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Joint claims for JSA extension:
A technical report on the quantitative evaluation of labour market effects

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Acknowledgements

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Glossary

DiD	Difference-in-differences.
The Extension	Joint Claims for Jobseeker's Allowance age range extension.
IB	Incapacity Benefit.
IS	Income Support.
Joint Claims	Joint Claims for Jobseeker's Allowance.
JSA	Jobseeker's Allowance.
JSAPS	Jobseeker's Allowance Payment System.
JUVOS	Joint Unemployment & Vacancies Operating System, maintained by the Office for National Statistics.
LMS	Labour Market System (used by Jobcentre Plus advisers).
MIDAS	Matching Intelligence Data Analysis Service – provide data on JSA claims, derived from JSAPS.
RG	Random Growth.

1   Introduction

1.1   An overview of Joint Claims for JSA

Joint Claims for Jobseeker's Allowance (JSA) (hereafter, 'Joint Claims') was introduced on 19 March 2001. Previously, (income-based) JSA could be received at the increased rate for a dependent partner yet there was no obligation for that partner to seek work.[1] Joint Claims effectively removed the distinction between claimant and dependent partner such that both were required to satisfy the requirement of actively seeking, and being available for, work. The aim of this was to address the problem of workless households by extending the job search assistance provided to JSA claimants to both partners. Effectively, this brings a group of individuals closer to the labour market with the intention of increasing their chances of employment and, consequently, encouraging couples to move away from dependency on JSA.

The new rules only applied to couples without dependent children where at least one partner was aged 18 or over and born after 19 March 1976. At the time of introduction, this translated to couples with at least one partner aged between 18 and 24 years, but the age range has broadened naturally ever since. On 28 October 2002, the age range covered by Joint Claims was extended. Currently, the age criterion is that at least one partner be aged 18 or over and born after 28 October 1957. This is the Joint Claims 'Extension'.

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[1] Contribution-based JSA is payable, at a personal rate, for up to six months for those who have paid sufficient National Insurance contributions. Those who do not qualify for, or whose needs are not met by, contribution-based JSA may qualify for income-based JSA for themselves and their dependants. Similarly, those receiving contribution-based JSA may switch to income-based JSA when their period of eligibility for contribution-based JSA expires. Income-based JSA is payable for as long as needed, provided that the qualifying conditions continue to be met.

1.2 The evaluation of Joint Claims

The extent to which Joint Claims is successful in encouraging benefit exit and job entry is of clear policy interest. Earlier reports (Bonjour *et al.*, 2001, 2002) examined this issue when Joint Claims was originally introduced. The results suggest Joint Claims was eventually effective in speeding JSA exit.[2] The quantitative evaluation was accompanied by qualitative analyses comprising three elements:

• pre-implementation analysis of potential joint claimants (Fielding and Bell, 2001);

• case-study research on delivery (Fielding *et al.*, 2001);

• post-implementation analysis of joint claimants (Fielding and Bell, 2002).

This technical paper supports the previously published report 'Joint claims for JSA Evaluation – Synthesis of findings', Department for Work and Pensions (DWP) research report, no 235, March 2005. It is helpful to be aware of how the Extension may affect labour market behaviour. Two effects are relevant. 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 Extension. 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 and, therefore, do not commence an income-based JSA spell.

All that can be estimated for couples beginning a joint claim (the 'flow') is the direct effect since, by definition, they have not been deterred and so are observed in the available data. However, couples who became eligible for Joint Claims at the time of the Extension (the 'stock') are recorded in unemployment records, and for them the deterrent effect may be an important influence. This may be for a number of reasons. Fraudulently claiming for a non-existent partner is one possibility. The Joint Claims requirement for both partners to attend interviews makes this type of fraud more difficult. Consequently, the change in legislation might 'shake-out' such fraudulent claims. Other scenarios are possible. For example, couples with an existing JSA claim may increase job search activity or switch to other benefits as a result of notification of the need to convert to Joint Claims. Due to the nature of the data, it is not straightforward to distinguish the direct effect from the deterrent effect for the stock. Consequently, what is identified is the combined direct and deterrent effects. In view of this difference between stock and flow in the effects that the Extension could have, they are considered separately in the evaluation.

A final point to bear in mind is that the basic unit of analysis in the evaluation is the couple. No consideration is given to the case where couples break up during the observation period. While it is conceivable that Joint Claims influences partnership dissolution, the question of how to interpret its labour market effects on the individuals within couples who do break up is not straightforward.

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[2] Note that exits may be due to either partner finding work or another reason such as a move to a different benefit.

2 Overview of the data and the problems therein

As with the evaluation of the original Joint Claims, a considerable amount of attention was devoted to constructing, from administrative records, the database that was to be used for subsequent modelling. The main complication (as before) was that data were drawn from two sources and these would often not agree. Joint claimants were identified from the Labour Market System (LMS) while all other couples were identified ultimately from the Joint Unemployment and Vacancies Operating System (JUVOS).

The one intended difference is that, unlike before, the database constructed for this evaluation considers not only the latest spell for a couple, but all spells. This should provide a more accurate account of the population. A more important difference is that, since the time of the 18-24s evaluation, there has been an organisational change in the management of the data, with ASDIC assuming the role previously carried out by MIDAS[3]. While this should not have caused any problems, in practice it has proved impossible to completely reconcile the new data with the old.

There are a number of issues that are worth highlighting since they influence the approach to the evaluation. First, as with the original evaluation, far from all of those who appear eligible are actually recorded as making a Joint Claim. Figure 2.1 shows the number of cases identified from the transformed database as eligible for Joint Claims. On its introduction, only a quarter of those appearing eligible were recorded as making a Joint Claim; this is shown in Figure 2.1 by the fact that the 'unconverted' heavily outnumber those making a Joint Claim. This is not surprising since jobcentres were allowed a grace period of six months in introducing Joint Claims. That is, it was acknowledged that during this transition period there would be some eligible couples who made a Joint Claim and some for whom only one partner was required to seek work. However, it appears from the data that this conversion was never achieved. By October 2003, two-and-a-half years after Joint Claims was first

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[3] ASDIC and MIDAS use the same data, but processed by different sections within the Department for Work and Pensions (DWP). They process the data slightly differently so there may be some discrepancies.

introduced, the proportion of couples seemingly eligible for Joint Claims under its original definition who were in fact making a claim was only 54 per cent. There is a sudden jump in the number of eligible couples which corresponds to the Extension at the end of October 2002. The same pattern is evident; by the end of the period, only 44 per cent of eligible Extension couples were making a Joint Claim.

Figure 2.1   The population eligible for Joint Claims
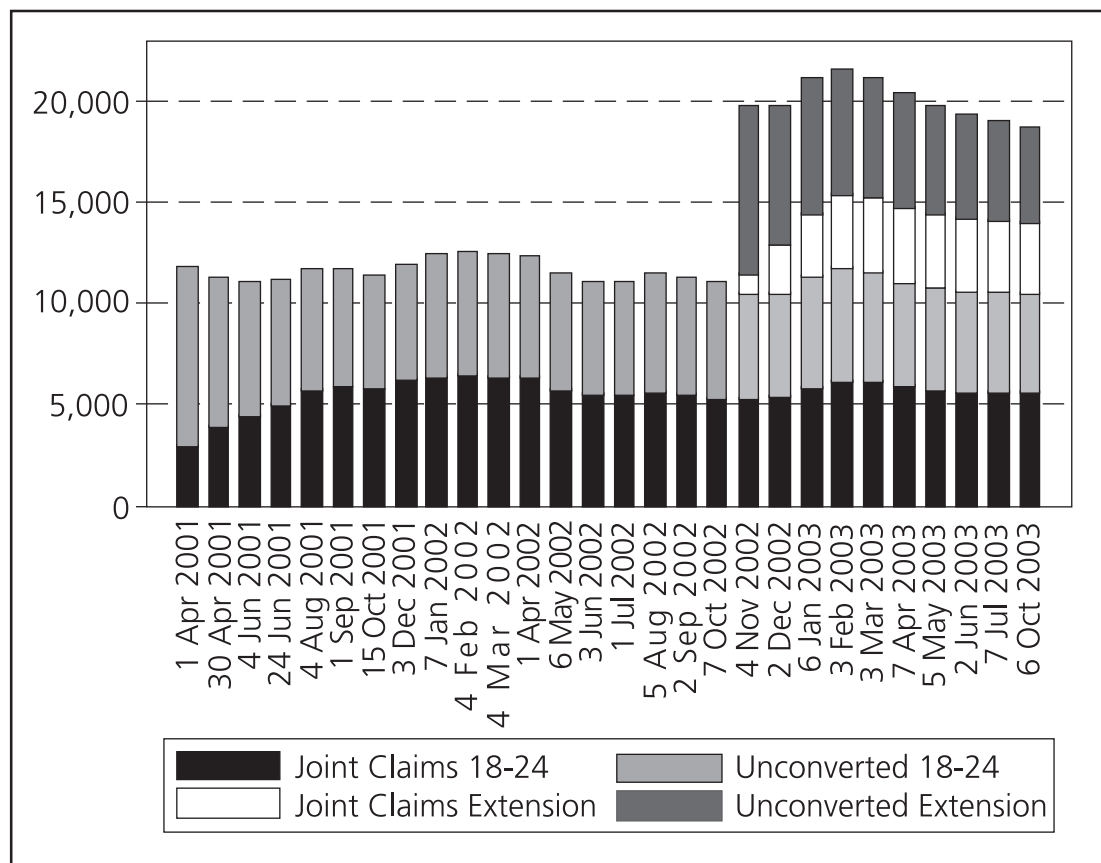
The high incidence of persistent unconverted claims could be due to a substantial number of claims which should have been converted but never are. Alternatively, there may be a uniform delay in conversion but no claims remain unconverted for long. Most likely, both explanations play a role. This is explored in Figure 2.2, which, for simplicity, considers only Extension couples.

In Figure 2.2, the eligible population at the time of each of the scans is divided not just into converted and unconverted claims but also by whether or not the claim is a stock claim. From this it appears that the number of unconverted stock cases declined with successive months, although a substantial minority had very long unconverted claims. The numbers of converted stock claims grew initially but then dropped again as couples ended their claim (or had children and, therefore, became ineligible). For non-stock cases (that is claims that began after the Extension), the number of unconverted cases grew over the first few months but seemed to reach a stable level thereafter. The same is true for converted non-stock cases. In short, both stock and non-stock cases contribute to the ongoing unconverted numbers, although the relative importance of the former declines over time.

Figure 2.2   Stock and non-stock conversions for Extension couples
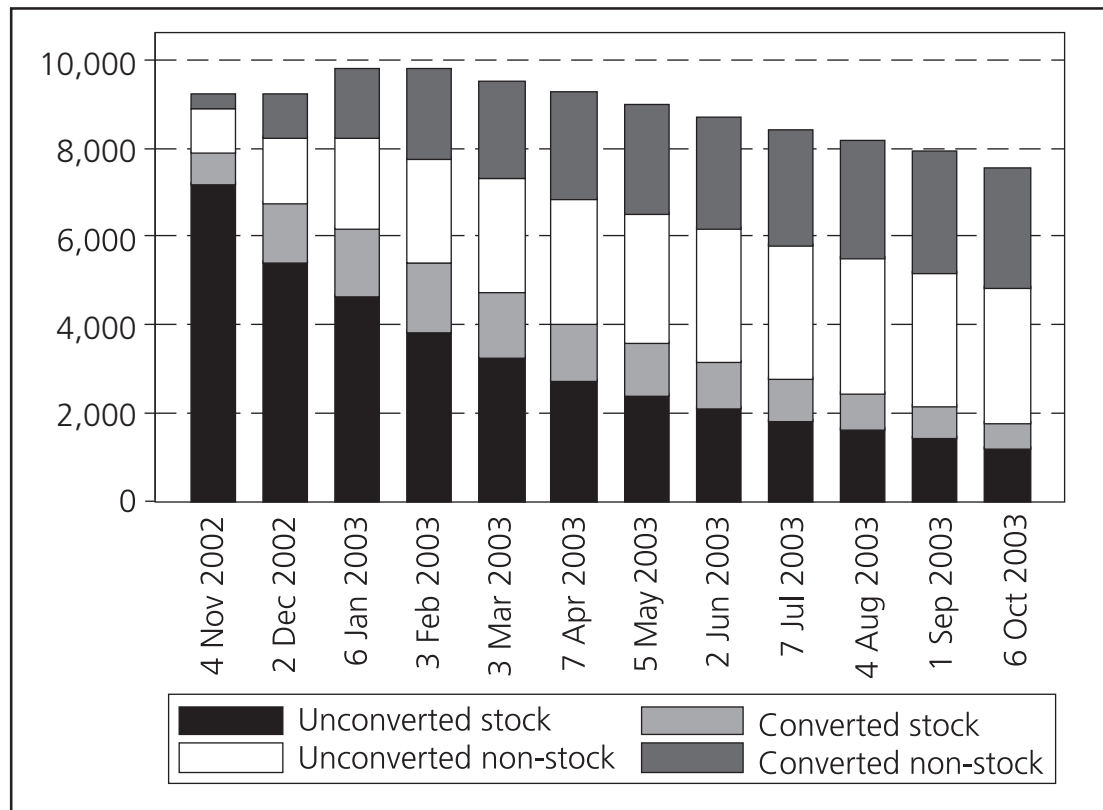
Despite detailed investigation into the source data, it remains unclear how to interpret the fact that so few of the apparently eligible couples appear to be making a Joint Claim. Possibly, those not recorded as Joint Claimants are, in fact, subject to Joint Claims conditionality[4]. In this case, estimating the effect on all eligible couples would be a valid approach. Conversely, those not recorded as Joint Claimants may truly not be subject to Joint Claims conditionality. In this case, other approaches are available to recover the effect of Joint Claims on those people to whom it applies (see, for example, Bloom, 1984).[5] In all probability, the true position will lie somewhere between these two extremes.

One problem with the data that may be partly responsible for the apparent low rate of conversion to Joint Claims is that couples claiming IB may be included erroneously among the eligible group. The structure of the data means that it is not possible to be

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[4] Under Joint Claims, one person in the couple can be claiming Incapacity Benefit (IB) and the other Jobseeker's Allowance (JSA). They are still classed as being part of the Joint Claim, but the person on IB is exempt from fortnightly job reviews.

[5] It is important to interpret the estimate resulting from the Bloom adjustment carefully. In particular, the estimate applies only to those who are in fact making a Joint Claim. This group is likely to comprise couples who are systematically different from those who appear eligible yet are not making a Joint Claim. For this reason, it is not valid to assume that, if those not making a Joint Claim were in fact doing so, the impact for them would be the same.

definite about this since receipt of IB at the time of a past claim is not recorded; all that is observed is whether IB is claimed currently. The effect of removing those couples receiving IB when last observed from the database is illustrated in Figure 2.3. The same overall pattern is evident, although the conversion rate is slightly higher; 63 per cent and 53 per cent for the original Joint Claims population and the Extension population, respectively, by October 2003.

Figure 2 .3   The population eligible for Joint Claims, excluding those receiving IB
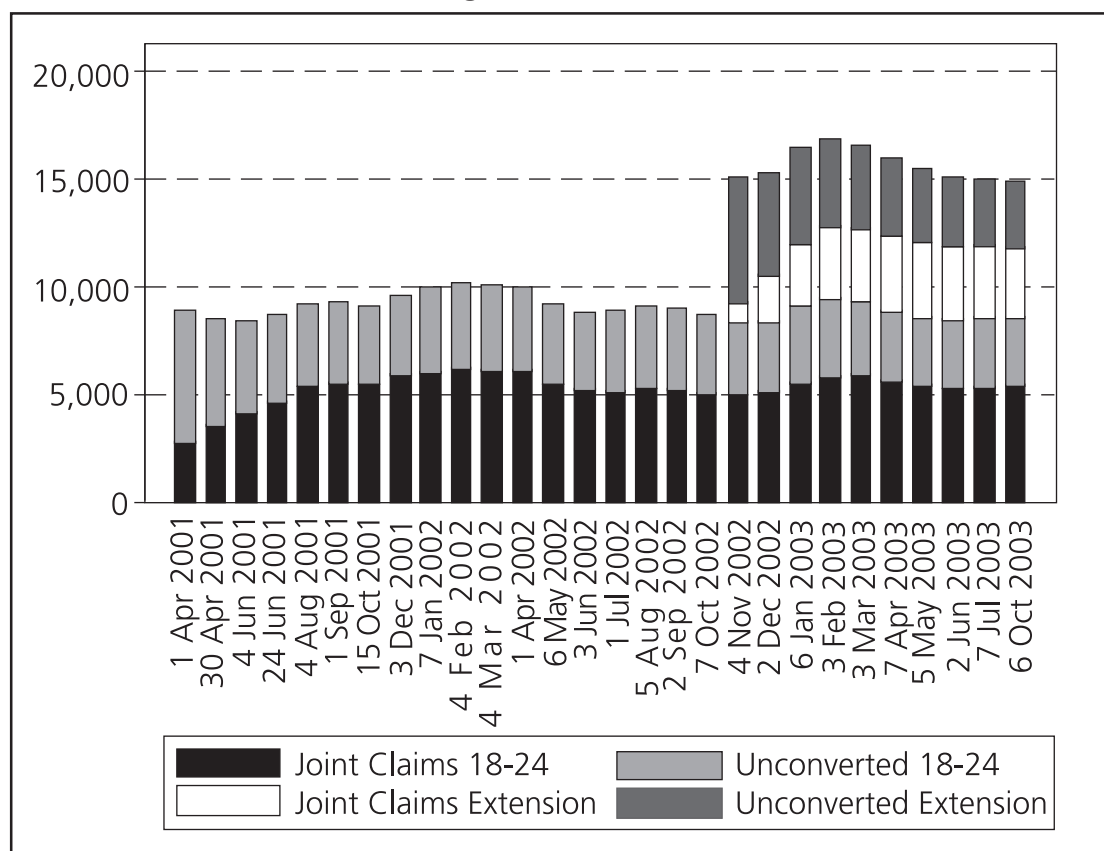
In a number of cases, the destination on spell exit is recorded as a 'transfer to clerical claim'. There is a difficult question of how to deal with such spells since it is not clear whether the spell has actually ended or whether it is ongoing but is being dealt with clerically. It may be the case that the spell has ended but that a problem with Jobseeker's Allowance Payment System (JSAPS) was encountered when attempting to close the claim on the system and that the workaround solution is to record the destination as a transfer to clerical claim. In this case, there is no difficulty for the evaluation since the claim truly has ended. Alternatively, the 'transfer to clerical' destination may capture cases where a couple separate. This would also be unproblematic if this were the sole reason for transfers; since the evaluation only focuses on couples that remain intact, we could proceed by excluding couples who separate from the analysis. The real difficulty for the evaluation arises when it is problems with live claims that can lead to transfer. In principle, such spells should be re-entered onto JSAPS at a later date. There is no way of knowing whether, and when, such spells actually end. If the couple appears in JSAPS as having a

subsequent spell, there is no way of knowing whether this is indeed a new spell (and thereby indicates that the preceding spell must have ended) or whether the first spell has simply been re-entered on JSAPS as it should be.

Information provided by the DWP suggests that the main reason for claims being transferred to clerical has changed over time. In the past, it was largely due to user error. Joint claims are problematic in processing terms as there is only one possible process to follow. Once a certain point is reached, the only way to remedy any mistakes is to transfer to a clerical claim, then rebuild the claim on JSAPS. When Joint Claims was first introduced, many processing problems were reported. At first, few staff were trained in, or experienced at, processing Joint Claims.

As staff have become more familiar with Joint Claims, so user error has reduced. There are still some issues with front-line staff registering people incorrectly for Joint Claims, or processing them incorrectly. However, the number of transfers resulting from user error should have fallen. Now, transfers more commonly result from the limitations of the IT system. JSAPS only has capacity for a certain amount of information about a claim, and cannot handle claims subject to certain changes. Particularly problematic are the following circumstances:

- Joint Claimants going onto Training for Work/New Deal when they have an overpayment outstanding commonly creates difficulties on the system which lead to exit to a clerical claim.

- Exemptions/hardship can create problems which lead to a claim being exited to clerical.

- If a claim for IB makes someone eligible for an exemption, the case must be exited to a clerical claim, as the system does not recognise JSA and IB as compatible[6].

- If a customer notifies a change which occurred in the past, it cannot be put on the system. For example, an exemption entitlement due to sickness which was reported after the event would mean that the claim had to be made clerical.

- Bulk Office Transfers[7] – when these are carried out Joint Claims have to be made into 2 single claims, and then moved back into a Joint Claim. Quite often people do not carry this out, which leads to the claim having to go to clerical before being put back onto the system.

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[6] Under Joint Claims, one person in the couple can be claiming IB and the other JSA. They are still classed as being part of the Joint Claim, but the person on IB is exempt from fortnightly job reviews. Therefore, JSA and IB are compatible under the Joint Claims regime but not under the computer system.

[7] Bulk Office Transfers occur when an office changes, for example, if it moves location, merges with other offices or changes office code. They involve migrating all records for an office at once.

Transferring to a clerical claim wipes all but the key information off the system and, if it is still live, the claim can then be put back on the system. The time taken to do this depends upon the level of experience in the office concerned, and how busy the office is. Some offices are likely to put claims back onto the system promptly; others may have large numbers which they find it easier to pay clerically.

The problem is illustrated in Figure 2.4. For each scan given on the x-axis, the proportion of exits that are accounted for by transfers is plotted. Two lines are shown. One gives the proportion of exits within one month of the scan accounted for by transfers. The other gives the proportion of exits within six months of the scan accounted for by transfers. There are very few transfers except among the treatment group. It is clear that the jump in the proportion of transfers occurs just at the time of the Extension. The jump is particularly evident when considering 'exits' one month after claim start but is still far from negligible when considering six month outcomes. From this it is likely (and later results will confirm) that the uncertainty surrounding transfers will introduce significant uncertainty into the Extension impact estimates. In view of this, the remainder of this chapter considers the issue in more detail.

Figure 2.4   The proportion of JSA exits recorded as transfers among Extension couples
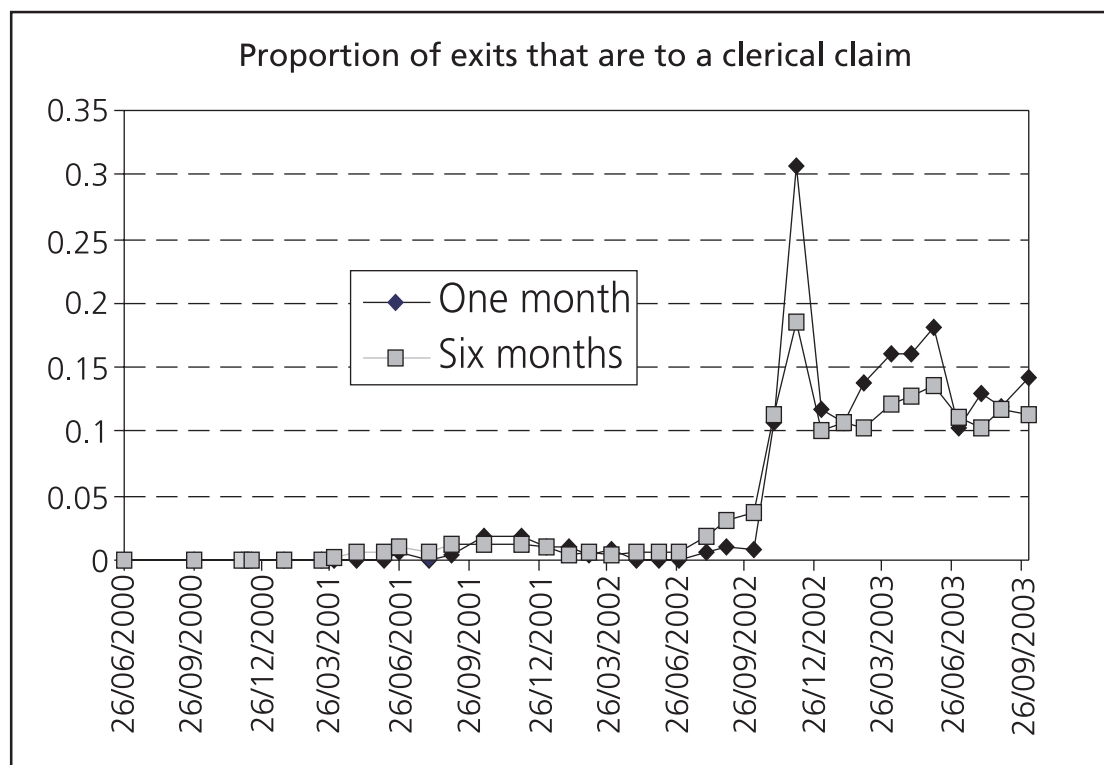
Obviously, the problem for the evaluation is that the JSA status of couples whose spell has transferred is not clear. One possibility is that the tendency to transfer to a clerical claim is essentially a random process. Under this scenario, a proportion of couples whose claim remained live would transfer, as would a roughly equal proportion of those whose claim was ending. To proceed, transfers could simply be excluded from the analysis. This should not bias impact estimates since it amounts to

estimating effects on a random sample of the population. The only drawback to doing this is that the precision of estimates will fall slightly as a result of reduced sample size.

It is helpful to consider whether it is plausible that the transfer process is random. If human error and IT limitations are the main reasons for transfers, there may be nothing systematic that characterises transfers. However, this is not the same as suggesting that the process is truly random. To see this, note that user error and IT problems are more likely to arise where claims are not straightforward and that there may well be a correlation between the complexity of a claim and subsequent labour market prospects. This means that, although there is no systematic intent to the user error or the IT limitations, there may be a systematic bias to the actual transfer process.

Table 2.1 presents some statistics to get an understanding of whether the randomness of transfers is plausible. Each column corresponds to those treatment group couples with a claim live at the time indicated in the column heading. The first row of results examines whether couples whose spell ends in a transfer differ in their likelihood of experiencing a subsequent spell from couples whose spell does not end in a transfer. Were the transfer process random (and the process of re-building claims random) there should be no significant differences. In fact, there are. The entry in the first column shows that, of all couples with a live claim on 4 November 2002, those whose claim ended in a transfer were more likely (by eight percentage points) to have a subsequent claim. Although the difference reduces over time (subsequent columns), this may simply reflect the fact that for later dates, there will have been less opportunity to accumulate subsequent spells.

Table 2.1   Testing differences between transfers and other spells

	Date of scan										
	4 Nov 2002	2 Dec 2002	6 Jan 2003	3 Feb 2003	3 Mar 2003	7 Apr 2003	5 May 2003	2 Jun 2003	7 Jul 2003	4 Aug 2003	1 Sep 2003
Comparing transfers to those not transferring											
Any later spells	0.08**	0.09**	0.05**	0.04**	0.04**	0.04**	0.03**	0.02**	0.02**	0.02**	0.01**
N	9,222	9,243	9,790	9,788	9,537	9,265	8,994	8,720	8,433	8,188	7,909
Comparing transfers to those exiting to a different destination											
Any later spells	0.07**	0.08**	0.04**	0.03*	0.03**	0.03**	0.02*	0.01	0.01	0.02**	0.01

Table 2.1   Continued

	Date of scan										
	4 Nov 2002	**2 Dec 2002**	**6 Jan 2003**	**3 Feb 2003**	**3 Mar 2003**	**7 Apr 2003**	**5 May 2003**	**2 Jun 2003**	**7 Jul 2003**	**4 Aug 2003**	**1 Sep 2003**
Spell length (days)	49.04**	3.72	4.74	4.02	-0.82	-4.86	-10.65	-10.13	-3.94	1.95	0.05
N	8,402	8,328	8,789	8,680	8,328	7,926	7,568	7,167	6,696	6,298	5,836

** - significant at 1 per cent, * significant at 5 per cent.

This result suggests that the transfer process is unlikely to be random. Furthermore, the available understanding of the how transfers arise suggests that factors that may explain it are, by definition, unobserved.

The second two rows of results in Table 2.1 compare couples whose spell ends in a transfer with couples whose spell ends with an exit to a different destination. Again, significant differences are apparent when considering the likelihood of having a subsequent claim. Now, though, it appears that these differences all but disappear over time. This provides some support for the view that, on average, claims ending in a transfer may resemble claims that end for other reasons. It also appears that it is only in November 2002 that there is a significant difference between transfers and other spell ends in the length of claim at the time of scan. It seems that in the early days of the Extension those with longer claims were disproportionately transferred. This might help interpret the spike evident at this time in Figure 2.3.

The fact that couples who transfer to a clerical claim perform differently from other couples undermines the argument that transfer couples may represent a random sample. The approach taken to address this problem is described in Chapter 4. It should be noted that, given the uncertainty surrounding the JSA status of couples whose spell 'ends' with a transfer, it is not possible to say that such cases are 'better' or 'worse' than other cases.

3 Characteristics of the Extension population

The database used in the 18-24s evaluation was constructed as a series of 'scans' of the population at specific dates. The frequency of these scans was approximately monthly. The database available for the Extension evaluation is continuous in the sense that it captures all spells, not just those live at particular dates. However, for the purposes of the evaluation, only spells live at set dates are considered. The reason for doing this is to maintain consistency with the structure of the 18-24s database. With this in mind, the early dates coincide with those used in the 18-24s evaluation. For claims falling after the point covered by the 18-24s evaluation, the first Monday of each month was used. Tables 3.1 and 3.2 show the characteristics of eligible couples for the stock and the flow separately.[8]

Table 3.1   Characteristics of flow couples eligible for the Extension

	Date of scan											
	4 Nov 2002	2 Dec 2002	6 Jan 2003	3 Feb 2003	3 Mar 2003	7 Apr 2003	5 May 2003	2 Jun 2003	7 Jul 2003	4 Aug 2003	1 Sep 2003	6 Oct 2003
M age	39	39	39	39	39	39	39	39	40	39	40	39
F age	37	37	37	37	37	37	37	37	38	37	37	38
M disabled %	20	22	20	20	20	22	21	23	22	21	21	22
M eth min (%)	13	11	11	12	13	14	14	13	12	11	11	11
												Continued

[8] The flow at a given date are those couples whose eligibility for Joint Claims commenced within a month of that date. Stock couple are those who, at 7 October 2003, had a claim of one month or longer.

Table 3.1 Continued

	Date of scan											
	4 Nov 2002	**2 Dec 2002**	**6 Jan 2003**	**3 Feb 2003**	**3 Mar 2003**	**7 Apr 2003**	**5 May 2003**	**2 Jun 2003**	**7 Jul 2003**	**4 Aug 2003**	**1 Sep 2003**	**6 Oct 2003**
M occupation:												
Managers/ senior	9	8	8	7	9	9	9	9	9	10	10	10
Professional	7	5	7	7	7	8	7	7	8	9	9	7
Associate prof	12	10	10	9	10	10	10	10	11	11	8	9
Admin	4	4	3	5	4	5	5	4	5	5	5	5
Skilled trades	18	20	20	22	19	18	16	17	18	16	18	17
Pers. services	2	2	3	2	2	3	2	2	2	2	2	2
Sales	3	4	3	4	4	4	4	4	3	3	4	3
machine ops.	20	19	19	17	19	18	19	18	16	17	18	18
elementary	26	28	28	26	27	26	29	29	27	26	27	28
rural area (%)	14	13	15	14	14	14	16	14	13	15	15	16
Region:												
Scotland	12	12	13	13	11	9	10	11	13	11	9	10
North East	6	6	7	7	6	7	5	5	6	6	7	7
North West	9	11	12	11	11	11	10	11	10	10	10	9
Yorks & Humb	9	12	11	9	9	10	10	11	12	10	10	11
Wales	5	5	5	4	5	5	5	5	4	5	6	5
West Mids	11	11	8	10	11	8	11	10	9	12	9	10
East Mids	7	8	9	8	6	8	9	8	8	7	7	8
East	8	7	7	8	8	9	8	8	7	8	7	7
South East	11	10	10	9	11	11	9	8	10	10	11	10
London	14	13	10	13	14	15	15	15	14	13	16	16
South West	8	6	8	6	8	6	8	7	7	8	7	7
Num claims since 95	4	4	4	4	4	4	4	4	4	4	4	4
JSA days since 95	78	74	77	75	79	78	77	80	71	76	83	77
Size of flow	1,359	1,451	1,297	1,556	1,310	1,273	1,021	1,053	1,083	1,024	953	1,037

Table 3.2   Characteristics of stock couples eligible for the Extension

	07 October 2002
M age	39
F age	37
M disabled %	23
M eth min (%)	16
M occupation:	
Managers/senior	7
Professional	6
Associate prof	10
Admin	5
Skilled trades	18
Pers. services	2
Sales	4
machine ops.	17
elementary	31
rural area (%)	11
Region:	
Scotland	11
North East	6
North West	11
Yorks & Humb	10
Wales	5
West Mids	11
East Mids	7
East	6
South East	8
London	18
South West	6
No. claims since 95	4
Time on JSA since 95	94
Length of JSA spell at 7 October 2002 (%):	
none	1
up to 2 weeks	7
2+ to 4 weeks	6
4+ to 12 weeks	18
12+ to 24 weeks	19
24+ to 52 weeks	24
1+ to 2 years	16
more than 2 years	9
Size of stock	**1,359**

Table 3.3 shows the tendency for flow couples to exit Jobseeker's Allowance (JSA). In the post-Extension period, there has been a tendency for eligible couples to exit

their current JSA spell more quickly. This was mainly concentrated in the first three months following the Extension, as shown in Figure 3.1.

Table 3.3   Percentage of eligible flow couples unemployed X months post-scan

| | 2002 | | 2003 | | | | | | | | | |
	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct
One month post-scan	84	88	79	79	82	84	81	81	82	80	81	80
Two months post-scan	77	74	64	66	69	69	66	68	66	64	65	65
Three months post-scan	66	63	53	56	60	57	56	58	55	53	56	58
Four months post-scan	57	55	44	48	50	48	49	48	46	46	48	51
Five months post-scan	49	48	38	42	43	43	43	42	41	42	44	43
Six months post-scan	42	41	32	38	38	36	35	35	37	38	39	36
Total	1,359	1,451	1,297	1,556	1,310	1,273	1,021	1,053	1,083	1,024	953	1,037

Figure 3.1   Changes over time in the percentage of eligible flow couples remaining on JSA
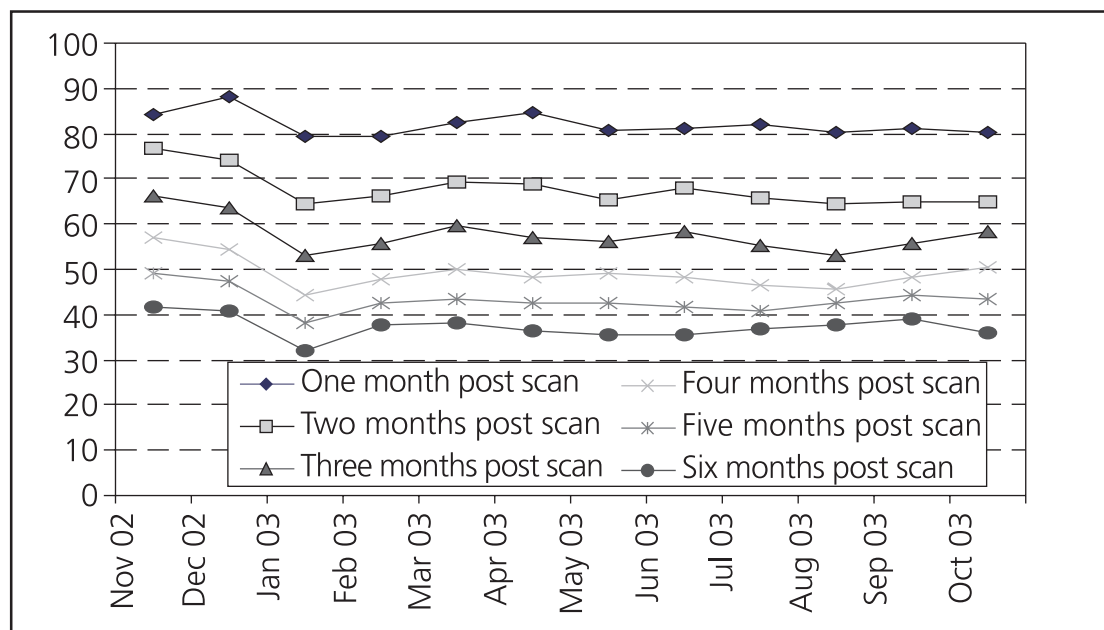
Table 3.4 shows analogous results for stock couples. As an overall comment, it is clear that stock couples take longer than flow couples to exit JSA. This is consistent with the wellknown phenomenon of exit rates falling as spell duration increases.

Table 3.4 Percentage of eligible stock couples unemployed X months post-scan

	unemployed
One month after Extension	86
Two months after Extension	75
Three months after Extension	70
Four months after Extension	62
Five months after Extension	55
Six months after Extension	49

Tables 3.5 and 3.6 show the destinations for stock and flow couples exiting JSA (where this is recorded). The biggest single destination for flow couples is employment. The second most common reason for the claim ending is a failure to attend. Transfers to government training account for six to eight per cent of recorded exits, a similar proportion to that accounted for by transfers to other benefits (Incapacity Benefit (IB), Income Support (IS)). Finally, transfers to a clerical claim account for a sizeable proportion of exits. This proportion is particularly high for December 2002.

Table 3.5 Destinations for eligible flow couples exiting JSA

	2002		2003									
	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct
Ceased claiming	2	2	2	2	2	3	1	2	2	2	3	2
Found work	41	37	46	42	42	40	40	39	41	45	42	40
Gone abroad	4	2	3	2	4	4	4	3	3	3	3	3
Claimed another benefit	2	2	1	2	2	1	1	2	1	1	1	1
Full-time education	0	0	0	0	0	0	0	0	1	0	1	0
Unknown reason	2	2	2	2	1	2	4	3	2	2	3	3
Failed to attend	19	18	14	17	17	18	17	17	18	16	16	21
Sickness benefit claimed	1	1	1	0	1	1	1	1	1	0	0	1
Transfer to Govt. training	8	7	7	8	8	8	6	8	8	8	6	6
Working 16 hrs+ (on average)	1	1	1	1	1	1	1	1	1	1	1	1
Claimed Incapacity Benefit	4	4	4	4	5	5	6	4	4	3	3	5
Claimed Income Support	3	2	3	2	3	3	3	3	2	2	3	2
Other reason	3	4	3	4	4	3	3	3	4	2	5	4
Transfer to clerical claim	10	17	10	12	12	12	13	14	12	11	13	12
Total	1,268	1,347	1,213	1,426	1,191	1,151	915	920	915	843	745	798

For stock couples, Table 3.6 shows that fewer claims (about a third) end due to job entry. Failure to attend is at a roughly similar level to that seen among flow couples. Transfers to government training and to other benefits are more common while transfers to a clerical claim are slightly less common.

Table 3.6   Destinations for eligible stock couples exiting JSA (those with live claims of more than four weeks on 7 October 2002)

	October 2002
Ceased claiming	3
Found work	32
Gone abroad	3
Claimed another benefit	2
Full-time education	0
Unknown reason	2
Failed to attend	17
Sickness benefit claimed	1
Transfer to Govt. training	16
Working 16 hrs+ (on average)	1
Claimed Incapacity Benefit	8
Claimed Income Support	4
Other reason	4
Transfer to clerical claim	8
Total	7,374

4   Modelling approach

4.1   An overview of the approach

The evaluation uses a difference-in-differences approach. This operates by comparing before-after differences for a 'treatment' group to before-after differences for a comparison group. Differencing in this way aims to net out unobservable fixed and trend effects so that any resulting difference can be attributed to the treatment. The parameter estimated is the average effect of the treatment on the treated.[9]

This is an attractive approach, particularly when the use of administrative data means that the information set is insufficiently rich to justify the use of alternative approaches. However, the plausibility of its underlying assumptions should be considered. The Difference-in-differences (DiD) estimator relies on the composition of the samples in the periods 'before' and 'after' periods remaining unchanged. If individuals can choose not to experience the intervention, this might have an effect on the accuracy of the DiD estimator, as particular factors may lead some individuals to be more likely to opt-out than others. Should these self-absenting individuals be different with regard to characteristics likely to affect outcomes, a bias in the achieved DiD estimate can result. In the case of the Extension, this is unlikely to be a problem for two reasons: First, in order to evade Joint Claims, couples would have to know about it in advance. Since it has not been widely publicised it is likely that levels of awareness in advance are low. Second, there are few other options open to couples eligible for Joint Claims. That is, the choice of whether to evade it is constrained. For these reasons, it is assumed that the assumption of constant composition holds.

Another assumption is that the before-after estimate for the comparison group is the same as would have been estimated for the treatment group had Modelling treatment not been introduced. Some insight into the plausibility of this assumption of

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[9]   This description relates to the standard exposition of the DiD model. In the current application, not all of those in the treatment group actually receive the treatment. In this case, the parameter identified is the average effect of intention to treat on the eligible group.

common trends can be achieved by inspecting the extent to which outcomes for the comparison group track those of the treatment group in the period before the Extension. It can also be investigated more formally using pre-programme tests (Heckman and Hotz, 1987). This involves estimating effects based on two periods of time that wholly pre-date the treatment. If the treatment and comparison groups are affected equally by general economic conditions and other influences, such estimates should be insignificant. If they are not, it suggests that using DiD to evaluate treatment effects will result in biased estimates. In this case, a modification to the standard DiD framework is needed. One possibility is to use the random growth model (Heckman and Hotz, 1987). This operates by regarding the results of the pre-programme estimates as estimates of the bias resulting from the inappropriateness of the comparison group. Essentially, the random growth model operates by subtracting the bias revealed through the pre-programme tests from the treatment effect estimated using DiD on the 'before' and 'after' periods. In this way, unbiased estimates can be achieved.

A third assumption is that any idiosyncratic, unobserved temporary effect (that is, any unobserved variable remaining after differencing out the fixed and trend effects) plays no part in selecting into the treatment group. With a mandatory treatment such as the Extension, this is less of a concern than with a voluntary training programme, for example.

The models are presented formally below. For completeness, three specifications are considered: DiD, the random growth model and an extension of the random growth model. For clarity, we abstract from additional regressors, X, although they are included when the model is estimated.

4.2   The difference-in-differences model

The variables included in the exposition are:

Y_i = outcome for individual i

D_i = 1 for treatment group members and $D_i = 0$ for comparison group members

$D_s = 1$ for observations in period s and $D_s = 0$ for observations not in period s.

Suppose that there are two time periods so that $s \in \{t', t\}$ and where the treatment starts in period k, with $t' < k < t$.

In period t'

$$Y_{it'} = \beta_0 + \beta_D D_i ,$$

and in period t

$$Y_{it} = \beta_0 + \beta_D D_i + \beta_t + \beta_2 D_i ,$$

where β_D is the differential effect for the treatment group, β_t is the common time trend and β_2 is the treatment effect. The DiD treatment effect is then estimated as in the following equation:

(1) $\qquad Y_i = \beta_0 + \beta_D D_i + \beta_t D_t + \beta_2 D_t D_i + \varepsilon_i$.

4.3   The random growth model

Suppose now that there are three periods, so that $s \in \{t, t', t''\}$, where $\qquad$ and where the treatment starts in period k, with $t' < k < t$.

In the first time period

$Y_{it''} = \beta_0 + \beta_D D_i$,

and in the second period

$Y_{it'} = \beta_0 + \beta_D D_i + \beta_t + \beta_1 D_i$,

where β_t is the common time trend and β_1 is the differential time trend for the treated in the absence of treatment. In the third time period,

$Y_{it} = \beta_0 + \beta_D D_i + 2\beta_t + 2\beta_1 D_i + \beta_2 D_i$, $\qquad$ $\beta_2 < t' < t$

where both time trends get incremented by an additional period, and the final term, the treatment effect, is added. Note that this formulation depends on equally spaced time periods; a slightly more general formulation would apply to the case of unequally spaced time periods.

Under this formulation, the treatment effect can be estimated under the random growth model using the single equation:

(2) $\qquad Y_i = \beta_0 + \beta_D D_i + \beta_t(D_{t'} + 2D_t) + \beta_1(D_{t'} D_i + 2D_t D_i) + \beta_2 D_t D_i + \varepsilon_i$

where the coefficient of interest is β_2, which gives the difference between the two underlying difference-in-differences estimates, under the assumption that the common time trend remains fixed over the three time periods.

4.4   Random-growth model with non-constant growth over time

In the first time period:

$Y_{it''} = \beta_0 + \beta_D D_i$,

and in the second period:

$Y_{it'} = \beta_0 + \beta_D D_i + \beta_{t'} + \beta_1 D_i$,

where $\beta_{t'}$ is the common time trend between period t'' and t' and $\beta_{1'}$ is the differential time trend for the treated in the absence of treatment. In the third time period,

$$Y_{it} = \beta_0 + \beta_D D_i + \beta_{t'} + \beta_t + 2\beta_1 D_i + \beta_2 D_i \,,$$

where β_t is the common time trend between period t' and t, the differential time trend, β_1, the same as in the second period, and the final term, the treatment effect, is added. Note that although this formulation assumes equally spaced time periods, there is no requirement for the common time trend over the three periods.

Under this formulation, the treatment effect can be estimated under the random growth (RG) model using the single equation:

(3) $\qquad Y_i = \beta_0 + \beta_D D_i + \beta_{t'}(D_{t'} + D_t) + \beta_t D_t + \beta_1(D_{t'} D_i + 2D_t D_i) + \beta_2 D_t D_i + \varepsilon_i$

where the coefficient of interest is β_2, which gives the difference between the two underlying difference-in-differences estimates.

This model also allows testing of the more restrictive DiD and random growth specifications. A significant value of β_1 amounts to a rejection of the hypothesis of the differential trends between the treatment and control groups and, therefore, implies that the straightforward DiD model is inappropriate. Significantly different values of $\beta_{t'}$ and β_t imply that the differential time trends between the treatment and comparison periods are not constant over time and, therefore, the random growth model that imposes this restriction is not appropriate.

4.5   Addressing the data problems

In Chapter 2, the data problems facing the evaluation were presented. These are substantial. In essence, the aim of the evaluation is to measure the effect of a treatment on an outcome when both the treatment and the outcome may be measured with (non-trivial) error.

Taking the treatment variable first, a bounding approach is used to address the uncertainty over whether those who appear to be eligible but for whom there is no record of having a Joint Claim, actually receive the treatment. First, the effect of eligibility (as assessed from the available data) is estimated. The second effect is estimated by dividing the eligibility effect by the proportion of the eligible group who are recorded as receiving the treatment (see, for example, Bloom, 1984).

These two estimates constitute the bounds. To see this, note that the effect of eligibility will be identical to the effect of treatment if **all** of those who appear eligible but are not recorded as receiving the treatment, do in fact receive it. Conversely, the Bloom-type estimator assumes that **none** of those who appear eligible but are not recorded as receiving the treatment receives it.[10] Hence, the two estimates accord

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[10] The Bloom-type estimator also assumes the Extension only has an effect on those who actually receive the treatment.

with the two extreme assumptions regarding the treatment status of those who are eligible with no treatment record. The true treatment effect should lie between these two bounds.

The situation is further complicated by the measurement error in the outcome variable. The standard econometric result that error in the measurement of the dependent variable does not bias estimates, relies on there being no correlation between this error and the regressors. Since the measurement error in this case only affects Extension couples in the post-Extension period, this condition does not hold and estimates of the parameter of interest will be biased. Since the measurement error is not known, it is not possible to achieve a point estimate of the parameter of interest. Again, a bounding approach is needed.

Here, estimates are produced under two opposing assumptions: that **all** spells ending in a 'transfer to clerical claim' constitute valid exits from JSA, and that **none** do. Estimates produced under the first assumption (all transfers are exits) represent an upper bound since they maximise the proportion of JSA exits in the post-Extension treatment group. Estimates produced under the second assumption minimise this proportion and, therefore, provide a lower bound. Other possible approaches such as removing all transfer cases from the analysis are complicated for the reasons discussed in Chapter 2 and are not pursued further.

Finally, there is also the standard problem that for a proportion of those whose claim ends (for reasons other than a transfer), their destination is unknown. This complicates the evaluation of the effect on employment. To address this, results are produced under the opposing assumptions that **all** of those exiting Jobseeker's Allowance (JSA) to an unknown destination enter work and that **none** of them does.

5 Selecting the comparison group

An important consideration in a Difference-in-differences (DiD) analysis is the choice of comparison group. The role of the comparison group is to represent the change in outcomes that the treatment group would have experienced had the treatment not been introduced. That is, the comparison group can provide an estimate for how things might have changed for those affected by the Extension had it not occurred.

Given the important role of the comparison group, it is worthwhile investigating the extent to which it can provide a credible counterfactual. This can be done by considering the extent to which differences between those in the treatment group and those in the comparison group have existed in the past. It is not necessary that there be no difference in outcomes between these groups; only that the changes over time be comparable to those of the treatment group. To see this, note that the DiD estimator is simply the difference between a before-after estimate for the treatment group and a before-after estimate for the comparison group. The reason why the DiD estimator is often preferred to the before-after estimator is that, as noted earlier, it can control for changes that would have affected outcomes of the treatment group anyway. Not to control for these changes would risk conflating the effect of the treatment with the effect of other ambient influences (such as the trends in the broader economy, seasonal effects, etc). Hence, it is only changes, not levels, which are important.

There were three main candidates for a comparison group:

- existing Joint Claims couples;
- couples claiming Jobseeker's Allowance (JSA) but who have children;
- single JSA claimants.

Arguments could be constructed for using any of these groups since none of them is directly affected by the extension but all of them can be seen to share two of the three defining characteristics of the treatment group (relating to age, presence of children and partnership status). The first group is made up of couples without

children but will have a different age distribution. The second group can be constructed to include similarly aged couples. However, they differ in that they have children. The third group differs in partnership status.

In fact, single JSA claimants were quickly ruled out as a potential comparison group. This was partly because of radically different flows away from JSA but also due to issues relating to the comparability of available variables. Specifically, it was desirable to include variables that captured characteristics of the couple as a whole when modelling outcomes. This was clearly not possible when considering single claimants. Accordingly, attention focused on choosing between existing Joint Claims couples (referred to as the 'young' comparison group in this section in order to avoid confusion) and those couples with children (the 'children' comparison group).

The remainder of this section assesses the suitability of the two potential comparison groups. This is done by focusing on the flow of eligible couples; that is, couples whose eligibility for Joint Claims commenced within a month of the scan in question.

5.1   Graphing movements off JSA

First, the tendency of couples in the comparison group to exit JSA is compared to that of Extension couples in the pre-Extension period. Specifically, the JSA status of couples one, two, three and six months after their scan date is graphed. Each chart has two panels. In the upper panel, the respective outcome is plotted for all three groups; Extension couples, young couples and couples with children. This allows inspection of the extent to which the candidate comparison groups tracked the Extension couples. In the bottom panel, this comparison is brought into sharper focus by plotting separately the differences in outcomes relative to Extension couples for both young couples and couples with children. Both panels also feature a vertical line. This indicates the point beyond which the outcome in question relates to a period after the Extension was introduced. Such outcomes cannot be considered unaffected by the Extension and so outcomes and differences to the right of this vertical line should be disregarded when assessing the suitability of the comparison group.

Following the four charts, a table presents the mean and standard deviation of the differences in outcomes at one to six months after the scan dates are presented. These calculations are performed over all scan dates for which the outcome in question pre-dates the Extension. This is equivalent to calculating the differences across all those scans that lie to the left of the vertical lines in the charts.

To address the possibility that the results vary according to whether those recorded as receiving Incapacity Benefit (IB) are included (see Chapter 2 for a discussion of this point), the charts are also presented after excluding IB claimants. Hence, eight charts (and two tables) appear rather than four charts (and one table).

It is clear from these charts that there is considerable volatility over time for all outcomes. However, it is also evident that there are marked differences in how well the comparison groups track the Extension group. This is best seen in the bottom panel of each chart. A close tracking would be reflected in a flat line. That is, we are not interested in the location of the line, but simply in how flat it is. Summarising all charts, the children comparison group is consistently flatter than the young comparison group, indicating a better performance of the former group.

These results show that, on average, the differences between Extension couples and those in the young comparison group are smaller than those for couples with children. However, as already noted, the size of these differences (which translates into the location of the line) is not as important as the extent to which outcomes for those in the comparison group change over time in a similar way to those for Extension couples. Such variability is captured by the standard deviation. The consistently closer tracking of the children comparison group evident in the charts, is seen again when examining the standard deviations of the differences. These are mostly lower than for the young comparison group and point to a more stable relationship with the Extension couples, especially when considering longer-term outcomes.

Figure 5.1 Assessing comparison group: 18-24s (young) versus those with children

Figure 5.1   Continued

Unemployment three month post-scan
excluding elig==5

Unemployment six month post-scan
excluding elig==5

Extension ----Young
Children (all)
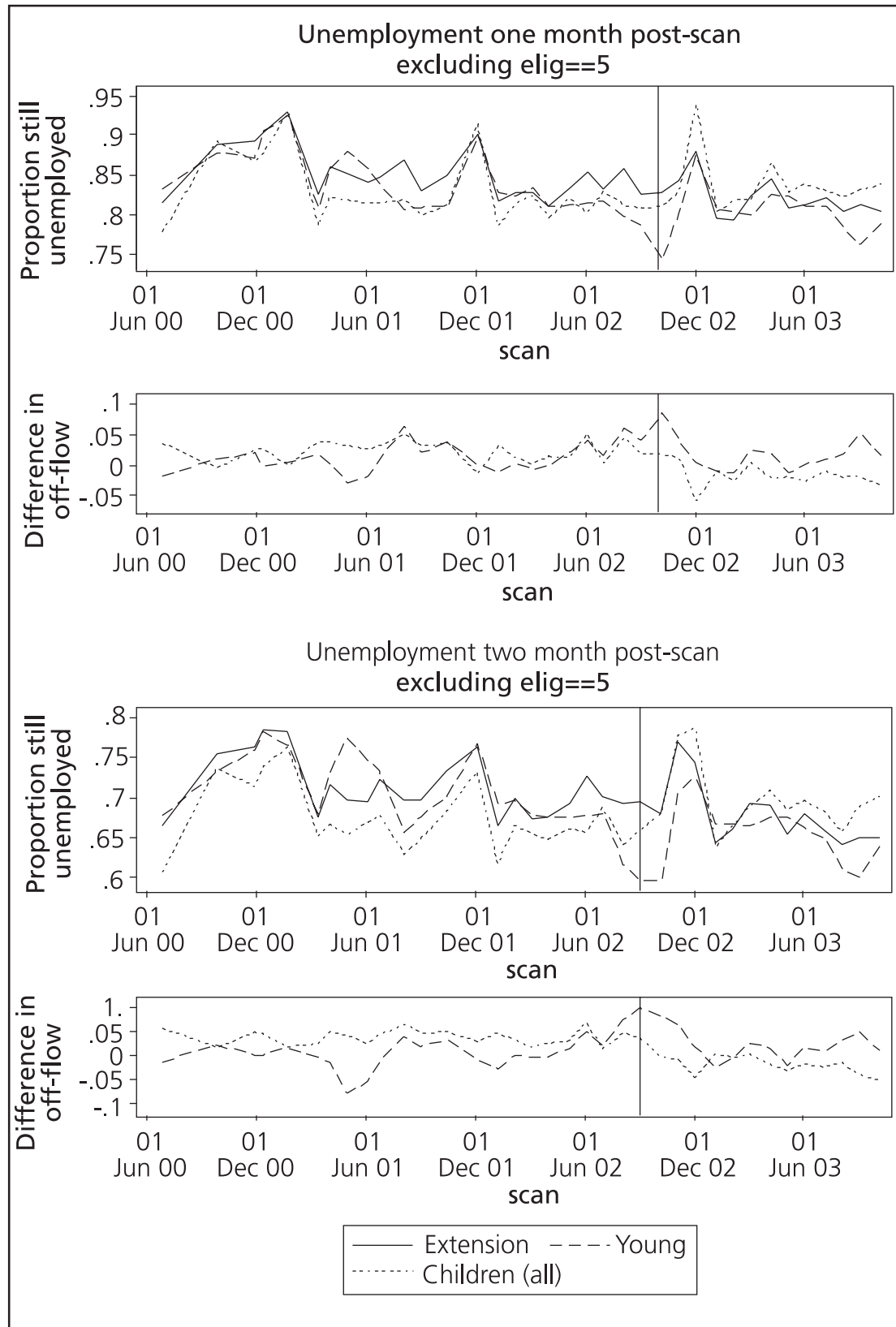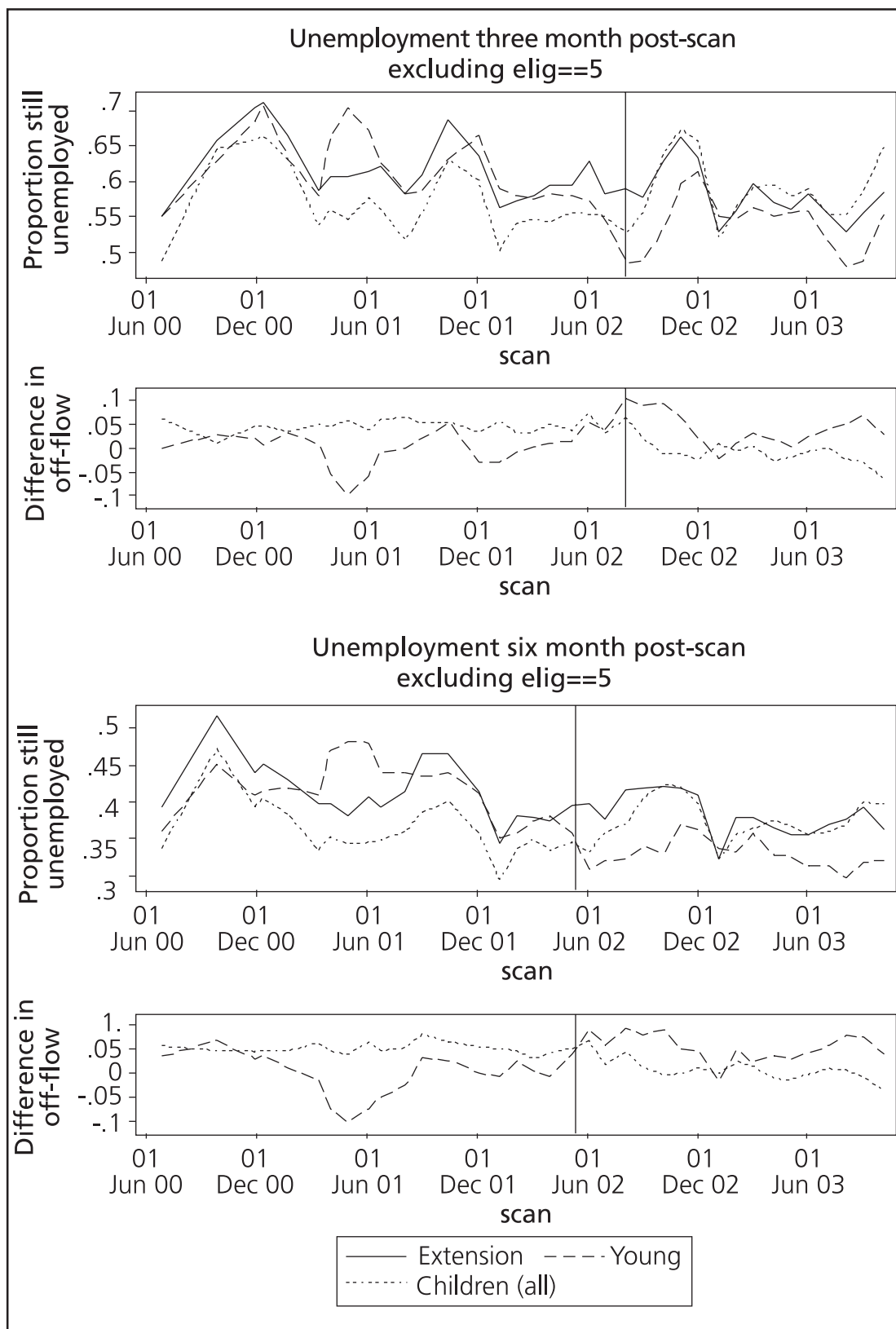
Table 5.1   Comparison group

Months post-scan	18-24 no children		27-45 children	
	mean	s.d.	mean	s.d.
1	0.01	0.02	0.02	0.02
2	0.00	0.03	0.04	0.02
3	0.00	0.04	0.05	0.01
6	0.00	0.04	0.05	0.01

Figure 5.2   Assessing comparison group: 18-24s (young) versus those with children, excluding any couples recorded as claiming IB
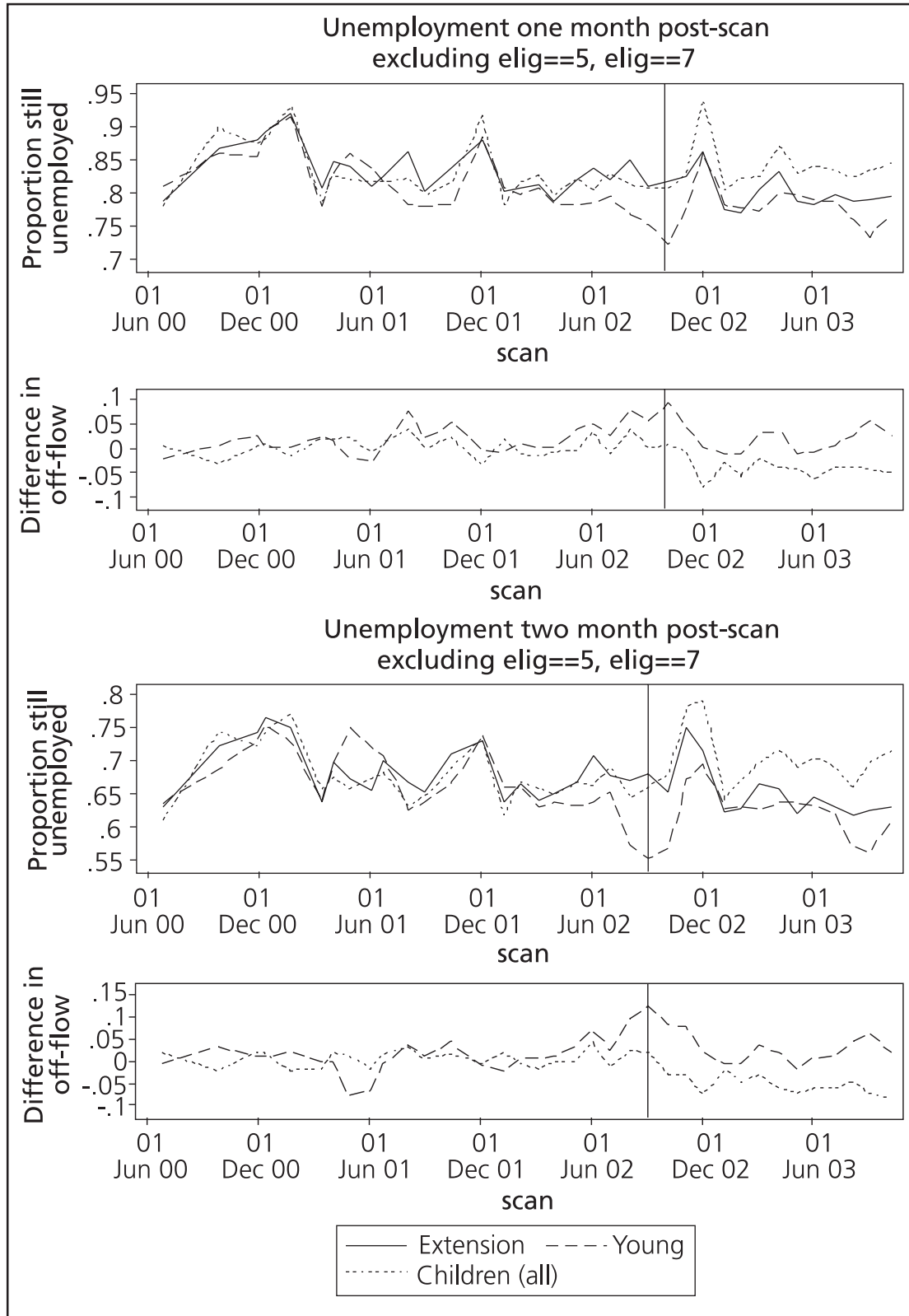
Figure 5.2   Continued
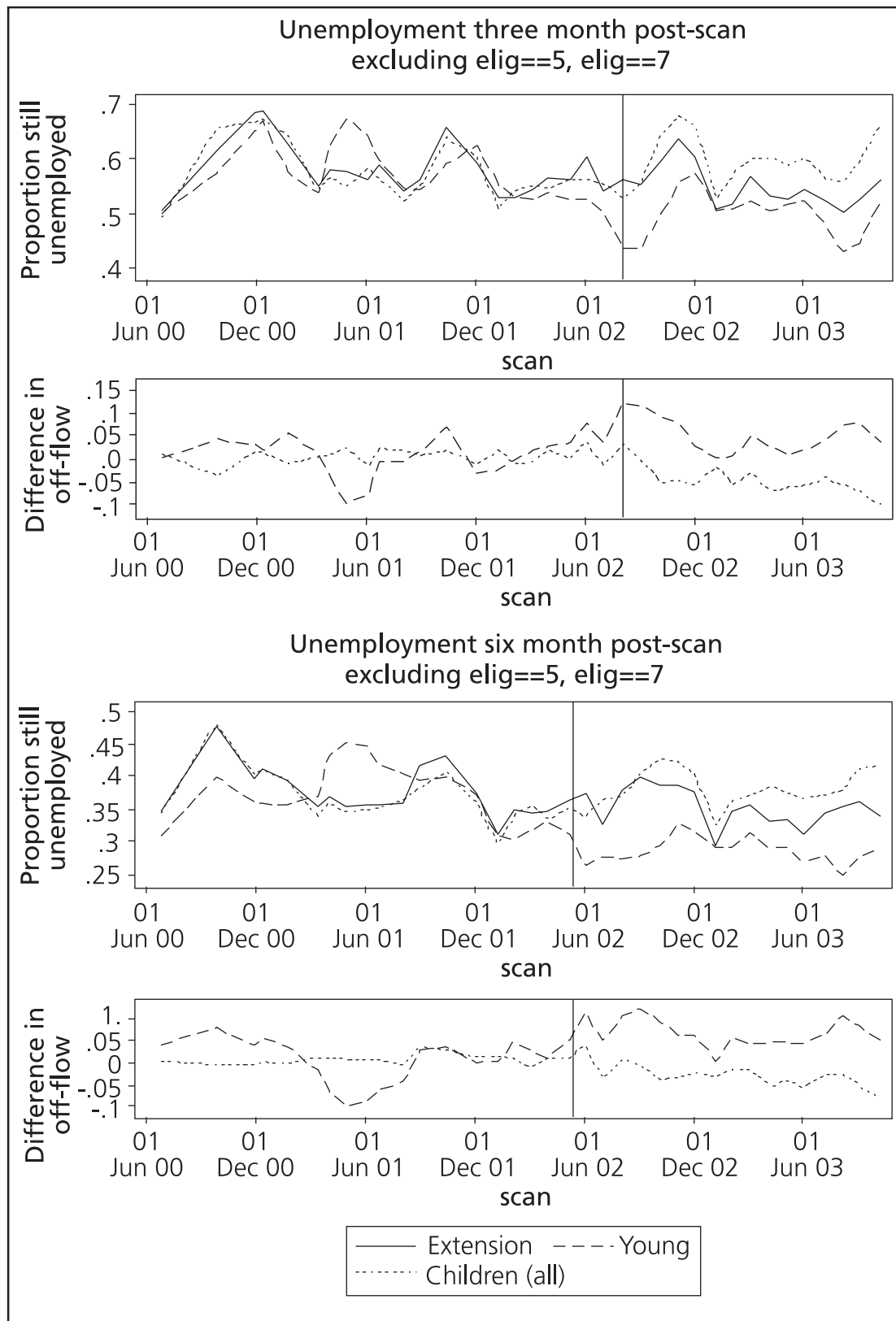
Table 5.2   Comparison group

Months post-scan	18-24 no children		27-45 children	
	mean	s.d.	mean	s.d.
1	0.02	0.03	0.01	0.02
2	0.01	0.04	0.01	0.02
3	0.01	0.04	0.01	0.02
6	0.00	0.05	0.01	0.01

5.2   Pre-programme tests of the comparison groups

The results presented above are suggestive of a preference for the children comparison group over the young comparison group. This can be investigated more formally through the use of pre-programme tests. Such tests are described in detail in Heckman and Hotz (1987) but the basic intuition is as follows. A suitable comparison group should track the outcomes of the treatment group (in this case, Extension couples). If it does, changes in outcomes for the comparison group should be similar to those of the Extension group. This can be assessed in the period prior to the Extension by considering two points in time and examining how changes among Extension couples differ from those among comparison group couples. Significant differences would indicate a poor performance of the comparison group while insignificant differences would indicate a better performance of the comparison group.

Put another way, the pre-programme tests amount to DiD estimates based on two periods that pre-date the treatment. If the tests show a significant impact when we know there has been no treatment, then it is a clear signal that no reliance can be placed on finding a significant effect when there is a treatment. However, if the pre-programme tests find no significant effects, this suggests that change among the comparison group can be viewed as a counterfactual for change among the treatment group. Plainly, the comparison group change can capture what change the treatment group would have experienced in the absence of the treatment. In this application, this is how Extension couples would have fared had there been no Extension.

The results of carrying out these tests are provided in Tables 5.3 to 5.6. Within each table each cell is a DiD estimate where the combination of the scan date listed down the left hand side of the table and that listed across the top of the table indicates the before and after periods. Unemployment statuses one to six months after the scan are considered as the outcomes. To aid interpretation, estimates that are significant (at the five per cent level) are shaded: blue where positive, green where negative.

Examining the results leaves a number of overall impressions. First, there are more significant effects with the young comparison group than with the children comparison group. Second, the estimated effects for the young comparison group tend to be large relative to those of the children comparison group. These two

results provide further evidence that couples with children provide the most suitable comparison group.[11] Accordingly, the main analysis proceeds on this basis.

A final point to note is that the combination of a 'before' scan with an 'after' scan about one year later provides estimates that are mostly insignificant (for the children comparison group). Such combinations have been highlighted by placing a box around the respective cells in Tables 5.3 to 5.6. It is intuitively appealing to consider periods that relate to similar months in the calendar year. It may be, for example, that there are different seasonal patterns for those in the treatment and comparison groups. Should this be the case, choosing similar periods of time over which to estimate effects can control for this. The most obvious reason for differences in seasonality is that outcomes are influenced by the school year for couples with children but not for those without. In any event, the message from these results appears to be that choosing 'before' periods to relate to the same month as 'after' periods is a sensible approach. Accordingly, such a strategy is followed in this evaluation.

Table 5.3   Pre-programme tests of the children comparison group

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep250 base													
One month post-scan	4	4	3	4	5	4	4	-1	3	2	1	2	2
Two months post-scan	2	2	1	3	4	3	2	0	2	2	-1	1	1
Three months post-scan	2	4	3	5	4	4	3	1	4	2	1	3	3
Four months post-scan	1	1	3	4	3	3	2	2	3	1	1	4	2
Five months post-scan	0	1	3	4	2	3	3	1	1	3	1	2	2
Six months post-scan	-1	-2	2	0	0	3	1	0	-1	0	-2	0	1

Continued

[11] The possibility that the age of children in the household may affect the suitability of the comparison group was investigated by repeating the analysis presented here but changing the comparison group to, first, include only those couples with dependent children less than 5 years of age and, second, only those couples where the youngest child was aged between 5 and 16 years. The results did not differ substantially from those obtained for dependent children of any age and are not presented.

Table 5.3   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Nov270 base													
One month post-scan			1	2	3	2	2	-3	1	0	-1	0	-1
Two months post-scan	-1		1	2	1	1	-1	1	0	-2	-1	-1	
Three months post-scan			1	3	3	3	2	0	2	1	0	2	1
Four months post-scan			1	3	2	2	1	0	2	0	0	2	0
Five months post-scan			4	4	3	4	4	2	2	4	2	3	2
Six months post-scan			4	2	2	5	3	2	1	2	0	2	3
Dec110 base													
One month post-scan			0	1	2	1	1	-4	0	-1	-2	-1	-2
Two months post-scan			-2	0	2	1	0	-2	0	-1	-3	-2	-1
Three months post-scan			0	2	2	1	1	-1	1	0	-2	0	0
Four months post-scan			2	3	2	3	2	1	2	0	0	3	0
Five months post-scan			2	3	1	2	2	0	0	2	0	0	0
Six months post-scan			3	1	1	4	2	1	0	1	-1	1	2
Jan221 base													
One month post-scan				5	3	4	-1	3	2	1	2	1	
Two months post-scan				5	3	4	1	3	2	0	2	1	
Three months post-scan				3	3	3	0	3	1	0	2	1	
Four months post-scan				3	3	2	1	3	1	1	3	1	
Five months post-scan				1	3	3	1	1	2	0	1	1	
Six months post-scan				1	4	3	1	0	1	-1	1	2	

Continued

Table 5.3   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Mar131 base													
One month post-scan						0	1	-5	-1	-2	-3	-1	-2
Two months post-scan						4	4	2	3	3	1	2	2
Three months post-scan						3	2	0	2	1	0	2	1
Four months post-scan						3	2	1	3	1	1	3	1
Five months post-scan						2	3	0	0	2	0	1	0
Six months post-scan						4	2	0	0	1	-1	0	
Apr011 base													
One month post-scan							0	-5	-1	-2	-3	-2	-2
Two months post-scan							0	-2	0	-1	-3	-2	-1
Three months post-scan							1	-1	1	0	-1	1	0
Four months post-scan							2	1	3	1	0	3	1
Five months post-scan							4	1	1	3	1	2	2
Six months post-scan							3	1	0	2	-1	1	2
Apr301 base													
One month post-scan								-5	-1	-2	-3	-1	-2
Two months post-scan								-1	0	0	-2	-1	-1
Three months post-scan								-2	0	-1	-3	0	-1
Four months post-scan								1	2	0	0	3	0
Five months post-scan								0	0	2	0	1	0
Six months post-scan								2	1	2	0	2	3

Continued

Table 5.3 Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Jun041 base													
One month post-scan								-4	0	-1	-2	-1	-2
Two months post-scan								0	1	1	-1	0	0
Three months post-scan								-1	1	0	-1	1	0
Four months post-scan								-1	1	-1	-2	1	-1
Five months post-scan								-2	-2	0	-2	-1	-1
Six months post-scan								-2	-2	-1	-4	-2	-1
Jun241 base													
One month post-scan									-1	-2	-3	-1	-2
Two months post-scan									0	-1	-3	-1	-2
Three months post-scan									-1	-2	-4	-1	-2
Four months post-scan									-1	-2	-3	0	-3
Five months post-scan									-3	-1	-3	-2	-2
Six months post-scan									-1	0	-2	0	1
Aug041 base													
One month post-scan										-3	-4	-3	-4
Two months post-scan										-2	-4	-3	-3
Three months post-scan										-1	-3	0	-1
Four months post-scan										-1	-2	1	-2
Five months post-scan										1	-1	0	0
Six months post-scan										1	-1	1	2

Continued

Table 5.3   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep011 base													
One month post-scan											-2	-1	-2
Two months post-scan											-3	-2	-2
Three months post-scan											-2	0	-1
Four months post-scan											-2	1	-2
Five months post-scan											-2	-1	-1
Six months post-scan											-5	-3	-2

Table 5.4   Pre-programme tests of the young comparison group

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep250 base													
One month post-scan	-1	-3	-3	0	5	1	2	-1	-3	-1	-1	-2	1
Two months post-scan	-4	-9	-7	-3	0	0	-1	-3	-6	-3	-3	-4	-1
Three months post-scan	-8	-12	-8	-3	-4	0	1	-7	-7	-4	-3	-3	-2
Four months post-scan	-12	-15	-10	-4	-6	-1	-2	-6	-6	-4	-2	-3	-3
Five months post-scan	-15	-17	-11	-6	-8	-2	-3	-7	-7	-2	-3	-5	-4
Six months post-scan	-13	-17	-13	-11	-11	-3	-6	-7	-8	-5	-6	-7	-3

Table 5.4　Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Nov270 base													
One month post-scan	-4	-1	4	0	1	-2	-4	-2	-3	-3	0		
Two months post-scan	-5	-1	3	2	2	-1	-3	0	-1	-1	1		
Three months post-scan	-6	-1	-2	2	3	-5	-5	-2	-1	-1	0		
Four months post-scan	-8	-3	-5	1	0	-5	-4	-2	-2	-1	-2		
Five months post-scan	-9	-5	-6	0	-1	-5	-5	0	-1	-3	-3		
Six months post-scan	-10	-7	-7	0	-2	-3	-4	-1	-3	-3	0		
Dec110 base													
One month post-scan	-2	1	6	2	3	-1	-3	0	-2	-1	2		
Two months post-scan	-6	-2	2	1	1	-2	-4	-1	-2	-2	1		
Three months post-scan	-6	-1	-2	1	3	-5	-4	-2	-1	-1	0		
Four months post-scan	-10	-4	-5	0	-1	-6	-5	-4	-3	-3	-3		
Five months post-scan	-11	-5	-7	-1	-2	-6	-5	-1	-2	-4	-4		
Six months post-scan	-11	-8	-8	-1	-3	-4	-5	-2	-4	-5	-1		
Jan221 base													
One month post-scan					5	1	2	-1	-3	-1	-2	-1	2
Two months post-scan					1	1	0	-3	-5	-2	-2	-3	0
Three months post-scan					-5	-1	0	-7	-7	-4	-4	-3	-2
Four months post-scan					-5	0	-1	-5	-4	-3	-2	-2	-2
Five months post-scan					-5	1	0	-3	-3	1	0	-2	-1
Six months post-scan					-6	2	-1	-2	-2	1	-2	-2	2

Continued

Table 5.4   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Mar131 base													
One month post-scan						0	1	-3	-4	-2	-3	-3	0
Two months post-scan						3	2	-1	-3	0	-1	-1	1
Three months post-scan						2	3	-5	-4	-2	-1	-1	0
Four months post-scan						3	2	-3	-2	-1	0	0	1
Five months post-scan						2	1	-3	-2	2	1	-1	0
Six months post-scan						4	2	1	0	3	1	0	4
Apr011 base													
One month post-scan							3	-1	-3	0	-1	-1	2
Two months post-scan							3	0	-2	1	0	0	3
Three months post-scan							9	2	2	5	5	6	7
Four months post-scan							11	6	6	8	9	9	10
Five months post-scan							12	8	8	13	11	10	11
Six months post-scan							8	7	6	9	7	7	11
Apr301 base													
One month post-scan								2	0	2	1	2	5
Two months post-scan								6	3	6	6	6	8
Three months post-scan								5	5	8	8	10	11
Four months post-scan								10	10	12	12	13	13
Five months post-scan								10	10	14	13	12	13
Six months post-scan								10	9	12	9	10	14

Table 5.4   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Jun041 base													
One month post-scan								1	-1	2	1	1	4
Two months post-scan								4	1	4	4	4	6
Three months post-scan								1	1	4	4	5	6
Four months post-scan								3	4	5	6	6	6
Five months post-scan								4	4	9	8	6	7
Six months post-scan								6	5	8	6	6	10
Jun241 base													
One month post-scan								-3	-1	-2	-2	1	
Two months post-scan								-3	0	0	-1	2	
Three months post-scan								-4	-1	-1	0	1	
Four months post-scan								-1	0	1	1	1	
Five months post-scan								0	4	3	1	2	
Six months post-scan								3	6	3	3	8	
Aug041 base													
One month post-scan										-6	-7	-7	-4
Two months post-scan										-3	-4	-4	-2
Three months post-scan										1	0	1	2
Four months post-scan										2	2	3	3
Five months post-scan										6	4	3	3
Six months post-scan										6	3	4	7

Continued

Table 5.4   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep011 base													
One month post-scan											-3	-3	0
Two months post-scan											-4	-3	-1
Three months post-scan											-3	-2	-2
Four months post-scan											-3	-3	-3
Five months post-scan											-1	-3	-3
Six months post-scan											-4	-4	0

Table 5.5   Pre-programme tests of the children comparison group, excluding those claiming IB

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep250 base													
One month post-scan	5	5	3	4	7	3	5	0	4	2	2	3	2
Two months post-scan	3	3	0	4	4	2	3	1	2	1	0	2	1
Three months post-scan	3	5	2	6	4	4	5	1	4	2	2	5	3
Four months post-scan	2	2	2	6	3	3	3	1	4	1	1	5	2
Five months post-scan	1	2	2	5	1	3	4	2	2	2	1	3	1
Six months post-scan	0	0	1	2	-2	3	2	1	0	1	-2	1	1

Continued

Table 5.5   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Nov270 base													
One month post-scan			0	2	4	1	2	-3	1	-1	-1	0	-1
Two months post-scan			-2	2	2	0	1	-1	0	-1	-3	0	-1
Three months post-scan			-1	3	1	1	2	-2	1	-1	-1	2	0
Four months post-scan			0	4	1	2	1	-1	2	-1	-1	3	0
Five months post-scan			3	6	2	4	5	3	3	4	2	4	2
Six months post-scan			4	4	1	6	5	3	3	3	1	4	4
Dec110 base													
One month post-scan			-1	0	3	0	1	-5	0	-2	-2	-1	-2
Two months post-scan			-3	0	1	-1	0	-3	-1	-2	-4	-2	-2
Three months post-scan			-2	2	0	0	0	-4	0	-2	-3	1	-1
Four months post-scan			1	4	1	2	1	-1	2	-1	0	3	0
Five months post-scan			1	4	0	2	3	0	1	1	-1	2	0
Six months post-scan			2	2	-1	3	2	0	0	1	-2	1	1
Jan221 base													
One month post-scan					5	2	4	-2	3	1	0	1	0
Two months post-scan					5	3	4	2	3	2	1	3	2
Three months post-scan					3	3	3	0	3	1	1	4	2
Four months post-scan					2	3	3	1	3	0	1	4	1
Five months post-scan					0	2	3	1	1	2	0	2	1
Six months post-scan					0	4	3	1	1	2	-1	2	2

Continued

Table 5.5   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Mar131 base													
One month post-scan						-1	1	-5	0	-2	-2	-1	-2
Two months post-scan						4	5	2	4	3	1	3	3
Three months post-scan						2	3	-1	2	0	0	3	1
Four months post-scan						3	3	0	3	0	1	4	1
Five months post-scan						1	2	0	0	1	-1	1	0
Six months post-scan						3	3	1	0	1	-1	1	2
Apr011 base													
One month post-scan							1	-5	-1	-2	-3	-2	-3
Two months post-scan							0	-2	-1	-2	-4	-1	-2
Three months post-scan							1	-2	1	-1	-2	2	0
Four months post-scan							1	-1	2	-1	-1	3	0
Five months post-scan							3	1	1	2	0	2	1
Six months post-scan							3	0	0	1	-2	1	2
Apr301 base													
One month post-scan								-6	-1	-3	-3	-2	-3
Two months post-scan								-2	0	-1	-3	-1	-1
Three months post-scan								-4	-1	-3	-3	1	-2
Four months post-scan								-1	1	-2	-1	3	-1
Five months post-scan								0	0	0	-1	1	-1
Six months post-scan								1	0	1	-1	2	2

Continued

Table 5.5   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Jun041 base													
One month post-scan								-3	1	0	-1	0	-1
Two months post-scan								0	2	1	-1	1	1
Three months post-scan								-1	2	0	0	3	1
Four months post-scan								-1	2	-1	-1	3	0
Five months post-scan								-1	0	0	-2	0	-1
Six months post-scan								-1	-1	-1	-3	0	0
Jun241 base													
One month post-scan									-1	-2	-3	-1	-2
Two months post-scan									-2	-3	-4	-2	-3
Three months post-scan									-2	-4	-5	-1	-3
Four months post-scan									-2	-5	-4	0	-4
Five months post-scan									-3	-2	-4	-2	-3
Six months post-scan									-1	-1	-3	0	0
Aug041 base													
One month post-scan										-4	-5	-4	-5
Two months post-scan										-3	-5	-2	-3
Three months post-scan										-2	-2	1	-1
Four months post-scan										-2	-2	2	-1
Five months post-scan										2	0	2	0
Six months post-scan										2	0	2	3

Continued

Table 5.5   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep011 base													
One month post-scan											-1	0	-1
Two months post-scan											-3	0	-1
Three months post-scan											-2	1	-1
Four months post-scan											-2	2	-1
Five months post-scan											-2	0	-1
Six months post-scan											-4	-2	-1

Table 5.6   Pre-programme tests of the young comparison group, excluding those claiming IB

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep250 base													
One month post-scan	0	-2	-4	0	6	1	3	-2	-3	-1	0	-2	2
Two months post-scan	-4	-11	-10	-4	-2	-3	-2	-5	-8	-5	-3	-5	-1
Three months post-scan	-10	-15	-12	-5	-7	-3	0	-10	-9	-7	-4	-4	-2
Four months post-scan	-13	-18	-14	-6	-9	-4	-4	-9	-8	-6	-3	-4	-3
Five months post-scan	-17	-19	-15	-9	-12	-5	-5	-8	-8	-4	-3	-6	-5
Six months post-scan	-14	-19	-16	-13	-14	-6	-8	-8	-9	-5	-6	-7	-3

Continued

Table 5.6   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Nov270 base													
One month post-scan			-5	-1	5	0	2	-3	-4	-2	-2	-3	1
Two months post-scan			-7	-1	3	1	2	-2	-4	-1	0	0	2
Three months post-scan			-9	-2	-4	0	3	-6	-6	-3	-1	0	1
Four months post-scan			-11	-3	-6	-1	0	-6	-5	-3	-1	0	-1
Five months post-scan			-12	-6	-9	-3	-2	-6	-5	-1	-1	-2	-3
Six months post-scan			-11	-8	-9	-1	-2	-4	-4	0	-2	-2	1
Dec110 base													
One month post-scan			-4	0	7	1	4	-2	-3	-1	-1	-1	2
Two months post-scan			-8	-2	1	-1	1	-4	-5	-3	-1	-2	1
Three months post-scan			-10	-3	-4	-1	3	-7	-6	-4	-1	0	1
Four months post-scan			-13	-6	-8	-3	-2	-9	-7	-6	-3	-3	-3
Five months post-scan			-13	-7	-10	-4	-3	-7	-6	-2	-2	-3	-4
Six months post-scan			-15	-11	-12	-4	-5	-6	-6	-3	-5	-5	-2
Jan221 base													
One month post-scan					7	1	4	-1	-2	0	0	-1	3
Two months post-scan					1	-1	0	-3	-5	-3	-1	-2	1
Three months post-scan					-7	-4	-1	-10	-9	-7	-4	-4	-2
Four months post-scan					-9	-4	-3	-9	-7	-6	-4	-4	-3
Five months post-scan					-9	-2	-2	-5	-4	-1	0	-2	-2
Six months post-scan					-10	-1	-3	-4	-4	0	-2	-3	1

Table 5.6   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Mar131 base													
One month post-scan						-1	2	-4	-5	-3	-3	-3	0
Two months post-scan						1	2	-2	-4	-1	0	-1	2
Three months post-scan						0	3	-6	-6	-3	-1	0	2
Four months post-scan						2	2	-4	-2	-1	2	2	2
Five months post-scan						0	1	-3	-2	2	2	0	0
Six months post-scan						3	2	1	1	4	3	2	6
Apr011 base													
One month post-scan							3	-2	-3	-1	-1	-1	2
Two months post-scan							3	-1	-3	-1	1	0	3
Three months post-scan							9	0	0	3	5	6	8
Four months post-scan							10	4	5	7	9	10	10
Five months post-scan							12	8	9	13	13	12	12
Six months post-scan							7	5	6	10	8	7	11
Apr301 base													
One month post-scan								1	0	2	2	1	5
Two months post-scan								5	4	6	8	7	10
Three months post-scan								5	6	9	11	12	13
Four months post-scan								9	11	12	15	15	15
Five months post-scan								11	11	15	15	14	14
Six months post-scan								10	10	13	12	12	15

Continued

Table 5.6   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Jun041 base													
One month post-scan								2	1	3	3	3	6
Two months post-scan								4	2	5	6	5	8
Three months post-scan								2	2	5	8	8	9
Four months post-scan								4	5	7	9	9	9
Five months post-scan								6	6	10	11	9	9
Six months post-scan								7	7	11	9	8	12
Jun241 base													
One month post-scan									-3	-1	-1	-1	2
Two months post-scan									-3	-1	0	-1	3
Three months post-scan									-4	-2	1	1	3
Four months post-scan									-2	0	2	2	2
Five months post-scan									1	5	5	3	3
Six months post-scan									4	8	6	5	9
Aug041 base													
One month post-scan										-7	-7	-8	-5
Two months post-scan										-4	-3	-3	-1
Three months post-scan										1	2	4	4
Four months post-scan										3	5	5	5
Five months post-scan										8	8	6	5
Six months post-scan										9	7	7	9

Continued

Table 5.6   Continued

	01 Apr 2001	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002
Sep011 base													
One month post-scan											-2	-2	1
Two months post-scan											-1	-2	1
Three months post-scan											-1	0	1
Four months post-scan											0	0	0
Five months post-scan											2	0	-1
Six months post-scan											-1	-2	2

5.3   Testing the model specification

As noted in Chapter 4, it is possible to use the results of estimating equation (3) to test the restrictions implied by the DiD and random growth specifications. A significant value of β_t amounts to a rejection of the hypothesis of the differential trends between the treatment and control groups and, therefore, implies that the straightforward DiD model is inappropriate. Significantly different values of $\beta_{t'}$ and β_t imply that the differential time trends between the treatment and comparison periods are not constant over time and, therefore, the standard random growth model that imposes this restriction is not appropriate.

The results of testing the DiD restriction are given in Table 5.7. These results are based on estimates of the random growth model specified as in equation (3) using data for the period shown in the column heading, and periods roughly one and two years prior to this. Entries in grey indicate cases where the restriction is not rejected in the data. The first panel in the table considers exits from JSA where all transfers to clerical claims are considered an exit. As such, these results correspond closely to the pre-programme tests reported above. Consequently, it is unsurprising to find few cases where the DiD restriction is rejected (i.e. where the pre-programme test fails).[12] The second panel shows analogous results under the assumption that none of the transfers exits JSA. The results are very similar and suggest that, on the whole, the DiD restriction is acceptable. In either panel, it is only among the short-term exits that the DiD restriction is rejected. The third and fourth panels consider employment outcomes. Again, results for both a 'lower bound' and an 'upper bound' are presented. This is necessitated by the fact that, for many of those leaving JSA, their

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[12] That is, there are few cases of P-values less than 0.05.

destination is unknown. The lower bound employment outcome assumes that none of those leaving for unknown destinations (including transfers to clerical claims) moves into employment, while the upper bound outcome assumes that all of those leaving for unknown destinations enter work. The results in both cases are similar and mostly resemble the results described above for the JSA outcome. On the whole, the DiD model appears appropriate. However, in these cases where the DiD restriction is rejected, it is worth considering adjusting for the pre-programme difference using the random growth model.

Table 5.7   P-values for the test of the DiD restriction

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Unemployment											
Assuming all transfers to clerical claims exit JSA:											
One month post-scan	0.00	0.01	0.05	0.18	0.10	0.34	0.18	0.14	0.05	0.89	0.69
Two months post-scan	0.17	0.55	0.11	0.14	0.77	0.43	0.68	0.03	0.12	0.59	.
Three months post-scan	0.07	0.98	0.19	0.46	0.94	0.59	0.55	0.11	0.18	.	.
Four months post-scan	0.20	0.77	0.14	0.57	0.74	0.16	0.94	0.19	0.18	.	.
Five months post-scan	0.10	0.25	0.81	0.21	0.89	0.39	0.87	0.39	.	.	.
Six months post-scan	0.48	0.25	0.94	0.50	0.46	0.58	0.18	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	0.00	0.01	0.04	0.15	0.10	0.37	0.16	0.15	0.04	0.92	0.71
Two months post-scan	0.15	0.56	0.11	0.13	0.75	0.43	0.71	0.02	0.09	0.65	.
Three months post-scan	0.06	0.95	0.18	0.47	0.95	0.63	0.56	0.09	0.13	.	.
Four months post-scan	0.17	0.74	0.13	0.57	0.72	0.19	0.94	0.16	0.15	.	.
Five months post-scan	0.07	0.25	0.76	0.20	0.95	0.47	0.89	0.36	.	.	.
Six months post-scan	0.39	0.26	0.88	0.51	0.52	0.66	0.20	.	.	.	.

Continued

Table 5.7   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Employment											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	0.00	0.01	0.02	0.02	0.59	0.15	0.50	0.97	0.01	0.54	0.39
Two months post-scan	0.21	0.59	0.08	0.07	0.19	0.22	0.94	0.25	0.02	0.27	.
Three months post-scan	0.35	0.92	0.35	0.31	0.47	0.88	0.58	0.29	0.03	.	.
Four months post-scan	0.45	0.97	0.19	0.12	0.28	0.61	0.70	0.60	0.19	.	.
Five months post-scan	0.80	0.73	0.75	0.08	0.39	0.88	0.92	0.82	.	.	.
Six months post-scan	0.40	0.82	0.95	0.13	0.62	0.66	0.56	.	.	.	.
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	0.02	0.00	0.05	0.15	0.22	0.33	0.21	0.20	0.01	0.87	0.68
Two months post-scan	0.65	0.18	0.03	0.10	0.44	0.52	0.68	0.03	0.03	0.91	.
Three months post-scan	0.52	0.41	0.14	0.42	0.72	0.38	0.48	0.05	0.05	.	.
Four months post-scan	0.95	0.49	0.06	0.29	0.37	0.15	0.74	0.06	0.09	.	.
Five months post-scan	0.60	0.86	0.30	0.10	0.63	0.43	0.65	0.08	.	.	.
Six months post-scan	0.12	0.95	0.63	0.25	1.00	0.84	0.21	.	.	.	.

Table 5.6 presents the results of testing the constant trend restriction in the random growth model. Again, entries in grey indicate that the restriction is not rejected by the data. The overwhelming message is that, regardless of which outcome is considered, the constant trend restriction is not supported by the data. Consequently, this specification of the model is not pursued further in this evaluation.

Table 5.8   **P-values for the test of the constant trend restriction in the random growth model**

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Unemployment											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	0.00	0.01	0.00	0.00	0.03	0.00	0.43	0.00	0.54	0.01	0.02
Two months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.00	0.85	0.06	.
Three months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.00	0.02	.	.
Four months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00	.	.
Five months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	.	.	.
Six months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.00	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	0.00	0.00	0.00	0.00	0.03	0.00	0.46	0.00	0.52	0.01	0.01
Two months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.05	0.00	0.87	0.05	.
Three months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.05	0.00	0.02	.	.
Four months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00	.	.
Five months post-scan	0.00	0.00	0.00	0.00	0.01	0.00	0.01	0.00	.	.	.
Six months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.00	.	.	.	.
Employment											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.48	0.00	0.65	0.00	0.16
Two months post-scan	0.00	0.01	0.00	0.00	0.01	0.00	0.13	0.00	0.47	0.05	.
Three months post-scan	0.00	0.00	0.00	0.00	0.01	0.00	0.20	0.00	0.12	.	.
Four months post-scan	0.00	0.00	0.00	0.00	0.18	0.00	0.07	0.00	0.03	.	.
Five months post-scan	0.00	0.00	0.00	0.00	0.23	0.01	0.02	0.00	.	.	.
Six months post-scan	0.00	0.00	0.00	0.00	0.23	0.04	0.03	.	.	.	.

Table 5.8   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	0.00	0.00	0.00	0.00	0.01	0.00	0.53	0.00	0.26	0.00	0.01
Two months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.09	0.00	0.33	0.00	.
Three months post-scan	0.00	0.00	0.00	0.00	0.00	0.00	0.11	0.00	0.13	.	.
Four months post-scan	0.00	0.00	0.00	0.00	0.01	0.00	0.06	0.00	0.04	.	.
Five months post-scan	0.00	0.00	0.00	0.00	0.06	0.00	0.02	0.00	.	.	.
Six months post-scan	0.00	0.00	0.00	0.00	0.09	0.00	0.02	.	.	.	.

As a final comment on specification testing, it should be noted that identical tests based on that subset of the data that excludes those claiming IB yielded essentially similar results.

6   The effects of the Extension

This chapter presents the estimates of the effect of the Extension. This is done separately for the stock and the flow. The outcomes considered are movements off Jobseeker's Allowance (JSA) and movements into work.

6.1   The effects of the Extension on the flow

Table 6.1 presents estimates of the effect of the Extension on couples' movements off JSA. The table has a format that is shared by many of the results in this section so it is helpful to describe this briefly. The table is split into two panels; one labelled 'Lower bound' the other labelled 'Upper bound'. This reflects the discussion in Chapter 4 of the uncertainty in the treatment status of those who appear to be eligible for Joint Claims. The lower bound is the estimated effect of eligibility. This is the effect of treatment if **all** of those who appear eligible but are not recorded as receiving the treatment do in fact receive it. The upper bound estimates (in the bottom panel) are achieved by dividing the estimates of the eligibility effect by the proportion of eligible couples whose claim was treated as a Joint Claim. This Bloom-type adjustment assumes that **none** of those who appear eligible but are not recorded as receiving the treatment receives it and that any Joint Claims effect will only manifest itself through those exposed to the treatment.[13]

Within each of these two panels, the results are sub-divided according to the assumption made about claims whose destination upon claim-end is a 'transfer to a clerical claim'. The first set of results within each panel are based on the assumption that **all** spells ending in a transfer constitute true exits from JSA. The second set of results within each panel are based on the assumption that **none** does and that the spell continues but is dealt with clerically.

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[13] It should be noted that the upper bound estimates will always have the same sign as the lower bound estimates. The level of significance attached to the two sets of estimates may differ slightly but are generally closely comparable

The effects in Table 6.1 are estimated using a Difference-in-differences (DiD) approach. This requires two points in time. The results in each column correspond to estimates from different periods. The post-Extension period is given in the column heading. The pre-Extension period is roughly a year before. Results presented in some of the later tables are based on the random growth model. In this case, three time periods are used and the two pre-Extension periods are respectively one and two years prior to the post-Extension period given in the column heading. For each combination of dates, the effects on outcomes one, two, three, four, five and six months after the scan are estimated. Rows are labelled in the table accordingly.

The entries in each cell correspond to a single DiD estimate. Hence, Table 6.1 summarises the results of 216 separate regressions. The results control for the characteristics of the couple using information available in the administrative data. Specifically, the results control for age, ethnic minority status, preferred occupation, disability, previous JSA history, rurality of residence, region and local unemployment at the time of the scan.

Table 6.1 DiD estimates of the effect of the Extension on unemployment

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Unemployment											
Lower bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-5*	-5**	-3	-4**	0	-3*	-2	-7**	0	-6**	-4
Two months post-scan	-8**	-7**	-4	-4*	-1	-5*	-5*	-8**	-1	-6**	.
Three months post-scan	-11**	-5*	-4	-5*	-3	-7**	-5*	-7**	-1	.	.
Four months post-scan	-10**	-4	-6**	-5**	-5*	-9**	-4	-7**	-2	.	.
Five month post-scan	-10**	-3	-4*	-5**	-3	-6**	-4	-6**	.	.	.
Six months post-scan	-9**	-4*	-4	-3	-2	-5*	-7**	.	.	.	.

Continued

Table 6.1   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-2	-1	-1	-2	2	-1	1	-4*	1	-4*	-2
Two months post-scan	-3	-1	0	0	2	-1	0	-3	2	-3	.
Three months post-scan	-4	3	0	0	1	-2	1	-1	3	.	.
Four months post-scan	-2	5**	-1	0	0	-3	2	0	3	.	.
Five months post-scan	-1	7**	2	1	2	1	3	1	.	.	.
Six months post-scan	1	7**	3	4	4	2	1	.	.	.	.
Upper bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-10*	-10**	-7	-10**	-1	-8*	-6	-17**	-1	-15**	-9
Two months post-scan	-17**	-15**	-8	-10*	-3	-11*	-12*	-19**	-3	-15**	.
Three months post-scan	-23**	-11*	-9	-11*	-6	-18**	-12*	-17**	-2	.	.
Four months post-scan	-20**	-8	-12**	-12**	-10*	-21**	-11	-18**	-4	.	.
Five months post-scan	-20**	-6	-9*	-11**	-8	-14**	-9	-16**	.	.	.
Six months post-scan	-19**	-9*	-8	-6	-4	-13*	-16**	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-4	-2	-2	-5	4	-3	2	-9*	3	-9*	-4
Two months post-scan	-7	-1	0	-1	4	-2	0	-7	4	-7	.
Three months post-scan	-8	6	1	-1	3	-6	2	-2	7	.	.
Four months post-scan	-4	11**	-1	1	1	-7	5	0	8	.	.
Five months post-scan	-2	16**	4	2	5	2	7	3	.	.	.
Six months post-scan	1	15**	6	8	9	5	2	.	.	.	.

** - significant at 1%; * - significant at 5%. Estimates not significant at the 10% level are shown in grey.

The first set of results in Table 6.1 show a mostly negative effect of the Extension on unemployment. To be clear, a negative effect on unemployment is a positive impact of the Extension. To interpret the estimates, consider the results for 4 November 2002 (first column of results). The entry of –5 for the outcome labelled 'One month post-scan' suggests that the Extension has had the effect of reducing, by five percentage points, the probability that eligible couples beginning a JSA claim in the month leading up to November 2002 will still be claiming JSA one month later. Considering the probability of still claiming in later months, the effect was higher, peaking at 11 percentage points for three month outcomes

It is also clear that there was variation over time. There are three reasons why this might be the case: First, it may be due to the variance that all estimates have to some degree. Second, it may reflect seasonality in the effect of the Extension or macroeconomic changes. For example, the ease with which it may be possible to encourage people to find work will vary according to the wider availability of jobs and the time of year. Third, it may be that the effect of the Extension is in fact evolving over time. This could arise if, for example, those responsible for administering the treatment become more confident over time in dealing with Joint Claims. Given the apparent complexity of Jobseeker's Allowance Payment System (JSAPS), this may be an important factor. Tapp and Thomas (2004) found some evidence to this effect.

These results assume that all couples whose JSA claim appears to end with a transfer to a clerical claim actually leave unemployment. The extent to which this is true is debatable, as discussed earlier. The second set of results is based on the alternative assumption that those who transfer are still claiming JSA. Unsurprisingly, this greatly reduces the size of the estimated effects and, in some cases, a significant positive effect is found (see column for December 2002). The overall impression from these results is that the Extension has had no discernible effect. The results for December 2002 appear something of an anomaly and are difficult to explain. The fact that the three, four and five month results for January 2003 are very different from the four, five and six month results for December 2002, suggests that the results are explained more by some characteristic of those claims in December 2002 than any feature of the months in which the outcomes are realised (April, May and June, 2003).

The results discussed so far are the lower bound estimates. If some of those who appear eligible are not exposed to the treatment, these estimates may be biased towards zero. The upper bound estimates are given in the bottom panel of Table 6.1. These results display a similar pattern to the lower bound estimates with regard to which effects are significant. However, the estimates are a little over twice as large (in absolute terms), reflecting the fact that fewer than half of all eligible couples in the flow are recorded as making a joint claim.

Table 6.2 presents the estimated effects of the Extension on job entry. These estimates make use of the information on destination on JSA exit recorded in the data. This destination information is not well-recorded. In addition to the problem discussed already of the 'transfer to a clerical claim' destination, there are other exits for which the destination is simply not known. The first results in the top panel are

based on the assumption that only those who are explicitly recorded as leaving JSA to enter work have done so; all claims ending for other reasons (or for which no reason is recorded) are presumed to have not entered work. Under this scenario, the results are mostly insignificant, although for some months, significant negative effects suggest that the Extension served to reduce the chances of entering work. Under the alternative assumption that all of those leaving JSA to unknown destinations enter work, the results are much more positive, suggesting the Extension mostly operated to significantly increase the probability of finding work. As with the estimates for the effects on unemployment, the upper bound results show a similar pattern of statistical significance, but the size of the effects are found to be roughly twice as large.

Table 6.2   DiD estimates of the effect of the Extension on employment

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Employment											
Lower bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	2	1	0	1	1	0	0	1	-2	3*	1
Two months post-scan	3	1	-2	-1	2	-1	1	1	-3	2	.
Three months post-scan	2	-1	-3	-1	0	-1	1	1	-5**	.	.
Four months post-scan	0	-4*	-2	0	1	0	1	0	-4*	.	.
Five months post-scan	-2	-5**	-3	-1	-1	-2	-1	-1	.	.	.
Six months post-scan	-4	-5**	-4*	-2	-2	-4	0	.	.	.	.
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	4*	4**	2	4*	0	3	2	7**	1	6**	4*
Two months post-scan	7**	7**	2	3	2	4*	5**	8**	2	6**	.
Three months post-scan	8**	6**	3	4*	3	6**	6**	8**	2	.	.
Four months post-scan	7**	4	5*	5**	5*	8**	6**	9**	2	.	.
Five months post-scan	7**	3	4	5*	4	5*	5*	9**	.	.	.
Six months post-scan	6*	5*	3	4*	2	4*	7**	.	.	.	.

Continued

Table 6.2 Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Upper bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One mouth post-scan	4	2	0	3	2	1	1	2	-3	6*	3
Two mouths post-scan	7	3	-4	-3	4	-3	2	2	-7	6	.
Three months post-scan	4	-2	-6	-3	0	-2	1	3	-11**	.	.
Four months post-scan	1	-9*	-5	-1	2	-1	2	0	-9*	.	.
Five months post-scan	-4	-11**	-7	-3	-2	-6	-2	-2	.	.	.
Six months post-scan	-8	-12**	-9*	-5	-5	-10	0	.	.	.	.
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	8*	9**	4	9*	1	7	6	16**	1	13**	9*
Two months post-scan	14**	15**	5	7	4	10*	13**	20**	5	15**	.
Three months post-scan	17**	12**	6	10*	6	15**	14*	21**	3	.	.
Four months post-scan	15**	8	10*	12**	11*	19**	14**	22**	5	.	.
Five months post-scan	15**	8	9	11*	8	13*	12*	22**	.	.	.
Six months post-scan	12*	10*	7	9*	4	11*	17**	.	.	.	.

** - significant at 1%; * - significant at 5%. Estimates not significant at the 10% level are shown in grey.

Tables 6.3 and 6.4 provide analogous results to those discussed above, after excluding those couples who were recorded as receiving Incapacity Benefit (IB) at the time of the latest scan. The rationale behind this is that couples receiving this benefit should not be eligible for JSA and, therefore, do not belong in the eligible group. The difficulty in practice with excluding such couples is that the data only provide their IB status at the time of the most recent scan and do not capture their IB status at a time contemporaneous to the period on which the estimates are based. Hence, while the current ineligibility of such couples can be established, it is not possible to infer from this that they were ineligible in the past. In fact, excluding such couples amounts to selection on the basis of an outcome and for this reason may result in biased estimates. For this reason, the results on this sub-sample are presented as a sensitivity analysis rather than the preferred estimates.

Table 6.3   DiD estimates of the effect of the Extension on unemployment, excluding couples with an IB claim

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Unemployment											
Lower bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-5*	-5**	-4*	-5**	-1	-3	-3	-8**	-2	-7**	-5*
Two months post-scan	-7**	-7**	-3	-5*	-2	-6*	-6*	-9**	-2	-7**	.
Three months post-scan	-11**	-4	-3	-5*	-3	-9**	-6*	-8**	-1	.	.
Four months post-scan	-9**	-3	-5*	-5*	-4	-10**	-5	-9**	0	.	.
Five months post-scan	-9**	-3	-4	-4*	-3	-7**	-3	-8**	.	.	.
Six months post-scan	-9**	-3	-4	-3	-1	-6**	-6*	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-2	0	-1	-3	1	0	1	-4*	0	-4*	-2
Two months post-scan	-2	1	1	0	1	-1	0	-3	1	-3	.
Three months post-scan	-3	6**	2	1	2	-3	1	-1	4	.	.
Four months post-scan	-1	8**	1	2	1	-4	4	0	5*	.	.
Five months post-scan	1	9**	3	3	3	1	6*	1	.	.	.
Six months post-scan	2	10**	4	5*	6*	3	3	.	.	.	.
Upper bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-9*	-8**	-7*	-10**	-3	-6	-6	-18**	-3	-14**	-9*
Two months post-scan	-13**	-13**	-6	-9*	-4	-12*	-12*	-20**	-5	-14**	.
Three months post-scan	-19**	-7	-6	-10*	-5	-18**	-12*	-18**	-1	.	.
Four months post-scan	-16**	-5	-9*	-9*	-8	-22**	-9	-19**	0	.	.
Five months post-scan	-16**	-5	-8	-8*	-5	-15**	-5	-17**	.	.	.
Six months post-scan	-15**	-6	-7	-5	-2	-13**	-11*	.	.	.	.

Continued

Table 6.3   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-3	0	-2	-5	2	-1	2	-9*	1	-9*	-5
Two months post-scan	-3	2	2	0	3	-3	0	-7	2	-6	.
Three months post-scan	-5	10**	4	1	4	-6	3	-2	8	.	.
Four months post-scan	-1	15**	2	4	3	-8	7	0	11*	.	.
Five months post-scan	1	17**	5	5	6	1	11*	3	.	.	.
Six months post-scan	3	18**	7	9*	11*	5	7	.	.	.	.

** - significant at 1%; * - significant at 5%

The lower bound results in Table 6.3 compare quite closely with those in Table 6.1. The gap between the lower and upper bound results is smaller than in Table 6.1 since excluding the current IB claimants reduces the proportion of the eligible group who were not exposed to the treatment. Similarly, Table 6.4 shows that excluding IB claimants does little to alter the impression gained from Table 6.2 of the effect of the Extension on employment.

Table 6.4   DiD estimates of the effect of the Extension on employment, excluding couples with an IB claim

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Employment											
Lower bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	2	0	1	2	2	0	1	1	-1	3*	2
Two months post-scan	2	0	-2	-2	3	-1	1	0	-3	3	.
Three months post-scan	0	-3	-3	-2	1	0	0	1	-6**	.	.
Four months post-scan	-2	-6**	-2	-2	2	0	0	-1	-6**	.	.
Five months post-scan	-5	-7**	-4	-3	0	-2	-2	-1	.	.	.
Six months post-scan	-7*	-8**	-5*	-3	-1	-4	-2	.	.	.	.

Continued

Table 6.4   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	4	5**	3	4*	1	3	3	8**	1	6**	5*
Two months post-scan	5*	7**	2	3	2	5*	6*	9**	2	7**	.
Three months post-scan	7**	5*	3	4	3	7**	6*	10**	0	.	.
Four months post-scan	6*	3	5*	5*	5*	9**	6*	11**	1	.	.
Five months post-scan	6*	4	5*	4*	4	7**	4	11**	.	.	.
Six months post-scan	5	4	4	4	2	6*	5*	.	.	.	.
Upper bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	3	1	1	3	4	1	1	2	-2	7*	4
Two months post-scan	3	0	-4	-4	5	-2	3	1	-6	5	.
Three months post-scan	1	-5	-5	-4	2	0	1	2	-12**	.	.
Four months post-scan	-3	-12**	-4	-4	3	0	0	-1	-12**	.	.
Five months post-scan	-8	-13**	-7	-5	-1	-5	-4	-2	.	.	.
Six months post-scan	-12*	-14**	-9*	-6	-3	-7	-3	.	.	.	.
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	7	8**	5	8*	2	5	6	18**	3	13**	10*
Two months post-scan	9*	14**	4	6	5	10*	12*	19**	4	15**	.
Three months post-scan	13**	9*	5	8	6	15**	12*	21**	1	.	.
Four months post-scan	11*	6	9*	9*	10*	19**	12*	23**	1	.	.
Five months post-scan	11*	7	8*	8*	8	14**	8	24**	.	.	.
Six months post-scan	8	8	7	7	4	12*	11*	.	.	.	.

** - significant at 1%; * - significant at 5%

Tables 6.5 to 6.8 present analogous results based on the Random Growth (RG) model. These are included in view of the fact that in a small number of cases the restriction required for the DiD model was rejected (see Chapter 5). For these cases, the results provided by the RG model are helpful and are to be preferred to the DiD results. In most cases, however, the DiD restriction was supported by the data and so the DiD results stand. It should be noted that even where the DiD restriction is accepted, it is not surprising to find some difference from the RG results. As noted already, the estimates are subject to standard sample variation so the fact that they are estimated over different samples will introduce discrepancies. Furthermore, the RG results will adjust for estimated bias even when this is shown to be insignificant.

Table 6.5   RG estimates of the effect of the Extension on unemployment

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Unemployment											
Lower bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-9**	-1	-6*	-6*	2	-2	-1	-10**	3	-6*	-3
Two months post-scan	-11**	-6*	-6*	-7*	-2	-3	-5	-13**	1	-5	.
Three months post-scan	-14**	-5	-7	-6	-2	-9*	-4	-10**	1	.	.
Four months post-scan	-12**	-5	-9*	-6	-5	-12**	-5	-10**	1	.	.
Five months post-scan	-13**	-5	-5	-8*	-3	-8*	-5	-8*	.	.	.
Six months post-scan	-10**	-6	-4	-4	-1	-7	-10**	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-6*	2	-4	-4	4	0	3	-6*	5	-4	-1
Two months post-scan	-6	0	-3	-3	1	0	0	-7*	5	-2	.
Three months post-scan	-7*	2	-2	-2	2	-4	2	-5	6	.	.
Four months post-scan	-5	4	-4	-1	0	-6	2	-3	6	.	.
Five months post-scan	-4	5	1	-2	2	-1	2	-1	.	.	.
Six months post-scan	-1	5	2	2	5	1	-2	.	.	.	.

Continued

Table 6.5 Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Upper bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-19**	-3	-13*	-14*	5	-5	-1	-24**	6	-15*	-7
Two months post-scan	-22**	-13*	-14*	-16*	-5	-8	-11	-31**	3	-13	.
Three months post-scan	-30**	-11	-14	-14	-6	-22*	-10	-26**	3	.	.
Four months post-scan	-25**	-10	-19*	-14	-12	-30**	-12	-25**	2	.	.
Five months post-scan	-27**	-11	-10	-17*	-7	-20*	-11	-21*	.	.	.
Six months post-scan	-21**	-14	-8	-9	-2	-17	-23**	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-13*	5	-8	-10	10	0	6	-15*	10	-10	-3
Two months post-scan	-12	1	-7	-7	2	1	1	-18*	11	-6	.
Three months post-scan	-15*	5	-5	-4	4	-9	4	-12	14	.	.
Four months post-scan	-9	10	-8	-2	-1	-15	4	-8	14	.	.
Five months post-scan	-9	11	2	-4	5	-3	5	-2	.	.	.
Six months post-scan	-2	10	5	5	12	2	-5	.	.	.	.

** - significant at 1%; * - significant at 5%

Table 6.6 **RG estimates of the effect of the Extension on employment**

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Employment											
Lower bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	5*	-2	3	4	0	-1	0	1	-5*	2	0
Two months post-scan	5	0	1	1	4	-3	1	3	-7*	1	.
Three months post-scan	3	-1	-1	1	1	0	0	4	-8**	.	.
Four months post-scan	2	-4	0	2	3	1	2	1	-6	.	.
Five months post-scan	-2	-5	-3	2	1	-2	0	0	.	.	.
Six months post-scan	-5	-5	-4	1	-1	-4	2	.	.	.	.
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	7**	1	5	6*	-1	1	1	9**	-3	6*	3
Two months post-scan	7*	5	6	6*	3	3	5	13**	-2	6	.
Three months post-scan	9**	4	6	6	3	8*	5	13**	-2	.	.
Four months post-scan	7	3	8*	7*	7	11**	7	13**	-1	.	.
Five months post-scan	6	4	6	8*	5	7*	7	13**	.	.	.
Six months post-scan	3	4	4	6	2	5	10**	.	.	.	.
Upper bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	11*	-3	6	9	1	-3	-1	3	-11*	5	0
Two months post-scan	10	1	2	3	9	-7	3	8	-15*	2	.
Three months post-scan	7	-2	-2	1	2	0	0	9	-19**	.	.
Four months post-scan	3	-9	1	5	7	3	5	3	-15	.	.
Five months post-scan	-4	-10	-6	4	2	-6	0	0	.	.	.
Six months post-scan	-11	-11	-9	1	-3	-11	4	.	.	.	.

Continued

Table 6.6   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	14**	2	10	13*	-3	4	2	22**	-7	14*	8
Two months post-scan	16*	11	13	13*	7	7	12	31**	-4	15	.
Three months post-scan	20**	9	13	13	8	20*	11	31**	-5	.	.
Four months post-scan	14	6	18*	17*	15	27**	17	33**	-2	.	.
Five months post-scan	12	8	13	19*	11	18	16	32**	.	.	.
Six months post-scan	6	10	9	14	4	12	24**	.	.	.	.

** - significant at 1%; * - significant at 5%

Table 6.7   RG estimates of the effect of the Extension on unemployment, excluding couples with an IB claim

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Unemployment											
Lower bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-10**	-1	-6*	-6*	1	-1	0	-12**	1	-8*	-5
Two months post-scan	-11**	-6	-6	-7	-3	-4	-5	-16**	1	-7	.
Three months post-scan	-15**	-2	-6	-6	-3	-11**	-5	-14**	3	.	.
Four months post-scan	-12**	-2	-8*	-5	-5	-14**	-4	-14**	5	.	.
Five months post-scan	-13**	-6	-5	-6	-2	-10*	-3	-12**	.	.	.
Six months post-scan	-11*	-7	-5	-5	0	-8	-8*	.	.	.	.

Table 6.7   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-7*	3	-4	-3	3	1	4	-8*	3	-5	-3
Two months post-scan	-5	2	-2	-2	0	0	1	-9*	5	-3	.
Three months post-scan	-8	7	-1	0	2	-5	2	-6	9*	.	.
Four months post-scan	-4	9*	-2	2	0	-7	4	-5	10*	.	.
Five months post-scan	-3	7	2	1	4	-2	6	-2	.	.	.
Six months post-scan	0	6	3	3	6	1	1	.	.	.	.
Upper bound											
*Assuming **all** transfers to clerical claims exit JSA:*											
One month post-scan	-19**	-2	-12*	-11*	2	-3	-1	-26**	2	-16*	-11
Two months post-scan	-19**	-11	-12	-13	-6	-9	-10	-33**	2	-13	.
Three months post-scan	-27**	-4	-11	-11	-5	-23**	-10	-29**	7	.	.
Four months post-scan	-22**	-5	-15*	-9	-10	-29**	-9	-30**	9	.	.
Five months post-scan	-23**	-11	-10	-12	-4	-20*	-5	-25**	.	.	.
Six months post-scan	-19*	-13	-9	-9	0	-16	-16*	.	.	.	.
*Assuming **no** transfers to clerical claims exit JSA:*											
One month post-scan	-13*	6	-7	-6	7	2	7	-17*	6	-10	-6
Two months post-scan	-10	4	-4	-4	0	0	2	-20*	10	-6	.
Three months post-scan	-13	13	-1	0	4	-10	5	-14	17*	.	.
Four months post-scan	-7	16*	-4	4	1	-15	7	-11	21*	.	.
Five months post-scan	-6	12	3	1	7	-4	11	-5	.	.	.
Six months post-scan	-1	12	5	6	12	3	2	.	.	.	.

** - significant at 1%; * - significant at 5%

Table 6.8   RG estimates of the effect of the Extension on employment, excluding couples with an IB claim

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
Employment											
Lower bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	6*	-2	3	3	2	-2	-1	2	-4	3	2
Two months post-scan	4	-1	1	0	6	-2	1	3	-8*	1	.
Three months post-scan	2	-3	-1	-1	4	2	0	4	-11**	.	.
Four months post-scan	-1	-7*	1	0	5	3	1	1	-10**	.	.
Five months post-scan	-5	-6	-2	0	2	0	-1	0	.	.	.
Six months post-scan	-10*	-7	-3	0	1	-2	0	.	.	.	.
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	8*	2	5	4	-1	1	0	12**	-2	7*	5
Two months post-scan	7	5	7	5	5	4	4	15**	-2	8*	.
Three months post-scan	11**	3	6	4	5	10*	5	16**	-5	.	.
Four months post-scan	7	2	10*	5	8*	13**	6	17**	-5	.	.
Five months post-scan	6	6	8*	7	5	10*	4	17**	.	.	.
Six months post-scan	2	6	7	6	4	8	7	.	.	.	.
Upper bound											
*Assuming **none** of those leaving to unknown destinations enter work:*											
One month post-scan	10*	-4	6	6	4	-3	-2	5	-8	5	5
Two months post-scan	6	-1	2	0	12	-4	3	6	-15*	1	.
Three months post-scan	4	-6	-1	-2	7	4	-1	8	-22**	.	.
Four months post-scan	-2	-13*	2	0	10	6	2	2	-20**	.	.
Five months post-scan	-10	-11	-4	0	4	0	-3	0	.	.	.
Six months post-scan	-18*	-13	-6	-1	1	-4	1	.	.	.	.

Continued

Table 6.8   Continued

	04 Nov 2002	02 Dec 2002	06 Jan 2003	03 Feb 2003	03 Mar 2003	07 Apr 2003	05 May 2003	02 Jun 2003	07 Jul 2003	04 Aug 2003	01 Sep 2003
*Assuming **all** of those leaving to unknown destinations enter work:*											
One month post-scan	14*	3	10	9	-1	2	0	26**	-4	15*	11
Two months post-scan	13	10	12	10	9	9	9	32**	-4	16*	.
Three months post-scan	19**	6	12	8	9	21*	9	34**	-10	.	.
Four months post-scan	13	3	18*	10	16*	27**	12	37**	-10	.	.
Five months post-scan	11	11	16*	13	11	20*	9	36**	.	.	.
Six months post-scan	4	11	12	11	7	16	15	.	.	.	.

** - significant at 1%; * - significant at 5%

The tables above have presented all relevant results. All tables present upper and lower bounds on the same parameters; the mean effect of treatment on the treated. With so many results it can be difficult to gain an impression of the key findings. To address this, all before periods were pooled and all after periods were pooled and an effect was estimated using DiD on these pooled data. This drawback to doing this is that the detail of the changing effect over time is not visible. The strength is that the main results are more readily apparent. These are presented in Table 6.9.

Table 6.9   DiD estimates on the pooled dataset

	Unemployment assuming:		Employment assuming:	
	all transfers to clerical exit JSA	no transfers to clerical exit JSA	no unknown destinations enter work	all unknown destinations enter work
One month post-scan	(-4 , -7)**	(-1 , -2)**	(1 , 2)*	(3 , 6)**
Two months post-scan	(-5 , -9)**	(-1 , -2)	(0 , 1)	(5 , 9)**
Three months post-scan	(-5 , -9)**	(0 , 0)	(0 , -1)	(5 , 10)**
Four months post-scan	(-6 , -10)**	(0 , 0)	(-1 , -1)	(6 , 10)**
Five months post-scan	(-5 , -8)**	(2 , 3)**	(-2 , -3)**	(5 , 9)**
Six months post-scan	(-4 , -7)**	(3 , 5)**	(-3 , -5)**	(4 , 8)**

** - significant at 1%; * - significant at 5%. The numbers within parentheses in each cell give the bounds on the effects.

If all transfers are true exits from JSA, the estimates suggest that the Extension was successful in reducing the probability of remaining on benefit by the order of somewhere between four and ten percentage points. This range reflects the uncertainty in the data about whether couples who appear eligible for the Extension

but who are not recorded as receiving it, are, in fact, subject to it. The upper and lower bounds on this range correspond to the polar situations where either all of those with uncertain treatment status receive the treatment or when none do. Since the most plausible scenario is that the treatment status of such couples is mixed (with some being subject to Joint Claims conditionality and others not) the bounds are probably unlikely to coincide with the true effect. More reasonable would seem to be that the effect lies somewhere in the middle of this range – perhaps six or seven percentage points.

Clearly, the assumption made about transfers to clerical claims is important. Under the assumption that claims apparently ending for this reason actually continue on a clerical basis, the estimates of the effect of the Extension are greatly reduced (second column of results). The effects on one-month outcomes remain significant, if small. Effects on outcomes two, three or four months later appear insignificant. The chances of still being unemployed after five or six months appear to have been increased by the Extension. This is a perverse result and suggests that it may not be appropriate to assume that none of the transfers represented a true exit. However, it is worth highlighting the finding from the results presented earlier (Table 6.1) that it was only when considering the December 2002 post-Extension period that a significant positive outcome was found.

The employment effects are shown in the final two columns and, again, differ according to the assumption made about unknown destinations. Assuming none of those exiting to unknown destinations (including transfers to clerical claims) enter work, results in the finding that the Extension has a negative effect on longer-term job outcomes. The size of these effects matches closely to those of the unemployment effects that similarly assume that transfers do not constitute a change in status. Assuming all of those exiting to unknown destinations enter work results in the expected positive effects. Again, the estimated effects are similar in size to the effects on unemployment when it is assumed that all transfers are true JSA exits. These similarities between the effect on employment and the corresponding effect on unemployment suggest that the Extension operates chiefly by encouraging couples to exit JSA and enter work.

6.2   The effects of the Extension on the stock

Table 6.10 presents estimates of the effects on those eligible couples who had a live claim of more than four weeks duration at the time of the Extension. The first column of results shows a significant negative effect on the probability of still claiming JSA in the months following the Extension. The effects appear considerably smaller than for flow clients. Interestingly, in the first three months following the Extension it makes little difference to the results what assumption is made about claims that end as transfers to a clerical claim. However, for unemployment status four, five or six months after the Extension, estimates based on the assumption that none of the transfers enter work are not significant. As with the flow clients, the effects on employment mirror those on unemployment.

Table 6.10   DiD estimates of eligibility

	Unemployment assuming:		Employment assuming:	
	all transfers to clerical exit JSA	no transfers to clerical exit JSA	no unknown destinations enter work	all unknown destinations enter work
One month post-scan	-2**	-2**	1**	2**
Two months post-scan	-3**	-3**	2**	3**
Three months post-scan	-4**	-3**	1**	4**
Four months post-scan	-3**	-1	0	4**
Five months post-scan	-4**	-1	0	4**
Six months post-scan	-4**	-1	0	5**

Table 6.11 shows the results after excluding those with later IB claims. This has very little effect on the estimates.

Table 6.11   DiD estimates of eligibility, excluding those with IB claims

	Unemployment assuming:		Employment assuming:	
	all transfers to clerical exit JSA	no transfers to clerical exit JSA	no unknown destinations enter work	all unknown destinations enter work
One month post-scan	-2**	-2**	1**	2**
Two months post-scan	-4**	-3**	2**	3**
Three months post-scan	-4**	-3**	2**	4**
Four months post-scan	-3**	-1	1	4**
Five months post-scan	-3**	-1	0	4**
Six months post-scan	-3**	0	0	5**

7   The effects of Joint Claims on younger couples

Before the Extensions, only couples where at least one partner was born after 19 March 1976 were eligible for Joint Claims. The effects of Joint Claims on these younger couples were evaluated in Bonjour *et al.* (2002). This chapter uses more recent data to examine how the effect has evolved over time. The analysis is based on flow clients.

7.1   The effects of Joint Claims on the flow

Table 7.1 presents the estimated effects of Joint Claims on unemployment. The interpretation of the results from Bonjour *et al.* (2002) was:

• No statistically significant effect is detected for any of the first three post-Joint Claims scans. This is true regardless of which pre-Joint Claims scan is considered.

• The results for the fourth and fifth post-Joint Claims scans reveal a significant effect. This is in the expected direction suggesting that Joint Claims reduced the likelihood of remaining on Jobseeker's Allowance (JSA). 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. This appears consistent with qualitative evidence (Tapp and Thomas, 2000).

• The results for the August 2001 post-Joint Claims scan show a statistically significant effect for JSA status after one month and also after two months but an insignificant effect after three months. 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.

• It is worth noting that the results based on the September pre-Joint Claims scan are always slightly different from those based on the other pre-Joint Claims scans. This raises some concerns about the quality of the data in the September 2000 scan.

Table 7.1   Estimates of the effect of Joint Claims on unemployment among younger couples, using old version of the database

	Date of post-Joint Claims scan (2001)				
	30 April	**June 04**	**June 24**	**August 4**	**Sept 1**
25 September 2000 base					
One month post-scan	-3	3	-4	-8**	-14**
Two months post-scan	3	5	-2	-7**	-11**
Three months post-scan	5	5	1	-4	
Four months post-scan	9**	5	2		
Five months post-scan	10**	7	5		
Six months post-scan	9**				
27 November 2000 base					
One month post-scan	-6*	-1	-6**	-11**	-17**
Two months post-scan	0	0	-7*	-12**	-17**
Three months post-scan	3	0	-3	-8**	
11 December 2000 base					
One month post-scan	-6**	0	-6**	-10**	-17**
Two months post-scan	0	0	-7*	-12**	-17**
Three months post-scan	1	-1	-5	-9**	
22 January 2001 base					
One month post-scan	-5*	0	-5*	-10**	-16**

These results were based on a different methodology from that used to estimate the effects of the Extension. Specifically, couples who appeared eligible for a Joint Claim but who were not recorded as having started such a claim, were used to obtain a measure of the bias arising from the possible inappropriateness of the comparison group.[14] This bias was deducted from the effect estimated on those who were recorded as having started a Joint Claim in order to achieve the estimates given in Table 7.1. The rationale for this was that the introduction of Joint Claims allowed for a transition period during which time some claims were dealt with as Joint Claims and others were not.

When considering effects over a longer period of time, such an approach is more difficult to justify. From Figure 2.1 it is clear that, throughout the period covered by the data, a substantial fraction of those seemingly eligible for the original Joint Claims are not recorded as having started a Joint Claim. This is similar to the situation with Extension couples which was the motivation behind the bounding approach presented already.

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[14] This bias was mostly insignificant and varied in size and sign depending on the choice of before and after scans between –6 and 6 percentage points.

Table 7.2 shows the effects of Joint Claims over the longer run. The estimates correspond to the lower bound parameter discussed earlier. That is, they are estimated over all eligible couples and are equal to the treatment effects if all of those eligible actually receive the Joint Claims treatment. The upper bound parameters are not presented but are slightly less than twice the lower bound estimates.

Table 7.2   Estimates of the effect of Joint Claims on unemployment among younger couples, using current version of the database

	30 Apr 2001	04 Jun 2001	24 Jun 2001	04 Aug 2001	01 Sep 2001	15 Oct 2001	03 Dec 2001	07 Jan 2002	04 Feb 2002	04 Mar 2002	01 Apr 2002	06 May 2002	03 Jun 2002	01 Jul 2002	05 Aug 2002	02 Sep 2002
25 September 2000 base																
One month post-scan	1	2	-2	-7**	-3	-3	0	3	0	-1	1	-3	-4*	-2	-5*	-5*
Two months post-scan	9**	9**	3	0	1	3	3	7**	3	2	4	2	-2	2	-2	.
Three months post-scan	10**	11**	3	4	2	1	8**	9**	4	2	4	3	-3	2	.	.
Four months post-scan	14**	11**	4	6*	2	4	8**	8**	3	0	3	4	-3	1	.	.
Five months post-scan	15**	12**	6*	8**	4	5	7*	8**	1	0	5	6	-4	.	.	.
Six months post-scan	16**	14**	9**	11**	4	7**	6*	9**	3	3	6	4	.	.	.	.
27 November 2000 base																
one month post-scan	4	4*	0	-4*	0	-1	3	5**	2	2	3	0	-2	1	-2	-2
Two months post-scan	6*	6*	1	-2	-1	0	1	4	1	-1	1	0	-5	-1	-4	.
Three months post-scan	9**	9**	1	3	1	0	6*	7**	2	1	2	1	-5	0	.	.
11 December 2000 base																
One month post-scan	2	3	-2	-7**	-2	-3	1	3	0	0	0	-2	-4*	-1	-5*	-5*
Two months post-scan	8**	8**	2	-1	0	1	2	6*	2	1	3	1	-4	1	-3	.
Three months post-scan	8**	9**	0	2	0	-1	5*	6*	1	0	1	0	-6*	-1	.	.
22 January 2001 base																
One month post-scan	2	3	-1	-6**	-2	-2	1	4	0	0	1	-2	-4*	-1	-5*	-4*

Those results in Table 7.2 that correspond to those in Table 7.1 are boxed for ease of comparison. Since the effect of eligibility is a different parameter from the effect of treatment, it is not expected that the results be similar. However, some of the differences contradict in the sense that one would expect the former to be smaller than the latter. This is not the case for the 30 April 2001 and 4 June 2001 after scans which show strong positive effects, suggesting that Joint Claims served to increase the likelihood of remaining on JSA. By the time of the 24 June 2001 scan, the effects are mostly insignificant in both tables. For the 4 August 2001 post-scan, the situation appears plausible with the effects in Table 7.2 shown to be smaller than those in Table 7.1. However, the significant effects disappear in the estimates based on the 1 September 2001 post-scan in Table 7.2. On the whole, the impression that emerges from Table 7.1 is of a treatment that operated perversely to begin with but later operated according to expectations, increasing the chances of JSA exit. The impression that emerges from Table 7.2 over the same period has some similarities but also some differences. Again, there are perverse effects to begin with and these subside over time. However, the expected effects when they appear do not seem stable. This is borne out by the results for the later post-scans which show stubborn positive effects on unemployment (that is, Joint Claims acting to increase the chances of remaining on JSA).

The overall results are given in Table 7.3. This follows a similar format to Table 6.9 and shows the average effect over the whole estimation period. Each cell contains two estimates: one corresponds to the lower bound results presented above, the other to the upper bound results. These estimates suggest that, contrary to intentions, Joint Claims served to increase, by between three and five percentage points, the probability that couples beginning a JSA claim would still be claiming six months later. This is assuming that all transfers to clerical claims are true JSA exits. If no transfers actually exit JSA, the effect rises to between nine and 15 percentage points. Given these estimates, it would be surprising to find a positive effect on employment. Assuming none of those leaving to unknown destinations enter work, Joint Claims appears to reduce the probability of entering work by between six and nine percentage points. Under the opposing assumption that all those leaving to unknown destinations enter work, the employment effect is insignificant.

Table 7.3 Difference-in-differences estimates on the pooled dataset

| | Unemployment assuming: | | Employment assuming: | |
	all transfers to clerical exit JSA	no transfers to clerical exit JSA	no unknown destinations enter work	all unknown destinations enter work
One month post-scan	(-1 , -2)	(0 , 1)	(-1 , -2)*	(1 , 1)
Two months post-scan	(1 , 2)	(3 , 6)**	(-3 , -6)**	(-1 , -1)
Three months post-scan	(2 , 4)*	(6 , 10)**	(-5 , -8)**	(-2 , -3)
Four months post-scan	(3 , 5)*	(7 , 12)**	(-5 , -8)**	(-1 , -2)
Five months post-scan	(3 , 4)*	(8 , 13)**	(-5 , -9)**	(-1 , -2)
Six months post-scan	(3 , 5)**	(9 , 15)**	(-6 , -9)**	(-1 , -2)

** - significant at 1%; * - significant at 5%. The numbers within parentheses in each cell give the bounds on the effects.

7.2   A comment on the comparability of the results

The disparity between the results based on the old version of the database and those based on the version used for the evaluation of the Extension warrants some further consideration. There are a number of differences between the two versions of the database. Organisationally, the delivery of the raw data moved from MIDAS to ASDIC. Although, in principal, this should not introduce any differences, in practice it was not possible to completely reconcile the two eventual datasets and some couples captured in the original dataset were not observed in the later dataset. Another important difference is that the destination on claim-end in the MIDAS data does not include the 'transfer to clerical claim' category.

Despite this, the ASDIC dataset suggested a larger eligible population than the MIDAS dataset. Figure 7.1 considers the estimated size of the population eligible for Joint Claims. The first bar in each trio is the estimated population size using the database used for the 18-24s evaluation. The second bar shows the same estimates based on the new data. Clearly, these are much greater. A possible explanation is that the ASDIC data captures all claims for a couple while the MIDAS data captures only the most recent spell. Importantly, the trends exhibited by both sets of estimates appear similar. To investigate this further, the possibility that the estimates can be reconciled is explored by applying a constant scaling factor to the new estimates. The results of doing this are shown in the third bar, where the factor is chosen as the average across all scans of the ratio of old to new estimates. Reassuringly, the resulting trends are very similar to the old estimates, suggesting that both are capturing the same changes.

Figure 7.1   Comparing the estimated size of the 18-24s population under the old database with that under the new database
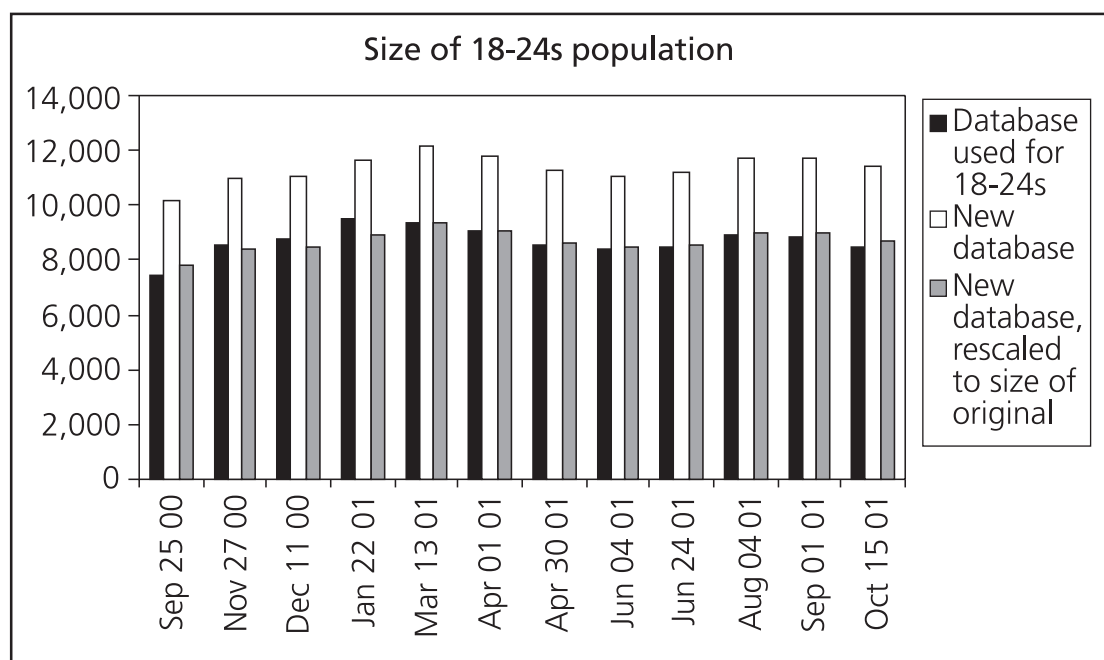
The question remains as to which estimates are viewed as being more accurate. As a first comment on this, it should be noted that the results achieved from the two datasets are not completely contradictory. Relative to the effects that were estimated using the MIDAS data, the estimates from the ASDIC data appear too large in the first two post-Joint Claims scans, broadly coincide for the next two scans and then fail to achieve significance in the final scan (while the final scan MIDAS estimates are significant). To reconcile these results, it is important to bear in mind the identifying assumptions of the different approaches. The MIDAS estimates regard eligible couples who were not making a Joint Claim as providing an estimate of the counterfactual outcome for those who were making a Joint Claim and uses this to derive an estimate of the bias arising from inappropriateness of the comparison group and thereby adjust the results. This approach was justified by the transition period at the time Joint Claims was introduced during which time old and new systems ran concurrently. Such an identifying assumption is most legitimate in those periods closest to the introduction of Joint Claims. In view of this, it seems appropriate to favour the MIDAS estimates for the first two scans following the introduction of Joint Claims. For the next two scans, the results are broadly consistent as noted above. For the September 2001 scan, there may be a reason to favour the ASDIC estimates.

As a final comment, it is important to remember that the data available do not extend back sufficiently to allow pre-programme tests to be carried out for the original Joint Claims. The consequence of this is that the appropriateness of the comparison group used for the Difference-in-differences (DiD) estimates is not formally established. In view of this, and in the light of the counterintuitive findings summarised in Table 7.3, the robustness of these results may be questionable.

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