{ait-1}}$	0.6323***	0.6313***	0.3470***	0.3556***
	(0.0100)	(0.0100)	(0.0314)	(0.0308)
$\boldsymbol{lnU_{a'it-1}}$	0.0105	0.0094	0.0497*	0.0582**
	(0.0097)	(0.0096)	(0.0257)	(0.0255)
N	29,159	29,159	26,450	26,450
District*Age FE	YES	YES	YES	YES
Age*Time FE	YES	YES	YES	YES
P-value		0.0000		0.0015

Notes: Columns 1&3 estimate equation (1) and columns 2&4 estimate equation (2), both with the dependent variable as the log of the outflow from benefit during a year-quarter. All estimations control for district, time and age fixed effects. The sample is a panel of 406 districts from 1999Q3 to 2007Q4. In columns 1 and 2 the dependent variable is log(outflow) for JSA. In columns 3 and 4, the dependent variable is log(outflow) for IB. 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