I'll (Not) be Watching You: Does Job Search Monitoring Intensity Affect Unemployment?

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Abstract

Because welfare reforms tend to couple together changes to job search monitoring and changes to job search assistance, few existing evaluations have been able to isolate the effects of job search monitoring intensity on the level of unemployment. Those few studies that do manage to separately identify changes to monitoring intensity draw mixed conclusions about its impact. This paper exploits an interesting natural experiment – or more precisely a series of natural experiments – to examine the effects of job search monitoring in isolation on unemployment. The natural experiments arise because of the area-by-area roll out of a new policy regime of enhanced job search assistance and job search monitoring called 'Jobs and Benefits' in Northern Ireland. Crucially, in the run up to introducing the new regime, each local benefit office has undergone an often lengthy period of refurbishment or rebuilding during which job search monitoring, but not search assistance, has been withdrawn. These periods of 'excused signing' allow us, therefore, to identify the impact of job search monitoring on unemployment separately from that of job search assistance. The results suggest that removal of monitoring suppresses outflows from unemployment leading to an increase in the number of registered unemployed, i.e. that monitoring 'matters' in isolation from job search assistance.

1. Introduction

One of the problems with unemployment benefits – designed to offer temporary income support to those not in work and searching for jobs – is that they reduce the incentives to engage in job search. Policy makers in the US, UK and elsewhere have responded to this problem by introducing monitoring of the job search activities engaged in by the unemployed, backed up by the threat of benefit sanctions for those not deemed suitably active. If this is the 'stick', the accompanying 'carrot' – and they usually do go together in practice – is offering the unemployed assistance with job search. Search models suggest that such monitoring and/or assistance can increase the probability of leaving (claimant) unemployment (e.g. Fredriksson and Holmlund, 2005; Manning, 2005).[1] Such predictions are generally supported by empirical evidence, although most evidence relates to the impact of monitoring and search assistance taken *together* (e.g. for US reviews see Meyer, 1995; Blank, 2002; for the Netherlands see Gorter and Kalb, 1996). A related literature on benefit sanctions[2] generally finds duration-reducing effects of their imposition that increase in the size of the sanction (e.g. Lalive et al., 2002; van den Berg et al., 2004; Abbring et al., 2005). Although individuals tend to be more closely monitored after imposition of a sanction, however, the impact of this tougher monitoring is not separately identified from the impact of any associated additional job search assistance and the impact of the benefit sanction itself in this literature.

Empirical studies that explicitly seek to *separately* identify the impacts of job search assistance and job search monitoring are less common (see Blank, 2002). Also, where studies have looked for separate job search monitoring effects on unemployment duration they have found contrasting results. Ashenfelter et al. (1999) find no significant job search monitoring effects on unemployment duration in 4 US experiments and argue therefore that the relationships between monitoring/assistance and unemployment duration found by Meyer (1995) and others are driven by job search assistance only. In contrast, Anderson (2001) does find a significant monitoring impact on unemployment

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[1] Manning (2005), however, shows that tougher monitoring may lead the unemployed to cease claiming benefits but still remain unemployed or, because of an induced reduction in search activity, inactive.
[2] Usually – but not only – imposed as a result of insufficient job search (see e.g. Lalive et al., 2002).

duration in 3 alternative US experiments, with the most stringent monitoring regime reducing average claim duration by 10% compared to zero monitoring. She suggests that the Ashenfelter et al. (1999) experiments involved monitoring changes that were just too minor to pick up. In the sanctions literature, Lalive et al. (2002) find *ex ante* effects of benefit sanctions in Switzerland, i.e. that unemployment duration is shorter for non-sanctioned individuals in areas where there is a greater *threat* of sanctions being imposed than in others. They use the rate at which sanction warning letters are issued to proxy the variation in regime toughness[3], but warn that this may be correlated with other differences between local public employment service regimes, e.g. in terms of the level of job search assistance provided. In other words, the degree to which the Lalive et al. (2002) estimates of *ex ante* sanction effects identify variations in monitoring toughness separately from other regime differences is not clear.

This paper exploits an interesting natural experiment – or more precisely a series of natural experiments – to examine the effects both of job search monitoring and job search assistance together, and more unusually the effects of *job search monitoring in isolation*, on unemployment stocks and flows. The natural experiments arise as a result of a new policy regime of enhanced job search assistance and job search monitoring called 'Jobs and Benefits' (J&B), which has been introduced on an area-by-area roll out basis across Northern Ireland (NI) since 1999. Comparing outcomes in areas that have introduced the new regime with outcomes in those areas yet to introduce it gives an estimate of the combined treatment effects of tougher monitoring and increased search assistance. More interestingly, in the run up to introducing the new regime, each benefit office has undergone an often lengthy period of refurbishment during which job search monitoring, but not job search assistance, has been withdrawn. Crucially therefore, these periods of 'excused signing' allow separate identification of the impact of job search monitoring on unemployment.

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[3] They find a one standard deviation increase in warning rate leads to an average reduction in unemployment duration of 5 calendar days.

The motivation and contribution of the paper is threefold. First, as discussed above, it is to isolate the impact of job search monitoring from that of job search assistance. The results show tougher job search monitoring increases outflows from unemployment and reduces the stock of unemployed, with effects of similar magnitude to those found by Anderson (2001). Second, it is to use (non-experimental) data from the UK to add to the existing international empirical literature examining the impact of tougher monitoring and job search assistance together on unemployment. The results here are consistent with this wider literature: stronger monitoring and search assistance together reduce unemployment. The third motivation is to draw out the policy implications of these findings for the J&B policy itself and for welfare reforms more generally.

The remainder of the paper is set out as follows. The following section provides more details on the J&B policy and excused signing periods which are the source of the natural experiments exploited in this paper. Section 3 presents a brief review of the policy context for J&B. Section 4 describes the data and presents simple unconditional difference-in-differences estimates of the treatment effects of excused signing and the new tougher J&B regime on the number of unemployed. Section 5 presents an econometric analysis of the treatment effects on unemployment flows and Section 6 concludes.

2. Jobs and Benefits

J&B is the name given to an ongoing programme of reforms of unemployment benefits (Jobseekers' Allowance, or JSA) and job search assistance services in NI, similar in many respects to the ONE reforms in Britain (see Section 3). The policy co-locates previously separate benefit delivery and job search assistance services in a single 'Jobs and Benefits Office' (JBO), with JSA claimants served in both respects by a single personal adviser. By combining benefits delivery and job search assistance services in a single location and with a single adviser, the purpose of the policy is to strengthen both the monitoring of job search and the quality of job search assistance offered to the unemployed. The policy aims to deliver stricter job search monitoring through tighter

enforcement of existing job search 'contracts' between the job seeker and the benefit adviser (these are called Jobseeker's Agreements and are discussed in Section 3). The effects of the policy on job search assistance are more ambiguous, but arguably the opportunity to repeatedly meet with a single adviser to discuss job search and to work together to overcome any particular barriers to employment the claimant might experience represents an increase in the amount and/or quality of job search assistance provided. Since existing job search assistance services continue to be offered, there is at the very least no reduction in job search assistance as a result of the policy.

For the purposes of benefits delivery and job search assistance services, NI is divided into 35 administrative areas, each served originally by a separate Social Security Office (benefits delivery) and a separate Job Centre (job search assistance). The policy has so far been implemented in 25 of these 35 areas, with each now served by a single JBO. This has been achieved by means of a staged roll-out of the policy, area by area, starting with two pilot JBOs in 1999. At the time of writing, the most recent area to introduce the policy (the 25th) was Coleraine in July 2005. The remaining 10 areas are not due to introduce the policy until 2007. These 10 remaining areas can therefore be used as controls in order to identify the treatment effect(s) using a simple difference-in-differences framework. Further, in the econometric analysis set out in Section 5 it is the staged roll-out of the policy that allows identification of the treatment effect(s) in a regression framework. The ordering of local areas in the roll out schedule is essentially random.[4]

Moving from a situation where a given area's job search assistance and benefit delivery services are delivered in two separate locations to one where they are delivered in a single location requires construction and/or refurbishment work of new or existing office buildings. In order for this to take place, and to a lesser extent in order to hire and train personal advisers for the new regime, normal fortnightly signing is suspended in the run up to implementation of the new policy, on average for a period of 8 months. This

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[4] It is weakly correlated only with the length of the excused signing period, i.e. in drawing up the roll out schedule policy makers appear to have had a slight tendency to put 'easier' offices earlier in the roll out.

fortnightly signing serves the purpose of job search *monitoring*, with JSA claimants required to attend the benefit office and show 'evidence' of search activity in order to receive their benefits, in addition to confirming their availability for work.[5] Although this no longer takes place during these periods, the full range of original job search *assistance* services continues to be offered – although on an entirely unmonitored basis – through Job Centres. These periods of excused signing are therefore periods in which job search monitoring is unambiguously lessened, and arguably zero, and in which job search assistance services are unaffected. It is these periods of excused signing, therefore, that allow identification of the monitoring intensity impact on unemployment.

3. Policy Context

Public job search assistance and benefit delivery services were originally co-located in the UK until their separation in 1974. Staff cuts followed in the deep recession of the early 1980s, with, for example, the dropping of the requirement to attend Job Centres in order to receive unemployment benefits. Since shortly after that time, beginning with Restart introduced in 1987, policy reforms have successively sought to strengthen the monitoring and assistance of job search (Van Reenen, 2003, provides a review). J&B is therefore the latest in a long line of reforms introduced in the UK over the last 20 years or so designed to strengthen the link between receipt of unemployment benefits and job search. One might argue – at least in terms of the co-location of job search assistance services and benefits administration – that the UK has now come full circle since 1974.

In 1987 the Restart programme introduced compulsory six-monthly job search interviews for the unemployed across the UK. Benefit sanctions could be (and in some cases were) applied for non attendance or non cooperation. Dolton and O'Neill (1996) evaluated the impact of Restart on hazard rates for outflows from unemployment by exploiting a rare randomly assigned control group (more typical of the US) that, although eligible, had not been invited to a Restart interview in order to enable unbiased estimation of the treatment

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[5] Such evidence includes, for example, records of visits to the local Job Centre (to avail of job search assistance), copies of job applications submitted and records of interviews attended.

effect. They found attendance at Restart interviews to significantly increase the hazard rates for outflows to employment, and from unemployment to an alternative state of 'not signing on'. Their results can be interpreted as UK evidence – consistent with US evidence reviewed in Meyer (1995) – of the joint impact of enhanced job search monitoring and assistance.

In 1996 the introduction of the JSA brought major reform to the rules governing receipt of unemployment benefits. Claimants were required and assisted to draw up a Jobseeker's Agreement (JSAg) on becoming unemployed that committed them to a programme of job search. This semi-contractual approach was enforced through fortnightly signing requiring unemployed individuals to provide evidence of job search activity in line with their JSAgs, e.g. attendance at the local Job Centre. Benefit sanctions were possible (and imposed in some cases) for those that did not provide evidence of sufficient job search. So the introduction of JSA can also be thought of as boosting both search monitoring and assistance. Given its simultaneous introduction across all parts of the UK for all those eligible, the evaluation of the introduction of JSA adopted a simple before and after approach, based, amongst other things, on a large scale client survey (see Rayner et al., 2000). This evaluation found that the introduction of JSA led to increased job search activity on the part of claimants and a large increase in outflows from unemployment in the first year of operation, with little change in the apparent outflow destinations of claimants. Thereafter effects on outflows were small, suggesting that the main impact of the introduction of JSA may have been one-off unemployment stock clearing. Manning (2005) questions these conclusions, using Labour Force Survey data to argue first that no such job search impact took place (after 'weeding out' claimants with low levels of search the remaining claimants will of course display a higher average level of search), and second that those leaving claimant unemployment as a result of the policy were moving not into employment but into non-claimant unemployment or inactivity.

In 1998 a number of active labour market programmes, of which the biggest was the New Deal for Young People (NDYP), were introduced across the UK. All young people aged 18-24 that had been claiming JSA for 6 months were required to attend a programme

consisting of up to 4 months of one-to-one advice and assistance with job search, followed, for those still unemployed, by compulsory placements in, for example, subsidised employment or education and training. A similar approach was adopted for over 25s, but with eligibility after 18 months rather than 6 months of unemployment. Again benefit sanctions were threatened (and again imposed in some cases) for non-compliance, so the New Deals can be thought of as further UK examples of regimes of increased search monitoring and enhanced assistance. Evaluations have taken various forms, but perhaps the most informative have been those of the NDYP using older age groups as controls (see e.g. Blundell et al., 2001; Riley and Young, 2001a, b; McVicar and Podivinsky, 2003). These evaluations suggested a positive effect of participation on the hazard rate for outflows from unemployment and specifically on outflows from unemployment to employment. A large outflow effect to education and training was also found.

Although J&B is unique to NI, a related policy has been introduced across Britain under the labels 'ONE' and more recently 'Jobcentre Plus'. Like J&B in NI, these reforms, similarly rolling out since pilots in 1999, integrate British job search and benefits services. Again there are benefit sanctions for those not attending regular in depth meetings with their adviser. Evaluation of ONE in GB has taken advantage of 12 pilot areas where the policy was introduced in 1999 in order to identify its treatment effects. There are three important differences between these ONE pilots, however, and the J&B reforms in NI. First, ONE only applied to *new* claimants, so the existing stock of unemployed continued to be served by the old regime of job search monitoring and job search assistance. Second, the ONE pilots predated significant refurbishment work of the kind that led to extended periods of excused signing in NI. Third, ONE also applied, albeit on a voluntary basis initially, to benefit claimants on benefits other than JSA. These design differences mean that evaluations of the ONE pilots can neither identify search monitoring effects in isolation – the primary motivation for this paper – nor the effects of monitoring and assistance together on the *existing* unemployed (the group that appeared to be most affected by the earlier JSA reforms of 1996).

Evidence from JSA client surveys in pilot areas compared to other areas suggested that meetings with personal advisers – carried out in Britain in addition to usual fortnightly signing – had not taken place as often as intended (every 3 months following an initial meeting for new claims), and perhaps partly as a result of this, that there was no statistically significant effect on outflows from unemployment for new JSA claimants (see Green et al., 2003). Evaluation based on administrative benefit records data supports this conclusion of no statistically significant ONE effect on outflows from JSA for new claimants (see Kirby and Riley, 2003). Kirby and Riley (2003) do find some evidence, however, of a temporary *negative* impact of ONE on outflow probabilities for new JSA claimants during the period that the ONE regime was extended from being voluntary to compulsory for claimants of benefits other than JSA. They speculate that this might show a temporary displacement of adviser time and/or job vacancies from JSA to other benefit claimants. Section 4 returns to this point in the context of the NI reforms.

In NI early evaluation of J&B pilots (two local areas – Dungannon and Lisburn – introduced J&B in March 1999 to act as pilots) was much more positive. A simple unconditional difference-in-differences analysis summarised in Deloitte and Touche (1999), selecting broadly similar non-pilot local areas to act as controls, estimated that the introduction of J&B in the pilot areas reduced unemployment levels by around 15%. Most of this fall took place in the first 3 months following the introduction of the policy, and was not subsequently reversed (although the evaluation took place within 6 months of the implementation of the new regime). There was no analysis of the effects of excused signing in the pilot study. Nevertheless, in contrast to the pilot evaluations of ONE in GB – restricted to new claimants – the pilot evaluation in NI suggested that J&B applied to all JSA claimants might turn out to have a significant stock clearing effect. The following two sections explore whether this early promise was fulfilled over the next 6 years of the policy roll-out.

4. Unconditional Difference-in-Differences Estimates

Monthly administrative data are available on unemployment stocks and flows from September 1997 until December 2005 for all 35 JBO areas of NI, i.e. a panel of 3500 monthly JBO level observations. Over this period aggregate registered unemployment in NI has been falling rapidly, from over 60,000 to under 30,000. All parts of NI have seen unemployment fall over this period and it is against this background that J&B has been rolling out. Table 1 lists excused signing (i.e. the period of zero job search monitoring) and J&B implementation ('go-live') dates for the 25 JBO areas that have so far introduced J&B. In all but one case – Londonderry/Foyle, where there is a concurrent phasing out of excused signing and phasing in of the new J&B regime – excused signing runs from the date specified until the J&B implementation date when the new regime is introduced for all claimants simultaneously. J&B implementation dates and dates for the start of excused signing are assigned to the nearest start of month.

<Table 1 around here>

A first pass at estimating the treatment effects of excused signing and J&B can be made by comparing changes in unemployment stocks in those areas where the policy has been introduced with those areas – the 10 remaining areas operating the old system – where it is yet to be introduced, i.e. a simple unconditional difference-in-differences estimator. Table 2 presents the estimated impacts on unemployment stocks of the excused signing periods in each of the 25 JBO areas compared to the 10 control areas. On average these excused signing periods lasted 8 months, although they ranged from 1 to 23 months. Our expectation, because of lack of search monitoring, is that where JSA claimants are excused from fortnightly signing they will be less likely to exit unemployment and therefore that the unemployment stock will increase relative to the controls. The evidence in Table 2 is consistent with this, with an average increase in unemployment of 6% in the zero monitoring JBOs compared to an average fall of 3% in the controls, i.e. a difference-in-differences of +9%. So, subject to the usual caveats about such estimates (e.g. see Card and Krueger, 1995), a first approximation of the impact of moving from the old monitoring regime to zero monitoring during excused signing is that it leads to a 9%

increase in the stock of unemployed. Notice this is similar in magnitude to Anderson's (2001) estimates of monitoring impacts on unemployment.

<Table 2 around here>

At individual JBO level these difference-in-differences estimators range from -5% to +27%.[6] Part of this variation can be explained by variation in the length of the excused signing period[7], but there also appear to be area specific factors, e.g. in the labour market, in the nature of the unemployed, or in the way excused signing is implemented on the ground, that drive variation in these difference-in-differences estimates of treatment effects. The implication is that conclusions based on these types of estimates from a single area (e.g. a pilot) or a small number of areas, if such area specific factors are not properly accounted for, may not themselves generalise well.

Table 3 presents similar difference-in-differences estimates of the impact of the new J&B regime itself. The average change in unemployment stock between the month prior to the introduction of J&B and the latest point of observation (December 2005) for the JBOs operating J&B is -36%. The average change over the same periods in the control areas is -16%. So a first approximation of the treatment effect of J&B – the introduction of tougher monitoring and enhanced job search assistance – is that it leads to a 20% reduction in unemployment. Of course in order to get the 20% fall (the good) you have to first experience the 9% rise (the bad), so the impact of the *overall* policy package is to reduce unemployment by an average of 13% (see Table 4). Again there is a considerable range of difference-in-differences estimates at individual JBO level, which is not obviously related to variation in the length of time that the new regime has been operating.[8]

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[6] Not counting Shaftesbury Square.
[7] The duration of excused signing is positively correlated with the impact on unemployment (ρ=.43), i.e. the longer the period of excused signing, the bigger the impact. Fitting a non-linear trend line to a scatter of JBO excused signing periods and their impacts on unemployment suggests the (cumulative) impact of excused signing grows with the duration of excused signing but at a decreasing rate.

<Table 3 around here>

<Table 4 around here>

The average sizes of these policy impacts – month by month – are shown by Figure 1.[9] As suggested by the pilot evaluation (Deloitte and Touche, 1999), most of the impact of J&B on unemployment occurs in the first 6-9 months following its introduction. There is little sign of this being reversed, at least during the first 30 months of the new regime.

<Figure 1 around here>

In Britain (i.e. the UK excluding NI) the ONE/Jobcentre Plus reforms cover not only JSA claimants but also claimants of other benefits, e.g. Incapacity Benefits (for those not working on grounds of long term sickness or disability) and Income Support (means tested benefits for those not working but with insufficient work history to be eligible for JSA). At the same time as the new regime for JSA claimants is introduced in an area, so too is enhanced job search assistance – with a compulsory element – for claimants of these other benefits. In NI this regime change for other benefit claimants is known has Enhanced Jobs and Benefits (EJ&B). The difference is that EJ&B only began to be introduced – again according to a staged roll-out across areas – in 2003, i.e. long after the first 10 JBOs had begun to operate J&B for JSA claimants. From then on, however, J&B and EJ&B have rolled-out simultaneously, just as in Britain. Kirby and Riley (2003) speculate that extending such services to non-JSA claimants in Britain may have been detrimental to JSA claimants because, in practice, staff time was diverted away from JSA claimants themselves and also from relationships with client firms that are the source of notified vacancies, to other benefit claimants. Added to this is the possibility of vacancy displacement if other benefit claimants compete more effectively with JSA claimants for

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[8] Both linear and non-linear trend lines on a scatter of J&B impacts and duration of operation are poorly determined.

[9] Figure 1 assumes an average length excused signing period (8 months) and the average period of time for which the new HJ&B regime has been in operation (31 months).

a limited number of vacancies and there are decreasing returns to scale in the matching function.

The timing of the NI roll-outs of J&B and EJ&B provide an opportunity to test Kirby and Riley's suggestion by comparing estimated average treatment effects for the first 10 JBOs to introduce the policy (separately from EJ&B) to those subsequently introducing J&B (together with EJ&B). The post-treatment period is restricted to six months in each case in order to account for different operating durations. For the first 10 JBOs the average difference-in-differences estimator of the treatment effect of J&B (including excused signing) is -15%. For the next 15 JBOs it is -9%. So, this evidence is consistent with Kirby and Riley's (2003) suggestion. In other words, perhaps because of staff time and/or vacancy displacement, it does appear that J&B introduced separately has a *bigger* impact on JSA claimants than J&B that is introduced as part of a wider benefits reform package.

5. Treatment Effects in an Aggregate Matching Function and a Combined Flows Model

Initial indications of the sign and size of the treatment effects of both excused signing (zero monitoring) and the new J&B regime (tougher monitoring and enhanced assistance) on the number of unemployed are given by the simple unconditional difference-in-differences estimates set out in the previous section. These difference-in-differences estimates show that the move from standard levels of search monitoring to zero monitoring leads to an increase in unemployment of 9% on average, and that the move from the zero monitoring to the new regime of tougher monitoring and enhanced search assistance leads to a fall in unemployment of 20% on average. The combined impact of these treatments is an average fall of 13% in unemployment. This section examines these treatment effects in more detail through simple econometric analysis of the unemployment *flows* that together drive the unemployment stock.

Our primary interest – given a search framework – is in estimating the treatment effects of excused signing and J&B on monthly *outflows* from unemployment. A simple

difference-in-differences estimator of the impact of J&B on the number of outflows, comparing the year before excused signing to the year following implementation of the new J&B regime, is that they are increased by an average of 11%. Figure 2, however, shows that such summary estimates of treatment effects may not be particularly informative given the time pattern – most of the impact of J&B on outflows takes place in the first 6 months of operation and the impact of excused signing on outflows is larger in the early part of excused signing periods – of the impacts.

An alternative empirical approach is to estimate average treatment effects in the context of an aggregate matching function (see Petrongolo and Pissarides (2001) for a recent review of the empirical matching literature). Each month in each JBO a number of unemployed job seekers will match with local job vacancies and move into employment, with the number of matches increasing with the number of unemployed and the number of vacancies. Vacancy stock data (for notified vacancies only), which is the usual measure of vacancies applied in the random matching literature, are only available for NI from April 1999. Data on the flow of new (notified) vacancies, however, are available for the entire study period. On the basis that most notified vacancies are either filled within a month or remain unfilled the matching function outlined below is estimated on these vacancy flow data as if they referred to stocks.[10] The measure of the number of matches used is simply the number of outflows from unemployment.[11]

Equation (1) gives the resulting aggregate matching function:

(1) $$M_{it} = m(U_{it-1}, V_{it}, J\&B_{it}, ES_{it}, EJ\&B_{it}, \mu_i, \tau_t)$$

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[10] Coles and Petrongolo (2003) note that 30% of UK notified vacancies are filled on the first day. The matching function is also estimated on the vacancy stock data for the shorter period, with little difference in results.

[11] This is something of a simplification, since not all outflows from JSA are to employment. In fact, around 50% of exits are recorded as being to employment and around 50% of the remainder are thought to be to employment, although not recorded as such. This inaccuracy in the recording of destinations of unemployment leavers makes more specific measures of the number of matches, at least those that are available for NI over this period, unreliable.

where M_{it} is the number of matches in area i in month t, which depends on the stock of unemployed job seekers at the start of the month (U_{it-1}), the number of vacancies (V_{it}), policy dummies for J&B, excused signing (ES) and EJ&B, and JBO and time fixed effects, according to functional form *m*. The functional form is assumed to be log linear, as is the case in much of the matching literature. Such aggregate matching functions have been estimated on panel data before (e.g. Anderson and Burgess, 2000; and again see Petrongolo and Pissarides, 2001). What is different here – apart from new data – is that the focus is not on the parameters of the matching function *per se* but on the estimated treatment effects of job search monitoring and assistance captured by the J&B and excused signing dummies.

The indication from Figure 1 is that the impact of both excused signing and of J&B may be stronger in the early months than in subsequent months. In order to investigate this further, excused signing and J&B dummies are specified separately for each month of excused signing and each month of operation of J&B. Single dummies are specified for all months of excused signing beyond the 8th month and all months of J&B beyond the 24th month. These month-specific policy dummies are identified, despite the presence of time fixed effects, because of the area by area nature of the policy roll-out.

Equation (1) is estimated, assuming fixed effects, jointly for both genders across all age groups. Results are presented in Table 5.[12] Estimated time and group fixed effects are omitted for presentational purposes.

<Table 5 around here>

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[12] A Hausman test suggests that Equation (1) could also be estimated as a random effects model. These results are not reported, however, given that there is little difference in the estimated policy impacts of either excused signing, J&B or EJ&B, or in overall fit, between the two specifications. Testing suggests further that estimates may be inefficient because of groupwise heteroskedasticity, cross sectional correlation and serial correlation of residuals (e.g. see Greene, 1993). Inference is largely unaffected by ignoring these properties in the estimates presented in Table 5, however, since all policy dummies are significant in any case. Nevertheless, for completeness Equation (1) estimated by Feasible Generalised Least Squares specifying groupwise heteroskedasticity, cross-sectional correlation and common AR(1) autocorrelation. The results of this exercise display very similar policy impacts, although standard errors are marginally lower as might be expected.

As one might expect with such a panel structure the model 'explains' a high degree (84%) of the variability in monthly outflows from unemployment. The estimated coefficient on unemployment has the correct sign and is within the usual range in the literature at 0.57. Vacancies, however, despite statistical significance, do not play any role in explaining the number of matches.[13] This contrasts with the usual results in the matching literature (where vacancies are generally found to have a significant coefficient of around 0.3). There are two reasons for this. First, a large part of variation in vacancy flows follows a common seasonal and temporal pattern across administrative areas. In a panel matching function framework this variation is captured by the time fixed effects.[14] Second, the poor quality of the vacancy data remains a concern, and may act to bias the vacancy coefficient downwards. Despite these concerns, the tentative conclusion is that there are decreasing returns to scale in the matching function.

The estimated coefficients for all the excused signing dummies are negative. In other words, moving from the old regime of job search monitoring to zero monitoring has a *strong negative* impact on outflows from unemployment. Just as with the unconditional difference-in-differences estimates, this suggests – in line with Anderson (2001) and contrary to Ashenfelter et al. (1999) – that monitoring *does* matter in isolation from job search assistance. The size of this effect is greatest in the second month of excused signing, with outflows 24% below what they would have been in the absence of excused signing, and falls off subsequently so that by the ninth month of excused signing outflows are only 13% below what they would have been otherwise.

The estimated coefficients on all the J&B dummies are positive: the new regime of tougher monitoring coupled with enhanced job search assistance increases outflows. Again the size of this effect is greatest in the second month following go live, with outflows increased by 33%.[15] The positive impact on J&B on outflows persists, however,

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[13] This is true whether flow data or stock data are used for vacancies.

[14] Anderson and Burgess (2000), for example, omit time fixed effects from their panel matching model. Omitting time fixed effects from Equation (1) here gives a vacancies coefficient of 0.15, i.e. well within the usual range.

[15] Remember our flow data observed at time t refer to the month running up to time t and excused signing and go live dates are assigned to the nearest start of month. It is therefore possible for some of the first

albeit with a smaller size, even beyond 24 months. In other words, J&B causes what appears to be a step change in monthly outflows by between 10% and 15%, other things being equal. In the first 7 months the effect is larger. It is likely that the strong initial impact is picking up a stock clearing effect, e.g. weeding out fraudulent or no-search claims, similar to that discussed by Manning (2005) in the case of the introduction of JSA in 1996. Periods of excused signing prior to the introduction of the new regime will intuitively increase the number of such claims, which may mean that their number is artificially high when the new regime is introduced. The fact that treatment effects on outflows are still significant even after 2 years, however, is consistent with an ongoing impact of J&B for new claims – in contrast to the findings of Kirby and Riley (2003) – in addition to the stock clearing effect.

The unemployment *stock* implications of these treatment effects on outflows can be explored using the identity that the unemployment stock in month t is equal to unemployment in month t-1 plus inflows in month t-1 minus outflows in month t-1, i.e.

$$(2) \qquad\qquad U_{it} \equiv U_{it-1} + I_{it-1} - M_{it-1}$$

where I_{it-1} denotes inflows to unemployment in area i in month t. Assume for now that inflows are constant. This is shown in Figure 3. Like Figure 1 – based directly on the stock data – Figure 3 shows a rapid increase in the liveload during excused signing (although reaching a peak of 20% above its original level rather than 9% as shown in Figure 1) and then a rapid fall as J&B itself is introduced, stabilising 6-9 months after the implementation month. After one year, the unemployment stock is around 10% below its original level before excused signing. This is close, although slightly smaller, to the difference-in-differences estimate based on the stock data discussed above, which suggests that the observed impacts of excused signing and J&B on the unemployment stock are largely driven by impacts on outflows.

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month of excused signing or J&B to correspond to a period where the policy is yet to operate. We might therefore expect the 'peak' in impacts in the second month.

The pattern shown in Figure 3 is driven by a combination of the direct policy impacts on outflows (negative in the case of excused signing and positive in the case of J&B) and the indirect policy impact on the stock itself because of the direct flow effects in earlier months. These indirect effects act to dampen the overall policy impact on outflows: in the case of excused signing the direct negative effect on outflows leads to a higher stock which in turn has a positive impact on outflows in the next month; in the case of J&B the direct positive effect on outflows leads to a lower stock which in turn has a negative impact on outflows in the next month. This indirect effect on outflows through the reduced stock is the explanation for the slight falling off of the J&B impact on the stock after 18 months or so shown in Figure 3. If Figure 3 is extended further beyond the implementation of J&B (e.g. into a fourth year and beyond), the unemployment stock appears to continue to head back towards its original pre excused signing level. This may signal that the unemployed 'cleared out' in the early months of the new regime drift back into unemployment over time, although such an exercise is perhaps making too much of the data currently available.[16]

EJ&B – the related policy for non-JSA claimants – enters the model significantly, with an apparent one-off 'push' to JSA outflows in its first month followed by a smaller, but still statistically significant, negative impact on outflows in the following 11 months. This econometric evidence is consistent with the descriptive evidence discussed in the previous section that suggests EJ&B may have a detrimental effect on JSA clients, either through displacement of adviser attention away from JSA claimants, or away from client firms in which JSA claimants are routinely placed, or perhaps through crowding out of a limited number of such vacancies.[17] In Britain, where the two policies have been introduced side by side under the ONE/Jobcentre Plus reforms, we might therefore expect a smaller estimated treatment effect even on similar JSA claimants.

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[16] Only 10 JBOs have a run of 30 months or more following implementation of J&B and only 2 JBOs (the early pilots) have a run of 4 years or more.

So far there is a pretty clear indication that excused signing – moving to zero job search monitoring – *reduces* outflows from unemployment and *increases* the unemployment stock. There is also a pretty clear indication that J&B – moving from zero to tougher monitoring coupled with enhanced search assistance compared to the previous regime – *increases* outflows from unemployment and *reduces* the unemployment stock. This appears to be the case whichever way one cuts the data, although there is some difference in the estimated sizes of these impacts between the two methods. A potential explanation for these differences is that excused signing and/or J&B also affects *inflows* to unemployment (so far they have been assumed constant).

Much of the welfare to work programme evaluation literature ignores potential impacts of search assistance and search monitoring on inflows, but there are a variety of reasons why such impacts might exist. If the increased outflows resulting from J&B are to worse job matches, for example, then subsequent inflows might increase. Even if matches are no worse on average following J&B, the increase in the number of outflows in previous months may lead to a subsequent increase in inflows simply because the at risk employed population is bigger. Inflows might also increase if the new job search assistance regime is perceived by potential claimants as a better service, thus encouraging new claims for those that might not otherwise claim. Spatial spillover effects are also possible where JSA claimants may switch the area in which they sign on for benefits in order to avoid tougher monitoring of job search.[18] This could lead to increased inflows in areas adjacent or close to those introducing the new regime. Simple difference-in-differences estimators of the impact of J&B on the number of inflows, although they tell us nothing about spatial spillovers or about the timing of policy impacts, suggest little impact of J&B, but perhaps a small negative effect of excused signing, on inflows.

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[17] Another possibility to be explored in a follow-up paper is that EJ&B will make switching from JSA to other benefits less attractive because monitoring is intensified for all benefits. This might reduce that part of the J&B impact that acts through increased outflows to other benefits.

[18] There are no regulations to stop this happening although there are likely to be travel costs involved, at least outside the major urban areas where JBOs are in close proximity to one another.

These issues can be explored in more depth by specifying a simple reduced form equation for monthly inflows as follows[19]:

$$(3) \qquad I_{it} = f(E_{it-1}, R_{it}, J \& B_{it}, ES_{it}, EJ \& B_{it}, J \& B_{jt}, \mu_i, \tau_t)$$

where E_{it-1} is the non-unemployed (at risk) population, R_{it} is notified redundancies, and $J \& B_{jt}$ is a dummy for the introduction of the new regime in (urban) area j geographically adjacent to area i. Equation (3) is estimated together with Equation (1) as a pair of Seemingly Unrelated Regressions (SUR), with results presented in Table 6.[20]. As before fixed group and time effects are included in each equation but omitted from Table 6 for presentational purposes. There is very little difference between the estimated outflows equation when estimated separately or when estimated as part of the SUR model together with the inflows equation.

<Table 6 around here>

The inflows equation shows up mostly significant excused signing impacts, with monthly inflows lower during excused signing by around 10%, at least from the third month of excused signing. In other words moving to zero monitoring appears to reduce inflows to registered unemployment. A possible intuition for this is that workers are less likely to enter unemployment either because of a perceived lack of service or because registering for unemployment benefits is – or is perceived to be – more difficult due to construction/refurbishment.[21] There is no clear statistically significant impact of J&B itself – the new tougher regime – on inflows. The inflows equation, however, does show a small but statistically significant impact of EJ&B on inflows to JSA, with inflows appearing to fall following the introduction of EJ&B. This may be because where additional job search assistance services are made available to claimants of other benefits

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[19] A search model with endogenous job separation rate dependent on the policy dummies could provide motivation for such an approach.
[20] Right hand side variables in both equations are treated as exogenous but the errors in Equations (1) and (3) are allowed to be correlated.
[21] During this time it will not be possible to register at the local benefits office or JBO.

the incentives to switch to JSA from such benefits are reduced, i.e. these other benefits become more like *de facto* unemployment benefits themselves. The other result of interest from the inflows equation is that J&B introduced in an *adjacent* JBO area, at least within the urban centres of Belfast and Londonderry, increases inflows to JSA over the following year. It appears that some claimants cease to claim in JBOs operating the new J&B regime and sign on instead in nearby JBOs not yet operating the new regime. The simple difference-in-differences estimates of the overall impact of J&B on the unemployment stock presented in the previous section will therefore slightly overestimate the size of the policy effect because of this spillover effect: part of what appears to be a reduction in unemployment as a result of J&B is in fact just a reallocation of JSA claimants from one JBO to another.

The excused signing impact on inflows implies that the estimated impact of J&B on unemployment stocks derived from the outflows model and the identity linking stocks and flows, as shown in Figure 3, will overestimate the increase in unemployment during excused signing (remember inflows were assumed constant for Figure 3). The results from the combined model shown in Table 6 therefore imply a modified path for the average unemployment stock as shown in Figure 4. This shows a less dramatic increase in the unemployment stock during excused signing and a slightly larger fall in the liveload as a result of J&B. These results are very close to those from the simple difference-in-differences analysis presented in the previous section, i.e. an average excused signing impact on the unemployment stock of around +9% and an average J&B impact on the stock of unemployed of around -12% after one year.

<Figure 4 around here>

6. Concluding Remarks

The paper set out to make three contributions. The first was to estimate the impact of changes in the intensity of job search monitoring in isolation from any changes in job search assistance. Few existing studies have been able to do this because changes to

monitoring and search assistance usually go hand in hand in welfare reforms. The J&B reforms in NI, however, and in particular the periods of excused signing that accompany these reforms, offer a rare opportunity to separately identify monitoring intensity effects. The paper shows – however one cuts the data – that tougher job search monitoring *on its own* increases outflows from unemployment and reduces the stock of unemployed, with effects of similar magnitude to those found by Anderson (2001).

Second was the desire to use (non-experimental) data from the UK to add to the existing international empirical literature examining the combined impact of tougher monitoring and job search assistance on unemployment. This literature, much of it from the US, suggests increasing job search assistance and toughening job search monitoring together can lead to significant reductions in unemployment duration (and therefore on unemployment stocks). The paper shows this is also the case in a UK context: tougher monitoring and enhanced search assistance increases outflows from unemployment, at least temporarily, and leads to a fall in the stock of unemployed that is not subsequently reversed, at least within the first 3 years or so following the reforms.

Third was the desire to draw out the policy implications of the empirical findings. As a straightforward evaluation exercise of an individual welfare reform the paper suggests that J&B has had some success in its stated aim of helping people off unemployment benefits and reducing benefits expenditure. The caveats to this are that unemployment rises during periods of excused signing in the run up to implementation of the new regime, and that some of those helped out of unemployment as a result of the policy may end up drifting back into unemployment over time. Viewed as a whole package, however, the J&B reform leads to a net reduction in JSA expenditure. If the roll out is to be extended in NI to the remaining 10 local areas, used here as controls, policy makers might want to look for an alternative to excused signing during refurbishment of offices, or at least speed up such refurbishment. More generally, there is an indication here that reformed processes for other benefit claimants may have detrimental impacts on unemployment claimant outflows, perhaps because of staff or vacancy displacement. More generally still, the implication of the paper is that reforms strengthening monitoring

can reduce claimant unemployment and *vice versa*. Recent moves in the UK and elsewhere to toughen search monitoring would therefore appear to be sensible – if reducing claimant unemployment is the aim, and if the resource cost of doing so is not too high – in the light of this evidence.

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Figure 1: Average Cumulative % Excused Signing and J&B Impact on Number of Unemployed by Month of Operation

Notes: ES1 refers to the first month of excused signing, ES2 to the second month of excused signing and so on. 'Go live' refers to the first month of operation of the new J&B regime, go live +1 refers to the second month of operation and so on.

Figure 2: Average Percentage Impact on Outflows and Outflow Rates by Month of Operation

Figure 3: Average Cumulative % Excused Signing and J&B Impact on Unemployment Stock by Month of Operation, According to Outflow Impacts, Inflows Held Constant, Fixed Effects Estimates

Figure 4: Average Cumulative % Excused Signing and J&B Impact on Unemployment Stock by Month of Operation, According to Combined Outflows-Inflows Model

Table 1: The Roll-out Schedule for J&B, with 'Go Live' and Excused Signing Dates

Social Security Office	Start of excused signing (month/year)	Go-live Date (month/year)
Dungannon	1/99	3/99
Lisburn	1/99	3/99
Lisnagelvin	10/01	3/02
Magherafelt	11/01	4/02
Ballymoney	1/02	5/02
Portadown	12/01	6/02
Foyle	1/02	6/02
Knockbreda	4/02	10/02
Falls Road	3/02	10/02
Newtwonabbey	3/02	2/03
Omagh	9/02	7/03
Kilkeel	2/03	7/03
Newry	8/02	9/03
Shankill Road	1/03	10/03
Enniskillen	1/03	12/03
Limavady	5/03	2/04
Antrim	4/03	3/04
Shaftesbury Square	8/03	4/04
Lurgan	6/03	5/04
Holywood Road	11/02	9/04
Larne	12/03	11/04
Carrickfergus	12/03	11/04
Banbridge	6/04	3/05
Armagh	6/04	4/05
Coleraine	5/05	7/05

Table 2: Net and Percentage Change in JBO Unemployment during Excused Signing

JBO Area	JBO Net Change (% Change) in Unemployment Stock	10 Control JBOs Net Change (% Change) in Unemployment Stock	% Point Difference
Dungannon	+47 (+3%)	+144 (+1%)	+2%
Lisburn	+127 (+9%)	+144 (+1%)	+8%
Lisnagelvin	-98 (-7%)	-344 (-3%)	-4%
Magherafelt	+26 (+4%)	-18 (0%)	+4%
Ballymoney	+41 (+5%)	+120 (+1%)	+4%
Portadown	-44 (-7%)	-224 (-2%)	-5%
Foyle	+126 (+4%)	-126 (-1%)	+5%
Knockbreda	+169 (+19%)	-175 (-2%)	+21%
Falls Road	+77 (+5%)	-393 (-4%)	+0%
Newtownabbey	+257 (+22%)	-530 (-5%)	+27%
Omagh	-52 (-5%)	-962 (-9%)	+4%
Kilkeel	+11 (+7%)	+22 (0%)	+7%
Newry	+162 (+9%)	+130 (+1%)	+8%
Shankill Road	+207 (+19%)	+319 (+3%)	+16%
Enniskillen	-85 (-5%)	-215 (-2%)	-3%
Limavady	+121 (+19%)	+276 (+3%)	+16%
Antrim	-5 (-1%)	+127 (+1%)	-2%
Shaftesbury Square*	-203 (-19%)	-537 (-5%)	-14%
Lurgan	-34 (-4%)	-117 (-1%)	-3%
Holywood Road	+237 (+18%)	-913 (-9%)	+27%
Larne	-21 (-4%)	-722 (-8%)	+4%
Carrickfergus	+89 (+15%)	-722 (-8%)	+23%
Banbridge	+60 (+16%)	-122 (-1%)	+17%
Armagh	+40 (+6%)	-986 (-10%)	+16%
Coleraine	-19 (-1%)	-168 (-2%)	+1%
Average	+60 (+6%)	-249 (-3%)	+9%

Notes: Industrial action at Shaftesbury Square meant neither excused signing nor the new J&B regime was fully implemented at the recorded dates. The averages reported in the table therefore omit Shaftesbury Square and are unweighted. Excused signing periods for Dungannon and Lisburn are assumed to be three months (dates are unavailable).

Table 3: Net and Percentage Change in JBO Unemployment between Go Live and December 2005

JBO Area	JBO Net Change (% Change) in Unemployment Stock	10 Control JBOs Net Change (% Change) in Unemployment Stock	% Point Difference
Dungannon	-1121 (-71%)	-6608 (-45%)	-26%
Lisburn	-941 (-59%)	-6608 (-45%)	-14%
Lisnagelvin	-256 (-21%)	-2213 (-22%)	+1%
Magherafelt	-282 (-46%)	-2059 (-20%)	-26%
Ballymoney	-349 (-38%)	-1946 (-19%)	-19%
Portadown	-126 (-21%)	-1579 (-16%)	-5%
Foyle	-863 (-26%)	-1600 (-17%)	-9%
Knockbreda	-451 (-43%)	-1884 (-19%)	-24%
Falls Road	-523 (-32%)	-1884 (-19%)	-13%
Newtownabbey	-614 (-42%)	-1687 (-17%)	-25%
Omagh	-347 (-31%)	-1280 (-14%)	-17%
Kilkeel	-72 (-46%)	-1280 (-14%)	-32%
Newry	-868 (-43%)	-1873 (-19%)	-24%
Shankill Road	-399 (-31%)	-1577 (-16%)	-15%
Enniskillen	-553 (-36%)	-1043 (-11%)	-25%
Limavady	-174 (-23%)	-1500 (-16%)	-7%
Antrim	-221 (-24%)	-1407 (-15%)	-9%
Shaftesbury Square*	-220 (-18%)	-1206 (-13%)	-5%
Lurgan	-245 (-29%)	-1099 (-12%)	-17%
Holywood Road	-668 (-43%)	-971 (-11%)	-32%
Larne	-139 (-27%)	-321 (-4%)	-23%
Carrickfergus	-202 (-29%)	-321 (-4%)	-25%
Banbridge	-106 (-25%)	-609 (-7%)	-18%
Armagh	-254 (-35%)	-421 (-5%)	-30%
Coleraine	-188 (-15%)	-217 (-3%)	-12%
Average	-424 (-36%)	-1750 (-16%)	-20%

Notes: Industrial action at Shaftesbury Square makes the date of full implementation of the new J&B regime uncertain. The averages reported in the table therefore omit Shaftesbury Square and are unweighted.

Table 4: Net and Percentage Change in JBO Unemployment between Month Prior to Excused Signing and December 2005

JBO Area	JBO Net Change (% Change) in Unemployment Stock	10 Control JBOs Net Change (% Change) in Unemployment Stock	% Point Difference
Dungannon	-1074 (-70%)	-6464 (-44%)	-24%
Lisburn	-814 (-55%)	-6464 (-44%)	-11%
Lisnagelvin	-354 (-27%)	-2561 (-24%)	-3%
Magherafelt	-256 (-43%)	-2077 (-20%)	-23%
Ballymoney	-300 (-35%)	-1726 (-18%)	-17%
Portadown	-170 (-27%)	-1803 (-18%)	-9%
Foyle	-737 (-23%)	-1726 (-18%)	-5%
Knockbreda	-282 (-32%)	-2059 (-20%)	-12%
Falls Road	-446 (-28%)	-2279 (-22%)	-6%
Newtownabbey	-357 (-30%)	-2217 (-22%)	-8%
Omagh	-399 (-34%)	-2242 (-22%)	-12%
Kilkeel	-61 (-41%)	-1258 (-13%)	-28%
Newry	-707 (-38%)	-2225 (-22%)	-16%
Shankill Road	-192 (-18%)	-1258 (-13%)	-5%
Enniskillen	-638 (-39%)	-1228 (-13%)	-26%
Limavady	-53 (-8%)	-1224 (-13%)	+5%
Antrim	-216 (-25%)	-1280 (-14%)	-11%
Shaftesbury Square*	-496 (-34%)	-1743 (-18%)	-16%
Lurgan	-279 (-31%)	-1216 (-13%)	-18%
Holywood Road	-431 (-33%)	-1884 (-19%)	-14%
Larne	-160 (-30%)	-1043 (-11%)	-19%
Carrickfergus	-113 (-19%)	-1043 (-11%)	-8%
Banbridge	-46 (-13%)	-731 (-8%)	-5%
Armagh	-214 (-32%)	-1407 (-15%)	-17%
Coleraine	-207 (-16%)	-385 (-5%)	-11%
Average	-360 (-31%)	-1982 (-18%)	-13%

Note: The averages reported in the table are unweighted.

Table 5: Matching Function Estimates of Policy Impacts on Aggregate Outflows

	Coefficient	Standard Error
Lag unemployment	.571**	.024
Vacancies	-.012**	.005
Excused signing month 1	-.151**	.028
Excused signing month 2	-.246**	.029
Excused signing month 3	-.220**	.030
Excused signing month 4	-.208**	.031
Excused signing month 5	-.164**	.032
Excused signing month 6	-.220**	.034
Excused signing month 7	-.180**	.037
Excused signing month 8	-.103**	.038
Excused signing month 9 plus	-.125**	.024
J&B 1st month	.179**	.037
J&B 2nd month	.326**	.029
J&B 3rd month	.254**	.029
J&B 4th month	.208**	.029
J&B 5th month	.177**	.029
J&B 6th month	.211**	,030
J&B 7th month	.201**	.030
J&B 8th month	.159**	.030
J&B 9th month	.134**	.030
J&B 10th month	.154**	.031
J&B 11th month	.179**	.031
J&B 12th month	.158**	.031
J&B 13th month	.114**	.031
J&B 14th month	.167**	.031
J&B 15th month	.148**	.032
J&B 16th month	.137**	.032
J&B 17th month	.077*	.033
J&B 18th month	.148**	.033
J&B 19th month	.118**	.033
J&B 20th month	.149**	.033
J&B 21st month	.094**	.034
J&B 22nd month	.140**	.036
J&B 23rd month	.145**	.037
J&B 24th month	.096*	.038
J&B month 25+	.116**	.015
EJ&B 1st month	.135**	.036
EJ&B 1st year	-.050**	.013
R^2 within	.812	
R^2 between	.874	
R^2 overall	.839	
No. obs	3332	

Notes: Variables that are statistically significant at 95% are marked with * and at 99% with **. Some observations drop out due to missing data in some series. JBO and time fixed effects are omitted from the Table for presentational purposes.

Table 6: SUR Estimates for Outflows and Inflows

	Outflows Coefficient	Outflows Standard Error	Inflows Coefficient	Inflows Standard Error
Lag unemployed	.501**	.022		
Lag non-unemployed			.246	.190
Vacancies	-.008	.005		
Redundancies			.005**	.002
Excused signing 1st month	-.149**	.028	.068*	.028
Excused signing 2nd month	-.242**	.028	-.019	.029
Excused signing 3rd month	-.214**	.029	-.084**	.030
Excused signing 4th month	-.201**	.030	-.139**	.031
Excused signing 5th month	-.156**	.031	-.130**	.032
Excused signing 6th month	-.213**	.033	-.126**	.034
Excused signing 7th month	-.171**	.036	-.142**	.037
Excused signing 8th month	-.095*	.037	-.068	.039
Excused signing 9th month on	-.115**	.023	-.078**	.024
J&B 1st month	.184**	.036	-.039	.037
J&B 2nd month	.327**	.028	.061*	.029
J&B 3rd month	.252**	.029	.073*	.030
J&B 4th month	.203**	.029	.040	.030
J&B 5th month	.171**	.029	.011	.030
J&B 6th month	.204**	.029	.052	.030
J&B 7th month	.192**	.029	.005	.030
J&B 8th month	.148**	.029	.059*	.030
J&B 9th month	.129**	.029	.001	.031
J&B 10th month	.143**	.030	.006	.031
J&B 11th month	.167**	.030	.067*	.031
J&B 12th month	.146**	.030	.058	.031
J&B 13th month	.102**	.030	.044	.031
J&B 14th month	.156**	.030	.017	.031
J&B 15th month	.135**	.031	.033	.032
J&B 16th month	.125**	.031	.014	.032
J&B 17th month	.064*	.032	.038	.033
J&B 18th month	.137**	.032	-.004	.033
J&B 19th month	.106**	.033	.040	.033
J&B 20th month	.138**	.033	.064	.033
J&B 21st month	.082*	.034	.012	.034
J&B 22nd month	.127**	.035	.022	.036
J&B 23rd month	.132**	.036	-.017	.037
J&B 24th month	.081*	.037	.072	.038
J&B 25th month on	.103**	.015	-.001	.015
EJ&B 1st month	.135**	.035	.035	.036
EJ&B 1st year	-.050**	.013	-.038**	.013
J&B adjacent 1st month (urban)			.008	.030
J&B adjacent 1st year (urban)			.071**	.012
R²	.948		.941	
No. obs	3332		3332	

Notes: Variables that are statistically significant at 95% are marked with * and at 99% with **. Correlation of residuals: $\rho=0.26$. Breusch-Pagan test of independence (of residuals) rejected chi2(1)=217.4.