Estimating the early labour market impacts of Universal Credit

Early analysis

Universal Credit Analysis Division

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Contents

Executive Summary

1.1 Introduction

Universal Credit (UC) is a radical reform of the benefit system designed to reduce poverty by making work pay. DWP has committed to a full evaluation of its delivery, its effects on attitudes and behaviours of claimants and, crucially, its impact on labour market outcomes. Assessing the extent to which UC helps move more people into work more quickly than those claiming benefits under the JSA regime is a key measure of success.

This paper summarises early results from a ground-breaking, early analysis of the impact of UC using new data on employed status from HMRC. It focuses on the early phases of UC in the 4 original Pathfinder offices: Ashton-Under-Lyne, Wigan, Warrington and Oldham. We will update the work as we extend the range of data further, obtain data on more claimants and develop the methods we use.

1.2 Method

To find out what impact UC has had on the labour market outcomes of people making new UC claims we need to estimate what would have happened to them under the JSA system. We do this by examining the outcomes of similar people making similar claims at the same time in similar areas. Getting the method right is central to developing a robust assessment of UC impact and demands a significant investment of time and resource. To achieve this, DWP analysts developed analytical methods in discussion with an independent external group of evaluation experts, drawing on advice from the Institute of Fiscal Studies and a peer review of the results by NIESR. NIESR's review is at section 7.6 whilst earlier work by the IFS and DWP to develop our evaluation plans can be found at https://www.gov.uk/government/publications/evaluating-the-impact-of-universal-credit-on-the-labour-market

The methodology presented in this paper is built on the back of that expert advice. We identified similar areas to those delivering UC based on historical off-flow rates and the volume and composition of on-flows at an office level. The analysis only compares new UC claimants with those making new claims to JSA who, as far as we can tell from the administrative data, meet the Pathfinder eligibility criteria. We use Propensity Score Matching to match the UC claimants with new JSA claimants in comparable areas at the same time who would have been eligible for UC had they made their claim in a Pathfinder office. Importantly we match new claimants on their detailed benefit and employment history, their age and gender (as well as all the Pathfinder eligibility criteria). This ensures that as much as possible we are comparing like with like and we can conclude that any remaining differences in outcomes are most probably due to UC.

1.3 Data

We used outcome measures derived by UC Analysts from the Real-Time Information (RTI) system and combined this with other administrative data to identify Pathfinder eligible new JSA claims and to obtain detailed benefit histories and demographics to ensure we only compare like-with-like. RTI is a new data source for the Department. RTI requires employers to submit a range of detailed data to HMRC on or before each payday for each employee (see Annex for more details). The RTI contains details of all payments made to employees. However, it cannot tell us how many hours people work or their wage rates. RTI has better coverage of employment than previous systems. The benefit of this data set is that we can more accurately measure employment outcomes for the vast majority of claimants compared to other data sources.

1.4 Results

The analysis examines nearly 6, 000 UC claimants over the first 120 days of their initial claim to UC between July 2013 and April 2014. Early results suggest that:

- New UC claimants are more likely to move into work than similar people making similar claims to JSA at the same time in similar areas.

- Our central estimates suggest that new UC claimants were 5 percentage points more likely to work in the four months/120 days after they made their claim than matched Pathfinder eligible new JSA claimants making similar claims in comparable offices at the same time.[1]

- UC claimants also tend to spend more time in work and to earn more on average. They spent an average of 4 more days in work during the first four months/120 days after the start of their claim and earned about £50 more during the period on average.

We have built in a range of exploratory sensitivity analyses to test how far the conclusions hold up to alternative approaches. These sensitivity tests do not significantly alter the estimates of UC's labour market impact.

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[1] In this report 1 month is 30 days.

1.5 Next Steps

This analysis marks a significant step forward in our understanding of how UC impacts on the labour market outcomes of people claiming the benefit. Results are encouraging. However, inevitably given the roll-out of UC and the time it takes to track outcomes this analysis only considers the impact on new claims during the very early stages of the policy in a small number of offices. We will develop and deepen our analysis to estimate what impact UC has on:

- the outcomes of more UC claimants as the number of UC claimants grows;
- different types of claimants in different areas; and
- longer-term outcomes including whether UC affects the sustainability of employment and labour market progression.

Over time we will continue to refine and develop the method and the data to improve our estimates.

2 Introduction

2.1 Background to Universal Credit

2.1.1 Policy

Universal Credit (UC) represents a fundamental reform of the benefits system. It is a new simpler, single monthly payment for people designed to reduce poverty. It makes work pay by tackling the problems of poor work incentives and the complexity of current arrangements. It helps claimants and their families to become more independent and simplifies the benefits system by replacing a range of working-age benefits[2] with a single payment.

The overarching aims are to:

- Encourage more people into work and to make even small amounts of work pay and be seen to pay.
- Smooth the transition into work by offering a single benefit that does not distinguish between being in or out of work.
- Offer a simpler support, with one system instead of multiple systems, therefore reducing administration costs and the propensity for fraud and error.
- Tackle poverty both through increased take-up since the system will be simpler and from increased reward from employment for the claimant.

Some of the main differences between Universal Credit and the current welfare system are:

- Universal Credit does not distinguish between people who are in work and on a low income, and those who are out of work.
- Most people apply online and will manage their claim through an online account.
- Universal Credit aims to be responsive – as people on low incomes move in and out of work, they will get on-going support.
- Many claimants on low incomes will still be paid Universal Credit when they first start a new job or increase their part-time hours.
- Claimants will receive just one monthly payment, paid into a bank account in the same way as a monthly salary.
- Support with housing costs will usually go direct to the claimant.

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[2] Income related Jobseeker's Allowance; Income related Employment and Support Allowance; Income Support. Child Tax Credits; Working Tax Credits; Housing Benefit.

2.1.2 Process

There are two stages to claiming UC as an unemployed person. The first stage is to make an online claim. The second stage is to attend a face-to-face interview (or Initial Work Search Interview) at a Jobcentre Plus office. Prior to the interview, a Personal Account Support Agent will deal with the administration of the claim, such as verification of identity. The interview itself is conducted by a Work Coach who will discuss the conditions of claiming UC. These are detailed in a Claimant Commitment. This is a two way agreement between the claimant and government setting out what claimants will do to find work in return for receipt of Universal Credit. The Coach will also advise of any support available and explain what will happen if responsibilities are not met.

The Claimant Commitment is updated if the claimant's circumstances change. UC claimants report changes of circumstance to the Universal Credit helpline. This is operated by UC Service Centres.

UC claimants who are looking for work are normally expected to attend interviews at the Jobcentre, including regular Work Search Reviews with an Assistant Work Coach, and additional Work Search Interviews with a Work Coach as required.

2.1.3 Roll-out

Universal Credit is being introduced in stages. Whether you can claim it will depend on where you live and your personal circumstances. It was initially introduced from April 2013 in certain areas of the North West, known as Pathfinder sites. Ashton-under-Lyne Jobcentre started to accept claims for Universal Credit from 29 April 2013, whilst Wigan, Warrington and Oldham Jobcentres trialled the new Claimant Commitment. From 1 July 2013, Wigan started to accept claims with Warrington and Oldham following suit on 29 July. Progressive roll out of Universal Credit began in October 2013, starting with Hammersmith Jobcentre and followed by Rugby, Inverness, Harrogate, Bath and Shotton by Spring 2014.

Initial eligibility criteria for UC focused on single, non-homeowning claimants without any children who would previously have been eligible for Jobseeker's Allowance (JSA). Details of the full eligibility criteria applied to the original Pathfinder areas – on which the analysis in this report is based – are set out on page 20.

The eligibility criteria was extended to couples from July 2014 and then to families with children in November 2014. Over the same period, UC began to rollout into other others across the North West of England. In September 2014, the Secretary of State announced the start of the national roll-out. From February 2015, UC will be progressively roll-out out to all Jobcentres in Great Britain.
Official Statistics[3] on the number of people claiming Universal Credit are published monthly. At the time of drafting the report, these showed that 54, 380 people have made a claim for Universal Credit up to 15th January 2015.

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[3] https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/396237/universal-credit-statistical-first-release-jan-15.pdf

2.2 Evaluation of Universal Credit

From the outset, DWP committed to a full evaluation of Universal Credit. In support of this DWP published initial thinking on an evaluation framework and an approach to testing and learning from UC in December 2012[4]. This highlighted the need to address a diverse range of evidence needs running from providing rapid operational information for those leading on delivery, through to much longer term evidence on the impact on labour market behaviour and outcomes. To ensure the evaluation is as robust as possible, DWP set up an advisory group of external evaluation experts. This Group's main role is to provide expertise and independent advice and challenge to the analytical team in relation to the development of the evaluation approach.

Last year, DWP published results[5] from the early evaluation of the first year of UC operations in the 4 original Pathfinder sites alongside a publication setting out the benefits and the strategy for rolling out UC[6]. This report builds on the published evidence by presenting results from detailed administrative data analysis using Propensity Score Matching designed to establish the impact of UC on labour market outcomes of claimants. As such it marks an important first step in assessing how well UC is delivering one of its core aims reducing poverty by helping people make the transition back into work.

2.3 Structure of report

The report is structured as follows:

- In **chapter 3**, the report lays out the core evaluation problem in assessing the labour market impact of UC. It explains the need to construct a robust counterfactual to measure what would have happened to UC claimants if they received JSA rather than UC.

- **Chapter 4** outlines the datasets available to carry out that matching as well as the specific outcome measures we can use drawing on the Real Time Information system which captures data on employment spells and earnings. It explains the decisions made in defining employment spells and the approach to quality assurance.

- In **chapter 5,** we explain the core method underpinning the analysis. This sets out in more detail how we have identified appropriate comparator areas with labour market characteristics similar to those in Pathfinder areas. In addition, it explains why and how the analysis uses a technique called Propensity Score Matching to robustly match UC claimants with JSA claimants with similar characteristics who would have been eligible for UC if they had lived in a Pathfinder area.

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[4] https://www.gov.uk/government/publications/universal-credit-evaluation-framework

[5] https://www.gov.uk/government/publications/universal-credit-pathfinder-evaluation

[6] https://www.gov.uk/government/publications/universal-credit-at-work

- On the back of this method, **chapter 6** outlines the results, stage by stage, from the success of the matching exercise through to reporting on the labour market outcome measures. It ends with an account of the sensitivity analysis we have conducted to quality assure the findings.

- Finally the annexes set out, in detail, the specific datasets we have drawn on, the detailed outputs from the matching exercise and the peer review.

3 Impact evaluation framework

3.1 Incentives for claimants under UC

By creating a single system, for those in and out of work, Universal Credit aims to ensure that work pays, and more work pays, for everyone:

- with the transition to/from work no longer putting household income at risk: the underlying entitlement to Universal Credit is simply adjusted to reflect earnings;
- with claimants in work able to retain all their Universal Credit while their earnings remain within the new Work Allowances, which are more generous than the equivalent earnings disregarded in the legacy system; and
- with a standard rate at which Universal Credit is then reduced as earnings increase, at 65% this is significantly lower than in the legacy system for many claimants, so that claimants see a financial increase from completing a few more hours of work.

In short, Universal Credit looks to remove perverse risks, barriers to work, and underlying complexity, to improve incentives. All of which clears the way to focus effort on finding work, instead of calculating whether a particular job will be beneficial. The Universal Credit Impact Assessment published in December 2012 concluded that "Universal Credit will lead to an increase in employment due to improved financial incentives, simpler and more transparent system, and changes to the requirements placed on claimants. Overall this could lead to the equivalent of up to 300,000 additional people in work"[7]

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[7] https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/220177/universal-credit-wr2011-ia.pdf

3.2 Outline of the key metrics

This paper aims to test whether and to what extent Universal Credit in the 4 original Universal Credit sites is delivering better labour market outcomes than legacy benefit system under JSA. It focuses on analysing some of the key outcome data available through new data on employment and earnings: Real Time Earnings/Information.

Initial analysis is on employment and earnings at different points in time after the benefit claim was made.

The results in this paper consider the impact of UC on the probability of being in employed[8]:
  a) 30 days after the award started;
  b) 60 days after the award started;
  c) 90 days after the award started;
  d) 120 days after the award started;
  e) 30 and/or 60 days after the award started;
  f) 30 and/or 60 and/or 90 days after the award started;
  g) 30 and/or 60 and/or 90 and/or 120 days after the award started;
  h) 30 and 60 and 90 and 120 days after the award started.

We also consider the impact of UC on:
  • days employed since the start of the award;
  • gross earnings since the start of the award; and
  • employed status in each week following the start of the award.

3.3 Defining the Treatment

Almost anyone of working age will, once UC is rolled out, be potentially eligible for UC. Therefore, we could evaluate the impact of UC on the working age population. However, this population is very large and diverse. Many working age people will be largely or wholly unaffected by UC because UC is not targeting the whole working age population. Consequently, if we focus on the working age population any impacts of UC will get diluted and be more difficult to detect. Therefore, we focus on evaluating the impact that UC has on sub-sets of the working age population who will be significantly affected (either intentionally or unintentionally) by UC. We focus on the impact of UC on new claimants because:
  • they will unambiguously be affected by Universal Credit;
  • the phased roll-out of UC to new claimants allows us to evaluate the impact on them reliably; and
  • the impact on new claimants is relevant to the steady-state impact of the policy.

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[8] Throughout the report in work, employment and employed are used interchangeably. However, it is important to note that the analysis does not capture self-employment.

We call new UC claimants the 'treated' population. Strictly the analysis in this report focuses on new claims that make it through to an award. For simplicity and brevity we use new claims and new awards interchangeably.

3.4 The evaluation question

To find out what impact UC has on the labour market outcomes of people making new UC claims we need to know what outcomes they would have achieved if they had instead - at the same time and in the same place - made a new claim to the equivalent legacy benefit.

The perennial evaluation question is that we never see both outcomes for the same individual at the same time. So, we have to **estimate** what outcomes the 'treated' new UC claimants would have achieved had they claimed JSA instead. That is, we estimate their counterfactual outcome.

We need to only use the outcomes of similar un-treated people or a comparison group to get a good estimate of what would have happened to new UC claimants in the absence of UC. For Pathfinder, a reasonable comparison group might comprise people who are making equivalent new JSA claims at the same time.

The difference between the estimated counterfactual outcome and the actual observed outcome gives the true impact of Universal Credit. This assumes the only relevant difference between the treated group of UC claimants and untreated comparison group of new JSA claimants is that one claims UC and the other claims JSA. This gives the average treatment on the treated (ATT).

A relevant difference is one that affects outcomes. If the non-treated group are the same as the treated group in terms of everything that affects outcomes, then their untreated outcomes will be an unbiased and an efficient estimate of what outcomes the treated group would have achieved had they not received the treatment. Unbiased means they will be right on average. Efficient means they will be close to the true value.

If the impact of UC is the same for everyone (homogeneous) then the impact on the treated is the same as the impact on the non-treated (ATNT), which would also be the same as the average treatment effect (ATE). If UC has a different impact on different people then the impact it would have on the untreated if they were subject to UC is likely to be different from its impact on those who do actually claim UC. To estimate what impact UC would have on non-treated if they were subject to UC we would have to estimate their counterfactual outcome using the outcomes of *similar* new UC claimants.

This report focuses on estimating the impact of UC on the treated – those who actually claim UC. This reflects that, for a reasonable time, we are going to have relatively few new UC claimants relative to the number of new legacy claimants. Consequently, we have a large pool of new untreated JSA claimants, which increases the chances that we will find claimants who are very similar to the people

making new UC claims. Nevertheless, in the future we will explore the scope for estimating the impact on untreated populations.

3.5 Anticipation and Entry Effects

The main limitation of focusing on new claimants is that we expect UC to affect take-up. Changes to eligibility and entitlement mean that UC will change who claims and the types of claims that *some* people make. Other factors such as awareness, attitudes, differences in in-work support and conditionality regimes might also affect take-up. Consequently, UC could change the composition of new claims through entry and/or anticipation effects.

People who might have claimed the equivalent legacy benefit before and formed part of the comparison group might decide to delay their claim in anticipation of UC becoming available in their area. This could lead to a difference in the composition of new claimants between the treated and comparison groups, which could mean that they would achieve different outcomes even in the absence of UC. If we can observe these differences they will not affect the reliability of the estimates. However, they would still reduce external validity since we can only evaluate the impact on the sub-set of new UC claims who would have made a new claim under both systems in the same circumstances – common support. If the compositional differences are unobservable and affect outcomes then the estimates would also be internally invalid (they would be biased).

Such anticipation effects are likely to be negligible during the period we focus on because UC would not become available for a reasonably long time in the comparator offices from which we draw the comparison group.

Entry effects might change the composition of the *treated* group if some people decide to claim UC who would not have claimed the equivalent legacy benefit. This would affect the internal validity of the estimates because the treatment and control or comparison groups will be different in ways that might affect the outcomes they achieve. This type of entry effect is likely to be relatively minor during Pathfinder because most people are likely to have been unfamiliar with the details of UC. Another type of entry effect is mechanical (rather than behavioural) and could arise because UC is being phased in by benefit type (as well as by geography) and once a person claims UC they will always be under the UC system from then onwards. This means that for legacy benefits that are replaced by UC later we can only estimate the impact of UC on the labour market outcomes of those new claims that have not already entered UC via another route. Again, this issue only becomes problematic when we come to evaluate later stages of UC.

Another entry effect is analogous to anticipation effects but involves the treatment rather than the comparison group. That is people could choose to accelerate or delay a claim around the time a new roll-out phase is introduced to affect which regime their claim falls under. Again, at this stage these are unlikely to be problematic

because we select the treatment sample from a time well away from the introduction of UC in the comparison areas.

So, overall the Institute for Fiscal Studies detailed feasibility study concluded that entry or anticipation effects are unlikely to compromise the reliability of our impact estimates during the earliest phases of UC because:

a) No-one making new claims to UC during this time would have had the opportunity to enter UC earlier via another benefit type or eligibility route because we focus exclusively on simple single new claims;

b) the eligibility and entitlement criteria under both benefit systems will be similar for the types of new claims replaced by UC during Pathfinder;

c) many people will not be aware of UC or familiar enough with it for it to produce entry effects from behavioural changes; and

d) we can select comparison samples far enough away from when UC is introduced.

4 Data

4.1 Data overview and sources

In order to isolate the effect that Universal Credit has on the likelihood of jobseekers finding and keeping work, we need to control for as many observable characteristics as possible that might influence differences between the "treatment" and "control" groups. These range from personal characteristics, and local labour market indicators, to benefit claim and employment histories.

Also, to ensure the analysis robust, we base it on the largest possible group of Universal Credit claimants, and compare their outcomes to the most closely matched control group possible, drawn from all JSA claimants making a claim over a similar time period. However, it is prohibitively expensive to run a claimant survey with such a wide respondent base as such surveys are expensive to administer. So, to support this analysis, we have assembled a wide-ranging evaluation database with data extracted from a number of DWP administrative systems to provide comparable information for JSA and UC claimants. This data is encrypted for purposes of data security and to prevent the identification of individuals, and then made available to analysts with the relevant security permissions via secure data servers. Different data items have then been linked together via encrypted National Insurance numbers, which provide a unique identifier across most DWP claimant data.

This evaluation dataset has also been linked to data from the HMRC Real-Time Information (RTI) system. RTI requires employers to submit a range of detailed data to HMRC on or before each payday for each employee (see Annex for more details). The RTI contains details of all payments made to employees. RTI has better coverage of employment than previous systems. People earning below the Lower Earnings Limit are included in the data providing at least one person being paid under the same PAYE scheme has earned above the Lower Earnings Limit in any period in that tax year. RTI can ultimately be used, not just to identify whether people were in work (though that is our starting point), but also to establish how much they were earning, and whether they are increasing their earnings. The RTI does not include information on hours worked, wage rates or earnings from self-employment. RTI data is shared securely between HMRC and DWP and only encrypted non-disclosive data is made available to analysts. This chapter details the process of producing this evaluation dataset and some of the quality assurance that has been undertaken to ensure it is fit for purpose.

4.2 Developing the evaluation dataset

A key challenge in comparing UC and JSA claimants is that the benefits are administered on different systems. In producing the evaluation dataset, we have transformed data obtained from these separate systems and combined them into a single, consistent file.

Table 1 shows the main categories of variables that were produced, and the administrative systems from which they were derived, both for Jobseekers Allowance and Universal Credit. Data on personal characteristics and current benefit claims are taken from different data sources for the control (JSA) and treatment groups (UC), whereas benefit history and outcome data are available on a consistent basis for both groups. Further details of the administrative systems used to derive these variables can be found in the Annex.

Table 1 Main data items in the evaluation dataset

Variable	Source	Drawn from same system?
Personal characteristics		
Encrypted National Insurance Number	Spans all administrative data	N/A
Gender	JSAPS for JSA, EM for UC claimants.	No
Age (5 year bands)	JSAPS for JSA, EM for UC claimants.	No
Marital Status (JSA only)	JSAPS	N/A
Benefit claim details		
- Date of new benefit claim (for UC or JSA)	JSAPS for JSA, EM and PMX for UC claimants.	No
- Jobcentre Plus Office Name	JSAPS for JSA, EM for UC claimants.	No
"UC Eligibility Indicator" (JSA only – indicates whether a JSA claimant would have been eligible for UC, had they lived in a UC area)	JSAPS and SHBE	N/A
Benefit and Employment Histories		
Whether claiming JSA or ESA in each week in the 2 years before current benefit claim	National Benefit Database (both JSA and UC claimants)	Yes
Whether claimed (1); Proportion of time spent claiming (2); number of spells (3) on the following benefits over the previous 2 years: AA, BB, DLA, ESA, IB, ICA, IS, PC, PIB, RP, SDA, WB [9]	National Benefits Database (both JSA and UC claimants)	Yes
Whether in work in each week in the 2 years before current benefit claim	Work and Pensions Longitudinal Study P45/P46 data (WPLS) (both JSA and UC claimants)	Yes
Previous participation in employment programmes (e.g. Work Programme, New Deals, etc.)	DWP Opportunity types database (both JSA and UC claimants)	Yes
Previous sanctions received during the last two years by level of sanction	JSA and ESA Sanctions	Yes
Employment Outcomes		
Whether employed 30, 60, 90, 120 days after claim start.	HMRC Real Time Information (RTI) (both JSA and UC claimants)	Yes
Days employed and gross earnings since claim start	HMRC Real Time Information (RTI) (both JSA and UC claimants)	Yes
Weekly Employed Status and Average Gross Earnings since claim start	HMRC Real Time Information (RTI) (both JSA and UC claimants)	Yes

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[9] AA - Attendance Allowance, BB - Bereavement Benefit, DLA – Disability Living Allowance, ESA – Employment and Support Allowance, IB – Incapacity Benefit, ICA – Carers Allowance, IS – Income Support, PC – Pension Credit, PIB - , RP – State Pension, SDA – Severe Disablement Allowance, WB – Widow's Benefit

4.3 Consistency between UC and JSA administrative data

As the table above highlights, different IT systems are used within DWP to administer UC and JSA claims, and record details about claimants. In this section we explain how we have ensured that we are only using data that is comparable between sources.

4.3.1 Determining eligibility for Universal Credit

For the period being evaluated in this analysis, Jobseekers were only admitted to Universal Credit if they met a number of eligibility criteria when they made a claim. To make a robust comparison, we restricted the analysis to only those JSA claimants who would have been eligible for UC if they had lived in a UC area. By combining data from the Jobseekers Allowance Payment System (JSAPS) with Housing Benefit data (SHBE), people making a new claim to JSA were assessed against most of the UC eligibility criteria. Most of this data was not retained for the final analysis, but summarised in a single variable which labelled people as being ineligible for UC if there was evidence that they:

- were not a UK national
- were aged under 18
- were aged over 60 years and 6 months
- had a partner[10]
- had a dependent child
- had capital exceeding the set limits
- had material earnings
- owned a home (specifically if they received support for mortgage interest)
- were homeless, or in temporary or supported accommodation
- were also receiving Carers Allowance or Disability Living Allowance
- were in receipt of Housing Benefit[11]
- had a previous JSA or ESA claim ending within 2 weeks of this claim beginning[12]

These conditions do not encapsulate all UC eligibility criteria. For example, the Armed Forces and Personal Acting Body[13] conditions are not considered. Similarly, we do not have data to identify claimants who are pregnant or those without bank accounts. Additionally, the conditions which are considered above will be contingent on the accuracy of DWP systems in capturing those personal details. Overall

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[10] Couples were admitted in pilot areas from June 2014, and in all live offices from end-July 2014, although they are not included in the cohorts used in the present analysis

[11] This condition is removed from Mid-June 2014 to reflect the introduction of a new singles gateway onto UC. This change does not affect the cohorts of benefit claimants examined in the present analysis.

[12] This condition is removed from Mid-June 2014 to reflect the introduction of a new singles gateway onto UC. It does not affect the cohorts of benefit claimants examined in this analysis, however.

[13] When another person or organisation acts on someone's behalf for benefit claim purposes

however, it is the best attempt at estimating UC eligibility, given the data readily available for this analysis.

4.3.2 Date of new benefit claim

The analysis in this report compares outcomes for people who actually received a benefit award after making an initial claim, so it is important that the conditions for successfully receiving a UC or JSA award are similar, and that definitional differences are not confounding the evaluation results.

The current approach for identifying Universal Credit awards in this analysis has 3 steps:
- Identify unique claim using the Encrypted National Insurance Number and Claim ID number (Source: Evidence Manager).
- Identify if/when a Claimant Commitment has been signed (Source: Evidence Manager).
- Identify if/when UC entitlement has been assessed (Source: PMX)

The date of award is therefore defined as the submission date of a valid claim that has a signed Claimant Commitment and a UC Assessment Period has been created. The approach used to create UC awards has been validated by comparing against a sample of claim outcomes observed via UC operational systems. This simple comparison suggests that 98% of the sampled claims that were observed to receive an award for UC are correctly identified via Evidence Manager and PMX.

The approach used to identify JSA awards is relatively simple since the JSAPS data allows us to isolate just those claims that were 'successful'. These data can be used to identify the date the claim is made and adjudicated. Claim exit dates and hence duration can be inferred from the 'event' date when claim entitlement is assessed and uploaded onto the system. Detailed analysis revealed a number of JSA claims to have durations of 1 day. It is understood that these 1 day claims are not real and simply a function of how agents record and close 'events' on the system. As such any 1 day claim is assumed to be a failed or rejected claim and not included in any analysis.

Detailed analysis of the elapsed time between the claim date and the event date when a claims entitlement has been established/recorded suggests that whilst JSA claimants are currently processed slightly faster than UC the overall profile is broadly similar and the impact of any difference, for example increased likelihood of one group finding work before their benefit entitlement is established, is assumed to be negligible. In future work we will explore whether these small differences in processing might affect the estimates. We are adding the date that the new claim was made into the data so we can measure outcomes from the date of the new claim as well as looking at outcomes from the date of the new award as we do here.

4.4 Benefit Histories and Employment Information

4.4.1 Benefit Histories

Detailed information on start and end dates for benefit claims were obtained via the National Benefit Database (NBD) which brings together data from a number of DWP benefit administrative systems into a single, consistent, evaluation dataset. Further information about the NBD can be found in the annex.

4.4.2 Employment Programme information

Information about previous participation on DWP Employment programmes was also included. This was obtained from the DWP Opportunities dataset which holds information about many DWP employment programmes and is taken from the Labour Market System. Initially we have included any participation in New Deals, Flexible New Deal, Work Choice and Work Programme.

4.4.3 Employment data

It is worth noting that we have used different data sources to calculate employment *histories* and *outcomes.* Histories were calculated using the Work and Pensions Longitudinal Study which contains details of employment start and end dates from P45 and P46 forms. This data was used as there is a consistent historical series. The quality of the data is known to be limited: HMRC did not require P45 and P46 forms to be completed for people whose earnings were below the Lower Earnings Limit or self-employed, and start and end dates were often only approximate, or missing.

For employments since 2013/14, we have been able to use data from HMRC's new Real Time Information (RTI) system, which has a much wider coverage of employees and much more detail on their earnings and periods in employment. RTI requires employers to submit a range of detailed data to HMRC on or before each payday for each employee (see Annex for more details). The major advantages of using RTI data to measure employment outcomes compared to P45 records are:
- RTI contains details of all payments made to employees, so we can identify periods when people were "employed". Also, missing employment start and end dates can be inferred from payment patterns.
- RTI has improved coverage. People earning below the Lower Earnings Limit are included in the data providing at least one person being paid under the same PAYE scheme has earned above the Lower Earnings Limit in any period in that tax year.
- RTI can ultimately be used, not just to identify whether people were in work (though that is our starting point), but also to establish how much they were earning, and whether they are increasing their earnings.

There are still some people whose employments aren't covered by the RTI, in particular, self-employed people[14] and people in PAYE schemes where no member earns more than the Lower Earnings Limit in any given pay period within a tax year (thought to be very small numbers).

This is the first time that RTI data has been used for an impact analysis of this kind, and as an administrative system, it requires a lot of processing before it can be used for analysis. The annex contains more information on steps we have taken to do this, and the main assumptions that have been made.

It is important to note that the impact estimates represent the impact of UC on employments covered by the RTI. If UC has a differential impact on employments that RTI does not capture we will not pick this up. For example, we cannot tell from this impact evaluation whether UC might lead to more self-employment. Similarly, if UC makes employers more likely to report small amounts of earnings so that more employment gets captured under UC than under the legacy system then our impact estimates would be biased upwards. We are exploring these issues through other strands of the evaluation.

4.4.4 Quality assurance of the RTI employment spells methodology

A detailed methodology note for analysing RTI data was produced which outlined the process for estimating employment spells. This was peer reviewed by DWP colleagues, and analysts from HMRC who are experts in the RTI data. The key recommendations are summarised:

- Use pay to date records to estimate missing/unusual payment schedules. This will allow for more accurate start and end date estimation.
- Identify breaks in individual's employments and investigate tolerance to their rule set.
- Identify robust variables to create unique periods of employment

These recommendations have been incorporated within this analysis (see Annex for details). We also conducted internal consistency checking of actual employer reported start and end dates versus those imputed based on earnings profiles. Detailed analysis of the most simple spells suggest that 75% of the estimated starts are within 1 week of the reported start date and 90% are within 4 weeks of the reported date. Only 40% of the estimated end dates are within 1 week of the reported date – but 90% are within 4 weeks.

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[14] A recent DWP destinations survey found that around 9% of people leaving JSA and immediately entering work were self-employed
(https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/214578/rrep791.pdf)

Finally, we also conducted extensive consistency checking of the RTI data against other relevant data sources:

- **The Universal Credit operational Real Time Earnings feed:** RTI isn't just used for the evaluation of Universal Credit. A separate operational feed of data is used to allow UC payment systems to automatically take into account earnings information when payments are calculated. However, this data isn't available for JSA claimants, so we obtained a separate sample for both UC and JSA claimants, and verified that the sample contains all of the records that were included in this operational data feed, and that none were omitted.

- **UC claimant survey**: DWP has commissioned a series of claimant surveys[15] to understand experience, attitudes and outcomes of UC claimants compared to similar JSA claimants. These surveys allow us to triangulate results from RTI analysis to develop a rounder picture of UC labour market outcomes.

- **Cohort survey data:** Internal analysis has looked to track the experience of different cohorts of claimants through UC drawing on internal DWP administrative systems. Analysts then compared this against data on the progress of a similar cohort of JSA claimants[16].

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[15] https://www.gov.uk/government/publications/universal-credit-pathfinder-evaluation

[16] https://www.gov.uk/government/statistics/comparison-of-universal-credit-and-jobseekers-allowance-outcomes

5 Methodology

5.1 Outline Method

Our approach reflects the independent peer review of our evaluation plans by researchers at the Institute for Fiscal Studies (IFS). IFS provided a number of invaluable pointers towards refining our analytical approach through their report: "Evaluating the labour market impacts of Universal Credit: a feasibility study". IFS's peer review of our proposed evaluation plans [17] concluded that, within the boundaries of what we are hoping to evaluate under the early phases of Universal Credit roll-out, "…the proposed evaluation strategy is wholly appropriate…".

5.2 Identifying the Comparison Group

As discussed earlier we need to estimate what outcomes new UC claimants would have achieved had they remained under the legacy system and claimed JSA instead by using a comparison group of new JSA claimants. We want the comparison group to be the same in all relevant respects to the UC group of claimants except that they are claiming JSA rather than UC. That is, the only relevant difference between the treated UC group and the untreated JSA comparison group should be UC. A relevant difference is anything that might affect the outcomes the two groups achieve. If there are any other relevant differences in addition to UC then we cannot isolate what contribution UC makes to any difference in outcomes that we might observe.

We know that the labour market outcomes of new claimants depend on many things. They vary depending on individual characteristics, local labour market conditions and JCP office performance and they vary over time with changing economic conditions. We therefore need to ensure that relative to the UC group of new claims the comparison group comprises similar people making similar claims in similar areas at similar times.

The phased introduction of UC by geography and time means we can construct a comparison group of similar claimants who remain under the legacy system in two ways. First, we can look at similar people who make a similar new claim at the same time but who do not claim UC because of **where** they claim (geographical variation in treatment). Second, we can look at similar people who made a similar new claim in the same area but did not claim UC because of **when** they claimed, i.e. they claimed before UC was introduced in that area (time variation in treatment).

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[17] https://www.gov.uk/government/publications/evaluating-the-impact-of-universal-credit-on-the-labour-market

We focus on identifying a comparison group using geographical variation. This reflects that it is difficult to identify truly comparable time periods because of seasonal and cyclical factors. Theoretically, we could potentially construct comparison groups based on legacy benefit type and demographic group since the roll-out of UC varies according to these factors too. However, we think it would be much more challenging to identify a suitable comparison group from another legacy benefit or another demographic group because we would expect their labour market outcomes to be different anyway making it very difficult to isolate any difference that might be due to UC.

As already highlighted new claimants are only eligible for UC if they satisfy certain criteria. For example, during the earliest phases of the policy new claimants had to be single, have no dependent children and no Housing Benefit claim. Consequently it would be inappropriate to construct a comparison group from all new JSA claims. Instead we focus only on the sub-set of new JSA claimants who, as far as we can tell from the administrative data, would have met all the eligibility criteria for UC under the original Pathfinder policy. This roughly halves the number of new JSA claims can use in our comparison group. However, this is not a concern because the sample of new Pathfinder eligible JSA claims is still very large relative to the population of treated new UC claims and so we are still confident that we will be able to identify enough new JSA claims to form a very good comparison group.

We have already noted that existing data do not capture all the UC eligibility criteria. It is consequently possible that we are not able to control for all potentially relevant differences between the UC treated group and the JSA comparison group. This would bias our estimates if the criteria we cannot capture means that the two groups are: a) different; and b) these differences have an additional effect on outcomes over and above all the criteria we can capture and the large number of factors we control for in the matching process.

We think the eligibility criteria we can take into account combined with the formal matching on many other factors, which we know are very important in explaining new claimants' outcomes and which are also likely to be correlated with the eligibility criteria cannot currently capture, means that this risk is low. This view is supported by the sensitivity analysis we describe in section 6.5. Nevertheless, we will continue to explore the scope for drawing on other data sources to control directly and explicitly for more eligibility criteria. Later phases of the evaluation will also be able to explore the importance of this issue as the eligibility criteria change.

Whilst we confine the comparison group to the same benefit and household type who also meet a range of eligibility criteria there are still two other potential sources of selection bias we need to address. Firstly, we know that UC is only available in certain areas and that people's labour market outcomes depend on where they live. For example, a new claimant's labour market outcomes will vary with local labour market conditions, the performance of their local JCP office and the policy context in their area, i.e. what other policies exist or are being introduced in their area that can affect the outcomes achieved by new claimants. Secondly, there may still be differences between the individuals making new UC claims in the Pathfinder offices and those making new Pathfinder eligible JSA claims in other offices that are not captured by the eligibility criteria.

5.3 Establishing comparator offices

Since the outcomes a new claimant achieves depends on where they are, e.g. the local labour market conditions and JCP performance, we want to make sure that the comparison group of Pathfinder eligible new JSA claims are drawn from areas that are as similar as possible to the Pathfinder areas. We identify the best comparator offices for each Pathfinder office in turn by analysing historical labour market outcomes at an office level and taking into account the size and composition of new claims. We focus on the historical outcomes for Pathfinder eligible new JSA claims.

Section 7.3 provides more details about the measures we use to identify the comparator offices. We only identify comparator offices for the original four Pathfinder offices. This reflects that it is only these offices that have had enough new UC claims and sufficient time to track their outcomes. As the volume of new UC claims increases in other offices we will extend the work to include more locations. This has important implications for the conclusions we can draw. We are evaluating the impact of UC on new claims in the original four Pathfinder offices during the early phases of the roll-out. However, this is inevitable because of where we are in the roll-out and because the outcomes we are interested in occur over time.

We only select the comparison group of individuals – those making new Pathfinder eligible new JSA claims - from the sub-set of comparator offices. This ensures that the balance of offices is similar between the comparison group and the UC treatment group. We have also estimated impacts separately for individual offices. That means for each Pathfinder office we only compare new UC claims with new Pathfinder eligible new JSA claims made in the comparator offices that are specific to that Pathfinder office. When we stratify the matching in this way it does not change the overall average estimate. After we identify the comparator offices using the combination of measures described in section 7.3 (which include matching at an office level) then we estimate impacts using matching at an individual claimant level.

Inevitably there is a risk that offices and local labour markets that have been similar in the past become dissimilar over time. We mitigate this risk, at least to some extent, by including multiple comparator offices for each Pathfinder office. Other reasons for including multiple comparator offices (the best for each Pathfinder office) include:
- to increase size of the comparison group to increase the chances that we will be able to identify a sufficient number of individuals who are the same in all observable relevant respects to the treatment group; and
- it is difficult to identify any single office that is a much closer match than any other.

In addition we conduct various sensitivity analyses around the selection of comparator offices. For example, we see if the results change if we select alternative comparator offices. We also estimate the results without confining the comparison group to comparator offices. Part of the rationale for this is that we include detailed benefit and employment history at an individual level and this information should capture recent local labour market conditions and trends.

In the future we will examine whether there are any differential trends between the Pathfinder and comparator offices by exploring the outcomes of other claimant groups over the same period. This will help assess whether recent macro trends might have had a differential affect in the Pathfinder offices that we have not yet identified.

5.4 Matching methods

We have restricted the comparison group to: Pathfinder eligible new JSA claims; and areas or offices with historically similar labour markets and performance. The next step matches the UC and JSA new claims to ensure that they are as similar as possible in terms of everything that might affect their employment outcomes. The more things we try to match on the more difficult it becomes to identify good matches on all the criteria. We use Propensity Score Matching (PSM) to reduce this dimensionality problem. PSM estimates a propensity score for a Pathfinder office and each non-Pathfinder office. This score is the propensity to receive the treatment conditional on the observed variables. Rosenbaum and Rubin, 1993 showed that matching on a single index representing the probability of treatment given the observed variables could achieve consistent estimates in the same way as if we matched on all variables. Thus, identifying individuals with a propensity score that is most similar to each new UC claimant helps identify the new JSA claims that are similar across all the variables.

Propensity Score Matching has two other main advantages over other regression-based non-experimental methods. Firstly, it emphasises and restricts the analysis to only estimating impacts on treated people for whom we can identify suitable matches in the non-treated sample. So, it only compares like with like.

Secondly, matching is non-parametric - it does not make any restrictive assumptions about how outcomes are determined and about how the observables affect impacts. Once treated and non-treated samples are matched we can just compare mean outcomes as we would if we did an experiment. Strictly, using the propensity score means matching becomes semi-parametric as it does involve estimating a model of participation. The main objective of this model is to ensure the treated and comparison groups are well balanced.

The validity of Propensity Score Matching and other non-experimental evaluation methods relies on the assumption of conditional independence. This assumption means that conditional on observed characteristics the counterfactual outcome is independent of treatment. This means we can observe all the things that affect both the likelihood of treatment and outcomes. This assumption is untestable. If there are unobservable traits that affect both treatment and outcomes then the estimates will be biased. Various studies suggest that the rich administrative data available and particularly the detailed information we have about people's past labour market and benefit claim history may be sufficient to obtain reliable estimates. This is true even if the detailed labour market and benefit history do not reflect all the usually unobserved factors that might bias the results such as motivation, attitudes to work, etc. (e.g. Caliendo et al 2014). Arguably the decision to claim UC or JSA is not entirely voluntary and will, in many cases, be driven by need. Consequently, unlike

many voluntary active labour market policies there is less risk that some of these unobserved factors attributes will bias the impact estimates because we have less reason to think that they will differ between the treatment and comparison groups.

We report the results for each outcome but for the purposes of describing the approach we focus on the outcome which measures the number of days that people have spent in work during the 120 days after they made their benefit claim.

We start by estimating the probability of treatment using the observed individual characteristics of the treated and comparison group individuals as independent variables. In particular we include gender, age, month of on-flow, number of recent benefit claims, past benefit and employment history, past participation in other DWP programmes and past sanction history. To reflect that many of these things are inter-dependent we include a number of interaction terms between them in the model.

As discussed, we have identified comparator offices based on past outcomes and the volume and composition of on-flows. This may, to some extent, capture the policy context. That is, offices that have achieved similar outcomes in similar circumstances are likely to have faced comparable policy environments. However, this is not necessarily the case. During the period we focus on (July 2013 to April 2014) the JSA claimant commitment was rolled out across the country. To reflect this, we include a dummy variable to indicate whether the JSA claimant commitment had been introduced when the claim was made.

This has implications for how we interpret the impact estimates. In particular, for some new UC claims (made before the claimant commitment was introduced in a Pathfinder office) we will be estimating the impact of UC compared with the original JSA regime. However, for new UC claims made after the claimant commitment was introduced in a Pathfinder office we are comparing UC with the JSA regime with the claimant commitment. Consequently, the overall impact we estimate is a weighted average of the two and so reflects the impact of UC versus a JSA regime in transition. We have conducted some sensitivity analysis to explore this issue in more detail (see section 6.5).

Following recent other applications of PSM to evaluate DWP programmes[18], we include a dummy variable for each week prior to the start of the most recent claim to indicate whether the individual was in receipt of benefit in each week prior to the start of their claim. In this initial analysis we only consider JSA and ESA receipt. So, for JSA receipt we have 104 dummy variables one for each week during the two years prior to the new claim. We have the same number of dummy variables for ESA claim history and for employment history.

We derive each of the two weekly benefit claim dummy variables and the employment history dummy variable from over 700 dummy variables which indicate whether the claimant was in receipt of JSA (in receipt of ESA or in work) on each day during the two years prior to their most recent claim. We define people as being in receipt of JSA in a week if they received JSA every day during that week. This helps

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[18] E.g. Ainsworth, P. and Marlow, S. (2011) Early Impacts of the European Social Fund 2007-13, DWP In-House Research Report No.3.

avoid counting very short and potentially incorrectly recorded past claims. The data suggest that the JSA Pathfinder eligible group were more likely to have claims lasting less than one week. To take these into account we also included a variable to indicate the amount of time in total each person spent in receipt of JSA during the previous two years. We also include this term squared to reflect that its *marginal* impact appears to diminish as the proportion of time spent on JSA increases.

The results from experimenting with different specifications showed differences between the UC and JSA claimants in terms of their age and recent benefit and employment histories. We include interaction terms in the treatment model to account for this. We also include interaction terms to reflect that older people are more likely to have more recent benefit spells. These terms are significant in the model and unbalanced in the unmatched samples. This suggests we are right to include them to help ensure greater comparability between the matched samples.

UC claimants are much less likely to have claimed other types of benefits. However, we include variables to identify those that have claimed a different type of benefit, the number of other benefit spells they have made during the last two years and the proportion of the last two years they have spent claiming other benefits. These variables are jointly significant in the treatment model. We use similar variables to capture the past participation in other DWP programmes (Work Choices, Work Programme and the Flexible New Deal).

Having estimated the propensity score – the probability of an individual claiming UC – we use Kernel matching. This uses all JSA claimants with propensity scores close enough to the score of the relevant treated individual, and weights them using an Epanechnikov distribution. The bandwidth selected determines how close the propensity score of individuals in the comparison group has to be for them to be included as a match. The weight given to those within the chosen bandwidth (in this case 0.0001) is bigger the closer their propensity score to the treated individual's score. The result is for each new UC claim we have a weighted matched comparison drawn from all JSA claims with relatively close propensity scores to the UC claimant with bigger weights given to those that are most similar.

This matching approach (and choice of bandwidth) is the one that several recent DWP evaluations have used. It has proved effective at balancing the characteristics of the treatment and comparison groups. We have carried out sensitivity analysis using different matching methods and different specifications of the treatment model. The results we report are based on Kernel matching as that approach produced the best matching of the treatment and comparison groups. However, the choice of matching approach did not significantly alter the actual impact estimates.

6   Results

6.1 The Treatment and Comparison Groups

It is important to be clear about how we select the treatment and comparison groups before we compare their outcomes and then estimate the impact of UC.

In the last chapter we stressed the importance of making sure as far as we can with the data available that the comparison and treatment groups are the same in all relevant observable respects except for Universal Credit. We discussed this in the context of ensuring the two groups comprised the same types of individuals and households, making the same types of benefit claim, at the same time in similar areas. However, it is just as important that we have consistent data for both groups. This is not necessarily the case because the way we collect data is changing under UC and continues to evolve.

Chapter 3 outlined how we have sought to ensure comparability between the treatment and comparison groups in terms of the data we use. For example, we confine the comparison group to include only the new JSA claims that we think would meet the UC eligibility criteria and we exclude any new JSA claims with very short durations to ensure the on-flows we identify are comparable. Together these reduce the number of new JSA claims in the comparison group by about half leaving just over 1 million new JSA claims.

The outcome data we use is from the RTI. The RTI was rolled out between April 2013 and October 2013. Internal analysis shows that, prior to July 2013, outcomes recorded by the RTI in the original Pathfinder offices, were volatile. This reflects the small volume of UC claims and the roll-out of the RTI. In the data currently available for analysis, after April 2014 outcomes recorded by the RTI are much lower than those for earlier monthly on-flow cohorts. This reflects the relative lack of retrospection for later cohorts. These patterns are the same whichever outcome we consider. We do not have any reason to believe that either the RTI roll-out or the lack of retrospection for later cohorts would have a differential impact on UC compared to JSA claimants. Nevertheless, just in case it does, we limit comparisons to those cohorts for whom we think we have reasonably complete outcome measures. Therefore, we only estimate the impact on new UC claims made between July 2013 and April 2014.

We conduct sensitivity analysis by confining the analysis to different periods. This reflects that whilst the roll-out of RTI was not done geographically there is still a risk that it rolled out differently in different areas (e.g. because some areas might have had more big employers who introduced RTI earlier).

During this period some people make more than one UC claim. We only evaluate the impact of UC on first new UC claims. This reflects that we can only identify matched new Pathfinder eligible JSA claims for people who are making their first UC claim. People making a second UC claim have recent UC benefit history which we do not

have for JSA claimants. Differences between UC and the legacy benefits means we cannot reliably estimate what UC history new JSA claimants might have had if they had made a previous UC claim. This does not mean we exclude people who make multiple UC claims – we evaluate the impact of UC on their outcomes from the time they make their first UC claim and their outcomes will reflect their subsequent UC claims.

Lastly, the main analysis only draws the comparison group from the sub-set of comparator offices for the original four Pathfinder offices. Figures 1 and 2 show how we select the sample for the main analysis for the UC treatment group and the JSA comparison group respectively. Some steps involve excluding observations with unreliable data. For example, we exclude observations where an individual is recorded as having made a new JSA claim *after* they claimed UC.

Figure 1 The Treated Sample

UC Sample

Reason for Dropping Observation
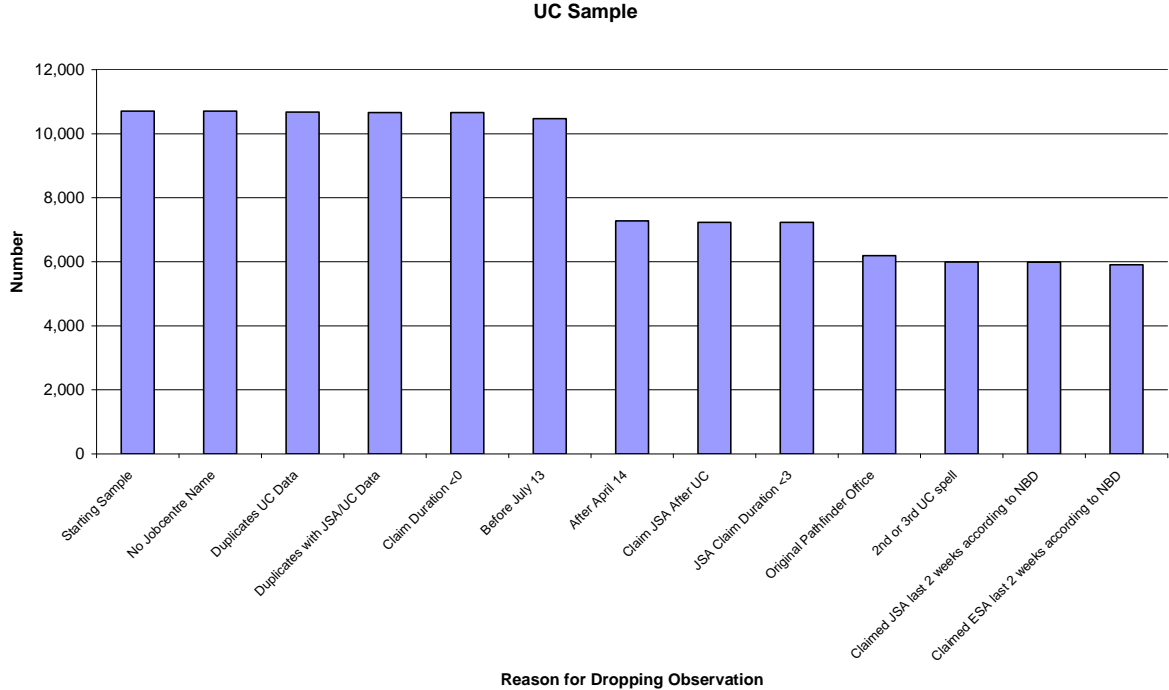
Figure 2 The Untreated Sample – the "Pathfinder Eligible" Comparison Group

JSA Sample

Reason for Dropping Observation
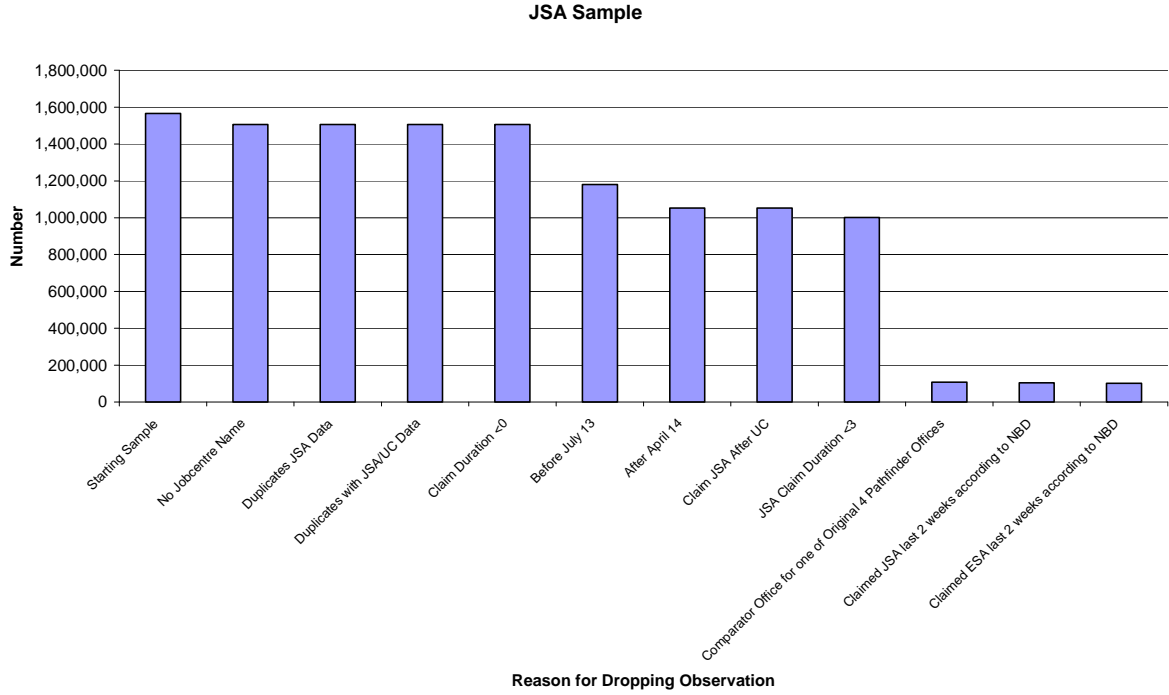
6.1 Descriptive Analysis

Figure 3 shows the outcomes achieved by all new first UC awards made between July 2013 and April 2014 in the original Pathfinder offices compared with all Pathfinder eligible new JSA claims during the same period in comparator offices before we match cases. It shows that people making a new 1st UC claim are significantly more likely to be employed at each point in time after the start of their claim. These differences are all statistically significant at 5%.

Figure 3 Unmatched Percentage Point Differences In Outcomes

Percentage Point Difference in Outcomes
1st New UC claims in Original Pathfinder Office and All JSA Pathfinder Eligible in Comparator Offices
July 13-April-14 New Claims
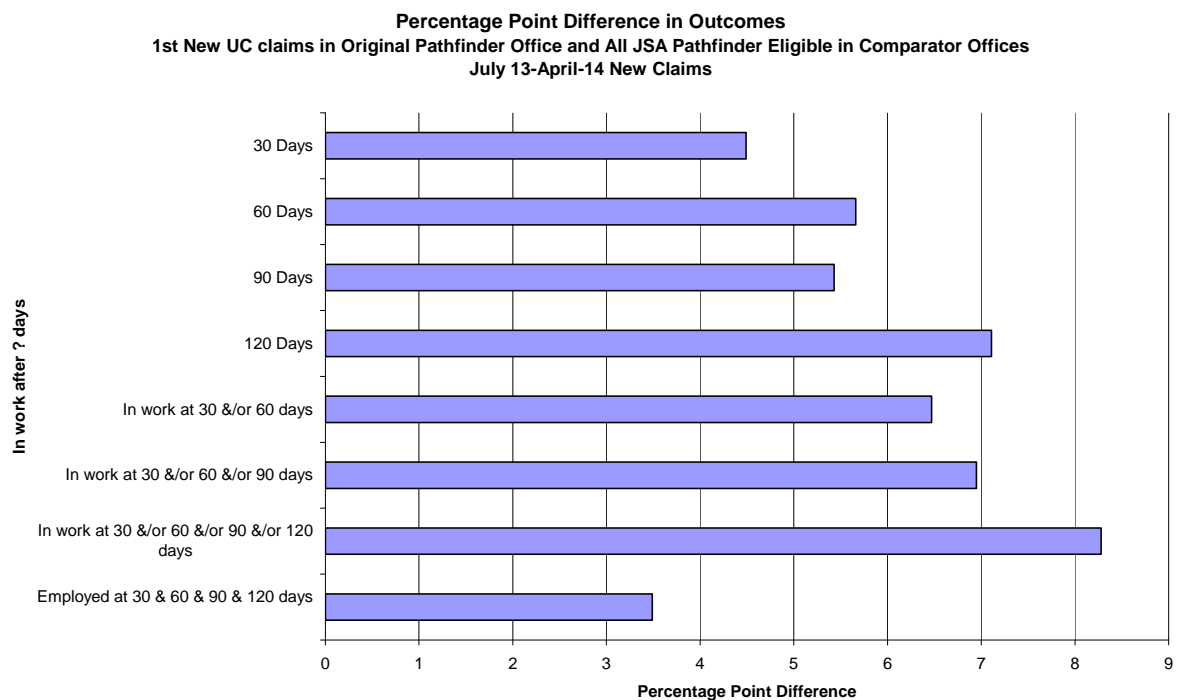
Whilst Figure 3 limits the JSA group to those new awards that meet the Pathfinder eligibility criteria as far as we can tell from administrative data and are claiming in comparable offices to the Pathfinder offices there may still be differences between the JSA claimants and those claiming UC. In particular, we know that we can only approximate eligibility from the administrative data. For example, even after UC has been introduced we find people who we think (based on the administrative data) should be claiming UC who are claiming JSA. Moreover, it is clear that these JSA claimants tend to achieve worse outcomes than their UC counterparts. For example, whilst about 47% of the UC claimants in the original Pathfinder offices are employed at some point during the four months after their award started, only 38% of those who we identify, based on the administrative data, should have been eligible for UC but actually claim JSA instead are employed during the same period.

Some of this difference in outcomes could be due to UC. However, it seems likely that our inability to accurately identify eligibility means that there remain significant differences between the UC and JSA 'Pathfinder eligible' new claimants, which could contribute to the different outcomes they achieve. For example, comparing the UC and JSA residual samples in the Pathfinder offices we find that the JSA residual group tend to be older and have spent more time claiming benefits during the two years before their claim (see Figure 4). Therefore, it is important that we match the two groups to ensure we only compare like with like.

Figure 4 UC vs. Residuals

Pathfinder Eligible New Claims in Pathfinder Offices
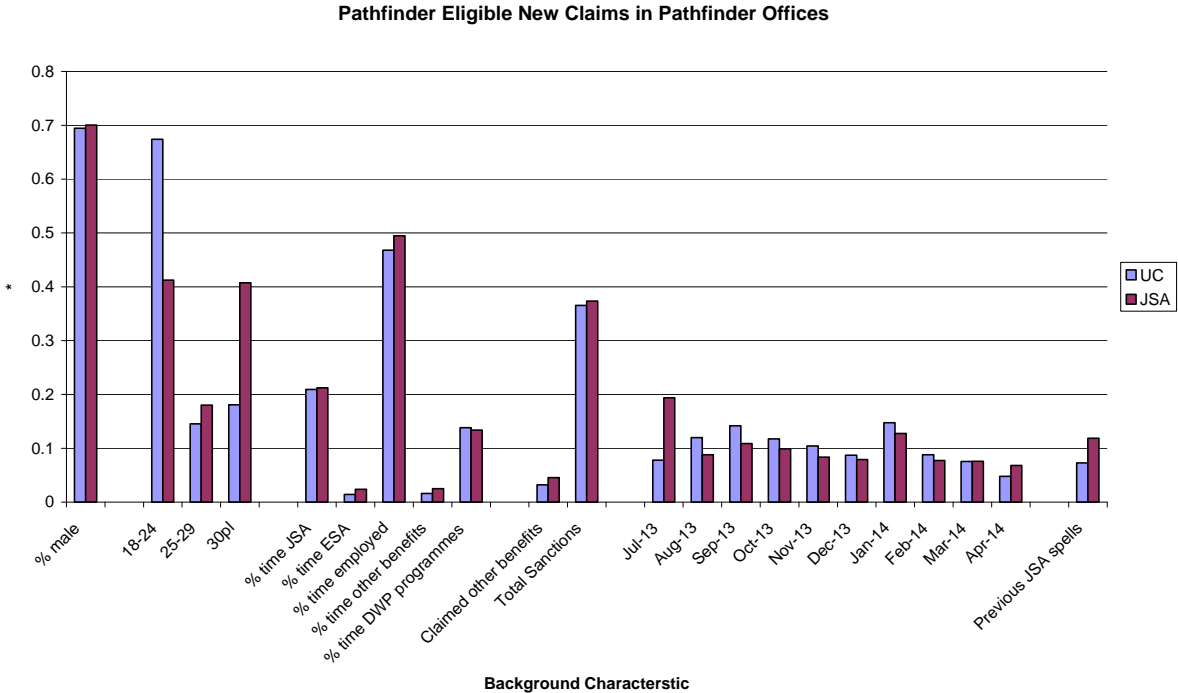
6.2   Results from matching

Table 2 shows the variables we use to match new JSA claims to the new UC claims. This matching is done for all first new UC claims between July 2013 and April 2014 in the original four Pathfinder offices using Pathfinder eligible new JSA claims in comparator offices.

In total we estimate a propensity score for nearly 108 thousand new claims of which nearly 6 thousand are in the treatment group. Figure 5 shows the overlap between the propensity scores of the UC and JSA groups. This is important. We only estimate the impact of UC on new claims that we can identify good matches in the comparison group. This means we only estimate the impact for new UC claims for whom we can identify a JSA claim with a similar propensity score (since a JSA claimant with a similar propensity score will be similar in terms of all the observables used to estimate the propensity score). This is the common support assumption. We have to have similar people claiming JSA at the same time to be able to estimate the impact on UC claimants.

The employment dummy variables are not jointly significant. Nevertheless, we retain them because doing so leads to a better balance on employment history than if we exclude them. Again, this highlights that the primary purpose of the model is to ensure the comparison sample is as similar as possible to the treatment group in terms of everything that we think might affect their outcomes. We know past employment helps predict the future employment of new claimants. Therefore, it is better to include past employment in the matching even if the unmatched samples look to have similar histories. This is particularly true because when we match on other characteristics and benefit history this could lead to the samples becoming less balanced in terms of their employment history.

Table 2 Matching variables

Variables Used to Select Comparison Group	Variables Used to Estimate Propensity Score
Single	Comparator office dummies for each Pathfinder Office
No Children	JSA claimant commitment in place/not in place at time of claim
No Housing Benefit	Age dummies
Not receiving support for Mortgage Interest	Gender
British	Gender/age interaction
No capital	Age recent JSA receipt interaction
Not homeless	Age recent ESA receipt interaction
Not claimed JSA or ESA during last 2 weeks	Age recent other benefit receipt interaction
In a comparator office	Age recent employment interaction
	Age recent sanction interaction
	Age recent employment programme particiaption interaction
	Gender JSA receipt interaction
	Gender ESA receipt interaction
	Gender other benefit receipt interaction
	Gender recent employment interaction
	Gender recent sanction interaction
	Gender recent programme participation interaction
	Month of claim
	Previous JSA spells July 13-April 14
	JSA receipt dummies for each week during last 2 years (102 dummy variables)
	ESA receipt dummies for each week during last 2 years (102 dummy variables)
	Employment status dummies for each week during last 2 years (104 dummy variables)
	JSA receipt dummies for each week during last 2 years (104 dummy variables)
	Received 1 high level sanction during last 2 years
	Received 2+ high level sanctions during last 2 years
	Received 1 intermediate level sanction during last 2 years
	Received 2+ intermediate level sanctions during last 2 years
	Received 1 low level sanction during last 2 years
	Received 2+ low level sanctions during last 2 years
	Total sanctions received last 2 years
	Sanction and JSA receipt interaction
	Sanction and ESA receipt interaction
	Sanction and employment status interaction
	Has participated in Work Choices, Flexible New Deal or Work Programme during 2 years prior to claim
	% of last two years spent on Work Choices &/or FND &/or WP
	No of spells on Work Choices &/or FND &/or WP
	Programme participation and JSA receipt interaction
	Programme participation and recent employment history interaction
	Claimed other benefits (other than JSA/ESA) during 2 years prior to claim
	% of time spent claiming other benefits during 2 years prior to claim
	No of benefit claims made to other benefits during 2 years prior to claim
	Other benefit claims and recent employment history interaction
	Other benefit claims and recent JSA receipt interaction
	Other benefit claims and recent ESA receipt interaction
	Other benefit claims and recent programme participation interaction

Figure 5 Common Support between the UC and JSA Groups
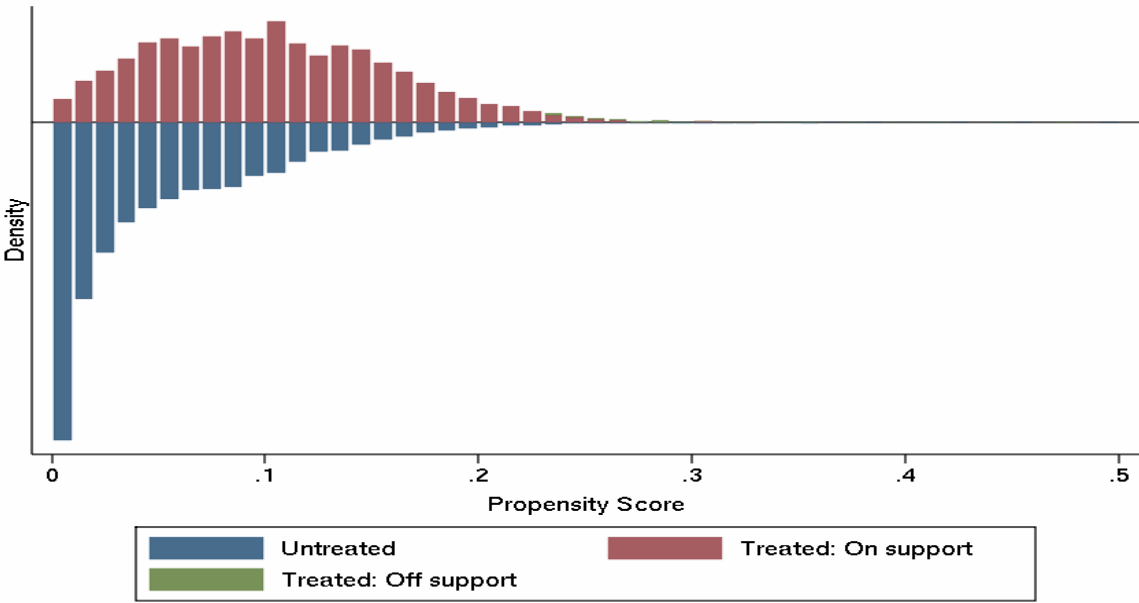

```
psmatch2: |   psmatch2: Common
 Treatment |        support
assignment | Off suppo  On suppor |    Total
-----------+----------------------+----------
 Untreated |      0    101,935 |  101,935
   Treated |     60      5,848 |    5,908
-----------+----------------------+----------
     Total |     60    107,783 |  107,843
```

Section 7.5 shows how effective the PSM methodology has been in balancing the groups on all the listed variables – this includes all 312 dummy variables that represent the detailed benefit and employment history of new claimants. There are no statistically significant differences between the UC and JSA groups in terms of any of the variables we have matched on. For example, whilst 18.2% of the unmatched treated sample were aged 30 or over compared with 30% of the unmatched comparison sample in the matched sample the proportions are 18.1% and 18.2% respectively. As the t-test shows, the matched samples are the same whilst, on this characteristic (and several others), the unmatched samples are significantly different from one another.

Figures 6 and 7 show how matching improves the comparability of the UC and JSA samples on a range of characteristics. Full details are in section 7.5.

The model to estimate the propensity score shows that the UC treatment group differs from the Pathfinder eligible JSA comparison group in terms of some of the variables we observe and which we think might affect their outcomes. Consequently, we would expect the relative difference in outcomes between the two groups to change when we focus only on matched individuals. That is, the unmatched results do not take into account the different age composition and employment and benefit history of the two groups. When we take these into account we see that the large difference in favour of UC reduces but remains positive.

Figure 6 Selected Characteristics of Unmatched Samples

Selected Characteristics of Unmatched UC and JSA Samples
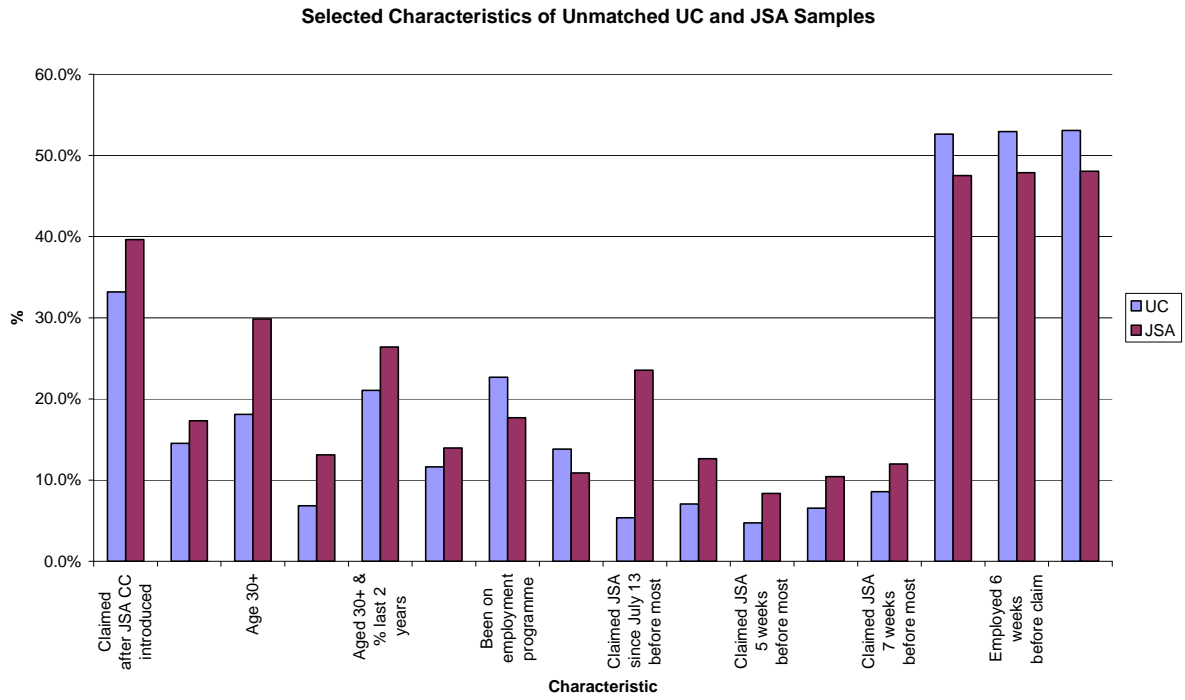
Figure 7 Selected Characteristics of Matched Samples

Selected Charactersitics of Matched Samples
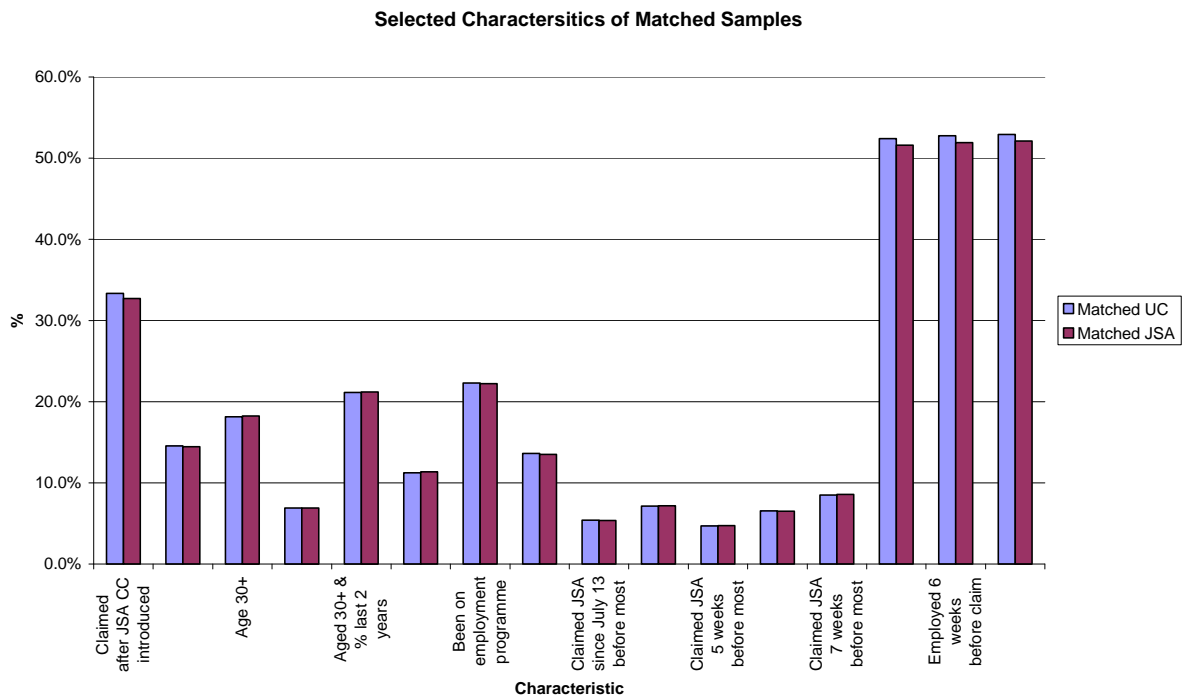
Figure 8 and 9 show the results for the matched samples. Figure 8 reports the percentage point impacts whilst figure 9 shows the overall proportion in work. The results suggest that new UC claimants are more likely than similar JSA claimants to be in work at different points in time after the start of their claim. For example, compared with similar people who made similar Pathfinder eligible new claims to JSA in similar offices during the same period UC new claimants are:

- 5 percentage points more likely to work during the first 120 days after the start of their claim; and
- 3-4 percentage points more likely to be employed 30, 60, 90 and 120 days after the start of their claim.

Figure 8 Percentage Point Difference in Probability of being in employed – (matched sample)

Percentage Point Difference in MATCHED Outcomes
1st New UC claims in Original Pathfinder Office and All JSA Pathfinder Eligible in Comparator Offices
July 13-April-14 New Claims
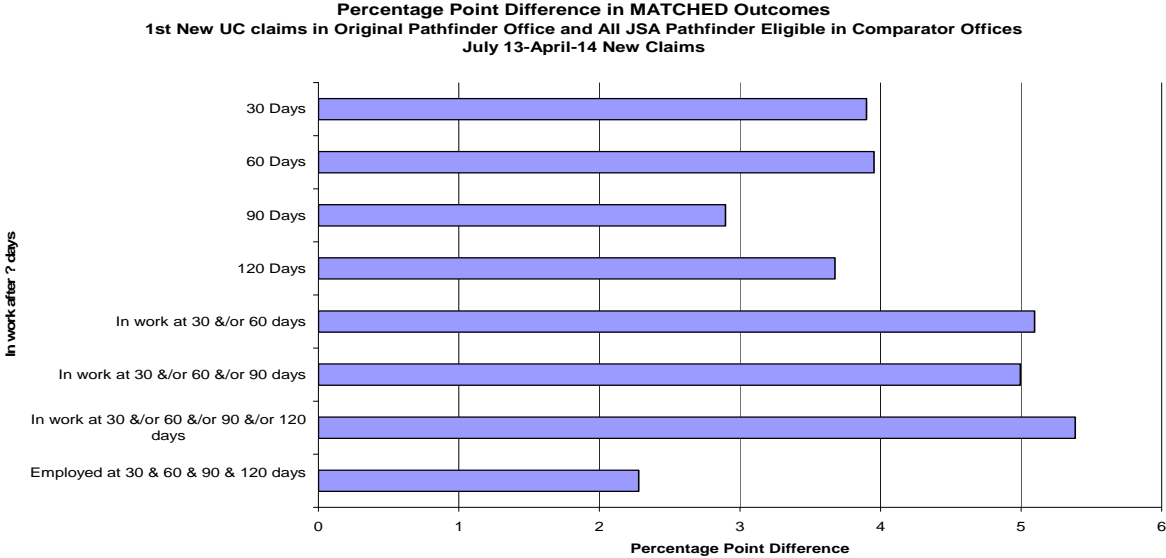
Figure 9 Difference in Proportion Employed (matched sample)

MATCHED Outcomes 1st New UC claim in Original 4 Pathfinder Offices and JSA Pathfinder Eligible in Comparator Offices July 13-April-14 New Claims
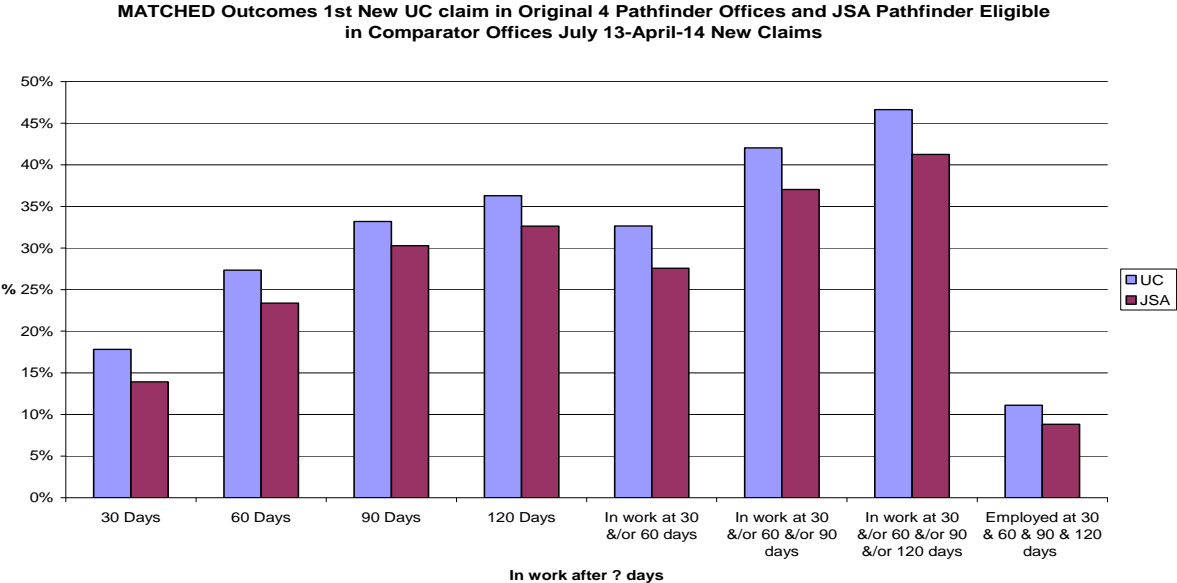
6.3 Impact on Other Outcomes

We can use the RTI data to derive more outcome measures so we can explore in more depth the impact that UC is having on people's labour market outcomes. Using the same matching method we estimate the impact of UC on days employed and earnings since the new claim was made. Figures 10 and 11 show the results. All the differences between the UC and JSA claimants are statistically significant at 5%. For example, during the first four months/120 days after the new claim, UC claimants, on average, spend 4 more days employed and earn about £50 gross earnings more than matched new Pathfinder eligible JSA claimants in comparator offices. It is important to note that we cannot estimate the impact of UC on hours worked or hourly wage rates as this information is not contained in the RTI.

Figure 10 Difference in Days Employed (matched sample)[19]

Days in Work Since Claim Started (Matched Estimates)

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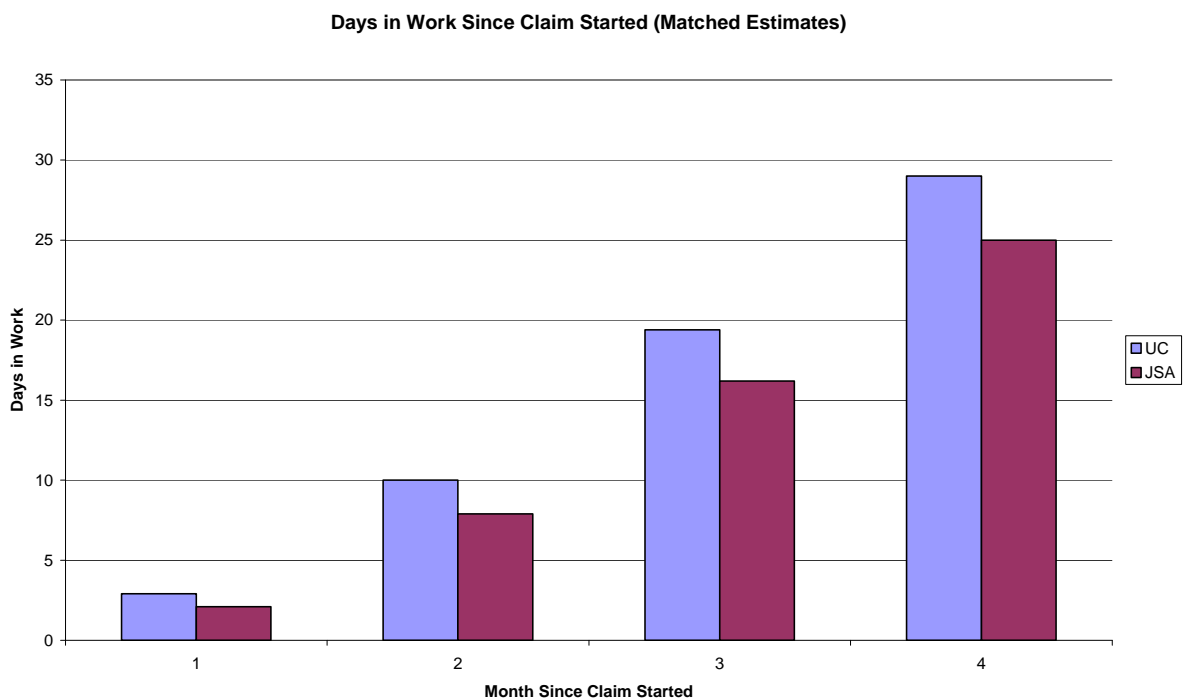
[19] 1 month is equal to 30 days in this analysis.

Figure 11 Difference in Earnings (matched sample)

Earnings in Work Since Claim Started (Matched Estimates)
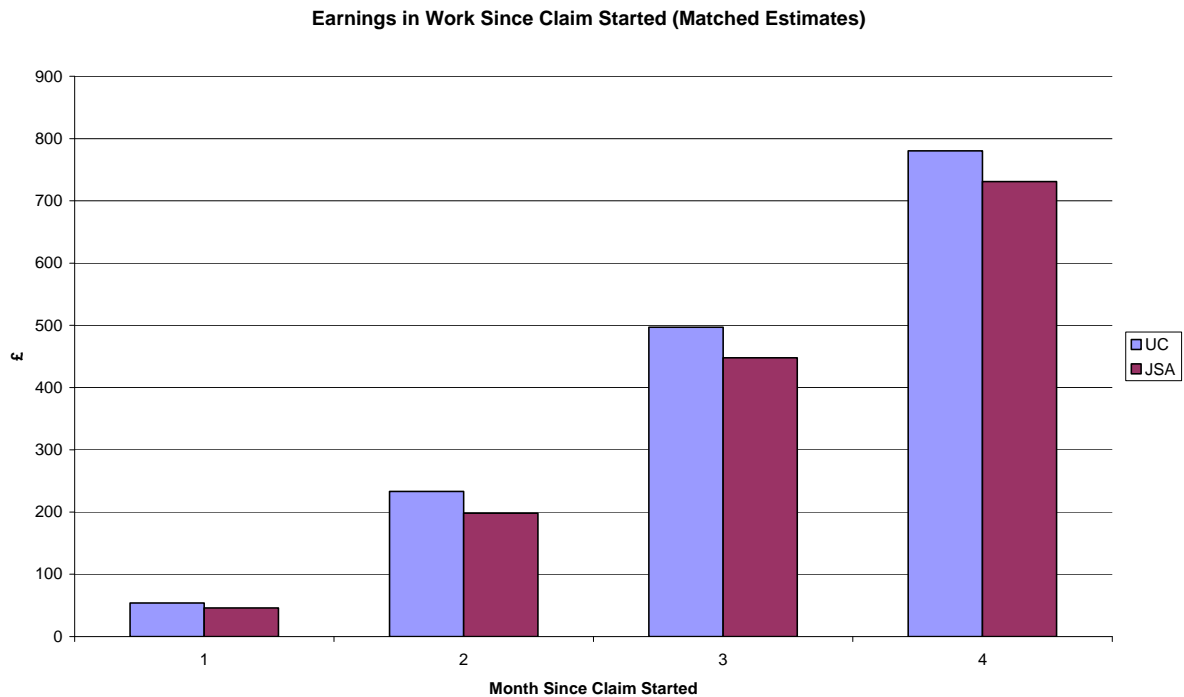
Figure 12 shows the percentage point impact on the probability of being employed in each week before and after the new claim was made. This uses a more granular measure of outcomes than the earlier analysis. We can also use historical data on the same outcome to adjust our impact estimates for any pre-existing difference between the UC treated group and the JSA comparison group.

Historical employment status is taken from the WPLS whilst after the new claim employment status comes from the RTI. We have already noted that the RTI should capture more employment spells than the WPLS. This analysis consequently assumes that the difference between WPLS and RTI is the same for the UC and JSA groups. Further work will explore this assumption and seek to ensure that we have consistent outcome data over time. This will become easier when we come to look at later cohorts of new claims for whom we will have some historical RTI data.

As section 7.5 shows there is no statistically significant difference in the employment status of the UC treatment group and the JSA comparison group in any individual week prior to the start of their claim. However, whilst none of the individual differences in section 7.5 are statistically significant the UC group has a consistently slightly higher probability of being employed prior to making their new claim. This difference between the UC and JSA groups looks reasonably stable over time. It suggests that the matching may not have completely controlled for all the factors that affect outcomes that differ between the treatment and comparison groups.

Since the pre-claim difference is stable over time it suggests that any factors that we might not be capturing in the matching model are not changing differentially between the two groups over time. Therefore, we can use difference-in-differences to take out these constant pre-treatment differences. Figure 12 deducts the average pre-difference in the probability of being employed shown in section 7.5 from the

difference post-treatment. It shows that prior to UC there was no difference in the probability of being employed (after differencing out the small pre-treatment difference) but that after UC there is a statistically significant difference between the two groups at 5%.[20] This suggests that the positive impact of UC on employment outcomes is robust.

Figure 12 Difference in Likelihood of being Employed Each Week (matched sample)

DiD Adjusted Impact on Likelihood of Being in Work

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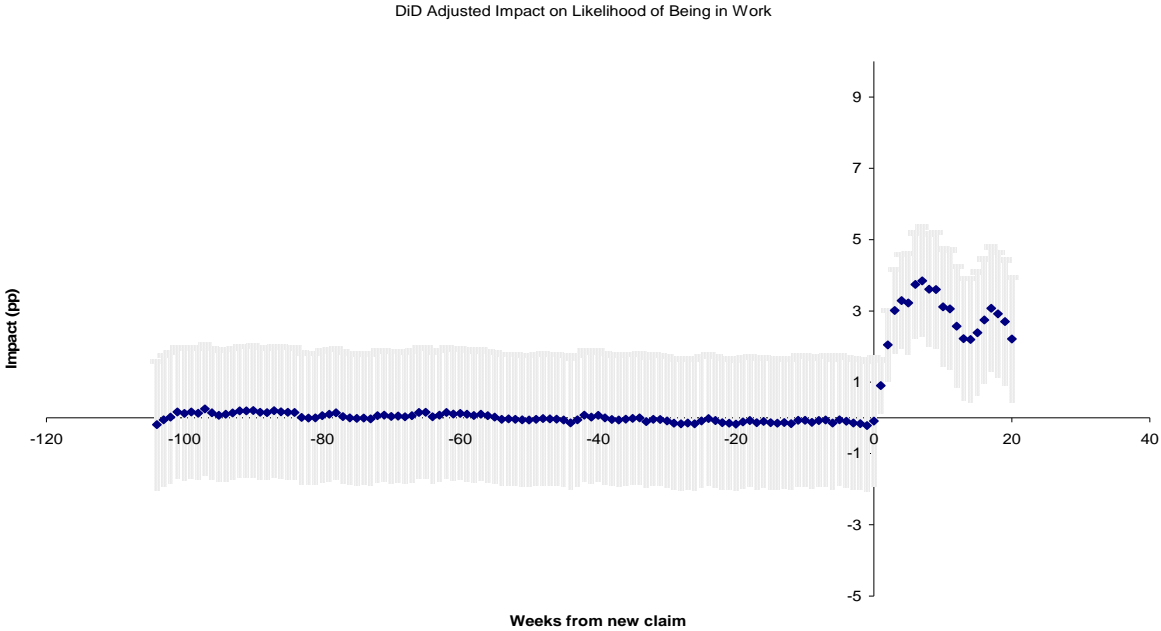
[20] Standard errors for figure 20 are calculated using a linear probability model. There is some debate in the literature as to the best way to calculate standard errors without being too computationally intensive. This model estimates the treated impact in each week before and after UC using the weighted matched sample. We use the standard errors from this model in figure 12. Elsewhere we base statistical significance on the t-values obtained from matching. This does not take into account the fact that we estimate the propensity score. However, where we have estimated standard errors using bootstrapping the resulting t-values are very similar, which is likely to reflect the large comparison group sample.

6.4 Sensitivity Analysis

We have conducted various supplementary analyses to check the extent to which the estimates vary under different assumptions and matching approaches. In particular we have estimated impacts:

- Using alternative comparator offices. So rather than using the ten best comparator offices from the approach described in annex A we choose the next best ten. This did not significantly change the results. This might reflect that including individuals' detailed labour market and benefit history captures local area factors. We also estimated impact separately for each office and their respective comparator offices and again the overall result was not significantly different;

- Using all new UC claims (not just in the Pathfinder offices) and all new Pathfinder eligible JSA claims (not just in the comparator offices). This did not lead to different estimates again suggesting perhaps that individuals' employment and benefit histories capture area differences reasonably well.

- Restricting analysis to October 2013-April 2014 new claims (using the original four Pathfinder offices and the comparator offices). The rationale for this was to explore whether the RTI roll-out might have had a differential impact on the measurement of outcomes between the UC and JSA groups. We have also looked at different sub-periods and the results are not significantly different between them.

- Including within the treatment group the Pathfinder eligible JSA group in the original four Pathfinder offices (the JSA residual). This tends to reduce the size of the impacts as we would expect but they remain positive and statistically significant.

   This estimate is hard to interpret. The treatment group includes people who do not receive the treatment, i.e. UC. The purpose of running this analysis reflects that we cannot accurately identify UC eligibility from the administrative data. Consequently, including those who we think are eligible in the treatment group even if they claim JSA should help ensure greater comparability between the treatment and comparison groups. This is what we found.

   The explanatory power of the treatment model when we include the residual JSA group in the treatment group is lower. This implies that the treatment group including the residual is more similar to the JSA comparison group. However, there is very little difference between the comparability of the matched samples when we include or exclude the residual group, which means all the background characteristics we include to match on help ensure the UC treatment group is comparable to the JSA comparison group.

   The fact that the impact estimate is lower when we include the JSA residual is not surprising given that around half of the treatment group do not receive the

treatment.

We found no difference between the outcomes of between the JSA residual group in the Pathfinder offices and a matched sample of Pathfinder eligible new JSA claims in comparator offices. This is encouraging and may suggest that the variables we use in the matching are able to capture the effects on outcomes of the unobservable characteristics that determine selection into UC.

If we confine the comparison group to Pathfinder eligible new JSA claims in the original four Pathfinder offices the explanatory power of the treatment model increases. This reflects the significant differences between those who claim UC and those who we think based on the administrative data are eligible for UC but claim JSA instead. The relatively small comparison group in this model (relative to that available for the main analysis) meant it was not possible to balance the two groups completely.

- Using different matching specifications – makes very little difference to the estimates. For example, figure 14 shows the result when we use the 5 nearest neighbours and a calliper of 0.00005. We also used Mahalanobis matching on the comparator offices combined with Kernel matching on the propensity score to exactly balance the comparator offices. These are just two of several alternative matching methods we have tried and none significantly alter the estimates we obtain. This is what other applications of PSM have found. The approach favoured in the main results has been chosen because it seems to achieve the best balance between the JSA and UC groups (and it has been used in the evaluation of other DWP programmes and has been externally peer reviewed).

- Bootstrapped standard errors for one or two isolated results to get better estimates of statistical significance (because the standard errors produced from the matching software don't take into account that the treatment model is estimated – though this doesn't necessarily mean they are under-estimated). The results find that the bootstrapped standard errors are not very different to those we get from the matching outputs. This reflects the large sample sizes.

- We estimated the treatment model using a simpler model with fewer explanatory variables. For example, rather than using 105 dummy variables to capture weekly JSA receipt during the two years prior to the most recent claim we only included dummy variables for the 16 weeks and then included variables to capture the proportion of time spent claiming JSA prior to that. We simplified the treatment of employment and ESA in a similar way. We experimented with a number of different specifications and none significantly altered the results. Our central estimate remains based on the full model as this achieves the best balance between the treatment and comparison group and because intuitively we know that the variables we include in the model are likely to influence future outcomes.

- We estimated the model separately by age and gender. The results were not significantly different between the different age groups. The impact estimate is slightly higher for males but the difference is not statistically significant. The estimate for women is more uncertain as the sample size is relatively small. To address this we also estimated the impact by gender using the whole treated

sample (not just those in the original four Pathfinder offices) and drawing on all Pathfinder eligible new JSA claims for the comparison group (i.e. not just those made in comparator offices). These estimates suggested an identical impact for men and women.

- Used alternative methods for counting employment spells using the RTI data. The RTI is a new data source and we are continually improving our understanding of the data and developing ways to use it effectively. We have estimated impacts using different iterations of the RTI outcome data based on different approaches to counting employment spells and estimating start and end dates. The changes made to the RTI methodology have not had any significant impact on the results.

- We estimated the impact of UC separately for new claims made before the JSA claimant commitment was in place in the office separately from new claims made after the JSA claimant commitment was introduced. This did not change the estimated impact significantly. This suggests that UC has an additional impact. However, it is difficult to interpret what this means for the impact of the JSA claimant commitment because when we look at the impact of UC before and after the JSA claimant commitment we are also comparing the impact of UC on earlier and later cohorts.

Figure 13 summarises the results of the sensitivity analyses we have carried out and how these affect the estimate on a particular outcome. It shows that the central estimate is robust to different methods and approaches. The only exception, as expected, is when the treatment group includes a large number of people who do not actually receive the treatment, i.e. in the case when we are not really estimating the impact of UC on those who receive the treatment but on a wider population some of whom receive UC and some of whom receive JSA.

Figure 13 Sensitivity Analysis

Employed at 30 &/or 60 &/or 90 &/or 120 days
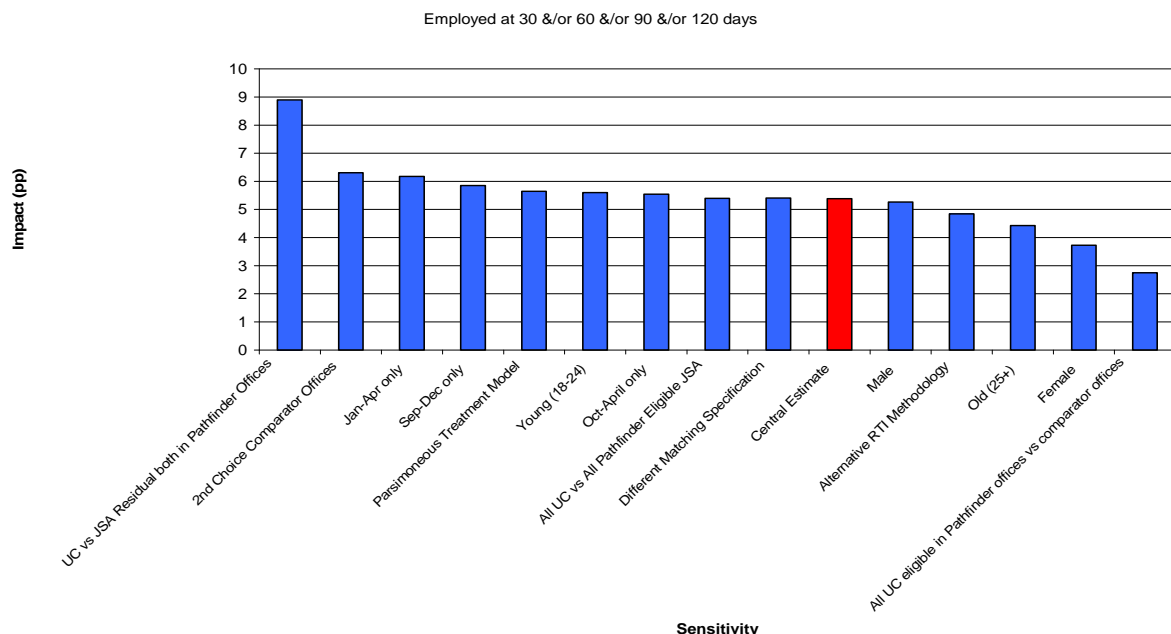
7   Annexes

7.1   Detail of Administrative Systems

7.1.1 Jobseekers Allowance Payment System (JSAPS) and the Atomic Data Store (ADS)

The JSA Atomic Data Store draws data from the JSA Payment System and Labour Market System (LMS) to create a single data source encompassing the claimant journey from claim submission through to termination for people who claim Jobseekers Allowance or Employment Support Allowance.

Key claimant characteristics such as ethnicity, date of birth etc. are available either within the Atomic Data Store itself or by merging in from existing analytical data sets such as LMS Client dataset.

7.1.2 Evidence Manager (EM) and Payment Manager (PMX)

The EM and PMX data sources form part of the larger data system known as 'UC Core' which is used to administer Universal Credit claims. The EM system provides information on the benefit components that claimants are entitled based on their families' circumstances. This is then passed to the PMX system to calculate the final payment to the claimant based on their earnings and any repayment arrangement, deductions, or sanctions that are in place.

7.1.3 Work Services Platform (WSP) and Labour Market System (LMS)

The LMS administrative systems are used by front-line staff to manage the conditionality regime for JSA claimants. For UC, WSP includes claimant details that are not used in the payment of benefit but are used to manage the conditionality regime and access to provision and support. This encompasses information on claimant disabilities, substance and alcohol dependency, homeless and vulnerable person markers and veteran status.

7.1.4 National Benefit Database (NBD) & the Work and Pensions Longitudinal Study (WPLS)

The National Benefit Database can be used to look at claims to key DWP benefits at an individual level. Data can be used to identify benefit caseloads and flows (on and off) over time, broken down by various claim and claimant characteristics. Data is sourced from 100% data scans of DWPs key benefit systems – JSAPS; ISCS; PSCS. The Work and Pensions Longitudinal Study (WPLS) was introduced in January 2004 (and enhanced in 2005) to link benefit and programme information held by DWP on its customers to employment records from HMRC.

Data-sharing provisions introduced in the Employment Act 2002 opened the way for DWP to receive further data on employment from HMRC and use the information for more purposes. DWP and HMRC have been working together to enable this data sharing to take place and to develop safeguards for the initiative.
https://www.gov.uk/government/statistics/work-and-pensions-longitudinal-study

7.1.5 Real Time Information

HMRC recently modernised the PAYE reporting process, with employers now required to notify them each time an employee is paid (See HMRC guidance on PAYE: https://www.gov.uk/business-tax/paye). This has replaced the previous annual process, and gives us an unprecedented ability to evaluate the employment outcomes for DWP claimants.

RTI effectively produces a stream of payslips that can be used to infer details about an employment. Employers send electronic returns to HMRC on or before the employee's actual payday containing details of payment date, gross and net earnings, Income Tax, National Insurance and pension contributions amongst others.

RTI began as a pilot scheme in 2012/13, and was rolled out nationally in 2013/14 with almost full coverage of PAYE schemes by October 2013. The RTI data effectively covers almost all employees in the UK, and over 99% of eligible employments are now submitting RTI to HMRC. There are, however, some workers who aren't covered by RTI:

- Self-employed people
- People in PAYE schemes where no member earns more than the Lower Earnings Limit in any given pay period within a tax year (thought to be very small numbers)

7.1.6 Single Housing Benefit Extract (SHBE)

SHBE contains the Local Authority (LA) Housing Benefit (HB) returns for Individual HB claimants. This is the most comprehensive administrative DWP dataset for HB and contains details about each Housing Benefit claim, and household characteristic information.

7.1.7 LMS client dataset

The LMS client dataset contains a historical record of the characteristics of Jobcentre Plus customers which are recorded in the Labour Market System (LMS).

7.1.8 Opportunities Database

The Opportunities Database contains records of referrals to employment programmes through Jobcentre Plus. It is used to identify whether claimants had previously participated in contracted employment programmes such as Work Programme, Work Choice, Flexible New Deal, etc.

7.2 Employment spells methodology

Periods when people are working, their employment spells, have been constructed from the RTI data. The RTI data itself only gives information about the payments reported for work done, the date of the payment and the amount paid. It does not specify directly the period in which the work was done and so this has been estimated using the methodology described below.

The RTI data also contains a number of incorrect entries, repeated entries and there are some payments which are not reported. Therefore, cleaning up of the data is required before analysis.

In the RTI data all the payments to an individual can be split down into payments from a particular employer and then for that employer all the payments under a particular payroll identifier. In building up to the employment spells this payroll identifier level is taken as the starting point but also includes low level payment information. The approach outlined below has been developed in consultation with analyst colleagues in DWP and HMRC.

7.2.1 Payroll spells

Having identified all the payments made to a person by an employer under a particular payroll identifier, the first step in the analysis is to determine the normal frequency of payment. This is most commonly either monthly or weekly but some are paid 4-weekly and some 2-weekly. The RTI data does indicate what the payment frequency is but the information is not always reliable and so this is combined with information on the actual time gaps between payments to determine this frequency.

The following example shows a case with a series of 5 individual payments and the time gaps between them. In this case the normal frequency can be seen to be weekly although it is not always exactly so.
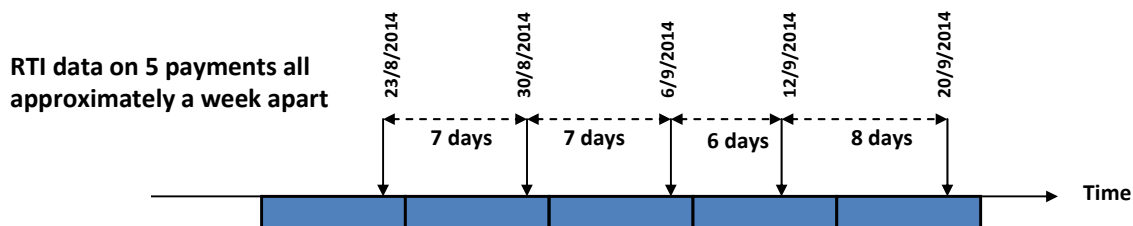
Having determined that the payments are weekly each individual payment has then been assigned to a period of time by assuming in this case that the weekly payments are paid towards the end of the week in which the work happened. Similarly for someone paid monthly the payment period would normally run from the first of the month to the last of the month in which the payment was made. Usually the actual payment occurs near the end of the month.

In this case the result is a continuous period of employment of 5 weeks. In other cases there can be longer gaps between payments. Here the information recorded in the RTI to indicate how many periods were covered by the payment can be used to determine whether there is a gap where no payment was made or if the payment period was longer than normal. In the example below of someone paid weekly, there are two gaps of 14 days. In the first case the payment is just for one period so the assumption is that there is a gap in payments for a week. In the second the payment made was for a two week period so there is no gap.
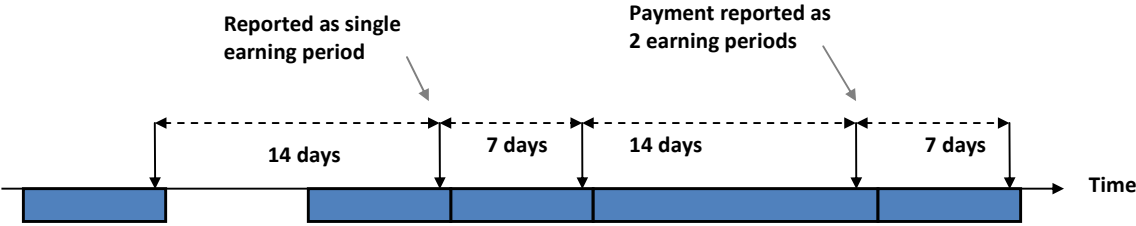
The work period for the first and last payments can be further adjusted to allow for people who may have started or ended work part way through. This matters most for those paid monthly, with an example shown in the following of 5 monthly payments recorded in RTI.
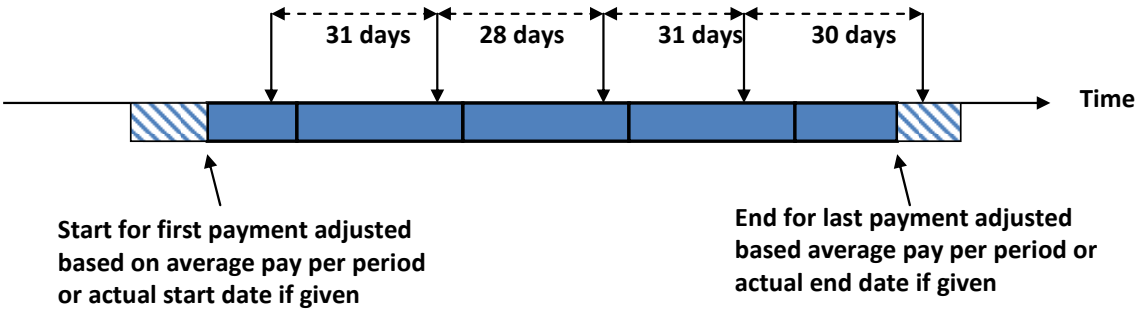
7.2.2   Creating employment spells

The result of the payrolls work is to convert a series of payment dates under a particular payroll identifier into one or more continuous spells of employment. However, the RTI data may contain late and missing payments and so the gaps found have been judged to represent continued employment as long as the gap is less than 4 weeks long. Above that there is assumed to have been a break in the employment and the person is assumed not to have been working for the whole of the gap.
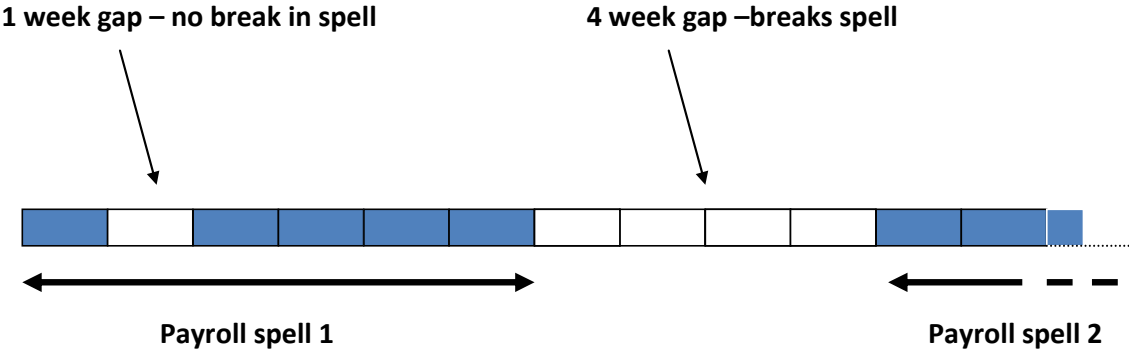
One person may have several spells of work for the same employer under different payroll identifiers. These have been combined to produce spells of work for that employer.
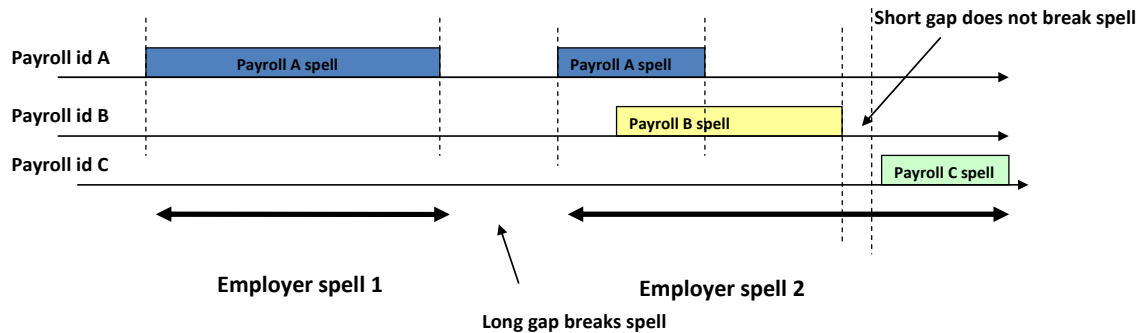
A person may have a number of employer spells. We combine these to produce overall employment spells for each person.

7.2.3 Confidence and quality rating

Each individual employment spell is rated to reflect the likely level of confidence we have in the quality of the data and the estimate. Employment spells are discarded if any of the following conditions apply:

- the total pay from the employment spell is less than £10;
- the taxable pay to date on the first RTI payment suggests more that one payment is missing;
- the difference between the estimated start date and the reported start date for the spell is more than 50 days;
- the difference between the estimated end date and the reported end date for the spell is more than 50 days.

In addition, the following filters are applied to ensure an appropriate alignment between each benefit spell and relevant work spell. Employment spells are discarded if any of the following conditions apply:

- the spell ends before or on the day of the benefit claim;
- the spell starts before the benefit claim and ends within 28 days of the benefit claim.

Having created employer spells, these have then been combined again to produce overall employment spells for that person using similar rules.

7.3 Explanation of method to derive comparator offices

7.3.1 Introduction

Our evaluation strategy relies primarily on the phased geographical roll-out of Universal Credit (UC). In particular, we construct a comparison group to estimate what would have happened to UC claimants in the absence of UC from areas where UC has yet to be introduced. As outlined in the main report, we want to ensure that people in the comparison group are the same in all relevant respects as the people who are claiming UC. Relevant means anything that might affect the labour market outcomes they achieve.

The labour market outcomes new claimants achieve depend on a range of factors including:
- Individual and household demographics. These factors also affect the type of benefit claim made;
- Labour market experience and past benefit history;
- Location, e.g. because of differences in local labour markets, policy environments, JCP office performance, etc.; and
- How we measure outcomes.

We want all these factors to be the same between the UC treatment group and the JSA comparison group so that the only difference between them that might lead to a difference in outcomes is UC. This annex describes how we try to ensure that our comparison group of new Pathfinder eligible JSA claims are in similar areas to the new UC claims made in the original four Pathfinder offices. That is, clearly the roll-out of UC and the labour market outcomes of new claimants both depend on geography. We want to identify areas that are as similar as possible to the Pathfinder areas so that any differences in outcomes between the UC and JSA groups is not due to differences between where the claims are made.

Pathfinder areas are defined at an office level. Therefore, we focus on identifying similar or comparator offices. We do the analysis separately for each of the four Pathfinder offices in turn. This reflects that the Pathfinder offices are different from one another. Therefore, a particular office might be a good comparator for one Pathfinder office but not the others. Therefore, if we consider the Pathfinder offices collectively we risk identifying an office that is very similar to a hypothetical combination of the Pathfinder offices but which is not similar to any one of the individual Pathfinder offices. Such a hypothetical office may consequently not be a fair comparator.

It is only factors that might affect the outcomes the new claimants achieve that we need to ensure are the same between the Pathfinder and comparator areas. Area level factors that affect outcomes include:

- Local labour market conditions including the number and composition of: i) vacancies and ii) other claimants (especially those competing for similar jobs); and
- The policy environment including JCP office performance.

We can only identify comparator offices using information <u>before</u> UC was introduced in the Pathfinder offices. This reflects that if UC does affect outcomes then we would expect it to change the local labour market in the Pathfinder areas.

We capture local area effects by analysing historical labour market outcomes at an office level. The rationale is that offices with similar local labour markets, similar volumes and types of claimants in similar policy environments will achieve similar outcomes over time.

It is important to focus on Pathfinder eligible new claims because they are a non-representative sub-set of JSA claimants. Their labour market outcomes are different from those of JSA claimants more generally. Moreover, some offices may do better or worse for this sub-group than they do for JSA claimants as a whole.

7.3.2 Data

Ideally we would use the same outcomes that we use to evaluate the impact of Pathfinder to identify comparator areas. However, we are evaluating Pathfinder using data from the Real Time Information (RTI) system, which is a new data source. We need historical information on outcomes to identify comparator offices.

The main outcome we consider is the JSA off-flow rate. This is highly correlated with employment outcomes. Consequently, we expect offices with historically similar off-flow rates for Pathfinder eligible new JSA claims to also have historically similar employment outcomes for the same group of claimants.

We use data from the Atomic Data Store (ADS). The data captures all "Pathfinder eligible" new JSA claims between April 2011 to April 2013. As with the main analysis we confine the analysis to claimants that, as far as we can tell from the administrative data, would have met all the eligibility criteria for UC under Pathfinder (except that they were claiming before UC was introduced in the Pathfinder offices and they could be claiming in any office). In particular, from ADS we identify those who are single, have no evidence of having children, have no capital, are not homeless, are not receiving SMI, have not claimed benefit during the last two weeks and for whom there is no evidence that they are non-British. We use SHBE to exclude people with a pre-existing Housing Benefit claim.

As well as the information on outcomes we include information about the number and composition of Pathfinder eligible new claimants including their detailed JSA and ESA claim history during the last two years, gender, age, marital status and address status.

7.3.3 Method

We want the comparator offices to have the same local conditions that the Pathfinder offices would have had if UC had not been introduced. So, our approach is analogous to that for identifying a comparison group of individuals. We want a comparator office to give an efficient (close as possible) and unbiased (right on average) estimate of what the local area conditions would have been like in the Pathfinder office in the absence of UC. This is more likely to be true if the comparator office's historical outcomes have been:
- the same on average as those achieved in the Pathfinder office;
- very close to those achieved in the Pathfinder office for each monthly cohort. Satisfying this particular criterion also means that the comparator office's outcomes would be following a similar pattern from month to month. That is, if in any month the rate or direction of change differed between the comparator and Pathfinder office then their off-flow rates would diverge;
- trending in the same direction and at a similar rate; and
- achieved in similar circumstances, i.e. in a similar policy environment and with a similar mix and volume of claimants.

Before we describe how we measure these, the first step we take is to exclude any outliers and any offices that are clearly very different to the Pathfinder office we are considering. Specifically, we exclude from the analysis any offices that are quite different to the Pathfinder office in terms of either:
- their recent off-flow rates,
- the variation in their recent off-flow rates; or
- the size of Pathfinder eligible claimants.

7.3.4 Similar Average Outcomes

We want the comparator office to have a similar average off-flow rate over time. This way the area level factors in the comparator office are more likely to provide an unbiased estimate (or right on average) of what the area level factors would have looked like in the Pathfinder office had UC not been introduced.

We estimate a simple fixed effects regression model which includes dummies for each:
- office – which control for average differences between offices that are constant throughout the period; and
- monthly cohort, which controls for trends in outcomes that are the same across all offices.

The estimated coefficient on each office's dummy in this model (when estimated without a constant) is the average off-flow rate for each office over the period. The offices with a coefficient closest to the Pathfinder office are consequently those with the most similar average off-flow rate.

It is clearly not sufficient to look at average outcomes. An office might have the same average outcome over the whole period but achieve very different outcomes a lot of the time. For example, figures (i) - (vi) illustrate how offices with the same average

outcome during the period might be very dissimilar a lot of the time. None of the potential comparator offices in these scenarios would provide a good guide to the sorts of outcomes the Pathfinder office A would achieve in future in the absence of UC. Consequently, they are unlikely to be a good proxy for local factors that might influence the outcomes of new claimants.

These scenarios show why it is important to consider how close the comparator office is to the Pathfinder office in each month and how its outcomes are trending as well as considering average outcomes. For example, in some cases the trend in outcomes differs only in the rate of change (scenario 3) whilst in others the trend differs both in direction and pace (scenario 4). In some scenarios the pattern is in the same direction but more exaggerated (scenario 2) and in others the pattern is in a different direction but of a similar size (scenario 5) and in scenario 6 the pattern is in a different direction and bigger. These scenarios are not exhaustive examples of how relying on average outcomes is insufficient.

Figure (i)

Comparing Offices - Same Average but Poor Comparators (Scenario 1)

Figure (ii)

Comparing Offices - Same Average but Poor Comparators (Scenario 2)

Figure (iii)

Comparing Offices - Same Average but Poor Comparators (Scenario 3)

Figure (iv)

Comparing Offices - Same Average but Poor Comparators (Scenario 4)

Figure (v)

Comparing Offices - Same Average but Poor Comparators (Scenario 5)

Figure (vi)

Comparing Offices - Same Average but Poor Comparators (Scenario 6)
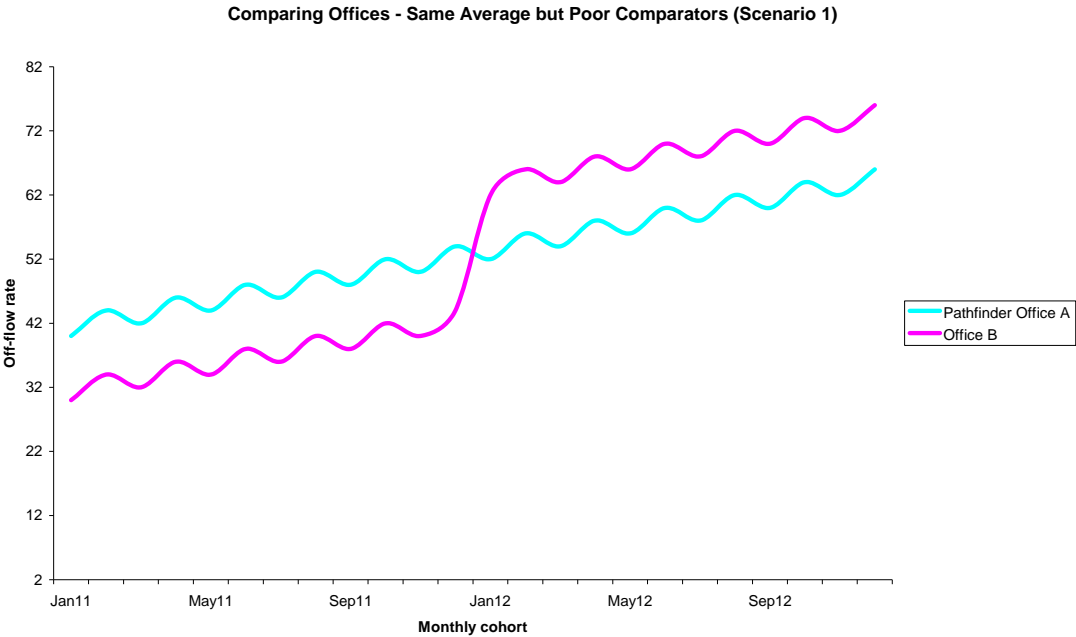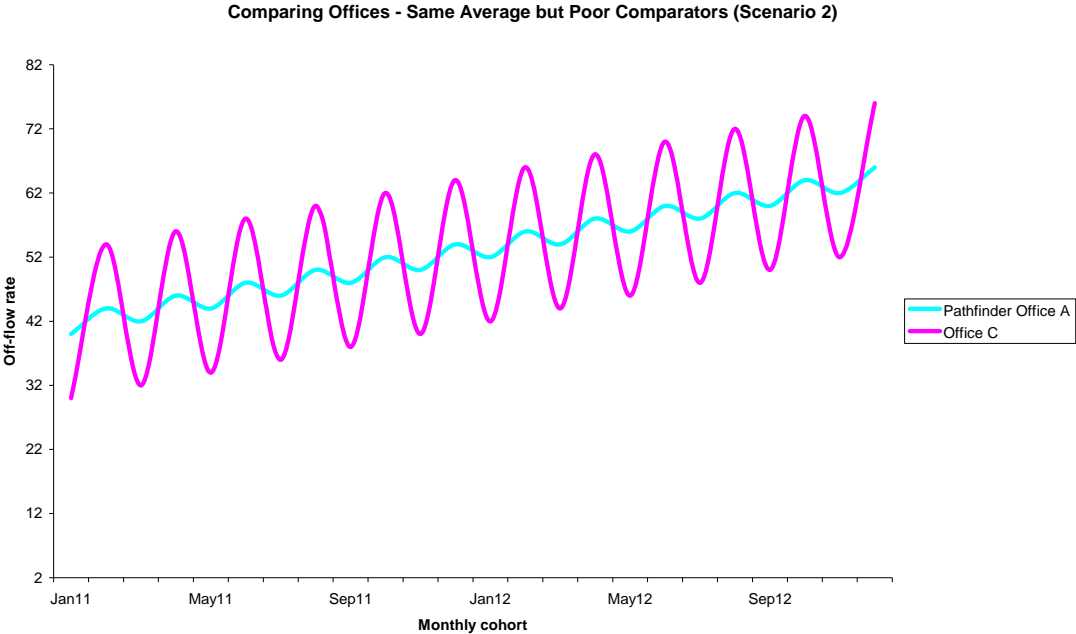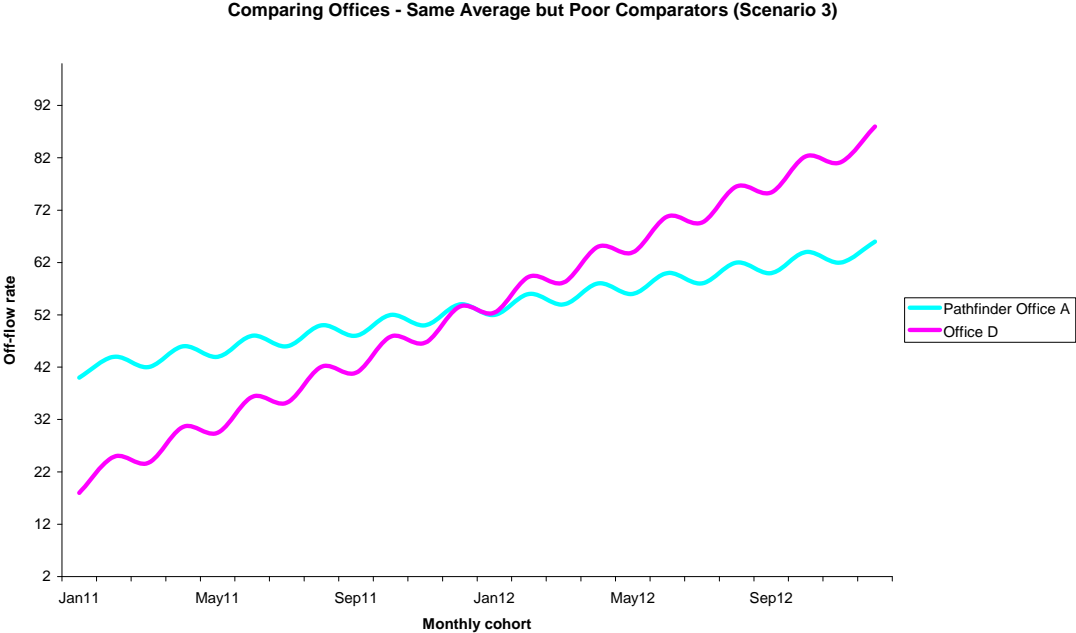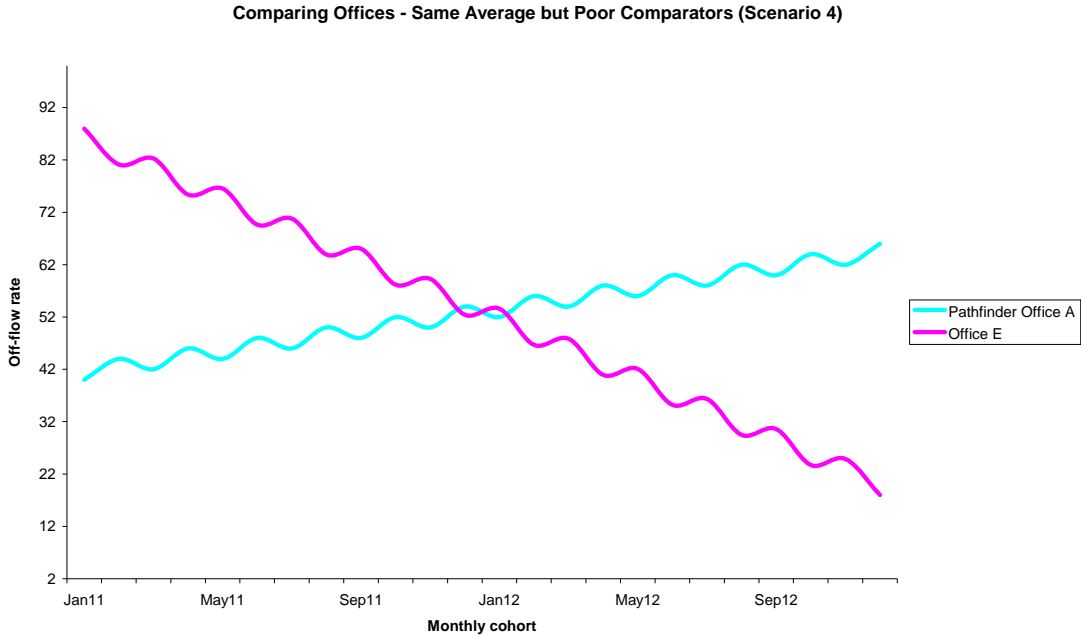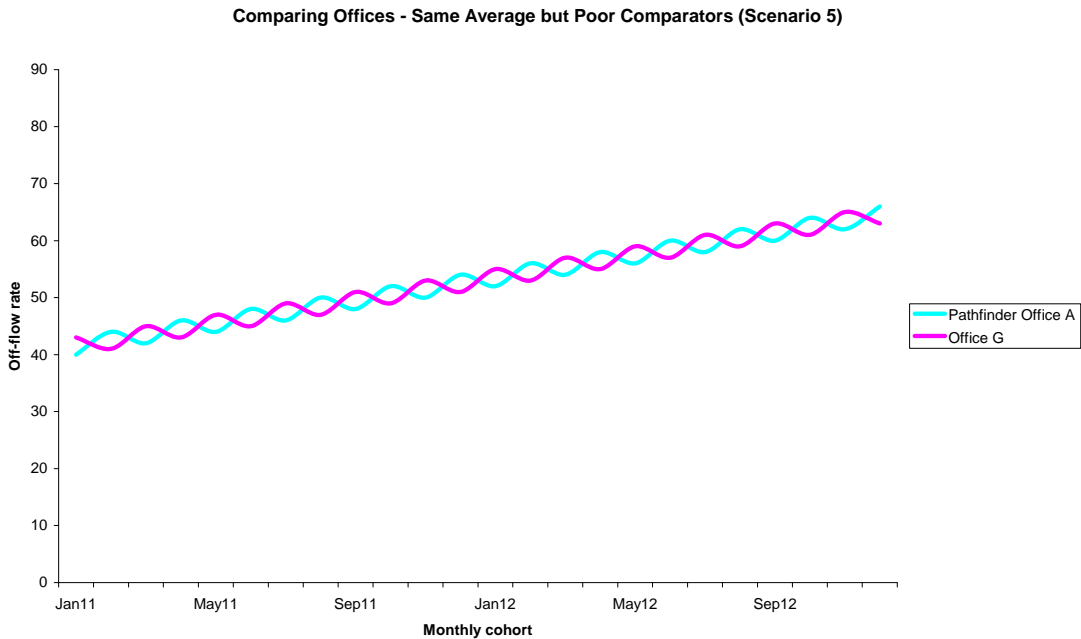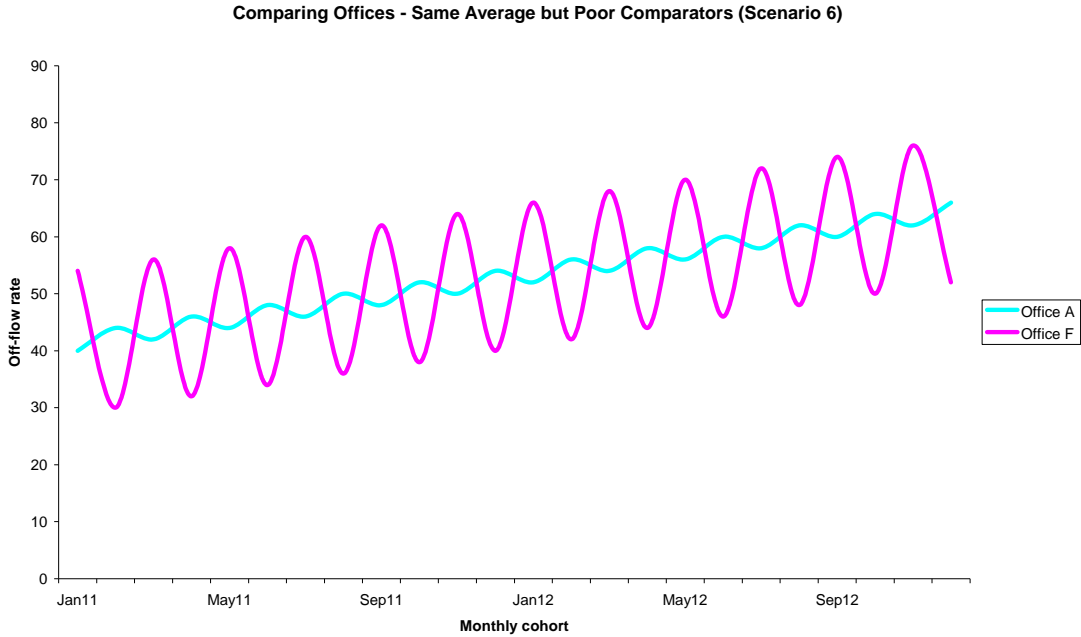
7.3.5 Similar Outcomes for Each Monthly On-Flow Cohort

We want the outcomes in the comparator office to be as close as possible for each monthly on-flow cohort to the Pathfinder office's outcome. This is about efficiency. We can measure how close each office's off-flow rate is in each month by calculating the squared difference between the Pathfinder outcome each month and that of each potential comparator office and then summing over all the months (sum of squared differences). The office with the lowest sum of squared differences is the one whose off-flow rates for each monthly cohort, across all the monthly cohorts, is most similar to the Pathfinder office.

This is about minimising the gap between the lines in charts Ai-Avi. In every case except scenario 5 there is considerable scope for finding an office that has outcomes closer to office A in each period than the potential comparator office – i.e. because there is quite a significant difference for nearly every monthly cohort even though on average their outcomes over the whole period are the same. We illustrate this just for scenario 2 below. Here office C and office Z both have the same average outcome over the period as the Pathfinder office but office Z would provide an estimate of the Pathfinder office's outcome that would be closer to the true value in each and every period and would consequently have a lower sum of squared differences.

Figure (vii)

Comparing Offices - Same Average but Office Z Has Closer Outcomes to the Pathfinder Office than Office B (Smaller sum of squared differences)
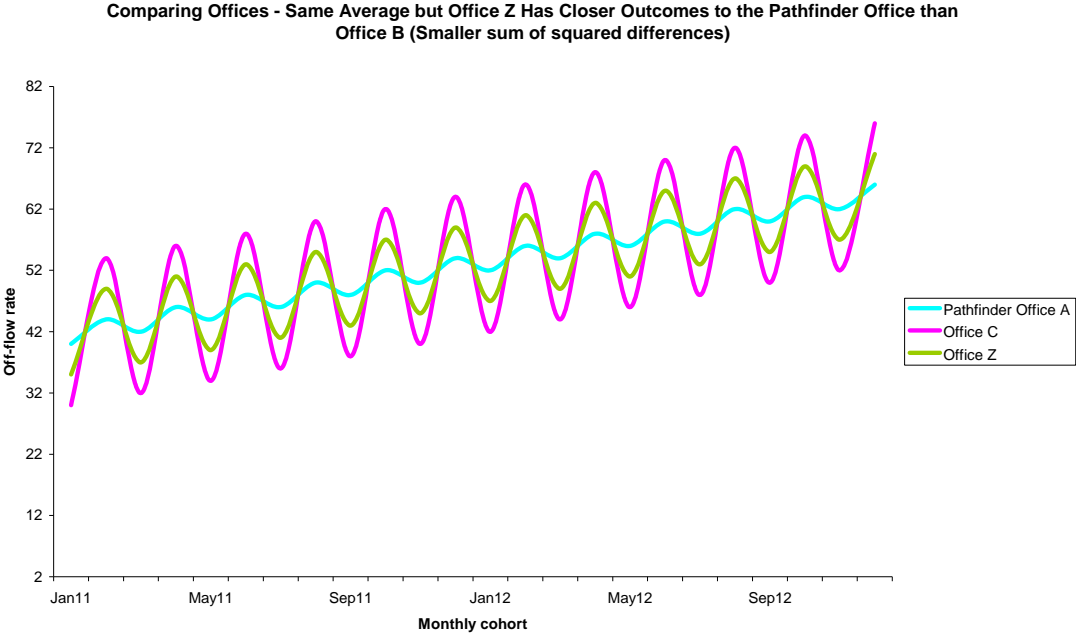
However, offices whose more recent off-flow rates are more similar to the Pathfinder office's are likely to provide a more reliable guide to what the local area would look like in the Pathfinder office in future compared with a comparator office that was more similar to the Pathfinder office 12-18 months ago. To reflect this we multiply the squared difference at each time point by a weight. This weight is determined by the time point that the difference is for. The weighting factor increases as the time point gets closer to the current time. As is convention all weights sum to one.

So, for example, in scenario 3 illustrated in figure (viii) below office Z would score more highly (have a lower weighted sum of squared differences) than office Y because its off-flow rate was more similar more recently whilst office Y was more similar to the Pathfinder office A during the first year.

Figure (viii)

Comparing Offices - Same Average.
Office Y Has Outcomes Closer to Pathfinder Office in More Recent Periods than Office Z
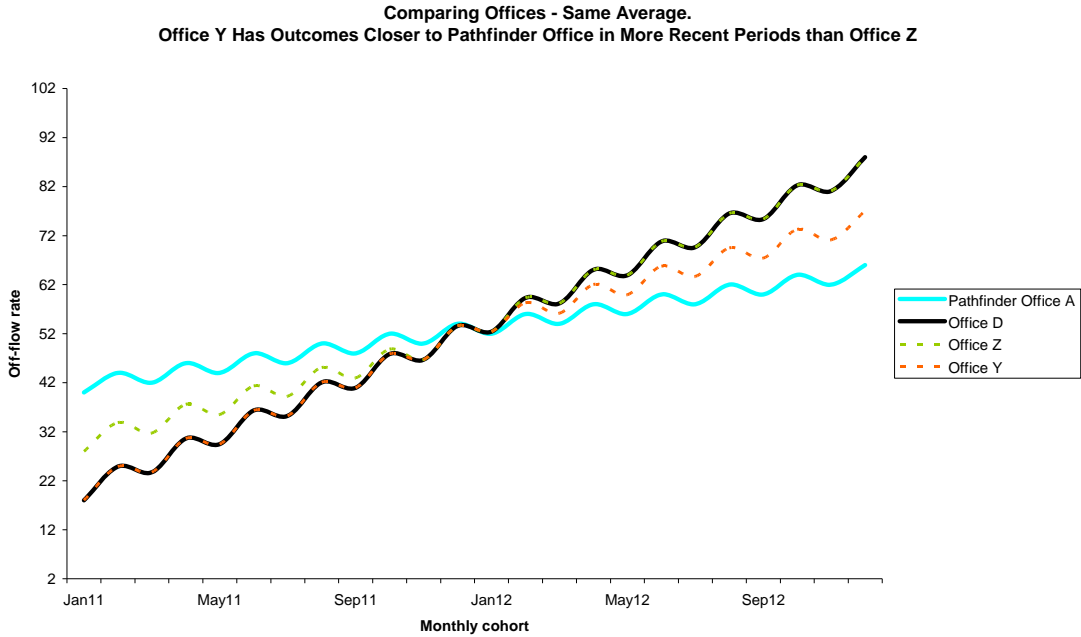
Minimising the sum of squared differences between the off-flow rate for each monthly cohort in the Pathfinder office and the comparator office also helps identify offices that share a similar pattern in their off-flow rates. For example, in figure (ix) the average difference between the off-flow rate in Pathfinder office A and the off-flow rate in offices B and C is the same (2 ½ percentage points). In this scenario sometimes office C has an outcome closer to office A and in alternate months office B has an outcome closer to office A. However, because office C follows a different monthly pattern when it is further away from A than B it is further away by a bigger margin (5pp). This gets captured in the sum of <u>squared</u> differences. In this illustrative example the sum of squared differences for office B compared with Pathfinder office A is 150 and this compares with 300 for office C.

Figure (ix)

Sum Of Squared Differences Captures Pattern in Outcomes
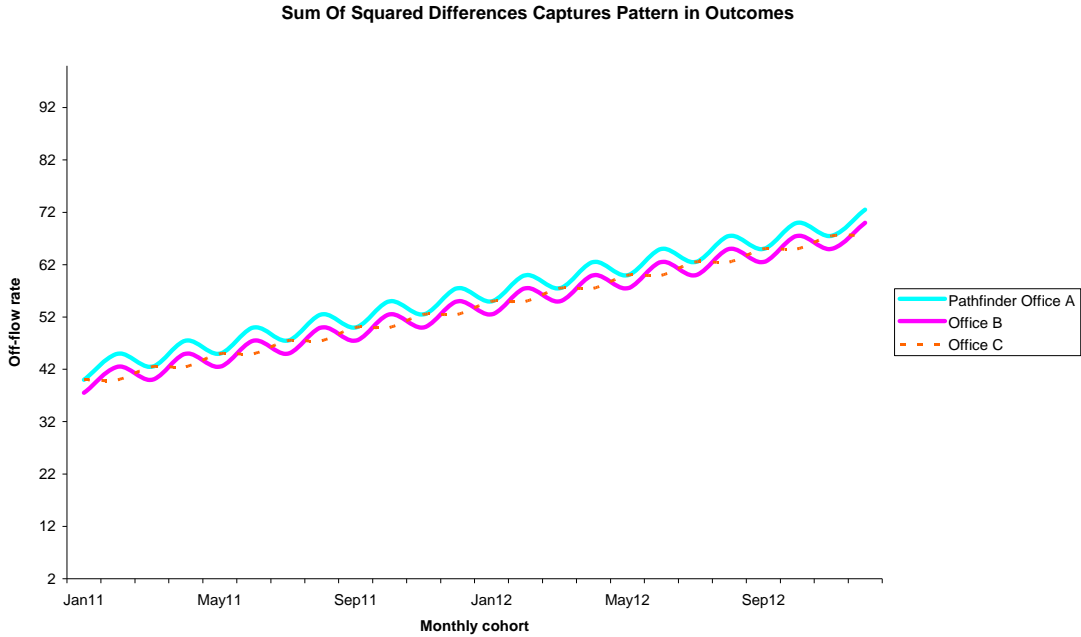
Using the mean adjusted sum of squared differences would also show B to be a better comparator than C in figure (ix). However, when we are interested in getting a good approximation of local area conditions for each and every monthly cohort the mean adjusted sum of squared differences does not help. For example, in figure (x) office B has a mean adjusted sum of squared differences of zero compared with office A. In contrast the mean adjusted sum of squared differences for office C relative to office A is 150. However, in this case the outcome in office C is always as close or closer to the off-flow in office A than B. The mean adjusted sum of squared differences is likely to be a much more useful measure when we start to look at difference-in-difference estimates where we want a constant difference between the Pathfinder office and its comparator offices.

Figure (x)

**Comparing Offices - Same Average but Poor Comparators (Scenario 10
Here B is always worse than C or the same - so zero mean adj ssd but not as good as B which has
lower SSD)**
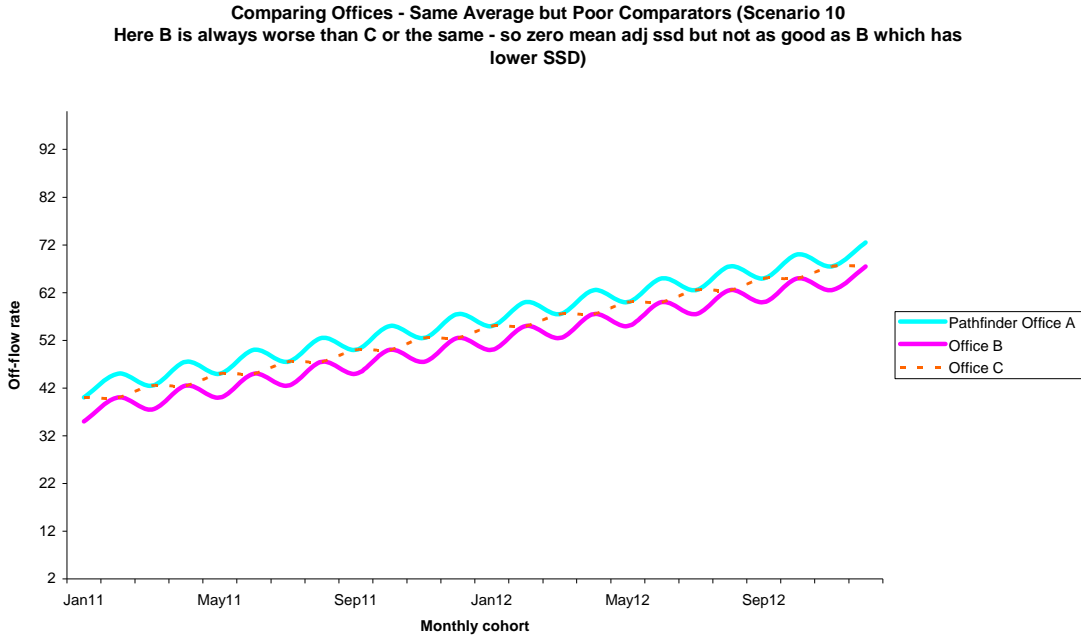
7.3.6 Similar Trend

We want the change in off-flow rates over the period to be similar in the comparator office. A comparator office whose outcomes (and therefore whose whose local circumstances) are trending in a different direction and/or at a different rate will provide an increasingly unreliable guide to how the local area factors would have changed in the Pathfinder office over time had UC not been introduced. For example, in scenario 9 in figure (xi) office Z has a trend closer to Pathfinder office A and so over time will produce a more reliable guide as to what might happen in office A than office D.

Figure (xi)

Comparing Offices - Same Average but Office Z has More Similar Trend in Outcomes to Pathfinder Office A
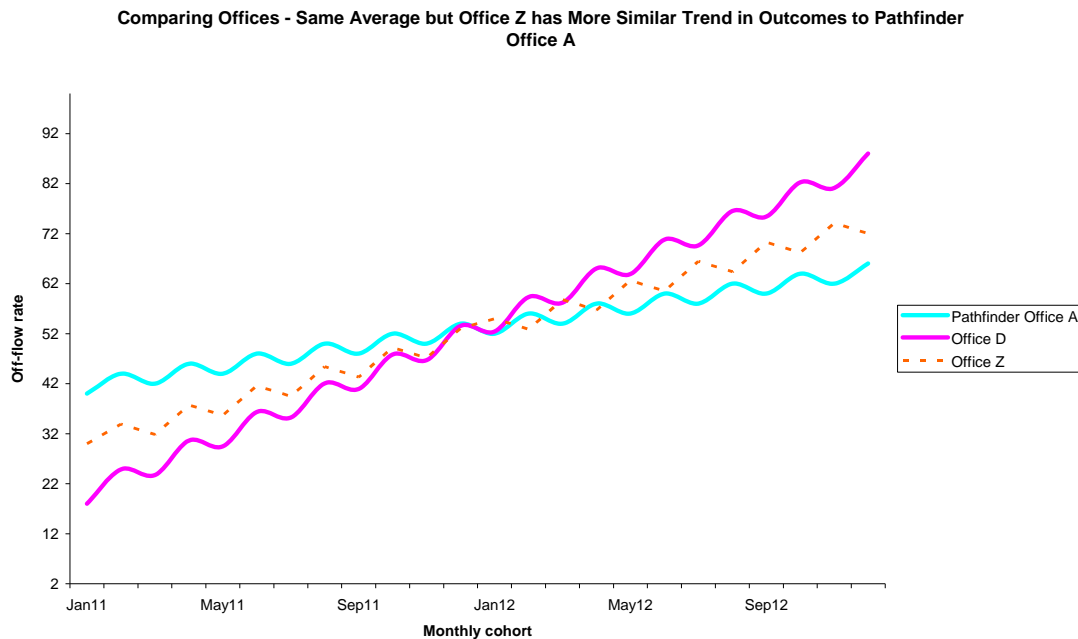
We estimate the average linear trend in off-flow rates over the period separately for each office and then see which offices have the most similar linear trend in outcomes over the two years from April 2011 to April 2013.

7.3.7 Similar Circumstances

The average outcome, sum of squared differences and linear trend in outcomes all focus exclusively on outcomes achieved at an office level. We want to ensure that offices are not achieving similar outcomes by chance. We want them to be genuinely similar. That is, we want them to be achieving very similar outcomes in similar circumstances. So, we want to make sure that things that might affect the outcomes an office achieves are similar between the comparator office and the Pathfinder office. We can control for residual differences in these factors at the analysis stage. However, the more similar offices are to begin with the more likely we will be able to detect impacts.

Our analysis focuses on the size and composition of claimants the offices are dealing with. This reflects that a lot of the variation over time and between offices is due to changes in the composition of claimants.

We match offices on past outcomes and a range of other factors that can affect the outcomes they achieve for Pathfinder eligible new claimants. In particular, we include the size and composition of Pathfinder eligible on-flows.

Trying to match on many factors makes it very difficult to identify good matches on all the criteria. We use Propensity Score Matching (PSM) to reduce this dimensionality problem. PSM estimates a propensity score for a Pathfinder office and each non-Pathfinder office. This score is the propensity to receive the treatment conditional on the observed variables. Rosenbaum and Rubin, 1993 showed that matching on a

single index representing the probability of treatment given the observed variables could achieve consistent estimates in the same way as if we matched on all variables. Identifying offices with propensity scores that are most similar to the Pathfinder office helps identify those offices that are most similar to the Pathfinder office across all the variables in the model, i.e. their past outcomes and the size and composition of their on-flows.

We use PSM here as a statistical tool to help identify offices that are most alike to the Pathfinder office in terms of a range of past outcomes and things that can influence the outcomes they achieve. The probit model includes dummies for each monthly on-flow, the age and gender composition of each monthly on-flow, the proportion of time on average spent claiming JSA during the last two years for those on-flowing each month and the number of monthly on-flows. We also include monthly on-flows squared.

We standardise the scores from each of these four measures so that they are given an equal weight before combining them to produce an overall score. For each Pathfinder office we identify and use the most similar offices and only identify matched individuals for each Pathfinder office from the new Pathfinder eligible JSA claimants in their respective comparator offices.

Figure (xii) illustrate how the 'best' comparator offices (shown in green) for Warrington look much more aligned in terms of their outcomes and how their outcomes are changing over time than the offices that score less well (shown in red). This is particularly true given that we exclude the offices that are very different from the analysis.

Figure (xii) also shows that it is impossible to identify offices that are very similar in terms of their past performance for each and every monthly cohort. Even the offices that appear similar may achieve quite different off-flow rates for particular monthly on-flow cohorts. This is likely to reflect changes in the relative composition of on-flows in particular months to some extent. However, it is one of the reasons for including a number of comparator offices for each Pathfinder office. This also mitigates the risk that offices that have been similar to a given Pathfinder office in the past may have started to diverge from it in the future (even if UC had not been introduced). Lastly, including multiple comparator offices for each Pathfinder office increases the pool of new Pathfinder eligible JSA claims that are available for a comparison group.

Table 1 lists the ten comparator offices used for each of the original four Pathfinder offices in the main model.

Figure (xii)

Warrington Good and Bad Comparator Offices
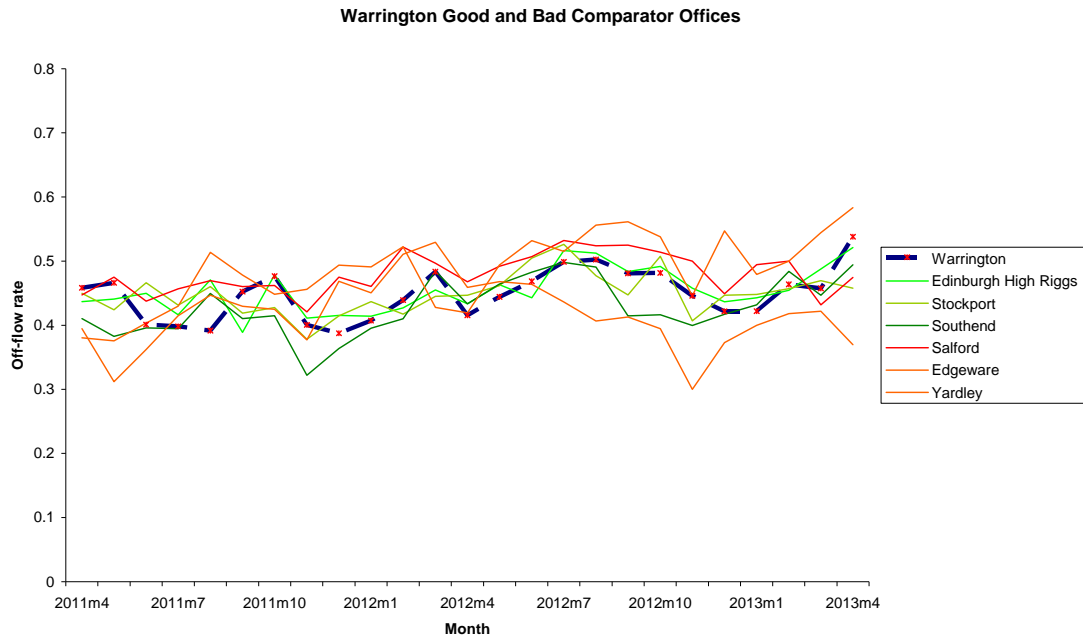
Table 1

	Ashton Under Lyne JC	Oldham JC	Warrington Nolan House JC	Wigan JC
1	Barrow JC	Worsley JC	Edinburgh High Riggs JC	Lincoln Orchard Street JC
2	Seacroft JC	Rochdale JC	Stockport JC	Sheffield Cavendish Court JC
3	Bury JC	Peckham JC	Southend JC	Barnsley JC
4	Cheetham JC	Cardiff Charles Street JC	Bedford JC	Newport Charles Street JC
5	Newcastle City JC	Nottingham Station Street JC	Mansfield Hillhouse JC	Rotherham JC
6	Longton JC	Beeston JC	Bexleyheath JC	Southend JC
7	Southport JC	Poplar JC	Streatham JC	Crossgate House Doncaster JC
8	Dalston JC	Kirkby St Chads JC	Scunthorpe JC	Dundee Wellgate JC
9	Longsight JC	Walsall Bayard House JC	Leicester New Walk JC	Leicester Wellington Street JC
10	Edmonton JC	Tottenham JC	Redbridge JC	Batley JC

7.4 Probit Treatment Model from Main Estimates: Using Original 4 Pathfinder Offices and Comparator Offices, July 13 to April 14

```
Probit regression, reporting marginal effects          Number of obs = 107843
                                                       Wald chi2(368)=      .
                                                       Prob > chi2  =      .
Log pseudolikelihood = -20133.326                      Pseudo R2    = 0.1209

                                      (Std. Err. adjusted for 93427 clusters in indid)
--------------------------------------------------------------------------------
             |              Robust
treato~c |      dF/dx    Std. Err.       z     P>|z|      x-bar  [    95% C.I.    ]
---------+----------------------------------------------------------------------
Ashton~c*|   -.1376956    .0021004        .        .    .170999  -.141812 -.133579
 OldCdec*|   -.2588372    .0036828   -234.31    0.000   .269679  -.266055 -.251619
WarrCdec*|   -.3770781    .0043107   -244.98    0.000   .317397  -.385527 -.368629
 WigCdec*|   -.2810832    .0037573   -242.28    0.000   .272248  -.288447 -.273719
   CCJSA*|   -.0205368     .001177    -17.04    0.000   .392895  -.022844  -.01823
 age2529*|   -.0230765    .0010357    -16.15    0.000   .171611  -.025106 -.021046
 age30pl*|    -.034383    .0013104    -22.36    0.000   .292175  -.036951 -.031815
oldgen~r*|   -.0064511    .0016251     -3.62    0.000   .127676  -.009636 -.003266
   oldjsa |    .0173851    .0053579      3.25    0.001   .099972   .006884  .027886
   oldesa |    .0582011    .0116608      4.99    0.000   .010112   .035346  .081056
otherb~d |    .0116435    .0075002      1.55    0.121   .008092  -.003057  .026344
   oldemp |    .0159579    .0021419      7.42    0.000   .26102     .01176  .020156
sancti~d |    .0003422    .0009812      0.35    0.727   .138303  -.001581  .002265
  progold |   -.0017128    .0039959     -0.43    0.668   .058073  -.009545  .006119
 agejsa10*|    .0058505    .0024151      2.55    0.011   .226793   .001117  .010584
 gender~1*|    -.002679    .0014515     -1.81    0.070   .315459  -.005524  .000166
 sancti~r |    .0014359    .0011615      1.24    0.216    .06095  -.000841  .003712
 jsagen~r |   -.0040739    .0050371     -0.81    0.419   .039536  -.013946  .005799
 esagen~r |    .0146804    .0117677      1.25    0.212   .003625  -.008384  .037745
 otherb~r |   -.0001351    .0073283     -0.02    0.985   .006191  -.014498  .014228
 empgen~r |     .000512    .0022277      0.23    0.818   .149147  -.003854  .004878
 progge~r |    .0058051    .0046806      1.24    0.215    .01967  -.003369  .014979
 monst~m4*|   -.0201288    .0012204    -11.60    0.000   .123457  -.022521 -.017737
 monst~m5*|   -.0064394    .0019647     -2.97    0.003   .100906   -.01029 -.002589
 monst~m6*|   -.0079538    .0018606     -3.81    0.000   .124208    -.0116 -.004307
 monsta~7*|   -.0073409    .0019123     -3.43    0.001   .103688  -.011089 -.003593
 monsta~8*|   -.0031251    .0021133     -1.41    0.158   .093423  -.007267  .001017
 monsta~9*|    .0014838    .0022877      0.66    0.508    .08517      -.003  .005968
 monst~10*|    .0050006    .0022408      2.37    0.018   .131895   .000609  .009393
 monst~11*|    .0065087    .0025236      2.80    0.005   .084586   .001563  .011455
 monst~12*|     .006697    .0025543      2.85    0.004   .080784   .001691  .011703
 prspel~1*|   -.0334996    .0007313    -35.95    0.000   .225448  -.034933 -.032066
 prspel~2 |   -.0378189    .0018166    -18.95    0.000   .120397  -.041379 -.034258
 jsa_w~_3*|   -.0213501    .0012743     -7.25    0.000   .028291  -.023848 -.018853
 jsa_w~_4*|   -.0055938    .0029057     -1.75    0.081   .054885  -.011289  .000101
 jsa_w~_5*|    .0038555    .0037052      1.10    0.273   .081656  -.003406  .011117
 jsa_w~_6*|   -.0054439    .0029564     -1.70    0.090   .102158  -.011238  .000351
 jsa_w~_7*|    .0010504    .0036188      0.29    0.769   .118098  -.006042  .008143
 jsa_w~_8*|    .0012337    .0034606      0.36    0.717   .132943  -.005549  .008016
 jsa_w~_9*|    .0037864    .0038058      1.04    0.299   .143857  -.003673  .011246
 jsa_w~10*|   -.0025036    .0034217     -0.71    0.478   .151776    -.00921  .004203
 jsa_w~11*|    .0038118    .0037889      1.05    0.294   .160224  -.003614  .011238
 jsa_w~12*|   -.0011633    .0033588     -0.34    0.733   .167475  -.007746   .00542
 jsa_w~13*|    .0000159    .0035353      0.00    0.996   .174847  -.006913  .006945
 jsa_w~14*|   -.0013928    .0034643     -0.40    0.692   .181078  -.008183  .005397
 jsa_w~15*|    .0061378    .0041994      1.55    0.120   .185242  -.002093  .014368
 jsa_w~16*|   -.0017949    .0033831     -0.52    0.603   .189767  -.008426  .004836
 jsa_w~17*|    .0006279    .0034701      0.18    0.855   .192864  -.006173  .007429
 jsa_w~18*|   -.0021683    .0034216     -0.62    0.536   .195905  -.008874  .004538
 jsa_w~19*|   -.0006288    .0036397     -0.17    0.864   .197936  -.007762  .006505
 jsa_w~20*|    .0011914    .0037081      0.33    0.745   .200977  -.006076  .008459
```

```
jsa_w~21*|   -.002371    .0034718    -0.67   0.505     .20375    -.009176    .004434
jsa_w~22*|    .000524     .003755     0.14   0.888    .206244    -.006836    .007884
jsa_w~23*|   .0015056    .0038482     0.40   0.691    .207663    -.006037    .009048
jsa_w~24*|   .0028602    .0038281     0.77   0.442     .20885    -.004643    .010363
jsa_w~25*|  -.0057926    .0031571    -1.73   0.084    .210844     -.01198    .000395
jsa_w~26*|   .0087478    .0042654     2.22   0.026    .212262     .000388    .017108
jsa_w~27*|   .0008278    .0037848     0.22   0.825    .212874     -.00659    .008246
jsa_w~28*|   -.002071    .0036217    -0.56   0.575    .213839     -.00917    .005027
jsa_w~29*|  -.0014489    .0035878    -0.40   0.691    .215063    -.008481    .005583
jsa_w~30*|   .0024191    .0038578     0.64   0.521    .215693    -.005142     .00998
jsa_w~31*|   .0029264    .0040907     0.74   0.462     .21509    -.005091    .010944
jsa_w~32*|  -.0033875    .0036151    -0.91   0.365    .215406    -.010473    .003698
jsa_w~33*|  -.0028659    .0035494    -0.78   0.433    .216426    -.009823    .004091
jsa_w~34*|   .0028254    .0039873     0.73   0.467    .217149    -.004989     .01064
jsa_w~35*|   .0053402    .0041145     1.36   0.172    .217863    -.002724    .013404
jsa_w~36*|  -.0035261    .0034143    -1.00   0.319    .217863    -.010218    .003166
jsa_w~37*|   .0006648    .0037231     0.18   0.857    .217121    -.006632    .007962
jsa_w~38*|   .0046328    .0040066     1.21   0.227    .216129     -.00322    .012486
jsa_w~39*|  -.0013304    .0036727    -0.36   0.721     .21663    -.008529    .005868
jsa_w~40*|   -.004596    .0033821    -1.30   0.195    .216407    -.011225    .002033
jsa_w~41*|   .0031837    .0038388     0.85   0.393    .217001     -.00434    .010708
jsa_w~42*|    .000017    .0037171     0.00   0.996    .217807    -.007268    .007302
jsa_w~43*|  -.0000485     .003727    -0.01   0.990    .218113    -.007353    .007256
jsa_w~44*|  -.0012366    .0034717    -0.35   0.725    .217205    -.008041    .005568
jsa_w~45*|  -.0039908     .003468    -1.10   0.269    .217214    -.010788    .002806
jsa_w~46*|   .0004334    .0037463     0.12   0.908    .216815    -.006909    .007776
jsa_w~47*|   .0033009    .0038739     0.88   0.379    .217103    -.004292    .010894
jsa_w~48*|  -.0008498    .0035648    -0.24   0.813    .216658    -.007837    .006137
jsa_w~49*|   -.000532    .0036257    -0.15   0.884    .216305    -.007638    .006574
jsa_w~50*|   .0042609    .0039393     1.13   0.260    .215267     -.00346    .011982
jsa_w~51*|  -.0005533     .003556    -0.15   0.877    .212976    -.007523    .006416
jsa_w~52*|  -.0017674    .0034508    -0.50   0.615    .210352    -.008531    .004996
jsa_w~53*|  -.0007574    .0034813    -0.22   0.829     .20732    -.007581    .006066
jsa_w~54*|  -.0009039    .0035884    -0.25   0.803    .205382    -.007937    .006129
jsa_w~55*|   .0033066    .0039167     0.87   0.383    .203759     -.00437    .010983
jsa_w~56*|  -.0035902    .0034706    -1.00   0.319    .203073    -.010393    .003212
jsa_w~57*|  -.0030612    .0036227    -0.82   0.413    .202628    -.010162    .004039
jsa_w~58*|   .0042564    .0041314     1.07   0.283    .202072    -.003841    .012354
jsa_w~59*|  -.0006077    .0036758    -0.16   0.870    .201543    -.007812    .006597
jsa_w~60*|   .0054864    .0041337     1.40   0.161    .200032    -.002616    .013588
jsa_w~61*|  -.0004916    .0036642    -0.13   0.894    .199475    -.007673     .00669
jsa_w~62*|   .0000232     .003813     0.01   0.995    .199503     -.00745    .007496
jsa_w~63*|  -.0050255      .00359    -1.33   0.185    .199373    -.012062    .002011
jsa_w~64*|   .0044738    .0042014     1.11   0.266    .199392    -.003761    .012708
jsa_w~65*|   -.002637    .0036119    -0.71   0.478    .199401    -.009716    .004442
jsa_w~66*|   .0015418    .0040025     0.39   0.696    .199373    -.006303    .009387
jsa_w~67*|   .0069462    .0043583     1.70   0.088    .198594    -.001596    .015488
jsa_w~68*|   -.006485    .0033507    -1.80   0.072    .198019    -.013052    .000082
jsa_w~69*|  -.0002148    .0037614    -0.06   0.955    .198075    -.007587    .007157
jsa_w~70*|  -.0005958    .0037484    -0.16   0.874    .197565    -.007942    .006751
jsa_w~71*|   .0018799    .0041625     0.46   0.645    .198724    -.006278    .010038
jsa_w~72*|  -.0005298    .0038368    -0.14   0.891    .198947     -.00805     .00699
jsa_w~73*|  -.0019597     .003629    -0.53   0.597     .19878    -.009072    .005153
jsa_w~74*|  -.0008957    .0037592    -0.24   0.813    .199067    -.008263    .006472
jsa_w~75*|   .0088067    .0047538     2.01   0.044    .199243    -.000511    .018124
jsa_w~76*|   .0001886    .0039814     0.05   0.962    .199364    -.007615    .007992
jsa_w~77*|  -.0028073     .003556    -0.77   0.444    .198956    -.009777    .004162
jsa_w~78*|   .0006631    .0040437     0.17   0.869     .19878    -.007262    .008589
jsa_w~79*|    .000793    .0042203     0.19   0.850    .198882    -.007479    .009065
jsa_w~80*|  -.0030809    .0038155    -0.78   0.435    .198557    -.010559    .004397
```

jsa_w~81*	.000191	.0041109	0.05	0.963	.19877	-.007866	.008248
jsa_w~82*	.0048613	.0045714	1.12	0.264	.198604	-.004098	.013821
jsa_w~83*	-.0048732	.0037639	-1.23	0.220	.197982	-.01225	.002504
jsa_w~84*	-.0075108	.0035202	-1.96	0.050	.197593	-.01441	-.000611
jsa_w~85*	.0122792	.0051336	2.68	0.007	.196768	.002218	.022341
jsa_w~86*	-.00983	.0032206	-2.72	0.006	.197203	-.016142	-.003518
jsa_w~87*	.0129952	.0052802	2.77	0.006	.196387	.002646	.023344
jsa_w~88*	-.008535	.0034314	-2.25	0.024	.196489	-.01526	-.00181
jsa_w~89*	.0055114	.0047747	1.22	0.223	.196693	-.003847	.01487
jsa_w~90*	.0056253	.0046829	1.27	0.204	.196675	-.003553	.014804
jsa_w~91*	-.0039779	.0037692	-1.01	0.312	.196341	-.011365	.00341
jsa_w~92*	-.0002651	.0041481	-0.06	0.949	.196109	-.008395	.007865
jsa_w~93*	.0026314	.0045447	0.59	0.552	.195748	-.006276	.011539
jsa_w~94*	-.0046447	.0039338	-1.12	0.262	.19572	-.012355	.003065
jsa_w~95*	-.0006581	.004176	-0.16	0.876	.195423	-.008843	.007527
jsa_w~96*	.0034652	.0044169	0.81	0.416	.194932	-.005192	.012122
jsa_w~97*	.0007598	.0041124	0.19	0.852	.194635	-.0073	.00882
jsa_w~98*	.0054763	.0046962	1.23	0.218	.194218	-.003728	.014681
jsa_w~99*	-.0024302	.0041894	-0.56	0.572	.193077	-.010641	.005781
jsa_~100*	-.001311	.0042007	-0.31	0.758	.192632	-.009544	.006922
jsa_~101*	-.0015391	.0040334	-0.38	0.707	.191621	-.009444	.006366
jsa_~102*	-.0053907	.003774	-1.34	0.179	.190601	-.012788	.002006
jsa_~103*	.0069827	.0049111	1.52	0.128	.190295	-.002643	.016608
jsa_~104*	.0041891	.0044762	0.98	0.329	.189322	-.004584	.012962
jsa_~105*	-.00192	.0028252	-0.67	0.506	.187031	-.007457	.003617
esa_w~_3*	.0071403	.0160641	0.49	0.622	.002021	-.024345	.038625
esa_w~_4*	-.002873	.0153952	-0.18	0.860	.003245	-.033047	.027301
esa_w~_5*	-.0078012	.0127262	-0.52	0.605	.003987	-.032744	.017142
esa_w~_6*	.0180952	.0265095	0.85	0.397	.004813	-.033863	.070053
esa_w~_7*	.0021734	.0181473	0.12	0.901	.005601	-.033395	.037741
esa_w~_8*	-.0099818	.0103459	-0.76	0.445	.006352	-.030259	.010296
esa_w~_9*	.029458	.0341744	1.16	0.247	.007019	-.037523	.096439
esa_w~10*	-.0190288	.0050403	-1.83	0.067	.007631	-.028908	-.00915
esa_w~11*	.0070787	.0201719	0.39	0.697	.008308	-.032457	.046615
esa_w~12*	-.0132529	.0079415	-1.17	0.241	.008809	-.028818	.002312
esa_w~13*	.0308562	.0308396	1.35	0.176	.009375	-.029588	.091301
esa_w~14*	.0404825	.0323914	1.78	0.076	.009866	-.023004	.103969
esa_w~15*	-.0151271	.007515	-1.30	0.194	.010423	-.029856	-.000398
esa_w~16*	.0054479	.0239822	0.25	0.805	.011007	-.041556	.052452
esa_w~17*	-.0004718	.0189625	-0.02	0.980	.011433	-.037638	.036694
esa_w~18*	-.0149925	.0073062	-1.34	0.182	.011702	-.029312	-.000673
esa_w~19*	.0205099	.0235334	1.10	0.272	.012092	-.025615	.066634
esa_w~20*	-.0026777	.0127463	-0.20	0.841	.012527	-.02766	.022305
esa_w~21*	.0201126	.0251688	1.00	0.315	.01288	-.029217	.069443
esa_w~22*	-.0063207	.0121726	-0.46	0.648	.013223	-.030179	.017537
esa_w~23*	-.0134943	.0078211	-1.21	0.227	.01352	-.028823	.001835
esa_w~24*	.0578115	.0450597	1.95	0.052	.013835	-.030504	.146127
esa_w~25*	-.0183062	.0059709	-1.63	0.104	.014141	-.030009	-.006603
esa_w~26*	.006705	.0212316	0.35	0.728	.014354	-.034908	.048318
esa_w~27*	.0544385	.0406087	2.01	0.044	.014642	-.025153	.13403
esa_w~28*	-.0188978	.0051182	-1.88	0.060	.014836	-.028929	-.008866
esa_w~29*	.0094137	.0221829	0.48	0.629	.014855	-.034064	.052891
esa_w~30*	-.0122452	.0082777	-1.09	0.275	.015077	-.028469	.003979
esa_w~31*	.0101469	.0186709	0.62	0.533	.015179	-.026447	.046741
esa_w~32*	.0253601	.0265621	1.25	0.213	.015532	-.026701	.077421
esa_w~33*	-.0046081	.0130496	-0.32	0.746	.015754	-.030185	.020969
esa_w~34*	-.0085736	.0105476	-0.68	0.499	.015921	-.029246	.012099
esa_w~35*	.0415288	.0362234	1.63	0.102	.015931	-.029468	.112525
esa_w~36*	-.0063594	.0126671	-0.44	0.659	.016042	-.031186	.018468
esa_w~37*	-.0073983	.0110354	-0.58	0.565	.016339	-.029027	.014231
esa_w~38*	-.0132059	.0083457	-1.13	0.260	.016376	-.029563	.003151

esa_w~39*	-.0101572	.0105931	-0.76	0.447	.016626	-.030919	.010605
esa_w~40*	.0159831	.0234863	0.83	0.409	.016487	-.030049	.062015
esa_w~41*	.0155028	.0226141	0.83	0.408	.016515	-.02882	.059826
esa_w~42*	-.0063669	.011999	-0.47	0.640	.016589	-.029884	.017151
esa_w~43*	.0082019	.0200547	0.46	0.647	.016524	-.031105	.047508
esa_w~44*	-.0055596	.0128387	-0.39	0.698	.016515	-.030723	.019604
esa_w~45*	-.0023797	.0143079	-0.16	0.873	.016515	-.030423	.025663
esa_w~46*	.0042106	.0182169	0.25	0.805	.016635	-.031494	.039915
esa_w~47*	-.0140111	.0078213	-1.23	0.218	.016654	-.029341	.001318
esa_w~48*	.0056151	.0193762	0.31	0.753	.016672	-.032362	.043592
esa_w~49*	-.0028613	.0150277	-0.18	0.857	.016858	-.032315	.026593
esa_w~50*	-.0045708	.0123996	-0.34	0.735	.016951	-.028873	.019732
esa_w~51*	.064087	.0406547	2.44	0.015	.017006	-.015595	.143769
esa_w~52*	-.011086	.0096075	-0.89	0.374	.01721	-.029916	.007744
esa_w~53*	.0081699	.0198095	0.46	0.644	.017182	-.030656	.046996
esa_w~54*	-.0055178	.013707	-0.36	0.718	.017062	-.032383	.021348
esa_w~55*	.0008	.0167134	0.05	0.961	.017053	-.031958	.033558
esa_w~56*	.0058493	.0191164	0.33	0.739	.017025	-.031618	.043317
esa_w~57*	-.0090367	.0110424	-0.67	0.502	.016941	-.030679	.012606
esa_w~58*	-.0122407	.0088347	-1.02	0.306	.017043	-.029556	.005075
esa_w~59*	.0012645	.0171506	0.08	0.940	.017164	-.03235	.034879
esa_w~60*	-.00019	.015965	-0.01	0.991	.017043	-.031481	.031101
esa_w~61*	.019059	.0283879	0.84	0.403	.016988	-.03658	.074698
esa_w~62*	.0172351	.0280638	0.75	0.451	.017117	-.037769	.072239
esa_w~63*	-.0090086	.0110333	-0.67	0.502	.017257	-.030634	.012616
esa_w~64*	.0046229	.0186474	0.27	0.790	.017192	-.031925	.041171
esa_w~65*	-.008372	.0120429	-0.58	0.561	.017266	-.031976	.015232
esa_w~66*	-.0031739	.0147419	-0.20	0.839	.017516	-.032068	.02572
esa_w~67*	.0199535	.0249754	1.00	0.317	.017488	-.028997	.068904
esa_w~68*	-.006212	.0126347	-0.43	0.664	.017507	-.030976	.018552
esa_w~69*	.0036316	.0194929	0.20	0.844	.017498	-.034574	.041837
esa_w~70*	-.0058545	.0129437	-0.40	0.687	.017414	-.031224	.019515
esa_w~71*	-.0119053	.0091604	-0.97	0.331	.017155	-.029859	.006049
esa_w~72*	-.0024195	.015347	-0.15	0.880	.017173	-.032499	.02766
esa_w~73*	-.0161311	.0072723	-1.37	0.170	.016932	-.030384	-.001878
esa_w~74*	.0519935	.0416225	1.86	0.063	.016941	-.029585	.133572
esa_w~75*	.0084709	.0219008	0.43	0.664	.016923	-.034454	.051396
esa_w~76*	-.0018473	.0171163	-0.10	0.917	.016821	-.035395	.0317
esa_w~77*	-.0075398	.0132979	-0.48	0.628	.016793	-.033603	.018524
esa_w~78*	-.0062056	.0140088	-0.39	0.695	.01683	-.033662	.021251
esa_w~79*	.0298514	.0325502	1.23	0.219	.016858	-.033946	.093649
esa_w~80*	-.00857	.0122662	-0.58	0.561	.016821	-.032611	.015471
esa_w~81*	.011475	.0244713	0.54	0.586	.016756	-.036488	.059438
esa_w~82*	-.0142931	.0080247	-1.21	0.227	.016635	-.030021	.001435
esa_w~83*	-.0039753	.0150321	-0.25	0.806	.016533	-.033438	.025487
esa_w~84*	.0080822	.0227332	0.40	0.691	.016348	-.036474	.052638
esa_w~85*	-.0121808	.0102498	-0.88	0.379	.016237	-.03227	.007908
esa_w~86*	.0080046	.0227599	0.39	0.694	.016162	-.036604	.052613
esa_w~87*	.0654512	.0499868	2.03	0.042	.016153	-.032521	.163424
esa_w~88*	-.0105587	.0115555	-0.72	0.474	.016014	-.033207	.01209
esa_w~89*	-.0173653	.0071002	-1.40	0.161	.015829	-.031281	-.003449
esa_w~90*	.0067526	.0242445	0.31	0.759	.015671	-.040766	.054271
esa_w~91*	-.0176734	.0059828	-1.66	0.098	.015615	-.029399	-.005947
esa_w~92*	.0361574	.0367267	1.37	0.171	.015541	-.035826	.10814
esa_w~93*	-.0042609	.0164603	-0.24	0.811	.015421	-.036522	.028001
esa_w~94*	.0112466	.0212468	0.61	0.539	.015383	-.030396	.05289
esa_w~95*	-.006367	.0132469	-0.42	0.672	.015254	-.03233	.019596
esa_w~96*	.0086347	.0244767	0.40	0.691	.015217	-.039339	.056608
esa_w~97*	-.004328	.0172702	-0.23	0.817	.015003	-.038177	.029521
esa_w~98*	.0034127	.0215894	0.17	0.868	.014771	-.038902	.045727
esa_w~99*	.0053867	.0200408	0.29	0.771	.014716	-.033893	.044666

esa_~100*|	.0135159	.0258229	0.62	0.535	.014549	-.037096	.064128
esa_~101*|	-.0135872	.009116	-1.04	0.298	.014512	-.031454	.00428
esa_~102*|	.0124822	.0233058	0.63	0.529	.014614	-.033196	.058161
esa_~103*|	.0019025	.0170631	0.11	0.908	.014465	-.031541	.035346
esa_~104*|	-.0160201	.0066295	-1.50	0.134	.014465	-.029014	-.003027
esa_~105*|	.0157139	.0177519	1.07	0.284	.014317	-.019079	.050507
emph_~_1*|	.0025475	.0015921	1.62	0.106	.377901	-.000573	.005668
emph_~_2*|	-.0064353	.0024364	-2.62	0.009	.443432	-.011211	-.00166
emph_~_3*|	.0025338	.0030491	0.83	0.404	.461819	-.003442	.00851
emph_~_4*|	.0013529	.0034477	0.39	0.694	.472465	-.005404	.00811
emph_~_5*|	.0035941	.0037308	0.97	0.333	.478075	-.003718	.010906
emph_~_6*|	-.0020077	.0036798	-0.55	0.586	.481608	-.00922	.005205
emph_~_7*|	.0010944	.0038361	0.29	0.775	.483379	-.006424	.008613
emph_~_8*|	.0025696	.0039741	0.65	0.517	.483536	-.00522	.010359
emph_~_9*|	.0036165	.0040305	0.90	0.368	.483128	-.004283	.011516
emph_~10*|	-.0057369	.0040394	-1.42	0.156	.482266	-.013654	.00218
emph_~11*|	.0011502	.0040403	0.28	0.776	.481394	-.006769	.009069
emph_~12*|	-.0003548	.0041406	-0.09	0.932	.480402	-.00847	.007761
emph_~13*|	.0025124	.0042335	0.59	0.552	.478121	-.005785	.01081
emph_~14*|	.0000357	.0041845	0.01	0.993	.476526	-.008166	.008237
emph_~15*|	.0012009	.0041056	0.29	0.770	.475599	-.006846	.009248
emph_~16*|	-.0053585	.0041252	-1.29	0.195	.47393	-.013444	.002727
emph_~17*|	.0037943	.0043196	0.88	0.378	.472428	-.004672	.012261
emph_~18*|	-.0008596	.0042787	-0.20	0.841	.471306	-.009246	.007527
emph_~19*|	-.0064184	.0042244	-1.52	0.130	.470332	-.014698	.001861
emph_~20*|	.0031587	.0043318	0.73	0.464	.469238	-.005332	.011649
emph_~21*|	-.001911	.0044182	-0.43	0.666	.46793	-.01057	.006749
emph_~22*|	-.0014377	.00431	-0.33	0.739	.466029	-.009885	.00701
emph_~23*|	.0067862	.0044901	1.53	0.126	.465492	-.002014	.015587
emph_~24*|	-.0035191	.0043611	-0.80	0.421	.46372	-.012067	.005029
emph_~25*|	.0043742	.0045759	0.96	0.336	.462209	-.004594	.013343
emph_~26*|	-.0052233	.0045154	-1.15	0.250	.460892	-.014073	.003627
emph_~27*|	.0043705	.0046973	0.94	0.349	.459798	-.004836	.013577
emph_~28*|	-.0060088	.0045792	-1.30	0.192	.459761	-.014984	.002966
emph_~29*|	-.0021976	.0044915	-0.49	0.626	.459779	-.011001	.006606
emph_~30*|	.006937	.0048352	1.45	0.146	.45913	-.00254	.016414
emph_~31*|	.0013393	.0049744	0.27	0.787	.45862	-.00841	.011089
emph_~32*|	-.004472	.0047905	-0.93	0.353	.457452	-.013861	.004917
emph_~33*|	.0038723	.0048839	0.80	0.425	.457211	-.0057	.013445
emph_~34*|	-.008396	.0048304	-1.73	0.084	.456673	-.017863	.001071
emph_~35*|	.0052722	.0049426	1.08	0.281	.456052	-.004415	.01496
emph_~36*|	.003014	.0046867	0.65	0.518	.455876	-.006172	.0122
emph_~37*|	-.0064959	.0046947	-1.37	0.169	.456005	-.015697	.002706
emph_~38*|	.0007835	.0049483	0.16	0.874	.455866	-.008915	.010482
emph_~39*|	-.0015785	.0047312	-0.33	0.739	.454958	-.010851	.007695
emph_~40*|	.0065621	.0049517	1.34	0.179	.45365	-.003143	.016267
emph_~41*|	-.0008423	.0047325	-0.18	0.859	.453066	-.010118	.008433
emph_~42*|	.0004773	.0045306	0.11	0.916	.452695	-.008402	.009357
emph_~43*|	.0036106	.0045172	0.80	0.421	.451907	-.005243	.012464
emph_~44*|	-.0053323	.0045849	-1.16	0.248	.45148	-.014319	.003654
emph_~45*|	.0007678	.0047737	0.16	0.872	.451508	-.008588	.010124
emph_~46*|	.0004542	.0045987	0.10	0.921	.451267	-.008559	.009468
emph_~47*|	-.007982	.0046041	-1.72	0.086	.450822	-.017006	.001042

emph_~48*	.0123049	.0051698	2.45	0.014	.450609	.002172	.022437
emph_~49*	-.0024752	.0049779	-0.50	0.620	.450395	-.012232	.007281
emph_~50*	-.0014661	.0047538	-0.31	0.758	.450191	-.010783	.007851
emph_~51*	-.0071277	.0046722	-1.51	0.131	.450025	-.016285	.00203
emph_~52*	.006415	.0046839	1.39	0.165	.45072	-.002765	.015595
emph_~53*	.003251	.0044423	0.74	0.461	.450683	-.005456	.011958
emph_~54*	-.0018059	.0046333	-0.39	0.698	.451406	-.010887	.007275
emph_~55*	.0024034	.0048254	0.50	0.617	.451425	-.007054	.011861
emph_~56*	-.0076946	.0045803	-1.67	0.096	.451638	-.016672	.001283
emph_~57*	.0014984	.0049207	0.31	0.760	.451703	-.008146	.011143
emph_~58*	.0042797	.0049286	0.88	0.381	.4511	-.00538	.01394
emph_~59*	.000346	.0048875	0.07	0.944	.450711	-.009233	.009925
emph_~60*	-.0079064	.0047417	-1.65	0.098	.450229	-.0172	.001387
emph_~61*	.0104089	.0049792	2.15	0.032	.449394	.00065	.020168
emph_~62*	-.0020865	.0047723	-0.44	0.663	.448012	-.01144	.007267
emph_~63*	-.0036875	.0048002	-0.76	0.445	.447419	-.013096	.005721
emph_~64*	-.0009159	.0048534	-0.19	0.851	.446974	-.010428	.008597
emph_~65*	.0048995	.0047693	1.04	0.299	.445138	-.004448	.014247
emph_~66*	-.0055394	.0046421	-1.18	0.237	.444479	-.014638	.003559
emph_~67*	.0068932	.0050841	1.38	0.168	.443552	-.003072	.016858
emph_~68*	-.0019251	.0048986	-0.39	0.695	.442458	-.011526	.007676
emph_~69*	.00486	.0050226	0.98	0.328	.441623	-.004984	.014704
emph_~70*	-.0127201	.004779	-2.62	0.009	.440056	-.022087	-.003354
emph_~71*	.0078478	.0049559	1.62	0.106	.439185	-.001866	.017561
emph_~72*	-.0029859	.0046482	-0.64	0.523	.437803	-.012096	.006124
emph_~73*	.0015248	.0051688	0.30	0.767	.437228	-.008606	.011656
emph_~74*	-.000914	.0052636	-0.17	0.862	.435735	-.01123	.009402
emph_~75*	.0067274	.0052978	1.29	0.196	.434242	-.003656	.017111
emph_~76*	-.006009	.0049219	-1.21	0.228	.433408	-.015656	.003638
emph_~77*	.0049301	.0051355	0.97	0.331	.433083	-.005135	.014995
emph_~78*	-.0051239	.0047286	-1.07	0.283	.432082	-.014392	.004144
emph_~79*	.0029182	.0051219	0.57	0.566	.431952	-.007121	.012957
emph_~80*	.0029858	.0053198	0.57	0.572	.431164	-.007441	.013412
emph_~81*	-.0032466	.0051244	-0.63	0.529	.430552	-.01329	.006797
emph_~82*	-.0007418	.0053561	-0.14	0.890	.429819	-.01124	.009756
emph_~83*	-.0007229	.0052164	-0.14	0.890	.429087	-.010947	.009501
emph_~84*	.0046497	.0052218	0.90	0.367	.429105	-.005585	.014884
emph_~85*	-.0048257	.0051208	-0.93	0.351	.428076	-.014862	.005211
emph_~86*	-.0018898	.0050809	-0.37	0.711	.427585	-.011848	.008069
emph_~87*	.0038566	.005061	0.77	0.441	.427121	-.006063	.013776
emph_~88*	-.0034917	.0049974	-0.69	0.488	.426305	-.013287	.006303
emph_~89*	.0045002	.0052262	0.87	0.383	.426166	-.005743	.014743
emph_~90*	.0005078	.0051402	0.10	0.921	.424923	-.009567	.010582
emph_~91*	.0043579	.0053049	0.83	0.406	.424738	-.00604	.014755
emph_~92*	-.0056124	.0050182	-1.10	0.270	.423532	-.015448	.004223
emph_~93*	-.0019601	.0051752	-0.38	0.706	.423152	-.012103	.008183
emph_~94*	.0031056	.0052512	0.60	0.551	.422633	-.007187	.013398
emph_~95*	.0031831	.0052279	0.61	0.539	.422104	-.007063	.01343
emph_~96*	-.005101	.0050863	-0.99	0.322	.420704	-.01507	.004868
emph_~97*	.0032582	.0054107	0.61	0.543	.419842	-.007347	.013863
emph_~98*	.0040665	.0056559	0.73	0.467	.419239	-.007019	.015152
emph_~99*	-.0095398	.0053167	-1.76	0.079	.418043	-.01996	.000881
emph~100*	.0058448	.0060164	0.99	0.322	.417514	-.005947	.017637
emph~101*	.0009638	.0055391	0.17	0.861	.416847	-.009893	.01182
emph~102*	-.0001991	.0050973	-0.04	0.969	.416587	-.01019	.009791
emph~103*	-.0009566	.0053961	-0.18	0.860	.415938	-.011533	.009619
emph~104*	.0068927	.0055673	1.27	0.205	.415558	-.004019	.017804
emph~105*	-.0055714	.0034997	-1.57	0.116	.414389	-.012431	.001288
san~1one*	-.0017288	.0033591	-0.50	0.617	.023256	-.008313	.004855
sa~1twop*	-.0138426	.005721	-1.64	0.101	.001725	-.025055	-.00263
san~2one*	-.0011861	.0027777	-0.42	0.675	.058455	-.00663	.004258

```
sa~2twop*|   -.0068085   .0041282    -1.44   0.151   .015662    -.0149    .001283
san~3one*|   -.004998    .0024664    -1.87   0.061   .085495   -.009832  -.000164
sa~3twop*|   -.0072691   .0044363    -1.43   0.152   .052002   -.015964   .001426
totals~s |    .0000225   .0029686     0.01   0.994   .358095   -.005796   .005841
sanct~s2 |    .0001858   .0002002     0.93   0.353   1.06599   -.000206   .000578
sanc~jsa |   -.0059557   .0021712    -2.75   0.006   .175511   -.010211    -.0017
sanc~esa |    .0071809    .006359     1.13   0.259   .005964   -.005283   .019644
sancti~l |    .0027363   .0012221     2.24   0.025   .107751    .000341   .005132
sancti~n |    -.01136    .0086525    -1.31   0.189   .003044   -.028319   .005599
sancti~s |    .0044072   .0014276     3.09   0.002   .119134    .001609   .007205
 empprog*|    .0678683   .0071681    13.76   0.000   .179789    .053819   .081918
timeem~g |    .0007514   .0059079     0.13   0.899   .110485   -.010828   .012331
spells~g |   -.0340139   .0019898   -16.68   0.000   .269438   -.037914  -.030114
progempl |   -.0094375   .0041506    -2.27   0.023    .03959   -.017573  -.001302
 progjsa |   -.0063416   .0068437    -0.93   0.354   .064004   -.019755   .007072
otherb~f*|     .014148   .0085305     1.97   0.049   .029905   -.002571   .030867
timeot~n |    .0142731   .0084456     1.69   0.091   .015032    -.00228   .030826
spells~n |    -.014669    .004705    -3.11   0.002   .040095   -.023891  -.005447
otherb~l |   -.0034716   .0090237    -0.38   0.700   .003394   -.021158   .014214
othe~esa |    .0442682   .0318956     1.39   0.165   .000504   -.018246   .106782
othe~jsa |     .079518   .0306758     2.59   0.010   .001319    .019395   .139641
otherb~s |   -.0174851   .0317159    -0.55   0.581   .000628   -.079647   .044677
---------+--------------------------------------------------------------------
   obs. P |    .0547833
  pred. P |    .0242609   (at x-bar)
---------------------------------------------------------------------------
(*) dF/dx is for discrete change of dummy variable from 0 to 1
    z and P>|z| correspond to the test of the underlying coefficient being 0
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7.5 Matching and Reduction in Bias

Variable	Sample	Mean Treated	Control	%bias	%reduct |bias|	t-test t	p>|t|
AshtonCdec	Unmatched	.14912	.17227	-6.3		-4.59	0.000
	Matched	.15014	.15926	-2.5	60.6	-1.36	0.172
OldCdec	Unmatched	.35664	.26464	20.0		15.51	0.000
	Matched	.35585	.35903	-0.7	96.5	-0.36	0.720
WarrCdec	Unmatched	.23443	.32221	-19.7		-14.11	0.000
	Matched	.23649	.2283	1.8	90.7	1.05	0.294
WigCdec	Unmatched	.25982	.27297	-3.0		-2.21	0.027
	Matched	.25752	.25341	0.9	68.7	0.51	0.610
CCJSA	Unmatched	.33192	.39643	-13.4		-9.87	0.000
	Matched	.33362	.32709	1.4	89.9	0.75	0.453
age2529	Unmatched	.14523	.17314	-7.6		-5.53	0.000
	Matched	.14552	.14452	0.3	96.4	0.15	0.878
age30pl	Unmatched	.18111	.29861	-27.8		-19.34	0.000
	Matched	.18143	.18232	-0.2	99.2	-0.12	0.901
oldgender	Unmatched	.06855	.1311	-21.0		-14.02	0.000
	Matched	.06908	.06892	0.1	99.7	0.03	0.972
oldjsa	Unmatched	.09131	.10047	-4.3		-3.24	0.001
	Matched	.09116	.0907	0.2	95.0	0.12	0.906
oldesa	Unmatched	.00968	.01014	-0.7		-0.53	0.599
	Matched	.00917	.0094	-0.4	49.0	-0.21	0.836
otherbenold	Unmatched	.0067	.00817	-1.7		-1.23	0.220
	Matched	.00653	.00648	0.1	96.5	0.03	0.972
oldemp	Unmatched	.2106	.26394	-13.7		-9.95	0.000
	Matched	.21135	.21205	-0.2	98.7	-0.10	0.920
sanctionold	Unmatched	.11628	.13958	-3.9		-2.87	0.004
	Matched	.11235	.11356	-0.2	94.8	-0.12	0.907
progold	Unmatched	.05709	.05813	-0.5		-0.37	0.708
	Matched	.05686	.05734	-0.2	54.1	-0.13	0.899
agejsa10	Unmatched	.19364	.22871	-8.6		-6.26	0.000
	Matched	.19306	.193	0.0	99.8	0.01	0.994
genderdum1	Unmatched	.30552	.31603	-2.3		-1.69	0.091
	Matched	.30575	.3085	-0.6	73.9	-0.32	0.747
sanctionge~r	Unmatched	.06686	.06061	1.4		1.14	0.252
	Matched	.06583	.065	0.2	86.6	0.10	0.918
jsagender	Unmatched	.04353	.0393	3.0		2.38	0.017
	Matched	.04281	.04274	0.0	98.5	0.02	0.981
esagender	Unmatched	.00366	.00362	0.1		0.06	0.950
	Matched	.0036	.00367	-0.2	-112.2	-0.10	0.923

otherbenge~r	Unmatched	.00626	.00619	0.1		0.08	0.937
	Matched	.00621	.00604	0.2	-123.9	0.13	0.898
empgender	Unmatched	.14603	.14933	-1.0		-0.78	0.437
	Matched	.14627	.14645	-0.1	94.6	-0.03	0.975
proggender	Unmatched	.02683	.01925	5.8		4.68	0.000
	Matched	.02572	.02549	0.2	96.9	0.09	0.925
monstartdum4	Unmatched	.07075	.12651	-18.8		-12.68	0.000
	Matched	.07148	.07202	-0.2	99.0	-0.11	0.909
monstartdum5	Unmatched	.12001	.0998	6.5		5.01	0.000
	Matched	.12004	.12685	-2.2	66.3	-1.12	0.263
monstartdum6	Unmatched	.14201	.12318	5.6		4.27	0.000
	Matched	.14244	.14353	-0.3	94.2	-0.17	0.866
monstartdum7	Unmatched	.1173	.1029	4.6		3.53	0.000
	Matched	.11765	.11902	-0.4	90.4	-0.23	0.818
monstartdum8	Unmatched	.10443	.09278	3.9		2.99	0.003
	Matched	.10431	.10395	0.1	96.9	0.06	0.949
monstartdum9	Unmatched	.08717	.08505	0.8		0.57	0.571
	Matched	.08653	.08645	0.0	96.5	0.01	0.989
monstartd~10	Unmatched	.14726	.13101	4.7		3.59	0.000
	Matched	.14655	.13868	2.3	51.6	1.22	0.224
monstartd~11	Unmatched	.08785	.0844	1.2		0.93	0.354
	Matched	.08738	.09009	-1.0	21.5	-0.51	0.607
monstartd~12	Unmatched	.07549	.08109	-2.1		-1.54	0.125
	Matched	.07558	.07034	2.0	6.4	1.09	0.276
prspelljsa1	Unmatched	.05366	.2354	-53.5		-32.66	0.000
	Matched	.05421	.05374	0.1	99.7	0.11	0.912
prspelljsa2	Unmatched	.0088	.12687	-33.1		-18.58	0.000
	Matched	.00889	.0107	-0.5	98.5	-0.70	0.485
jsa_week_3	Unmatched	.00406	.0297	-20.0		-11.56	0.000
	Matched	.0041	.00448	-0.3	98.5	-0.31	0.754
jsa_week_4	Unmatched	.02319	.05672	-17.2		-11.01	0.000
	Matched	.02308	.02381	-0.4	97.8	-0.26	0.795
jsa_week_5	Unmatched	.04722	.08365	-14.8		-9.95	0.000
	Matched	.04702	.04725	-0.1	99.4	-0.06	0.954
jsa_week_6	Unmatched	.06567	.10427	-13.9		-9.53	0.000
	Matched	.06566	.06516	0.2	98.7	0.11	0.913
jsa_week_7	Unmatched	.08582	.11997	-11.3		-7.91	0.000
	Matched	.08516	.08582	-0.2	98.0	-0.13	0.897
jsa_week_8	Unmatched	.10494	.13457	-9.1		-6.52	0.000
	Matched	.10363	.10466	-0.3	96.5	-0.18	0.855
jsa_week_9	Unmatched	.1195	.14527	-7.6		-5.49	0.000

jsa_week_10	Unmatched	.13033	.15302	-6.5		-4.73	0.000
	Matched	.12808	.13057	-0.7	89.0	-0.40	0.687
jsa_week_11	Unmatched	.1437	.16118	-4.9		-3.56	0.000
	Matched	.14107	.14299	-0.5	89.1	-0.30	0.767
jsa_week_12	Unmatched	.15369	.16827	-4.0		-2.92	0.004
	Matched	.15082	.15213	-0.4	91.0	-0.20	0.843
jsa_week_13	Unmatched	.16418	.17546	-3.0		-2.22	0.026
	Matched	.16108	.16198	-0.2	92.1	-0.13	0.895
jsa_week_14	Unmatched	.17502	.18143	-1.7		-1.24	0.213
	Matched	.17202	.1732	-0.3	81.7	-0.17	0.867
jsa_week_15	Unmatched	.18568	.18522	0.1		0.09	0.929
	Matched	.18228	.18309	-0.2	-74.0	-0.11	0.910
jsa_week_16	Unmatched	.1911	.18969	0.4		0.27	0.789
	Matched	.18793	.1888	-0.2	38.0	-0.12	0.904
jsa_week_17	Unmatched	.19567	.1927	0.7		0.56	0.574
	Matched	.19254	.19301	-0.1	84.3	-0.06	0.949
jsa_week_18	Unmatched	.20125	.1956	1.4		1.07	0.287
	Matched	.19802	.19843	-0.1	92.7	-0.06	0.956
jsa_week_19	Unmatched	.20735	.19739	2.5		1.87	0.062
	Matched	.20417	.20445	-0.1	97.2	-0.04	0.970
jsa_week_20	Unmatched	.21412	.20022	3.4		2.59	0.010
	Matched	.21135	.20936	0.5	85.6	0.26	0.791
jsa_week_21	Unmatched	.21852	.20289	3.8		2.90	0.004
	Matched	.2158	.21505	0.2	95.2	0.10	0.922
jsa_week_22	Unmatched	.22461	.20518	4.7		3.59	0.000
	Matched	.22179	.22057	0.3	93.7	0.16	0.874
jsa_week_23	Unmatched	.22935	.20641	5.6		4.23	0.000
	Matched	.22657	.2246	0.5	91.4	0.25	0.799
jsa_week_24	Unmatched	.23341	.20743	6.3		4.78	0.000
	Matched	.23051	.22832	0.5	91.6	0.28	0.778
jsa_week_25	Unmatched	.23629	.20937	6.5		4.93	0.000
	Matched	.23341	.23146	0.5	92.7	0.25	0.802
jsa_week_26	Unmatched	.24255	.21051	7.7		5.86	0.000
	Matched	.23923	.23774	0.4	95.4	0.19	0.850
jsa_week_27	Unmatched	.24306	.21112	7.6		5.83	0.000
	Matched	.23991	.23902	0.2	97.2	0.11	0.910
jsa_week_28	Unmatched	.24425	.21208	7.7		5.86	0.000
	Matched	.24077	.23949	0.3	96.0	0.16	0.871
jsa_week_29	Unmatched	.24729	.21319	8.1		6.20	0.000
	Matched	.2435	.2417	0.4	94.7	0.23	0.820
jsa_week_30	Unmatched	.24915	.21375	8.4		6.43	0.000

```
jsa_week_31  Unmatched |  .24882    .21314      8.5            |   6.49   0.000
               Matched |  .24555    .24254      0.7    91.5    |   0.38   0.704
                       |                                       |
jsa_week_32  Unmatched |  .24814    .21351      8.2            |   6.30   0.000
               Matched |  .24521     .2409      1.0    87.6    |   0.54   0.587
                       |                                       |
jsa_week_33  Unmatched |  .25017    .21447      8.5            |   6.48   0.000
               Matched |  .24675    .24273      1.0    88.7    |   0.51   0.613
                       |                                       |
jsa_week_34  Unmatched |  .25254     .2151      8.9            |   6.79   0.000
               Matched |  .24897    .24636      0.6    93.0    |   0.33   0.743
                       |                                       |
jsa_week_35  Unmatched |  .25389    .21577      9.0            |   6.90   0.000
               Matched |  .25034    .24718      0.7    91.7    |   0.40   0.692
                       |                                       |
jsa_week_36  Unmatched |  .25135    .21592      8.4            |   6.42   0.000
               Matched |  .24795    .24564      0.5    93.5    |   0.29   0.773
                       |                                       |
jsa_week_37  Unmatched |  .25135    .21514      8.6            |   6.57   0.000
               Matched |  .24795    .24597      0.5    94.5    |   0.25   0.804
                       |                                       |
jsa_week_38  Unmatched |  .25017    .21416      8.5            |   6.54   0.000
               Matched |  .24692    .24573      0.3    96.7    |   0.15   0.881
                       |                                       |
jsa_week_39  Unmatched |  .24661    .21489      7.5            |   5.76   0.000
               Matched |  .24453    .24425      0.1    99.1    |   0.03   0.972
                       |                                       |
jsa_week_40  Unmatched |  .24374    .21482      6.9            |   5.25   0.000
               Matched |  .24145    .24183     -0.1    98.7    |  -0.05   0.962
                       |                                       |
jsa_week_41  Unmatched |  .24543    .21535      7.1            |   5.45   0.000
               Matched |  .24333    .24186      0.3    95.1    |   0.19   0.853
                       |                                       |
jsa_week_42  Unmatched |  .24509    .21623      6.9            |   5.23   0.000
               Matched |  .24316    .24053      0.6    90.9    |   0.33   0.740
                       |                                       |
jsa_week_43  Unmatched |  .24408    .21661      6.5            |   4.97   0.000
               Matched |  .24248    .23919      0.8    88.0    |   0.42   0.678
                       |                                       |
jsa_week_44  Unmatched |  .24154    .21579      6.1            |   4.67   0.000
               Matched |  .24008    .23675      0.8    87.1    |   0.42   0.673
                       |                                       |
jsa_week_45  Unmatched |  .24103    .21583      6.0            |   4.57   0.000
               Matched |  .23957    .23624      0.8    86.8    |   0.42   0.672
                       |                                       |
jsa_week_46  Unmatched |  .24272    .21531      6.5            |   4.97   0.000
               Matched |  .24094    .23675      1.0    84.7    |   0.53   0.595
                       |                                       |
jsa_week_47  Unmatched |  .24475     .2155      7.0            |   5.30   0.000
               Matched |  .24299    .23917      0.9    86.9    |   0.48   0.629
                       |                                       |
jsa_week_48  Unmatched |  .24408    .21507      6.9            |   5.26   0.000
               Matched |  .24231    .23879      0.8    87.9    |   0.44   0.657
                       |                                       |
jsa_week_49  Unmatched |  .24357    .21473      6.9            |   5.24   0.000
               Matched |  .24213    .23893      0.8    88.9    |   0.41   0.685
                       |                                       |
jsa_week_50  Unmatched |  .24391    .21361      7.2            |   5.51   0.000
               Matched |  .24248    .23843      1.0    86.7    |   0.51   0.609
                       |                                       |
jsa_week_51  Unmatched |  .23866    .21149      6.5            |   4.96   0.000
```

jsa_week_52	Unmatched	.23341	.20902	5.9		4.47	0.000
	Matched	.2317	.22817	0.9	85.5	0.45	0.650
jsa_week_53	Unmatched	.22952	.20603	5.7		4.33	0.000
	Matched	.22811	.22391	1.0	82.1	0.54	0.587
jsa_week_54	Unmatched	.22715	.20412	5.6		4.26	0.000
	Matched	.22572	.22194	0.9	83.6	0.49	0.624
jsa_week_55	Unmatched	.2263	.20245	5.8		4.43	0.000
	Matched	.22486	.22172	0.8	86.8	0.41	0.684
jsa_week_56	Unmatched	.22529	.20179	5.7		4.37	0.000
	Matched	.22401	.22021	0.9	83.8	0.49	0.621
jsa_week_57	Unmatched	.2263	.20126	6.1		4.66	0.000
	Matched	.22452	.22119	0.8	86.7	0.43	0.666
jsa_week_58	Unmatched	.22867	.20053	6.9		5.24	0.000
	Matched	.22657	.22373	0.7	89.9	0.37	0.713
jsa_week_59	Unmatched	.22918	.19994	7.1		5.45	0.000
	Matched	.22726	.22423	0.7	89.7	0.39	0.696
jsa_week_60	Unmatched	.22935	.19833	7.6		5.80	0.000
	Matched	.22726	.22533	0.5	93.8	0.25	0.803
jsa_week_61	Unmatched	.22681	.19789	7.1		5.41	0.000
	Matched	.22521	.22299	0.5	92.3	0.29	0.774
jsa_week_62	Unmatched	.22512	.19802	6.6		5.07	0.000
	Matched	.22367	.22237	0.3	95.2	0.17	0.867
jsa_week_63	Unmatched	.22393	.19795	6.4		4.86	0.000
	Matched	.2223	.22061	0.4	93.5	0.22	0.826
jsa_week_64	Unmatched	.22613	.19784	6.9		5.29	0.000
	Matched	.22435	.22169	0.7	90.6	0.35	0.730
jsa_week_65	Unmatched	.22563	.19788	6.8		5.19	0.000
	Matched	.22418	.2221	0.5	92.5	0.27	0.787
jsa_week_66	Unmatched	.22698	.19777	7.1		5.46	0.000
	Matched	.22503	.22231	0.7	90.7	0.35	0.724
jsa_week_67	Unmatched	.22664	.19697	7.3		5.56	0.000
	Matched	.22469	.22187	0.7	90.5	0.37	0.714
jsa_week_68	Unmatched	.2219	.19664	6.2		4.74	0.000
	Matched	.22025	.21628	1.0	84.3	0.52	0.604
jsa_week_69	Unmatched	.22089	.19675	5.9		4.53	0.000
	Matched	.21888	.21636	0.6	89.6	0.33	0.742
jsa_week_70	Unmatched	.22106	.1962	6.1		4.66	0.000
	Matched	.21922	.21585	0.8	86.4	0.44	0.658
jsa_week_71	Unmatched	.22393	.19726	6.5		5.00	0.000
	Matched	.22196	.21912	0.7	89.4	0.37	0.712
jsa_week_72	Unmatched	.22427	.19748	6.6		5.02	0.000

```
jsa_week_73  Unmatched |  .22478   .19727    6.7           |  5.15   0.000
             Matched   |  .22315   .21744    1.4    79.2   |  0.74   0.456
                       |                                   |
jsa_week_74  Unmatched |  .22664   .19747    7.1           |  5.46   0.000
             Matched   |  .22503   .21879    1.5    78.6   |  0.81   0.417
                       |                                   |
jsa_week_75  Unmatched |  .22901   .19752    7.7           |  5.89   0.000
             Matched   |  .22692   .22252    1.1    86.0   |  0.57   0.569
                       |                                   |
jsa_week_76  Unmatched |  .22664   .19778    7.1           |  5.40   0.000
             Matched   |  .22469   .21985    1.2    83.2   |  0.63   0.529
                       |                                   |
jsa_week_77  Unmatched |  .22309   .19756    6.3           |  4.78   0.000
             Matched   |  .22196   .21641    1.4    78.3   |  0.72   0.469
                       |                                   |
jsa_week_78  Unmatched |  .22224   .19742    6.1           |  4.65   0.000
             Matched   |  .22144   .21564    1.4    76.6   |  0.76   0.448
                       |                                   |
jsa_week_79  Unmatched |  .22241   .19752    6.1           |  4.66   0.000
             Matched   |  .22161   .21555    1.5    75.6   |  0.79   0.428
                       |                                   |
jsa_week_80  Unmatched |  .22106   .19725    5.9           |  4.46   0.000
             Matched   |  .22008   .21272    1.8    69.1   |  0.97   0.334
                       |                                   |
jsa_week_81  Unmatched |  .22072    .1975    5.7           |  4.35   0.000
             Matched   |  .21956   .21363    1.5    74.5   |  0.78   0.436
                       |                                   |
jsa_week_82  Unmatched |  .22106    .1973    5.8           |  4.45   0.000
             Matched   |  .22025    .2134    1.7    71.2   |  0.90   0.369
                       |                                   |
jsa_week_83  Unmatched |  .21733   .19686    5.1           |  3.84   0.000
             Matched   |  .21631   .21019    1.5    70.1   |  0.81   0.419
                       |                                   |
jsa_week_84  Unmatched |  .21716   .19646    5.1           |  3.89   0.000
             Matched   |   .2158   .21025    1.4    73.2   |  0.73   0.464
                       |                                   |
jsa_week_85  Unmatched |   .2197   .19544    6.0           |  4.56   0.000
             Matched   |  .21785   .21351    1.1    82.1   |  0.57   0.568
                       |                                   |
jsa_week_86  Unmatched |  .21869   .19596    5.6           |  4.27   0.000
             Matched   |  .21717   .21238    1.2    79.0   |  0.63   0.529
                       |                                   |
jsa_week_87  Unmatched |  .22207    .1949    6.7           |  5.11   0.000
             Matched   |  .22025   .21644    0.9    86.0   |  0.50   0.618
                       |                                   |
jsa_week_88  Unmatched |  .22021   .19511    6.2           |  4.72   0.000
             Matched   |  .21837   .21451    1.0    84.6   |  0.51   0.613
                       |                                   |
jsa_week_89  Unmatched |  .22461   .19508    7.3           |  5.55   0.000
             Matched   |   .2223   .21837    1.0    86.7   |  0.51   0.608
                       |                                   |
jsa_week_90  Unmatched |  .22546   .19501    7.5           |  5.73   0.000
             Matched   |  .22315    .2178    1.3    82.4   |  0.70   0.485
                       |                                   |
jsa_week_91  Unmatched |  .22309   .19479    7.0           |  5.32   0.000
             Matched   |  .22059   .21521    1.3    81.0   |  0.70   0.481
                       |                                   |
jsa_week_92  Unmatched |  .22173   .19462    6.7           |  5.10   0.000
             Matched   |  .21939   .21481    1.1    83.1   |  0.60   0.548
                       |                                   |
jsa_week_93  Unmatched |  .22173   .19424    6.8           |  5.18   0.000
```

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jsa_week_94   Unmatched |  .22021    .1943     6.4          |  4.88  0.000
              Matched   |  .21768   .21328     1.1   83.0   |  0.58  0.563
                        |                                   |
jsa_week_95   Unmatched |  .22156   .19391     6.8          |  5.21  0.000
              Matched   |  .21871    .2146     1.0   85.1   |  0.54  0.590
                        |                                   |
jsa_week_96   Unmatched |  .22224   .19335     7.1          |  5.45  0.000
              Matched   |  .21973   .21635     0.8   88.3   |  0.44  0.657
                        |                                   |
jsa_week_97   Unmatched |   .2219   .19305     7.1          |  5.45  0.000
              Matched   |  .21939   .21652     0.7   90.1   |  0.38  0.707
                        |                                   |
jsa_week_98   Unmatched |  .22038    .1927     6.8          |  5.23  0.000
              Matched   |  .21785   .21488     0.7   89.3   |  0.39  0.697
                        |                                   |
jsa_week_99   Unmatched |  .21682    .1917     6.2          |  4.76  0.000
              Matched   |  .21426    .2115     0.7   89.0   |  0.36  0.716
                        |                                   |
jsa_week_100  Unmatched |  .21462   .19136     5.8          |  4.41  0.000
              Matched   |  .21272   .20949     0.8   86.1   |  0.43  0.669
                        |                                   |
jsa_week_101  Unmatched |  .21327   .19037     5.7          |  4.35  0.000
              Matched   |  .21135   .20753     1.0   83.3   |  0.51  0.612
                        |                                   |
jsa_week_102  Unmatched |  .21242   .18934     5.8          |  4.39  0.000
              Matched   |   .2105   .20613     1.1   81.1   |  0.58  0.561
                        |                                   |
jsa_week_103  Unmatched |  .21649   .18878     6.9          |  5.28  0.000
              Matched   |  .21426   .20997     1.1   84.5   |  0.57  0.570
                        |                                   |
jsa_week_104  Unmatched |  .21479   .18785     6.7          |  5.14  0.000
              Matched   |  .21255   .20794     1.1   82.9   |  0.61  0.541
                        |                                   |
jsa_week_105  Unmatched |  .21022   .18569     6.2          |  4.70  0.000
              Matched   |  .20793   .20397     1.0   83.9   |  0.53  0.596
                        |                                   |
 esa_week_3   Unmatched |  .00271   .00198     1.5          |  1.21  0.227
              Matched   |  .00256   .00232     0.5   66.2   |  0.27  0.788
                        |                                   |
 esa_week_4   Unmatched |  .00406    .0032     1.4          |  1.14  0.256
              Matched   |  .00393   .00372     0.3   75.8   |  0.18  0.855
                        |                                   |
 esa_week_5   Unmatched |  .00474   .00394     1.2          |  0.94  0.345
              Matched   |  .00462   .00439     0.3   71.6   |  0.18  0.855
                        |                                   |
 esa_week_6   Unmatched |  .00609   .00474     1.8          |  1.46  0.143
              Matched   |  .00564   .00539     0.4   81.0   |  0.19  0.851
                        |                                   |
 esa_week_7   Unmatched |  .00677   .00553     1.6          |  1.24  0.215
              Matched   |  .00633   .00614     0.2   84.9   |  0.13  0.898
                        |                                   |
 esa_week_8   Unmatched |  .00745   .00629     1.4          |  1.09  0.276
              Matched   |  .00718   .00675     0.5   62.5   |  0.28  0.777
                        |                                   |
 esa_week_9   Unmatched |  .00846   .00694     1.7          |  1.37  0.172
              Matched   |  .00821   .00771     0.6   67.6   |  0.30  0.763
                        |                                   |
esa_week_10   Unmatched |  .00863   .00757     1.2          |  0.91  0.363
              Matched   |  .00838   .00787     0.6   51.6   |  0.31  0.757
                        |                                   |
esa_week_11   Unmatched |  .00982   .00822     1.7          |  1.31  0.189
```

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esa_week_12  Unmatched |  .01049   .00871      1.8            |   1.43  0.154
             Matched   |  .01009   .00962      0.5    73.5    |   0.26  0.796
                       |
esa_week_13  Unmatched |  .01202   .00922      2.7            |   2.17  0.030
             Matched   |  .01129    .0111      0.2    93.4    |   0.09  0.924
                       |
esa_week_14  Unmatched |  .01286   .00969      3.0            |   2.40  0.016
             Matched   |  .01214   .01196      0.2    94.3    |   0.09  0.928
                       |
esa_week_15  Unmatched |  .01303   .01027      2.6            |   2.03  0.042
             Matched   |  .01231   .01221      0.1    96.2    |   0.05  0.958
                       |
esa_week_16  Unmatched |  .01371   .01085      2.6            |   2.05  0.041
             Matched   |  .01265   .01334     -0.6    76.0    |  -0.33  0.744
                       |
esa_week_17  Unmatched |  .01405   .01128      2.5            |   1.94  0.052
             Matched   |   .013    .01364     -0.6    76.7    |  -0.30  0.761
                       |
esa_week_18  Unmatched |  .01422   .01156      2.4            |   1.85  0.064
             Matched   |  .01317   .01387     -0.6    73.6    |  -0.33  0.743
                       |
esa_week_19  Unmatched |  .01506   .01192      2.7            |   2.15  0.032
             Matched   |  .01385   .01465     -0.7    74.5    |  -0.37  0.715
                       |
esa_week_20  Unmatched |   .0154   .01236      2.6            |   2.04  0.041
             Matched   |  .01436   .01508     -0.6    76.5    |  -0.32  0.748
                       |
esa_week_21  Unmatched |  .01591    .0127      2.7            |   2.13  0.034
             Matched   |  .01471   .01539     -0.6    78.8    |  -0.30  0.762
                       |
esa_week_22  Unmatched |  .01591   .01307      2.4            |   1.86  0.063
             Matched   |  .01471   .01543     -0.6    74.5    |  -0.32  0.748
                       |
esa_week_23  Unmatched |  .01625   .01336      2.4            |   1.87  0.062
             Matched   |  .01505   .01589     -0.7    70.7    |  -0.37  0.711
                       |
esa_week_24  Unmatched |   .0171   .01365      2.8            |   2.21  0.027
             Matched   |   .0159   .01682     -0.7    73.5    |  -0.39  0.697
                       |
esa_week_25  Unmatched |  .01659    .014       2.1            |   1.64  0.101
             Matched   |  .01573   .01657     -0.7    67.7    |  -0.36  0.720
                       |
esa_week_26  Unmatched |  .01726   .01419      2.5            |   1.93  0.053
             Matched   |  .01607    .0168     -0.6    76.6    |  -0.31  0.759
                       |
esa_week_27  Unmatched |   .0176   .01447      2.5            |   1.95  0.051
             Matched   |  .01659   .01706     -0.4    84.8    |  -0.20  0.841
                       |
esa_week_28  Unmatched |  .01676   .01473      1.6            |   1.26  0.209
             Matched   |  .01607   .01638     -0.2    84.8    |  -0.13  0.895
                       |
esa_week_29  Unmatched |   .0171   .01473      1.9            |   1.46  0.143
             Matched   |  .01642    .0166     -0.1    92.1    |  -0.08  0.937
                       |
esa_week_30  Unmatched |  .01743   .01494      2.0            |   1.53  0.126
             Matched   |  .01659   .01629      0.2    88.1    |   0.13  0.900
                       |
esa_week_31  Unmatched |  .01811   .01501      2.4            |   1.90  0.058
             Matched   |  .01693   .01642      0.4    83.6    |   0.21  0.830
                       |
esa_week_32  Unmatched |  .01862   .01535      2.5            |   1.97  0.048
```

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esa_week_33  Unmatched |  .01828   .01561     2.1            |   1.60  0.109
                Matched |   .0171   .01701     0.1    96.5 |   0.04  0.969
                        |                                  |
esa_week_34  Unmatched |  .01811   .01579     1.8            |   1.38  0.167
                Matched |  .01693   .01698    -0.0    97.9 |  -0.02  0.984
                        |                                  |
esa_week_35  Unmatched |  .01811    .0158     1.8            |   1.38  0.169
                Matched |   .0171   .01693     0.1    92.7 |   0.07  0.944
                        |                                  |
esa_week_36  Unmatched |  .01743   .01596     1.1            |   0.88  0.381
                Matched |  .01659   .01642     0.1    88.7 |   0.07  0.944
                        |                                  |
esa_week_37  Unmatched |  .01676   .01631     0.3            |   0.26  0.794
                Matched |  .01607   .01544     0.5   -42.3 |   0.27  0.784
                        |                                  |
esa_week_38  Unmatched |  .01608   .01639    -0.2            |  -0.18  0.854
                Matched |  .01556   .01511     0.4   -45.1 |   0.20  0.842
                        |                                  |
esa_week_39  Unmatched |  .01659   .01663    -0.0            |  -0.02  0.981
                Matched |  .01607   .01594     0.1  -239.3 |   0.06  0.953
                        |                                  |
esa_week_40  Unmatched |  .01693   .01646     0.4            |   0.27  0.785
                Matched |  .01642   .01617     0.2    46.2 |   0.11  0.915
                        |                                  |
esa_week_41  Unmatched |  .01726   .01647     0.6            |   0.47  0.642
                Matched |  .01676   .01671     0.0    93.4 |   0.02  0.982
                        |                                  |
esa_week_42  Unmatched |  .01726   .01655     0.6            |   0.42  0.676
                Matched |  .01676   .01683    -0.1    90.5 |  -0.03  0.977
                        |                                  |
esa_week_43  Unmatched |   .0171   .01649     0.5            |   0.35  0.723
                Matched |  .01659   .01669    -0.1    83.2 |  -0.04  0.966
                        |                                  |
esa_week_44  Unmatched |  .01676    .0165     0.2            |   0.15  0.881
                Matched |  .01624   .01644    -0.1    25.3 |  -0.08  0.935
                        |                                  |
esa_week_45  Unmatched |  .01676    .0165     0.2            |   0.15  0.881
                Matched |  .01624   .01646    -0.2    17.7 |  -0.09  0.928
                        |                                  |
esa_week_46  Unmatched |  .01693   .01662     0.2            |   0.18  0.857
                Matched |  .01642   .01633     0.1    71.7 |   0.04  0.970
                        |                                  |
esa_week_47  Unmatched |  .01693   .01664     0.2            |   0.17  0.866
                Matched |  .01642   .01652    -0.1    65.0 |  -0.04  0.966
                        |                                  |
esa_week_48  Unmatched |   .0176   .01662     0.8            |   0.57  0.565
                Matched |  .01693   .01701    -0.1    91.7 |  -0.03  0.973
                        |                                  |
esa_week_49  Unmatched |  .01811   .01679     1.0            |   0.77  0.442
                Matched |  .01727   .01744    -0.1    87.3 |  -0.07  0.944
                        |                                  |
esa_week_50  Unmatched |  .01862   .01685     1.3            |   1.02  0.307
                Matched |  .01744   .01761    -0.1    90.2 |  -0.07  0.943
                        |                                  |
esa_week_51  Unmatched |   .0193   .01687     1.8            |   1.40  0.161
                Matched |  .01795   .01787     0.1    96.5 |   0.04  0.972
                        |                                  |
esa_week_52  Unmatched |  .01879   .01712     1.3            |   0.96  0.337
                Matched |  .01744   .01757    -0.1    92.6 |  -0.05  0.959
                        |                                  |
esa_week_53  Unmatched |  .01862    .0171     1.1            |   0.87  0.382
```

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esa_week_54  Unmatched |  .01811     .017      0.8              |   0.64   0.522
             Matched   |  .01676    .01702    -0.2     76.5     |  -0.11   0.913
                       |                                        |
esa_week_55  Unmatched |  .01794     .017      0.7              |   0.54   0.587
             Matched   |  .01676    .01691    -0.1     84.2     |  -0.06   0.950
                       |                                        |
esa_week_56  Unmatched |   .0176    .01699     0.5              |   0.35   0.724
             Matched   |  .01642    .01668    -0.2     56.0     |  -0.11   0.909
                       |                                        |
esa_week_57  Unmatched |   .0171    .01693     0.1              |   0.09   0.925
             Matched   |  .01607     .0162    -0.1     20.4     |  -0.06   0.956
                       |                                        |
esa_week_58  Unmatched |  .01693    .01705    -0.1              |  -0.07   0.943
             Matched   |  .01624    .01619     0.0     53.2     |   0.02   0.980
                       |                                        |
esa_week_59  Unmatched |  .01726    .01716     0.1              |   0.06   0.951
             Matched   |  .01659    .01654     0.0     60.2     |   0.02   0.986
                       |                                        |
esa_week_60  Unmatched |  .01726    .01703     0.2              |   0.14   0.892
             Matched   |  .01676    .01661     0.1     38.7     |   0.06   0.952
                       |                                        |
esa_week_61  Unmatched |  .01743    .01696     0.4              |   0.27   0.785
             Matched   |  .01693    .01679     0.1     70.7     |   0.06   0.954
                       |                                        |
esa_week_62  Unmatched |  .01743     .0171     0.3              |   0.19   0.847
             Matched   |  .01693     .017     -0.1     78.7     |  -0.03   0.976
                       |                                        |
esa_week_63  Unmatched |  .01642    .01731    -0.7              |  -0.51   0.611
             Matched   |  .01607    .01641    -0.3     61.6     |  -0.15   0.884
                       |                                        |
esa_week_64  Unmatched |  .01574    .01728    -1.2              |  -0.88   0.378
             Matched   |  .01556    .01599    -0.3     72.2     |  -0.18   0.853
                       |                                        |
esa_week_65  Unmatched |  .01523    .01738    -1.7              |  -1.23   0.217
             Matched   |  .01505    .01579    -0.6     65.4     |  -0.33   0.744
                       |                                        |
esa_week_66  Unmatched |  .01523    .01765    -1.9              |  -1.38   0.169
             Matched   |  .01505    .01585    -0.6     66.9     |  -0.35   0.726
                       |                                        |
esa_week_67  Unmatched |   .0154    .01761    -1.7              |  -1.26   0.208
             Matched   |  .01505    .01573    -0.5     69.3     |  -0.30   0.766
                       |                                        |
esa_week_68  Unmatched |   .0149    .01766    -2.2              |  -1.57   0.115
             Matched   |  .01453    .01551    -0.8     64.6     |  -0.44   0.663
                       |                                        |
esa_week_69  Unmatched |  .01456    .01767    -2.5              |  -1.77   0.076
             Matched   |  .01436    .01526    -0.7     71.1     |  -0.40   0.687
                       |                                        |
esa_week_70  Unmatched |  .01405    .01761    -2.9              |  -2.03   0.042
             Matched   |  .01385     .0146    -0.6     79.1     |  -0.34   0.734
                       |                                        |
esa_week_71  Unmatched |  .01337    .01737    -3.3              |  -2.30   0.021
             Matched   |  .01317    .01351    -0.3     91.4     |  -0.16   0.871
                       |                                        |
esa_week_72  Unmatched |  .01354    .01738    -3.1              |  -2.21   0.027
             Matched   |  .01334    .01384    -0.4     87.0     |  -0.23   0.816
                       |                                        |
esa_week_73  Unmatched |  .01354    .01713    -2.9              |  -2.08   0.038
             Matched   |  .01334    .01377    -0.4     88.0     |  -0.20   0.840
                       |                                        |
esa_week_74  Unmatched |   .0149    .01706    -1.7              |  -1.25   0.210
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esa_week_75  Unmatched |  .0149    .01704    -1.7            |  -1.24  0.214
                Matched |  .01368   .01501    -1.1    37.8    |  -0.61  0.544
                        |                                     |
esa_week_76  Unmatched |  .01439   .01696    -2.1            |  -1.50  0.135
                Matched |  .01317   .01477    -1.3    37.8    |  -0.74  0.460
                        |                                     |
esa_week_77  Unmatched |  .01422   .01694    -2.2            |  -1.58  0.113
                Matched |   .013    .01464    -1.3    39.5    |  -0.76  0.445
                        |                                     |
esa_week_78  Unmatched |  .01439   .01697    -2.1            |  -1.50  0.133
                Matched |  .01317   .01477    -1.3    37.9    |  -0.74  0.460
                        |                                     |
esa_week_79  Unmatched |  .01456   .01699    -2.0            |  -1.41  0.158
                Matched |  .01334   .01477    -1.1    41.3    |  -0.66  0.511
                        |                                     |
esa_week_80  Unmatched |  .01405   .01698    -2.4            |  -1.70  0.088
                Matched |  .01317   .01449    -1.1    55.0    |  -0.61  0.541
                        |                                     |
esa_week_81  Unmatched |  .01388   .01692    -2.5            |  -1.77  0.076
                Matched |  .01317   .01441    -1.0    59.2    |  -0.58  0.565
                        |                                     |
esa_week_82  Unmatched |  .01354   .01681    -2.7            |  -1.91  0.056
                Matched |  .01282   .01391    -0.9    66.7    |  -0.51  0.608
                        |                                     |
esa_week_83  Unmatched |  .01337   .01672    -2.7            |  -1.96  0.050
                Matched |  .01265   .01395    -1.1    61.3    |  -0.61  0.542
                        |                                     |
esa_week_84  Unmatched |  .01354   .01651    -2.4            |  -1.75  0.080
                Matched |  .01282   .01407    -1.0    58.2    |  -0.58  0.560
                        |                                     |
esa_week_85  Unmatched |  .01337    .0164    -2.5            |  -1.79  0.073
                Matched |  .01265   .01394    -1.1    57.4    |  -0.61  0.543
                        |                                     |
esa_week_86  Unmatched |  .01405   .01628    -1.8            |  -1.33  0.185
                Matched |  .01317   .01428    -0.9    50.2    |  -0.52  0.605
                        |                                     |
esa_week_87  Unmatched |  .01422   .01627    -1.7            |  -1.21  0.225
                Matched |  .01334   .01435    -0.8    50.5    |  -0.47  0.639
                        |                                     |
esa_week_88  Unmatched |   .0132   .01618    -2.5            |  -1.77  0.077
                Matched |  .01248   .01321    -0.6    75.6    |  -0.35  0.728
                        |                                     |
esa_week_89  Unmatched |  .01269   .01601    -2.8            |  -1.99  0.047
                Matched |  .01197   .01271    -0.6    77.7    |  -0.36  0.717
                        |                                     |
esa_week_90  Unmatched |  .01269   .01584    -2.7            |  -1.89  0.058
                Matched |  .01197    .0129    -0.8    70.6    |  -0.45  0.651
                        |                                     |
esa_week_91  Unmatched |  .01269   .01578    -2.6            |  -1.86  0.063
                Matched |  .01197    .0127    -0.6    76.5    |  -0.36  0.722
                        |                                     |
esa_week_92  Unmatched |  .01371   .01565    -1.6            |  -1.17  0.242
                Matched |   .013     .0137    -0.6    63.4    |  -0.33  0.738
                        |                                     |
esa_week_93  Unmatched |  .01405    .0155    -1.2            |  -0.88  0.379
                Matched |  .01334   .01405    -0.6    50.7    |  -0.33  0.739
                        |                                     |
esa_week_94  Unmatched |  .01439   .01544    -0.9            |  -0.64  0.522
                Matched |  .01385   .01434    -0.4    53.3    |  -0.23  0.821
                        |                                     |
esa_week_95  Unmatched |  .01422   .01531    -0.9            |  -0.67  0.504
```

esa_week_96	Unmatched	.01439	.01526	-0.7		-0.54	0.592
	Matched	.01385	.01425	-0.3	54.6	-0.18	0.855
esa_week_97	Unmatched	.01439	.01504	-0.5		-0.40	0.689
	Matched	.01351	.01387	-0.3	43.9	-0.17	0.865
esa_week_98	Unmatched	.01422	.0148	-0.5		-0.36	0.717
	Matched	.01334	.01365	-0.3	47.1	-0.15	0.885
esa_week_99	Unmatched	.01405	.01475	-0.6		-0.44	0.661
	Matched	.01317	.01359	-0.4	40.5	-0.20	0.843
esa_week_100	Unmatched	.01371	.0146	-0.8		-0.55	0.580
	Matched	.01282	.01324	-0.3	53.8	-0.20	0.845
esa_week_101	Unmatched	.0132	.01459	-1.2		-0.87	0.387
	Matched	.01265	.01288	-0.2	83.8	-0.11	0.914
esa_week_102	Unmatched	.01337	.01469	-1.1		-0.82	0.413
	Matched	.01265	.01291	-0.2	80.5	-0.12	0.902
esa_week_103	Unmatched	.01303	.01455	-1.3		-0.95	0.343
	Matched	.01231	.01269	-0.3	74.9	-0.19	0.853
esa_week_104	Unmatched	.01269	.01457	-1.6		-1.17	0.241
	Matched	.01214	.01229	-0.1	91.9	-0.07	0.940
esa_week_105	Unmatched	.01303	.01439	-1.2		-0.85	0.393
	Matched	.01231	.01266	-0.3	74.3	-0.17	0.865
emph_week_1	Unmatched	.40657	.37624	6.2		4.67	0.000
	Matched	.40527	.40054	1.0	84.4	0.52	0.602
emph_week_2	Unmatched	.47309	.44171	6.3		4.72	0.000
	Matched	.47144	.46532	1.2	80.5	0.66	0.507
emph_week_3	Unmatched	.50102	.45955	8.3		6.22	0.000
	Matched	.49863	.49178	1.4	83.5	0.74	0.459
emph_week_4	Unmatched	.5176	.46985	9.6		7.15	0.000
	Matched	.51539	.50708	1.7	82.6	0.90	0.369
emph_week_5	Unmatched	.5264	.47527	10.2		7.65	0.000
	Matched	.52411	.51619	1.6	84.5	0.86	0.391
emph_week_6	Unmatched	.52945	.47883	10.1		7.57	0.000
	Matched	.52753	.5192	1.7	83.5	0.90	0.367
emph_week_7	Unmatched	.53064	.48064	10.0		7.48	0.000
	Matched	.52924	.52129	1.6	84.1	0.86	0.389
emph_week_8	Unmatched	.52945	.48088	9.7		7.27	0.000
	Matched	.52873	.51954	1.8	81.1	0.99	0.320
emph_week_9	Unmatched	.52657	.48061	9.2		6.87	0.000
	Matched	.52633	.51829	1.6	82.5	0.87	0.384
emph_week_10	Unmatched	.52048	.48005	8.1		6.05	0.000
	Matched	.52069	.51334	1.5	81.8	0.80	0.426
emph_week_11	Unmatched	.51879	.47923	7.9		5.92	0.000

```
emph_week_12  Unmatched |  .51574   .47835    7.5           |  5.59   0.000
              Matched   |  .51676   .50877    1.6    78.6   |  0.86   0.387
                        |                                   |
emph_week_13  Unmatched |  .51253   .47613    7.3           |  5.45   0.000
              Matched   |  .51385   .50524    1.7    76.3   |  0.93   0.351
                        |                                   |
emph_week_14  Unmatched |  .50829   .47468    6.7           |  5.03   0.000
              Matched   |  .50975   .50115    1.7    74.4   |  0.93   0.353
                        |                                   |
emph_week_15  Unmatched |  .50406   .47395    6.0           |  4.51   0.000
              Matched   |  .50564   .49692    1.7    71.0   |  0.94   0.346
                        |                                   |
emph_week_16  Unmatched |  .49814   .47253    5.1           |  3.83   0.000
              Matched   |  .49983   .49164    1.6    68.0   |  0.89   0.376
                        |                                   |
emph_week_17  Unmatched |  .49695   .47101    5.2           |  3.88   0.000
              Matched   |  .49829   .49019    1.6    68.8   |  0.88   0.381
                        |                                   |
emph_week_18  Unmatched |  .49238   .47008    4.5           |  3.34   0.001
              Matched   |  .49384   .4852     1.7    61.2   |  0.94   0.350
                        |                                   |
emph_week_19  Unmatched |  .48934   .46923    4.0           |  3.01   0.003
              Matched   |  .49094   .48258    1.7    58.4   |  0.90   0.366
                        |                                   |
emph_week_20  Unmatched |  .48951   .46806    4.3           |  3.21   0.001
              Matched   |  .49077   .48258    1.6    61.8   |  0.89   0.376
                        |                                   |
emph_week_21  Unmatched |  .48781   .46678    4.2           |  3.15   0.002
              Matched   |  .48906   .48088    1.6    61.1   |  0.88   0.377
                        |                                   |
emph_week_22  Unmatched |  .48646   .46485    4.3           |  3.24   0.001
              Matched   |  .48752   .48078    1.3    68.8   |  0.73   0.466
                        |                                   |
emph_week_23  Unmatched |  .48697   .46425    4.6           |  3.40   0.001
              Matched   |  .48786   .48128    1.3    71.0   |  0.71   0.477
                        |                                   |
emph_week_24  Unmatched |  .4829    .46261    4.1           |  3.04   0.002
              Matched   |  .48376   .47712    1.3    67.3   |  0.72   0.473
                        |                                   |
emph_week_25  Unmatched |  .48087   .46113    4.0           |  2.96   0.003
              Matched   |  .4817    .47446    1.5    63.3   |  0.78   0.433
                        |                                   |
emph_week_26  Unmatched |  .47698   .45996    3.4           |  2.55   0.011
              Matched   |  .47794   .47028    1.5    55.0   |  0.83   0.407
                        |                                   |
emph_week_27  Unmatched |  .47596   .45886    3.4           |  2.56   0.010
              Matched   |  .47692   .46886    1.6    52.9   |  0.87   0.383
                        |                                   |
emph_week_28  Unmatched |  .4741    .45893    3.0           |  2.28   0.023
              Matched   |  .47503   .46803    1.4    53.8   |  0.76   0.448
                        |                                   |
emph_week_29  Unmatched |  .47529   .45888    3.3           |  2.46   0.014
              Matched   |  .47606   .46942    1.3    59.5   |  0.72   0.472
                        |                                   |
emph_week_30  Unmatched |  .47664   .45812    3.7           |  2.78   0.005
              Matched   |  .47743   .47093    1.3    64.9   |  0.70   0.482
                        |                                   |
emph_week_31  Unmatched |  .47444   .4577     3.4           |  2.51   0.012
              Matched   |  .47538   .46878    1.3    60.6   |  0.71   0.475
                        |                                   |
emph_week_32  Unmatched |  .47139   .45664    3.0           |  2.21   0.027
```

emph_week_33	Unmatched	.47156	.45638	3.0		2.28	0.023
	Matched	.47281	.46556	1.5	52.2	0.79	0.432
emph_week_34	Unmatched	.47021	.45589	2.9		2.15	0.032
	Matched	.47144	.46405	1.5	48.3	0.80	0.423
emph_week_35	Unmatched	.47258	.45509	3.5		2.62	0.009
	Matched	.47332	.46626	1.4	59.6	0.76	0.444
emph_week_36	Unmatched	.47309	.45488	3.7		2.73	0.006
	Matched	.47367	.4665	1.4	60.6	0.78	0.437
emph_week_37	Unmatched	.47258	.45504	3.5		2.63	0.009
	Matched	.47298	.46604	1.4	60.4	0.75	0.452
emph_week_38	Unmatched	.47393	.45482	3.8		2.87	0.004
	Matched	.47435	.4671	1.5	62.1	0.79	0.432
emph_week_39	Unmatched	.47461	.45382	4.2		3.12	0.002
	Matched	.47486	.46669	1.6	60.7	0.89	0.376
emph_week_40	Unmatched	.47512	.45241	4.6		3.41	0.001
	Matched	.47503	.46682	1.6	63.8	0.89	0.373
emph_week_41	Unmatched	.47393	.45186	4.4		3.31	0.001
	Matched	.47384	.46686	1.4	68.4	0.76	0.450
emph_week_42	Unmatched	.47393	.45146	4.5		3.37	0.001
	Matched	.4735	.46607	1.5	67.0	0.80	0.421
emph_week_43	Unmatched	.47258	.45071	4.4		3.28	0.001
	Matched	.47196	.46377	1.6	62.6	0.89	0.375
emph_week_44	Unmatched	.46936	.45044	3.8		2.84	0.004
	Matched	.46905	.46134	1.5	59.2	0.84	0.403
emph_week_45	Unmatched	.46936	.45047	3.8		2.84	0.005
	Matched	.46871	.4608	1.6	58.1	0.86	0.391
emph_week_46	Unmatched	.46903	.45024	3.8		2.82	0.005
	Matched	.46888	.46123	1.5	59.3	0.83	0.407
emph_week_47	Unmatched	.46767	.44985	3.6		2.68	0.007
	Matched	.46785	.46055	1.5	59.0	0.79	0.429
emph_week_48	Unmatched	.47038	.44946	4.2		3.14	0.002
	Matched	.47042	.46275	1.5	63.3	0.83	0.406
emph_week_49	Unmatched	.46852	.44935	3.8		2.88	0.004
	Matched	.46871	.46146	1.5	62.2	0.79	0.432
emph_week_50	Unmatched	.46818	.44915	3.8		2.86	0.004
	Matched	.46854	.46166	1.4	63.9	0.75	0.456
emph_week_51	Unmatched	.46801	.44898	3.8		2.86	0.004
	Matched	.46837	.46213	1.3	67.2	0.68	0.499
emph_week_52	Unmatched	.4719	.44949	4.5		3.37	0.001
	Matched	.47196	.4656	1.3	71.6	0.69	0.491
emph_week_53	Unmatched	.4719	.44945	4.5		3.37	0.001

```
emph_week_54   Unmatched |  .47072    .45029     4.1          |  3.07   0.002
                Matched |  .47093    .46481     1.2    70.0   |  0.66   0.507
                        |                                     |
emph_week_55   Unmatched |  .47072    .45031     4.1          |  3.07   0.002
                Matched |  .47093    .46488     1.2    70.4   |  0.66   0.512
                        |                                     |
emph_week_56   Unmatched |  .46936    .45061     3.8          |  2.82   0.005
                Matched |  .46956    .46331     1.3    66.7   |  0.68   0.498
                        |                                     |
emph_week_57   Unmatched |  .47072    .4506      4.0          |  3.02   0.003
                Matched |  .47076    .4643      1.3    67.9   |  0.70   0.484
                        |                                     |
emph_week_58   Unmatched |  .47106    .44994     4.2          |  3.17   0.002
                Matched |  .47127    .46493     1.3    69.9   |  0.69   0.492
                        |                                     |
emph_week_59   Unmatched |  .4697     .44961     4.0          |  3.02   0.003
                Matched |  .47008    .4638      1.3    68.8   |  0.68   0.496
                        |                                     |
emph_week_60   Unmatched |  .46818    .44919     3.8          |  2.85   0.004
                Matched |  .46854    .46251     1.2    68.3   |  0.65   0.514
                        |                                     |
emph_week_61   Unmatched |  .46953    .44823     4.3          |  3.20   0.001
                Matched |  .46956    .46434     1.0    75.5   |  0.57   0.571
                        |                                     |
emph_week_62   Unmatched |  .46666    .44693     4.0          |  2.96   0.003
                Matched |  .46717    .46109     1.2    69.2   |  0.66   0.510
                        |                                     |
emph_week_63   Unmatched |  .46513    .44639     3.8          |  2.82   0.005
                Matched |  .4658     .45839     1.5    60.5   |  0.80   0.422
                        |                                     |
emph_week_64   Unmatched |  .4653     .44591     3.9          |  2.91   0.004
                Matched |  .46614    .4593      1.4    64.7   |  0.74   0.458
                        |                                     |
emph_week_65   Unmatched |  .46513    .44398     4.2          |  3.18   0.001
                Matched |  .4658     .45846     1.5    65.3   |  0.80   0.426
                        |                                     |
emph_week_66   Unmatched |  .46344    .44338     4.0          |  3.02   0.003
                Matched |  .46409    .45745     1.3    66.9   |  0.72   0.471
                        |                                     |
emph_week_67   Unmatched |  .46378    .44238     4.3          |  3.22   0.001
                Matched |  .46409    .45789     1.2    71.0   |  0.67   0.501
                        |                                     |
emph_week_68   Unmatched |  .46158    .44135     4.1          |  3.04   0.002
                Matched |  .46153    .45546     1.2    70.0   |  0.66   0.510
                        |                                     |
emph_week_69   Unmatched |  .46022    .44055     4.0          |  2.96   0.003
                Matched |  .45999    .45368     1.3    67.9   |  0.68   0.493
                        |                                     |
emph_week_70   Unmatched |  .45599    .43913     3.4          |  2.54   0.011
                Matched |  .45588    .4494      1.3    61.5   |  0.70   0.481
                        |                                     |
emph_week_71   Unmatched |  .45752    .43812     3.9          |  2.92   0.003
                Matched |  .45759    .45095     1.3    65.7   |  0.72   0.471
                        |                                     |
emph_week_72   Unmatched |  .45599    .43675     3.9          |  2.90   0.004
                Matched |  .45605    .45044     1.1    70.8   |  0.61   0.542
                        |                                     |
emph_week_73   Unmatched |  .45616    .43613     4.0          |  3.02   0.003
                Matched |  .4564     .45017     1.3    68.9   |  0.68   0.499
                        |                                     |
emph_week_74   Unmatched |  .45548    .43459     4.2          |  3.15   0.002
```

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emph_week_75   Unmatched | .45396    .4331    4.2          |  3.15  0.002
               Matched   | .45417   .44838    1.2    72.3  |  0.63  0.529
                         |
emph_week_76   Unmatched | .45125   .43237    3.8          |  2.85  0.004
               Matched   | .45144    .4463    1.0    72.8  |  0.56  0.576
                         |
emph_week_77   Unmatched | .45227   .43197    4.1          |  3.06  0.002
               Matched   | .45212   .44711    1.0    75.3  |  0.54  0.586
                         |
emph_week_78   Unmatched | .45074     .431    4.0          |  2.98  0.003
               Matched   | .45041   .44523    1.0    73.8  |  0.56  0.573
                         |
emph_week_79   Unmatched | .45074   .43086    4.0          |  3.00  0.003
               Matched   | .45041    .4454    1.0    74.8  |  0.55  0.586
                         |
emph_week_80   Unmatched | .45007   .43007    4.0          |  3.02  0.003
               Matched   | .44973   .44393    1.2    71.0  |  0.63  0.529
                         |
emph_week_81   Unmatched | .44838   .42952    3.8          |  2.85  0.004
               Matched   | .44802   .44159    1.3    65.9  |  0.70  0.484
                         |
emph_week_82   Unmatched | .44753   .42879    3.8          |  2.83  0.005
               Matched   | .44716   .44129    1.2    68.7  |  0.64  0.523
                         |
emph_week_83   Unmatched | .44668   .42807    3.8          |  2.81  0.005
               Matched   | .44631   .44101    1.1    71.6  |  0.58  0.564
                         |
emph_week_84   Unmatched | .44702   .42807    3.8          |  2.86  0.004
               Matched   | .44682   .44168    1.0    72.9  |  0.56  0.576
                         |
emph_week_85   Unmatched | .44499    .4271    3.6          |  2.70  0.007
               Matched   | .44528   .44038    1.0    72.6  |  0.53  0.594
                         |
emph_week_86   Unmatched | .44431   .42661    3.6          |  2.67  0.008
               Matched   | .44443   .43891    1.1    68.8  |  0.60  0.548
                         |
emph_week_87   Unmatched | .44533   .42607    3.9          |  2.91  0.004
               Matched   | .44528   .43941    1.2    69.5  |  0.64  0.523
                         |
emph_week_88   Unmatched | .44414   .42527    3.8          |  2.85  0.004
               Matched   | .44425   .43897    1.1    72.0  |  0.58  0.565
                         |
emph_week_89   Unmatched | .44516   .42506    4.1          |  3.04  0.002
               Matched   | .44528   .43965    1.1    72.0  |  0.61  0.540
                         |
emph_week_90   Unmatched | .44347   .42385    4.0          |  2.97  0.003
               Matched   | .44357   .43829    1.1    73.1  |  0.58  0.565
                         |
emph_week_91   Unmatched | .44279   .42369    3.9          |  2.89  0.004
               Matched   | .44306   .43791    1.0    73.1  |  0.56  0.575
                         |
emph_week_92   Unmatched |  .4394   .42261    3.4          |  2.54  0.011
               Matched   | .43947   .43411    1.1    68.1  |  0.58  0.559
                         |
emph_week_93   Unmatched | .43974   .42219    3.5          |  2.65  0.008
               Matched   | .43981   .43475    1.0    71.2  |  0.55  0.582
                         |
emph_week_94   Unmatched | .44008   .42162    3.7          |  2.79  0.005
               Matched   | .44032   .43435    1.2    67.7  |  0.65  0.515
                         |
emph_week_95   Unmatched |  .4389   .42113    3.6          |  2.69  0.007
```

emph_week_96	Unmatched	.43534	.41986	3.1		2.34	0.019
	Matched	.43553	.43016	1.1	65.3	0.59	0.558
emph_week_97	Unmatched	.43466	.41898	3.2		2.37	0.018
	Matched	.43485	.42895	1.2	62.4	0.64	0.520
emph_week_98	Unmatched	.43348	.41841	3.0		2.28	0.023
	Matched	.43365	.4276	1.2	59.9	0.66	0.509
emph_week_99	Unmatched	.43077	.41731	2.7		2.04	0.041
	Matched	.43126	.42608	1.0	61.5	0.57	0.572
emph_wee~100	Unmatched	.43196	.41668	3.1		2.32	0.021
	Matched	.43263	.42652	1.2	60.0	0.67	0.505
emph_wee~101	Unmatched	.43077	.41604	3.0		2.23	0.026
	Matched	.43109	.42538	1.2	61.3	0.62	0.533
emph_wee~102	Unmatched	.42891	.41587	2.6		1.98	0.048
	Matched	.42921	.42403	1.0	60.3	0.57	0.572
emph_wee~103	Unmatched	.42756	.41526	2.5		1.86	0.062
	Matched	.42784	.4228	1.0	59.0	0.55	0.582
emph_wee~104	Unmatched	.42739	.41487	2.5		1.90	0.058
	Matched	.4275	.42299	0.9	64.0	0.49	0.622
emph_wee~105	Unmatched	.42366	.41385	2.0		1.49	0.137
	Matched	.42425	.41848	1.2	41.3	0.63	0.528
sanction1one	Unmatched	.02167	.02335	-1.1		-0.83	0.404
	Matched	.02137	.02079	0.4	65.3	0.22	0.826
sanction1t~p	Unmatched	.00102	.00177	-2.0		-1.35	0.177
	Matched	.00103	.00105	-0.1	97.0	-0.04	0.970
sanction2one	Unmatched	.05619	.05859	-1.0		-0.76	0.446
	Matched	.05575	.05522	0.2	78.1	0.12	0.902
sanction2t~p	Unmatched	.0132	.0158	-2.2		-1.57	0.117
	Matched	.01334	.01325	0.1	96.8	0.04	0.968
sanction3one	Unmatched	.08006	.08581	-2.1		-1.54	0.124
	Matched	.0802	.07975	0.2	92.2	0.09	0.928
sanction3t~p	Unmatched	.05586	.05178	1.8		1.37	0.170
	Matched	.05489	.05199	1.3	28.8	0.70	0.485
totalsanct~s	Unmatched	.36561	.35766	0.8		0.61	0.540
	Matched	.36012	.34819	1.2	-50.2	0.65	0.515
sanctions2	Unmatched	1.1926	1.0587	2.2		1.76	0.079
	Matched	1.1498	1.0665	1.4	37.8	0.75	0.452
sanctionjsa	Unmatched	.18974	.17469	2.4		1.88	0.060
	Matched	.18735	.18016	1.2	52.2	0.62	0.537
sanctionesa	Unmatched	.00663	.00593	0.9		0.77	0.439
	Matched	.00615	.0067	-0.7	21.0	-0.40	0.690
sanctionempl	Unmatched	.12932	.1065	4.9		4.05	0.000

```
sanctionot~n  Unmatched |  .00259   .00307    -0.8          |  -0.59   0.556
                Matched |  .00253   .00269    -0.3   67.8   |  -0.16   0.874
                        |                                   |
sanctionpr~s  Unmatched |  .15814   .11687     6.4          |   5.29   0.000
                Matched |  .15307   .14539     1.2   81.4   |   0.62   0.536
                        |                                   |
    empprog   Unmatched |  .22664   .17707    12.4          |   9.65   0.000
                Matched |  .22298    .2224     0.1   98.8   |   0.08   0.939
                        |                                   |
 timeempprog  Unmatched |  .13822   .10888    10.3          |   8.06   0.000
                Matched |  .13624   .13498     0.4   95.7   |   0.23   0.816
                        |                                   |
 spellsempp~g Unmatched |  .25322   .27038    -3.5          |  -2.58   0.010
                Matched |  .24983   .24878     0.2   93.9   |   0.12   0.903
                        |                                   |
    progempl  Unmatched |  .05254   .03884     8.6          |   6.86   0.000
                Matched |   .0518   .05099     0.5   94.0   |   0.27   0.790
                        |                                   |
    progempl  Unmatched |  .05254   .03884     8.6          |   6.86   0.000
                Matched |   .0518   .05099     0.5   94.0   |   0.27   0.790
                        |                                   |
    progjsa   Unmatched |  .08123   .06301     9.8          |   7.71   0.000
                Matched |  .08009   .07963     0.2   97.5   |   0.13   0.897
                        |                                   |
   otherbenf  Unmatched |  .03199   .02978     1.3          |   0.97   0.333
                Matched |  .03129    .0304     0.5   59.5   |   0.28   0.780
                        |                                   |
timeotherben  Unmatched |  .01636   .01496     1.3          |   0.95   0.340
                Matched |  .01592   .01526     0.6   53.0   |   0.32   0.747
                        |                                   |
spellsothe~n  Unmatched |  .03893   .04016    -0.5          |  -0.39   0.694
                Matched |   .0383   .03785     0.2   63.3   |   0.11   0.912
                        |                                   |
otherbenempl  Unmatched |  .00413   .00335     1.6          |   1.28   0.200
                Matched |  .00386   .00388    -0.0   97.3   |  -0.02   0.981
                        |                                   |
 otherbenesa  Unmatched |  .00064    .0005     1.2          |   0.94   0.348
                Matched |  .00053   .00063    -0.9   30.5   |  -0.44   0.661
                        |                                   |
 otherbenjsa  Unmatched |  .00181   .00129     3.2          |   2.62   0.009
                Matched |  .00159   .00162    -0.2   94.1   |  -0.10   0.920
                        |                                   |
otherbenpr~s  Unmatched |  .00094   .00061     2.1          |   1.80   0.071
                Matched |  .00083    .0008     0.2   88.6   |   0.13   0.895
                        |                                   |
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----------------------------------------------------------------
            Summary of the distribution of the abs(bias)
----------------------------------------------------------------


                         BEFORE MATCHING
----------------------------------------------------------------
      Percentiles      Smallest
  1%    .0958576       .0317409
  5%    .4694125       .0820619
 10%    .8422438       .0845356       Obs                   370
 25%    2.013278       .0958576       Sum of Wgt.           370

 50%    3.819357                      Mean             4.637478
                       Largest        Std. Dev.        4.616372
 75%     6.30564       20.98052
 90%    8.165325       27.78119       Variance         21.31089
 95%    10.01207        33.0536       Skewness         4.787894
 99%    20.98052       53.50453       Kurtosis          41.9243
----------------------------------------------------------------


                         AFTER MATCHING
----------------------------------------------------------------
      Percentiles      Smallest
  1%    .0375711       .0142628
  5%    .0696066       .0266102
 10%    .1294407       .0326227       Obs                   370
 25%    .3062014       .0375711       Sum of Wgt.           370

 50%    .7638964                      Mean             .8005155
                       Largest        Std. Dev.        .5258627
 75%    1.228408       1.951454
 90%    1.489305       2.177763       Variance         .2765315
 95%    1.639442       2.274712       Skewness         .2750984
 99%    1.951454       2.485725       Kurtosis         2.140334
----------------------------------------------------------------


----------------------------------------------------------------
     Sample |    Pseudo R2     LR chi2       p>chi2
------------+---------------------------------------------------
  Unmatched |      0.121       5537.64        0.000
    Matched |      0.002         38.59        1.000
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7.6 NIESR Review of DWP's Early Impact Analysis of UC

Review of: *Estimating the employment impacts of Universal Credit: Preliminary Analysis*

Universal Credit Analysis Division
January 2015

Rebecca Riley, National Institute of Economic and Social Research, 26 January 2015

This report by DWP (draft 16 January) aims to provide a first assessment of the employment impacts of Universal Credit (UC). More specifically, it considers the impact of UC relative to the pre-existing Jobseeker's Allowance (JSA) regime on the probability that individuals are employed in the initial months after claim start. These effects are identified by comparing the outcomes of new UC claimants in UC Pathfinder offices to the outcomes of a control group consisting of new JSA claimants in a set of comparator offices that were largely unaffected by UC.

The main finding is that new UC claimants July 2013 - April 2014 were 5 percentage points more likely to be in work in the four months after their claim start compared to the control group.

This brief review considers the extent to which this preliminary analysis is methodologically robust and appropriate, comments on the main strengths and limitations of the work and suggests options for improving the analysis where possible. Further comment and suggestion that has been provided in email correspondence with DWP is not included here, but will also inform the on-going evaluation of UC's labour market impacts.

Main methodology

The identification strategy exploits geographical and time variation in the roll-out of UC. The analysis adopts a multi-stage matching approach to selecting a control group that is intended to measure the counterfactual outcome for new UC claimants (the treated).

In an initial stage the dataset is restricted to include only new "Pathfinder eligible" (PE) claims (over the relevant time period). This includes new UC claims in the Pathfinders and new JSA claims that, according to the administrative data that is available, should have been eligible for UC had they occurred in the Pathfinders.

In a second stage comparator offices are selected that most resemble Pathfinder offices in the two years prior to the initial UC roll-out. For each Pathfinder office a set

of offices is selected that minimise differences in average off-flow rates for PE new claims, the sum of mean squared deviations in off-flow rates for monthly cohorts of PE claims (with greater weight attached to recent cohorts), differences in the linear trend in off-flow rates for new PE claims, and differences in a propensity score that takes into account key demographic characteristics of new PE claims.

In a third stage a probit model is used to estimate the probability of treatment (of being a UC claim) using the sample of new PE claims in the comparator offices and new UC claims in Pathfinder offices. This is used to rebalance the treatment and control groups on key covariates, such as very detailed employment and claim histories, which either influence employment outcomes directly or which proxy for unobserved factors that influence employment outcomes.

The analysis clearly pays careful attention to finding a suitable control group, and manages to balance observable individual level characteristics between the treated and the controls. (The report does not show the underlying statistical analysis used to arrive at a set of comparator offices, but explains the process in detail.) This is crucial to identifying the policy effect.

The report discusses some of the data issues and potential threats to identification that arise and explores variations of the main model to assess the robustness and validity of the results. This is important.

Overall, the main methodology seems appropriate to the task at hand in the sense that it seems a very reasonable way to proceed given the available data. The analysis appears to be thorough, carried out competently and with attention to detail. The estimated magnitude of the UC impact (relative to JSA) in but the first few months following a new claim is non-negligible: the 5 percentage point difference in the probability of being employed corresponds approximately to a 12% increase in the likelihood of being in work. The question is whether we can be sure that these findings are wholly down to changes in claimants' behaviour in response to UC. Additional discussion and supplementary analysis is likely to be helpful in further gauging the extent to which the impact estimates here are likely to be capturing policy (UC) effects alone.

Potential biases to the estimated employment impacts

1. One of the limitations of the analysis stems from differences in the information available to identify the treatment and control groups. UC was initially rolled out to a select group of people. The administrative data allows analysts to proxy these selection criteria to identify new PE claims in a set of comparator areas that can then be used to identify the counterfactual. In order for the control group to constitute a good counterfactual it is important that these PE selection criteria match the UC selection criteria closely in so far as they affect the outcome measure of interest.

The information that is available to identify PE claims is discussed in the report. Importantly, this does not include all the information required to identify whether or not individuals qualify for UC.[21] For example, individuals will not qualify for UC in the initial roll-out phases if they are pregnant or if they do not have a bank account. Does this matter? It matters in so far as these factors are likely to affect (directly or by proxy) individuals' likelihood of moving into work (or reporting work) after claim start independently of UC and are not taken into account when selecting and matching the control group. What can then happen is that the effect of UC becomes conflated with the effect of having a particular set of (unobservable) characteristics that mean an individual is more or less likely to move into work.

In discussion, DWP suggests that half the group of new PE claims in Pathfinder offices, identified using the information available in the administrative data, do not claim UC. Thus, there clearly are a significant set of selection criteria that the administrative data fail to capture. Sensitivity analysis in the report suggests that new UC claims in Pathfinder offices and new non-UC PE claims in Pathfinder offices cannot be matched on the covariates used. The resulting "impact" estimate on the probability of employment is significantly higher when new UC claims are compared to this group of new non-UC PE claims in Pathfinder offices. This illustrates that UC claimants are a distinct group, having (observed, and potentially and of more concern unobserved) characteristics associated with higher employment probabilities. The question is whether those factors that can be measured for both UC claimants in the Pathfinder offices and PE JSA claims in the comparator offices are sufficient to capture the impacts of unobservable characteristics of UC claimants on employment outcomes. If not, there is some concern that there may be an upward bias to the central estimates.

In this regard, it is encouraging that both Pathfinder office groups (UC and PE non-UC) can be balanced on the key covariates used when considering comparator offices, and that the difference in outcomes between the non-UC group in the Pathfinder offices and the matched controls is zero (rather than negative, as might be expected given the potential selection bias discussed above). The balancing of observable characteristics between both the UC and PE non-UC groups in Pathfinder offices with a control group from the comparator offices is possible because of the large number of claims from which it is possible to select the control group, and, together with the finding of no difference in outcomes between the PE non-UC group in Pathfinder offices and the matched control group, may suggest that the combination of covariates that are used in the matching process are sufficient to capture the effects on outcomes of the unobservable characteristics that determine selection into UC.

The report also estimates the difference in employment probabilities between the entire PE group in Pathfinder offices and matched PE claims from the comparison areas. This difference is positive and statistically significant, further supporting the

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[21] Full details of the selection criteria are set out in a Social Security document "The Universal Credit (Transitional Provisions) Regulations 2013" provided by DWP.

suggestion in this report that UC has had a positive effect on employment probabilities. This difference will underestimate the impact of UC on employment probabilities because not all PE claims in Pathfinder offices receive UC, but there is much less concern that this estimate suffers from the types of selection bias discussed above.

A difference-in-differences approach is often used when there is a concern that selection into the policy (here UC) on unobservable characteristics influence the outcome of interest. But, this approach is unlikely to address in full the potential selection problem discussed above. This is because it is not feasible to replicate the exact UC selection criteria in past cross-sections of the data and because (even if a sufficient sample could be found using the longitudinal data) individual fixed effects will not capture those UC selection criteria that vary across time.

These issues merit further discussion in the report. In particular, there could be more discussion of what the (important) selection criteria are that are unobservable in the administrative data and how (if at all; in what direction) they may influence the estimates that are presented.

2. Another potential bias to the estimates arises because the data on outcome measures differs between the treatment and control groups. The report suggests that JSA claims are processed slightly faster than UC claims. This means that, on average, for two identical claims (one JSA and one UC), it will appear that the JSA claimant took a longer time to find work than the UC claimant. In other words, compared to the JSA claimant it will look like the UC claimant found work more quickly measured relative to the start of their award, even though there may be no difference between the time it took to find work measured relative to the point in time of first contact.

Discussion with DWP suggests these measurement differences are relatively minor. For example, after 5 days 64% of JSA claims are processed as compared to 57% of UC claims. 95% of both types of claim are processed within 10 days. Because the samples are quite large even small differences are likely to be statistically significant and could potentially bias the employment impact estimates upwards. One option that appears to be possible with the data that is now available is to measure claim duration relative to first contact. This would be a useful additional robustness check.

3. There is no correction for current macro trends. Comparator offices are selected on the basis of similarity to the pathfinders in the past. Is there any concern that developments in the local areas where pathfinder offices are situated are unusual post UC implementation (i.e. distinct from developments elsewhere)? It would be interesting at this early stage, when macro trends are unlikely to be endogenous to the policy, to check the sensitivity of the estimates to including in the matching some local area characteristics post UC implementation. Alternatively, further assessment of "common macro trends" in the Pathfinder and comparator offices post UC implementation might be assessed by estimating similar "treatment" effects for

groups of claimants in these offices that are not directly affected by UC. Common trends between Pathfinder and comparator offices should imply that these "treatment" effects are statistically no different from zero.

4. The national roll-out of the Claimant Commitment (CC) during the evaluation period, which will affect JSA claimants in the comparator areas, makes it less clear what is being evaluated. As the report suggests, the estimated impacts of UC will measure the effect of UC against a hybrid of the legacy system plus CC rather than the effect of UC against the legacy system. If CC is an important (and effective) part of UC this means estimated UC impacts may be biased downwards. Is there a possibility for using the initial roll-out window for CC to get a sense of how important (or not) this bias is? E.g. is it possible to conduct sensitivity analysis excluding claims that are affected by CC from the comparator group (and does this change the estimates)?

Additional outcome variables

The analysis considers impact estimates on the probability of being in work at different points in time after claim start. Additional analysis considers impact estimates on the number of days in work and earnings. When considering additional outcomes such as earnings (which depend on the time in work as well as pay) it might be worth considering additional covariates in the matching process (e.g. past earnings, skill composition of the local area) if this is possible with the data that is available.

Hours worked are not considered. This is likely due to data limitations, but, given the way that UC is intended to function it is an important omission and should therefore be discussed.

Conclusions

This report details a carefully conducted evaluation of the early impacts of UC on the employment outcomes of new UC claimants in UC pathfinders.

The methodology is informed by a feasibility study by the Institute for Fiscal Studies (IFS) and an initial evaluation plan for the UC pathfinders set out by DWP that was peer-reviewed by IFS. The main methodology seems appropriate, but there are potential sources of bias to the estimates that might be further explored and discussed.

The report discusses a number of threats to identification and conducts sensitivity checks to assess the robustness of the estimated employment impacts. This is very welcome.

Sources of bias to the estimates that merit further investigation stem from measurement problems:

- selection into UC on factors that are observable for the treatment group only;
- potential differences in the measurement of outcomes between the treated and control groups arising because of differences in the processing of UC and JSA claims;

or concern potential differences between the pathfinders and comparator areas:

- differential macro trends after policy implementation;
- partial implementation of the policy in comparator offices (the national roll-out of the Claimant Commitment).

This review recommends some further discussion and analysis to consider the likely magnitude and direction of potential biases.

There are several reasons why the employment impacts in this report might not be representative of the employment impacts of UC: the impact estimates concern the early stages of UC, for a particular group of claimants, in a confined set of offices, concern in-flows to work only, and do not take into account wider effects (which are impossible to gauge at this early stage). This is not a criticism of the analysis presented here, but is simply to be borne in mind when interpreting the estimates.

Mention of the likely internal and external validity of the estimates should be given more prominence in the executive summary.

DWP Response to NIESR Review

Summary

DWP is grateful for NIESR's review. Overall the review is positive. This is consistent with the positive feedback we received from our expert evaluation group, which advises us on our whole evaluation strategy for UC. It is also consistent with the Institute for Fiscal Studies' review of our proposed approach.

This note summarises the further analysis we have undertaken following NIESR's recommendations, and outlines additional work we will be progressing.

1. Identifying Pathfinder Eligibility

The data we use enables us to capture the vast majority of eligibility criteria. In addition we match on a large number of characteristics that are likely to be related to the eligibility criteria we cannot directly measure. Our analysis includes the factors that past research has shown to be the most important for getting reliable estimates. In particular, we include very detailed information on people's past employment and benefit claims to control for the affect that these have on the outcomes new claimants achieve.

We have also conducted a range of sensitivity analysis to explore this issue, which are described more fully in the report. These sensitivity analyses suggest that the estimates are likely to be robust. For example, the matching work balances the comparison and treatment group very well and we find no impact when we compare the claimants we think should be claiming UC but are still claiming JSA with the comparison group in comparator areas.

We will continue to improve the data and carry out further sensitivity analysis as the volume of claims increases. Whilst we cannot capture everything we believe that we have captured the most important things and the sensitivity analyses suggest that the results are robust.

2. Consistent Outcome Measures

We analysed the elapsed time between the claim date and the event date when a claims entitlement has been established. This suggests that whilst JSA claimants are currently processed slightly faster than UC the overall profile is broadly similar and the impact of any difference is likely to be negligible.

We are continuing to enhance the available for the evaluation and in the future we should be able to carry out further sensitivity analysis around this issue by measuring

outcomes from the claim date as well as from the date the award started.

3.   Macro trends.

We conducted a range of sensitivity analysis which suggests this is unlikely to be a significant issue. For example, we re-estimated impacts using alternative comparator areas and separately only controlling for individuals' labour market and benefit claim histories. All the sensitivity analysis generated very similar estimates suggesting that there were not differential macro trends in the Pathfinder areas.

We are continuing to develop the data and will be able to explore this issue further by comparing the outcomes of claimants not directly affected by UC over the same period in the Pathfinder and comparator offices.

4.   JSA Claimant Commitment (CC) roll-out.

We estimated the impact of UC separately for claims made before the JSA claimant commitment had been introduced from the impact on claims made after the JSA claimant commitment had been introduced. The results suggest that the impact of UC is stable and is not affected by the roll out of the JSA claimant commitment.

5.   Additional Outcome Variables

The earnings information is obtained from a relatively new data source – the Real Time Information system. This means we do not have historical earnings information that we can use in the matching for this early cohort of new UC claimants. However, we will be using similar methods to evaluate the impact of UC on more claimants in more areas and so it should become feasible to start using historical RTI data in future. We will continue to explore and draw on more data sources to refine our matching process in the future.

6.   External validity

We accept that impacts in this report might not be representative of the employment impacts of UC overall: the impact estimates concern the early stages of UC, for a particular group of claimants, in a small number of offices, concern in-flows to work only, and do not take into account wider effects (which are impossible to gauge at this early stage). We acknowledge this in the report. We will use similar methods to evaluate the impact of UC on more claimants in more offices and on more outcomes as the scale and scope of UC grows.