Department
for Work &
Pensions

# Universal Credit and Earnings Progression

Evidence from a Regression Discontinuity Design

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# Universal Credit and Earnings Progression: Evidence from a Regression Discontinuity Design

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## Summary

Many Universal Credit claimants are in employment. An important question for the department is how working claimants can be supported to increase their earnings. Using the presence of an administrative threshold that determines the level of conditionality applied to claimants, this paper addresses this question via a regression discontinuity design approach.

Claimants on single contracts with earnings below the administrative threshold are placed in the Intensive Work Search regime. They are required to undertake more work search activity than claimants with earnings above the threshold who are placed in the Light Touch regime, who are not subject to work search requirements. By comparing the change in earnings of claimants who enter Universal Credit *just* below and *just* above the threshold, this paper estimates the impact of work search requirements on the earnings progression of working claimants. Claimants who enter Universal Credit very close to the threshold are likely to have similar characteristics, making a comparison of the two groups possible.

We estimate point-in-time impacts as well as showing how the effect changes over time. The headline results show that claimants who began Universal Credit just below the threshold in the Intensive Work Search regime experienced higher earnings progression in the months afterwards, compared to those who joined just above in the Light Touch regime. After 12 months, the former group experienced approximately £100 higher earnings progression per month compared to the latter group, on average. Claimants entering Universal Credit just below the threshold do not have more volatile earnings or spend more months in unsubsidised unemployment.

Overall, this suggests that regular support via the Jobcentre has positive effects on the future earnings outcomes of working claimants.

## Introduction

Universal Credit (UC) comprises of several labour market regimes. Which regime a claimant is assigned to determines how much activity they are expected to undertake

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to search for work. Previous research by the department has found that this activity can have positive impacts on claimants' labour market outcomes (DWP, 2015; DWP, 2018a; DWP, 2018b). Claimants in the Intensive Work Search regime are usually unemployed, and are required to undertake the most activity (up to 35 hours per week) to either search for work or search for more work/higher earnings (else their benefit award can be reduced). On the other hand, claimants in the Light Touch regime are mostly already working, and are therefore not subject to these requirements. The boundary between these two regimes is the Administrative Earnings Threshold (AET). It is a monthly earnings threshold that determines whether claimants are in the Intensive regime or the Light Touch regime. Claimants on single contracts are placed in Light Touch if their earnings are above the AET; they are placed in Intensive if their earnings are below the AET. For claimants in couple contracts, the combined earnings of both individuals are taken into account.

When it was introduced, the AET was linked to Jobseeker's Allowance (JSA) rates, so the AET increased when JSA rates increased. However, in the time since the AET's introduction, the National Living Wage (NLW) has grown at a faster rate. As a result, the implied hours of work needed to earn above the AET had been falling. For claimants in single contracts, the implied number of weekly hours worked at the NLW required to earn at the AET decreased from nearly 12 to just under 9.

This 'erosion' raised the question of whether the AET should be raised, and whether it would be effective to do so. One key outcome of interest for the department is earnings progression. Raising the AET would largely impact claimants who are already in work but on low earnings by bringing them into the Intensive regime. An important question is therefore whether the Intensive regime has a positive impact on earnings progression for claimants who have low incomes.

The presence of the earnings threshold lends itself to a regression discontinuity design (RDD). RDD is a type of quasi-experiment, often used when a treatment or policy is given to individuals who, on the basis of some characteristic, lie on one side of a cut-off/threshold. Claimants who lie below the AET are in Intensive Work Search and therefore undertake work search activity (such as meetings with Work Coaches), while claimants who lie above the AET are in Light Touch and are not required to undertake these actions. In this context, the basic idea of this research is to compare the earnings outcomes of claimants who lie *just* above and *just* below the threshold. The RDD approach provides a framework to estimate the causal impact of being below the threshold.

Using administrative data for the UK, we find that claimants who entered Universal Credit just below the AET saw approximately £100 higher earnings progression per month after a year compared to claimants who entered just above, on average. In addition, using an RDD event study strategy, we show how this impact evolves over time. We find no significant evidence that this increase in earnings progression is at the cost of more volatile earnings. Subgroup analysis suggests that men and younger claimants may experience higher earnings progression as a result of entering UC just under the AET, compared to women and older age groups. Robustness checks indicate that the results are not sensitive to key choices or modelling assumptions.

## Literature review

The existing literature on the effect of work search requirements and conditionality on the labour market outcomes of benefit claimants largely focuses on the unemployed. There has been comparatively little research into the impact on those who are already working with low earnings.

Theoretical models predict that unemployment duration decreases as the level of work search requirements increase. As Arni and Schiprowski (2019) describe, without requirements, claimants provide a voluntary level of search effort. Search requirements introduce a minimum level of effort that claimants should supply. The possibility of a reduction in benefit award if the requirements aren't met represents a potential cost to claimants. If a claimant's voluntary effort is lower than the requirement, then the presence of the requirement increases the claimant's effort if the cost of doing so is exceeded by the benefits (prevention of benefit reduction and an increase in job offers). If greater search effort increases the job offer rate, the claimant's duration on benefit is expected to decrease.

Empirical studies that focus on the unemployed have found mixed evidence of the effectiveness of work search requirements. For example, a DWP (2015) report analysed the results of a randomised controlled trial (RCT) of JSA claimants. When claimants attended the Jobcentre, they were required to sign, which involved reviewing their work search activity, along with identifying areas of support. The trial tested whether weekly signing, rather than fortnightly, affected the amount of time claimants spent on benefits. The report found that claimants in the weekly signing group spent 2.6 fewer days on DWP benefits compared to the fortnightly group, on average over the following year.

In addition, a later DWP (2018a) study also investigated the impact of weekly Work Search Review (WSR) meetings with Work Coaches, compared to fortnightly meetings for JSA claimants. Again, an RCT was implemented in order to ensure the comparison between the treatment and control groups was unbiased. The results show that claimants assigned to weekly WSRs spent around 6.4 fewer days on benefit, on average, and 7.3 days more in employment over the year following claim, compared to the fortnightly group.

Petrongolo (2009) studied a reform from 1996 that increased job search requirements for JSA claimants. In contrast to the results from the DWP (2015; 2018a) reports, Pentrongolo found that claimants who entered unemployment shortly after the reform were more likely to move into health-related benefits and less likely to be employed in the subsequent year (compared to those who entered 6 months earlier). The same reform has also been studied by Manning (2009) and Morescalchi and Paruolo (2020). Both papers found that increased conditionality made claimants more likely to exit unemployment benefits into unsubsidised job-search; however, the latter paper shows that these outflows were primarily composed of individuals with high levels of job search. The authors suggest that this is not a positive outcome, since these committed job seekers are more likely to be liquidity constrained, but do not receive the insurance of unemployment benefit.

Arni and Schiprowski (2015) use Swiss data to show that when job search requirements exceed the claimant's search effort without requirements, the rate at which claimants find employment increases. However, the authors show that the requirements also increase non-compliance and sanction rates, and decrease job stability. In a 2019 paper, the same authors use variation in caseworker leniency to estimate the effect of additional required job applications. They find that an extra job application reduces the duration of unemployment, with small decreases in job stability, and no effect on wages.

There has been much less research into the effects of work search requirements on individuals who are already in work. One example of this is the DWP (2018b) In-Work Progression RCT. Between March 2015 and March 2017, claimants who entered Universal Credit in the Light Touch regime were randomised into one of three groups:

- Frequent support group: eight weeks after the initial appointment, claimants met with their Work Coach every fortnight.
- Moderate support group: same as above, but meetings are every eight weeks, instead of fortnightly.
- Minimal support group: claimants receive an initial telephone appointment to identify voluntary actions, and then receive telephone appointments every eight weeks.

On average, claimants in the Frequent and Moderate groups experienced modest increases in earnings progression of £5.25 and £4.43 per week, respectively, compared to those in the Minimal group. The report also investigated whether the effects vary for different subgroups, finding that most subgroups experienced similar treatment effects. This suggests that work search requirements can have positive effects on the labour market outcomes of claimants who are in employment. However, the sample analysed in this report consists of claimants along the entire earnings distribution of the Light Touch group. This is important to bear in mind when considering the potential impact of an increase in the AET, which would affect claimants at the lowest part of the Light Touch distribution. Therefore, inference from the In-Work Progression RCT about the effects of such a policy change is challenging. For instance, claimants further up the earnings distribution may differ in ways that affect their response to work search requirements (e.g. they may have higher levels of education, or be in more stable part-time employment). As a result, providing evidence on the potential impact of changing the AET *on the group of interest* is a clear priority for DWP.

By investigating the impact of conditionality on the employment outcomes of claimants with earnings (close to the AET), this paper not only addresses an important evidence question for the department, but also a gap in the academic literature on work search requirements.

## Data and methodology

*Data*

We combine UK administrative data on Universal Credit claimants held by DWP with Real Time Earnings data provided by HMRC. This allows us to determine the individual's Universal Credit group (Intensive Work Search or Light Touch) in a given month or assessment period, and their monthly gross employee earnings. While a claimant is on UC, their gross employee earnings in each UC assessment period are also observed, which determine whether they are above or below the AET. These are the key data sources necessary for this analysis. Combining the data produces a panel dataset in which we observe claimants for 16 months before they join UC and 18 months after. Some businesses are not required to operate PAYE if they meet certain exemption criteria (i.e. they are exempt if all their employees are paid less than £123 a week, none of their employees receive expenses and benefits, and none of their employees have another job or get a pension). These exemption criteria are very narrow, so in practice the vast majority of employees are captured by the data, and this is a minor limitation.

After linking these sources of data, we observe the following claimant information: monthly gross earnings, age, sex, Jobcentre site, date of entry into UC, UC payment, monthly income tax, number of children eligible for the child element of UC, whether the claimant has a health condition that limits their ability to work, and type of housing. Unfortunately, we do not observe the number of hours that claimants work. We therefore cannot decompose any estimated effects into impacts on hours and hourly wages, which could limit the level of detail on mechanisms that the results can uncover without making strong assumptions.

Using data on online job vacancies from Adzuna, and claimant count statistics from the ONS, we create a monthly measure of local labour market tightness, defined as:

$$\theta_{lt} = \frac{v_{lt}}{u_{lt}}$$

Where $v_{lt}$ and $u_{lt}$ are the number of online vacancies and the claimant count in local authority $l$ at time $t$, respectively. To test whether the effects of the policy vary along this dimension, we add this measure into the dataset by linking local authorities to Jobcentres.

Adzuna's online job vacancies data contains some limitations. Namely, not all job adverts are posted online – some employers may recruit via others means, such as word of mouth, which could be more prevalent for low paying roles. Additionally, not all online job vacancies include a location. This means that around 21% of job adverts are not included in the local authority level data. ONS (2021) discuss the use of Adzuna data in further detail on their website (see *References*). The use of this experimental data does not form a large part of this paper; therefore, while these limitations are important to keep in mind, they do not affect the main conclusions.

When calculating claimants' earnings progression, we remove observations with negative values of monthly earnings, as these are unlikely to reflect an individual's true employee earnings in that period (negative monthly earnings can appear for reasons such as retrospective data and tax corrections). The proportion of earnings observations that are negative is very small (less than 0.2%), so this does not significantly affect the data. Nevertheless, this results in an unbalanced panel, since the number of observations with negative values for earnings varies from month to month. We also test the robustness of the results to removing individuals who have ever had a negative earnings value, which restores the balanced panel. For the point-in-time results, the earnings progression outcome variable is the difference between a claimant's monthly gross earnings 12 months after entering UC and their gross earnings for their first month of UC. For the event study estimates, we utilise the full panel component of the dataset and calculate the dependent variable as the difference between a claimant's monthly gross earnings for a given number of months after joining UC and their gross earnings in their first month of UC.

*Methodology*

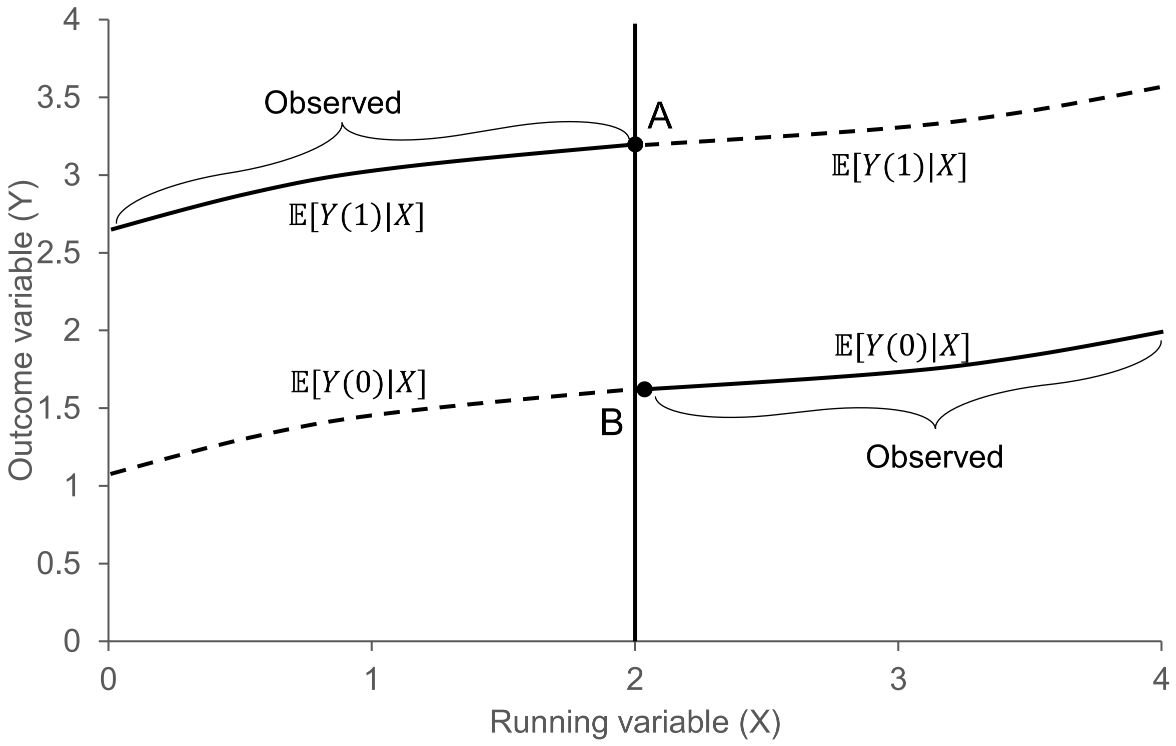


Figure 1: Stylised representation of RDD. Adapted from Lee and Lemieux (2014).

RDD is a quasi-experimental technique that is used when a threshold or cut-off determines whether an individual receives a particular intervention or not. The AET is an example of such a threshold, as it determines whether claimants are placed in the Intensive Work Search regime or the Light Touch regime. Informally, the intuition behind the regression discontinuity design is that as one zooms in further on the threshold, the two groups either side are more likely to be comparable. In technical terms, the key assumption for internal validity in regression discontinuity designs is one of 'continuity' (Hahn, Todd and van der Klaauw, 2001). A visual representation of this can be seen in Figure 1, which is adapted from Lee and Lemieux (2014). The potential outcomes of individual $i$ are denoted by $Y_i(1)$ and $Y_i(0)$, which represent the outcome of the individual in the treated state and the untreated state, respectively. To the left of the threshold, we observe $\mathbb{E}[Y_i(1)|X_i = x]$, but we don't observe this to the right of the cut-off, since individuals above the threshold are not treated. Similarly, to the right of the threshold we observe $\mathbb{E}[Y_i(0)|X_i = x]$, but not $\mathbb{E}[Y_i(1)|X_i = x]$. We're interested in the difference between $\mathbb{E}[Y_i(1)|X_i = x]$ and $\mathbb{E}[Y_i(0)|X_i = x]$, but we don't observe both at the same point. Instead, we can estimate this difference *at the cut-off*, which is the average treatment effect (ATE) at the cut-off.

For this difference to be equal to the ATE at the cut-off (the LATE), $\mathbb{E}[Y_i(1)|X_i = x]$ and $\mathbb{E}[Y_i(0)|X_i = x]$ should be continuous at the cut-off. In other words, RDD requires that the only variable that is discontinuous (jumps) at the threshold is treatment status (Cattaneo, Titiunuk, and Vasquez-Bare, 2020) – observable and unobservable characteristics that affect outcomes should not jump discontinuously at the cut-off. In turn, this requires that the characteristic that determines whether someone lies above or below the cut-off cannot be perfectly manipulated by individuals. If this was the case, then we might observe changes in outcomes at the cut-off that are not due to the treatment itself and mistakenly attribute this to the treatment. Otherwise, if the key assumption of continuity is satisfied, we can estimate a 'local average treatment effect' by comparing the outcomes of individuals who lie just either side of the cut-off.

This assumption is important in the context of the AET. If claimants can perfectly manipulate their earnings to be just above or below the AET, the RDD approach could be invalidated – we may not be comparing like with like, and the estimates could be biased. While a few claimants may be aware of the AET and choose to earn just above it, we believe that this is unlikely to be true for the vast majority, and conversations with Work Coaches have supported this view. Manipulation is also unlikely because job offers usually represent a fixed number of hours and a fixed hourly wage, making it difficult for individuals to perfectly control their earnings to be slightly above or below the threshold. However, to further address this concern, we select the sample to only include claimants who enter UC for the first time between May 2017 and February 2019. The rationale here is that claimants are less likely to be aware of the AET prior to joining UC. Selecting those who are first-time claimants should therefore help to allay concerns that self-selection is at play. In addition, selecting first-time claimants from May 2017 onwards avoids contamination of the control group, since recruitment of Light Touch claimants to the In-Work Progression RCT (see *Literature review* for details) ended in March 2017.

This strategy also allows for up to 12 months of earnings outcomes to be observed before any effects of Covid-19 take hold[1]. Outcomes over a longer timeframe can be observed, but with the caveat that an increasing proportion of the sample become affected by Covid-19. To avoid the complications of having two thresholds, the sample is limited to claimants on single contracts. It should also be noted that the AET for single contracts was increased from £338 per month to £343 per month in April 2020. This could affect claimants in the control group who were still on UC at this point and had earnings between £338 and £343 per month. However, there are very few of these instances (fewer than 10), so contamination of the control group via this source is

[1] In addition to the two years in which claimants enter the sample, there is a further year in which outcomes are tracked. The last observation on 12-month outcomes is February 2020.

highly unlikely. We keep claimants who are in either Light Touch or Intensive Work Search when they enter UC. Claimants who are classed as self-employed in their first month of UC are removed, since they are subject to different conditionality requirements. Furthermore, we initially choose a narrow window ranging from £4 to £12 either side of the AET, so claimants in the sample are at most £24 apart in terms of their monthly gross earnings to begin with. The resulting sample size ranges from 2,359 to 6,947 individuals. As a robustness check, we explore the impact on the results of varying this window size between £4 and £12.

Figure 2 shows that the AET determines a claimant's labour market regime. Almost everyone below the AET is in the Intensive Work Search group, and almost everyone above is in the Light Touch group. Note that in the sample used for this analysis, the AET was £338 per month for single contracts. Given that the number of claimants who are in Light Touch/Intensive Work Search but below/above the AET is very small, we choose to proceed with a sharp regression discontinuity design approach (as opposed to a fuzzy RDD). That is, we treat everyone below the AET as if they were treated, and vice versa. Strictly speaking, this means we are estimating the impact of being just below the AET in the first month of UC, rather than the impact of being in the Intensive regime, although Figure 2 shows that there is very little difference between the two in practice.

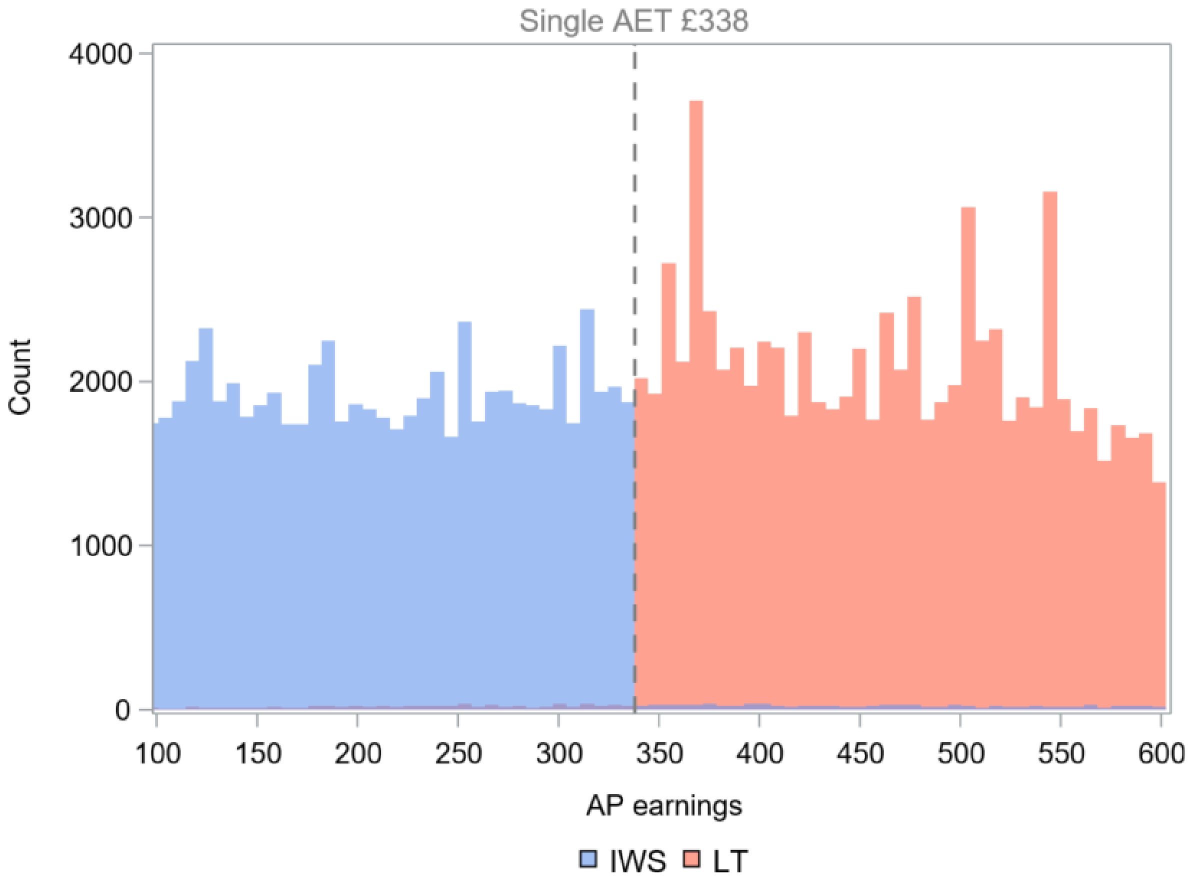



Figure 2: Distribution of claimants' gross earnings around the AET. The red bars show claimants in the Light Touch regime, the blue bars show claimants in the Intensive Work Search regime. A small number of claimants in Light Touch have earnings below the AET and vice versa; Work Coaches can, under certain circumstances (e.g. if the claimant is an apprentice), override the automatic regime allocation, but this is very rare in practice.

Figure 2 also helps to investigate whether claimants sort themselves either side of the AET when then they enter UC, which would violate the key assumption of the RDD methodology. Since being just above the threshold entails less conditionality, one might expect to see 'bunching' just above the AET. This 'bunching' does not appear to be present in the histogram above, suggesting (though not proving) that claimants do not know about the threshold at the time of entering UC.

We also partially test this assumption by looking at the distribution of claimants' earnings changes between their initial assessment period and the subsequent month. If claimants became aware of the AET and decided to earn just above it to avoid the Intensive regime, then we would expect the distribution of earnings changes to be shifted to the right of £0 for those who were initially below the AET. Figure 3 shows that this does not appear to be the case. The histogram of earnings changes for those who enter UC below the AET is approximately symmetric, visually and empirically – the skewness of the distribution is small (-0.11, compared to -0.18 for the "above" group). This provides some evidence in favour of the assumption that claimants do not sort themselves either side of the threshold early on in their UC claim, although it is still important to recognise that this assumption is not directly testable.

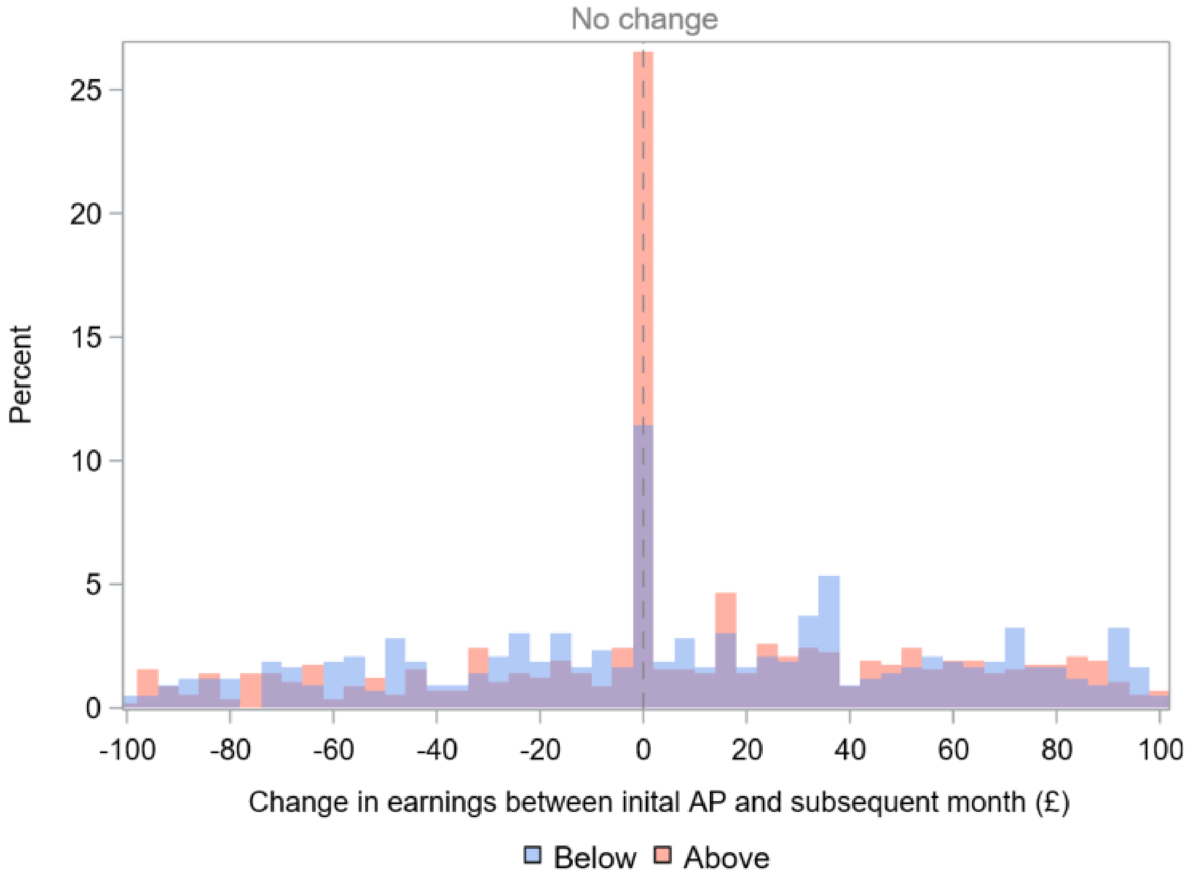



Figure 3: Distribution of gross earnings changes between claimants' initial AP and subsequent month, split by whether claimants were initially below (blue bars) or above (red bars) the AET.

*Empirical specification*

To begin, we estimate point-in-time treatment effects. Specifically, we estimate the impact on claimants' earnings progression 12 months after entering UC just below the AET, compared to entering UC just above. As a first step, we estimate the following simple regression:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 (X_i - c) + \varepsilon_i \qquad (1)$$

Where $Y_i$ is the claimant's change in monthly gross earnings 12 months after entering UC; $D_i$ is the treatment dummy variable equal to 1 if the individual entered UC with earnings just below the AET, and 0 otherwise; $X_i$ is the claimant's gross earnings in their first month of UC; $c$ is the value of the individual AET during the time period (£338), so $X_i - c$ is the claimant's 'distance' from the AET in their first month of UC, normalised to zero; and $\varepsilon_i$ is an idiosyncratic error term.

In the above specification, the coefficient of interest is $\hat{\beta}_1$ – the estimate of the treatment effect. This is a reasonable place to start, but this model constrains the slope of the regression lines to be the same on either side of the cut-off, which may not reflect the underlying relationship and could therefore introduce bias (Lee and Lemieux, 2010). Allowing the regression function to differ on either side of the threshold reduces the chance of misspecification. As a result, we add an interaction term to the previous equation:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 (X_i - c) + \beta_3 (X_i - c) D_i + \varepsilon_i \qquad (2)$$

Again, the estimate of the treatment effect is given by $\hat{\beta}_1$, but now the model is more flexible.

Following a similar approach to Girardi (2020), the empirical specification can be extended further to assess the dynamics of the treatment effect; that is, how the impact evolves over time. To do this, we create time dummies for each relative time period, and interact the terms in specification (2) with these relative time dummies. The resulting regression model is:

$$\begin{aligned} Y_{it} = \alpha &+ \sum_{t=-16}^{-1} \beta_t (D_i * \gamma_t) + \sum_{t=1}^{18} \beta_t (D_i * \gamma_t) + \sum_{t=-16}^{-1} \delta_t [(X_i - c) * \gamma_t] \\ &+ \sum_{t=1}^{18} \delta_t [(X_i - c) * \gamma_t] + \sum_{t=-16}^{-1} \theta_t [D_i * (X_i - c) * \gamma_t] \\ &+ \sum_{t=1}^{18} \theta_t [D_i * (X_i - c) * \gamma_t] + \gamma_t + \varepsilon_{it} \end{aligned} \qquad (3)$$

Where $Y_{it}$ is earnings progression of individual $i$ in time $t$, and $\gamma_t$ is a time dummy taking a value of 1 if the number of months since entering UC is equal to $t$ (e.g. when 10 months has passed since a claimant started UC, $\gamma_{10} = 1$). Consequently, each time period has its own estimated treatment effect, given by $\hat{\beta}_t$. This can be shown as follows:

$$\begin{aligned} LATE &= \lim_{x\uparrow c} \mathbb{E}[Y_{it}|X_i = x] - \lim_{x\downarrow c} \mathbb{E}[Y_{it}|X_i = x] \\ &= \mathbb{E}[Y_{it}|D_i = 1, X_i - c = 0] - \mathbb{E}[Y_{it}|D_i = 0, X_i - c = 0] \\ &= (\alpha + \beta_t + \gamma_t) - (\alpha + \gamma_t) \\ &= \beta_t \end{aligned}$$

Each estimated coefficient $\hat{\beta}_t$ is the same as would be estimated by running separate regressions for each time period, $t$. For instance, $\hat{\beta}_{10}$ estimated by equation (3) is equal to the point-in-time treatment effect on earnings progression at 10 months, estimated by equation (2). Hence, equation (3) is an RDD event study specification that shows the dynamics of the treatment effect. Note that time period zero is left out of the specification, so coefficients $\hat{\beta}_t$ show the impact on earnings progression relative to month zero (the month when claimants join UC). Estimating the treatment effect at each month via specification (3) is also more convenient than running separate regressions, since it allows for appropriate clustering of standard errors.

In addition to estimating the effect on claimants' earnings, we also investigate whether entering UC just below the AET affects the *variance* of earnings. This can shed light on whether work search requirements have an impact on the stability/sustainability of employment. As Arni and Schiprowski (2015; 2019) found, it's possible that requirements can improve employment outcomes, but at the cost of lower job stability. Specifically, we estimate the following regression:

$$\varphi_i = \alpha + \beta_1 D_i + \beta_2 (X_i - c) + \beta_3 (X_i - c) D_i + \varepsilon_i \qquad (4)$$

Where $\varphi_i$ is the variance of claimant $i$'s monthly earnings over the 12 months after entering UC. The coefficient of interest is again $\hat{\beta}_1$. Standard economic theory also suggests that agents prefer to smooth their consumption over time; whether the IWS regime aids or hinders this can be investigated via this specification.

Another area that has been examined in the literature is the effect of work search requirements on unsubsidised unemployment. In a given month, an individual is unemployed and unsubsidised if they have both zero earnings and zero UC payment. One limitation of this definition is that it may misclassify those who receive non-UC benefits. However, within the available data, we only observe spells on UC. We therefore adapt equation (2) by changing the outcome variable to the cumulative number of months in unsubsidised unemployment, 12 months after entering UC. We also modify equation (2) to investigate the effect of entering UC below the AET on the cumulative number of months spent on UC in non-working conditionality groups. This aims to provide evidence on whether the Intensive regime affects the chances of becoming economically inactive.

Finally, we perform subgroup analysis to test for the presence of heterogeneous treatment effects. We do so by splitting the sample into subgroups along four dimensions (age, sex, previous earnings, and local labour market tightness) and estimating equation (2) within each subsample.

## Results and findings

*Descriptive results*

Figure 5 plots the average monthly earnings over time of claimants who enter UC just below the AET and claimants who enter UC just above.

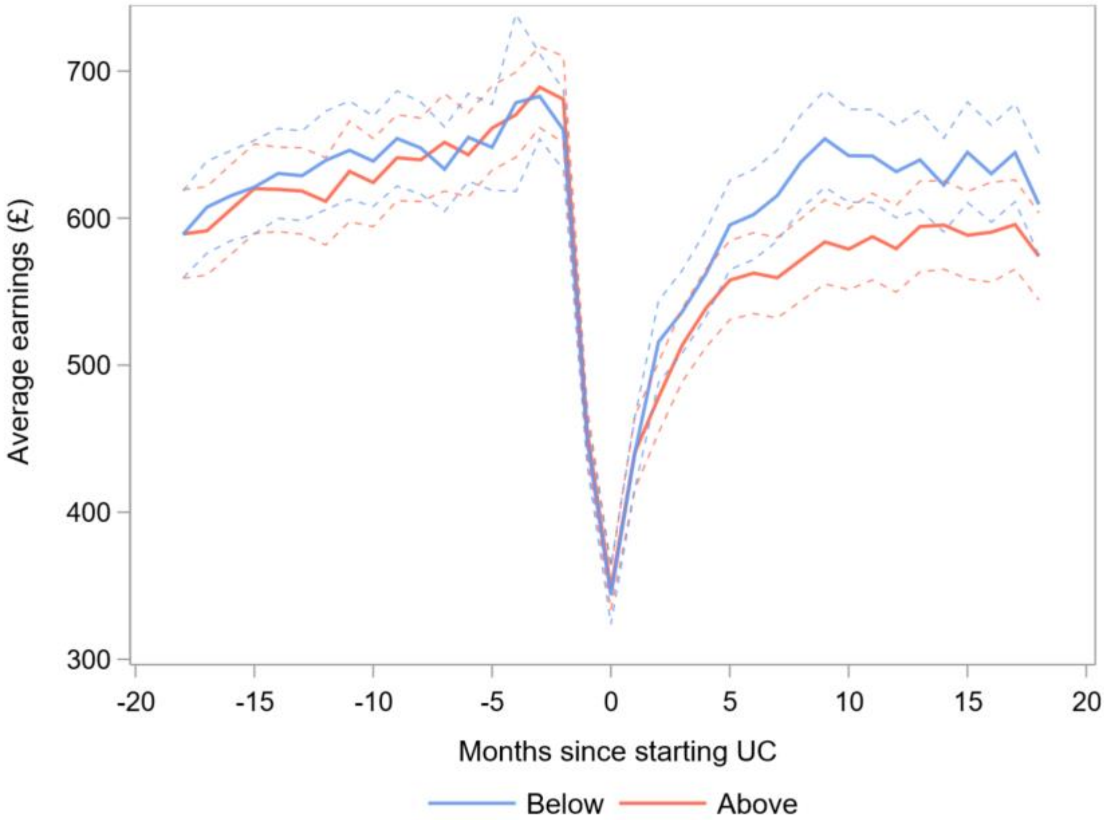



Figure 5: Average gross earnings before and after starting UC, split by whether claimants were initially below (blue lines) or above (red lines) the AET. Dashed lines represent 95% confidence intervals.

The results are quite striking. In the period before joining UC, the average earnings of both groups track each other closely. However, in the months afterwards, claimants who started UC just below the AET see higher earnings compared to those who started just above, on average. The following sections interrogate this result further to see whether it holds within the formal RDD framework, and after applying a variety of robustness checks.

*Point-in-time results*

We begin with the point-in-time findings. The results from specifications (1) and (2) are presented in Table 1 below:

| | **Treatment effect (£ pm)** | | |
|---|---|---|---|
| **Window size either side of cut-off (£)** | (1) Linear, same slopes | (2) Linear, different slopes | Obs. |
| **12** | 73.00** | 72.28** | 6,947 |
| **11** | 97.04** | 97.46** | 6,257 |
| **10** | 119.84*** | 119.50*** | 5,774 |
| **9** | 118.71*** | 118.42*** | 5,104 |
| **8** | 101.16** | 101.37** | 4,613 |
| **7** | 105.15** | 105.38** | 4,032 |
| **6** | 141.39*** | 142.58*** | 3,311 |
| **5** | 113.43** | 113.90** | 2,835 |
| **4** | 98.29 | 98.29 | 2,359 |

Table 1: *** p<0.01, ** p<0.05, * p<0.10. P-values calculated using heteroskedasticity robust standard errors.

The headline results above suggest that the point-in-time treatment effect after 12 months lies around £100 per month. That is, 12 months after entering Universal Credit, claimants who began their claim just below the AET earn approximately £100 more per month than claimants who entered just above. Reassuringly, the results are consistent across both specifications. As the window size is decreased from £12 to £4 either side of the AET, the estimated impact fluctuates around £100. At all window sizes other than £4, the estimated treatment effect is statistically significant at either the 5% or 1% level.

Figure 6 and Figure 7 show a graphical view of the results from specification (2). Figure 6 shows the 95% confidence bands of the estimated treatment effect for each window size, from £12 to £4. The results appear to be robust to varying the window size. The estimated coefficients hover around and above £100 for nearly all bandwidths, and they do not exhibit large jumps in magnitude from one window to the next.


Figure 6: Estimated treatment effects at different window sizes for specification (2). 95% confidence intervals are constructed using heteroskedasticity robust standard errors.

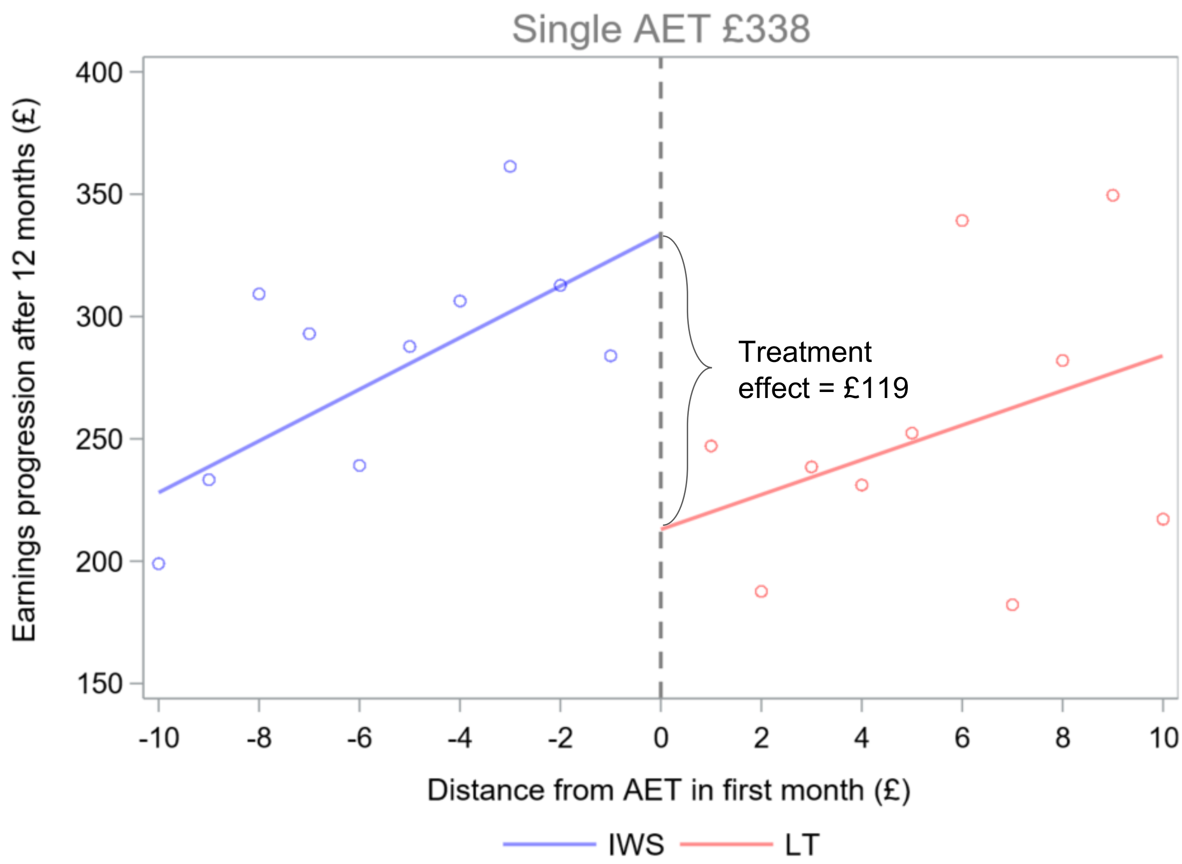



Figure 7: Graphical representation of estimated treatment effect using a window size of £10 either side of the AET. Data have been grouped into £1 ‘bins’ for visual clarity.

Taking the £10 window as an example, Figure 7 displays the fitted regression lines either side of the AET. The grouped data points are somewhat noisy; however, there is a noticeable discontinuity above and below the threshold. The vertical distance between the regression lines on either side of the AET is equal to the estimated impact (£119). This jump suggests that being just below the AET in the first month of the UC claim is important for future earnings progression.

Overall, the results indicate that the Intensive regime can have positive earnings impacts for Universal Credit claimants who are in employment but working relatively few hours.

*RDD event study results*

In addition to the point-in-time analysis, the evolution of the treatment effect over time can also be examined. As explained earlier, using an RDD event study framework allows us to estimate the impact of entering UC just below the AET compared to just above at each month.

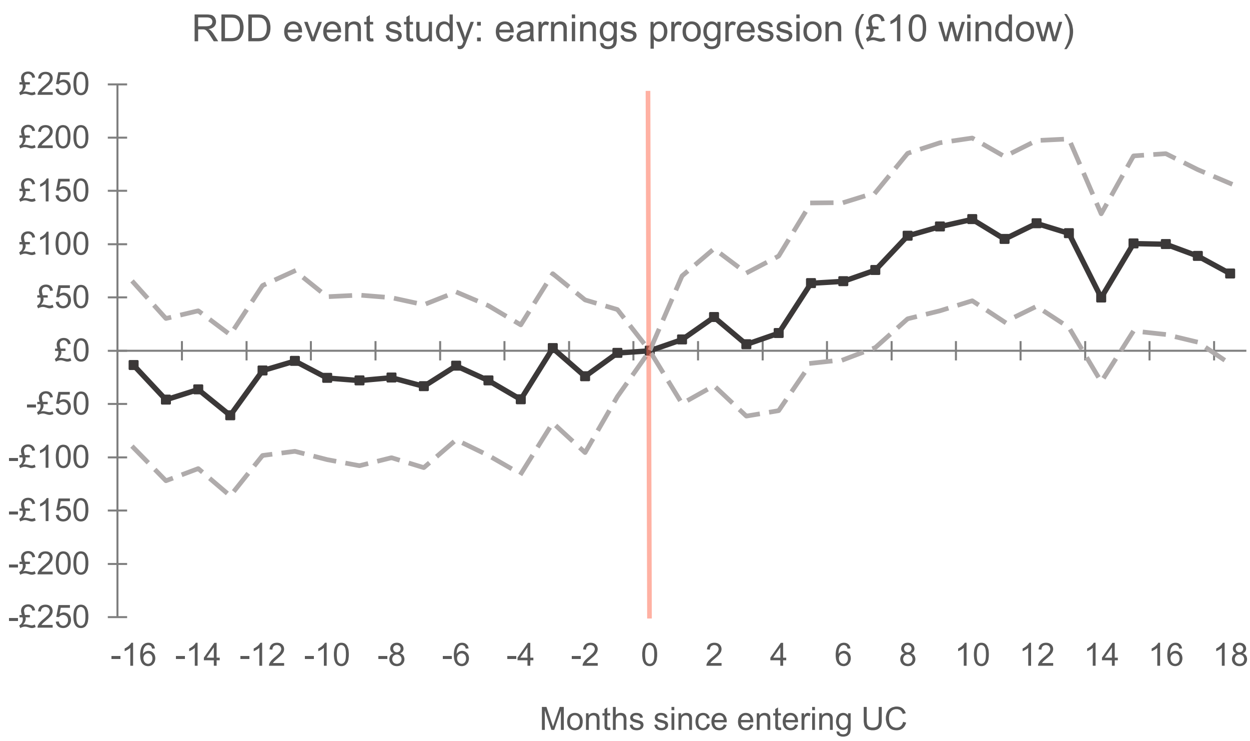



Figure 8: Graphical representation of the results from specification (3). The graph plots coefficients from the RDD event study specification. Window size of £10 either side of the threshold. 95% confidence intervals are calculated using standard errors clustered by individual.

Figure 8 shows the results of the RDD event study analysis, taking a window size of £10 as an example (see the Annex for plots using alternative window sizes). The graph shows how the impact on earnings progression of entering UC just below the AET changes over time. The vertical red line represents the point at which claimants join UC (month zero). Before this point, there appears to be no significant impact, which is expected since the intervention hasn't occurred yet. This lack of pre-treatment trend is reassuring, as it indicates that the treatment and control groups' outcomes were evolving in a similar fashion prior to entering UC. After starting UC however, the treatment effect begins to emerge, becoming statistically significant at around 7 months. It gradually increases and stabilises at around £110 from month 8 to 13, after which it begins to fall. Not only does this provide insight into the time it takes for impacts to materialise, it also shows that the point-in-time results reported earlier aren't dependent on the choice of month (within a reasonable range). It should be noted that up to 12 months since joining UC, none of the sample are impacted by Covid-19. Results from 13 to 18 months should therefore be treated with a degree of caution, although they do offer some insight into the persistence of treatment effects during an economic downturn.

*Volatility of earnings*

| **Effect of entering UC below the AET on variance of monthly earnings** | | |
|---|---|---|
| **Window size either side of cut-off (£)** | (4) Linear, different slopes | Obs. |
| **12** | 8691 (21008) | 6,947 |
| **11** | 10465 (21662) | 6,257 |
| **10** | 19931 (22274) | 5,774 |
| **9** | 21987 (24076) | 5,104 |
| **8** | 20045 (26228) | 4,613 |
| **7** | 22035 (30297) | 4,032 |
| **6** | 33349 (29006) | 3,311 |
| **5** | 26535 (31401) | 2,835 |
| **4** | 36172 (36640) | 2,359 |

Table 2: *** p<0.01, ** p<0.05, * p<0.10. Standard errors in brackets. P-values calculated using heteroskedasticity robust standard errors.

As described in the *Methodology* section, we also investigate the impact of work search requirements on the volatility of claimants' earnings. Table 2 presents the results from specification (4) at various window sizes. The estimated coefficients are all positive; however, the standard errors are large, and none of the estimates are close to statistical significance. Consequently, the null hypothesis of no effect on volatility of earnings cannot be rejected. This suggests that the trade-off between increased earnings and lower job stability found in previous studies of work requirements is not present here to the same degree. Claimants in this study are already in work, so there may be less scope for conditionality to have large impacts on the variance of earnings. Having said this, at the lower end of the earnings distribution, employment is more likely to be casual and irregular.

*Unsubsidised job search and non-working conditionality groups*

| Effect on unsubsidised job search and non-working conditionality groups | | | |
|---|---|---|---|
| **Window size either side of cut-off (£)** | Cumulative months with zero earnings and zero UC payment | Cumulative months in non-working conditionality groups | Obs. |
| **12** | 0.010 | 0.444*** | 6,947 |
| **11** | -0.027 | 0.231* | 6,257 |
| **10** | -0.021 | 0.158 | 5,774 |
| **9** | -0.010 | 0.073 | 5,104 |
| **8** | 0.036 | 0.087 | 4,613 |
| **7** | -0.035 | 0.024 | 4,032 |
| **6** | -0.141 | 0.284* | 3,311 |
| **5** | -0.208* | 0.353** | 2,835 |
| **4** | -0.194 | 0.183 | 2,359 |

Table 3: Results from specification (2) with cumulative months in unsubsidised unemployment and non-working conditionality groups as the dependent variables. *** p<0.01, ** p<0.05, * p<0.10. P-values calculated using heteroskedasticity robust standard errors.

To see how the results compare with the existing literature, we also investigate the effect of entering UC just below the AET in the Intensive group on the number of months spent in either unsubsidised unemployment or non-working UC conditionality groups. Table 3 shows that there is no strong evidence of an impact on unsubsidised unemployment. The estimated coefficients are negative at almost all window sizes, but only one is statistically significant, at the 10% level. There is weak evidence of an increase in the number of months spent in non-working conditionality groups. The point estimates are positive, but they do vary across window sizes, and only four are statistically significant. The estimates are fairly small: all correspond to less than half a month more spent in 'inactive' conditionality groups.

These findings contrast with the results of Manning (2009), and Morescalchi and Paruolo (2020) who found that work search requirements used in Jobseeker's Allowance led to an increase in unsubsidised unemployment. The results also differ from Petrongolo (2009), as there is only weak evidence of an increase in the amount of time spent in non-working conditionality groups. This may be because this paper focuses on claimants who are in employment when they enter UC.

*Heterogeneous treatment effects*

We also explore whether the effects estimated in Table 1 are heterogenous, i.e. whether they vary by claimant characteristics (sex, age, and previous 12 months earnings) or with local labour market tightness. To that end, we calculate the median age, level of previous earnings, and local labour market tightness, before estimating specification (2) on each subsample above and below the median. For local labour market tightness, we calculate the mean for each claimant over the 12 months after they enter UC. The median of this 12-month average is then used to split the sample into high and low labour market tightness subgroups.

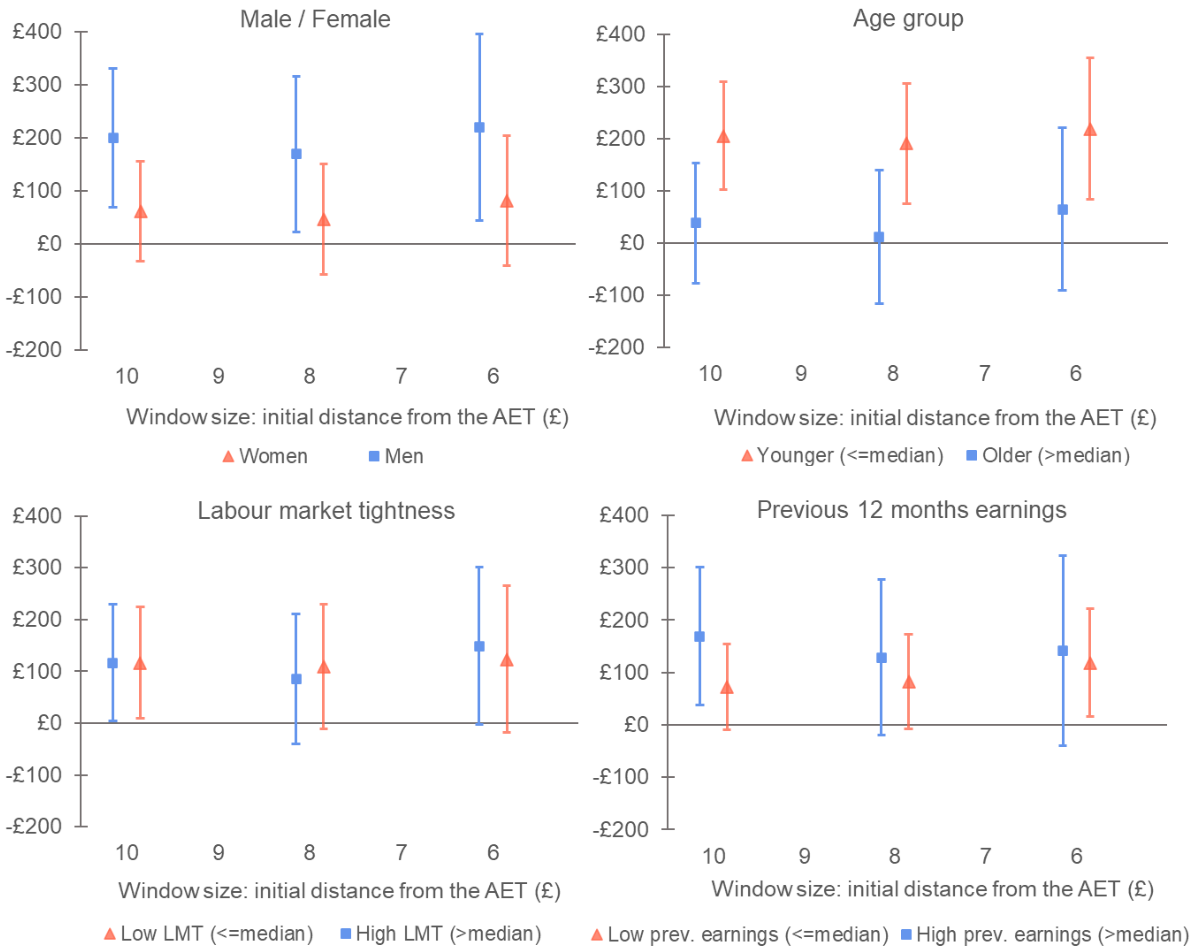



Figure 9: Estimated treatment effects from specification (2) for different subgroups at different window sizes. 95% confidence intervals are constructed using heteroskedasticity robust standard errors. The median levels of age, previous 12 months earnings, and local labour market tightness are 33.8, £5958.40, and 0.77, respectively.

The graphs above show the estimated treatment effects separately for men and women, younger and older claimants, claimants in slack and tight local labour markets, and claimants with low and high previous earnings. Although not definitive, on average, men and younger claimants appear to benefit more from entering UC in the Intensive regime than women and older claimants. The results suggest that the effects do not differ in tight vs slack labour markets. This may indicate that there is a benefit to operating the work search requirements policy throughout the economic cycle, although the sample does not contain a recession. Finally, the treatment effects are somewhat larger for claimants with relatively high previous earnings, although the differences are less substantial than the male/female and younger/older comparisons.

These subgroup results are tentative and should be interpreted with caution, due to the smaller sample sizes, and the potential for issues of multiple hypothesis testing. The policy implications of these heterogeneous effects are not immediately obvious – the AET is a threshold that applies to all claimants and is not based on personal characteristics. However, notwithstanding the large confidence intervals, it is encouraging that none of the estimated effects were negative, suggesting that a rise in the threshold would not systematically disbenefit these groups.

## Robustness checks

Having set out the headline results above, we now discuss several robustness checks. As discussed in the *Methodology* section, the key assumption of the regression discontinuity design is continuity in conditional expectation functions. While this assumption cannot be directly tested, several falsification methods can be used to provide evidence for or against it. In addition to the alternative choices of window size explored in the *Results* section, we conduct balance tests of observable characteristics to see whether those above/below the AET are different from one another at the threshold, and placebo tests to assess whether jumps occur at fake thresholds.

*Balance tests*

As discussed in the *Data* section, the only non-outcome variable that should 'jump' at the cut-off is treatment status. If any baseline characteristics also change at the threshold, the RDD may not be valid – the results could be caused by changes in other characteristics, rather than the treatment. In other words, these covariates should be 'smooth' at the cut-off. Again, we estimate specification (2), but this time replacing the outcome variable with initial characteristics:

$$\eta_i = \alpha + \beta_1 D_i + \beta_2 (X_i - c) + \beta_3 (X_i - c) D_i + \varepsilon_i \qquad (5)$$

Where $\eta_i$ is individual $i$'s value of some characteristic (e.g. age).

If the characteristics are smooth at the AET, the coefficient $\beta_1$ should not be significantly different from zero. Table 5 presents the results of these checks, using a window size of £10 either side of the cut-off:

| Dependent variable | Parameter estimate | P-value |
|---|---|---|
| **Age** | -1.93 | 0.005 |
| **Male** | -0.016 | 0.524 |
| **Previous 12 months earnings** | -310.00 | 0.360 |
| **Previous 16 months earnings** | -466.78 | 0.306 |
| **UC payment** | -10.91 | 0.566 |
| **Income tax paid** | 1.54 | 0.681 |
| **Eligible children** | 0.030 | 0.385 |
| **Health issue** | -0.033 | 0.094 |
| **Day of entry** | -0.405 | 0.367 |
| **Month of entry** | -0.476 | 0.020 |
| **Year of entry** | -0.011 | 0.709 |
| **England** | -0.0027 | 0.890 |
| **Scotland** | -0.0017 | 0.917 |
| **Wales** | 0.0044 | 0.698 |
| **Council rent** | -0.050 | 0.042 |

Table 5: Results from specification (5). P-values calculated using heteroskedasticity robust standard errors. Other housing type variables (e.g. private renter, own property, etc) were also tested in addition to council rent – no other imbalances were found.

Imbalances were found in three variables at the 5% significance level, and one more at the 10% level. To test whether the estimated treatment effect is being driven by this, we include the variables that appear to be discontinuous at the cut-off in the main regression specification. Figure 10 presents the estimated treatment effects at the various window sizes after controlling for the covariates where significant differences were found.

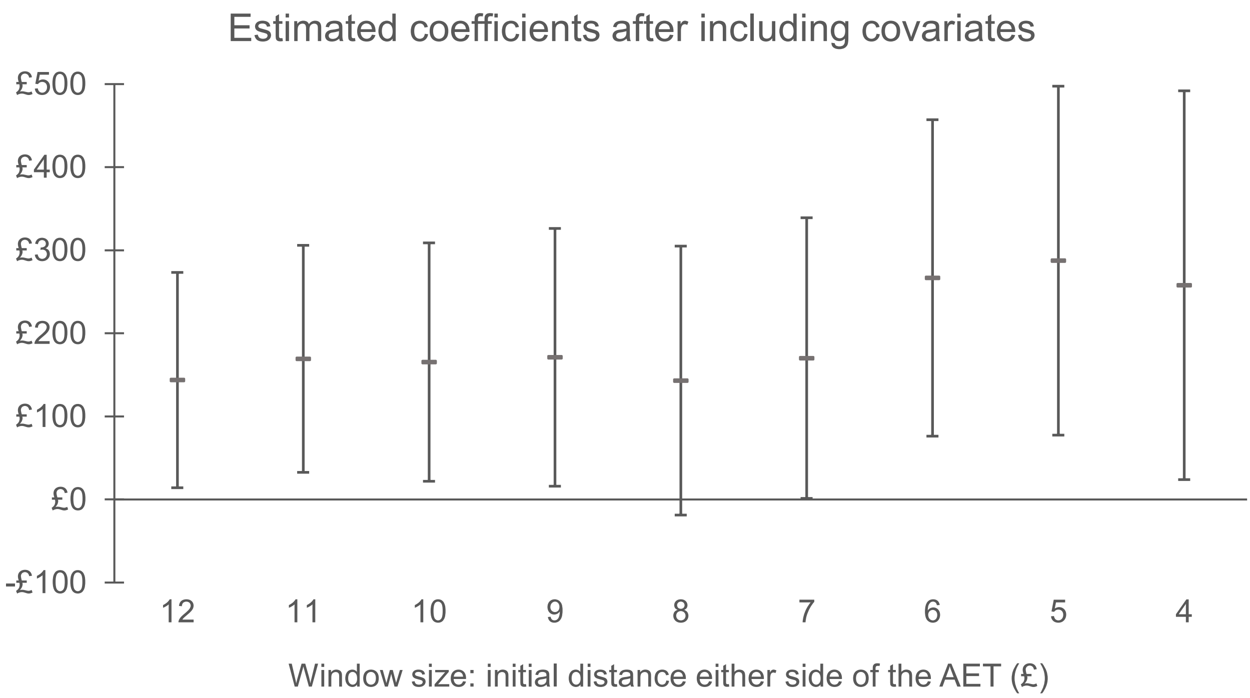



Figure 10: Estimated treatment effects at different window sizes for specification (2), after including the following covariates: age, health issues, month of entry, and council renting. Interaction terms between these characteristics and the treatment dummy variable were also included. 95% confidence intervals are constructed using heteroskedasticity robust standard errors.

Although the confidence intervals become wider, the inclusion of covariates increases the estimated treatment effects at all window sizes, suggesting that the observable imbalances found previously are likely not driving the positive impacts of entering UC just under the AET. Consequently, we can have more confidence that the headline results are estimating a causal impact, although some caution should be exercised since it is not possible to test whether unobserved characteristics are balanced. For instance, less motivated individuals could aim to be just above the AET to avoid work search requirements (although this may be correlated with observables such as previous earnings, which don't show imbalances).

*Placebo tests*

Another useful robustness check for RDDs are placebo tests. The intuition here is that there should only be a discontinuous jump in earnings progression at the real threshold (the AET) – there should not be many discontinuities at other points in the income distribution since the actual treatment status of the individuals isn't changing. Following the permutation test method used in Girardi (2020), we extend the approach suggested by Imbens and Lemieux (2008). First, we split the sample into two groups: one below the AET and one above. Then we randomly select 200 'fake' thresholds (placebos) in each half of the sample. Finally, we assign people below and above the placebo thresholds to 'fake' treatment and control groups. This results in 400 fake thresholds and 400 placebo tests.

This approach reveals how often we observe significant discontinuities when there is in fact no difference in treatment intervention (Eggers, Tuñón, and Dafoe, 2021). If such discontinuities are found commonly, one might question whether the impact observed at the true threshold is actually caused by the treatment. For example, an incorrectly specified regression model could result in estimating apparently significant effects even when there are none.

Figure 11 shows distributions of both the t-statistics and coefficients from estimating specification (2) for each of the 400 placebos, using window sizes of £10, £8, and £6 either side of the placebo threshold.

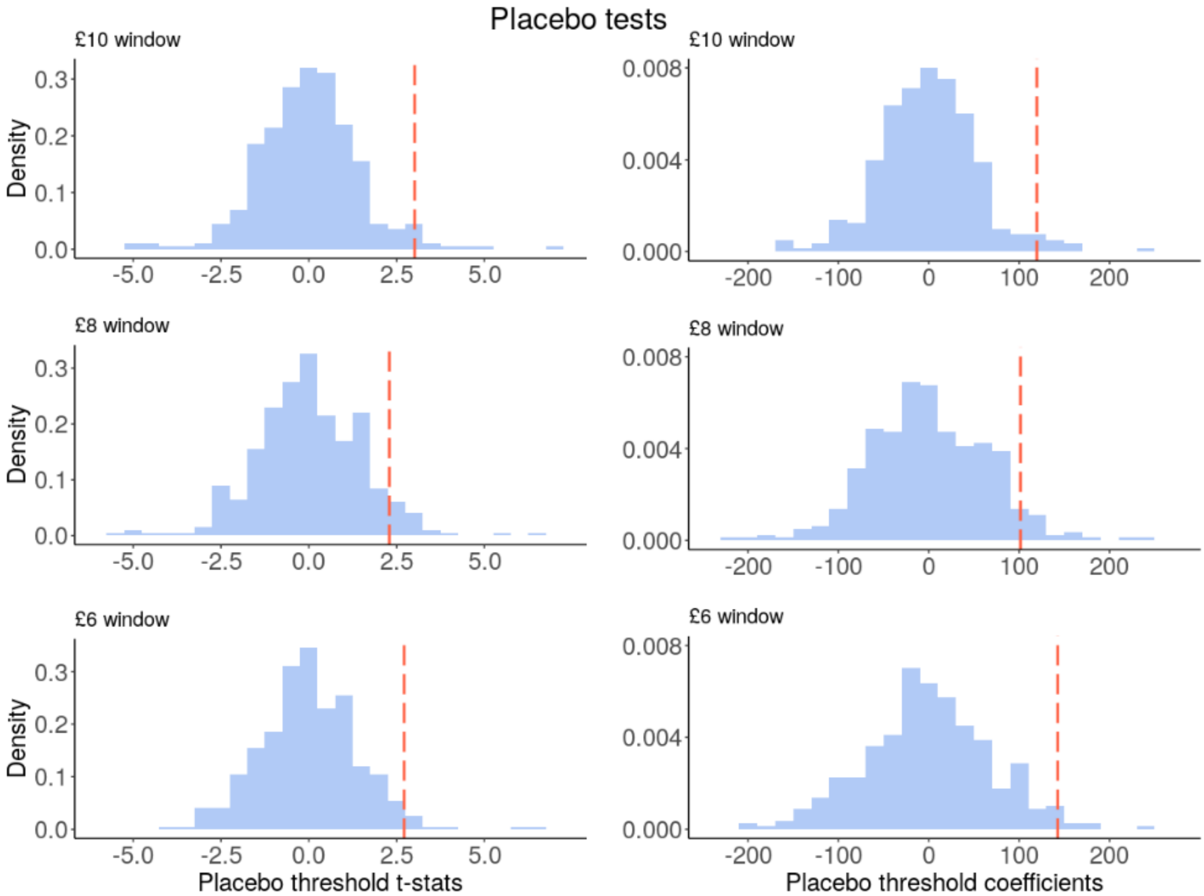



Figure 11: Distribution of t-statistics and coefficients from placebo tests using 400 randomly drawn thresholds. The vertical dashed red line represents the t-statistic/coefficient observed at the true threshold. Placebo t-statistics calculated using heteroskedasticity robust standard errors.

Reassuringly, the t-statistics and the coefficients observed from the true threshold lie in the tails of the distributions of the placebo t-statistics and coefficients, and the distributions of placebo t-statistics and coefficients are centred around zero. This indicates that it is rare to find results that are as extreme as the estimates from the real threshold. This is the case across the three different window sizes, although more so for the £10 and £6 windows. For the £10 window, the t-statistic observed at the true threshold (3.01) is exceeded in absolute value in only 4.5% of placebo tests, while the coefficient (£119.50) is exceeded in just 4% of cases. The corresponding results using the £8 window are 11.75% and 11%, respectively. For the £6 window, the results are 5% and 3.75%.

Although the result from the real threshold using the £8 window is more uncertain, overall, the placebo tests suggest that the treatment effects found at the true threshold are unlikely to be merely a statistical fluke.

## Discussion and conclusions

In this paper, we have implemented a regression discontinuity design to estimate the impact of entering Universal Credit just below the Administrative Earnings Threshold. The findings show that, on average, claimants who start Universal Credit just below the threshold see higher earnings progression in the months after joining – approximately £100 more per month after 12 months. The results are robust to the numerous tests applied.

Those who are below the AET are almost all in the Intensive Work Search regime, while those above are almost all in the Light Touch regime. Therefore, these results suggest that the Intensive regime can have positive impacts on the earnings progression of claimants who are in employment, but with low earnings. Although the effects take some time to emerge, they don't appear to fall to zero in the timeframe observed. Furthermore, the impacts are stronger than those found in the In-Work Progression RCT (DWP, 2018b). Comparing across studies is difficult, but one potential explanation for this disparity might be that the claimants in the IWP RCT were sampled from the entire Light Touch distribution, while this study focuses on the lower end of the earnings distribution. The returns to work search activity could be higher at lower points on the earnings distribution if, for example, claimants have more 'room' to increase their working hours. In addition, participants in the IWP RCT were both existing claimants and new 'inflow' claimants. Hence, it could also be that the initial labour market regime a claimant is allocated to is of particular importance.

The results partially support those of Arni and Schiprowski (2015; 2019), who find that work search requirements improve labour market outcomes. However, we find no significant evidence that these requirements impose a cost in terms of job stability. In contrast to other studies, we do not find that search requirements increase the number of months spent in unsubsidised unemployment. In addition, we only find weak evidence of an increase in the number of months spent in inactive groups on UC, while Petrongolo (2009) finds that claimants become more likely to receive health related benefits. Although these concepts are not identical, the differences in results may be because this paper focuses on claimants who are in-work when they join UC, who may be less likely to suffer from a health problem, and less prone to becoming unemployed.

This paper does contain some limitations. For example, we do not analyse the presence of general equilibrium effects or spillovers. It may be that the benefits of the Intensive regime are smaller if a larger number of claimants receive it (Crépon et al., 2013). Having said this, we found no difference in effects between tight and slack labour markets, which could indicate that the impacts are not lessened when many workers are competing for relatively few jobs. To avoid complications associated with the couple AET, this analysis is restricted to claimants on single contracts. We therefore cannot estimate whether the impact differs for claimants on couple contracts. Like any quasi-experiment, the identifying assumption in the analysis cannot be directly proven; however, we have made efforts to reduce the potential for bias via the sample selection, and the results of the numerous robustness checks are reassuring. In addition, one limitation of the RDD method is that it does not shed light on impacts away from the value of the threshold in the study. Therefore, the 'optimal' value of the

AET is not known, although evaluation of any future rises in the threshold would improve the evidence on this question. Finally, this study does not focus on the mechanisms underlying the estimated effects, which may be important when designing new active labour market policies.

Overall, the results from this research suggest that raising the AET would likely benefit both claimants (increased earnings progression) and the department (lower benefit expenditure). However, the RDD method provides *local* effects. That is, the larger any increase in the AET, the less confidence we can have in applying the results from this paper. It might also be that the impacts diminish for claimants who are already higher up the earnings distribution. Therefore, there may be a trade-off between a higher threshold encompassing more claimants, and the effect of the work search requirements on those claimants. Furthermore, raising the AET would increase the number of meetings held between Work Coaches and claimants, which would require an increase in departmental expenditure, *ceteris paribus*.

## Annex

*Alternative measures of earnings progression, and imputation of assessment period gross earnings*

Ideally, gross *assessment period* (AP) earnings would be used when calculating earnings progression, since AP earnings determines whether someone was just above/below the AET in their first month. However, we only observe AP earnings when the claimant is on UC; we have monthly RTI earnings when they are not on UC. As a result, there are a few options for calculating earnings progression:

- use AP earnings for initial earnings, and then monthly RTI earnings thereafter,
- use monthly RTI earnings throughout, or
- impute AP earnings from monthly earnings via the following method:

Some claimants (over one third) are still on UC 12 months after they enter. We are interested (initially) in progression at 12 months, so for these claimants, we can estimate the relationship between their monthly RTI earnings and their AP earnings. Since APs don't line up with calendar months, earnings from the previous/next calendar month may be captured in AP earnings; consequently, we regress AP earnings on monthly earnings, plus a lag and lead:

$$g_{it}^{AP} = \delta_1 g_{it}^{RTI} + \delta_2 g_{it-1}^{RTI} + \delta_3 g_{it+1}^{RTI} + v_{it} \qquad (6)$$

Where $g_{it}^{AP}$ is the claimant's gross earnings in the AP that corresponds to their 12th month on UC, $g_{it}^{RTI}$ is monthly RTI gross earnings in their 12th month on UC, $g_{it-1}^{RTI}$ is a one month lag of $g_{it}^{RTI}$, $g_{it+1}^{RTI}$ is a one month lead, and $v_{it}$ is an error term. The results of this regression are shown in Table 6 below:

| Variable | Parameter estimate | P-value |
|---|---|---|
| $\boldsymbol{g_{it}^{RTI}}$ | 0.37635 | <.0001 |
| $\boldsymbol{g_{it-1}^{RTI}}$ | 0.05357 | 0.0608 |
| $\boldsymbol{g_{it+1}^{RTI}}$ | 0.59812 | <.0001 |

Table 6: Results from specification (6). No intercept included. P-values calculated using heteroskedasticity robust standard errors.

We use the values of the estimated coefficients, $\widehat{\delta_1}$, $\widehat{\delta_2}$, and $\widehat{\delta_3}$ to impute AP earnings in month 12 for claimants who were no longer on UC at this point:

$$earnings_{it}^{AP} = \left(\widehat{\delta_1} \times earnings_{it}^{RTI}\right) + \left(\widehat{\delta_2} \times earnings_{it-1}^{RTI}\right) + \left(\widehat{\delta_3} \times earnings_{it+1}^{RTI}\right)$$

As a robustness check, we explore the sensitivity of the results to alternative measures of earnings progression in Table 7 and Table 8 below:

**Monthly RTI earnings throughout**

| Window size either side of cut-off (£) | Treatment effect (£ pm) (1) Linear, same slopes | (2) Linear, different slopes | Obs. |
|---|---|---|---|
| **12** | 67.22* | 67.06* | 6,947 |
| **11** | 90.43** | 90.96** | 6,257 |
| **10** | 121.12*** | 120.43*** | 5,774 |
| **9** | 116.15*** | 116.19*** | 5,104 |
| **8** | 103.48** | 103.02** | 4,613 |
| **7** | 110.82** | 111.75** | 4,032 |
| **6** | 161.87*** | 161.07*** | 3,311 |
| **5** | 108.03* | 107.86* | 2,835 |
| **4** | 94.81 | 94.81 | 2,359 |

Table 7: Results from specifications (1) and (2) using the difference in initial monthly RTI earnings and month 12 RTI earnings as the measure of earnings progression. *** p<0.01, ** p<0.05, * p<0.10. P-values calculated using heteroskedasticity robust standard errors.

**Imputed AP earnings**

| Window size either side of cut-off (£) | Treatment effect (£ pm) (1) Linear, same slopes | (2) Linear, different slopes | Obs. |
|---|---|---|---|
| **12** | 79.52** | 76.83** | 6,919 |
| **11** | 92.03** | 92.01** | 6,262 |
| **10** | 118.39*** | 118.48*** | 5,771 |
| **9** | 119.83*** | 119.48*** | 5,106 |
| **8** | 105.43** | 105.82** | 4,611 |
| **7** | 119.75** | 119.64** | 4,019 |
| **6** | 132.43** | 134.50*** | 3,307 |
| **5** | 102.00* | 102.36* | 2,838 |
| **4** | 110.44* | 110.49* | 2,347 |

Table 8: Results from specifications (1) and (2) using the difference in initial AP earnings and imputed AP earnings 12 months later as the measure of earnings progression. *** p<0.01, ** p<0.05, * p<0.10. P-values calculated using heteroskedasticity robust standard errors.

Reassuringly, the results from both alternative measures of earnings progression are similar to the headline results in Table 1, and this holds across both regression specifications. This shows that the headline results aren't being driven by the choice of earnings progression measure, or differences between assessment period earnings and monthly earnings.

*Including earnings from self-employment*

The earnings progression measure used so far only includes employee earnings – self-employment earnings are not taken into consideration. It's possible that the treatment (or the absence of it) could affect the likelihood of entering self-employment, in which case using just employee earnings would not account for this. We therefore test the sensitivity of the results to including self-employment earnings in the earnings progression variable; however, self-employment earnings data should be treated with more caution, since it can take negative values if the business makes a loss in the period and is only available in our data while claimants are on UC.

| | **Treatment effect (£ pm)** | | |
|---|---|---|---|
| **Window size either side of cut-off (£)** | (a) Employee earnings only | (b) Employee and self-employment earnings | Obs. |
| **12** | 72.28** | 70.61* | 6,947 |
| **11** | 97.46** | 96.25** | 6,257 |
| **10** | 119.50*** | 118.63*** | 5,774 |
| **9** | 118.42*** | 116.97*** | 5,104 |
| **8** | 101.37** | 100.47** | 4,613 |
| **7** | 105.38** | 105.04** | 4,032 |
| **6** | 142.58*** | 143.82*** | 3,311 |
| **5** | 113.90** | 112.78** | 2,835 |
| **4** | 98.29 | 97.01 | 2,359 |

Table 9: Results from specification (2), including either just earnings from employment or earnings from both employment and self-employment in the outcome variable. *** p<0.01, ** p<0.05, * p<0.10. P-values calculated using heteroskedasticity robust standard errors.

Table 9 compares the estimated treatment effects when we include just employment earnings in the earnings progression variable (a), and when we also include self-employment earnings (b). Both measures of earnings progression produce very similar results across all window sizes, indicating that the headline results are not sensitive to the inclusion of self-employment earnings.

*Negative earnings values*

The results presented so far are based on an unbalanced panel, where we remove observations with negative earnings values in a given month. Here, we test whether the headline findings are robust to an alternative approach of removing individuals who ever receive a negative earnings value. This may be appropriate if the negative earnings value reflects a correction to incorrect PAYE entries in previous months. It also returns the dataset to a balanced panel, with an equal number of observations in each relative time period.

| Window size either side of cut-off (£) | Treatment effect (£ pm) (a) Remove monthly observations with negative earnings values only | (b) Remove anyone who ever receives a negative earnings value | Obs. (b) |
|---|---|---|---|
| **12** | 72.28** | 65.00* | 6,646 |
| **11** | 97.46** | 90.94** | 5,978 |
| **10** | 119.50*** | 112.17*** | 5,516 |
| **9** | 118.42*** | 115.35*** | 4,872 |
| **8** | 101.37** | 97.79** | 4,407 |
| **7** | 105.38** | 102.84** | 3,856 |
| **6** | 142.58*** | 133.03** | 3,158 |
| **5** | 113.90** | 100.59* | 2,702 |
| **4** | 98.29 | 91.67 | 2,241 |

Table 10: Results from specification (2), using two different methodologies for treating negative earnings values. *** $p<0.01$, ** $p<0.05$, * $p<0.10$. P-values calculated using heteroskedasticity robust standard errors.

Table 10 shows the headline results in column (a) and the results using the alternative process of removing negative earnings values in column (b). The treatment effects are slightly lower when removing those who ever receive a negative earnings value, but not dramatically so. Indeed, the results are still significant at most window sizes, and above £100 in five out of eight bandwidths (compared to six out of eight for the main results).

*Calendar time fixed effects*

In addition, we test the robustness of the RDD event study results to the inclusion of calendar time controls in specification (3). This could be important if, for example, the inflow into the treatment and control groups differs between calendar months. If the calendar time dummies are correlated with earnings progression (in addition to the treatment dummy), then this could impact the estimated treatment effects. We consider two approaches: including dummies for the calendar month of inflow on UC, and including dummies for the calendar month of the earnings progression observation.

Table 11 presents the coefficients $\beta_t$ from specification (3), with and without controls for calendar time. Column (a) shows the results without calendar time dummies, while columns (b) and (c) include the controls described above. The results are similar across all three specifications, indicating that the findings are not affected by controlling for calendar time effects.

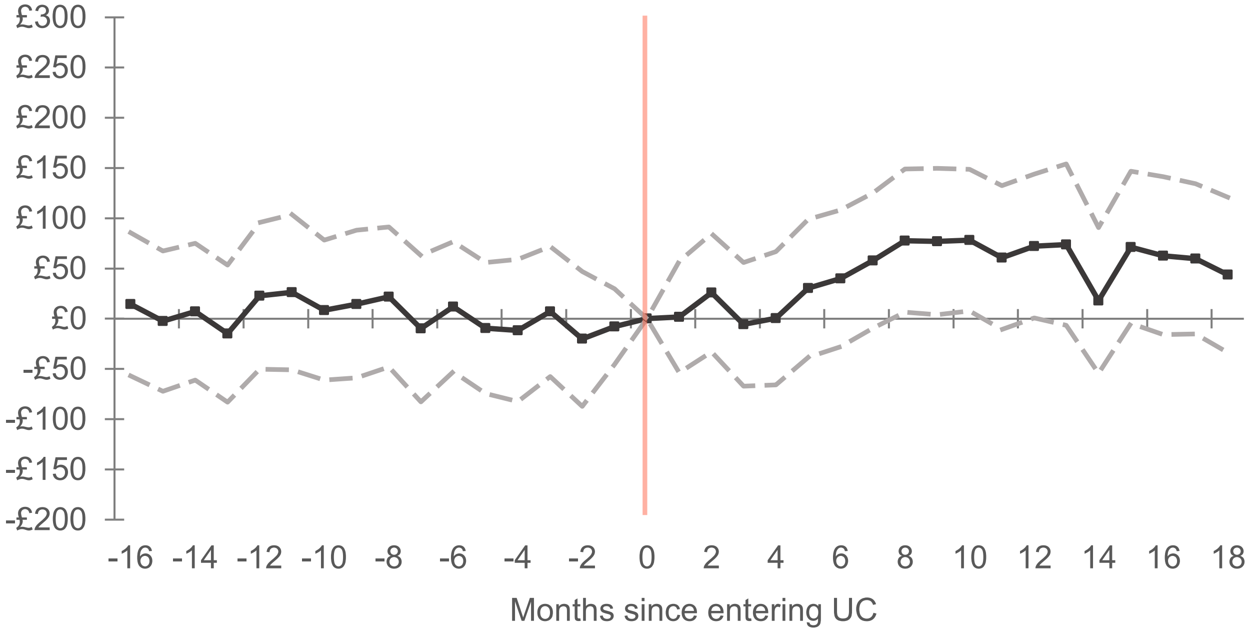
| Treatment effect (£pm) | | | |
|---|---|---|---|
| **Months since joining UC** | (a) Without calendar time controls | (b) Controls for calendar month of inflow | (c) Controls for calendar month of observation |
| **-16** | -13.49 | -17.66 | -17.97 |
| **-15** | -45.88 | -49.93 | -46.88 |
| **-14** | -36.54 | -40.63 | -39.14 |
| **-13** | -60.71 | -64.77* | -63.48* |
| **-12** | -18.56 | -22.71 | -16.15 |
| **-11** | -9.74 | -13.86 | -11.45 |
| **-10** | -25.72 | -29.82 | -30.95 |
| **-9** | -27.88 | -31.86 | -29.91 |
| **-8** | -25.22 | -29.31 | -30.62 |
| **-7** | -33.29 | -37.45 | -39.60 |
| **-6** | -14.31 | -18.37 | -17.22 |
| **-5** | -28.01 | -32.23 | -32.56 |
| **-4** | -45.73 | -49.98 | -50.65 |
| **-3** | 2.44 | -1.64 | -0.49 |
| **-2** | -24.05 | -28.18 | -29.24 |
| **-1** | -2.18 | -6.17 | -3.26 |
| **0** | 0 | 0 | 0 |
| **1** | 10.41 | 6.35 | 7.76 |
| **2** | 31.38 | 27.18 | 27.16 |
| **3** | 5.87 | 1.78 | 3.33 |
| **4** | 16.33 | 12.24 | 11.37 |
| **5** | 63.42* | 59.21 | 61.51 |
| **6** | 65.24* | 61.18 | 63.46* |
| **7** | 75.76** | 71.73* | 73.42** |
| **8** | 107.73*** | 103.63*** | 107.12*** |
| **9** | 116.45*** | 112.36*** | 115.66*** |
| **10** | 123.30*** | 119.27*** | 121.20*** |
| **11** | 104.65*** | 100.55** | 106.10*** |
| **12** | 119.50*** | 115.46*** | 121.67*** |
| **13** | 110.24** | 106.18** | 110.54** |
| **14** | 49.73 | 45.62 | 50.33 |
| **15** | 100.71** | 96.65** | 97.83** |
| **16** | 100.07** | 96.11** | 96.22** |
| **17** | 89.01** | 84.87** | 84.16** |
| **18** | 72.31* | 68.20 | 68.24 |
| **Obs.** | 213,467 | 213,467 | 213,467 |

Table 11: Results from RDD event study using a window size of £10. Column (a) reflects the results from specification (3). Columns (b) and (c) add different calendar time controls to specification (3). *** p<0.01, ** p<0.05, * p<0.10. P-values calculated using standard errors clustered by individual.

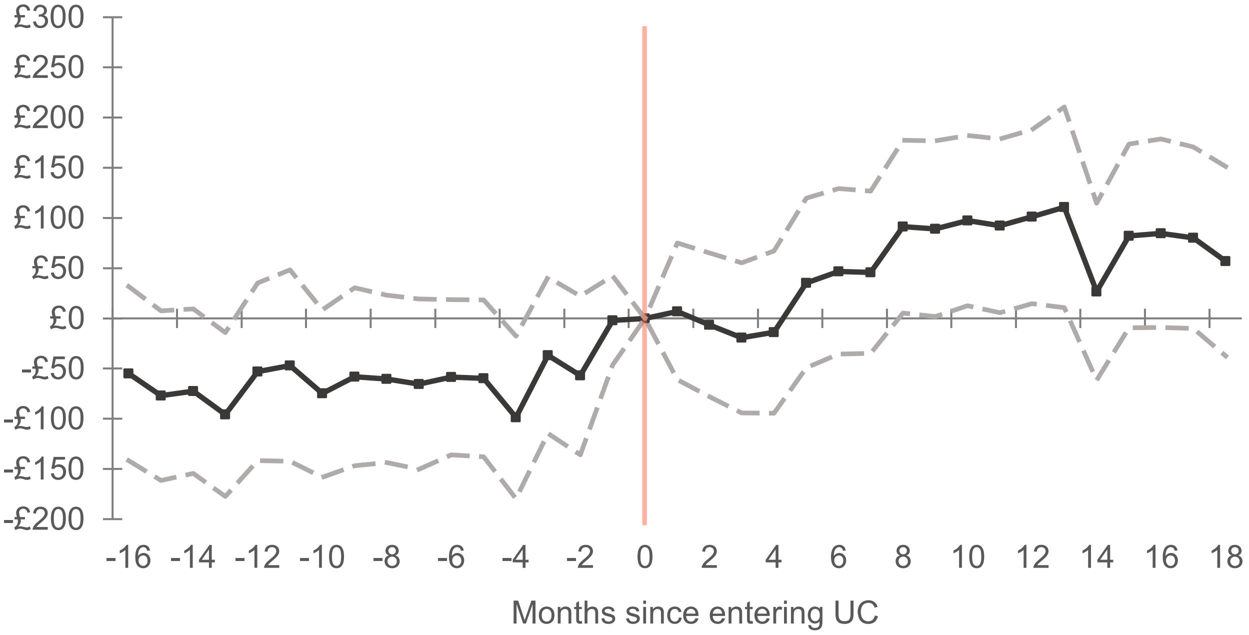
*RDD event study using alternative window sizes*

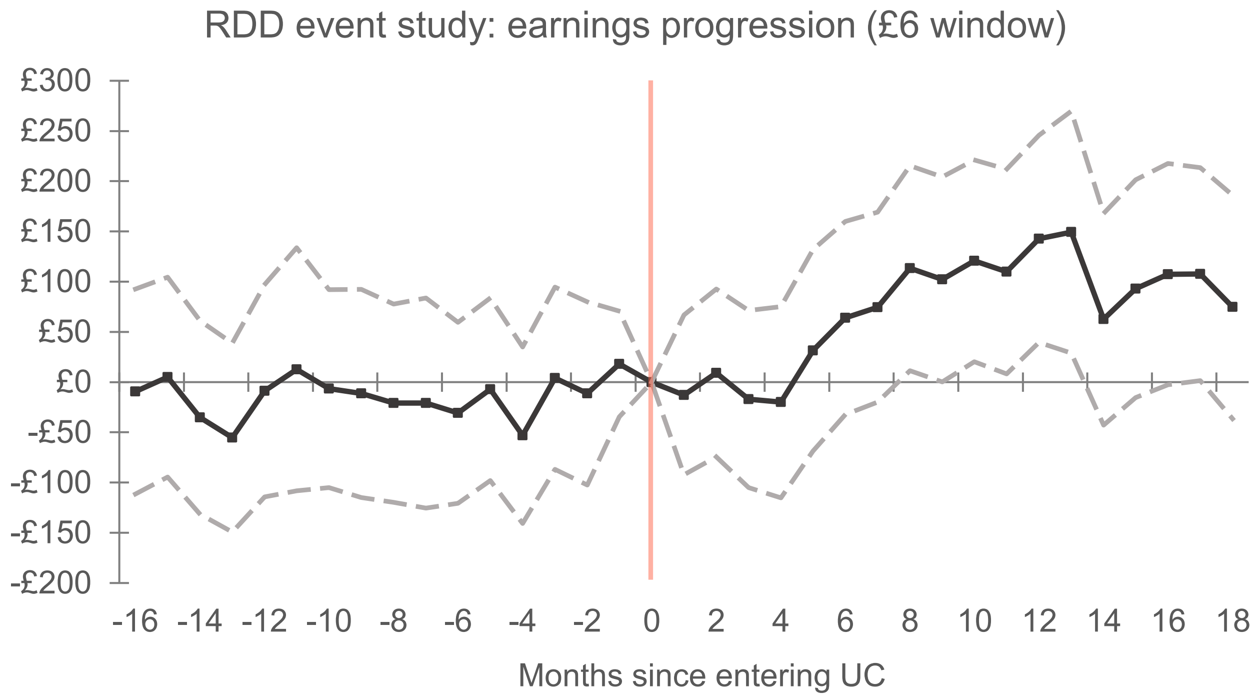
The *Results* section presented a plot of the treatment effect over time using a window size of £10 either side of the threshold. Here we assess whether the result observed using a £10 window remains with alternative window sizes of £12, £8, £6, and £4.

RDD event study: earnings progression (£12 window)




RDD event study: earnings progression (£8 window)





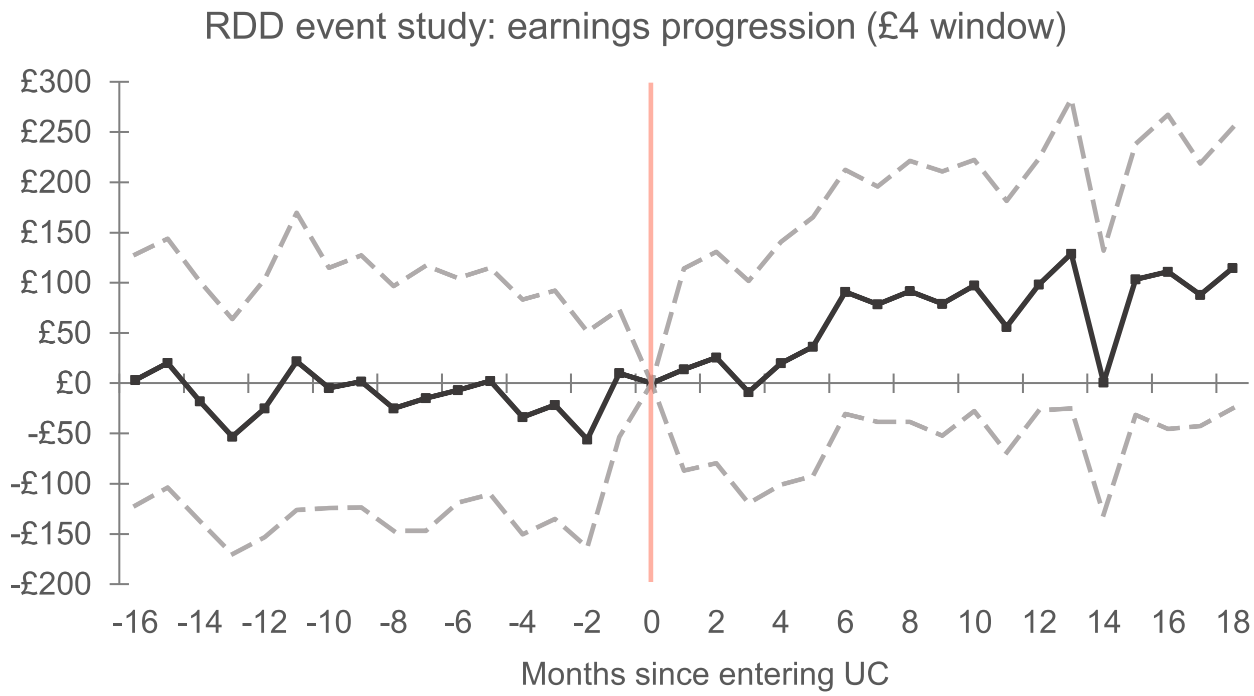




Figure 12: Graphical representation of the results from specification (3). The graphs plots coefficients from the RDD event study specification. Window sizes of £12, £8, £6, and £4 either side of the threshold. 95% confidence intervals are calculated using standard errors clustered by individual. Results from 13 to 18 months should be treated with some caution, since Covid-19 begins to affect an increasing proportion of individuals during this period.

Figure 12 displays the RDD event study plots for each window size (£12, £8, £6, and £4). The results are generally similar across the plots: flat pre-trends prior to month zero, and treatment effects that gradually increase after joining UC, before showing signs of decreasing towards the end of the timeframe. The treatment effects are somewhat smaller when using a £12 window, though the coefficients hover around statistical significance at the 5% level. In the case of the £4 window, the standard errors are larger due to the smaller sample size, and hence the treatment effects are not significant.

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