Unemployed couples: the labour market effects of making both partners search for work

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Abstract

This paper evaluates the effect of a recent change to unemployment benefit in the UK requiring both partners in a couple (rather than just one) to search for work. The difference-in-differences estimator is extended in two ways. First, variations in when the change was implemented are exploited to test and adjust for bias resulting from differential trends among the comparison group. Second, the approach is combined with matching to relax functional form restrictions. After some months, positive effects on benefit exit were detected but effects on job entry were less apparent. Mostly, the effect operated through the female partner.

JEL classification: C14, C21, J64, J68
Keywords: unemployment, matching, difference-in-differences, treatment effects

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Introduction

For many years, the unemployed have been the main focus of employment policy in the UK. As an illustration of this, active labour market programmes have traditionally focused almost exclusively on encouraging individuals to move from unemployment into work. More recently, there has been increased emphasis on the economically inactive. Lone parents, the disabled and partners of benefit claimants have all been targeted by 'New Deals', the principal form of active labour market programme in the UK.

In addition to these voluntary programmes, a change to the legislation surrounding unemployment benefit claims for certain couples was introduced in March 2001. Whereas previously only one partner within a couple was required to look for work, the change in legislation now required both partners to do so or be faced with benefit sanctions. This paper considers what effect this has had. In particular, the interest is in the effect that it has had on newly-unemployed couples.

The evaluation is based on a difference-in-differences (DiD) approach. However, there are two significant extensions to this. The first exploits the fact that there were variations in how soon the legislation was implemented in practice. These delays were unintended and varied in length both within and between local offices. They offer the possibility of

testing the assumptions underlying the DiD approach in a similar spirit to the pre-programme tests of Heckman and Hotz (1989). Such a test can provide a reassurance as to the suitability of the comparison group. Clearly, this is a crucial consideration since the DiD approach relies on this suitability for identification of the effect. Moreover, the results of the test are used to adjust the DiD estimator to take account of possible biases introduced by inadequacies of the comparison group.

The second adaptation is to combine the DiD approach with propensity score matching. This has the advantage that some of the functional form restrictions inherent in linear models are avoided, and the possible problems associated with changes in the composition of the treatment group can be addressed. These points are discussed further below. The estimation results contribute to the still very small empirical literature on this approach, particularly when the data used are repeat cross-section rather than longitudinal.

The results suggest that the effects of the legislation took some time to materialise but that, about five months after its introduction, Joint Claims was acting to encourage couples to exit benefit. It seems likely that administrative complications associated with the introduction of the legislation were responsible for the delay in observing an effect. The employment effects were less evident although it appears that these were beginning to emerge. Furthermore, it seems that where the legislation had encouraged the couple to exit worklessness, this had been mostly due to the female partner finding work.

The remainder of the paper proceeds as follows. In section I, the institutional framework is set out. This provides an overview of the unemployment benefit regime in the UK along with an account of trends in worklessness which provided the impetus for the legislation. The economic and econometric frameworks are set out in Section II. The data are described in Section III. Section IV presents the main results and Section V concludes.

I. CONTEXT FOR THE LEGISLATION

The main form of unemployment benefit in the UK is Jobseeker's Allowance (JSA). It is payable once a fortnight and in the 2001/2 tax year amounted to £42 per week for those aged 18-24 without dependent children. For couples, the amount payable was roughly double at £83.25 (if both partners are aged over 18). It is essentially payable for an unlimited period, although during certain employment programmes (for example, the New Deal for Young People) it may be replaced with an allowance of equal value. This is essentially an administrative artefact and the JSA payment resumes upon exiting the programme for those unsuccessful in finding work.

Receipt of JSA is conditional on actively seeking and being available for work. Failure to satisfy these requirements can result in withdrawal of benefit. Historically, for couples dependent on JSA, there was a distinction between the claimant and the non-claimant partner. Only the claimant partner was obliged to meet the labour market requirements; nothing was expected of the other partner. However, legislation introduced on 19 March 2001 changed this for certain couples. The distinction between claimant and non-claimant partner was removed and both were now required to actively seek and be available for work. Importantly, not all couples were affected. Specifically, only those couples with no dependent children and where at least one partner was aged over 18 and born after 19 March 1976 were affected. At the time of introduction, this age criterion translated into those couples with at least one partner aged between 18 and 24 years.

The new arrangement was titled 'Joint Claims for JSA' (hereafter, 'Joint Claims') and was introduced with the specific aim of addressing the problem of workless households. Giving equal status to both partners means that the job search assistance provided to JSA claimants is now extended to both partners in a Joint Claims couple. It also extended the threat of sanctions for those not complying with their obligations. The combined effect is intended to bring a group of individuals closer to the labour market with the aim of increasing the chances of a couple finding employment and leaving benefit.

As noted, Joint Claims was introduced to address the growth in workless households in the UK. Over the last thirty years, there has been an increased tendency for households to be either 'work-rich' (all adults in work) or 'work-poor' (no adults in work) with the intermediate status of a mix between working and non-working adults becoming increasingly rare. As a dramatic illustration of this, Gregg et al. (1999) showed that the proportion of households with nobody in work almost tripled from a level of 6.5 per cent in 1975 to 17.9 per cent in 1998. Going back further, the rates are even lower. Over the same period, the proportion of households where all adults were in work rose from 56 per cent to 63 per cent. In 1996 the UK had the fourth highest rate of workless households out of all the OECD countries. The level of polarisation was higher than in *any* OECD country. All this is set against the backdrop of the lowest level of unemployment in the UK for more than twenty years.

There are reasons why this is an important development. From the macroeconomic perspective, received wisdom suggests there is a relationship between the extent to which

unemployment is concentrated among certain groups and the extent to which it is effective in reducing wage pressure (Layard et al., 1991). Hence, an even spread of unemployment maximises its inflation-quelling efficiency. There are also concerns at the household and individual level. Earnings are the main generator of wealth and households without work are more likely to be poor. In 1996 some 70 per cent of workless households had less than half mean household income. Furthermore, unemployment can have scarring effects. Gregg (2001) shows that British men experiencing unemployment when young are likely to endure long-term labour market disadvantage as a result. Arulampalam (2001) and Gregory and Jukes (2001) show that this scarring effect is also evident when considering wages: unemployment imposes a penalty on future earnings.

An obvious question to ask is why this increase in worklessness arose. One possibility is that it simply reflects a demographic change. The increased prevalence of single adult households will, by definition, increase polarisation. However, this only accounts for a fraction of the trend that has been seen. Dickens et al. (2000) show that only a third of the observed polarisation can be explained by changing household composition. The bulk of observed polarisation is accounted for by different underlying factors. This is clear when considering a single type of household. For couples, 10.4 per cent of those without children and 7.5 per cent of those with children were workless in 1996. This represents a huge rise on the corresponding proportions in 1968: 2.7 and 1.6 per cent respectively (Gregg et al., 1999).

Some further insight is possible. Two notable labour market trends over the past twenty

or so years have been the increase in female participation and the increase in male

inactivity. If these transitions do not take place in tandem within the household,

increased polarisation must result. Further examination of the trends shows that, despite

the overall rise in women's employment, there has been little change for those partnered

with jobless men (Desai et al., 1999). Almost all the increase has been among those with

working partners. The main increases in male inactivity, on the other hand, have been

among those aged over 50. Taken together, these trends are unlikely to both be found

within a single household. Consequently, polarisation has resulted.

II. THE ECONOMIC AND ECONOMETRIC FRAMEWORK

Economic background

Economic theory suggests that secondary earners within a household are more likely to

seek work if the primary worker becomes unemployed. This is the 'added worker' effect

(AWE) and it operates through two channels. First, assuming leisure to be a normal

good, the income effect associated with the drop in household income increases the

likelihood of participation for other household members. Second, there is a substitution

effect since the primary worker's nonmarket time can be substituted for that of the

secondary worker.

However, empirical studies in the U.S. have struggled to find evidence of such an effect (for example, Speltzer, 1997; Maloney, 1991). There are a number of reasons why this may be unsurprising. First, preferences for work among partners of those who become unemployed may differ from partners of those who do not become unemployed. Second, if the job loss resulted from a general worsening of economic conditions, the employment prospects of the dependent partner may be similarly affected. This is the 'discouraged worker effect'. Related to these two points is the possibility that, due to assortative mating, partners share similar labour market characteristics. Hence, difficulties experienced in finding work are likely to be shared by both partners. Theoretically, while the direction of the AWE will remain the same, the reasons listed above may mean that it is smaller and more difficult to detect.

The situation in the UK is no different. Davies et al. (1992) show that wives of unemployed men are less likely to be employed than wives of employed men and suggest the assortative mating argument may carry the most explanatory weight for this. An insight into the dynamics of the relationship is provided by Pudney and Thomas (1992), who consider the labour market transitions made by the wives of men who had entered unemployment nine months earlier. Their results suggest that the effect of the husband losing his job is a large drop in his wife's desire to participate in the labour market. While demographic characteristics appeared to be the dominant influence on wives' participation, evidence of strong complementarity between the leisure of husbands and wives was found.

Another possibility is that the social security system acts as a disincentive for married women to engage in paid employment when their husbands are unemployed (see, for example, Kell and Wright, 1990). Essentially, spousal labour supply can be viewed as insurance against unemployment (Ashenfelter, 1980; Heckman and MaCurdy, 1980; Lundberg, 1985). However, welfare benefits themselves may serve to mitigate against the added worker effect as they lessen the income effect of job loss. Cullen and Gruber (2000) find strong evidence in the USA supporting the view that unemployment insurance crowds out spousal labour supply. The change to the unemployment benefit legislation considered in this paper can be seen as an attempt to address this crowding-out effect. The secondary worker is encouraged to look for work with the threat of benefit withdrawal for non-compliance.

Econometric framework

This evaluation proceeds using a difference-in-differences (DiD) approach (see, for example, Blundell and MaCurdy, 1999). The policy change is viewed as a natural experiment and the aim of the analysis is to estimate how outcomes for those affected by the introduction of the legislation differ from what they would have been had the legislation not been introduced. The resulting parameter is the average effect of treatment on the treated and, in the DiD framework, is identified by comparing changes in the treatment group with changes over a similar time period among a group of non-

treated individuals who are in some sense comparable. More will be said about the
nature of this comparability below.

The properties of the DiD estimator are well-known. In this evaluation, participation is
mandatory so for an outcome Y and exogenous variables X, and assuming the policy
change to take place at time 0, the relationship of interest can be written:

(1) $Y_{it} = X_{it}\,\beta + d_{it}\,\alpha + U_{it}$

where i indexes the individual, t is a time subscript, d_{it} is an indicator taking value 1 for
the eligible group when t>0, 0 otherwise. The parameter of interest is α. Since
participation is mandatory for those in the eligible group, d_{it} is sufficient to denote
treatment for those in the eligible group.

U_{it} is assumed to have the following components:

(2) $U_{it} = \phi_i + \theta_t + \mu_{it}$

where ϕ_i is the individual fixed effect, θ_t is a common temporal effect and μ_{it} is an
individual time-varying error independent of X and the other error components. The DiD
estimator requires observations both before and after the intervention for both the
treatment group and a comparison group. First differencing removes the fixed effects and
differencing across the treatment and comparison groups removes the trend effects,

yielding the desired parameter. Abstracting from regressors apart from the treatment

indicator, the estimator can be written:

$$(3) \qquad \hat{\alpha}_{\text{DiD}} = (\bar{Y}_t^{\,T} - \bar{Y}_\tau^{\,T}) - (\bar{Y}_t^{\,C} - \bar{Y}_\tau^{\,C})$$

where the superscripts T and C denote treatment and comparison groups respectively and

t and τ are time subscripts such that $\tau<0<t$. The success of this approach relies on three

key assumptions. First, the differenced temporal individual-specific effects must be

independent of the participation decision. Writing

$$(4) \qquad E\,(\hat{\alpha}_{\text{DiD}}) = \alpha + E\,(\,\mu_{it} - \mu_{i\tau} \mid d{=}1\,) - E\,(\,\mu_{it} - \mu_{i\tau} \mid d{=}0\,),$$

it is clear that unless the two expectation terms on the right hand side are identical (the

constant bias assumption), the resulting estimator will be biased (Ashenfelter, 1978;

Heckman and Smith, 1999). A scenario often used to illustrate the possibility of such a

bias is the case of individuals participating in a training programme. Should enrolment in

the programme be more likely following a temporary pre-programme dip in earnings, the

DiD estimator will overstate the effect of the programme since higher earnings would be

expected among the treated even without participating (assuming earnings to be mean-

reverting). In the context of mandatory participation this is less of a concern than it

would be for a voluntary programme.

The second assumption is that the temporal effect is common to both the treatment and

the comparison group. If instead

(5) $E(U_{it}|\ d_{it}\ ) = E(\phi_i\ |\ d_{it}\ ) + k_g\theta_t$

where k_g represents the differential temporal effects, the DiD estimator will identify

(6) $E(\hat{\alpha}_{DiD}) = \alpha\ + (k^T - k^C)(\theta_t - \theta_\tau)$

which will not in general recover the true effect. Heckman and Hotz (1989) recommend

the use of pre-programme tests to investigate whether significant effects are (erroneously)

detected before the programme takes place. In this application, sufficient data are not

available to do this so a different test was used. This test makes use of the fact that there

was variation in practice in when the new legislation was adopted. Such variation may

have been due to efficiency variations across local offices or to variations across

individuals in how straightforward their claim was to accommodate within the new

system. In fact, the new system was running concurrently with the old system for some

months. As will be discussed below, this variation, which was not an intended feature of

the introduction of the legislation, can be exploited to refine estimates of the treatment

effect. The effect of interest is that relating to those who had converted to the new

system (the 'converted').   However, a contemporaneous effect, using the same

comparison group, can be estimated for those whose claims were still subject to the pre-

Joint Claims rules - the 'unconverted'. An insignificant effect for the unconverted would suggest that the macro trends are similar across the treatment and comparison groups.

The third assumption is that the composition of the treatment group remains unchanged following the intervention. This may be more of a problem when using repeat cross-section data as in this evaluation than when using panel data, although systematic attrition in panel data can be equally damaging. As will be discussed below, matching can be helpful in addressing the potential change in sample composition.

Adjusting the linear DiD model for differential trends in the comparison group

As mentioned earlier, two extensions to the linear DiD model were considered. The first adjusts for the possibility of a differential trend across the treatment and comparison groups. Bell et al. (1999) address this point. Intuitively, their approach corrects for the possibility of differential trends by controlling for an effect estimated by DiD for a hypothetical treatment before the true treatment.[1] Implicit in this is the assumption of zero effect in the pre-treatment period. The choice of timing for the hypothetical treatment is important since it should relate to a period over which a similar macro trend has occurred so that the $(k^T - k^C)(\theta_{t1} - \theta_{t-1})$ term can be differenced out. Abstracting from regressors other than the treatment variable, their trend-adjusted estimator takes the form

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[1] The trend-adjusted estimator is also the random growth model of, for example, Heckman and Hotz (1989).

(7) $\hat{\alpha}_{\text{TADiD}} = [(\bar{Y}_t^T - \bar{Y}_\tau^T) - (\bar{Y}_t^C - \bar{Y}_\tau^C)] - [(\bar{Y}_{\tau^{**}}^T - \bar{Y}_{\tau^*}^T) - (\bar{Y}_{\tau^{**}}^C - \bar{Y}_{\tau^*}^C)]$

where t denotes a post intervention period and $\tau^* < \tau^{**} < \tau$ denote pre intervention

periods.[2]

In this paper, an analogous estimator is derived by controlling for differential trends

between the unconverted couples and those in the comparison group. That is, assuming

the unconverted couples can be regarded as representing how the converted couples

would have fared had Joint Claims not been introduced, effects estimated using the

unconverted and the comparison group should be insignificant if the comparison group is

satisfactory. If significant effects are detected, these can be used as a measure of bias by

which to adjust the estimates for converted couples. A strength of this approach is that

the bias estimate is contemporaneous to the estimates for the converted couples and is

therefore subject to identical macro trends.

Algebraically, this conversion-adjusted DiD can be written:

(8) $\hat{\alpha}_{\text{CADiD}} = [(\bar{Y}_t^{TC} - \bar{Y}_\tau^{TC}) - (\bar{Y}_t^C - \bar{Y}_\tau^C)] - [(\bar{Y}_t^{TU} - \bar{Y}_\tau^{TC}) - (\bar{Y}_t^C - \bar{Y}_\tau^C)]$

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[2] Equation (7) requires four periods of data. Alternatively, it could be estimated using three periods of data if $\tau^{**} = \tau$. This is the approach adopted in Heckman and Hotz (1989).

where TC denotes treatment group members whose claims have been converted and TU

denotes treatment group members whose claims are unconverted. Clearly, this simplifies

to

$$(9) \qquad \overset{\wedge}{\alpha}_{\text{CADiD}} = (\bar{Y}_t^{\text{TC}} - \bar{Y}_\tau^{\text{TC}}) - (\bar{Y}_t^{\text{TU}} - \bar{Y}_\tau^{\text{TC}}) = \bar{Y}_t^{\text{TC}} - \bar{Y}_t^{\text{TU}}$$

which removes altogether the need for a comparison group or any pre-intervention

observations. In fact, this simplification is obvious if the conversion process is

considered random (conditional on observables) – the comparison of means is the same

approach that would be used with experimental data. However, the formulation in (8) is

useful since the first bracketed term on the right hand side represents the estimated

unadjusted effect and the second bracketed term provides a test of the suitability of the

comparison group. Furthermore, when controlling for X in a regression, (8) and (9) will

yield different results since the conditioning is through the means of the treatment and

comparison members combined in (8) but only the treatment group in (9).

Combining DiD with propensity score matching

The second extension is to combine DiD with propensity score matching. This

combination has become popular in empirical research (see, for example, Heckman et al.

(1998) or Blundell et al. (2001) for an application to the UK) although I am aware of only

one paper which applies it to the case of repeat cross section data rather than panel data –

Eichler and Lechner (2002). This approach avoids the functional form restrictions on

observable characteristics inherent in parametric approaches. However, the error

components are restricted by functional form in the way already discussed, hence the

resulting estimator cannot be regarded as fully nonparametric.[3] Furthermore, while the

constant bias assumption underlying matched DiD is not weaker than the identifying

assumption for matching (the conditional independence assumption, CIA), it is plausible

to believe that in this application it is more likely to be satisfied. This is considered

below.

The CIA (Rubin, 1977) can be expressed:

(10) $Y^0 \amalg D \mid X = x$

where Y^0 is the potential outcome associated with non-treatment and $\amalg$ denotes

independence. This is undermined should the decision to participate be influenced by

unobserved factors likely to determine outcomes. The data available for this evaluation is

not rich enough to observe all influences on participation and outcomes. Hence,

matching estimates are likely to be biased and another identification strategy is required.

Combining matching with DiD allows for unobserved influences on participation, so long

as these are either individual fixed effects or common trend effects. As noted earlier,

voluntary training programmes provide an example of when this assumption may not be

met since the temporary pre-programme dip implies a greater change in μ for those

receiving treatment. However, when considering a compulsory programme such as Joint

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[3] In fact, propensity score matching cannot be regarded as fully nonparametric when it uses a parametric

Claims there is less scope for µ to influence participation. The identifying assumption
can be written:

(11) $\mu_{it} - \mu_{i\tau} \amalg D \mid X = x.$

This is the constant bias assumption referred to earlier and, if it holds, the comparison
group outcomes evolve in the same way the treatments would have had they not
participated (Blundell and Dias, 2000; Eichler and Lechner, 2002). Rosenbaum and
Rubin (1983) showed that the vector of attributes, X, could be replaced with
P(X)=P(D=1|X=x), the probability of receiving treatment, or propensity score. Hence,
the identifying assumption becomes:

(12) $\mu_{it} - \mu_{i\tau} \amalg D \mid P(X) = P(x).$

Under this approach, the average effect of treatment on the treated can be estimated as

(13) $\hat{\alpha}_{MDiD} = \sum_{i \in \{d_i = 1\}} [(Y_{it} - Y_{j(i)\,\tau}) - (Y_{k(i)t} - Y_{l(i)\tau})] / N^T$

where the j(i), k(i) and l(i) subscripts denote observations j, k and l respectively used as
comparators for observation i and where N^T is the number of treated couples.
Alternatively:

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model to estimate the propensity score.

(14) $E(\hat{\alpha}_{MDiD}) = \quad E\,(\,Y_t\,|\,X,\,d{=}1) -$

$$E\,\{\,E\,(\,Y_t\,|\,X,\,d{=}0) - [\,E\,(\,Y_\tau\,|\,X,\,d{=}1) - E\,(\,Y_\tau\,|\,X,\,d{=}0)\,]\,|\,d{=}1\,\}.$$

Under this formulation, matching is performed three times. Specifically, each treated individual is matched to post-intervention comparison group members and to pre-intervention treatment and comparison group members. This is a different approach from that of Eichler and Lechner (2002). In their analysis, the treated are matched to the non-treated before participation in the programme to yield an estimated pre-treatment effect. An analogous procedure after participation provides a post-treatment effect. The overall effect is estimated as the difference between these two estimates. In this paper, the matching approach ensures that observable characteristics before and after the introduction of Joint Claims are balanced for both the treatment and comparison groups. This controls for compositional change over time and therefore represents an improvement to the Eichler and Lechner approach. It has already been shown that compositional changes can undermine DiD estimates. Maintaining the sample composition in this way is most obvious with single nearest-neighbour matching (implied by equation (13)). A related point is that matching balances observable characteristics across the treated and the non-treated, thereby helping ensure that the comparison group provides a suitable counterfactual for the treated.

A note on heterogeneity

For ease of exposition, the assumption implicit in equation (1) is of a treatment effect that is common across all couples. This is unlikely to be valid. A more realistic model is

(1a) $Y_{it} = X_{it}\,\beta + d_{it}\,\alpha_i + U_{it}$

which allows for heterogeneous treatment effects. In this more general scenario, there is a distinction between the average effect of treatment on the treated and the average treatment effect for the population. Only the former is identifiable under DiD (see, for example, Blundell and Dias, 2000). The estimators considered in this section are all based on DiD and can therefore recover this parameter but not the population impact.

III. THE DATA AND THE ELIGIBLE GROUP CONSIDERED

The evaluation uses administrative records of couples claiming JSA. While not as rich as survey data, the clear advantage of administrative data is that they allow estimates to be based on the full population of claimants. More specifically, all couples meeting the age criteria for eligibility and without dependent children are observed. These couples comprise the eligible group. Those eligible after the introduction of the legislation and whose claims had been subject to the new rules from the outset are regarded as receiving

the 'treatment'.[4] These are the 'converted' cases mentioned already. Unconverted cases

comprise all those eligible couples who, by the time of the outcome variable in question,

were still subject to the former system. In addition, a comparison group of couples was

observed. These couples are also without dependent children but do not meet the age

criteria. To be included in the comparison group, neither partner could satisfy the age

criteria for the legislation, but at least one partner had to be aged between 27 and 35

years.

The dataset was constructed as a series of snapshots of the population. These are referred

to as 'scans' in the remainder of this paper. Figure 1 gives an impression of the changing

numbers of couples eligible for Joint Claims. From a peak of 9,500 in January 2001,

there was a gentle decline of about 1,000 couples to a level that has remained broadly

stable since May 2001. The slight dip in numbers in the summer months suggests some

seasonal variation, although without a longer run of data it is not possible to be more

definite about this. The timing of the beginning of the decline is consistent with the

introduction of Joint Claims in March 2001. It is also worth noting that any decline

would have been offset to some extent due to the fact that eligibility is set with reference

to a birth date of 19 March 1976. A consequence of this is that the eligible age group

expands naturally with time. At the time of its introduction, Joint Claims only related to

couples where at least one partner was between the ages of 18 and 24 years but, one year

later, those aged between 18 and 25 years were affected.

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[4] This is to avoid the complications arising from couples who began their claim under the old system and
converted part way through.

< FIGURE 1 HERE >

In trying to evaluate the effect of Joint Claims, it is important to be aware of its possible effects. These are twofold. First, there is the 'direct' effect – the extent to which the economic behaviour of joint claimant couples is affected by the changed JSA environment brought about by the introduction of the legislation. This is the focus of this paper. Second, there is the 'deterrent' effect. It may be that one consequence of Joint Claims is that couples take action in order to avoid its requirements. As an example of this, consider the case of a sole-earner couple faced with imminent job-loss. Pre-Joint Claims, a spell claiming JSA might have ensued until finding a new job. Post-Joint Claims, should the aversion to the idea of both partners having to look for work be sufficiently strong, there may be increased job search effort in order to avoid this, possibly resulting in avoiding a spell claiming JSA.

It is not possible to observe the deterrent effect operating in this way on couples in work. This is because such couples cannot be observed in unemployment records. All that can be estimated for couples entering Joint Claims (the flow) is the direct effect. However, those couples who were eligible for Joint Claims at the time of its introduction (the stock) are recorded in unemployment records, and for them both deterrent effects and direct effects may be important influences on exits. One important reason for a deterrent effect among the stock, for which some anecdotal evidence exists, is the increased difficulty of fraudulently claiming for a non-existent partner due to the Joint Claims requirement for

24

both partners to attend interviews at the job centre. The decline shown in Figure 1 in the numbers claiming is consistent with the existence of a deterrent effect.

In this paper, only the flow is considered. The main reason for this is that flow effects are a better guide to the long-run equilibrium effects of Joint Claims. That is, over time the stock will deplete and the population of Joint Claims couples will increasingly assume the characteristics of the flow. These characteristics can differ substantially from those of the stock who by definition have longer experiences of unemployment. A further complication with considering the stock is that it is difficult to separate the deterrent effect from the direct effect. With the flow, only the undeterred are observed in the data; all identified effects are direct effects.

The flow is taken to comprise all couples who, at the time of each scan, had been unemployed for up to one month. Since the scans cover a number of points in time, it is possible to examine the extent to which outcomes of interest evolve over time. Figure 2 provides an example of one such outcome; the proportion of couples captured in a particular scan who remain unemployed three months later.

< FIGURE 2 HERE >

In this chart, three lines appear. The solid line shows the change over time for the converted cases. The unconverted cases are shown by the dashed line while the dotted line shows the trend for the comparison group. It can be seen that up to three months

before the introduction of the legislation (in March 2001), the converted and the unconverted follow the same path. A number of further points are evident. Before the introduction of the legislation, there is little difference between the treatment and the comparison couples in the probability of being unemployed three months hence. After the introduction, the converted couples initially appear to be more likely than the comparison couples to remain unemployed. However, from about June 2001 onwards, the situation for converted couples had improved to the extent that they were now less likely than those in the comparison group to remain unemployed for three months or longer. Hence, the impression created from this chart is that the intervention took a few months to take effect but then had a positive effect on unemployment exits.

The other important point from this chart is that the trend for unconverted couples follows that of the comparison couples quite closely. Assuming that the conversion process is random and that unconverted couples can be regarded as providing a counterfactual case to converted couples, this similarity is reassuring since it suggests that the comparison group successfully shadows the trend among the treatment group.

Given this distinction between converted and unconverted treatment group couples, it is instructive to inspect the degree to which there is variation in their characteristics. Were the process random, one would expect them to be very similar. Table 1 considers the April 30 2001 scan and shows the profile of those who had converted by this time to be very similar to those who had not converted. No statistical difference was evident for ethnic group, region of residence or rurality of residence. Some differences in age were

detected which were significant at conventional levels, although these were small in magnitude. Preferred occupation was also found to differ significantly yet again the differences were not large. Most notably, there was a significant difference in the proportion disabled (for partner 1). The fact that many more unconverted appear to be disabled may be explained by the fact that disabled individuals may be eligible for exemption if they are not able to fulfil the labour market requirements of seeking and being available for work. Consequently, their conversion could be delayed pending further consideration.

< TABLE 1 HERE >

Overall, it is not valid to regard the conversion process as random since some significant differences are apparent. This precludes use of the simple evaluation techniques possible with experimental data. However, the differences are not substantial on the whole and, furthermore, can be controlled for where observed. The key point is that the policy intention was to bring the legislation into effect at the same time in all areas and across all clients. Any variation to this intention arose in an unpredictable manner. Since delays at the individual level would vary with those client characteristics known to and recorded by the local officer, it is likely that administrative data contains sufficient information to explain reasons for delays in conversion. In view of this, it appears plausible to exploit the variations in conversion to aid identification of the treatment effect, controlling for observed differences as appropriate.

IV. ESTIMATING THE EFFECTS

The first results presented (Table 2) are those for the standard regression-adjusted DiD estimator. Since these results are presented in a similar way to subsequent results, some explanation of the format is provided below.

Two scans are needed to get these DiD estimates; one before and one after the intervention. The dates of the 'before' scans are given in the leftmost column of Table 2. Four such scans are considered: September, November and December 2000 and January 2001.[5] Five 'after' scans are considered and these are detailed at the top of each column: one in April, two in June, one in August and one in September 2001. The outcome measure considered is whether the couple were still claiming JSA at some point after the scan dates. Six points were considered: 1, 2, 3, 4, 5 and 6 months after the scan date. The entry in each cell represents the estimate of the effect of Joint Claims on exits from unemployment. More specifically, they represent percentage point differences. Finally, a number of cells are empty. This indicates that the outcome measure in question relates to a point in time for which unemployment information was not available at the time of writing (information was only available up to mid-November 2001). The other reason for cells being empty is that the outcome measure in question in the 'before' period would span the introduction of Joint Claims, making it difficult to identify a clear effect.

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[5] Hamermesh (2000) suggests that if estimates are robust to varying the pre-intervention period, it is more plausible to assume that the bias is stable. Since in this analysis the periods are quite close together, there is more reason to believe the bias to be stable.

< TABLE 2 HERE >

Presented in this way, only the effect of Joint Claims itself is shown. However, the

models used to obtain these results included a number of other variables that may have

affected transitions away from unemployment. Such factors as age, ethnicity, preferred

occupation, disability, JSA history, region, rurality and the local unemployment rate may

be thought to influence outcomes and these were controlled for in the model. It is not

practical to present these results in full (Table 2, for example, summarises the results of

54 separate estimations).

The overall impression from inspecting Table 2 is that the effect matured over time,

eventually achieving the intended effect of encouraging benefit exit. Considering

unemployment exits over the first three months, there is broad consistency across the

'before' scans. It is not until the August and September 'after' scans that a relatively

stable position is reached. The early effects on longer-term unemployment appear

perverse. More specifically, the results suggest couples were now more likely to remain

on JSA in the longer-term.

< TABLE 3 HERE >

However, these results take no account of the suitability of the comparison group. Table

3 gives the estimated DiD effects for the unconverted cases. The results suggest that, on

the whole, the comparison group performed well in providing a counterfactual for the treatment group trends. In almost all cases the estimated effect is statistically insignificant. There are two instances where this was violated. Therefore, to address the possibility of the results in Table 2 being biased due to an inappropriate comparison group, the unconverted results (Table 3) can be deducted from the converted results (Table 2). Table 4 shows the results of doing this.

< TABLE 4 HERE >

Adjusting for the comparison group in this way does not alter the overall impression gained from the simple DiD estimates but it does make clearer the evolution of the effect. In particular, it appears that there is less variation across the 'before' scans in the estimated effects for any given 'after' scan. Summarising the results rather boldly, it appears that the effects on short-term unemployment are in the region of 10 percentage points by the time of the August 'after' scan and roughly 15 percentage points by the time of the September scan. Hence, the evidence suggests an evolving Joint Claims effect; after an initial period of ineffectiveness, about five months after its introduction its influence on JSA exits could be observed. Third, the results for the June and August 2001 'after' scans show greater effects for JSA status after one month and also after two months than after three months (which is often insignificant). This hints at the possibility that Joint Claims may act to speed exit from JSA for some people but not to have an effect on those who would go on to have a longer JSA spell. However, without further observations it is not possible to be more definite about this.

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It is notable that some of the perverse early effects on longer-term unemployment remain after adjusting for bias in the comparison group. At first sight, this suggests the possibility that those with a greater tendency to prolonged unemployment were more likely to convert early. However, the results in Table 3 provide evidence against this since there are no significant differences between unconverted couples and those in the comparison group in terms of their unemployment exits. Furthermore, DiD estimates using the full treatment group (ie irrespective of conversion) gave qualitatively similar results.[6] This provides a further indication that non-random conversion is unlikely to be the cause of the early perverse effects on longer-term unemployment. Indeed, the descriptive results in Table 1 show few systematic differences in terms of observable characteristics. An alternative explanation for these early results is that the linear functional form of the DiD estimator is not sufficiently flexible to control adequately for variation in outcomes associated with observables. This is returned to below.

The results of estimating the model given in equation (9) are shown in Table 5. The same overall pattern is evident for short-term exits from unemployment. However, the early long-term estimated effects are much more in line with expectations and no significant positive effects are evident. Since these estimates amount to a regression-adjusted difference between converted and unconverted couples only, the implication is that the positive effects in Table 4 arise from the use of the comparison group in estimating effects.

< TABLE 5 HERE >

In Table 6, the estimated effects using the matched DiD approach are shown. The propensity score for each combination of scans was calculated using a probit model of participation with similar controlling variables to those used in the DiD analysis. However, the results of the balancing score test of Rosenbaum and Rubin (1983) suggested that a quadratic term in the length of unemployment should be included. Consequently, the set of conditioning variables was expanded to include this additional variable. The results for short-term unemployment exits are qualitatively very similar to those already presented. This provides some reassurance as to the robustness of these results. The results for unemployment exits over the longer-term are more in line with expectations in that no significant positive effects were evident. As noted earlier, this may be due to the linear DiD estimator being unduly restrictive in its functional form. Since the results shown in Table 5 suggest that plausible results are achieved in the linear framework when considering only converted and unconverted couples, the implication is that it is when making use of a comparison group that linearity may become over-restrictive. Another possibility is that the results in Table 4 were affected by compositional changes post-treatment, but that the matched DiD approach addresses this by ensuring observable characteristics are balanced.

< TABLE 6 HERE >

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[6] These results are not presented but are available on request.

Since the matched DiD estimates are not fully constrained by the functional form of the linear DiD estimates, an examination of the sensitivity of the results to the inclusion/exclusion of variables was carried out. The results are presented in Table 7. An attempt has been made to keep this sensitivity analysis within manageable proportions by concentrating only on those outcomes corresponding to the latest available scan (September 2001) while still allowing the pre-Joint Claims scans to vary. The rationale for choosing the latest scan is that the results presented so far suggest that by this point the effects of Joint Claims had achieved some stability. Each column in Table 7 corresponds to results based on a different set of conditioning variables. The first column is identical to the results in Table 6 since no variables are excluded. However, in subsequent columns, successively more variables are omitted. The omission is cumulative in that the results in one column also exclude the information omitted in the previous column. The final column conditions only on the couples' JSA history.

< TABLE 7 HERE >

Table 7 shows that the results are fairly robust to a reduction in information; taking standard errors into account, the point estimates are not that different from each other. In fact, there appears to be little systematic pattern in the way the estimated effects vary with available information, a finding that is unsurprising given the lack of functional form restrictions on the observable variables. However, although the size of the estimated effects vary, there is little change in the direction or significance of the results.

Overall, it appears that the information set is important although the qualitative interpretation of the findings is reasonably robust.

In addition to considering movements away from unemployment, the effect of Joint Claims on employment is also of great policy relevance. However, a limitation of administrative records is that destination on unemployment exit is often characterised by a large number of missing values. This results in difficulties when using such data to consider transitions into employment. To address this, estimates of the effect on employment are derived under two opposing assumptions; that *no* unrecorded destinations are accounted for by job entry and that *all* unrecorded destinations are accounted for by job entry. This is a form of sensitivity analysis in that it provides an indication of how robust the results are to the assumptions surrounding missing destinations information.[7]

< TABLE 8 HERE >

Table 8 presents the resulting estimates. These are based on equation (9) and follow a similar presentational format to Table 5. There are two panels in Table 8, corresponding to the opposing assumptions regarding missing values. Overall, Joint Claims appears less effective on employment entry than on unemployment exit. There is little evidence of a

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[7] These opposing assumptions relating to destinations cannot be regarded as representing bounds on the true effect. Such bounds can be achieved by assuming that no exits to unknown destinations among the converted were to employment but all among the unconverted were to employment (lower bound) or *vice versa* for the upper bound. However, there is little reason to believe a difference exists between the converted and unconverted in terms of the proportion of exits to unknown destinations that are accounted for by employment. Consequently, the resulting bounds are very wide and do little to aid the interpretation of the results. These bounds are not presented but are available on request.

positive effect on employment in the short-term. In fact, it is not until the most recent 'after' scan that both panels give a significant positive effect on employment (one month later) of between 4 and 10 percentage points. This suggests that, as with the effect on unemployment exit, the effect on employment entry may mature over time. No significant effects on employment after three months are found, and for longer-term outcomes the effects are predominantly negative.

Finally, it is interesting to consider the extent to which the apparent employment effect has operated through the male or the female partner. Before Joint Claims, it was most commonly the female partner who was economically inactive while the male partner had to satisfy the JSA job search requirements. Since the introduction of Joint Claims had the effect of forcing the inactive partner to seek work, the effect was expected to be greater for women. This is examined in Figure 3 which shows the changing proportion of exits to employment accounted for by women rather than men finding work. Only exits within three months of the scan date are considered, since this is the period to which the bulk of the results already presented relate. Overall, it is clear that where a couple finds work, it is most often the male partner who has done so. However, the pattern for women is interesting. Most notably, there is a fall in the proportion of jobs accounted for by women at a time that roughly corresponds to the introduction of Joint Claims in March 2001. This reaches its lowest point in June 2001, after which it rises steadily. This suggests there was a negative effect initially for women but that this disappeared over time such that, by the latest period for which data are available, the proportion of jobs accounted for by women was comparable to the sort of levels seen before Joint Claims.

Combining this finding with the result in Table 8 that the positive effects on employment took some time to appear suggests that the employment effect may well have operated primarily through the female partner. It will be interesting to see if this upward trend shown in Figure 3 continues as more up to date information becomes available.

< FIGURE 3 HERE >

However, Figure 3 is not directly comparable with the results in Table 8 since it includes both stock and flow couples. The pattern among flow couples is shown in Figure 8. Here the trend is less obvious and more volatile. This reflects the smaller sample size for the flow and the consequent higher standard errors associated with the means. In spite of this, there does appear to be a tendency for the exits to employment to be increasingly accounted for by female jobs. In support of this, the proportions in the last three scans are all significantly higher than the proportion in March 2001. This provides further support for the belief that the employment effect operated mainly through the female partner.

V. CONCLUSION

In this paper, the effect of Joint Claims has been examined using two different approaches. While there were some differences in the size of the estimates, the broad patterns revealed were similar, suggesting the results may be robust. The effect of adjusting for bias resulting from a possibly inappropriate comparison group was evident.

This is true despite the fact that testing revealed the comparison group to be acceptable by and large. This adjustment was possible due to delays in implementing the legislation. In more general applications, such an adjustment may not be possible, so it is reassuring that the matched DiD results are comparable to these adjusted results. It appears plausible that matching across the treated and non-treated groups may go some way to ensuring the comparison group bears a resemblance to the treated group. This is an important advantage of the approach.

Substantively, the overall results are that Joint Claims appears to have been successful in accelerating JSA exit but not necessarily in helping couples to exit worklessness. There was an indication that its main effect was on short-term rates of exit and that longer-term exits may be less affected. Importantly, the effect appears to have evolved over time. About five months after the introduction of Joint Claims, significant effects on unemployment in the expected direction were detected. There was also evidence that the effect on employment was evolving such that Joint Claims was starting to encourage employment entry, at least in the short-term. Furthermore, there is reason to believe that the effect operates largely through the female partner.

The fact that Joint Claims had a greater effect on JSA exit than on entering work implies that some couples leaving JSA were either moving onto other benefits or were managing without any benefits. With respect to the latter, since they cannot be observed beyond the point of JSA exit, it is not possible to know how long such couples manage without receiving a benefit. It is plausible to believe that a proportion of them will in fact move

into employment after some time. Should this be the case, it appears reasonable to view this employment effect as being indirectly attributable to Joint Claims. While this clearly cannot be quantified, it may go some way to helping understand what happens to those couples who simply disappear from the unemployment register.

A number of reasons for the effect maturing over time are possible. Generally, it is not uncommon for new interventions to require a period of time to 'bed-down'. Qualitative research (Fielding et al, 2001) suggests that there was a learning curve for Jobcentre staff in coping with Joint Claims clients. Staff had to deal with a number of cases before they could be confident of delivering an effective service. Aggravating this problem of needing to accrue experience of Joint Claims was the fact that the training provided for staff often occurred too far in advance of the introduction of the legislation. The consequence of this was that staff may have forgotten much of what they had learned by the time they were actually meant to make use of it, and maintaining awareness was difficult. Even identifying when a client would have been eligible for Joint Claims was sometimes problematic, particularly for couples where there was a sizeable age difference between partners. Less experienced reception staff would sometimes mistakenly assume the client's partner to be of a similar age and thereby not consider the couple eligible for Joint Claims. The use of temporary reception staff did little to help this problem. Fielding et al. (2001) also note that there were deficiencies within the IT systems at the time of introduction of Joint Claims and that these persisted for some months thereafter.

Finally, it is important to note that one effect of Joint Claims is to ensure that both partners within a couple are visible to the JSA process and all that that entails. A key consequence of this is that both partners become eligible for labour market programmes when their period of JSA claiming reaches the required duration. In most cases, this will be the New Deal for Young People after a period of six months unemployment. Hence, Joint Claims not only applies the standard JSA incentive to job search, but acts as a springboard to other programmes which will then exert their own particular influence.

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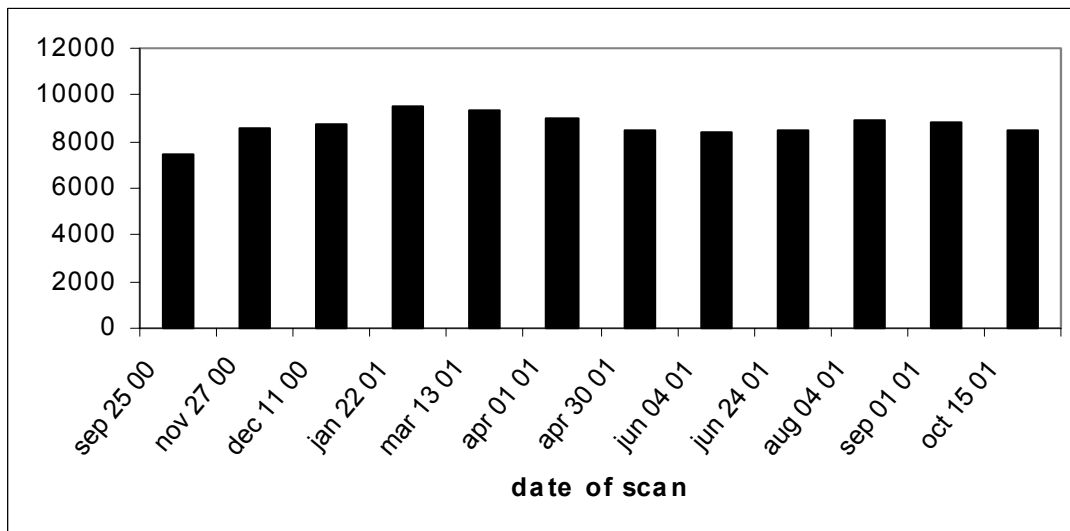
Figure 1 Changing size of the Joint Claims population
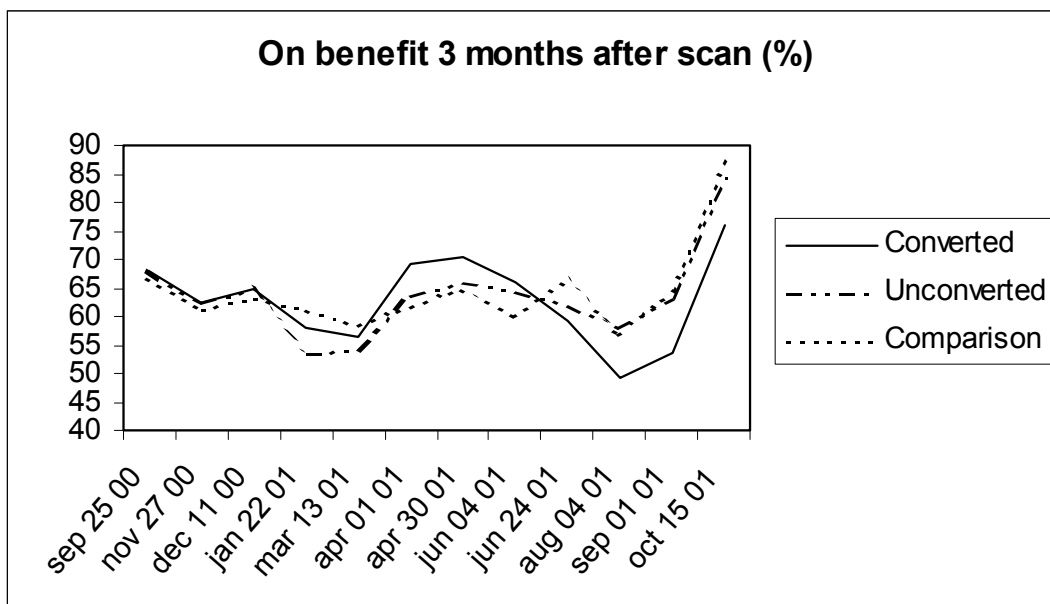
Figure 2 Trends over time in exits from benefit among the flow

bars show mean +/- 2 standard errors
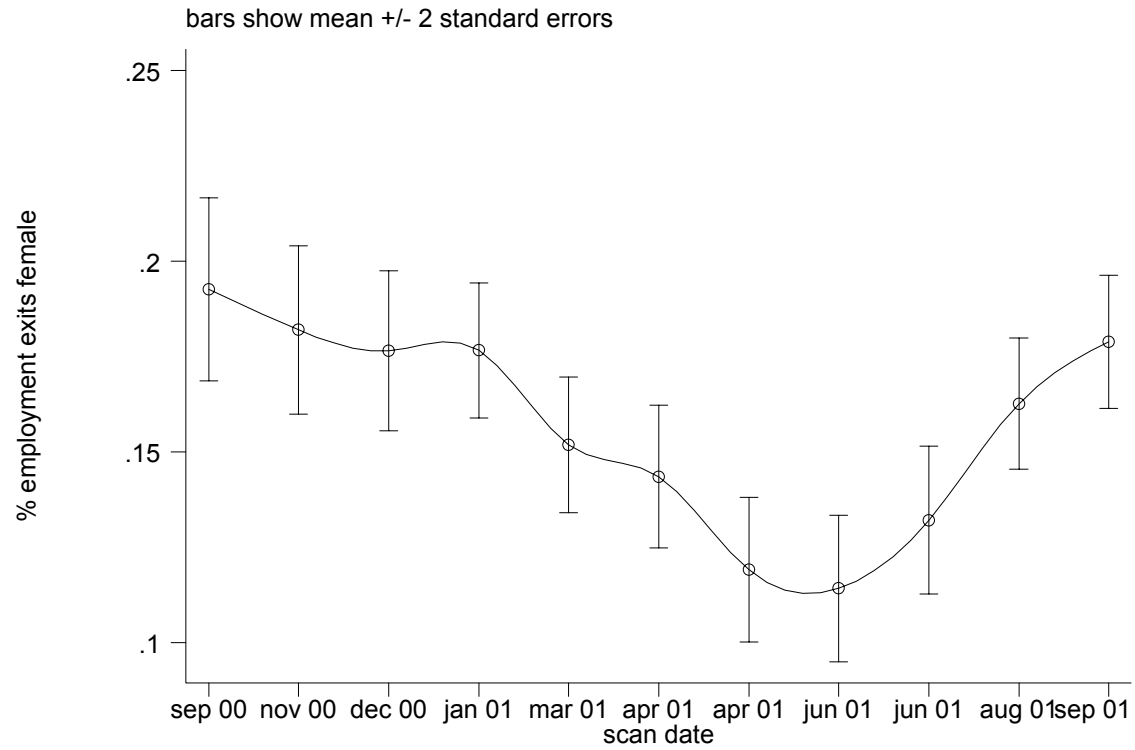
Figure 3 Changes over time in the proportion of exits to employment accounted for by female jobs (stock and flow)

bars show mean +/- 2 standard errors
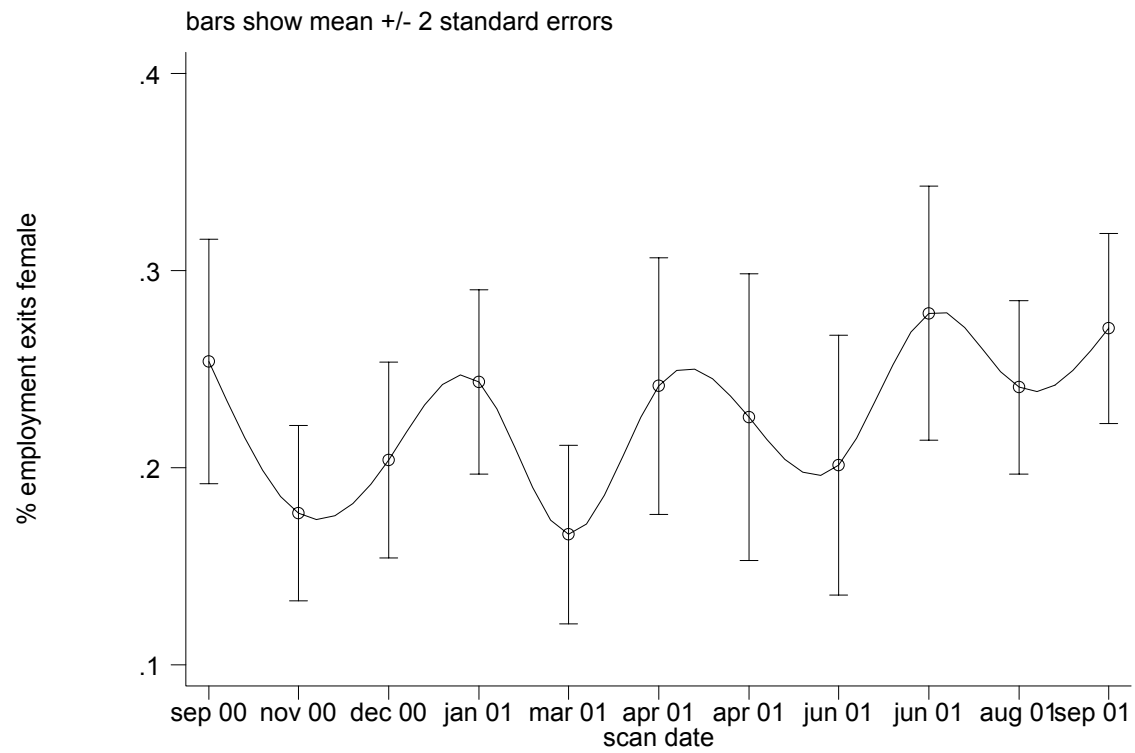
Figure 4 Changes over time in the proportion of exits to employment accounted for by female jobs (flow)

Table 1: Comparing converted and unconverted treatment group couples

	Type of treatment group couple:		Testing difference:
	Converted	Unconverted	P-value
Partner 1 age*	23.1	23.8	0.02
Partner 2 age*	21.4	20.9	0.03
Partner 1 disabled (%)*	9.5	16.0	0.00
Partner 2 disabled (%)*	10.0	8.3	0.36
Ethnic group of partner 1:			0.63
White	85.7	82.7	
Black-Caribbean	85.7	82.7	
Black-African	0.6	0.4	
Black-other	0.4	0.4	
Indian	0.6	0.4	
Pakistani	2.8	1.9	
Bangladeshi	4.7	7.4	
Chinese	1.7	2.3	
Other	3.6	4.4	
Preferred occupation of partner 1:			0.02
Managers and senior officials	1.8	2.4	
Professional occupations	1.3	3.8	
Associate professional & technical	6.9	3.8	
Administrative and secretarial	11.6	11.0	
Skilled trades	14.4	16.4	
Personal service	5.8	4.8	
Sales and customer service	15.2	12.4	
Process, plant & machine operatives	8.7	12.6	
Elementary occupations	34.5	32.9	
Region:			0.85
Scotland	9.3	7.4	
Northern	6.4	8.3	
North west	14.1	12.9	
Yorkshire and the Humber	11.9	12.3	
Wales	5.7	6.4	
West midlands	12.5	12.5	
East midlands & eastern	10.9	12.5	
South west	10.0	8.9	
London and South East	19.4	18.9	
Rural ward (%)	15.5	16.0	0.81
Total	562	541	

*Before Joint Claims, partner 1 was the claimant partner. Joint Claims removed this distinction with the consequence that the question of which partner was recorded as partner 1 was arbitrary.

Table 2: Regression-adjusted DiD estimates of the effect of Joint Claims on the probability of still claiming JSA after X months

	Date of post-Joint Claims scan (2001)				
	30 Apr	4 Jun	24 Jun	4 Aug	1 Sep
25 Sep 2000 base					
JSA after 1 month	0.4	3.2	-4.8	-13.1**	-11.7**
	(0.15)	(1.21)	(1.85)	(5.16)	(4.08)
JSA after 2 months	5.9	6.0	-4.6	-8.0*	-9.8**
	(1.85)	(1.86)	(1.47)	(2.53)	(2.90)
JSA after 3 months	8.2*	8.8*	-2.5	-2.6	.
	(2.32)	(2.51)	(0.73)	(0.76)	.
JSA after 4 months	11.2**	10.9**	-0.7	.	.
	(3.01)	(2.97)	(0.20)	.	.
JSA after 5 months	12.6**	12.6**	0.4	.	.
	(3.32)	(3.41)	(0.10)	.	.
JSA after 6 months	15.1**	.	.	.	.
	(4.00)	.	.	.	.
27 Nov 2000 base					
JSA after 1 month	-1.8	0.3	-7.6**	-15.2**	-14.3**
	(0.70)	(0.11)	(3.04)	(6.08)	(5.07)
JSA after 2 months	0.1	-0.9	-11.4**	-13.8**	-16.1**
	(0.03)	(0.27)	(3.67)	(4.37)	(4.78)
JSA after 3 months	5.1	4.3	-7.2*	-5.5	.
	(1.46)	(1.24)	(2.11)	(1.65)	.
11 Dec 2000 base					
JSA after 1 month	-3.1	-1.0	-8.7**	-16.8**	-15.6**
	(1.22)	(0.41)	(3.60)	(7.00)	(5.72)
JSA after 2 months	1.3	0.1	-10.5**	-13.0**	-15.3**
	(0.40)	(0.03)	(3.35)	(4.12)	(4.54)
JSA after 3 months	4.0	3.0	-8.8*	-7.4*	.
	(1.14)	(0.85)	(2.53)	(2.20)	.
22 Jan 2001 base					
JSA after 1 month	-0.3	1.5	-6.5*	-14.0**	-13.1**
	(0.13)	(0.58)	(2.56)	(5.57)	(4.58)

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses.

Table 3: Regression-adjusted DiD estimates of the effect of Joint Claims on the probability of still claiming JSA after X months for 'unconverted' couples

	Date of post-Joint Claims scan (2001)				
	30 Apr	4 Jun	24 Jun	4 Aug	1 Sep
25 Sep 2000 base					
JSA after 1 month	3.6	0.5	-1.3	-5.5*	2.1
	(1.46)	(0.19)	(0.51)	(2.20)	(0.75)
JSA after 2 months	2.8	1.0	-2.9	-0.5	1.1
	(0.86)	(0.30)	(0.90)	(0.16)	(0.32)
JSA after 3 months	2.7	4.1	-3.7	1.5	.
	(0.75)	(1.14)	(1.03)	(0.41)	.
JSA after 4 months	2.4	5.5	-2.7	.	.
	(0.61)	(1.48)	(0.70)	.	.
JSA after 5 months	2.9	5.8	-5.0	.	.
	(0.73)	(1.52)	(1.30)	.	.
JSA after 6 months	6.3	.	.	.	.
	(1.61)	.	.	.	.
27 Nov 2000 base					
JSA after 1 month	3.9	1.2	-1.2	-4.4	3.0
	(1.65)	(0.47)	(0.50)	(1.82)	(1.11)
JSA after 2 months	0.3	-0.9	-4.8	-1.5	0.5
	(0.10)	(0.28)	(1.53)	(0.46)	(0.14)
JSA after 3 months	2.5	4.1	-3.9	2.6	.
	(0.71)	(1.15)	(1.11)	(0.74)	.
11 Dec 2000 base					
JSA after 1 month	2.5	-0.8	-2.6	-6.4**	1.4
	(1.07)	(0.32)	(1.13)	(2.74)	(0.52)
JSA after 2 months	1.6	-0.1	-3.7	-1.1	1.5
	(0.50)	(0.02)	(1.18)	(0.34)	(0.44)
JSA after 3 months	2.9	3.8	-4.2	1.9	.
	(0.81)	(1.07)	(1.15)	(0.54)	.
22 Jan 2001 base					
JSA after 1 month	4.8*	1.3	-1.0	-4.3	2.7
	(1.97)	(0.50)	(0.43)	(1.75)	(0.97)

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses.

Table 4: Regression-adjusted DiD estimates of the effect of Joint Claims on the probability of still claiming JSA after X months, adjusted for comparison group 'bias'

	Date of post-Joint Claims scan (2001)				
	30 Apr	**4 Jun**	**24 Jun**	**4 Aug**	**1 Sep**
25 Sep 2000 base					
JSA after 1 month	-3.18	2.73	-3.51	-7.67**	-13.81**
	(1.52)	(1.11)	(1.59)	(3.26)	(4.99)
JSA after 2 months	3.12	5.04	-1.76	-7.49**	-10.92**
	(1.12)	(1.68)	(0.65)	(2.67)	(3.39)
JSA after 3 months	5.44	4.76	1.15	-4.06	.
	(1.80)	(1.46)	(0.37)	(1.29)	.
JSA after 4 months	8.83**	5.33	1.97	.	.
	(2.69)	(1.61)	(0.57)	.	.
JSA after 5 months	9.74**	6.77	5.42	.	.
	(2.92)	(1.90)	(1.62)	.	.
JSA after 6 months	8.80**	.	.	.	.
	(2.74)	.	.	.	.
27 Nov 2000 base					
JSA after 1 month	-5.72*	-0.94	-6.37**	-10.79**	-17.27**
	(2.57)	(0.42)	(2.89)	(4.43)	(7.00)
JSA after 2 months	-0.25	0.05	-6.58*	-12.29**	-16.57**
	(0.08)	(0.02)	(2.21)	(4.14)	(5.27)
JSA after 3 months	2.53	0.24	-3.24	-8.12**	.
	(0.75)	(0.08)	(0.97)	(2.74)	.
11 Dec 2000 base					
JSA after 1 month	-5.51**	-0.22	-6.07**	-10.42**	-16.98**
	(2.70)	(0.10)	(2.72)	(4.31)	(6.42)
JSA after 2 months	-0.37	0.18	-6.73*	-11.93**	-16.86**
	(0.12)	(0.06)	(2.46)	(4.03)	(5.36)
JSA after 3 months	1.07	-0.86	-4.63	-9.32**	.
	(0.32)	(0.28)	(1.42)	(3.02)	.
22 Jan 2001 base					
JSA after 1 month	-5.09*	0.19	-5.43*	-9.74**	-15.72**
	(2.30)	(0.08)	(2.55)	(4.22)	(6.52)

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses based on bootstrapped standard errors with 250 replications.

Table 5: Regression-adjusted mean difference estimates of the effect of Joint Claims on the probability of still claiming JSA after X months

	Date of post-Joint Claims scan (2001)				
	30 Apr	**4 Jun**	**24 Jun**	**4 Aug**	**1 Sep**
JSA after 1 month	-5.64**	-0.01	-5.36*	-10.48**	-15.69**
	(2.65)	(0.01)	(2.19)	(4.26)	(5.75)
JSA after 2 months	-2.36	1.21	-4.24	-12.26**	-13.65**
	(0.80)	(0.39)	(1.38)	(3.94)	(3.92)
JSA after 3 months	0.90	1.09	-1.82	-8.42*	.
	(0.27)	(0.32)	(0.53)	(2.46)	.
JSA after 4 months	3.55	2.96	0.42	.	.
	(0.97)	(0.82)	(0.12)	.	.
JSA after 5 months	4.68	5.89	5.14	.	.
	(1.20)	(1.57)	(1.39)	.	.
JSA after 6 months	5.01	.	.	.	.
	(1.25)	.	.	.	.

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses.

Table 6: Matched DiD estimates of the effect of Joint Claims on the probability of still claiming JSA after X months

	Date of post-Joint Claims scan (2001)				
	30 Apr	4 Jun	24 Jun	4 Aug	1 Sep
25 Sep 2000 base					
JSA after 1 month	-5.74	-5.88	-3.77	-14.26**	-16.47**
	(1.21)	(1.20)	(0.82)	(3.26)	(3.50)
JSA after 2 months	-5.08	-0.39	-13.64*	-14.56**	-12.86*
	(0.86)	(0.07)	(2.41)	(2.57)	(2.27)
JSA after 3 months	-2.87	1.76	-9.34	-11.71	.
	(0.45)	(0.27)	(1.49)	(1.91)	.
JSA after 4 months	4.64	4.90	-0.18	.	.
	(0.69)	(0.73)	(0.03)	.	.
JSA after 5 months	2.65	5.29	3.95	.	.
	(0.39)	(0.78)	(0.59)	.	.
JSA after 6 months	6.40	.	.	.	.
	(0.95)	.	.	.	.
27 Nov 2000 base					
JSA after 1 month	-4.86	1.79	-9.82*	-16.36**	-13.83**
	(1.08)	(0.40)	(2.34)	(4.03)	(3.10)
JSA after 2 months	-2.87	-0.20	-16.00**	-9.70	-11.97*
	(0.50)	(0.04)	(3.01)	(1.78)	(2.17)
JSA after 3 months	3.31	6.37	-8.36	-0.45	.
	(0.53)	(1.04)	(1.41)	(0.08)	.
11 Dec 2000 base					
JSA after 1 month	-4.64	0.78	-8.44*	-17.33**	-16.92**
	(1.09)	(0.18)	(2.14)	(4.41)	(3.94)
JSA after 2 months	-0.66	1.36	-16.34**	-8.59	-14.93**
	(0.11)	(0.24)	(3.04)	(1.53)	(2.65)
JSA after 3 months	5.08	1.75	-10.41	-3.56	.
	(0.79)	(0.28)	(1.73)	(0.58)	.
22 Jan 2001 base					
JSA after 1 month	-3.53	-0.59	-8.11	-13.06**	-16.25**
	(0.79)	(0.13)	(1.87)	(3.08)	(3.55)

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses based on analytical approximations that regard the propensity score as fixed. These were calculated using the following variance formula

$$\mathrm{Var}\,(\hat{\alpha}_{\mathrm{MDiD}}) = (1/N^2)\,\mathrm{Var}\,(Y\mid T, t) + (1/N^2)\ \mathrm{x}$$

$$\left\{ \sum_{j>0} w_j^2\,\mathrm{Var}\,(Y\mid T, \tau, w_j > 0) + \sum_{k>0} w_k^2\,\mathrm{Var}\,(Y\mid C, t, w_k > 0) + \sum_{l>0} w_l^2\,\mathrm{Var}\,(Y\mid T, \tau, w_l > 0) \right\}$$

where N is the number of treated couples and w_j, w_k and w_l are the matching weights for those in the pre-Joint Claims treatment group, the post-Joint Claims comparison group and the pre-Joint Claims comparison group respectively.

Table 7: Matched DiD estimates of the effect of Joint Claims on the probability of still claiming JSA after X months using the September 1, 2001 'post' scan: the effect of varying the information set

	None	**Disab'ty**	**Rural area**	**region**	**Ethnic group**	**Pref. Occup.**	**Local unemp.**
			Information omitted from conditioning set:				
25 Sep 2000 base							
JSA after 1 month	-16.47**	-19.19**	-21.91**	-16.36**	-27.53**	-17.54**	-19.09*
	(3.50)	(4.06)	(4.53)	(3.40)	(5.85)	(3.72)	(2.44)
JSA after 2 months	-12.86*	-17.17**	-19.73**	-14.38*	-22.06**	-18.36**	-23.98*
	(2.27)	(3.01)	(3.38)	(2.47)	(3.88)	(3.23)	(2.51)
27 Nov 2000 base							
JSA after 1 month	-13.83**	-16.97**	-20.53**	-12.27**	-18.46**	-20.55**	-11.46
	(3.10)	(3.79)	(4.57)	(2.71)	(4.16)	(4.68)	(1.49)
JSA after 2 months	-11.97*	-18.64**	-19.87**	-13.58*	-15.36**	-16.80**	-14.08
	(2.17)	(3.37)	(3.58)	(2.42)	(2.80)	(3.10)	(1.45)
11 Dec 2000 base							
JSA after 1 month	-16.92**	-18.60**	-10.60*	-13.70**	-16.45**	-19.02**	-16.10*
	(3.94)	(4.23)	(2.44)	(3.17)	(3.83)	(4.60)	(2.30)
JSA after 2 months	-14.93**	-14.19*	-8.65	-7.83	-15.64**	-11.54*	-7.64
	(2.65)	(2.45)	(1.52)	(1.39)	(2.77)	(2.13)	(0.80)
22 Jan 2001 base							
JSA after 1 month	-16.25**	-15.70**	-13.28**	-11.56*	-16.45**	-12.52**	-14.66
	(3.55)	(3.37)	(2.84)	(2.52)	(3.64)	(2.83)	(1.91)

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses based on analytical approximations that regard the propensity score as fixed – see footnote to Table 6 for variance formula.

Table 8: Regression-adjusted mean difference estimates of the effect of Joint Claims on the probability of at least one partner working after X months

	Date of post-Joint Claims scan (2001)				
	30 Apr	4 Jun	24 Jun	4 Aug	1 Sep
Assuming *none* of those leaving to unknown destinations enter work					
Emp after 1 month	3.56**	-1.22	1.34	1.98	4.32*
	(2.72)	(0.81)	(0.91)	(1.38)	(2.42)
Emp after 2 months	0.63	-3.44	2.39	0.81	2.61
	(0.31)	(1.65)	(1.28)	(0.36)	(1.15)
Emp after 3 months	-2.29	-4.43	0.72	-0.74	.
	(0.98)	(1.91)	(0.33)	(0.29)	.
Emp after 4 months	-3.55	-7.55**	-3.64	.	.
	(1.39)	(2.84)	(1.37)	.	.
Emp after 5 months	-6.03*	-9.85**	-6.03*	.	.
	(2.12)	(3.36)	(1.99)	.	.
Emp after 6 months	-8.29**	.	.	.	.
	(2.64)	.	.	.	.
Assuming *all* of those leaving to unknown destinations enter work					
Emp after 1 month	3.44	-0.65	3.05	2.70	10.08**
	(1.86)	(0.29)	(1.44)	(1.27)	(4.38)
Emp after 2 months	-0.13	-3.50	0.43	-0.16	5.67
	(0.05)	(1.26)	(0.16)	(0.06)	(1.89)
Emp after 3 months	-4.38	-4.29	-2.88	-2.53	.
	(1.49)	(1.41)	(0.95)	(0.80)	.
Emp after 4 months	-7.37*	-6.46	-8.02*	.	.
	(2.26)	(1.94)	(2.39)	.	.
Emp after 5 months	-12.28**	-9.99**	-12.93**	.	.
	(3.44)	(2.83)	(3.56)	.	.
Emp after 6 months	-13.36**	.	.	.	.
	(3.44)	.	.	.	.

* - significant at 5%; ** - significant at 1%. t-ratios in parentheses.